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

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September 23, 2021

Abstract

Children who face significant disadvantage early in life are often found to be worse off years or even decades later. Can social protection 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 randomized 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.

Keywords: fetal origins hypothesis, early life, children, safety net, conditional cash transfers, education, employment, Mexico
JEL Classification Codes: I14, I24, I38

* *This paper was previously titled "Recovering from Early Life Trauma: Dynamic Substitution Between Child Endowments and Investments."* We thank Doug Almond, Prashant Bharadwaj, Hoyt Bleakley, Flavio Cunha, Valentina Duque, Snaebjorn Gunnsteinsson, Victor Lavy, Manoj Mohanan, Kiki Pop-Eleches, T. Paul Schultz, John Strauss, Duncan Thomas, Atheen Venkataramani, Tom Vogl, Miriam Wust, and seminar participants at Hitotsubashi University, the SRCD Biennial Meeting, AEA Annual Meeting, PHS Research Workshop, Barcelona GSE Summer Forum, NBER (Children and Cohort Studies), USD, Michigan, USC, PAA, PacDev, Cal State Long Beach, NEUDC, UConn, and the CDC for helpful comments. Adhvaryu acknowledges funding from the NIH/NICHD (5K01HD071949). Molina gratefully acknowledges funding from the USC Provost's Ph.D. Fellowship, USC Dornsife INET graduate student fellowship, and Oakley Endowed Fellowship. Cristian Chica provided excellent research assistance.
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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? This is essentially a question about heterogeneous returns to social protection *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 general safety net 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 "may be asking for 'lightning to strike' twice: two identification strategies affecting the same cohort but at adjacent developmental stages."

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 randomized controlled trial of cash transfers for school enrollment in Mexico. In our study's agrarian setting, where weather plays a significant 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 have worse educational attainment and employment outcomes 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 randomized to receive conditional cash transfers 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%. 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 (who were 18 at the time of the 2003 survey and therefore have some realized measures of continued education and initial employment), 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 conditional cash transfers provided in their school-aged years. This implies that public investment can indeed help children who faced 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 very 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 that, consistent with our findings, documents 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. 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., 2019). Other studies find the opposite result or no evidence of significantly different impacts (Aguilar and Vicarelli, 2011; Duque et al., 2018; Johnson and Jackson, 2019; Malamud et al., 2016). These latter studies emphasize 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 cash transfer programs around the world (Blattman et al., 2013; Haushofer and Shapiro, 2013) – 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.

The rest of the paper is organized as follows. Section 1 provides background on the PROGRESA program in Mexico. Section 2 describes the survey data and rainfall data we use. We lay out our empirical strategy in section 3 and discuss our results in section 4. Section 5 concludes.

1 Program Background

1.1 Description of Program

In 1997, the Mexican government began a conditional cash transfer 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 summarizes the monthly grant amounts for the second semester of 1997, 1998 and 2003. From seventh grade onwards, the grants increase with grade level, with higher amounts for girls than boys.[2] At the program's onset, grants were provided only for children between third and ninth grade (the third year of junior high school). In 2001, the grants were extended to high school.

For evaluation purposes, the program was implemented experimentally in 506 rural localities from the states of Guerrero, Hidalgo, Michoacan, Puebla, Queretaro, San Luis de Potosi and Veracruz. 320 localities (the "treatment group") 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 randomized 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 summarize the key findings of this literature, categorizing studies based on the age of the analysis sample – specifically, how old they

[2] Given the lower rates of attendance of girls in rural Mexico, the policy's intention was to provide additional incentives to girls (Skoufias, 2005). However, as Behrman et al. (2005) note, girls tended to progress through schooling grades more quickly and therefore had higher educational attainment than boys. Skoufias and Parker (2001), Skoufias (2005), Behrman et al. (2009), and Behrman et al. (2011) cover additional program details in depth.

were during the years of PROGRESA being used to identify its effects. We also classify studies as education-related, health-related, cognitive or behavioral, and 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. In the first panel of Table D2, we show that existing work on school-aged children has focused almost exclusively on education outcomes: in the short and medium term, PROGRESA has been found to have improved educational attainment, grade progression, and other measures of schooling success. That the benefits of PROGRESA for school-aged children were primarily educational 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 in this paper 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 and for whom we observe schooling outcomes past primary school. When interpreting our results, therefore, we view the education subsidy channel as the main driving mechanism behind the results we find, not the health component or the actual cash received.[3] 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. As we summarize in Table 1, we use the 2003 survey to obtain the outcome variables for our main analysis,

[3]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).

and the baseline survey to construct control variables. For supplementary analysis, we also draw on both the 2003 and 2007 waves.

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
Behavioral	2007	8-10
Cognitive Tests	2003	2-6, 15-18

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 D3), while individuals over 18 are more likely to have moved out of the household by the 2003 survey and are therefore not surveyed.[4] While survey respondents (usually mothers or grandmothers) are still asked some questions about non-resident individuals, these responses are likely to introduce greater measurement error, potentially correlated with our regressors of interest. To avoid this issue, which is particularly problematic for our employment outcomes (which are missing for non-resident household members), we exclude individuals over 18 years old.

This issue is also what limits our use of the 2007 survey, during which our sample individuals were aged 16 to 22. Attrition is too high for us to continue to follow our sample individuals and use

[4]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.

their 2007 outcomes.[5] However, as summarized in Table 1, for some of our supporting analysis, we use child development measures collected for younger children in 2007. We also use other physical, cognitive, and behavioral outcomes collected during the 2003 survey for specific age groups (in most cases, different from our main sample of interest).

2.1.1 Outcome Variables

Our main education outcome 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. Given the fairly young age restrictions of our sample, the latter two variables are used as potentially more appropriate variables for individuals who have yet to complete their schooling. Educational attainment is constructed using information on the last grade-level achieved in 2003. "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. For an individual who is 7 years old, we expect them to have completed one grade, for an 8 year-old, two grades, and so on. In order to study differential effects by grade, we also use 12 dummy variables, each indicating whether the individual completed at least 3, 4, and up to 12 grades of 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). Similarly, we are interested in whether an individual was employed in the past week, employed in the past year, and employed in a non-laborer 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 laborers with the unemployed). We verify using the Mexican Family Life Survey that youths who are 18 to 20 years old and working in a laborer job during the 2002 survey have among the lowest hourly wages during

[5] 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. This unfortunate feature of the 2007 data has resulted in its limited use in the literature: the few studies that do use the 2007 data (for example, Behrman et al. (2008) and Fernald et al. (2009)) focus exclusively on PROGRESA's health effects on a much younger cohort, for whom migration is less of an issue.

the 2009 survey (amounting to about two-thirds of the average of the rest of the sample).[6]

2.1.2 PROGRESA Exposure Variable

One of our main independent variables of interest is years of PROGRESA exposure. Due to the features of the policy and rollout described above, the length of exposure to the education component of the PROGRESA program depends on a child's locality and birth year. Table D3 shows, for each birth cohort, the number of years of exposure to PROGRESA by treatment status. We obtain this by first 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 (while they should have started in May 1998, they appear to have been initiated earlier for some (Skoufias, 2005) and later for others (Hoddinott and Skoufias, 2004)). Years of exposure to PROGRESA varies across treatment and control villages and also across ages within village type.

For the majority of cohorts, the difference between treatment and control exposure is 2 years, but the difference is only 1 year for the youngest cohort with any differential exposure at all (who aged into the program) and the oldest cohort with differential exposure (because the control group aged out at the end of 1999 and started receiving benefits when the program was expanded to include high school in 2001). Creating a continuous years of exposure 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. In robustness checks, we explore different variants of this PROGRESA variable: we use a simple treatment dummy, as well as a years of exposure variable that is not rounded to the nearest year.

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). 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.

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

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.

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 instead of an absolute measure of rainfall in order to capture the fact that the same amount of rainfall may have different consequences for different regions based on 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. Importantly, defining the shock based on comparisons to *locality-specific* means ensures that we are not simply comparing areas that typically get a lot of rainfall (or very little rainfall) to areas with more moderate rainfall, as these areas could be substantially different on a number of dimensions. Instead, identification relies on comparing locality-years that received a lot more (or a lot less) rainfall than is typical for that specific locality, to locality-years that received close to that locality's average amount of rainfall.

In our analysis, we use a dummy equal to 1 if the rainfall in an individual's locality during their year of birth was greater than a standard deviation above or below the locality-specific historical mean. Although we use this parsimonious specification in our main results, we also show that our conclusions are robust to a more flexible treatment of rainfall that allows for asymmetric effects across floods and droughts. In this specification, asymmetries do not appear to be significant (see Table C11).

We use rainfall in an individual's calendar year of birth in their locality of residence in 1997.[7] To calculate rainfall levels, we simply sum all monthly rainfall during an individual's calendar year

[7]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.

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.

Figure 1: PROGRESA Localities by Treatment Status and Rainfall Shock in 1987

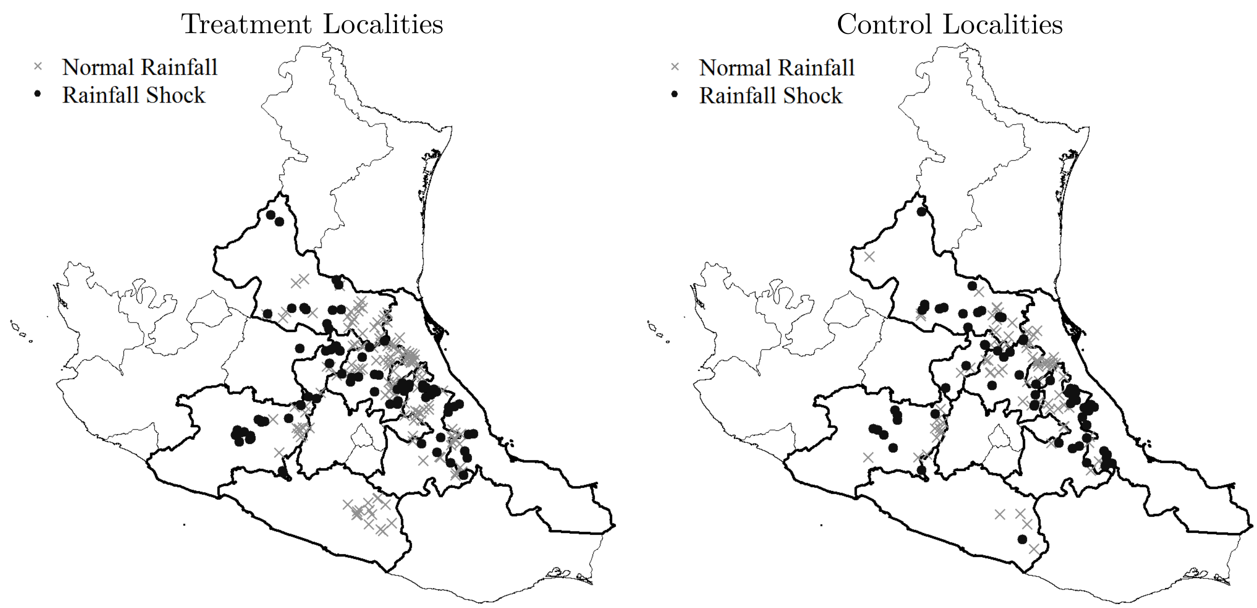


Figure 1 maps all PROGRESA localities by their rainfall status, separately for treatment and control. Black dots represent localities that experienced a rainfall shock in 1987 (chosen, for illustrative purposes, because this is the modal birth year in our sample), while gray crosses represent those that 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 2 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. All education measures are significantly higher for treatment than control villages, but employment outcomes for 18 year olds do not differ by treatment status on average. In section 3,

Table 2: Summary Statistics for Individual-Level Variables in 2003

	Full Sample	*Treatment Villages*	*Control Villages*	*Treatment - Control Differences*
12 to 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	0.46	0.48	0.44	0.037***
Completion	(0.50)	(0.50)	(0.50)	(0.0094)
Number of individuals	11829	7193	4636	
Number of localities	420	257	163	
18-year-olds				
Currently Enrolled w/	0.061	0.058	0.064	-0.0057
HS Degree	(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-Laborer	0.35	0.36	0.35	0.0051
Job	(0.48)	(0.48)	(0.48)	(0.029)
Number of individuals	1597	942	655	
Number of localities	368	218	150	

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.

we outline how we analyze these differences using regressions that control for covariates and take into account heterogeneous impacts for individuals with different early-life circumstances.

Table 3: Summary Statistics for Shock Variables

	Full Sample	*Treatment Villages*	*Control Villages*	*Treatment - Control Differences*
A. Full Sample				
Years of PROGRESA	4.84	5.57	3.69	1.88***
exposure	(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)
Normalized 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	
B. Trimmed Sample				
Years of PROGRESA	4.81	5.58	3.71	1.87***
exposure	(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)
Normalized 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.

Panel A of Table 3 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 normalized 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 is 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 D2 that the rainfall distributions for treatment and control localities indeed share a common support and are actually quite similar overall. Moreover, in Figure 1, 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 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, noting that such localities would not contribute to coefficient estimates. As shown in Panel B of Table 3, this trimming results in a sample of balanced rainfall shocks across treatment and control. Appendix Figure D3, which maps the geographic distribution of shocks for this trimmed sample, is not noticeably different from Figure 1, emphasizing 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 C4).

Despite the randomized nature of the PROGRESA experiment, previous literature has found that some household-level and locality-level characteristics are not fully balanced across treatment and control villages (Behrman and Todd, 1999). For this reason, in keeping with empirical methods used in previous studies of PROGRESA impacts, we include a rich set of controls that are summarized in Appendix Table D4, which shows a few variables that are unbalanced across treatment and control (household head age, several household composition variables, two parental education variables, father's language, access to a public water network, and garbage disposal techniques).

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 randomized rollout of PROGRESA provides the latter. For the former, we turn to variation generated by rainfall shocks.

