Helping Children Catch Up: Early Life Shocks and the *Progresa* Experiment[*]

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Abstract

Can investing in children who faced adverse events in early childhood help them catch up? We answer this question using two orthogonal sources of exogenous variation–resource availability at birth (local rainfall) and cash incentives for school enrollment (the *Progresa* experiment)–to identify an interaction effect between early endowments and childhood investment. We find, in a sample of Mexican children, that adverse rainfall around the time of birth substantially decreases educational attainment. But children whose families were randomized to receive conditional cash transfers through *Progresa* were able to mitigate about 80 percent of this negative impact.

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JEL Classification Codes: I15, I25, O12

Poor circumstance in early life often has long-lasting negative impacts (Almond and Currie, 2011; Currie and Vogl, 2012; Heckman, 2006).[1] What role can important change agents – parents, communities, governments – play in lessening the burden of adverse events in a young child's life? Recent work has shown that in many contexts, parents, for example, do provide more time and material resources to disadvantaged children (Almond and Mazumder, 2013). But how much of a difference does this extra investment make? That is: to what extent is remediation possible, and which behaviors and policies can generate meaningful catch-up? This is a fundamentally different question than asking, as influential previous work has done, whether disadvantaged children who received support of various types were better off as a result (e.g., Aizer et al. (2014); Almond et al. (2010); Chetty et al. (2015); Hoynes et al. (2012)).

Ours is a difficult question to answer rigorously, given that investments following a shock are, in general, endogenous responses. Investments and resulting outcomes are jointly determined by parents' preferences, families' access to resources, and the like. Comparing the outcomes of two people who faced the same shock but were privy to different levels of corrective investment will therefore produce a biased impact of the remediation value of investments. As Almond and Mazumder (2013) put it in their recent review, resolving this identification problem "may be asking for 'lightning to strike' twice: two identification strategies affecting the same cohort but at adjacent developmental stages. Clearly this is a tall order."

In this study we attempt to overcome this difficulty. We demonstrate that recovery from early life shocks is possible, at least in the case of educational attainment, via conditional cash transfers during childhood. We leverage the combination of a natural experiment that induced changes in initial endowments and a large-scale randomized controlled trial of cash transfers for school enrollment in Mexico. We show that Mexican children born during periods of adverse rainfall have worse educational attainment than those born in normal rainfall periods. Our estimates suggest that exposure to adverse rainfall in the year of one's birth

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[1]Shocks to the early life environment – disease, poverty, maternal stress, nutritional or income availability, conflict, and pollution, among many others – affect a wide range of adult outcomes (see, e.g., Adhvaryu et al. (2014); Almond (2006); Bharadwaj et al. (2014, 2013b); Duque (2014); Maccini and Yang (2009); Persson and Rossin-Slater (2014)).

decreased years of completed education 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 education policy, each additional year of exposure mitigated the long-term impact of rainfall shocks on education by about 0.1 years.[2] At the average years of program exposure (4.84), cash transfers mitigated 84 percent of the disadvantage caused by adverse rainfall at birth, at least in terms of educational attainment. 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.

Put another way, there is substantial heterogeneity in the treatment effect of *Progresa* across the distribution of initial endowments, as determined by economic circumstance in early life. The effect of conditional cash transfers on schooling in our case is driven in large part by the impact on disadvantaged children. At the mean length of program exposure, children born in "normal" circumstances get around 0.5 years of additional schooling. But program exposure increases schooling for disadvantaged children by double this amount – slightly over 1 year.

Our study furthers the understanding of a fundamental question regarding the production of child quality: how do initial endowments and subsequent investments interact to determine long-run outcomes (Bhalotra and Venkataramani, 2011; Bitler et al., 2014; Cunha et al., 2013, 2010)? Our attempt to answer this question exploits two orthogonal sources of variation: exposure to abnormal rainfall around the time of birth and exposure to a large-scale randomized conditional cash transfer program. In this regard, our work is most related to three recent working papers: Gunnsteinsson et al. (2014), who examine the interaction of a natural disaster and a randomized vitamin supplementation program in Bangladesh; Rossin-Slater and Wüst (2015), who study the interaction of nurse home visitation and high quality preschool daycare in Denmark; and Aguilar and Vicarelli (2011), who analyze the interaction between *El Niño* shocks and contemporaneous *Progresa*-related transfers in Mexico. Despite the vastly

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[2]See Skoufias and Parker (2001), Behrman et al. (2009) and Behrman et al. (2011) for a detailed description of the program and some of its effects.

different contexts and types of programs studied, the results of the first two papers mentioned mirror what we find in our work–a negative interaction effect–indicating that remediation of early-life shocks via investments can indeed be successful. Aguilar and Vicarelli (2011) do not find remediation for children's anthropometric or cognitive outcomes, suggesting that while cash infusions can generate catch-up in schooling, they may not substitute directly for initial declines in the stock of health.

Part of the implicit argument for targeting low-endowment children is the idea that the return on investment is highest for this group, but we do not have credible evidence that this is indeed the case. While there is substantial evidence that early interventions for disadvantaged children can have large long-term impacts (Bharadwaj et al., 2013a; Bleakley, 2007, 2010; Gould et al., 2011; Heckman et al., 2010, 2013; Hoynes et al., 2012; Katz et al., 2001), we know little about how large are those returns compared to the returns of similar intervention on less disadvantaged populations. The ethical imperative for parents, communities, and the government to improve the circumstance of disadvantaged children may be clear. But if returns to investment are highest for high-endowment children (i.e., if "skill begets skill"), then this moral argument would be at odds with the economic drive to invest where return is largest. Our results show that in the case of schooling choices, children disadvantaged at birth are actually the highest-return beneficiaries of remediating investments. This result is consistent with new evidence from the Head Start program in the United States (Bitler et al., 2014).

We also contribute to the growing literature on the impacts of cash transfers to low-income households, adding to the smaller subset of studies that look at longer run outcomes (Behrman et al., 2011; Blattman et al., 2013; Haushofer and Shapiro, 2013; Schultz, 2004). Despite a large body of research on conditional cash transfers and *Progresa* in particular, there is still much to be learned about the ability of cash transfers to mitigate the impact of shocks. De Janvry et al. (2006) find that *Progresa* protected child enrollment from declining due to contemporaneous climatic shocks, but Aguilar and Vicarelli (2011) find that the program did not mitigate the negative impact of a 1999 El Nino shock on child development. Instead of contemporaneous shocks, we look at shocks that took place over a decade prior to the beginning of the program

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and find strong evidence for mitigation in terms of educational attainment.

Our empirical exercise is appealing because of its relatively high degree of external validity. Adverse rainfall is the most common type of shock in much of the developing world (Dinkelman, 2013), and has large short- and long-term consequences for low-income households (Maccini and Yang, 2009; Paxson, 1992; Shah and Steinberg, 2013). Given the rising importance of wide-scale cash transfer programs around the world, it is important to learn here that these programs, if administered as successfully as *Progresa* was in Mexico, can mitigate a sizable portion of the adverse impacts of poor rainfall around 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. Section 3 describes our empirical strategy. Section 4 details our results. Section 5 concludes.

1 Program Background

In 1997, the federal government of Mexico started a conditional cash transfer program called *Progresa*, aimed at alleviating poverty and improving the health, education and nutritional status of poor families, particularly children and mothers, in small rural communities.

In this paper, we focus on the education component of *Progresa*, which consists of bimestrial cash payments to mothers during the school year, contingent on their children's regular school attendance (an attendance record of 85% is required to continue receiving the grant). Initially ranging from 60 to 205 pesos in 1997, the size of the subsidy depends on the number of children enrolled in school and the grade levels and genders of the children. As shown in Table 1, from seventh grade onwards, the grants increase with grade level, with higher amounts for girls than boys.[3] At the beginning, the program provided grants only for children between third and ninth grade (3rd year of secondary). In 2001, the grants were extended to high school. Table 1 summarizes the monthly grant amounts for the second semester of 1997,

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[3]Given the lower rates of attendance of girls in rural Mexico the idea was to provide additional incentives to girls ((Skoufias, 2005)). See Skoufias and Parker (2001), Behrman et al. (2009) and Behrman et al. (2011) for more details about the program.

1998 and 2003.

Table 1: Monthly Amount of Educational Transfers to Beneficiary Households

| | 1997 | | 1998 | | 2003 | |
	Boys	Girls	Boys	Girls	Boys	Girls
Primary						
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
Secondary						
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.

The program was implemented using an experimental design on 506 rural localities from the states Guerrero, Hidalgo, Michoacan, Puebla, Queretaro, San Luis de Potosi and Veracruz. From the 506, 320 localities (which we call the treatment group) were randomly assigned to receive benefits around March and April of 1998. 186 localities were kept as a control group and started receiving *Progresa* benefits at the end of 1999. We take advantage of this random assignment and treat *Progresa* as an exogenous shock to the cost of schooling. Because only households who were classified as poor by the program administration were eligible to receive the benefits from the program, we focus on this subset of the population in our analysis. The next section describes the surveys conducted as part of this program and identifies the specific datasets and variables used for this paper.

2 Data

2.1 *Progresa* Data

The data collected for the *Progresa* program includes a baseline survey of all households in *Progresa* villages (not just eligible poor households) in October 1997 and follow-ups every six months thereafter for the first three years of the program (1998 to 2000). These surveys collect detailed information on many indicators related to household demographics, education, health, expenditures, and income.

To evaluate the medium-term impact of the program, a new follow-up survey was carried out in 2003 in all 506 localities that were part of the original evaluation sample. By that time all localities that had participated in the baseline survey as control localities had also received the treatment. Like previous surveys, the 2003 one contains detailed information on household demographics and individual socioeconomic, health, and schooling outcomes. A follow-up survey was also conducted in 2007, but we do not use this wave due to high attrition rates.

In this paper we use data from the first survey and the survey carried out in 2003, focusing only on households who were eligible for the program ("poor" households). We construct different education outcomes using the information provided by the 2003 follow-up survey. Similarly, based on the findings of Behrman and Todd (1999) and Skoufias and Parker (2001), we also construct control variables related to parental characteristics, demographic composition of the household, and community level characteristics using the baseline survey.

