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 variation – resource availability at birth (local rainfall) and cash incentives for school enrollment – to identify the interaction between early endowments and investments in children. We find that adverse rainfall in the year of birth decreases grade attainment, post-secondary enrollment, and employment outcomes. But children whose families were randomized to receive conditional cash transfers experienced a much smaller decline: each additional year of program exposure during childhood mitigated more than 20 percent of early disadvantage.

Keywords: fetal origins, early life, dynamic complementarities, cash transfers, education, employment, Mexico
JEL Classification Codes: I15, I25, O12

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Poor circumstance in early life often has long-lasting negative impacts (Almond and Currie, 2011; Currie and Vogl, 2012; Heckman, 2006, 2007).[1] What role can important change agents – parents, communities, governments – play in lessening the burden of adverse events in a young child's life? Research has demonstrated that in many contexts, parents provide more time and material resources to their more disadvantaged children (Almond and Mazumder, 2013). We ask: 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 relates closely to recent work evaluating the impacts of policies that provide support to disadvantaged children (Aizer et al., 2016; Chetty et al., 2016; Conti et al., 2015; Gertler et al., 2014; Hoynes et al., 2016; Lavy and Schlosser, 2005; Lavy et al., 2016).

The answer to this question is neither theoretically obvious nor empirically straightforward. The theory of dynamic human capital formation suggests that timing matters a great deal (Cunha et al., 2010; Heckman and Mosso, 2014). Due to the decreasing degree of static (within-period) substitutability of investments and stocks of human capital as individuals age, investing in children very early in their lives yields the largest returns; attempting to correct for disadvantage in later childhood (say, adolescence) or adulthood may be economically inefficient (Conti and Heckman, 2014; Heckman and Mosso, 2014). It is yet unclear at what ages this drop in returns kicks in, and thus when the potential remediating effects of investments may disappear.

The main empirical challenge in answering this question rigorously is 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 estimate of the remediation value of investments if these investments are correlated with unobserved determinants of the outcomes in which we are interested. As Almond and Mazumder (2013) put it in their

[1] Shocks to the early life environment – disease, poverty, maternal stress, nutritional or income availability, and conflict, among many others – affect a wide range of adult outcomes (see, e.g., Adhvaryu et al. (2016); Almond (2006); Bhalotra and Venkataramani (2011); Duque (2016); Fink et al. (2015); Gould et al. (2011); Maccini and Yang (2009); Persson and Rossin-Slater (2014); Venkataramani (2012)).

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 with regard to educational attainment and employment outcomes, via conditional cash transfers during childhood. 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 health inputs for children), we verify that adverse rainfall lowers the agricultural wage, and 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 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 educational attainment by 0.1 years. By reducing the opportunity 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. Each additional year of program exposure during childhood mitigated more than 20 percent of early disadvantage. 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, although data limitations preclude the analysis of longer-term outcomes for much of our sample, for the oldest individuals (who were 18 at the time of the 2003 survey), we find a similar pattern of coefficients in regressions on continued education (after high school) and employment outcomes.[2] Adverse rainfall in the year of birth leads to a reduction of 17 percentage points in the probability of working; while each additional year of *Progresa* exposure offsets nearly 8 percentage points of this impact. At 2 years of program exposure (the within cohort difference due to randomized treatment), *Progresa* offsets more than 88 percent of the disadvantage caused by adverse rainfall in the year of birth in terms of employment at age 18.

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. With respect to employment at age 18, we find that *Progresa* has little to no effect on children born during normal rainfall, with roughly the entire impact of *Progresa* exhibited for disadvantaged children.

Our study furthers the understanding of a crucial aspect of the complex process of human capital formation: how do early stocks of human capital and subsequent investments interact to determine long-run outcomes (Cunha et al., 2010; Heckman and Mosso, 2014)? 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. (2016), 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 Malamud et al. (2016), who examine the interaction of access to abortion and better schooling in Romania. Despite the vastly different contexts and types of programs studied,

[2] Attrition and low quality data in the 2007 wave of the survey make this wave unusable. Accordingly, we have post-secondary schooling and employment outcomes only for 18 year olds in 2003, who are also impacted by both sources of exogenous variation.

the results in these papers, quite remarkably, mirror what we find in our work – an (at least weakly) negative interaction effect – indicating that remediation of early-life shocks via investments can indeed be successful.