3.1 Early-Life Rainfall Shocks

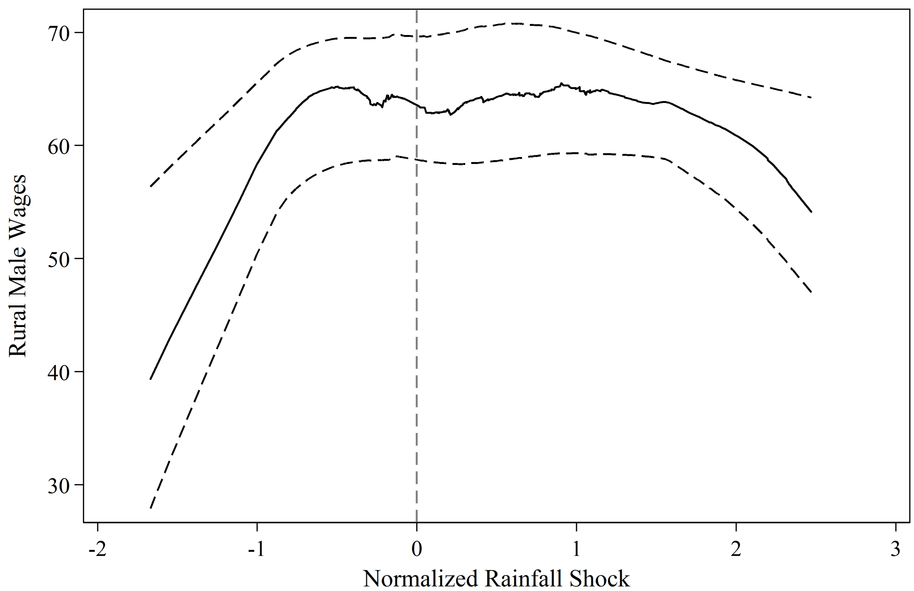
In rural settings, good rainfall in early childhood means higher income, which may translate into increased nutritional availability during a crucial stage of development. Children exposed to negative rainfall shocks early in life often remain disadvantaged many years later, in terms of their health, human capital, and labor 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. Figure 2 depicts the lowess-smoothed relationship between average male wages from the 2003 survey and rainfall in that same year, normalized 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 (among the sample of individuals whose height and weight were measured in the 2003 and 2007 PROGRESA surveys). 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.

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

Figure 2: Locality Wages



Notes:
Solid line represents the lowess-smoothed relationship between rural male wages and normalized rainfall. Dashed lines represent 95% confidence intervals, calculated from 1000 bootstrapped samples.

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).

Finally, we also examine whether other dimensions of human capital are affected by birth-year rainfall, focusing on cognitive test scores and behavioral measures collected in 2003. We find that adverse birth-year rainfall had no significant effects on cognitive or behavioral measures for 2 to 6 year-olds, but did increase the likelihood of behavioral problems (externalizing 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 behavioral problems later in childhood (Goyal et al., 2010). 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.

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 difficult 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 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. In the next sub-section, we describe the regression specification used to answer these questions.

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 section E of the Appendix 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. The randomized rollout of PROGRESA, as well as differences in child ages within villages, is what generates variation in this variable.

Our basic specification includes state x birth year fixed effects (μ_s x δ_t). In some specifications we add municipality 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). As discussed in section 2.2.1, using a rainfall shock dummy instead of rainfall levels reduces the spatial correlation in our independent variable of interest, but these methods correct for any spatial correlation that may remain, using a weighting function that allows for dependence between observations located within a specified distance of each other. 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. All of our specifications include controls for individual gender, household size, household head age, household head gender, household composition variables (listed in Table D4), 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. We also include dummies for missing control variables (parental education, parental language, distance to secondary school, and distance to bank), in order to avoid dropping individuals who are missing these variables. In Appendix Table C6, we show specifications that include interactions between the rainfall shock and each of the characteristics that are not balanced across treatment and control (similar to to the strategy used in Acemoglu et al. (2004)).

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 Table D5, 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. Across a total of 120 coefficients, only 14 are significant at the 10 percent level (and only 7 at the 5 percent level), which is approximately the number of significant coefficients we would expect to see by chance. Importantly, the vast majority of coefficients are small in magnitude relative to the means.[8]

Another question related to the interaction of the two shock variables is whether rainfall shocks in an individual's year of birth could affect the likelihood of that individual being eligible for PROGRESA, by affecting their household's long-run income-generating capabilities, for example. As we show in the first column of Appendix Table D6, we do not find any significant differences in the likelihood of being categorized as poor (and therefore eligible for PROGRESA) across individuals born during rainfall shock years compared to normal years.

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 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 behavioral 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 generalizability 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 labor 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 D7, 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

[8] One exception is age, but as we discuss in section C.4 and show in Table C7, these age imbalances do not appear to be driving our main results.

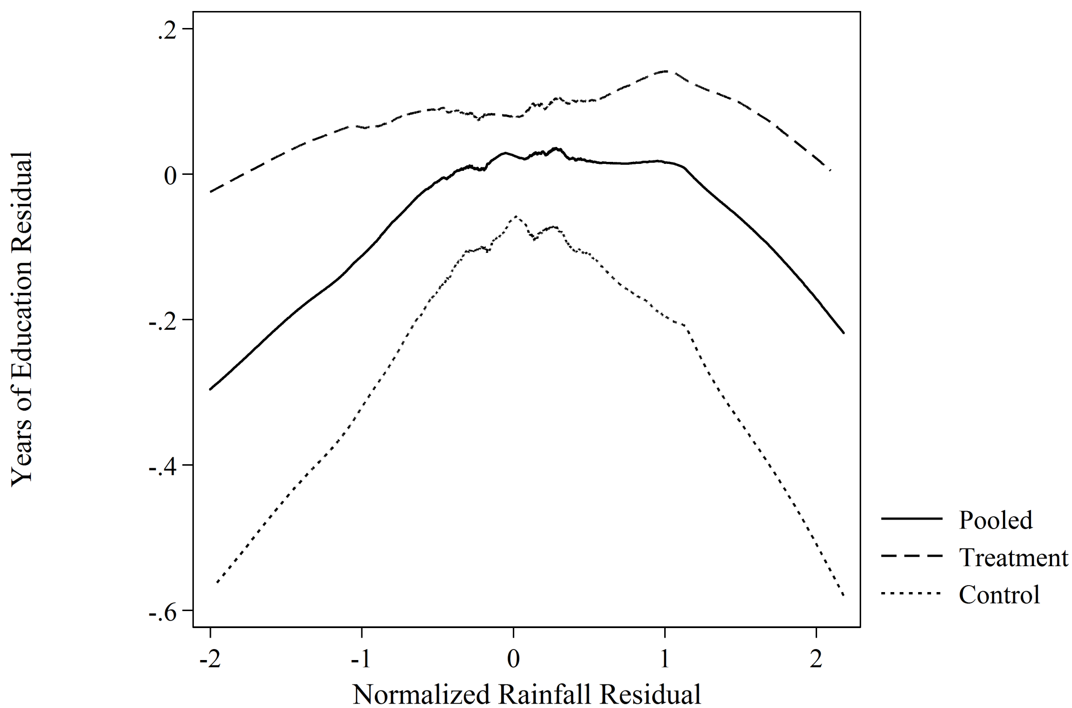
are able to observe.

4 Results

In this section, we report estimation results from the strategy discussed above, beginning with a graphical illustration of our educational attainment results. We then discuss possible mechanisms driving our findings, followed by robustness checks to address various threats to internal validity.

4.1 Education Results

Figure 3: 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 normalized rainfall residuals are calculated after regressing each variable on state by birth-year fixed effects and the control variables described in section 3. Normalized rainfall residuals are trimmed at the 5th and 95th percentiles.

Figure 3 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 normalized 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 normalized rainfall residuals on the x axis. The solid line represents the relationship for the pooled sample, including both treatment and control villages, which had varying degrees of exposure to the PROGRESA experiment.

We also examine the same education-rainfall relationships separately for treatment and control villages. The control group has an inverted U- shape, which reinforces the idea that extreme deviations from mean rainfall are harmful for children. Comparing the dotted control group line to the dashed treatment line, the treatment line is above the control line across the entire range of rainfall deviations. Consistent with our summary statistics and previous work on PROGRESA, education outcomes are improved for those exposed longer to PROGRESA. Second, the distance between the treatment and control lines is smallest around a normalized 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.

The following tables report parametric regression estimates analogous to the graphical analysis above. Before discussing the results of equation 1, we report in Panel A of Table 4 the results of regressions that include only the main effects of rainfall and PROGRESA exposure. The first three columns show the regression results from our base specification, 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.

The results in column 1 show that one year of PROGRESA exposure leads individuals to complete 0.13 more grades of schooling on average: this effect is significant at the 5% level. Multiplying this coefficient by 1.5 years (the number of years between the treatment and control villages' first exposure to PROGRESA), we obtain a treatment effect of 0.2 years, which is consistent with previous work by Behrman et al. (2009, 2011). We argue the main reason for this result is the conditional component of the cash transfer, as opposed to a general income effect: as mentioned in section 1.2, consumption effects of PROGRESA were generally small in magnitude (Parker et al., 2017). Children who were exposed to the PROGRESA program earlier (due to living in a treatment village or being of eligible age when the program was introduced) were more likely to stay enrolled and less likely to repeat grades in the initial years of their PROGRESA exposure, leading to an accumulation of more grades of schooling by the time outcomes were measured in 2003.[9]

Individuals who experienced a negative income shock at birth show a reduction in educational attainment of 0.10 years, significant at the 10% level. This is slightly smaller than the negative effect of early-life exposure to political violence in Peru – estimated to be 0.3 years of schooling (Leon, 2012) – and the positive effect of *in utero* exposure to iodine, an important micronutrient, in Tanzania – estimated to be around 0.4 years (Field et al., 2009). Since our sample includes children who may not have completed their 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 the specification with municipality fixed effects, none of the main effects are significant at the 5% level. These results, however, do not allow PROGRESA to have heterogeneous impacts on individuals with different early-life experiences.

We allow for this in Panel B of Table 4 which displays the results from equation 1. Again, columns 1 to 3 show the results with the baseline set of controls, while columns 4 to 6 add the municipality fixed effects. As above, we report three sets of standard errors, which are generally quite similar.

For educational attainment in column 1, the main effect of PROGRESA – which now represents the effect of PROGRESA for those who were not exposed to a rainfall shock – is positive, the main effect of a rainfall shock is negative, and the interaction is positive; all are significant at the 5% level (10% level when using the 500km Conley standard errors). Interestingly, the coefficients on the rainfall shock variable, which represent the effects of adverse rainfall on someone with no exposure to PROGRESA, are much larger than the estimates in Panel A. In short, adverse rainfall was most harmful for those who did not have access to the PROGRESA program in later years.

The same pattern holds for grade progression and appropriate grade completion. In columns 2 and 3, the rainfall shock coefficients 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 sum of the

[9]Though PROGRESA effects on education outcomes are documented to have been smaller for younger children, the program did still affect those in primary school: Schultz (2004) documents significant effects on enrollment starting with children who had completed at least 4 grades of school, while (Behrman et al., 2005) detect significant effects on grade progression and grade repetition across all primary school ages.

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
Panel A: Main Effects Only						
Years of PROGRESA Exposure	**0.13**	**0.015**	**0.017**	**0.042**	**-0.0082**	**-0.0061**
	(0.037)***	(0.0096)	(0.0074)**	(0.046)	(0.012)	(0.011)
	[0.026]***	[0.0063]**	[0.0064]***	[0.033]	[0.0085]	[0.0082]
	{0.021}***	{0.0054}***	{0.0066}**	{0.031}	{0.0077}	{0.0092}
Rainfall Shock	**-0.10**	**-0.012**	**-0.027**	**-0.066**	**0.00075**	**-0.021**
	(0.056)*	(0.014)	(0.012)**	(0.054)	(0.014)	(0.011)*
	[0.062]*	[0.015]	[0.013]**	[0.050]	[0.012]	[0.012]*
	{0.068}	{0.015}	{0.015}*	{0.049}	{0.012}	{0.012}*
Panel B: Main Effects and Interaction						
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.

PROGRESA coefficient and the interaction term are positive and statistically significant, indicating large PROGRESA effects for those exposed to early-life disadvantage.

These coefficient estimates imply that PROGRESA was able to generate substantial catch-up for individuals exposed to adverse rainfall. A negative rainfall shock decreases educational attainment by 0.65 years (in column 1). However, one year of PROGRESA exposure mitigates this reduction by 0.11 years. Put differently, 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. In short, PROGRESA narrowed the gap generated by adverse early-life rainfall.

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 coefficient and the sum of the coefficient and interaction) are close to zero, likely due to lack of variation in treatment and control status within municipalities. Although municipality fixed effects are appealing in the sense that they 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 (reported in columns 1 through 3) for the remainder of the paper.

The large magnitudes of the interaction terms in all regressions suggests a large potential for policy interventions like PROGRESA to remediate inequalities in early-life disadvantage. 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).

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. We start with 3 years of school because this is the youngest grade directly affected by the conditional cash transfers. In each column, we restrict the sample to individuals old enough to have completed the number of grades used in the dependent variable, which means smaller samples starting in column 5.

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.

In columns 1 to 8, the impact of PROGRESA on completing grades 2 to 10 is positive and significant, though the coefficient magnitudes are very small (especially relative to the means) up until the beginning of secondary school. The size of this main effect is largest in magnitude for the 7th grade of schooling, which Behrman et al. (2011) highlight as a critical transition period (between primary and secondary school) during which many children drop out. Previous literature has documented that the positive effects of PROGRESA on educational outcomes are largest for children who were in late primary school when the program began (Behrman et al., 2009; Schultz, 2004), which is consistent with these by-grade results. Taken together, these suggest that our main results in Table 4 are being driven by the older children in the sample.

The main 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), we see significant positive interaction coefficients that reveal the potential for interventions to mitigate the effects of early life shocks by encouraging the completion of secondary schooling among those hit by these shocks. 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.

Although we are also interested in whether cognitive ability, not just educational attainment, was impacted by PROGRESA and birth-year rainfall, the evidence on this is somewhat inconclusive. Woodcock-Johnson dictation, word identification, and applied problems tests were administered to a sub-sample of individuals aged 15 to 21, as part of the 2003 survey. Unfortunately, treatment status is significantly negatively related to the probability of an individual having a non-missing test score, and our main schooling results restricted to this sub-sample reveal smaller and imprecisely estimated coefficients, casting doubt on whether it is representative of our population of interest. Another issue is that the tests may have been unable to capture sufficient variation in cognitive ability: in the letter-word identification test, for example, almost 30% of the sample answered everything correctly (and over 50% only made 2 mistakes) in a test of 58 questions. With these caveats in mind, Appendix Table D8 reveals no significant effects of PROGRESA, rainfall, or their interaction on these test scores, consistent with previous work documenting a null main effect of PROGRESA (Behrman et al., 2009).

4.2 Employment Outcomes

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-Laborer Job	Enrolled or Currently Working	Enrolled or Worked this Year	Enrolled or Worked in Non-Laborer 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.