We focus on individuals in poor households aged 12 to 18 in 2003. We restrict to these ages because 12 year-olds are the youngest cohort for which there is differential exposure to *Progresa* in treatment and control villages (see Table A1), while individuals over 18 are likely to be living outside of the household during the 2003 survey and are thus excluded for data quality reasons. As Figure A1 shows, the proportion of 19-year-olds not living in the household is over 40%, and this proportion continues to grow with age. While survey respondents (usually mothers or grandmothers) are still asked about these individuals, these responses are likely to introduce greater measurement error.

Following Behrman et al. (2011), we drop individuals who have non-matching genders across the 1997 and 2003 waves, as well as those who report birth years that differ by more than 2 years. For those with non-matching birth years, we use the birth year reported in the 1997 wave.

2.2   Rainfall Data

We also exploit variation in early life rainfall to identify changes in early-life circumstances not correlated with the initial conditions of the parents. We use rainfall data from local weather stations collected by Mexico's National Meteorological Service (CONAGUA) and match those rainfall stations to the localities of the program using GPS coordinates. Due to the changes in the weather stations in use as well as irregular reporting by some stations, there are some localities for which the nearest rainfall station has missing observations for the period we are interested in. In order to deal with this, we use data from all of the stations within a 20 kilometer radius of the locality. Then, we take a weighted average of rainfall from these nearby stations, weighting each value by the inverse of the distance between that station and the locality.[4] Using this procedure, 69 of the 506 localities were still missing rainfall measurements for our study period. 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.

2.3   Outcome Variables

Our main outcome variables include continuous years of schooling, a dummy for grade progression, and a dummy for having completed the appropriate years of schooling 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

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[4]Weights are normalized to add to one

achieved in 2003.[5] 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 years of schooling for their age. For an individual who is 7 years old, we expect them to have completed one year of schooling, for an 8 year-old, two years, and so on. In order to study differential effects by grade, we also use 12 dummy variables, each indicating whether the individual received at least 3, 4, and up to 12 years of schooling.

2.4 *Progresa* Exposure Variable

Our two dependent variables of interest represent two types of shocks: an early-life endowment shock and an investment shock. The investment shock we use is the *Progresa* program. In particular, we calculate the years an individual was exposed to *Progresa*, which depends on their locality (treatment or control status) and age. Table A1 shows, for each birth cohort, the number of years of exposure to *Progresa* by treatment status, calculated by first calculating the number of months, dividing by 12, and rounding to the nearest year. 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). In addition to the exogenous variation generated by the randomization of the *Progresa* program, creating a more continuous years of exposure variable takes advantage of the variation in exposure lengths across different age cohorts within the treatment and control groups.

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[5]Students with complete primary education have a maximum of 6 years of schooling; secondary school adds a maximum of three additional years; and high school three years more. College education adds a maximum of five additional years of schooling and graduate work an additional one. We do not count years in preschool and kindergarten.

2.5 Rainfall Shock Variable

For our early-life endowment shock, we use annual rainfall during an individual's calendar year of birth in their locality of residence in 1997.[6] To calculate the rainfall levels, we simply sum all monthly rainfall during an individual's calendar year of birth. We do not use month of birth to define this annual shock because in our sample, approximately 30% of individuals report different birth months in the 1997 and 2003 surveys. In robustness checks (not shown here but available on request), we find that our results using calendar-year annual rainfall are very similar to results using the sum of monthly rainfall from the 6 months before and 6 months after birth (using either the 1997 reported month or 2003 reported month). This suggests that most of the effects we find are coming from input shocks in the latest prenatal and earliest post-natal months.

Our interest is not in the absolute level of rainfall itself, but rather in a measure of rainfall that maps to household incomes at the time of birth (and therefore to a child's biological endowment). Specifically, we define a rainfall shock as one that involves a level of rainfall one standard deviation above or below the locality-specific mean (calculated over the 10 years prior to the birth year). In our analysis, we use a "normal rainfall" dummy in order to represent a positive shock rather than a negative shock (for ease of interpretation). This dummy equals 1 if the rainfall in an individual's locality during their year of birth fell within a standard deviation of the locality-specific mean.

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 section 3, both previous literature and the data show this shock variable appropriately captures the relationship between rainfall and agricultural wages: normal years are associated with better outcomes than shock years.

It is also important to note that this shock variable eliminates much (but not all) of the spatial correlation that typically poses a problem in studies of rainfall, a highly spatially cor-

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[6]The data does not include locality of birth, which would be the ideal geographic identifier to use; we therefore use locality of residence in 1997, which should be equivalent for most of the individuals in our sample as they are relatively young.

related variable. This is illustrated in Figure 1, which maps all *Progresa* localities by their rain-fall status. Black diamonds represent localities that experienced a rainfall shock (according to our definition) in 1987, while gray crosses represent those that experienced normal rainfall in that same year. We see a great deal of variation within states, and even within clusters of neighboring localities, in the rainfall shock variable.[7] We picked one year for the simplicity of illustration (which maps well to our estimating equation, which includes birth year fixed effects), and chose 1987 because it is the birth year of the largest number of individuals in our sample. In the Appendix, Table A2 uses rainfall from all birth years.

Figure 1: *Progresa* Localities by Rainfall Shock in 1987
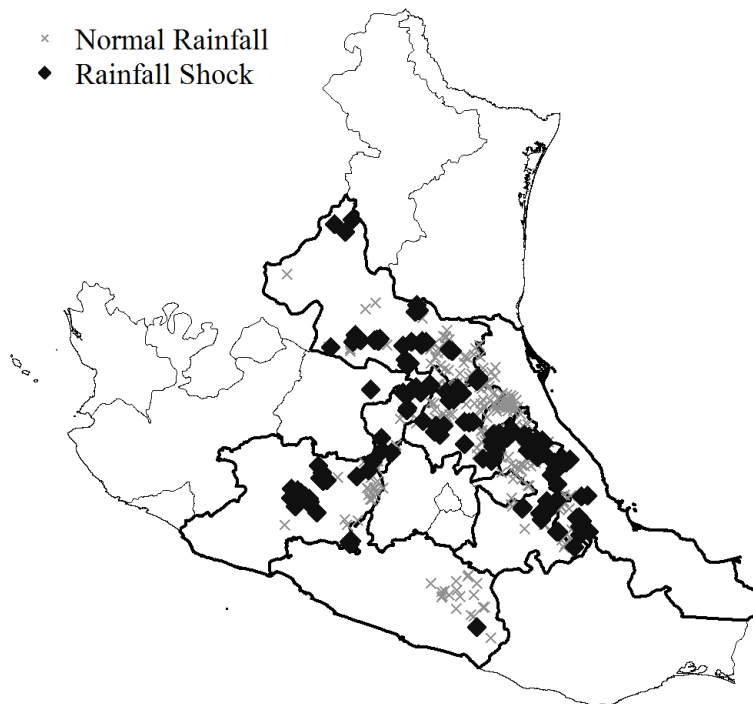
Since we ultimately care about the interaction between rainfall and *Progresa* exposure, it is also important to note that for both treatment and control villages, we see still substantial variation in rainfall shock status, even within small geographic areas, as shown in Figure 2.

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[7]While it may be surprising to see some localities situated so close together take on different values for this shock variable, we are able to detect these differences because of the vast number of rainfall stations (most localities have several stations within 20km) as well as our use of inverse-distance weighting, which can assign even very closely situated localities with different rainfall values.

Figure 2: *Progresa* Localities by Treatment Status and Rainfall Shock in 1987

Treatment Localities

× Normal Rainfall
◆ Rainfall Shock

Control Localities

× Normal Rainfall
◆ Rainfall Shock
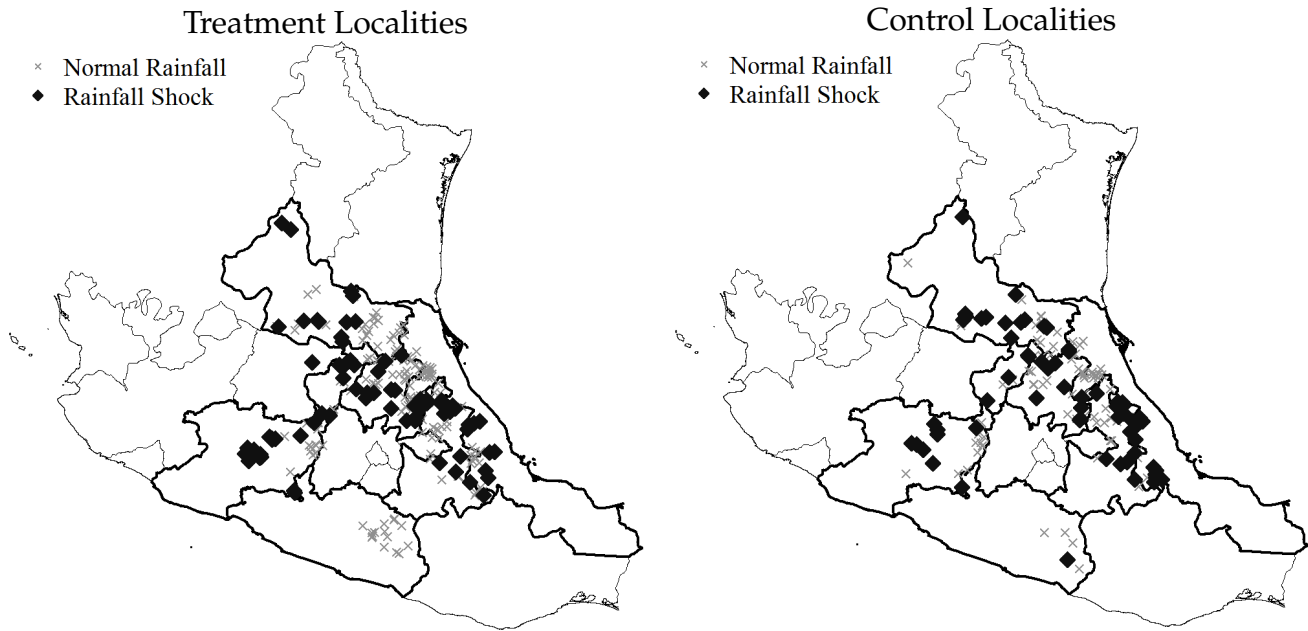
2.6 Summary Statistics

Table 2: Summary Statistics for Individual-Level Variables

	Treatment Villages	Control Villages	Full Sample	Treatment - Control Differences
Educational Attainment	6.847	6.692	6.786	0.154***
	(2.094)	(2.128)	(2.109)	(0.0397)
Grade Progression	0.591	0.561	0.579	0.0295***
	(0.492)	(0.496)	(0.494)	(0.00955)
Appropriate Grade Completion	0.479	0.442	0.465	0.0366***
	(0.500)	(0.497)	(0.499)	(0.00939)
Male	0.512	0.506	0.509	0.00593
	(0.500)	(0.500)	(0.500)	(0.00929)
Number of individuals	7193	4636	11829	

(*** $p<0.01$, ** $p<0.05$, * $p<0.1$).