Part of the 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 (Chetty et al., 2016; Gould et al., 2011; Heckman et al., 2010, 2013; Hoynes et al., 2016; Lavy et al., 2016), 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 the return is largest.[3] Our results show that in terms of schooling and employment outcomes, 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).[4]

Our empirical context is appealing because of the relatively high potential for external validity. Adverse rainfall is likely the most common type of shock experienced by poor households in much of the developing world (Dinkelman, 2013), and has large short- and long-term consequences (Maccini and Yang, 2009; Paxson, 1992; Shah and Steinberg, 2013). Given the rising importance of wide-scale cash transfer programs around the world (Blattman et al., 2013; Haushofer and Shapiro, 2013), 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 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. Section

[3]In other words, there would be an equity-efficiency tradeoff for late stage child investments (Heckman, 2007).

[4]In both contexts, it should be noted that what is being estimated is the return to an intervention for the *poorest* among a disadvantaged population, as both *Progresa* and Head Start already target low-income households.

3 describes our empirical strategy. Section 4 details our results and section 5 concludes.

1 Program Background

In 1997, Government of Mexico began 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 rural communities. In this paper, we focus on the education component of *Progresa*, which consisted of bimonthly 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).[5] Initially ranging from 60 to 205 pesos in 1997, the size of the subsidy depended 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.[6] 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. Table 1 summarizes the monthly grant amounts for the second semester of 1997, 1998 and 2003.

The program was initially implemented 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. For instance, studies have found that *Progresa* improved educational outcomes and decreased child work (Behrman et al., 2011; Schultz, 2004; Skoufias and Parker, 2001), reduced infant and elderly mortality (Barham, 2011; Barham and Rowberry, 2013), increased investment in farm assets (Gertler et al., 2012), and improved health and

[5] The health component involved conditional cash transfers that incentivized health behaviors.

[6] Given the lower rates of attendance of girls in rural Mexico, the policy's intention was to provide additional incentives to girls (Skoufias, 2005). Skoufias and Parker (2001), Behrman et al. (2009), and Behrman et al. (2011) cover additional program details in depth.

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.

nutrition across a number of dimensions (Barber and Gertler, 2008; Fernald et al., 2008a,b,c; Gertler, 2004; Hoddinott and Skoufias, 2004).

Like these studies, we take advantage of the random assignment and treat *Progresa* as an exogenous shock to the cost of schooling. We also exploit additional variation in years of treatment exposure across cohorts. We follow the majority of previous studies in utilizing the extensive margin of program exposure and ignoring actual receipt of transfers or specific grant amounts, which depend on fertility and other endogenous characteristics and decisions of the household. However, it should be noted that the vast majority of households eligible for the program actually did receive benefits (Hoddinott and Skoufias, 2004).[7] Because only households who were classified as poor by the program administration were eligible to receive the benefits from the program, we focus, as many previous studies do, 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 in this study.

[7] Hoddinott and Skoufias (2004) report that only 5% of the households in treatment localities who were defined as eligible to receive benefits and formally included in the program in 1998 had not received any benefits by March 2000.

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 wave contains detailed information on household demographics and individual socioeconomic, health, schooling and employment outcomes. A follow-up survey was also conducted in 2007, but we do not use this wave due to high attrition rates.[8]

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 more likely to have moved out of the household by the 2003 survey and are therefore not surveyed.[9]

[8]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.

[9]As Figure A1 shows, the proportion of 19-year-olds not living in the household is over 40%, and this pro-

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.

Following Behrman et al. (2011), we also 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 with smaller than 2 year differences, we use the birth year reported in the 1997 wave.

2.2 Rainfall Data

We 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 program localities using their geocodes. Due to changes in the use of weather stations as well as irregular reporting by some stations, there are some localities for which the nearest rainfall station has missing observations during the period of time relevant for our study.

To deal with this issue, 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.[10] Using this procedure, 69 of the 506 localities were 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.

portion continues to grow with age.

[10]Weights are normalized to sum to 1.

2.3 Outcome Variables

Our main education 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 achieved in 2003.[11] "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.

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 lowest skill and least stable jobs from the rest of the employment categories (by grouping those working as spot laborers with the unemployed).

2.4 *Progresa* Exposure Variable

Our two independent 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.

[11] Students with complete primary education have a maximum of 6 years of schooling; junior high 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.

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). 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.