We are also interested in whether rainfall shocks and PROGRESA exposure have similar effects on longer-run labor 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 Figure D1) and therefore missing detailed employment information, but as we show in column 4 of Table C2, the likelihood of a missing employment variable in this sample is not driven by PROGRESA, rainfall, or their interaction.[10] In Table 6, we report the results of regressions on variables related to continuing

[10]The fraction living outside of the household grows even higher after age 18, which is why we do not examine

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 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-laborer job; that is, those who were working as spot laborers were grouped in the same category as the unemployed. In the last 3 columns, we take the stance that both continued enrollment and employment are "desirable" outcomes, and create dummies that combine the continued enrollment variable with each of our employment variables. 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.

In columns 4 and 7, for example, we show that adverse rainfall significantly decreases the probability of an individual being employed in a non-laborer (with higher earning potential) job. While the effect of PROGRESA is essentially zero for children who did not experience adverse rainfall, the effect of PROGRESA is positive and significant for individuals who experienced negative rainfall shocks. That is, PROGRESA has significant impacts on the probability of stable employment immediately following high school completion among disadvantaged children, but no impact on the rest of the sample. 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 incentivized by the program.

those older than 18 in 2003.

4.3 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, as reported above, Table D7 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 Table D9 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 section E and summarize the main implications in this section.

4.3.1 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 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.[11]

We assume that rainfall shocks at birth do not affect current household income. This is supported by the evidence in Table D6, 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 correlated with current household income.

In Table D10, 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 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 Table D10 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,

[11] 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.

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.

4.3.2 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, $u\left(c\left(t\right)\right)$, and the effort cost for the t-th year of schooling, $\phi\left(\omega, t\right)$. As we show in 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 as 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 second derivative of the earnings function with respect to schooling is decreasing in the initial endowment, this would also contribute to remediation.

4.4 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 C1) or attrition (Table C2). Rainfall shocks do not appear to be correlated with various migration-related outcomes (Table C3). Our results are robust to the use of trimmed and re-weighted samples that address the imbalance in the rainfall shock variable across treatment and control (Tables C4 and C5), as well as specifications that address the imbalances in other characteristics, including age (Tables C6 and C7).

In addition, we show that our results are robust to the inclusion of controls for other government programs (Table C8). 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 C9) . We also find similar results when we change the time period used to calculate historical rainfall in order to construct our rainfall shocks (Table C10). In Table C11, 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 effects on rainfall in the year of birth. All of these tables are discussed in more detail in section C.

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.

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ONLINE APPENDIX (Not for Publication)

A Additional Program Details

A.1 Health Component

The health and nutrition component of the program involved conditional cash transfers intended to incentivize healthy behaviors. For instance, in order for a household to receive a cash grant for food, all members were required to visit the health facility a specified number of times per year and to attend nutrition and health education lectures. The required number of visits varied by age and gender, with pregnant women and infants required to go every 1-3 months, while anyone aged 5 and older only required to attend 1 or 2 times per year. The program also provided nutrition supplements and other preventative care for pregnant and lactating mothers and young children, and supported the improved provision of primary health care services in PROGRESA localities.

A.2 Program Targeting

PROGRESA was targeted toward poor households in poor localities. To determine which localities would receive the program, a set of marginalized localities was identified using data from the 1990 and 1995 censuses. Within these selected localities, household-level eligibility for PROGRESA was determined based on the results of an income survey administered to all households in each locality. First, household per capita income (excluding child income) was calculated, and households were categorized as above or below a poverty line. Then, separately for each region, the program identified the household characteristics that were the best predictors of poverty status, which were then used to construct the index that ultimately classified households as poor (eligible for PROGRESA) or nonpoor.[1]

[1] Skoufias et al. (2001) contains more details about the selection of localities and households.

B Rainfall Shocks and Health

In this section, we discuss evidence on the effects of rainfall shocks on BMI, stunting, cognitive test scores, and behavioral measures, using various age cohorts from the 2003 and 2007 surveys.

To study effects on BMI, we pool all individuals for whom BMI was measured across the 2003 and 2007 surveys.[2] For each individual, we calculate gender- and age-specific BMI z-scores using WHO tables,[3] and regress this variable on a rainfall shock dummy (more than one standard deviation from the locality-specific historical mean) in the individual's locality of residence in the relevant survey year (controlling for state-by-survey-year fixed effects and a host of other individual and household-level controls, described in the table notes). In column 1 of Table B1, we see that adverse rainfall in the survey year has negative effects on BMI for the entire sample. This supports the idea that the lower wages and expenditures that result from bad rainfall also translate into lower nutritional intake. In column 2, we show that this result persists (and is much larger) for children under two years old, for whom these measurements are a closer proxy to their initial health endowment. Because BMI is not a conventional measure for young children, we also explore weight-for-length and weight-for-age z-scores in columns 3 and 4. We see that rainfall shocks have a significant negative effect on weight-for-length. In sum, this provides us with evidence that rainfall around the time of birth affects the nutritional intake and therefore BMI of infants.

We next ask whether these contemporaneous nutrition effects have longer-term implications for child health. To answer this question, we use height data, collected for children aged 0-2 in 2007, aged 2-6 in 2003, and aged 8-10 in 2007. We calculate age- and gender-specific height z-scores (once again using WHO tables) and create an indicator for stunted children, with heights falling more than 2 standard deviations below their group-specific mean. We then regress this indicator on the rainfall shock variable that we use in our main analysis. In columns 6 and 7 of Table B1, we see that there is a significant positive relationship between

[2] Specifically, height and weight were measured for sub-samples of children aged 2 to 6 in 2003, adolescents aged 15 to 21 in 2003, infants aged 0 to 2 in 2007, children aged 8 to 10 in 2007, and adults aged 30 and older in 2007.

[3] We use the means and standard deviations for 20-year-olds, the oldest available age category, for all older adults.

bad rainfall and stunting for children aged 2 and older. In other words, year of birth rainfall shocks have physical health effects that persist into early childhood.

Table B1: Effect of Rainfall on Weight and Height Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	BMI z-score	BMI z-score	Weight-for-length z-score	Weight-for-age z-score	Stunted	Stunted	Stunted
Rainfall Shock	-0.058	-0.16	-0.19	0.067			
(in survey year)	(0.033)*	(0.082)*	(0.098)*	(0.099)			
Rainfall Shock					-0.00056	0.042	0.037
(in birth year)					(0.026)	(0.019)**	(0.019)**
Observations	9596	1184	1184	1187	1243	1978	1426
Mean of Dependent Variable	0.56	0.55	-0.38	0.26	0.19	0.22	0.087
Sample Ages (in Survey Year)	All	0-1	0-1	0-1	0-2	2-6	8-10
Survey year(s)	2003; 2007	2007	2007	2007	2007	2003	2007
Fixed Effects	Birth year, state, survey year x state				Birth year x state		

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Rainfall shock" = 1 for individuals whose survey year or 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. For adults in column 1, all parental variables are missing.
-Column 1 includes all individuals whose height and weight were measured in either 2003 or 2007: 0-2 year-olds in 2007, 2-6 year-olds in 2003, 8-10 year-olds in 2007, 15-21 year-olds in 2003, adults 30 and older and mothers of young children in 2007.

Taking advantage of other measures of child development collected in 2003 (for 2-6 year-olds) and 2007 (for 8-10 year olds), we also explore whether other dimensions of human capital – cognitive and non-cognitive skills – are affected by birth-year rainfall. During the 2003 surveys, a number of cognitive development tests (Woodcock Johnson tests, Peabody Picture Vocabulary tests, and MacArthur communication tests) were administered to a sample of 2-6 year-olds. In addition, mothers were asked to rate their children's behaviors using the Achenbach Child Behavior Checklist. For cognitive measures, we calculate z-scores for each of the cognitive tests and take the mean across all cognitive z-scores. For the Achenbach checklist, we create a z-score after summing the responses to all checklist questions. In 2007, mothers of children aged 8-10 answered the Strengths and Difficulties Questionnaire (SDQ), a list of questions about the behaviors of their children. Using existing recommended methods for scoring and grouping questions, we create z-scores for externalizing problems, internalizing problems, and anti-social problems (and an overall z-score that averages all three).

Results are reported in Table B2, where we use the cognitive z-score (from 2003), the behavioral z-score (from 2003), and multiple behavioral z-scores (from 2007) as our dependent variables, and run regressions identical to the ones in Table B1. We find that birth-year rainfall had no significant effects on cognitive or behavioral measures for 2 to 6 year-olds, but did increase the likelihood of behavioral problems (externalizing 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 behavioral problems later in childhood (Goyal et al., 2010).

Table B2: Effect of Birth-Year Rainfall on Cognitive and Behavioral Outcomes in Childhood

	(1)	(2)	(3)	(4)	(5)	(6)
			Behavioral problems z-scores			
	Cognitive measure z-score	Personality measures z-score	Overall	Externalizing problems	Internalizing problems	Social problems
Rainfall Shock (in birth year)	0.015 (0.039)	0.0020 (0.023)	0.083 (0.050)*	0.12 (0.065)*	0.017 (0.067)	0.11 (0.071)
Observations	2032	2014	1488	1488	1488	1488
Mean of Dependent Variable	-0.052	0.034	-0.013	0.014	-0.0084	-0.044
Sample Ages (in Survey Year)	2-6	2-6	8-10	8-10	8-10	8-10
Survey year	2003	2003	2007	2007	2007	2007
Fixed Effects	Birth year x state					

Notes:

- Standard errors clustered at the municipality level are in parentheses (*** 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

C Robustness Checks

C.1 Selective Fertility

Table C1: Effects of PROGRESA and Birth-Year Rainfall on Fertility

	(1)	(2)	(3)	(4)	(5)	(6)
	Locality-Level	Individual-Level				
	Total number of children	Number of younger siblings	Birth spacing (in days) between younger sibling	Mother's ideal number of children	Mother wants more children	Number of additional children desired
Years of PROGRESA Exposure		0.0026	-7.42	-0.14	0.018	-0.027
		(0.0085)	(10.1)	(0.12)	(0.011)	(0.026)
Rainfall Shock	-0.090	-0.10	29.1	-0.067	0.089	0.18
	(0.16)	(0.066)	(80.0)	(0.52)	(0.074)	(0.17)
Rainfall Shock x Exposure		0.020	-6.37	0.018	-0.022	-0.046
		(0.013)	(15.5)	(0.10)	(0.015)	(0.034)
Observations	2519	11686	7230	2057	2027	2091
Mean of Dependent Variable	4.83	1.98	1107.9	4.34	0.091	0.076
Sample	Cohorts aged 12-18 in 2003	Individuals aged 12-18 in 2003				
Fixed Effects	Birth year x state	Birth year x state				

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
- For locality-level analysis, the unit of observation is birth-year-locality.
-All specifications include locality controls and individual/household characteristics (gender, household head gender and age, household size, household composition, parental education and language). For the locality-level variables, these are averaged at the locality-birth-year level.
-"Rainfall shock" = 1 for individuals whose birth-year rainfall was more than one standard deviation from the 10-year historical locality-specific mean.
-The dependent variables in columns 4 to 6 are only available for a random subset of mothers.

In Table C1 we investigate how PROGRESA and rainfall shocks may have affected fertility, which could lead to potential selection issues. One concern might be that negative rainfall shocks during a year may affect the number of children that are born and/or survive to school-aged years. If this were the case, the composition of individuals in our sample who were born in shock years would be different from those in our sample born in regular years. In order to check this, we collapse to the locality by birth year level and count the total number of children born in a particular year in each locality. We then use this constructed panel to regress the total number of children born that year on our rainfall shock. Column 1 of Table C1 reports results from this regression. We find no evidence of selective fertility or selective child mortality.

Our next test is to check whether PROGRESA, rainfall shocks, and their interaction had any impact on mothers' subsequent fertility decisions. Specifically, we might be concerned that a good rainfall shock would increase the likelihood of having more children (or total fertility), or decrease the birth spacing between children, just as exposure to PROGRESA may do the same (by lowering the opportunity cost of having children). If this were the case, an individual's exposure to PROGRESA or rainfall shocks would also be related to intrahousehold allocation issues that may vary with the total number of siblings and spacing between siblings. To check for this, we estimate equation 1, again at the individual level, using number of younger siblings and birth spacing between next youngest sibling (in days) as dependent variables. The main effects and interaction effects in columns 2 and 3 are all insignificant.

In addition to investigating effects on actual fertility, we also ask whether PROGRESA or rainfall affected planned or expected fertility. To answer this question, we use questions on expected and desired fertility for the mothers of our sample children who were part of a detailed fertility questionnaire sub-sample. We do not find that rainfall shocks (or PROGRESA) affected the total number of desired children or the desire for additional children, as we show in the last three columns of Table C1.

C.2 Attrition

As in any longitudinal study, we must consider the extent to which selective attrition may be confounding our results. In Table C2, we show that although attrition between the baseline and 2003 surveys was sizeable, it appears to be uncorrelated with our regressors of interest. In this table, we simply regress various attrition indicators on years of PROGRESA exposure, the rainfall shock indicator, their interaction, and state by birth year fixed effects. In column 1, we investigate household attrition, including all eligible individuals in the baseline survey who would have been aged 12 to 18 in 2003. We do not find that our investment or endowment shocks influenced the likelihood of a household being dropped from the 2003 sample. In column 2, conditional on the household being found in 2003, we show that our regressors of interest do not significantly predict the likelihood of an individual being included in our sample given the data quality restrictions we impose (matching genders across surveys and birth year differences of less than 2 years). Finally, in columns 3 and 4, we investigate

Table C2: Effects of PROGRESA and Birth-Year Rainfall on Attrition

	(1)	(2)	(3)	(4)
	Household found in 2003	Meets Data Quality Restrictions	Non-missing education variable	Non-missing employment variable
Years of PROGRESA Exposure	0.0024	0.0015	0.0035	0.0065
	(0.0083)	(0.0032)	(0.0030)	(0.014)
Rainfall Shock	0.034	0.030	-0.0017	0.13
	(0.038)	(0.023)	(0.019)	(0.14)
Rainfall Shock x Exposure	-0.0065	-0.0049	-0.000028	-0.039
	(0.0076)	(0.0045)	(0.0037)	(0.040)
Observations	14525	12917	12159	1646
Mean of Dependent Variable	0.89	0.94	0.97	0.70
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	18
Fixed Effects	Birth year x state			

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** 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.
- The sample in column 2 restricts to households found in 2003, while columns 3 and 4 restrict to those that meet data quality restrictions.

whether the shocks predict the probability that an individual – who is found in 2003 and meets the data quality restrictions – has non-missing education and employment variables (restricting to 18-year-olds in column 4). We do not find any evidence of either.