Table 2 reports summary statistics for individual-level variables for our two samples of interest. Average educational attainment is 6.8 years for the pooled sample, with individuals in treatment villages receiving on average 0.154 more years of schooling than control

villages. This difference is significant at the 1% level. Similarly, the proportion of children who progressed at least 5 grades from 1997 to 2003 and the proportion that completed the appropriate number of years of schooling for their age is significantly higher in the treatment villages, which received 1.5 more years of treatment than the control villages. In the next section, we outline how we analyze these differences in more robust specifications, controlling for covariates and taking into account heterogeneous impacts for individuals with different endowments.

Table 3: Summary Statistics for Shock Variables

	Treatment Villages	Control Villages	Full Sample	Treatment - Control Differences
A. Full Sample				
Years of Progresa exposure	5.574	3.695	4.841	1.879***
	(0.727)	(0.720)	(1.168)	(0.0296)
Annual rainfall in year of birth	1180.6	1185.3	1182.4	-4.752
	(654.8)	(628.0)	(644.3)	(26.32)
Rainfall Shock	0.223	0.272	0.242	-0.0483***
	(0.417)	(0.445)	(0.428)	(0.0175)
Number of locality-birth-year observations	1536	983	2519	
B. Trimmed Sample				
Years of Progresa exposure	5.576	3.707	4.812	1.869***
	(0.724)	(0.707)	(1.166)	(0.0313)
Annual rainfall in year of birth	1171.1	1195.5	1181.1	-24.43
	(654.8)	(628.0)	(644.0)	(28.12)
Rainfall Shock	0.266	0.294	0.277	-0.0279
	(0.442)	(0.456)	(0.448)	(0.0195)
Number of locality-birth-year observations	1282	888	2170	

(*** p<0.01, ** p<0.05, * p<0.1).

Table 3 reports summary statistics for the variables related to our two shocks, *Progresa* exposure and rainfall. Years of *Progresa* exposure, annual rainfall during the year of birth, and occurrence of a rainfall shock all vary at the locality birth-year level. Summary statistics are calculated accordingly and reported in two panels, one for the full sample and one for a trimmed sample described below. By experimental design, treatment villages were exposed to *Progresa* for longer than control villages. On average, treatment individuals received 1.9 more years of *Progresa*: the treatment-control difference is 2 years for the majority of cohorts,

but 1 for the youngest cohort, as shown in Table A1). Mean rainfall, both in raw levels and in normalized terms, is not significantly different across treatment and control villages.

However, there appears to be a small but statistically significant difference in prevalence of a one-standard deviation shock between treatment and control villages. 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 (especially because, as we describe in section 3, we control for the main effects of *Progresa* and rainfall and focus on the sign of the interaction). 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 Figure 3 that the rainfall distributions for treatment and control indeed share a common support are actually quite similar overall. Moreover, looking at Figure 2, it is clear that though there are more shocks in the treatment group, the spatial distribution of rainfall shocks are similar across the two groups (and both quite disperse).

Figure 3: Normalized Rainfall Distributions in Treatment and Control Villages
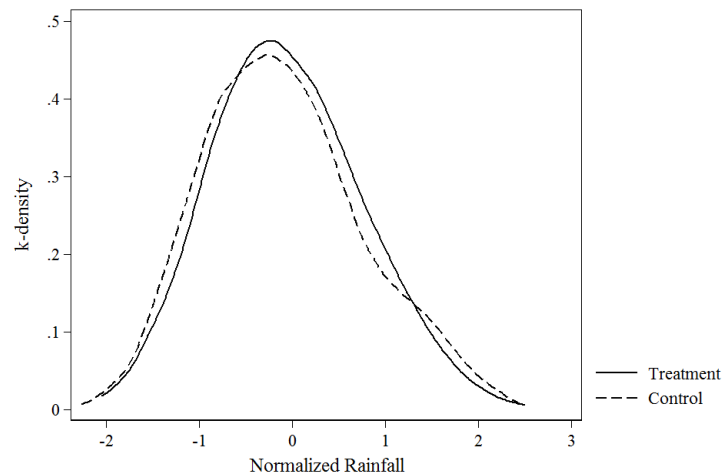
Nevertheless, in order to alleviate concerns that this imbalance is driving our results, we also trim the sample by excluding localities that could be considered outliers. That is, we drop any localities that either experienced no rainfall shocks throughout the sample period or only experienced rainfall shocks throughout the period. As shown in Panel B of Table 3, this trimming results in a sample of balanced rainfall shocks across treatment and control. Figure 4,

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which maps this trimmed sample, is not noticeably different from Figure 2, emphasizing that this trimming did not substantially change the distribution of rainfall shocks (by removing localities only from a particular area, for example). Lastly, in the results section below, we calculate and plot treatment effects non-parametrically along the entire common support of the rainfall distributions for treatment and control villages, guaranteeing that average treatment effects are not picking up artefacts due to a lack of common support.

Figure 4: *Progresa* Localities by Treatment Status and Rainfall Shock in 1987, Trimmed Sample
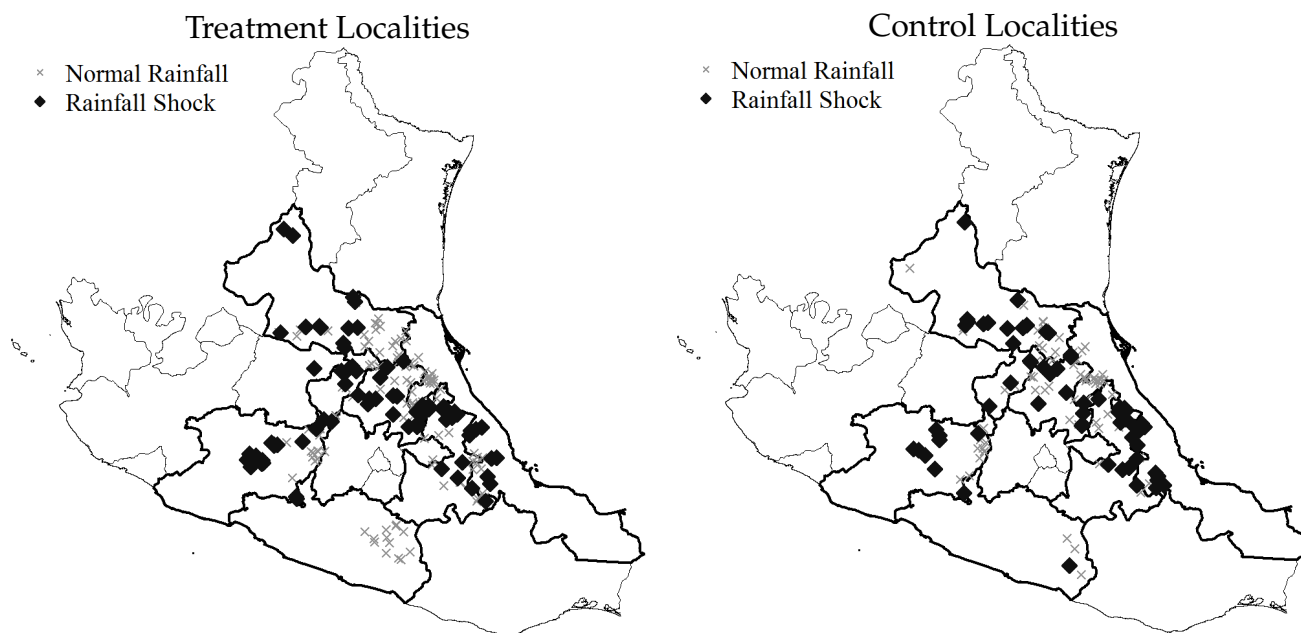
Despite the randomized nature of the *Progresa* experiment, previous literature has found that some household-level and locality-level characteristics are not fully balanced across treatment and control villages (Behrman and Todd, 1999). For this reason, in keeping with empirical methods used in previous studies of *Progresa* impacts, we include a rich set of controls that are summarized in Appendix Table A2. At the household level, the sample is fairly balanced across the groups with the exception of household head age, several household composition variables, two parental education variables, and father's language. At the locality-level, access to a public water network as well as garbage disposal techniques are significantly dif-

ferent across treatment and control villages, at the 10% level. We control for all of these house-

hold and locality-level in our regression analysis, which we outline in the following section.

3   Empirical Strategy

In this section, we describe the empirical strategy we use to identify the effects of an endow-

ment shock, an investment shock, and their interaction on educational attainment. We use

rainfall during an individual's year of birth as a shock to that individual's biological endow-

ment. Maccini and Yang (2009) have shown that early-life rainfall shocks can impact adult

outcomes like health and educational attainment, and this operates through the positive im-

pact rainfall has on agricultural output in rural settings. Increased household income means

increased nutritional availability for the fetus or infant during a crucial stage of development.

Like the Indonesian villages in Maccini and Yang (2009), the *Progresa* villages are also rural,

suggesting that rainfall also serves as an important income shock to these communities. Bobo-

nis (2009) confirms that negative rainfall shocks have a large negative impact on household

expenditures in rural Mexico.