2.5 Rainfall Shock Variable

For our early life shock, we use annual rainfall during an individual's calendar year of birth in their locality of residence in 1997.[12] 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 best to household incomes at the time of birth (and therefore to a child's biological

[12]The data does 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 equivalent for most of the individuals in our sample, as migration is minimal due to their young ages.

endowment). Specifically, we define a shock as a level of rainfall that is 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 the absence of a negative shock (for ease of interpretation of the interaction coefficients). 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 historical 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 detail in section 3, both previous literature as well as our own data show that defining the shock variable in this way 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 correlated variable. This is illustrated in Figure 1, which maps all *Progresa* localities by their rainfall status. Black dots 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.[13]

We show only one year in Figure 1 for illustrative purposes, and chose 1987 because it is the birth year of the largest number of individuals in our sample. This exercise also maps well to our estimating equation, which includes birth year fixed effects and accordingly identifies using within birth year variation. In the Appendix, Figure A2 uses rainfall from all birth years.

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

[13]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 large number of rainfall stations (most localities have several stations within 20km) as well as our use of inverse-distance weighting, which assigns different rainfall values to even very closely situated localities.

Figure 1: *Progresa* Localities by Rainfall Shock in 1987

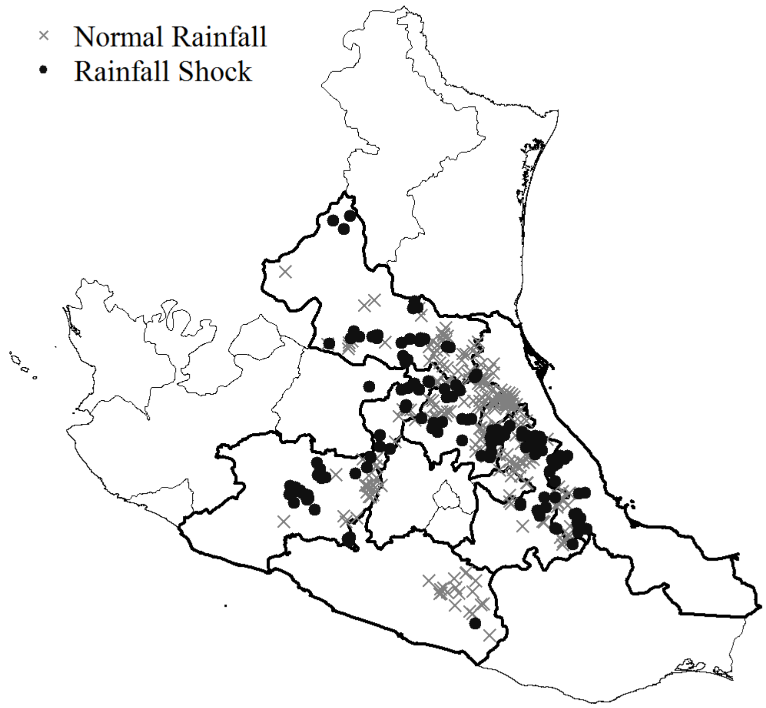
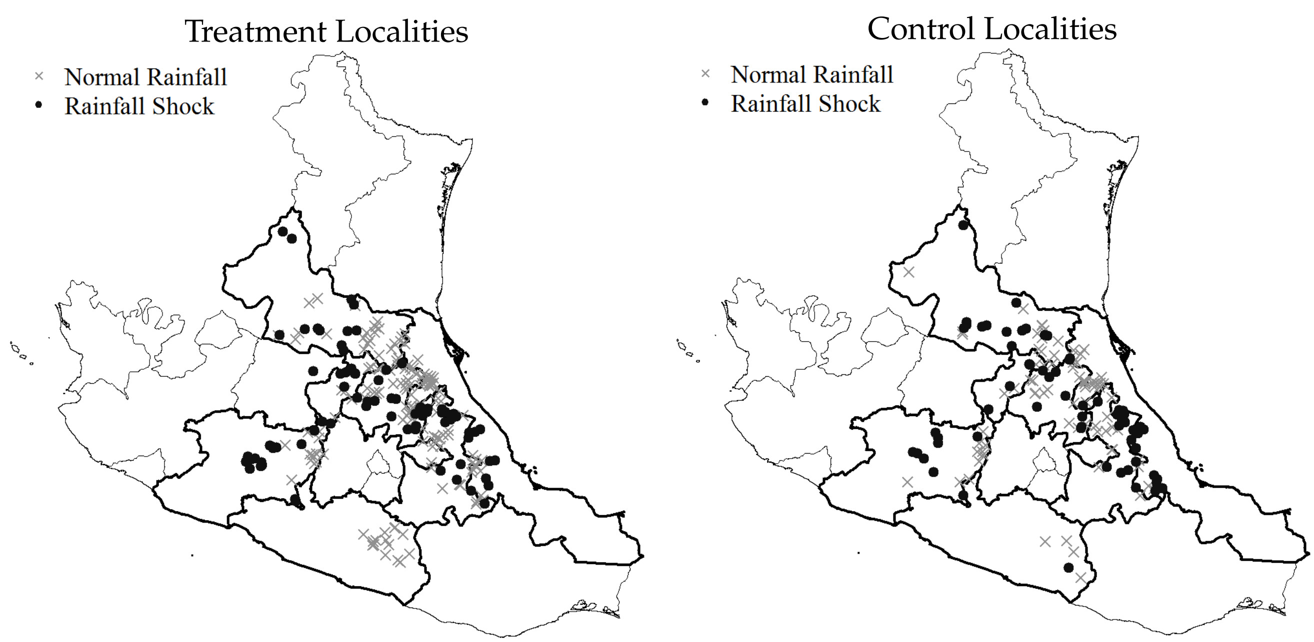