C.3 Migration

In addition to selective fertility and attrition, selective migration in response to rainfall shocks could also be a concern. In particular, we might worry that permanent household-level migration responds to rainfall shocks, which would mean that year-of-birth rainfall shocks might affect the probability of an individual showing up in our PROGRESA localities in the first place. Unfortunately, we cannot study the migration behavior of households that never made it into our sample. What we can do is check whether rainfall shocks in 1997 affect the probability of a household participating in the 2003 survey: this six-year gap between the shock and migration outcome would capture more delayed migration responses, similar to what might have occurred for individuals whose household migrated before they started

Table C3: Effects of Rainfall on Migration-Related Variables

	(1)	(2)	(3)	(4)
	Household found in 2003	Household found in 2003	Father living in household in 1997	Father living in household in 1997
Rainfall Shock (in 1997)	0.0047	0.021		
	(0.014)	(0.016)		
Rainfall Shock (in birth year)			-0.0025	-0.0059
			(0.0070)	(0.0074)
Observations	6684	6684	12156	12156
Mean of Dependent Variable	0.88	0.88	0.91	0.91
Sample	Households with children aged 12-18 in 2003		Individuals aged 12-18 in 2003	
Fixed Effects	None	Birth year x state	None	Birth year x state

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** 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.
-Columns 1 and 2 are household-level regressions

school. Although migration is not the only reason a household could be missing from the sample in 2003, it is likely the main one, and as we show in columns 1 and 2 of Table C3, we find no effects of 1997 rainfall shocks on this migration-related outcome.

Another potential issue is that rainfall shocks during an individual's year of birth might affect the temporary (rather than permanent) migration decisions of their parents. If a child grows up without a father in the household as a result, this could generate effects on their development separate from the mechanisms we have focused on in this paper. Therefore, in Table C3, we check to see whether rainfall shocks at birth affect the likelihood of a child's father being present in the household during the 1997 survey (the survey closest to the time of birth), and find no evidence of this (columns 3 and 4).

C.4 Balance

We investigate further the implications of the small but statistically significant imbalance in rainfall shock prevalence across PROGRESA treatment and control villages in our baseline sample. First, to test whether our results are being driven by this imbalance, we repeat our analysis using the trimmed sample described in Section 2, in which rainfall shock prevalence is the same across treatment and control villages. This sample omits localities exhibiting shocks in every year, or no shocks in any year, over the study period. As Table C4 shows, our results are virtually identical to the full sample results.

Table C4: Effects of PROGRESA and Birth-Year Rainfall on Education and Employment Outcomes: Trimmed Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.12	0.0099	0.014	0.028	0.017	0.023	-0.0060	-0.021	-0.019
	(0.043)***	(0.011)	(0.0087)	(0.012)**	(0.012)	(0.013)*	(0.0090)	(0.016)	(0.015)
Rainfall Shock	-0.71	-0.12	-0.14	-0.21	-0.25	-0.27	-0.096	-0.32	-0.35
	(0.29)**	(0.058)**	(0.053)**	(0.071)***	(0.072)***	(0.085)***	(0.056)*	(0.13)**	(0.15)**
Rainfall Shock x Exposure	0.12	0.022	0.021	0.033	0.043	0.048	0.016	0.12	0.12
	(0.056)**	(0.011)*	(0.011)**	(0.014)**	(0.013)***	(0.016)***	(0.018)	(0.041)***	(0.046)***
Observations	10236	9713	10236	8689	7160	5684	1320	966	962
Mean of Dependent Variable	6.78	0.59	0.47	0.56	0.51	0.45	0.065	0.35	0.41
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality are reported in parentheses (*** 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

Another way to approach this issue is by comparing our original estimates to results obtained from a weighted regression, where we use inverse probability weighting to re-weight the observations so that the distribution of rainfall shocks is the same across treatment and control groups. As shown in Table C5, the point estimates are almost identical to the results in Tables 4, 5, and 6, suggesting that the statistically significant imbalance was too small in magnitude to substantially affect our original estimates.

We also conduct a robustness exercise regarding the unbalanced demographic characteristics across treatment and control villages in Table D4, which we discuss in section 2. Table C6 reports the results of regressions on our main outcomes of interest, additionally controlling for interactions between the rainfall shock variable and each of the control variables that are not balanced across treatment and control groups. The results are once again very similar to the main results reported above.

Finally, we address the imbalance in age across treatment villages and rainfall shock

Table C5: Effects of PROGRESA and Birth-Year Rainfall on Education and Employment Outcomes, Re-weighted on Shock Probability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.10	0.010	0.012	0.023	0.014	0.019	-0.0049	-0.014	-0.014
	(0.038)***	(0.0097)	(0.0076)	(0.011)**	(0.011)	(0.011)*	(0.0077)	(0.016)	(0.015)
Rainfall Shock	-0.65	-0.11	-0.12	-0.20	-0.23	-0.25	-0.10	-0.22	-0.26
	(0.28)**	(0.056)*	(0.051)**	(0.070)***	(0.071)***	(0.083)***	(0.053)*	(0.13)*	(0.13)*
Rainfall Shock x Exposure	0.11	0.020	0.019	0.032	0.040	0.047	0.018	0.099	0.10
	(0.053)**	(0.011)*	(0.010)*	(0.014)**	(0.013)***	(0.016)***	(0.017)	(0.040)**	(0.042)**
Observations	11824	11216	11824	10068	8285	6618	1597	1143	1138
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** 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
-All specifications are weighted to produce a sample that is balanced on rainfall shocks across treatment and control groups.

Table C6: Effects of PROGRESA and Birth-Year Rainfall on Education and Employment Outcomes, Controlling for Rainfall Shock Interactions with Unbalanced Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.11	0.010	0.013	0.023	0.014	0.020	-0.0045	-0.015	-0.015
	(0.038)***	(0.0098)	(0.0076)*	(0.011)**	(0.011)	(0.011)*	(0.0078)	(0.016)	(0.015)
Rainfall Shock	-0.51	-0.14	-0.070	-0.17	-0.16	-0.24	-0.20	-0.56	-0.63
	(0.36)	(0.084)*	(0.077)	(0.096)*	(0.10)	(0.12)**	(0.086)**	(0.36)	(0.38)*
Rainfall Shock x Exposure	0.11	0.019	0.019	0.032	0.039	0.041	0.027	0.096	0.11
	(0.054)**	(0.011)*	(0.010)*	(0.014)**	(0.013)***	(0.015)***	(0.018)	(0.052)*	(0.051)**
Observations	11824	11216	11824	10068	8285	6618	1597	1143	1138
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

- Standard errors clustered at the municipality level are in parentheses (*** 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
-All specifications include interactions between the rainfall shock variable and each of the control variables that are unbalanced across treatment and control villages (see Table D4).

groups, which was revealed by the balance checks conducted in Table D5. Because we include birth year fixed effects in our regressions, we are not concerned about these imbalances affecting the estimation of the PROGRESA and rainfall shock main effects. However, these imbalances could be affecting the estimation of the interaction effect, if there is any heterogeneity by age. To address this problem, we use inverse probability weighting to re-weight our sample so that the age distributions are balanced across treatment and control as well as across individuals born during normal and shock years. The results in Table C7, if anything, are stronger after using this weighting procedure, which suggests that our results were not a spurious consequence of this age imbalance.[4]

Table C7: Effects of PROGRESA and Birth-Year Rainfall on Education and Employment Outcomes, Re-weighted on Age

	(1)	(2)	(3)	(4)	(5)	(6)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades
Years of PROGRESA Exposure	0.10	0.011	0.015	0.022	0.014	0.019
	(0.038)***	(0.0097)	(0.0077)*	(0.011)**	(0.011)	(0.011)
Rainfall Shock	-0.93	-0.17	-0.13	-0.22	-0.24	-0.28
	(0.31)***	(0.052)***	(0.044)***	(0.071)***	(0.073)***	(0.088)***
Rainfall Shock x Exposure	0.16	0.030	0.019	0.035	0.042	0.050
	(0.059)***	(0.010)***	(0.0090)**	(0.014)**	(0.014)***	(0.017)***
Observations	11824	11216	11824	10068	8285	6618
Mean of Dependent Variable	6.76	0.57	0.46	0.56	0.51	0.44
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18
Fixed Effects	Birth year x state					

- Standard errors clustered at the municipality level are in parentheses (*** 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.
-All specifications are weighted to produce the same age distributions across the four groups defined by treatment status and rainfall type.

C.5 Other Programs

One potential threat to validity is the rollout of other programs during the period between the birth years of our sample individuals and our survey year, 2003. In particular, though we argue that the occurrence of a rainfall shock is random, it is possible that a rainfall shock in a given year affects the probability of a household or locality being the target of another

[4] We only report our educational outcomes here because the age-weighting is irrelevant for the employment outcomes that involve only one age cohort.

program in subsequent years. This of course is more of a concern in situations where localities are hit by repeated shocks, which are more likely to affect future agricultural activity than a single shock. To this end, the exercise conducted in Table C4 helps alleviate these concerns by showing that the exclusion of localities hit by multiple consecutive shocks does not affect our results. We also directly address this issue by controlling specifically for programs or reforms targeted to individuals based on agricultural activity.

The Program for Direct Assistance in Agriculture (PROCAMPO) was a cash transfer program introduced in 1994 in order to compensate for the anticipated negative effects of NAFTA on rural incomes (Sadoulet et al., 2001). Land use in 1993 was used to determine eligibility for the program as well as the size of all future payments: transfers were made per hectare of land that was used to grow at least one of the following crops: corn, beans, rice, wheat, sorghum, barley, soybeans, cotton, or cardamom. The 2003 survey asks whether anyone in the household receives PROCAMPO payments, and we use this as an additional control in our next set of regressions.

Table C8: Effects of PROGRESA and Birth-Year Rainfall on Education and Employment Outcomes, Controlling for Other Government Programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.099	0.010	0.011	0.022	0.013	0.017	-0.0036	-0.016	-0.015
	(0.039)**	(0.0097)	(0.0078)	(0.011)**	(0.011)	(0.012)	(0.0076)	(0.016)	(0.015)
Rainfall Shock	-0.64	-0.10	-0.12	-0.20	-0.22	-0.26	-0.082	-0.25	-0.26
	(0.28)**	(0.054)*	(0.050)**	(0.071)***	(0.073)***	(0.084)***	(0.047)*	(0.12)**	(0.12)**
Rainfall Shock x Exposure	0.11	0.019	0.019	0.031	0.040	0.048	0.013	0.11	0.10
	(0.054)**	(0.011)*	(0.010)*	(0.014)**	(0.013)***	(0.016)***	(0.015)	(0.037)***	(0.039)***
Observations	11734	11135	11734	9992	8225	6575	1587	1134	1131
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.060	0.35	0.41
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** 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
-All specifications control for household receipt of PROCAMPO cash transfers, indicators for corn, sugar, and kidney bean growing localities interacted with birth year dummies, and the individual's age in the year PROCEDE reached its locality (along with a dummy for individuals missing PROCEDE information, for whom the PROCEDE age variable is set to zero).

In general, the effects of the trade liberalization reforms that took place in the 1990's likely varied across localities, and one important source of variation in these effects were the types of crops grown in each village. Price changes as a result of trade liberalization were clearly crop-specific, as were the support policies implemented to protect farmers.[5] In short, an important concern is whether trends over time varied for localities growing different types of crops. To address this concern, we create indicators for whether a locality reports corn, kidney beans, or sugar as one of their top three crops, and interact these indicators with individual birth year dummies.

Finally, we also control for the rollout of a land certification program (PROCEDE) that essentially eliminated the link between land use and land rights in communally farmed agricultural communities called *ejidos*. PROCEDE has been found to have affected migration decisions (De Janvry et al., 2015) and therefore might have also affected the returns to and opportunity costs of schooling. We control for the age of an individual in the year their locality was certified to address concerns that correlations between PROCEDE's rollout and rainfall shocks might be confounding our estimates.[6]

Table C8 estimates our main regressions with the addition of several controls: an indicator for PROCAMPO recipients, crop variables interacted with birth year dummies, and individual age during PROCEDE rollout. Our results are robust to these adjustments.

C.6 Alternate Variable Definitions

We investigate the robustness of our results to other methods of defining our two main independent variables of interest. First, we show that our results are robust to replacing our PROGRESA exposure variable with a simple treatment village indicator (Table C9). The effects are slightly weaker for some outcomes, which indicates that the additional cohort-level variation linked to the schooling incentive is important; however, the pattern of results is preserved across all regressions.

[5]For example, import quotas for most traditional crops – except maize and beans – were eliminated in 1991. Similarly, although tariffs for most commodities were phased out by 2006, transitional tariffs for maize, dry edible beans, milk, and sugar were not scheduled to be phased out until 2008 (OECD, 2006).

[6]We obtain this data from De Janvry et al. (2015), which restricts attention to *ejidos* that were certified after 1996. Therefore, we are unable to distinguish between *ejido* localities certified in 1993, 1994, 1995, 1996, and localities that were not part of an *ejido* at all. For individuals in this category, we set the PROCEDE age variable to zero and include a dummy for missing PROCEDE information.

Table C9: Effects of PROGRESA and Birth-Year Rainfall, Using Treatment Village Dummy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Treatment Village	0.21	0.020	0.022	0.049	0.031	0.045	-0.0097	-0.029	-0.029
	(0.073)***	(0.019)	(0.015)	(0.022)**	(0.022)	(0.023)*	(0.016)	(0.031)	(0.030)
Rainfall Shock	-0.15	-0.026	-0.050	-0.072	-0.068	-0.063	-0.068	-0.023	-0.059
	(0.094)	(0.022)	(0.019)***	(0.026)***	(0.029)**	(0.033)*	(0.024)***	(0.061)	(0.062)
Rainfall Shock x Treatment	0.080	0.025	0.040	0.050	0.069	0.062	0.035	0.20	0.20
	(0.11)	(0.025)	(0.023)*	(0.033)	(0.034)**	(0.040)	(0.033)	(0.079)**	(0.083)**
Observations	11824	11216	11824	10068	8285	6618	1597	1143	1138
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects	Birth year x state								

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** 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.
-"Treatment village" = 1 for individuals in villages assigned to PROGRESA in the first wave (1998).
-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

In our main results, we generate rainfall shocks by calculating historical means (and standard deviations) for each locality birth-year observation using locality-level rainfall over the 10 years prior to each relevant year. In Table C10, we instead calculate these values using rainfall over the 20-year period centered around the median birth year in the sample, from 1978 to 1998. Our results are robust to the use of this longer time-frame that is consistent across all years.