Unlike in Indonesia, however, where the relationship between rainfall and income appears

to be more monotonic, Bobonis (2009) finds that expenditures can be negatively impacted by

large deviations from the mean in either direction. Specifically, he finds that rainfall shocks,

defined as monthly rainfall above or below one standard deviation from the historical mean,

reduce household expenditures by 16.7%. In the same setting as Bobonis (2009), we allow for

droughts and floods to both have negative impacts on household income. Using locality-level

wages reported by village leaders in the *Progresa* data, we show graphically that this is indeed

the appropriate relationship to use. Figure 5 depicts the relationship, using local polynomial

smoothing, between average male wages from the 2003 surveys and normalized rainfall in

that same year (using the mean and standard deviation of the 10-year historical mean). The

clear 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. Motivated by this figure and

the prior literature, we define a shock as a realized rainfall level that is over one standard deviation above or below the locality-specific mean calculated over the 10 years prior.

Figure 5: Locality Wages
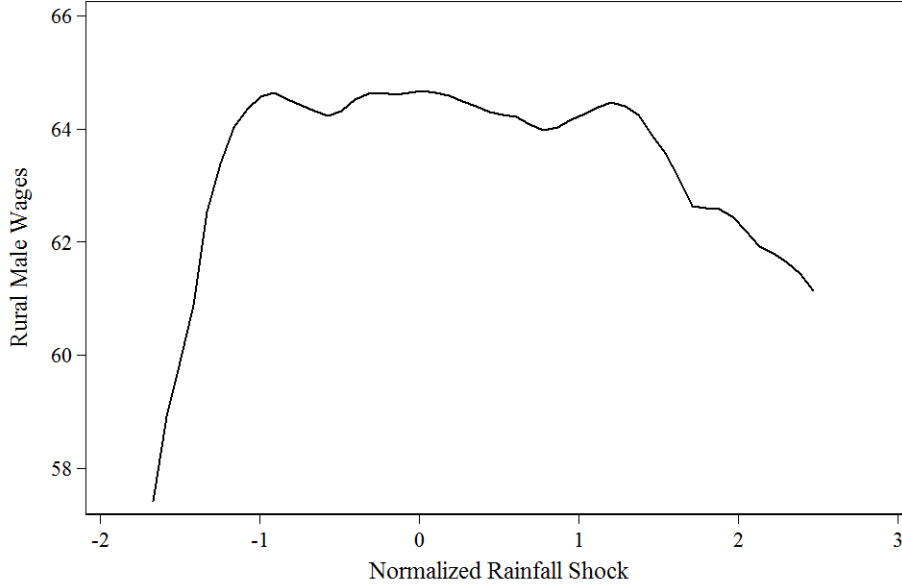
Our investment shock, which is the total number of years of *Progresa* exposure, also depends on the year of birth and locality of residence during the *Progresa* program. The rainfall shock, years of exposure, and their interaction form the basis of our empirical specification.

For individual i, living in state s and locality l at the beginning of the *Progresa* program, born in year t, their education outcomes y_{islt} can be expressed as follows:

$$y_{islt} = \beta_1 R_{slt} + \beta_2 P_{slt} + \mathbf{X}'_{islt}\alpha + \mu_s \times \delta_t + \epsilon_{islt} \tag{1}$$

where R_{slt} represents a normal rainfall dummy, indicating that rainfall during the individual's year of birth was within one standard deviation of the ten-year locality-specific mean. In order for this variable to be interpreted as a positive endowment shock (in the same way *Progresa* is seen as a positive investment shock), we use a 1 to indicate a normal year (or absence of a shock) and 0 to indicate a shock year. P_{slt} represents the number of years of *Progresa* exposure, which varies across treatment and control villages as well as across different

birth cohorts within villages. Our basic specification includes state-by-birth-year fixed effects ($\mu_s \times \delta_t$). In some specifications we add municipality fixed effects, which is the smallest set of geographic fixed effects we can use, given that one of our primary sources of exogeneity – the *Progresa* randomization – varies at the locality level.

In our base specification, we cluster our standard errors at the municipality level, which is a larger administrative unit than the locality, in order to allow for correlation in the error terms across individuals in the same municipality. In addition to this, we also show standard errors that adjust for spatial correlation (unrelated to administrative boundaries) using the methods described in Conley (1999). As discussed in section 2.5, using a rainfall shock dummy instead of rainfall levels reduces the spatial correlation in our independent variable of interest, but we correct our standard errors for any spatial correlation that may remain. We show two sets of standard errors that allow for spatial correlation. First, we allow for dependence between observations located less than 100km apart, but no dependence between those further than that. Our second weighting function allows for dependence between observations up to 500km apart. For both of these standard errors, we impose a weight that decreases linearly in distance until it hits zero at the relevant cutoff point.

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 control for some significant differences across treatment and control villages that exist despite the randomization, and also to increase precision. All of our specifications include controls for individual gender, household size, household head age, household head gender, household composition variables,[8] 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 in the locality.

Although parental education and language (specifically, a dummy for whether the parents speak the indigenous language) are important controls (Behrman et al., 2011; Schultz, 2004; Skoufias and Parker, 2001), these are missing for over 30% of the sample, and around 10%

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[8]These include counts of the number of children aged 0-2, children aged 3-5, males aged 6-7, males aged 8-12, males aged 13-18, females 6-7, females aged 8-12, females aged 13-18, females aged 19-54, females aged 55 and over, and males aged 55 and over.

of the sample, respectively. Similarly, distance to secondary school and distance to bank are missing for 58% and 12% of localities. In order to include these variables without reducing sample size, we control for missing values instead of dropping missing observations. Parental education and parental language are represented by a set of dummy variables, with the omitted category representing a dummy for missing.[9] Similarly, distance to bank and distance to secondary school are set to zero for missing observations but missing dummies for each variable are added to the specification.

In equation 1, β_1 represents the average effect of a positive early-life shock on our outcomes of interest, while β_2 represents the average effect of a positive investment shock: specifically, we measure the effect of one more year of exposure to *Progresa*, which incentivized and decreased the opportunity cost of schooling. This specification, however, does not take into account potential heterogeneity in the effect of the investment shock on individuals with different endowments. The following specification adds an interaction term, which takes this heterogeneity into account.

$$y_{islt} = \beta_1 R_{slt} + \beta_2 P_{slt} + \beta_3 R_{slt} P_{slt} + \alpha' X_{islt} + \mu_s \times \delta_t + \epsilon_{islt} \tag{2}$$

Now, β_1 represents the main effect of a positive early-life income shock, and β_2 represents the effect of a positive investment shock for individuals who did not experience a positive rainfall shock. $\beta_2 + \beta_3$ represents the total effect of the *Progresa* shock on individuals who also experienced a positive rainfall shock, and β_3 therefore gives us the differential effect of *Progresa* for the higher endowment individuals (who experienced a positive shock). If β_3 is positive, this would suggest that *Progresa* had a larger effect for higher endowment individuals than lower endowment individuals, while a negative β_3 would suggest the opposite: that *Progresa* helped to mitigate the negative impact of an early life shock.

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[9]For parental education, the included dummies are less than primary school completion, completion of primary school, and completion of secondary school; for parental language, the included dummies are a dummy for speaking the indigenous language and a dummy for not speaking the indigenous language

4 Results

4.1 Education Results

Figure 6: Years of Educational Attainment by Rainfall in Year of Birth
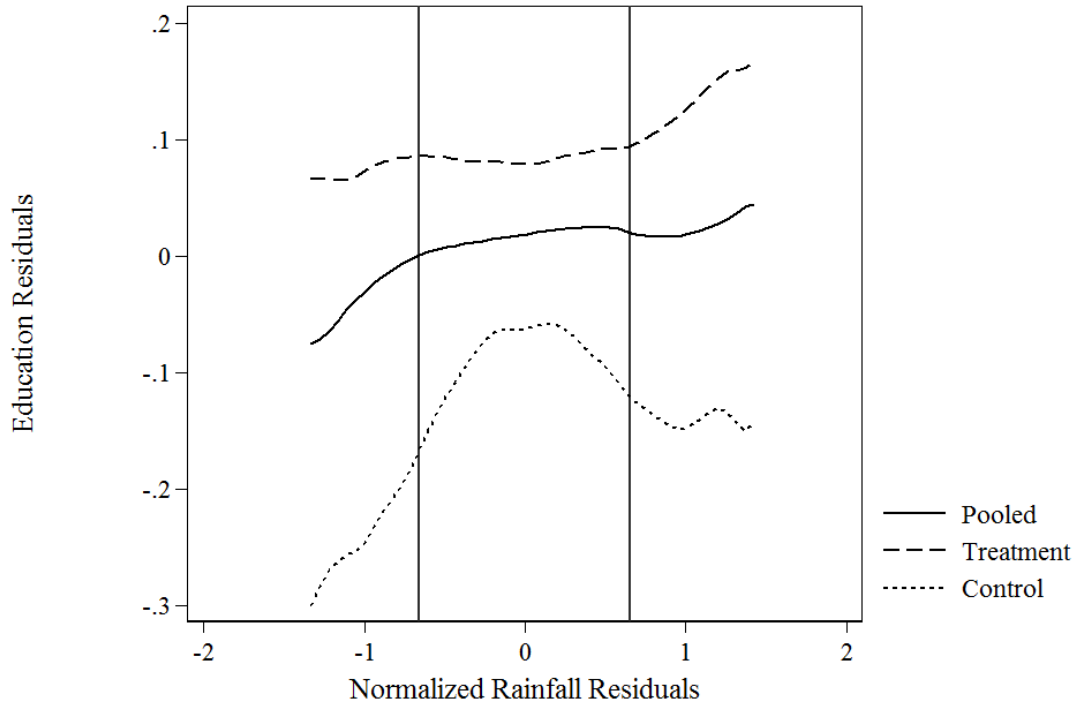
Notes: Vertical lines depict one standard deviation above and below the mean of rainfall residuals. Rainfall is trimmed at the 2nd and 98th percentiles.

Figure 6 graphically illustrates the intuition underlying our identification strategy, using local polynomial smoothing to depict the relationship between rainfall at birth and educational attainment. We first regress educational attainment and normalized rainfall deviations 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 in the figure the nonparametric relationship between the residuals from those regressions (educational attainment residuals on the y axis and 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 can be harmful. Comparing the dotted control group line to the dashed treatment line, there are two important features to note. First, 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*. Secondly, the distance between the treatment and control lines is smallest around a normalized rainfall deviation residual of zero and grows larger in the tails (below and above one standard deviation in terms of the residuals, depicted by the vertical lines). Furthermore, the treatment line is essentially flat, as compared to the control line, indicating that *Progresa* offset nearly entirely the impacts of extreme rainfall at birth on educational attainment.