Figure 2: *Progresa* Localities by Treatment Status and Rainfall Shock in 1987



variation in rainfall shock status, even within small geographic areas, as shown in Figure 2.

2.6 Summary Statistics

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	6.786	6.847	6.692	0.154***
	(2.109)	(2.094)	(2.128)	(0.0397)
Grade Progression	0.579	0.591	0.561	0.0295***
	(0.494)	(0.492)	(0.496)	(0.00955)
Appropriate Grade Completion	0.465	0.479	0.442	0.0366***
	(0.499)	(0.500)	(0.497)	(0.00939)
Number of individuals	11829	7193	4636	
18-year-olds				
Currently Enrolled w/ HS Degree	0.0607	0.0584	0.0641	-0.00574
	(0.239)	(0.235)	(0.245)	(0.0122)
Worked this Week	0.502	0.514	0.485	0.0290
	(0.500)	(0.500)	(0.500)	(0.0301)
Worked this Year	0.532	0.543	0.515	0.0284
	(0.499)	(0.498)	(0.500)	(0.0301)
Worked in Non-Laborer Job	0.354	0.356	0.351	0.00511
	(0.479)	(0.479)	(0.478)	(0.0288)
Number of individuals	1597	942	655	

Notes:
Standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Variable definitions:
-Educational attainment: years of schooling
-Grade progression: 1(progressed 5 grades between 1997 and 2003)
-Appropriate grade completion: 1(completed the age-appropriate years of schooling, eg: 1 for age 7, 2 for age 8, etc)
-Currently enrolled w/ HS degree: 1(still enrolled in school after having received a high school degree)
-Worked last week: 1(worked in the week before survey)
-Worked last year: 1(worked in year before survey)
-Worked in non-laborer job: 1(worked in year before survey at a job other than as a spot laborer)

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) living in households eligible for Progresa.[14] Average educational attainment is 6.8 years for the pooled sample, with individuals in treatment villages receiving on average 0.154

[14] In this table, as in the rest of the analysis, we restrict to individuals who satisfy the data quality requirements described in section 2.1.

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. Note that employment outcomes for 18 year olds do not appear to be impacted significantly by treatment on average. 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

	Full Sample	*Treatment Villages*	*Control Villages*	*Treatment - Control Differences*
A. Full Sample				
Years of Progresa	4.841	5.574	3.695	1.879***
exposure	(1.168)	(0.727)	(0.720)	(0.0296)
Annual rainfall	1182.4	1180.6	1185.3	-4.752
	(644.3)	(654.8)	(628.0)	(26.32)
Normalized rainfall	-0.0704	-0.0539	-0.0962	0.0423
	(0.812)	(0.792)	(0.841)	(0.0332)
Rainfall Shock	0.242	0.223	0.272	-0.0483***
	(0.428)	(0.417)	(0.445)	(0.0175)
Number of locality x birth-year observations	2519	1536	983	
B. Trimmed Sample				
Years of Progresa	4.812	5.576	3.707	1.869***
exposure	(1.166)	(0.724)	(0.707)	(0.0313)
Annual rainfall	1181.1	1171.1	1195.5	-24.43
	(644.0)	(654.8)	(628.0)	(28.12)
Normalized rainfall	-0.0667	-0.0511	-0.0891	0.0379
	(0.844)	(0.833)	(0.859)	(0.0368)
Rainfall Shock	0.277	0.266	0.294	-0.0279
	(0.448)	(0.442)	(0.456)	(0.0195)
Number of locality x birth-year observations	2170	1282	888	