We have chosen the simple rainfall indicator that we use in the main results because it is parsimonious, captures non-linearities, and easy to interpret. However, we can certainly allow for greater flexibility in this specification, which we do in Table C11. Here, we replace our simple indicator with four dummy variables for normalized birth-year rainfall below the 20th percentile ("droughts"), between the 20th and 40th percentile ("below normal"), between the 60th and 80th percentile ("above normal"), and above the 80th percentile ("floods") of the normalized rainfall distribution. Rainfall around the median – 40th to 60th percentile – is the omitted category.[7] Consistent with our main results, we see that floods and droughts have negative effects on our outcomes of interest (larger in magnitude than the insignificant effects

[7]Droughts and floods are roughly (though not exactly) equivalent to using the one-standard-deviation cutoff that we use for our main rainfall shock dummy.

Table C10: Effects of PROGRESA and Birth-Year Rainfall, Using Alternate Historical Rainfall Calculations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.12	0.012	0.012	0.025	0.017	0.022	-0.0042	-0.011	-0.011
	(0.038)***	(0.0097)	(0.0075)	(0.011)**	(0.011)	(0.011)*	(0.0073)	(0.016)	(0.015)
Rainfall Shock	-0.26	-0.056	-0.12	-0.12	-0.13	-0.15	-0.055	-0.19	-0.20
	(0.26)	(0.064)	(0.049)**	(0.061)**	(0.067)*	(0.083)*	(0.061)	(0.12)	(0.14)
Rainfall Shock x Exposure	0.060	0.014	0.023	0.025	0.028	0.033	0.0070	0.082	0.077
	(0.051)	(0.013)	(0.0099)**	(0.012)**	(0.013)**	(0.016)**	(0.018)	(0.042)*	(0.046)*
Observations	11824	11216	11824	10068	8285	6618	1597	1143	1138
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** 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 20-year locality-specific mean from 1978-1998 (centered on the median birth year)
-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

of the "below median" and "above median" dummies). We also find that the interactions between both droughts and floods with PROGRESA exposure are significant and indicative of remediation, across the majority of education outcomes. There is very little precision in any of our employment regressions, due to the very small sample sizes and more demanding empirical specification. In terms of magnitudes, the drought main effects and interactions appear to be slightly larger than the respective flood coefficients, but these differences are not significantly different from zero, which validates our use of a simple indicator that combines these two types of shocks.

We focus on rainfall shocks in an individual's year of birth, specifically, because we are interested in shocks that affect a child's endowment very early in life. A shock to the endowment during the year of birth should provide the cleanest and earliest source of exogenous variation, but it is of course possible that shocks during early childhood could also affect later-life outcomes. To investigate whether shocks in other years of life had similar positive effects on later-life outcomes, and similar interactions with PROGRESA, we add additional rainfall shock variables to our regressions and report the results in Table C12. Specifically, we add indicators for rainfall shocks during the year before birth, the

Table C11: Effects of PROGRESA and Birth-Year Rainfall, Using Flexible Definition of Rainfall Shock

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.068	0.0038	0.013	0.019	0.011	0.015	0.0016	-0.055	-0.042
	(0.048)	(0.011)	(0.0090)	(0.014)	(0.015)	(0.015)	(0.012)	(0.029)*	(0.029)
Drought	-1.03	-0.22	-0.18	-0.24	-0.27	-0.28	-0.060	-0.16	-0.16
	(0.40)**	(0.072)***	(0.064)***	(0.094)**	(0.091)***	(0.11)***	(0.063)	(0.19)	(0.19)
Below normal rainfall	-0.16	0.031	0.020	0.028	0.021	0.032	0.065	-0.21	-0.12
	(0.30)	(0.073)	(0.059)	(0.067)	(0.071)	(0.073)	(0.076)	(0.14)	(0.14)
Above normal rainfall	-0.30	0.0029	-0.014	-0.065	-0.096	-0.089	-0.025	-0.18	-0.14
	(0.27)	(0.059)	(0.050)	(0.070)	(0.075)	(0.077)	(0.052)	(0.14)	(0.14)
Flood	-0.65	-0.19	-0.11	-0.19	-0.18	-0.28	-0.054	-0.20	-0.18
	(0.28)**	(0.075)**	(0.064)*	(0.10)*	(0.11)*	(0.11)**	(0.079)	(0.19)	(0.22)
Drought	0.17	0.037	0.027	0.040	0.047	0.050	-0.00068	0.080	0.067
x Exposure	(0.075)**	(0.014)***	(0.013)**	(0.018)**	(0.017)***	(0.021)**	(0.022)	(0.054)	(0.055)
Below normal rainfall	0.027	-0.0024	-0.0051	-0.0051	-0.0057	-0.0097	-0.028	0.073	0.036
x Exposure	(0.058)	(0.015)	(0.012)	(0.013)	(0.014)	(0.014)	(0.020)	(0.044)*	(0.044)
Above normal rainfall	0.049	-0.00084	-0.0030	0.0099	0.014	0.015	0.0028	0.067	0.055
x Exposure	(0.052)	(0.012)	(0.011)	(0.014)	(0.015)	(0.016)	(0.015)	(0.040)	(0.041)
Flood	0.12	0.041	0.014	0.032	0.032	0.053	-0.0033	0.085	0.063
x Exposure	(0.056)**	(0.015)***	(0.013)	(0.019)*	(0.021)	(0.023)**	(0.022)	(0.061)	(0.069)
Observations	11824	11216	11824	10068	8285	6618	1597	1143	1138
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Tests for equality of coefficients (p-values)									
drought = flood	0.66	0.68	0.57	0.19	0.16	0.17	0.22	0.82	0.86
below normal = above normal	0.36	0.73	0.31	0.66	0.49	0.99	0.95	0.88	0.95
drought x exposure = flood x exposure	0.72	0.91	0.86	0.29	0.20	0.15	0.09	0.88	0.62
below normal x exposure = above normal x exposure	0.51	0.84	0.34	0.73	0.53	0.90	0.93	0.95	0.96
Fixed Effects					Birth year x state				

Notes:

- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

-"Drought", "Below normal rainfall," Above normal rainfall," and "Flood" are dummy variables indicating individuals whose birth-year rainfall (normalized using the locality-specific historical 10-year mean and standard deviation) fell below the 20th percentile, between the 20th and 40th percentile, between the 60th and 80th percentile, and above the 80th percentile, respectively.

-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

second year of life, and the third year of life, along with their interactions with PROGRESA exposure. Consistent with Maccini and Yang (2009), we find that year of birth rainfall is the only one that has consistently large and significant effects across all outcomes. Accordingly, PROGRESA's ability to remediate is only apparent with respect to birth-year rainfall and not rainfall in any other year (with the exception of column 3 for rainfall in the year before birth). Across both employment and education outcomes, our three main coefficients of interest are almost identical in magnitude to the estimates in Tables 4, 5, and 6.

Table C12: Effects of PROGRESA and Rainfall in Year of Birth and Other Early-Life Years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.087	0.0098	0.013	0.015	0.010	0.013	0.0061	-0.0093	-0.0027
	(0.050)*	(0.011)	(0.0099)	(0.014)	(0.013)	(0.014)	(0.011)	(0.026)	(0.026)
Rainfall Shock in year of birth	-0.63	-0.12	-0.13	-0.21	-0.23	-0.25	-0.10	-0.32	-0.35
	(0.30)**	(0.060)**	(0.053)**	(0.072)***	(0.074)***	(0.086)***	(0.062)*	(0.14)**	(0.15)**
Rainfall Shock in year of birth x Exposure	0.11	0.022	0.023	0.035	0.042	0.048	0.022	0.12	0.12
	(0.057)*	(0.012)*	(0.011)**	(0.014)**	(0.014)***	(0.017)***	(0.019)	(0.043)***	(0.047)**
Rainfall Shock in year before birth	-0.41	0.021	-0.082	-0.071	-0.092	-0.072	-0.014	-0.12	-0.17
	(0.25)	(0.052)	(0.044)*	(0.067)	(0.070)	(0.073)	(0.053)	(0.11)	(0.10)*
Rainfall Shock in year before birth x Exposure	0.082	-0.0043	0.016	0.013	0.018	0.015	0.0053	0.026	0.049
	(0.050)	(0.011)	(0.0091)*	(0.014)	(0.014)	(0.015)	(0.015)	(0.034)	(0.032)
Rainfall Shock in second year	-0.25	-0.087	-0.052	-0.11	-0.11	-0.12	-0.028	0.090	0.064
	(0.29)	(0.071)	(0.053)	(0.065)*	(0.069)	(0.078)	(0.042)	(0.11)	(0.100)
Rainfall Shock in second year x Exposure	0.038	0.016	0.0091	0.019	0.014	0.020	-0.0037	-0.040	-0.042
	(0.056)	(0.013)	(0.010)	(0.013)	(0.013)	(0.015)	(0.012)	(0.029)	(0.029)
Rainfall Shock in third year	-0.064	0.038	0.088	-0.021	-0.027	-0.061	0.13	0.11	0.21
	(0.28)	(0.046)	(0.047)*	(0.068)	(0.068)	(0.078)	(0.054)**	(0.11)	(0.097)**
Rainfall Shock in third year x Exposure	0.0095	-0.0055	-0.017	0.0070	0.0035	0.0046	-0.039	-0.044	-0.073
	(0.052)	(0.0097)	(0.0097)*	(0.013)	(0.013)	(0.016)	(0.015)**	(0.032)	(0.031)**
Observations	10607	10057	10607	8940	7404	5860	1450	1028	1025
Mean of Dependent Variable	6.76	0.58	0.46	0.56	0.51	0.45	0.061	0.35	0.41
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects	Birth year x state								

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Rainfall shock" = 1 for individuals whose rainfall (in the relevant year) 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

D Additional Tables and Figures

Table D1: Monthly Amount of Educational Transfers to Beneficiary Households (from Behrman et al. 2011)

	1997		1998		2003	
	Boys	Girls	Boys	Girls	Boys	Girls
Primary School						
3rd year	60	60	70	70	105	105
4th year	70	70	80	80	120	120
5th year	90	90	100	100	155	155
6th year	120	120	135	135	210	210
Junior High School						
1st year	175	185	200	210	305	320
2nd year	185	205	210	235	320	355
3rd year	195	205	220	625	335	390
High School						
1st year	-	-	-	-	510	585
2nd year	-	-	-	-	545	625
3rd year	-	-	-	-	580	660

Notes:
1. Amounts (in pesos) are for the second semester of the year
2. Grants extended to high school in 2001.

Figure D1: Proportion of Individuals Not Living in Household, by Age

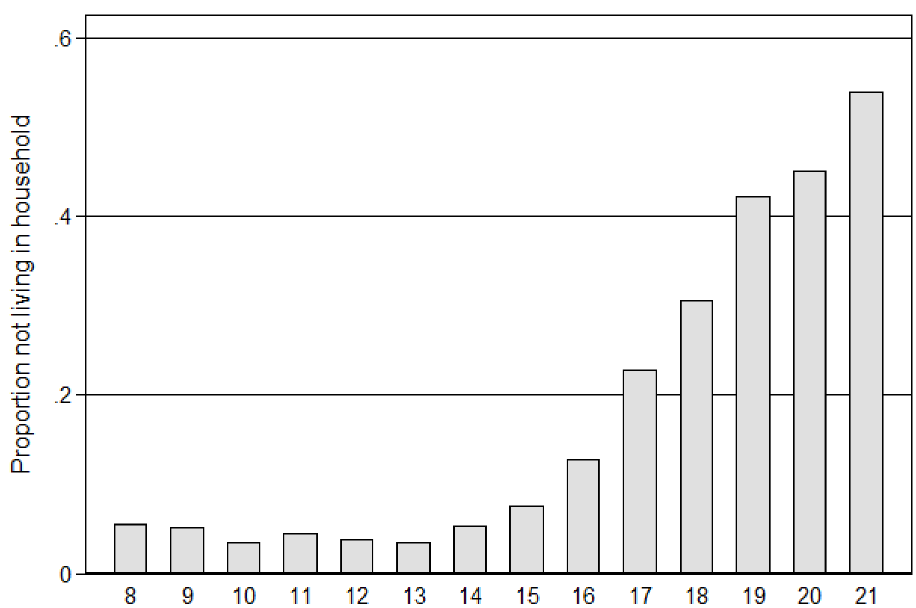
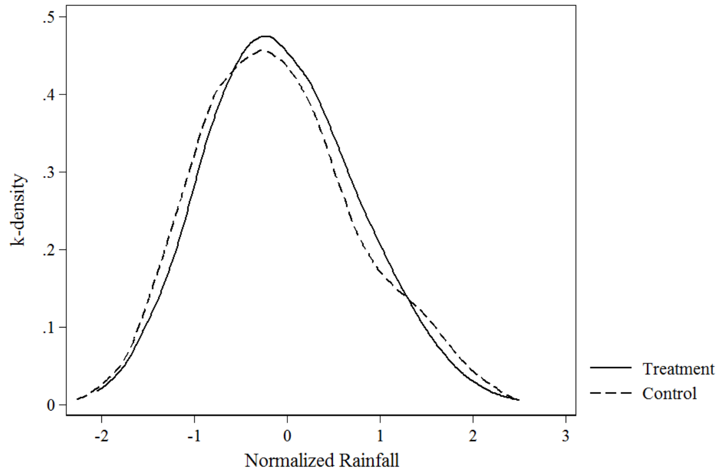


Figure D2: Normalized Rainfall Distributions in Treatment and Control Villages



Notes:
Rainfall levels are normalized using each locality's location-specific 10-year historical mean and standard deviation.