Figure 7: Treatment Effect on Years of Educational Attainment by Endowment

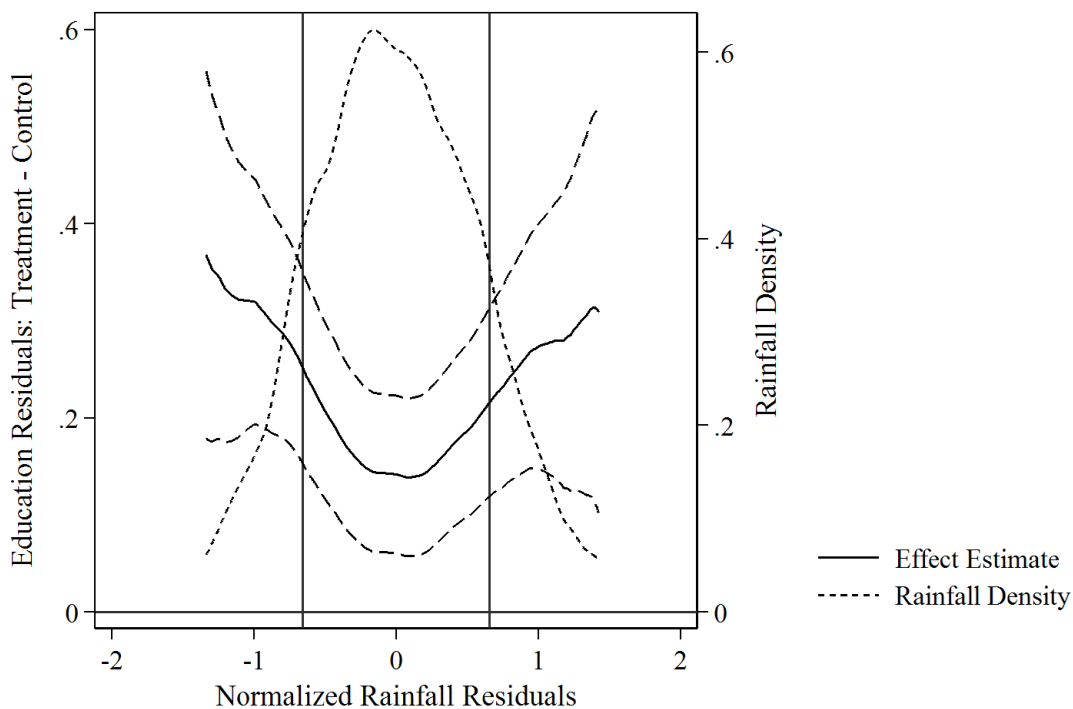Notes: Dashed lines represent the 95% confidence interval around the difference. Horizontal line depicts difference of 0. Vertical lines depict one standard deviation above and below the mean of rainfall residuals. Rainfall is trimmed at the 2nd and 98th percentiles.

Figure 7 plots the difference (solid line) between the treatment and control lines in Figure 6, along with confidence interval bands (dashed lines) and rainfall probability density (dotted

Figure 8: Treatment Effect on Grade Progression by Endowment

Notes: Dashed lines represent the 95% confidence interval around the difference. Horizontal line depicts difference of 0. Vertical lines depict one standard deviation above and below the mean of rainfall residuals. Rainfall is trimmed at the 2nd and 98th percentiles.
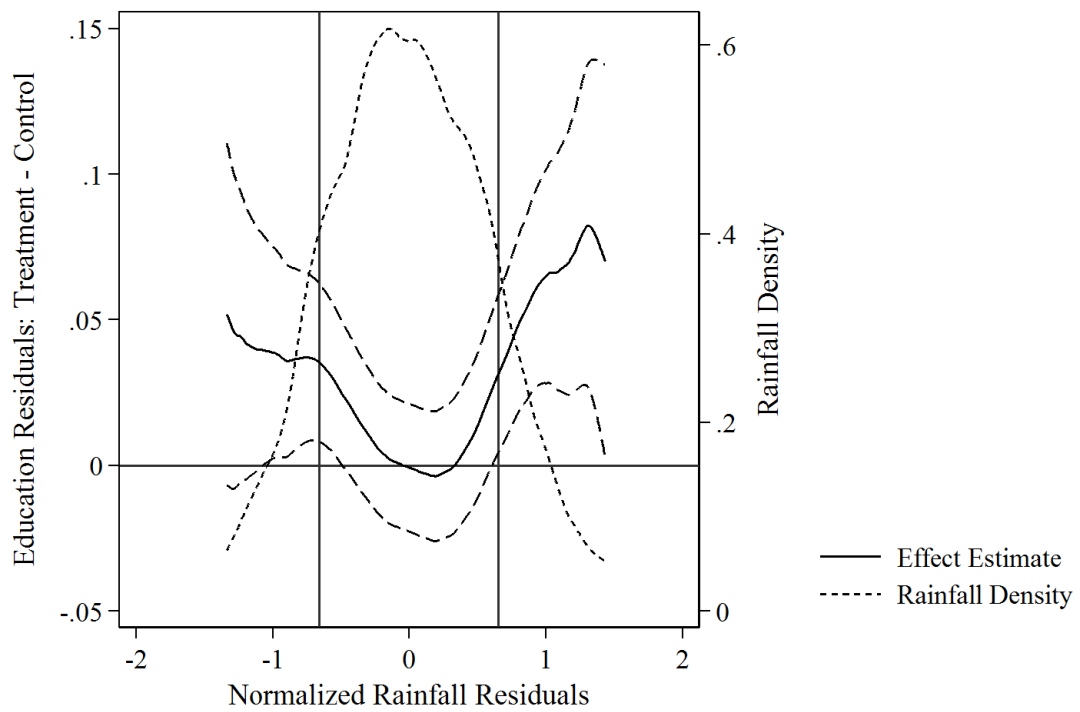
Figure 9: Treatment Effect on Appropriate Grade Completion by Endowment

Notes: Dashed lines represent the 95% confidence interval around the difference. Horizontal line depicts difference of 0. Vertical lines depict one standard deviation above and below the mean of rainfall residuals. Rainfall is trimmed at the 2nd and 98th percentiles.
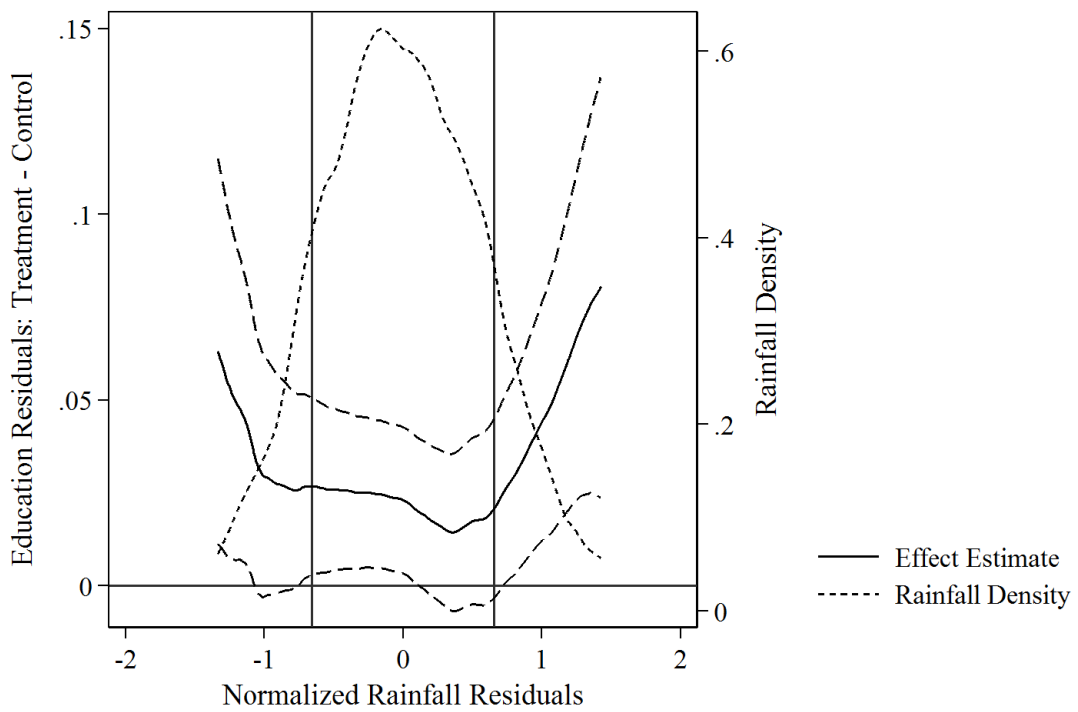
line). This figure, accordingly, illustrates graphically the main result of the paper. Figures 8 and 9 plot the same results from the corresponding exercises for the other two education outcomes (grade progression and appropriate grade completion, respectively). In these figures, the larger treatment effects in the tails of the rainfall distribution are key. While *Progresa* had positive impacts on education outcomes overall, its greatest impact was on the most disadvantaged, those who experienced the worst rainfall shocks during their year of birth. This focused nature of the *Progresa* impact on education suggests that investments may be able to mitigate the negative impacts of early life shocks.

Note also that concern regarding the degree to which imbalance in the prevalence of rain shocks across treatment and control villages could be driving our results should be alleviated by Figures 7 through 9. These plots illustrate the pattern of main results squarely on the common support of the rainfall distributions for treatment and control villages.

Next, we report the analogous parametric regression estimates from the specifications discussed in Section 3. Panel A of Table 4 displays the results from specification 1, which includes 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. In column 1, one year of *Progresa* exposure leads individuals to obtain 0.129 more years 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 0.1935 years, which is consistent with previous work by Behrman et al. (2009, 2011), which estimated a treatment effect of 0.2 years (using a slightly different sample).

Individuals who did not experience a negative rainfall shock at birth show a similarly sized boost in educational attainment of 0.102 years, marginally significant using the first two types of standard errors reported. Since our sample includes children who may not have com-

pleted their schooling yet, we also look at the two other variables that adjust for age. Grade progression is positively impacted by both years of exposure and normal rainfall, although these coefficients are not generally significant at the 5% level. In column 3, we see that *Progresa* and normal rainfall have positive and significant impacts on appropriate grade completion.

Table 4: Effects of *Progresa* and Rainfall on Education Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of Education	Grade Progression	Appropriate Grade Completion	Years of Education	Grade Progression	Appropriate Grade Completion
Panel A: Main Effects Only						
Years of Progresa Exposure	0.129	0.0145	0.0167	0.0423	-0.00819	-0.00610
	(0.0365)***	(0.00960)	(0.00740)**	(0.0462)	(0.0121)	(0.0109)
	[0.0257]***	[0.00628]**	[0.00638]***	[0.0327]	[0.00849]	[0.00817]
	{0.0205}***	{0.00543}***	{0.00660}**	{0.0314}	{0.00774}	{0.00918}
No Rainfall Shock	0.102	0.0119	0.0272	0.0664	-0.000747	0.0205
	(0.0557)*	(0.0145)	(0.0117)**	(0.0539)	(0.0138)	(0.0110)*
	[0.0617]*	[0.0147]	[0.0133]**	[0.0499]	[0.0124]	[0.0120]*
	{0.0677}	{0.0154}	{0.0146}*	{0.0487}	{0.0123}	{0.0117}*
Panel B: Main Effects and Interaction						
Years of Progresa Exposure	0.217	0.0304	0.0315	0.145	0.0107	0.0136
	(0.0546)***	(0.0132)**	(0.0110)***	(0.0582)**	(0.0149)	(0.0140)
	[0.0456]***	[0.0111]***	[0.00970]***	[0.0428]***	[0.0112]	[0.0112]
	{0.0562}***	{0.0118}**	{0.00909}***	{0.0435}***	{0.0106}	{0.0108}
No Rainfall Shock	0.648	0.111	0.120	0.703	0.116	0.142
	(0.279)**	(0.0556)**	(0.0506)**	(0.267)***	(0.0570)**	(0.0536)***
	[0.271]**	[0.0583]*	[0.0487]**	[0.227]***	[0.0484]**	[0.0477]***
	{0.340}*	{0.0646}*	{0.0474}**	{0.247}***	{0.0458}**	{0.0433}***
No Shock x Exposure	-0.112	-0.0203	-0.0189	-0.130	-0.0238	-0.0248
	(0.0531)**	(0.0109)*	(0.0102)*	(0.0509)**	(0.0114)**	(0.0107)**
	[0.0528]**	[0.0121]*	[0.0102]*	[0.0435]***	[0.00955]**	[0.00956]***
	{0.0623}*	{0.0130}	{0.00911}**	{0.0452}***	{0.00859}***	{0.00813}***
Observations	11824	11216	11824	11824	11216	11824
Mean of Dependent Variable	6.787	0.579	0.465	6.787	0.579	0.465
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).
-"No rainfall shock" = 1 for individuals whose birth-year rainfall was within one standard deviation of 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 characteristic dummies. Controls for parental language/education and locality distance include dummies for missing values

In our more stringent specification with municipality fixed effects, none of the main effects are significant at the 5% level. These preliminary results, however, do not allow the investment shock to have heterogeneous impacts on individuals with different endowments. We therefore move on to estimate specification 2, which includes the interaction term between the endowment and investment shock. Table 4 displays the results from this specification in Panel B. Again, columns 1 to 3 show the results with the baseline set of controls, while columns 4 to 6 add the municipality fixed effects. Again, we report three sets of standard errors, which are generally quite similar. For educational attainment in the base specification, the main effects of *Progresa* and normal rainfall are positive and significant while the interaction is negative and significant, all at the 5 % level (or the 10% level when using the 500km Conley standard errors errors). The same pattern holds for grade progression and appropriate grade completion.

Compared to the coefficients in Panel A, both the size and the significance of the main effects increase with the inclusion of the interaction. The coefficient on *Progresa* exposure in Panel B represents the effect of *Progresa* for those who experienced a negative rainfall shock. The fact that this is larger than the main effects in Panel A suggests that *Progresa* had a larger impact on those with a lower endowment, which is verified by the significant negative interaction terms. Looking at the magnitude of our estimates, having normal rainfall during the year of birth increases schooling by 0.648 years in our base specification; and although *Progresa* increases educational attainment for lower-endowment individuals by 0.217 years, it only increases educational attainment for higher-endowment individuals by 0.105 years (still positive and significant), suggesting that educational outcomes respond less for children with an already high endowment.

Looking at the more stringent specification in columns 4 to 6, the pattern of the results is the same, with positive main effects and negative interaction effects, which here almost completely dwarf the positive main effects of *Progresa*. In the regressions on grade progression and appropriate grade completion, the main effects of *Progresa* are positive but not significant, likely due to lack of variation in treatment and control status within a large proportion

of municipalities. Although municipality fixed effects are appealing in the sense that they control for location-specific observables 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 our baseline specification 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 endowments. At the average length of *Progresa* exposure (4.84 years), the program mitigated 84% of the disadvantage caused by the rainfall shock at birth. For grade progression and appropriate grade completion, the figures are similarly high: 89% and 76%, respectively.[10]

Table 5: Interaction Effects on Schooling Completion by Grade

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Primary School				*Secondary School*			*High School*		
	3 yrs	4 yrs	5 yrs	6 yrs	7 yrs	8 yrs	9 yrs	10 yrs	11 yrs	12 yrs
Years of Progresa Exposure	0.00224	0.0120	0.0183	0.0226	0.0501	0.0456	0.0421	0.0132	0.00656	0.00280
	(0.00380)	(0.00514)**	(0.00706)**	(0.00873)**	(0.0148)***	(0.0135)***	(0.0121)***	(0.00637)**	(0.00303)**	(0.00208)
	[0.00373]	[0.00494]**	[0.00629]***	[0.00780]***	[0.0115]***	[0.0113]***	[0.0102]***	[0.00615]**	[0.00312]**	[0.00240]
	{0.00410}	{0.00597}**	{0.00717}**	{0.00898}**	{0.0119}***	{0.0118}***	{0.0133}***	{0.00570}**	{0.00328}**	{0.00240}
No Rainfall Shock	-0.0122	0.00896	0.0311	0.0365	0.167	0.166	0.157	0.0512	0.0405	0.0161
	(0.0203)	(0.0283)	(0.0384)	(0.0473)	(0.0638)***	(0.0618)***	(0.0614)**	(0.0361)	(0.0189)**	(0.0136)
	[0.0186]	[0.0281]	[0.0339]	[0.0420]	[0.0613]***	[0.0608]***	[0.0594]***	[0.0286]*	[0.0194]**	[0.0159]
	{0.0205}	{0.0337}	{0.0351}	{0.0485}	{0.0640}***	{0.0633}***	{0.0709}**	{0.0261}**	{0.0221}*	{0.0194}
No Shock x Exposure	0.00203	-0.00253	-0.00515	-0.00471	-0.0267	-0.0303	-0.0291	-0.00923	-0.00637	-0.00273
	(0.00401)	(0.00536)	(0.00718)	(0.00897)	(0.0127)**	(0.0118)**	(0.0117)**	(0.00716)	(0.00356)*	(0.00251)
	[0.00369]	[0.00529]	[0.00657]	[0.00799]	[0.0126]**	[0.0126]**	[0.0119]**	[0.00599]	[0.00367]*	[0.00297]
	{0.00399}	{0.00614}	{0.00674}	{0.00872}	{0.0123}**	{0.0125}**	{0.0136}**	{0.00535}*	{0.00401}	{0.00360}
Observations	11824	11824	11824	11824	11824	11824	11824	11824	11824	11824
Mean of Dependent Variable	0.970	0.935	0.881	0.785	0.484	0.369	0.260	0.0610	0.0308	0.0123
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).
-"No rainfall shock" = 1 for individuals whose birth-year rainfall was within one standard deviation of 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 characteristic dummies. Controls for parental language/education and locality distance include dummies for missing values

In Table 5, we look at schooling completion by grade. We create separate dummy variables for the completion of 3 years to 12 years of school and estimate specification 2 using these

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[10]These proportions are calculated using the results from columns 1 to 3.

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 columns 2 to 9, we see that the impact of *Progresa* on completing grades 4 to 11 is positive and significant. The size of this main effect is largest in magnitude for the 7th year of schooling, which Behrman et al. (2011) highlight as a critical transition period (between primary and secondary school) during which many children drop out.

A comparison of the means of the dependent variables in Table 5 illustrates this clearly: completion of 6th grade is 78.5% but drops dramatically to 48.4% for 7th grade. This is clearly an important transition period, as it is also only starting in 7th grade that the main effect of normal rainfall becomes positive and significant. Prior to this, the high completion rates suggest that endowments may not matter much during this period, as the vast majority attend school and pass. Also starting in 7th grade, we see significant negative interaction coefficients that offer support for 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 over half of the size of the main effects of *Progresa*. These results are robust to the inclusion of municipality fixed effects, as shown in Table A3.

We are also interested in how our endowment and investment shocks interact to determine skill, not just educational attainment. We thus look at the Woodcock-Johnson dictation, word identification, and applied problems test scores available for a sample of the population, as a potential proxy for ability. We find no significant effects of *Progresa*, rainfall, or their interaction on these tests, which were administered to a sample of the population aged 15 to 21 in 2003 (results not reported here but available upon request). This is consistent with previous literature Behrman et al. (2009), which has found no impact of *Progresa* on test scores.

The lack of any *Progresa* impact on cognitive scores could potentially be due to low school quality as well as the absence of variation in *Progresa* exposure for the older ages in the sample of test-takers. For both the endowment and investment shocks, the smaller sample size also makes it difficult to detect their effects. Moreover, it is possible that the tests were unable to capture enough variation in skill or ability. In the letter-word identification test, for example,

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almost 30% of the sample answered everything correctly (and over 50% only made 2 mistakes) in a test of 58 questions. Given these issues, these test scores may not be an appropriate representation of skill, and educational attainment may actually be a better proxy, particularly in this setting of much lower rates of grade progression and completion than a developed country context.