Figure D3: PROGRESA Localities by Treatment Status and Rainfall Shock in 1987, Trimmed Sample

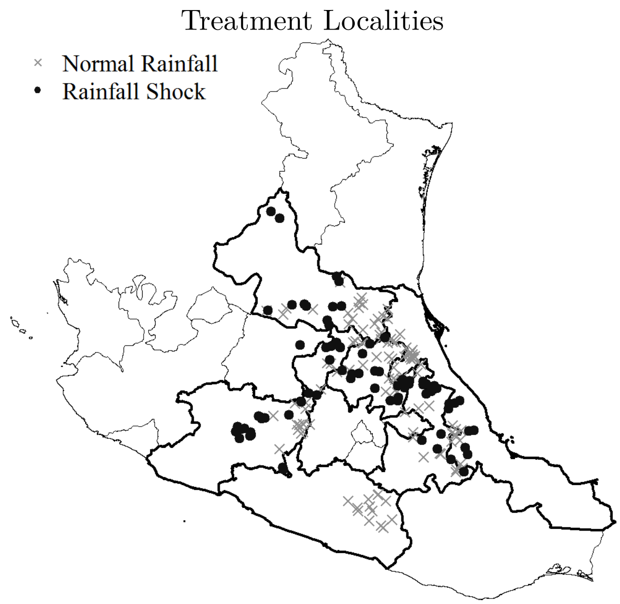


Table D2: Summary of Related PROGRESA Literature

Age of sample during Progresa exposure	Outcome Category	Outcome	Result	Analysis timeframe	Studies
School-aged	Education	Attendance	Increased, particularly for older ages	ST	Skoufias and Parker (2001)
		Dropouts	Decreased	ST	Behrman, Sengupta, Todd (2005)
		Educational attainment	Increased by ~0.66 years	ST	Schultz (2004); Behrman, Sengupta, Todd (2005)
		Educational attainment	Increased by ~0.2 years	MT	Behrman, Parker, Todd (2011); Behrman, Parker, Todd (2009)
		Enrollment	Increased for younger ages	MT	Behrman, Parker, Todd (2009)
		Enrollment	Increased for older ages	ST	Behrman, Sengupta, Todd (2005); Behrman, Sengupta, Todd (2000); Schultz (2004)
		Grade progression	Increased	ST	Behrman, Sengupta, Todd (2005)
		Grade progression	Increased	MT	Behrman, Parker, Todd (2009)
		Grade repetition	Decreased	ST	Behrman, Sengupta, Todd (2005)
		Re-entering school	Increased	ST	Behrman, Sengupta, Todd (2005)
		Schooling gaps	Decreased	ST	Behrman, Sengupta, Todd (2005); Behrman, Sengupta, Todd (2000)
		Work	Decreased	ST	Skoufias and Parker (2001); Schultz (2004)
		Work	Decreased for younger boys	MT	Behrman, Parker, Todd (2011); Behrman, Parker, Todd (2009)
	Health	Overweight	Decreased for girls (in most spec.'s)	MT	Andalon (2011)
	Cognitive and Behavioral	Cognitive tests	No significant effect	ST	Behrman, Sengupta, Todd (2000)
Younger than 3rd grade	Education	Age of school start	Decreased	MT	Behrman, Parker, Todd (2009b)
		Educational attainment	Increased	MT	Behrman, Parker, Todd (2009b)
		Grade progression	Increased	MT	Behrman, Parker, Todd (2009b)
Younger than school-age	Health	Anemia	Decreased	ST	Gertler (2004)
		BMI	No significant effect	LT	Fernald, Gertler, Neufeld (2009)
		Height	Increased	ST	Gertler (2004)
		Height	Increased only for children of mothers with no educatin	LT	Fernald, Gertler, Neufeld (2009)
		Salivary cortisol	Decreased for children of mothers with high depressive symptoms	MT	Fernald and Gunnar (2009)
		Self-reported morbidity	Decreased	ST	Gertler (2004)
	Cognitive and Behavioral	Behavioral problems	Decreased	LT	Fernald, Gertler, Neufeld (2009)
		Cognitive tests	No significant effect	LT	Fernald, Gertler, Neufeld (2009)
		Language tests	Increased	MT	Fernald, Gertler, Neufeld (2008b)
Younger than school-age (including not born)	Health	Anemia	Decreased in ST	ST	Rivera et al (2004)
		Birthweight (self-rep.)	Increased	ST/MT	Barber and Gertler (2008)
		Height	Increased	ST	Behrman and Hoddinott (2005); Rivera et al (2004)
		Infant mortality	Reduced	MT	Barham (2011)
		Neonatal mortality	No significant effect	MT	Barham (2011)
		Pre-natal care visits by mother	No significant effect	ST/MT	Barber and Gertler (2008)
Not born	Health	BMI	Decreased	MT	Fernald, Gertler, Neufeld (2008b)
		Height	Increased	MT	Fernald, Gertler, Neufeld (2008b)
		Motor development	Increased	MT	Fernald, Gertler, Neufeld (2008b)
	Cognitive and Behavioral	Cognitive tests	Increased	MT	Fernald, Gertler, Neufeld (2008b)
Adults	Health	Blood pressure	Increased (due to cash component)	MT	Fernald, Gertler, Hou (2008a)
		Blood pressure	Decreased	MT	Fernald, Hou, Gertler (2008c)
		BMI	Increased (due to cash component)	MT	Fernald, Gertler, Hou (2008a)
		Elderly mortality	Reduced	MT	Barham and Rowberry (2013)
		Hypertension	Decreased	MT	Fernald, Hou, Gertler (2008c)
		Overweight, obesity	Increased (due to cash component)	MT	Fernald, Gertler, Hou (2008a)
		Self-reported health	Increased	MT	Fernald, Hou, Gertler (2008c)
N/A - Households	Income and Consumption	Agricultural income, assets, production	Increased	ST	Gertler, Martinez, Rubio-Condina (2012)
		Consumption	Increased	ST	Djebbari and Smith (2008); Angelucci and De Giorgi (2012)
		Food consumption	Increased	ST	Hoddinott and Skoufias (2004); Angelucci and De Giorgi (2012)

- ST (short-term) estimates used outcomes measured before control group received treatment in 2000
- MT (medium-term) estimates used outcomes measured between 2000 and 2003
- LT (long-term) estimates used outcomes measured in 2007

Table D3: 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.

Table D4: Summary Statistics for Control Variables

Panel A: Household-level

	Full Sample	*Treatment Villages*	*Control Villages*	*Treatment - Control Differences*
Household size	7.41	7.42	7.40	0.019
	(2.19)	(2.22)	(2.15)	(0.041)
Household head age	41.7	41.4	42.2	-0.79***
	(11.3)	(11.1)	(11.6)	(0.21)
Female household head	0.057	0.056	0.057	-0.00047
	(0.23)	(0.23)	(0.23)	(0.0043)
Number of children aged 0-2	0.073	0.073	0.072	0.0016
	(0.086)	(0.086)	(0.087)	(0.0016)
Number of children aged 3-5	0.10	0.10	0.099	0.0034*
	(0.096)	(0.096)	(0.096)	(0.0018)
Number of boys aged 6-7	0.052	0.051	0.054	-0.0027*
	(0.077)	(0.076)	(0.079)	(0.0014)
Number of boys aged 8-12	0.12	0.13	0.12	0.0049**
	(0.11)	(0.11)	(0.11)	(0.0021)
Number of boys aged 13-18	0.070	0.070	0.069	0.00067
	(0.095)	(0.096)	(0.093)	(0.0018)
Number of girls aged 6-7	0.051	0.052	0.051	0.00082
	(0.076)	(0.077)	(0.076)	(0.0014)
Number of girls aged 8-12	0.12	0.12	0.12	-0.0023
	(0.11)	(0.11)	(0.11)	(0.0021)
Number of girls aged 13-18	0.066	0.065	0.067	-0.0014
	(0.091)	(0.091)	(0.091)	(0.0017)
Number of women aged 19-54	0.16	0.16	0.16	-0.00096
	(0.061)	(0.061)	(0.062)	(0.0011)
Number of men aged 55 and over	0.019	0.018	0.019	-0.00087
	(0.051)	(0.051)	(0.051)	(0.00094)
Number of women aged 55 and over	0.017	0.017	0.018	-0.0018*
	(0.050)	(0.050)	(0.051)	(0.00093)
Mother's educational attainment (categorical)	3.93	3.92	3.93	-0.0040
	(2.07)	(2.07)	(2.07)	(0.048)
Mother's educational attainment missing	0.34	0.33	0.36	-0.024***
	(0.47)	(0.47)	(0.48)	(0.0088)
Father's educational attainment (categorical)	3.98	4.03	3.89	0.14***
	(2.25)	(2.31)	(2.14)	(0.050)
Father's educational attainment missing	0.31	0.30	0.31	-0.0080
	(0.46)	(0.46)	(0.46)	(0.0086)
Mother speaks indigenous language	0.38	0.37	0.39	-0.012
	(0.48)	(0.48)	(0.49)	(0.0092)
Mother's language missing	0.041	0.039	0.044	-0.0046
	(0.20)	(0.19)	(0.20)	(0.0037)
Father speaks indigenous language	0.39	0.38	0.40	-0.021**
	(0.49)	(0.49)	(0.49)	(0.0095)
Father's language missing	0.096	0.097	0.094	0.0030
	(0.29)	(0.30)	(0.29)	(0.0055)
Number of households	6233	3795	2438	

Panel B: Locality-level

	Full Sample	*Treatment Villages*	*Control Villages*	*Treatment - Control Differences*
Community Well	0.38	0.37	0.39	-0.027
	(0.49)	(0.48)	(0.49)	(0.049)
Well Spring	0.48	0.51	0.44	0.074
	(0.50)	(0.50)	(0.50)	(0.050)
Public Water Network	0.15	0.12	0.19	-0.070*
	(0.36)	(0.33)	(0.39)	(0.035)
Bury Garbage	0.18	0.21	0.14	0.065*
	(0.39)	(0.41)	(0.35)	(0.039)
Public Dumpster	0.017	0.0078	0.031	-0.023*
	(0.13)	(0.088)	(0.17)	(0.013)
Public Drainage	0.038	0.035	0.043	-0.0079
	(0.19)	(0.18)	(0.20)	(0.019)
Public Phone	0.52	0.52	0.52	-0.0040
	(0.50)	(0.50)	(0.50)	(0.050)
Hospital or health center	0.15	0.13	0.18	-0.046
	(0.36)	(0.34)	(0.38)	(0.036)
Distance to health center	13.5	13.7	13.2	0.57
	(24.4)	(24.3)	(24.7)	(2.45)
DICONSA store	0.24	0.26	0.20	0.058
	(0.43)	(0.44)	(0.40)	(0.043)
Distance to Bank	38.7	40.5	36.0	4.48
	(51.8)	(59.3)	(37.6)	(5.50)
Distance to Bank Missing	0.12	0.13	0.098	0.030
	(0.32)	(0.34)	(0.30)	(0.032)
Distance to Secondary School	11.8	12.2	11.3	0.84
	(15.9)	(16.0)	(15.9)	(2.44)
Distance to Secondary School Missing	0.58	0.60	0.55	0.047
	(0.49)	(0.49)	(0.50)	(0.049)
Number of localities	420	257	163	

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). Missing indicators for parental education and language are binary variables equal to 1 for individuals missing the relevant information. Community well, well spring, public water network, public dumpster, public drainage, public phone, hospital or health center, and DICONSA store are all indicators equal to 1 for localities that have the relevant public good or facility. Bury garbage is an indicator equal to 1 for localities that report burying garbage as their main form of garbage dispolsal. Distances reported in kilometers. Missing distance variables are indicators for localities that did not report a distance to the nearest secondary school or bank.

Table D5: Rainfall Shocks, PROGRESA, and Baseline Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
	Mother's education	Father's education	Mother primary only	Mother secondary	Father primary only	Father secondary	Mother Education Missing	Father's Education Missing	Household head age	Female household head	Number of children aged 0-2	Number of children aged 3-5	Number of boys aged 6-7	Number of boys aged 8-12	Number of boys aged 13-18	Number of girls aged 6-7	Number of girls aged 8-12	Number of girls aged 13-18	Number of women aged 19-54	Number of men aged 55 and over
Treatment Village	-0.0020	0.16	0.0077	-0.0069	0.027	0.011	-0.047	-0.025	-0.74	-0.000036	-0.00033	0.0012	-0.0026	0.0057	0.0014	0.00049	0.00011	-0.0026	-0.0019	-0.00065
	(0.12)	(0.15)	(0.028)	(0.0077)	(0.030)	(0.0085)	(0.026)*	(0.021)	(0.40)*	(0.0071)	(0.0029)	(0.0033)	(0.0026)	(0.0038)	(0.0026)	(0.0023)	(0.0040)	(0.0029)	(0.0020)	(0.0016)
Rainfall Shock	-0.0043	-0.044	-0.00014	-0.0071	-0.0067	-0.0080	-0.011	0.044	-0.21	0.0020	-0.0025	-0.0015	0.0014	-0.0013	0.00097	-0.00076	0.0096	-0.0041	-0.0024	0.0012
	(0.093)	(0.12)	(0.020)	(0.0091)	(0.023)	(0.0077)	(0.023)	(0.019)**	(0.34)	(0.0089)	(0.0032)	(0.0033)	(0.0028)	(0.0045)	(0.0029)	(0.0027)	(0.0040)**	(0.0030)	(0.0023)	(0.0015)
Rainfall Shock x Treatment	-0.044	-0.013	-0.028	0.0058	0.022	-0.00064	0.026	-0.016	-0.041	-0.0051	0.00063	0.0018	-0.000041	0.0022	-0.0036	0.0017	-0.0088	0.0047	0.0056	-0.00071
	(0.12)	(0.14)	(0.028)	(0.012)	(0.029)	(0.011)	(0.026)	(0.023)	(0.45)	(0.0094)	(0.0037)	(0.0044)	(0.0029)	(0.0049)	(0.0037)	(0.0032)	(0.0050)*	(0.0039)	(0.0026)**	(0.0021)
Observations	7998	8424	7998	7998	8424	8424	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159
Mean of Dep. Var.	3.93	3.98	0.29	0.037	0.30	0.050	0.34	0.31	41.7	0.057	0.073	0.10	0.052	0.12	0.070	0.051	0.12	0.066	0.16	0.019
Sample Ages (in 2003)										12 to 18										
Fixed Effects										Birth year x state										

	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)	(33)	(34)	(35)	(36)	(37)	(38)	(39)	(40)
	Number of women aged 55 and over	Mother speaks indigenous language	Father speaks indigenous language	Mother's language missing	Father's language missing	Community well	Well spring	Public water network	Bury garbage	Public dumpster	Public drainage	Public phone	Hospital or health center	Distance to health center	DICONSA store	Distance to bank	Distance to bank missing	Distance to secondary school	Distance to secondary school missing	Age
Treatment Village	-0.00052	-0.0018	-0.011	-0.00065	0.0077	-0.037	0.063	-0.074	0.074	-0.020	-0.011	-0.028	-0.081	2.89	0.074	-0.57	0.0028	-0.14	0.056	-0.16
	(0.0014)	(0.065)	(0.066)	(0.0057)	(0.0079)	(0.062)	(0.064)	(0.045)	(0.044)*	(0.018)	(0.020)	(0.062)	(0.058)	(2.44)	(0.055)	(5.96)	(0.039)	(1.26)	(0.063)	(0.088)*
Rainfall Shock	0.00025	-0.088	-0.086	0.0025	0.016	0.00076	-0.031	0.036	-0.024	0.030	0.00090	-0.077	0.0073	0.58	-0.0046	-1.95	0.019	1.27	-0.032	-0.69
	(0.0018)	(0.046)*	(0.045)*	(0.0078)	(0.012)	(0.044)	(0.044)	(0.031)	(0.027)	(0.024)	(0.016)	(0.052)	(0.038)	(1.59)	(0.043)	(3.14)	(0.028)	(0.99)	(0.041)	(0.21)***
Rainfall Shock x Treatment	-0.0025	0.036	0.031	-0.0087	-0.015	0.044	-0.059	-0.045	0.020	-0.032	-0.0100	0.086	0.028	-2.54	0.042	3.83	-0.0091	-1.95	0.067	0.30
	(0.0025)	(0.049)	(0.049)	(0.0087)	(0.014)	(0.055)	(0.052)	(0.033)	(0.037)	(0.022)	(0.016)	(0.057)	(0.049)	(2.30)	(0.052)	(3.43)	(0.037)	(1.10)*	(0.051)	(0.32)
Observations	12159	11663	10995	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159
Mean of Dep. Var.	0.017	0.38	0.39	0.041	0.096	0.36	0.51	0.14	0.16	0.015	0.037	0.57	0.20	12.6	0.30	33.6	0.11	5.55	0.57	15.0
Sample Ages (in 2003)										12 to 18										
Fixed Effects										Birth year x state										State

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** 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.
-"Treatment village" = 1 for individuals in villages assigned to PROGRESA in the first wave (1998).

Table D6: Effects of Birth-Year Rainfall on Future Household Income

	(1)	(2)
	Eligible for PROGRESA	1997 Household Income Score
Rainfall Shock	-0.022	3.16
	(0.014)	(5.03)
Observations	16836	16818
Mean of Dependent Variable	0.72	677.7
Sample Ages (in 2003)	12 to 18	

Notes:
- Standard errors clustered at the municipality level are in parentheses
-"Rainfall shock" = 1 for individuals whose birth-year rainfall was more than one standard deviation from the 10-year historical locality-specific mean.

Table D7: Effects of Birth-Year Rainfall on Parent and Sibling Characteristics in Baseline Survey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Mother Employed	Father Employed	Mother's Days Worked	Father's Days Worked	Mother's Hours Worked	Father's Hours Worked	Average Sibling Educational Attainment	Average Sibling Grade Completion
Rainfall Shock	0.013	-0.0072	0.063	-0.0058	0.11	0.023	-0.021	-0.0066
	(0.011)	(0.0057)	(0.060)	(0.047)	(0.081)	(0.064)	(0.038)	(0.0079)
Observations	11677	11011	11659	10950	11665	10944	10577	10360
Mean of Dependent Variable	0.12	0.95	0.66	5.20	0.91	7.91	3.60	0.64
Sample Ages (in 2003)	12 to 18							
Fixed Effects	Birth year x state							

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** 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.
-Outcome variables taken from the baseline survey

Table D8: Effects of PROGRESA and Birth-Year Rainfall on Woodcock-Johnson Test Scores

	(1)	(2)	(3)	(4)
	Letter Word Identification	Applied Problems	Dictation	Average Score
Years of PROGRESA Exposure	0.0045	0.014	0.027	0.013
	(0.040)	(0.035)	(0.045)	(0.035)
Rainfall Shock	0.14	-0.14	-0.19	-0.062
	(0.25)	(0.25)	(0.29)	(0.22)
Rainfall Shock x Exposure	-0.060	0.00039	0.034	-0.0056
	(0.052)	(0.051)	(0.059)	(0.045)
Observations	1593	1586	1581	1571
Sample Ages (in 2003)	15 to 18			
Fixed Effects	Birth year x state			

Notes:

- Standard errors clustered at the municipality level are in parentheses (*** 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

-Scores are standardized by test type, and the average score in column 4 takes the average across all three z-scores.
-These tests were administered to a sample of individuals aged 15-21 in 2003, but we restrict to those aged 15-18 in order to remain consistent with the main sample.

Table D9: Effects of Own PROGRESA Exposure, Own Birth-Year Rainfall, Sibling PROGRESA Exposure, and Sibling Birth-Year Rainfall on Education and Employment Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.10	0.0095	0.012	0.028	0.010	0.027	-0.013	0.023	0.011
	(0.049)**	(0.011)	(0.0092)	(0.013)**	(0.013)	(0.015)*	(0.013)	(0.028)	(0.029)
Own Rainfall Shock	-0.60	-0.11	-0.11	-0.19	-0.22	-0.26	-0.086	-0.25	-0.28
	(0.29)**	(0.056)*	(0.054)**	(0.074)**	(0.074)***	(0.086)***	(0.054)	(0.14)*	(0.15)*
Own Rainfall Shock x Exposure	0.12	0.021	0.019	0.030	0.039	0.041	0.022	0.14	0.15
	(0.053)**	(0.011)*	(0.010)*	(0.014)**	(0.014)***	(0.016)**	(0.018)	(0.036)***	(0.039)***
Sibling Rainfall Shock	-0.14	-0.013	-0.028	0.012	-0.055	0.023	-0.065	0.38	0.33
	(0.31)	(0.069)	(0.059)	(0.068)	(0.077)	(0.092)	(0.077)	(0.17)**	(0.17)*
Sibling Rainfall x Exposure	0.017	0.0011	0.0018	-0.0083	0.0060	-0.011	0.0084	-0.12	-0.11
	(0.062)	(0.014)	(0.012)	(0.013)	(0.015)	(0.020)	(0.021)	(0.051)**	(0.052)**
Average Sibling PROGRESA Expsoure	-0.0091	-0.0038	0.00059	-0.0042	0.0012	-0.0057	0.011	0.023	0.042
	(0.028)	(0.0075)	(0.0075)	(0.0079)	(0.0093)	(0.010)	(0.012)	(0.024)	(0.026)
Sibling Exposure x Own Rainfall Shock	-0.032	-0.0019	-0.0027	0.00020	0.00086	0.013	-0.012	-0.047	-0.053
	(0.036)	(0.0088)	(0.0081)	(0.011)	(0.013)	(0.011)	(0.011)	(0.037)	(0.035)
Observations	11379	10797	11379	9679	7948	6314	1487	1065	1063
Mean of Dependent Variable	6.77	0.58	0.47	0.56	0.51	0.45	0.060	0.36	0.42
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality are reported in parentheses (*** 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.
-"Sibling rainfall shock" is the average of the rainfall shock variable across all of a child's siblings.
-"Average Sibling PROGRESA Exposure" is the average years of PROGRESA exposure across all of a child's siblings.
-These regressions drop all children with no siblings.
-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.

Table D10: Effects of PROGRESA, Birth-Year Rainfall, and Household Income on Education and Employment Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.10	0.0099	0.013	0.023	0.015	0.020	-0.0049	-0.013	-0.013
	(0.038)***	(0.0098)	(0.0076)*	(0.011)**	(0.011)	(0.011)*	(0.0077)	(0.016)	(0.015)
Rainfall Shock	-0.67	-0.12	-0.12	-0.21	-0.23	-0.25	-0.10	-0.23	-0.27
	(0.28)**	(0.056)**	(0.051)**	(0.071)***	(0.072)***	(0.083)***	(0.053)*	(0.13)*	(0.13)**
Rainfall Shock x Exposure	0.12	0.021	0.020	0.033	0.041	0.047	0.018	0.10	0.10
	(0.053)**	(0.011)*	(0.010)*	(0.014)**	(0.013)***	(0.016)***	(0.017)	(0.039)**	(0.042)**
Standardized Household Income Score	0.24	0.029	0.011	0.094	0.084	0.057	0.0038	0.12	0.12
	(0.11)**	(0.023)	(0.020)	(0.029)***	(0.026)***	(0.026)**	(0.021)	(0.040)***	(0.041)***
Income Score x Exposure	-0.022	-0.0011	0.0037	-0.011	-0.0094	-0.0040	0.000036	-0.036	-0.033
	(0.021)	(0.0049)	(0.0042)	(0.0054)**	(0.0048)*	(0.0047)	(0.0065)	(0.013)***	(0.013)***
Observations	11813	11205	11813	10058	8277	6612	1596	1142	1137
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Sample Ages (in 2003)	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects	Birth year x state								

Notes:
- Standard errors clustered at the municipality are reported in parentheses (*** 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.

E Model

We lay out a simple model of optimal schooling choices that incorporates heterogeneous endowments and a schooling subsidy akin to PROGRESA. We derive our estimating equations from this model and then discuss what could be driving the remediation we find in our empirical analysis.

E.1 Setup

Our model extends the canonical schooling choice model in Card (2001) by allowing individuals to have heterogeneous initial endowments that affect future earnings. We study how the optimal level of schooling changes with the initial endowment and with education policies that attempt to offset the cost of schooling. We assume that individuals have an infinite time horizon: they attend school during the first S periods of life and work full-time for the rest of it. Individuals have an initial level of endowment, ω, that affects the earnings function in each period. While in school, the utility in period t depends on the level of consumption, $u\left(c\left(t\right)\right)$,[8] and the effort cost for the t-th year of schooling given the initial endowment ω, $\phi\left(\omega, t\right)$: specifically, in-school utility is equal to $u\left(c\left(t\right)\right) - \phi\left(\omega, t\right)$. Out of school, the utility is $u\left(c\left(t\right)\right)$. Finally, individuals discount future flows at a rate ρ.[9]

Conditional on schooling S and a consumption profile, life-cycle utility is

$$V\left(S, c\left(t\right)\right) = \int_0^S \left[u\left(c\left(t\right)\right) - \phi\left(\omega, t\right)\right] e^{-\rho t} dt + \int_S^\infty u\left(c\left(t\right)\right) e^{-\rho t} dt.$$

Let $y\left(\omega, S, t\right)$ be the earnings function at period t of an individual with initial endowment ω, S years of schooling, and $t \geq S$ years of post-schooling experience. We assume that while in school, individuals pay tuition costs minus the scholarship provided by PROGRESA at each period of time, $T\left(t\right) - x\left(t\right)$, and work part-time earning $P\left(t\right)$ in period t. Individuals borrow or lend at a fixed interest rate R. Thus the intertemporal budget constraint is

[8] We assume that $u\left(\cdot\right)$ is increasing and concave.

[9] Let $\mathcal{S}$ be the set of feasible values of schooling, X the set of feasible values of PROGRESA and Ω the set of feasible values of initial endowment.

$$\int_0^{\infty} c(t)\, e^{-Rt} dt \leq \int_0^{S} [P(t) - T(t) + x(t)]\, e^{-Rt} dt + \int_S^{\infty} y(\omega, S, t)\, e^{-Rt} dt.$$

We introduce the following functional assumptions, which help us characterize the optimal level of schooling and resultant income:

(A1): $y(\omega, S, t) \equiv f(\omega, S)\, h(t - S)$, where $h(0) = 1$ and $f(\cdot)$ is a CES production function, $f(w, S) = A[\lambda w^k + (1-\lambda) S^k]^{\frac{1}{k}}$ with $k \in (-\infty, 1]$, $k \neq 0$.[10]

(A2): $\phi(\omega, t)\, e^{-\rho t}$ is increasing and convex with respect to t, and decreasing with respect to ω.

(A3): $u(c(t)) = \log c(t)$.

(A4): $T(t) - P(t) - x(t) = T - P - x$ for all t.

(A1) assumes that the log of earnings is additively separable in years of post-schooling experience, and a function of an individual's education and initial endowment. (A1) also implies that the earnings are increasing with respect to the initial endowment and the level of schooling, and exhibit decreasing marginal returns to schooling for all levels of the initial endowment.[11] (A2) is self-evident. (A3) is the standard assumption of log utility. (A4) states that tuition, part-time earnings and PROGRESA subsidies are constant over time.

Proposition 1. There exists a unique S^*, where $S^* \equiv S^*(\omega, x)$ is implicitly defined by:

$$\Gamma(\omega, S^*) - \left(\frac{1}{H(R)} + \frac{T - P - x}{f(\omega, S^*)\, H(R)} + \rho \phi(\omega, S^*)\, e^{-\rho S^*} \right) = 0, \tag{2}$$

where $\Gamma(\omega, S) \equiv \frac{f_S(\omega, S)}{f(\omega, S)}$, which is the marginal return to schooling.[12]

[10] The results in this section can be generalized by assuming that $f(\cdot)$ is increasing with respect to ω and S, and concave with respect to S, for all ω, and that $\lim_{S \to 0^+} f_S(\cdot) = \infty$ and $\lim_{S \to +\infty} f_S(\cdot) = 0$ for all $\omega \in \Omega$.

[11] Note also that $f(\cdot)$ is log-supermodular with respect to (S, ω) if and only if $k < 0$, and log-submodular with respect to (S, ω) if and only if $k > 0$.

[12] In order to simplify the discussion we follow Card (2001) and impose three additional assumptions: first, we assume that tuition cost minus part-time earnings minus PROGRESA subsidy are constant over time. Second, we assume that tuition costs minus part-time earnings minus PROGRESA are small relative to lifetime earnings. Finally, we assume that the cost of schooling standardized by the life time earnings is increasing with S, i.e., $d_S = -\frac{(T-P-x) f_S(\omega, S)}{f(\omega, S)^2 H(R)} + \rho \frac{\partial}{\partial S} \left(\phi(\omega, S)\, e^{-\rho S} \right) > 0$, $\forall S \in \mathcal{S}$.