Since our survey data do not allow us to track individuals after 21,[11] and since there is no variation in *Progresa* exposure after age 19 in the 2003 survey, we unfortunately cannot speak to socioeconomic outcomes in adulthood (e.g., wage/income and asset accumulation).

4.2 Robustness Checks

First, we discuss the implications of the small but statistically significant imbalance in rainfall shocks across *Progresa* treatment and control villages in our baseline sample. To verify that our results are not 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. As Tables 6 and 7 show, our results are virtually identical to the full sample results. Because our previous results revealed little difference across the three types of standard errors used, we only show standard errors clustered at the municipality level in this section.

As a second robustness exercise, we investigate how the *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 birth year level and count the total number of children born in a particular year in each locality. We then use this locality year panel to regress our rainfall shock on the total number of children born that year. Column 1 of Table 8 reports results from this regression. We find no evidence of selective

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[11]We do not use the 2007 survey due to high attrition rates.

Table 6: Effects of *Progresa* and Rainfall on Education Outcomes: Trimmed Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of Education	Grade Progression	Appropriate Grade Completion	Years of Education	Grade Progression	Appropriate Grade Completion
Panel A: Main Effects Only						
Years of Progresa Exposure	0.144***	0.0152	0.0191**	0.0832*	-0.00219	0.00265
	(0.0410)	(0.0108)	(0.00829)	(0.0499)	(0.0138)	(0.0119)
No Rainfall Shock	0.129**	0.0101	0.0321***	0.0698	-0.00624	0.0200*
	(0.0585)	(0.0151)	(0.0117)	(0.0578)	(0.0145)	(0.0114)
Panel B: Main Effects and Interaction						
Years of Progresa Exposure	0.234***	0.0315**	0.0352***	0.192***	0.0170	0.0235
	(0.0576)	(0.0138)	(0.0114)	(0.0611)	(0.0163)	(0.0146)
No Rainfall Shock	0.711**	0.116**	0.136**	0.767***	0.118**	0.154***
	(0.293)	(0.0579)	(0.0531)	(0.277)	(0.0594)	(0.0553)
No Shock x Exposure	-0.119**	-0.0216*	-0.0213**	-0.142***	-0.0253**	-0.0274**
	(0.0556)	(0.0113)	(0.0107)	(0.0528)	(0.0118)	(0.0111)
Observations	10236	9713	10236	10236	9713	10236
Mean of Dependent Variable	6.780	0.586	0.470	6.780	0.586	0.470
Fixed Effects	Birth year x state			Birth year x state, Municipality		

Notes:

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

-"No rainfall shock" = 1 for individuals whose birth-year rainfall was within one standard deviation of 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 characteristic dummies. Controls for parental language/education and locality distance include dummies for missing values

Table 7: Interaction Effects on Schooling Completion by Grade: Trimmed Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Primary School				*Secondary School*			*High School*		
	3 yrs	4 yrs	5 yrs	6 yrs	7 yrs	8 yrs	9 yrs	10 yrs	11 yrs	12 yrs
Years of Progresa Exposure	0.00317	0.0125**	0.0178**	0.0217**	0.0565***	0.0501***	0.0459***	0.0147**	0.00760**	0.00272
	(0.00399)	(0.00535)	(0.00723)	(0.00925)	(0.0151)	(0.0139)	(0.0125)	(0.00670)	(0.00323)	(0.00226)
No Rainfall Shock	-0.00988	0.00827	0.0325	0.0309	0.177***	0.187***	0.173***	0.0586	0.0479**	0.0166
	(0.0216)	(0.0295)	(0.0394)	(0.0503)	(0.0643)	(0.0633)	(0.0634)	(0.0393)	(0.0208)	(0.0151)
No Shock x Exposure	0.00199	-0.00214	-0.00487	-0.00308	-0.0278**	-0.0335***	-0.0316***	-0.0103	-0.00742*	-0.00277
	(0.00416)	(0.00554)	(0.00735)	(0.00950)	(0.0126)	(0.0121)	(0.0121)	(0.00778)	(0.00394)	(0.00286)
Observations	10236	10236	10236	10236	10236	10236	10236	10236	10236	10236
Mean of Dependent Variable	0.970	0.934	0.881	0.783	0.483	0.368	0.258	0.0610	0.0311	0.0124
Fixed Effects					Birth year x state					

Notes:

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

-"No rainfall shock" = 1 for individuals whose birth-year rainfall was within one standard deviation of 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 characteristic dummies. Controls for parental language/education and locality distance include dummies for missing values

Table 8: Effects of *Progresa* and Rainfall on Fertility

	(1)	(2)	(3)
	Locality-Level[1]	Individual-Level	
	Total Number of Children	Number of Younger Siblings	Birth Spacing (in days) between younger sibling
Years of Progresa Exposure		0.0228*	-13.79
		(0.0132)	(13.71)
No Rainfall Shock	0.0898	0.101	-29.07
	(0.161)	(0.0665)	(80.00)
No Shock x Exposure		-0.0202	6.373
		(0.0126)	(15.52)
Observations	2519	11686	7230
Mean of Dependent Variable	4.827	1.982	1107.9
Fixed Effects	Birth year x state	Birth year x state	Birth year x state

Notes:

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

-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 specification, these are averaged at the locality-birth-year level.

-"No rainfall shock" = 1 for locality birth years that experienced rainfall levels within one standard deviation of the 10-year historical locality-specific mean

1. Locality-level analysis: unit of observation is birth-year-locality.

fertility or selective child mortality.

Our third 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 2, again at the individual level, using number of younger siblings and birth spacing between next youngest sibling (in days) as dependent variables. With one exception, the main effects and interaction are all insignificant. Given that the coefficient on *Progresa* exposure in column 2 is weakly significant and very small in magnitude, we interpret these results as finding little evidence in support of selection bias.

5   Conclusion

In this paper, we leverage the combination of two exogenous shocks at different points in the life cycle to study whether investments can mitigate the impact of early-life shocks, a question that is usually complicated by the endogeneity of investment responses. Using the *Progresa* experiment and year-of-birth rainfall shocks, we study the impacts of these investment and endowment shocks on educational attainment, an important investment but also potentially a proxy for skill in this rural developing country context. We find that they both significantly increase educational attainment individually.

Most notably, the coefficient on the interaction between *Progresa* exposure and normal rainfall is negative and significant across a variety of education measures, suggesting that re-mediation of early-life shocks is possible through investments. That is, the positive impact of *Progresa* exposure on educational outcomes is largest for individuals with low endowment realizations due to adverse early-life shocks. The magnitude of the interaction term is telling:

in most cases, it is over half of the size of the main effect of *Progresa* (and sometimes completely overwhelms this effect), which implies that programs like *Progresa* have the potential to reverse the majority of inequality generated by early life circumstances.

Our study contributes to the large literature evaluating *Progresa* and conditional cash transfer programs more generally. While most evaluations of such programs tend to focus on average effects, we compare impacts across individuals with different unobserved endowments, exploiting rainfall shocks as our source of exogenous variation in this unobservable. Indeed, unlike the few other studies attempting this sort of exercise, the continuous nature of the endowment shock we observe allows us to calculate treatment effects of *Progresa* at every point along the endowment distribution. *Progresa* appeared to have had a very targeted impact on those who experienced negative shocks early in life. An important finding for policymakers, this suggests that programs like these may be most efficient if targeted toward the disadvantaged – not just in terms of income (as *Progresa* already targets the poor) but also in terms of endowments. While the challenges involved with this sort of targeting are not trivial, our results offer reason for optimism about the ability of policies to mitigate the negative impacts and inequality generated by early life shocks.

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A Additional Tables

Figure A1: Proportion of Individuals Not Living in Household, by Age
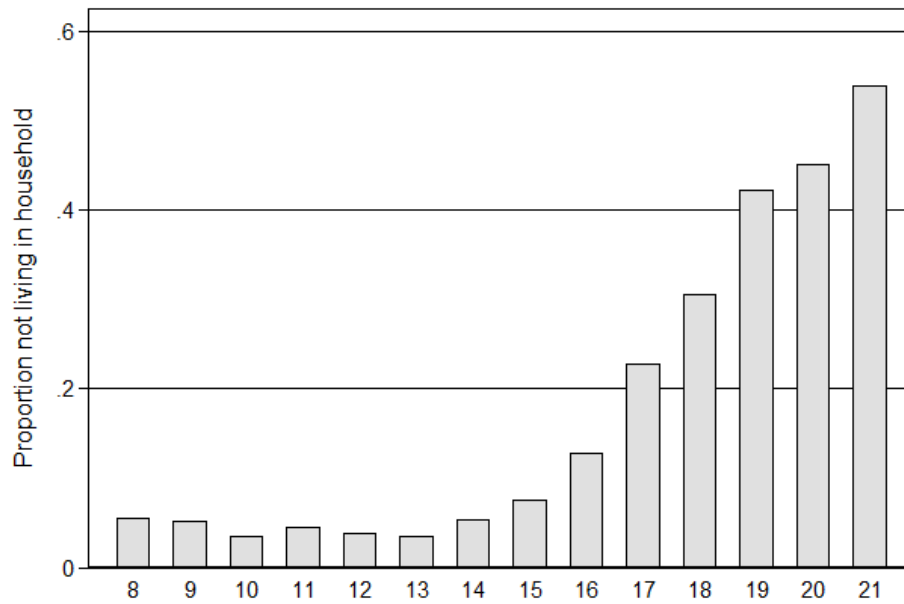
Table A1: Exposure to *Progresa*

Age in 1998	School Grade in 1998	Age in 2003	Years Exposed to PROGRESA in 2003		
			Treatment Villages	Control Villages	Difference in Exposure
5	-	10	3	3	0
6	1st year primary	11	4	4	0
7	2nd year primary	12	5	4	1
8	3rd year primary	13	6	4	2
9	4th year primary	14	6	4	2
10	5th year primary	15	6	4	2
11	6th year primary	16	6	4	2
12	1st year junior high	17	6	4	2
13	2nd year junior high	18	4	2	2
14	3rd year junior high	19	2	1	1
15	1st year high school	20	0	0	0
16	2nd year high school	21	0	0	0

Figure A2: *Progresa* Localities by Proportion of Years with a Rainfall Shock, 1985-1991

◆ < 20% shocks
◆ 20-40% shocks
◆ 40-60% shocks
◆ 60-80% shocks
◆ 80-100% shocks
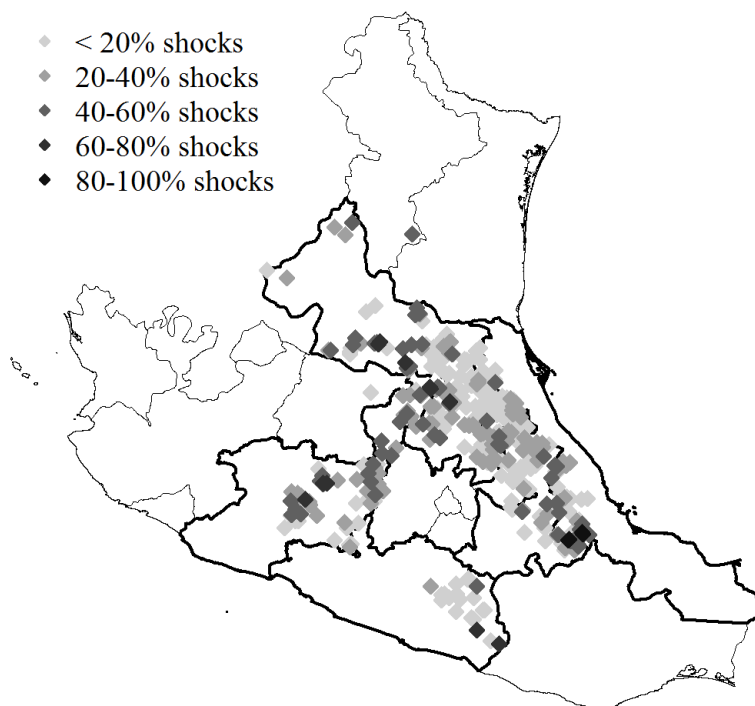
Notes: Percentages in the legend correspond to the proportion of years from 1985 to 1991 (in which rainfall data was available for that locality) that a rainfall shock was experienced.

Table A2: Summary Statistics for Control Variables

Panel A: Household-level

	Treatment Villages	Control Villages	Full Sample	Treatment - Control Differences
Household size	7.422	7.403	7.415	0.0190
	(2.215)	(2.150)	(2.190)	(0.0407)
Household head age	41.42	42.21	41.73	-0.794***
	(11.09)	(11.58)	(11.29)	(0.210)
Female household head	0.0563	0.0568	0.0565	-0.000474
	(0.231)	(0.231)	(0.231)	(0.00429)
Children aged 0-2	0.0735	0.0719	0.0729	0.00158
	(0.0860)	(0.0872)	(0.0865)	(0.00161)
Children aged 3-5	0.103	0.0992	0.101	0.00336*
	(0.0961)	(0.0960)	(0.0961)	(0.00178)
Boys aged 6-7	0.0509	0.0537	0.0520	-0.00275*
	(0.0761)	(0.0793)	(0.0774)	(0.00144)
Boys aged 8-12	0.126	0.121	0.124	0.00488**
	(0.113)	(0.113)	(0.113)	(0.00210)
Boys aged 13-18	0.0699	0.0692	0.0696	0.000671
	(0.0958)	(0.0929)	(0.0947)	(0.00176)
Girls aged 6-7	0.0516	0.0508	0.0513	0.000818
	(0.0766)	(0.0758)	(0.0763)	(0.00142)
Girls aged 8-12	0.119	0.121	0.120	-0.00226
	(0.111)	(0.114)	(0.112)	(0.00208)
Girls aged 13-18	0.0653	0.0667	0.0658	-0.00144
	(0.0914)	(0.0908)	(0.0911)	(0.00169)
Women aged 19-54	0.159	0.160	0.160	-0.000961
	(0.0608)	(0.0617)	(0.0611)	(0.00114)
Men aged 55 and over	0.0182	0.0190	0.0185	-0.000871
	(0.0506)	(0.0507)	(0.0506)	(0.000941)
Women aged 55 and over	0.0166	0.0184	0.0173	-0.00179*
	(0.0496)	(0.0513)	(0.0503)	(0.000934)
Mother's educational attainment	3.924	3.928	3.926	-0.00403
	(2.068)	(2.075)	(2.071)	(0.0476)
Mother's educational attainment missing	0.333	0.357	0.342	-0.0238***
	(0.471)	(0.479)	(0.474)	(0.00881)
Father's educational attainment	4.033	3.889	3.977	0.144***
	(2.313)	(2.136)	(2.247)	(0.0502)
Father's educational attainment missing	0.304	0.312	0.307	-0.00798
	(0.460)	(0.463)	(0.461)	(0.00857)
Mother speaks indigenous language	0.373	0.385	0.378	-0.0124
	(0.484)	(0.487)	(0.485)	(0.00920)
Mother's language missing	0.0390	0.0436	0.0408	-0.00460
	(0.194)	(0.204)	(0.198)	(0.00367)
Father speaks indigenous language	0.383	0.405	0.392	-0.0215**
	(0.486)	(0.491)	(0.488)	(0.00953)
Father's language missing	0.0969	0.0939	0.0957	0.00305
	(0.296)	(0.292)	(0.294)	(0.00546)
Number of households	3795	2438	6233	

Panel B: Locality-level

	Treatment Villages	Control Villages	Full Sample	Treatment - Control Differences
Community Well	0.366	0.393	0.376	-0.0269
	(0.483)	(0.490)	(0.485)	(0.0486)
Well Spring	0.510	0.436	0.481	0.0741
	(0.501)	(0.497)	(0.500)	(0.0500)
Public Water	0.121	0.190	0.148	-0.0696*
	(0.326)	(0.394)	(0.355)	(0.0354)
Bury Garbage	0.206	0.141	0.181	0.0651*
	(0.405)	(0.349)	(0.385)	(0.0385)
Public Dumpster	0.00778	0.0307	0.0167	-0.0229*
	(0.0880)	(0.173)	(0.128)	(0.0128)
Public Drainage	0.0350	0.0429	0.0381	-0.00793
	(0.184)	(0.203)	(0.192)	(0.0192)
Public Phone	0.518	0.521	0.519	-0.00396
	(0.501)	(0.501)	(0.500)	(0.0501)
Hospital or health	0.132	0.178	0.150	-0.0456
	(0.339)	(0.384)	(0.357)	(0.0358)
Distance to health	13.74	13.17	13.52	0.574
	(24.31)	(24.67)	(24.43)	(2.449)
DICONSA store	0.261	0.202	0.238	0.0582
	(0.440)	(0.403)	(0.426)	(0.0427)
Distance to Bank	40.50	36.01	38.72	4.482
	(59.25)	(37.62)	(51.76)	(5.497)
Distance to Bank	0.128	0.0982	0.117	0.0302
	(0.335)	(0.298)	(0.321)	(0.0322)
Distance to School	12.17	11.33	11.82	0.836
	(15.95)	(15.91)	(15.89)	(2.438)
Distance to School Missing	0.599	0.552	0.581	0.0471
	(0.491)	(0.499)	(0.494)	(0.0495)
Number of localities	257	163	420	

(*** p<0.01, ** p<0.05, * p<0.1).

Table A3: Effects of *Progresa*, Rainfall, and their Interaction on Schooling Completion by Grade (with Municipality Fixed Effects)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Primary School				*Secondary School*			*High School*		
	3 yrs	4 yrs	5 yrs	6 yrs	7 yrs	8 yrs	9 yrs	10 yrs	11 yrs	12 yrs
Years of Progresa Exposure	0.000162	0.00638	0.00807	0.00948	0.0333	0.0345	0.0347	0.0111	0.00320	0.00312
	(0.00454)	(0.00566)	(0.00810)	(0.00984)	(0.0180)*	(0.0166)**	(0.0126)***	(0.00644)*	(0.00321)	(0.00211)
	[0.00430]	[0.00530]	[0.00631]	[0.00807]	[0.0113]***	[0.0113]***	[0.00990]***	[0.00628]*	[0.00338]	[0.00231]
	{0.00405}	{0.00533}	{0.00602}	{0.00731}	{0.0110}***	{0.0101}***	{0.0114}***	{0.00573}*	{0.00370}	{0.00208}
No Rainfall Shock	-0.00818	0.00558	0.0177	0.00903	0.190	0.186	0.173	0.0650	0.0469	0.0237
	(0.0215)	(0.0297)	(0.0370)	(0.0464)	(0.0580)***	(0.0604)***	(0.0588)***	(0.0380)*	(0.0196)**	(0.0138)*
	[0.0190]	[0.0266]	[0.0301]	[0.0382]	[0.0521]***	[0.0533]***	[0.0509]***	[0.0291]**	[0.0196]**	[0.0150]
	{0.0199}	{0.0293}	{0.0271}	{0.0365}	{0.0450}***	{0.0460}***	{0.0541}***	{0.0274}**	{0.0227}**	{0.0190}
No Shock x Exposure	0.00109	-0.00235	-0.00377	-0.00107	-0.0324	-0.0354	-0.0328	-0.0126	-0.00779	-0.00451
	(0.00419)	(0.00557)	(0.00703)	(0.00885)	(0.0115)***	(0.0117)***	(0.0114)***	(0.00766)	(0.00374)**	(0.00256)*
	[0.00380]	[0.00511]	[0.00588]	[0.00745]	[0.0101]***	[0.0106]***	[0.0102]***	[0.00588]**	[0.00369]**	[0.00282]
	{0.00383}	{0.00546}	{0.00547}	{0.00728}	{0.00843}***	{0.00876}***	{0.0104}***	{0.00545}**	{0.00426}*	{0.00355}
Observations	11824	11824	11824	11824	11824	11824	11824	11824	11824	11824
Mean of Dependent Variable	0.970	0.935	0.881	0.785	0.484	0.369	0.260	0.0610	0.0308	0.0123
Fixed Effects	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).
- "No rainfall shock" = 1 for individuals whose birth-year rainfall was within one standard deviation of 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 characteristic dummies. Controls for parental language/education and locality distance include dummies for missing values