Proof. The first order condition with respect to S is

$$-\phi(\omega,S)e^{-S\rho}+\lambda\left\{[P(S)-T(S)+x(S)]e^{-RS}-y(\omega,S,S)e^{-RS}+\right. \tag{3}$$

$$\left.\int_S^\infty \frac{\partial y(\omega,S,t)}{\partial S}e^{-Rt}dt\right\}=0,$$

using (A1)

$$f_S(\omega,S)H(R)=f(\omega,S)+[T(s)-P(s)-x(S)]+\frac{\phi(\omega,S)e^{-(\rho-R)S}}{\lambda}, \tag{4}$$

where $H(R)$ is a decreasing function of the interest rate (e.g., following Card (2001), if earnings are fixed after completion of schooling $H(R)=\frac{1}{R}$). From the first order conditions with respect to the consumption, together with the budget constraint

$$\frac{1}{\lambda\rho}=\int_0^S [P(t)-T(t)+x(t)]e^{-Rt}dt+f(\omega,S)H(R)e^{-RS}. \tag{5}$$

Using (4), (5), and A4, S is implicitly defined by

$$\frac{f_S(\omega,S^*)}{f(\omega,S^*)}=\frac{1}{H(R)}+\frac{T-P-x}{f(\omega,S^*)H(R)}+\rho\phi(\omega,S^*)e^{-\rho S^*}\left[1-\frac{1}{R}\frac{(T-P-x)\left(e^{RS^*}-1\right)}{f(\omega,S^*)H(R)}\right]. \tag{6}$$

Next, given that tuition costs minus part-time earnings minus PROGRESA are small relative to lifetime earnings, then the term in square brackets is close to 1, i.e., $1-\frac{1}{R}\frac{(T-P-x)\left(e^{RS^*}-1\right)}{f(\omega,S^*)H(R)}\approx 1$. Then, S^* is uniquely defined by

$$\Lambda(x,\omega,S^*)\equiv\Gamma(\omega,S^*)-\underbrace{\left\{\frac{1}{H(R)}+\frac{T-P-x}{f(\omega,S^*)H(R)}+\rho\phi(\omega,S^*)e^{-\rho S^*}\right\}}_{d(x,\omega,S^*)}=0, \tag{7}$$

where $\Gamma(\omega,S)\equiv\frac{f_S(\omega,S)}{f(\omega,S)}$, which is the marginal return to schooling. Note that $\Lambda(x,\omega,S)$ is decreasing with respect to S. Moreover, note that $\Lambda(x,\omega,S) > 0$ as $S \to 0$, and as $S \to \infty$, $\Lambda(x,\omega,S) \to -\infty$. Thus, given $(x,\omega) \in X \times \Omega$, there exists a unique S^* that satisfies (7). By the Implicit Function Theorem, S^* can be expressed as a differentiable function of (ω, S), $S^* \equiv S^*(\omega, x)$. This completes the proof. $\square$

Let $d(x,\omega,S) \equiv \frac{1}{H(R)} + \frac{T-P-x}{f(\omega,S)H(R)} + \rho\phi(\omega,S)e^{-\rho S}$, be the marginal cost of schooling standardized by lifetime earnings. From Proposition 1 it follows that the optimal level of schooling is increasing with respect to x, i.e., $\frac{\partial S^*}{\partial x} > 0$. Also, the optimal level of schooling is increasing with respect to the initial endowment, ω, i.e., $\frac{\partial S^*}{\partial \omega} > 0$ if $\Gamma_\omega(\omega, S^*) - d_\omega(x,\omega,S^*) > 0$.[13,14]

E.2 Estimating Equations

The optimal level of schooling, $S^*(\omega, x)$, and the optimal level of income, $y^*(\omega, x)$, are thus non linear functions of PROGRESA, x, and the initial endowment, ω. To test empirically the predictions of the model we use the fact that $S^*(\omega, x)$ and $y^*(\omega, x)$ are differentiable functions to derive linear approximations for both functions.

Proposition 2. If $S^* \equiv S^*(\omega, x)$ is implicitly defined by (2) and $y(\omega, S, t)$ is a C^2-function then S^* and $y^* \equiv y^*(\omega, x)$ can be approximated by the polynomials

$$S^* = a_1\omega + a_2 x + a_3 \omega \cdot x + \varepsilon_s(\omega, x) \tag{8}$$

$$y^* = b_1\omega + b_2 x + b_3 \omega \cdot x + \varepsilon_y(\omega, x) \tag{9}$$

where $\varepsilon_s(\omega, x) = a_4\omega^2 + a_5 x^2 + o(||\,(\omega,x)\,||^2)$ and $\varepsilon_y(\omega, x) = b_4\omega^2 + b_5 x^2 + o(||\,(\omega,x)\,||^2)$.[15]

[13] For the rest of this appendix we assume that $\Gamma_\omega(\omega, S^*) - d_\omega(x,\omega,S^*) > 0$.

[14] We also explore the effects of PROGRESA and the initial endowment on the equilibrium income, y^*, in the following corollary. From Proposition 1 it follows that the optimal level of income is increasing with respect to x, i.e., $\frac{\partial y^*}{\partial x} > 0$. Also, the optimal level of income is increasing with respect to the initial endowment, ω, i.e., $\frac{\partial y^*}{\partial \omega} > 0$ if $\Gamma_\omega(\omega, S^*) - d_\omega(x,\omega,S^*) > 0$.

[15] If $f(\omega,S)$ is approximated by a CES production function and we assume a cost function of the form $\phi(\omega,S) = [S + (\overline{\omega} - \omega)]e^{\rho S}$, for $\omega \in [0, \overline{\omega}]$, then from the Implicit Function Theorem and equation (2), $\left.\frac{\partial^2 S^*}{\partial x^2}\right|_{(T-p-x)=0} = o(R)$ when $R \to 0$. Thus, a_5 is close to 0. Similarly, for b_5 (i.e., $\left.\frac{\partial^2 y^*}{\partial x^2}\right|_{(T-p-x)=0} = o(R)$ when $R \to 0$).

Proof. From equation (7) and the Implicit Function Theorem we know that $S^* \equiv S^*(\omega, x)$ is as smooth as $\Lambda(x, \omega, S)$, and this last function is a C^2-function since $f(\omega, S)h(t-S)$ is also C^2-function. Therefore $S^*(\omega, x)$ is a C^2-function and by the Multivariate Taylor Theorem it can be approximated by a polynomial plus an error:

$$S^*(\omega, x) = a_1\omega + a_2 x + a_3\omega \cdot x + \varepsilon(\omega, x) \tag{10}$$

where $\varepsilon_s(\omega, x) = a_4\omega^2 + a_5x^2 + o(||\left(\omega, x\right)||^2)$.

Similarly, from the Implicit Function Theorem there exist neighborhoods $U \subset \mathcal{S}$ and $W \subset X \times \Omega$ of S^* and (x, ω), respectively, on which there is a function $\xi : W \to U$ such that $(x, \omega, \xi\left(x, \omega\right))$ satisfy (2) for all $(x, \omega) \in W$. Then, $y^*(\omega, x) = y(\omega, \xi\left(x, \omega\right), t)$ is a C^2-function and by the Multivariate Taylor Theorem it can be approximated by a polynomial plus an error:

$$y^* = b_1\omega + b_2 x + b_3\omega \cdot x + \varepsilon_y(\omega, x) \tag{11}$$

where $\varepsilon_y(\omega, x) = b_4\omega^2 + b_5x^2 + o(||\left(\omega, x\right)||^2)$. $\square$

E.3 Remediation

Our empirical strategy uses adverse rainfall as a (negative) proxy for the initial endowment, i.e., $a_1\omega \approx a_1\left(-R\right)$, where R is an adverse rainfall shock. If outcomes are increasing in the endowment, as predicted above, we expect: $a_1 \approx -\beta_1 > 0$ and $b_1 \approx -\tilde{\beta}_1 > 0$, where β_1 and $\tilde{\beta}_1$ are the coefficients for the rainfall shock in our main estimating equation (1) for education and employment outcomes, respectively. Similarly, more years of PROGRESA should increase the optimal level of schooling and income, i.e., $a_2 \approx \beta_2 > 0$ and $b_2 \approx \tilde{\beta}_2 > 0$, where β_2 and $\tilde{\beta}_2$ are the coefficients for PROGRESA in our main estimating equation (1) for education and employment outcomes, respectively. Finally, note that $a_3\omega \cdot x \approx a_3\left(-R\right) \cdot x$. Thus, if $a_3 \approx -\beta_3 < 0$ and $b_3 \approx -\tilde{\beta}_3 < 0$, PROGRESA generates remediation in both schooling and income, where β_3 and $\tilde{\beta}_3$ are the coefficients for the interaction of PROGRESA and the rainfall shock in our main estimating equation (1) for education and employment outcomes, respectively. If the opposite is true (that is, if $a_3 \approx -\beta_3 > 0$ and $b_3 \approx -\tilde{\beta}_3 > 0$) PROGRESA generates reinforcement.

Our empirical results yield negative main effects for the adverse negative shock ($a_1 \approx -\hat{\beta}_1 > 0$, $b_1 \approx -\hat{\tilde{\beta}}_1 > 0$), positive main effects for PROGRESA ($a_2 > 0$, $b_2 > 0$), and positive cross-partials ($a_3 \approx -\hat{\beta}_3 < 0$ and $b_3 \approx -\hat{\tilde{\beta}}_3 < 0$), which means that PROGRESA generates remediation in both schooling and income. To explore what drives the remediation we find, we use this model to study how PROGRESA affects the optimal level of schooling and income, S^* and $y*$, respectively, differently for individuals with different levels of the initial endowment.

From the Implicit Function Theorem it follows that $\frac{\partial^2 S^*}{\partial\omega\partial x}$ is given by:

$$\frac{\partial^2 S^*}{\partial\omega\partial x} = \alpha_1 \left[\Gamma_{S\omega}(\omega, S^*) + \alpha_2 \Gamma_{SS}(\omega, S^*)\right] + \Theta(x, \omega, S^*), \tag{12}$$

where $\Gamma_{S\omega}(\omega, S) \equiv \frac{\partial^2}{\partial\omega\partial S}\Gamma(S, x, \omega)$ and $\Theta(x, \omega, S) \equiv -\frac{\Lambda_\omega}{\Lambda_S^2} d_{xS} + \frac{1}{\Lambda_S} d_{x\omega} + \rho \frac{\Lambda_x \Lambda_\omega}{\Lambda_S^3} \frac{\partial^2}{\partial^2 S}\left(\phi(\omega, S)\, e^{-\rho S}\right)$. Moreover, $\alpha_1 > 0$, $\alpha_2 > 0$ and $\Theta(x, \omega, S^*) < 0$.[16]

Note that if the expression in (12) is negative, PROGRESA generates remediation – larger PROGRESA effects (on the optimal level of schooling, S^*) for lower-endowment individuals. If this expression is positive, PROGRESA generates reinforcement – larger effects for higher-endowment individuals. Similarly, if (13) is negative (positive) PROGRESA generates remediation (reinforcement) on the optimal level of income, y^*, for lower (higher)-endowment individuals.

Given that our empirical results imply that the expression in (12) is negative, we now examine why this is the case. This expression depends on features of the production function (i.e., $\alpha_1 \left[\Gamma_{S\omega}(\omega, S^*) + \alpha_2 \Gamma_{SS}(\omega, S^*)\right]$) and features of the marginal cost function (i.e.,

[16] Also, let $\xi : W \to U$, a function such that $(x, \omega, \xi(x, \omega))$ satisfy (2) for all $(x, \omega) \in W$.[17] Then, $\frac{\partial^2 y^*}{\partial\omega\partial x}$ is given by the following expression:

$$\left[\underbrace{f_{SS}(\omega, \xi(x, \omega))}_{<0} \underbrace{\frac{\partial \xi(x, \omega)}{\partial \omega}}_{>0} + f_{S\omega}(\omega, \xi(x, \omega))\right] \underbrace{\frac{\partial \xi(x, \omega)}{\partial x}}_{>0} + \underbrace{f_S(\omega, \xi(x, \omega))}_{>0} \frac{\partial^2 \xi(x, \omega)}{\partial\omega\partial x}. \tag{13}$$

$\Theta(x,\omega,S^*)$). We begin with the latter. The first two terms of $\Theta(x,\omega,S^*)$, $-\frac{\Lambda_\omega}{\Lambda_S^2}d_{xS}+\frac{1}{\Lambda_S}d_{x\omega}$, capture how PROGRESA changes the cost of schooling (relative to foregone earnings) differently for high-endowment and low-endowment individuals. These two terms are negative, which means that PROGRESA decreases the relative cost of schooling more for lower-endowment individuals, and therefore generates larger increases in optimal schooling for these lower-endowment individuals compared to higher-endowment individuals. The third term of $\Theta(x,\omega,S^*)$, $\rho\frac{\Lambda_x\Lambda_\omega}{\Lambda_S^3}\frac{\partial^2}{\partial^2 S}\left(\phi(\omega,S^*)\,e^{-\rho S^*}\right)$, is negative due to the convexity of the cost function and the fact that $\frac{\Lambda_x\Lambda_\omega}{\Lambda_S^3}<0$. In other words, convex costs will make it more difficult to change optimal schooling levels for a high-endowment individual (who obtains more schooling) compared to a low-endowment individual.

With respect to the first term of expression (12), $\alpha_1\left[\Gamma_{S\omega}(\omega,S^*)+\alpha_2\Gamma_{SS}(\omega,S^*)\right]$: Γ_{SS} depends on the third derivative of $\log f(\cdot)$, which is difficult to interpret; and $\Gamma_{S\omega}(\omega,S)$ measures how the curvature of the log earnings function with respect to schooling changes with the initial endowment.[18]

In sum, PROGRESA could be generating remediation for one or more of the following three reasons. First, the transfer represents a larger share of foregone earnings for low endowment compared to high endowment children. Second, convex schooling costs would make it more difficult to shift a high-endowment's (higher) level of optimal schooling. Finally, if the second derivative of the earnings function with respect to schooling is decreasing in the initial endowment, this would also contribute to remediation.

[18]Whether we see remediation or reinforcement on the optimal level of income (equation 13), depends also on two terms. The second term, $f_S(\omega,\xi(x,\omega))\frac{\partial^2\xi(x,\omega)}{\partial\omega\partial x}$, depends on the size of the reinforcement or remediation effect on the optimal level of schooling. The first term (the bracket) depends also on two terms: the first term, $f_{SS}(\omega,\xi(x,\omega))\frac{\partial\xi(x,\omega)}{\partial\omega}$, is negative, due to the concavity of the earnings function. The second bracketed term, $f_{S\omega}(\omega,\xi(x,\omega))$, is positive from A2. Thus, remediation on the optimal level of income depends on the size of the remediation effect on the optimal level of schooling, and on the total derivative of the marginal return of education on earnings, with respect the initial endowment.