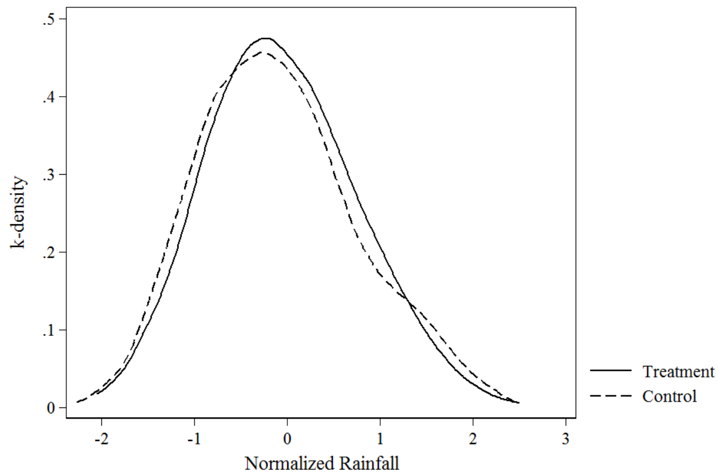
Notes:
Standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Variable definitions:
-Annual rainfall: Total annual rainfall in mm
-Normalized rainfall: Total annual rainfall, standardized using locality-specific, 10-year historical mean and standard deviation
-Rainfall shock: 1(Normalized rainfall greater than 1 or less than -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 x 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 and oldest cohorts, 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 the 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 localities indeed share a common support and 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).

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. Figure 4, 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

Figure 3: Normalized Rainfall Distributions in Treatment and Control Villages



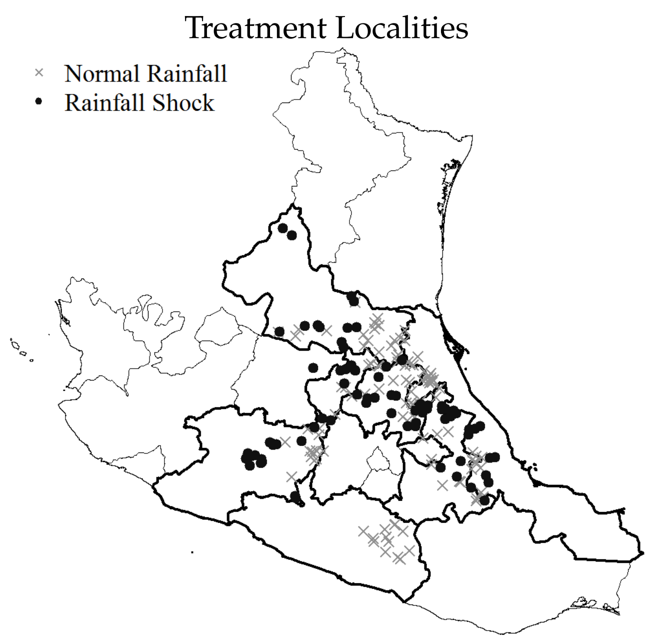
Notes:
Rainfall levels are normalized using each locality's location-specific historical mean and standard deviation.

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. 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.

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

Figure 4: *Progresa* Localities by Treatment Status and Rainfall Shock in 1987, Trimmed Sample



hold and locality-level variables in our regression analysis, which we outline in the following section. In the Appendix, we run additional specifications that control for the interaction of these unbalanced controls with the rainfall shock and find that this does not substantially change our results.[15]

3 Empirical Strategy

We use rainfall during an individual's year of birth as a shock to that individual's biological endowment. 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 impact 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, which could lead to improved physical health and cognitive ability. Like the Indone-

[15]Similar to the strategy used in Acemoglu et al. (2004), this ensures that the unbalanced characteristics do not confound the estimate of our treatment-rainfall interaction.

sian 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. Bobonis (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.

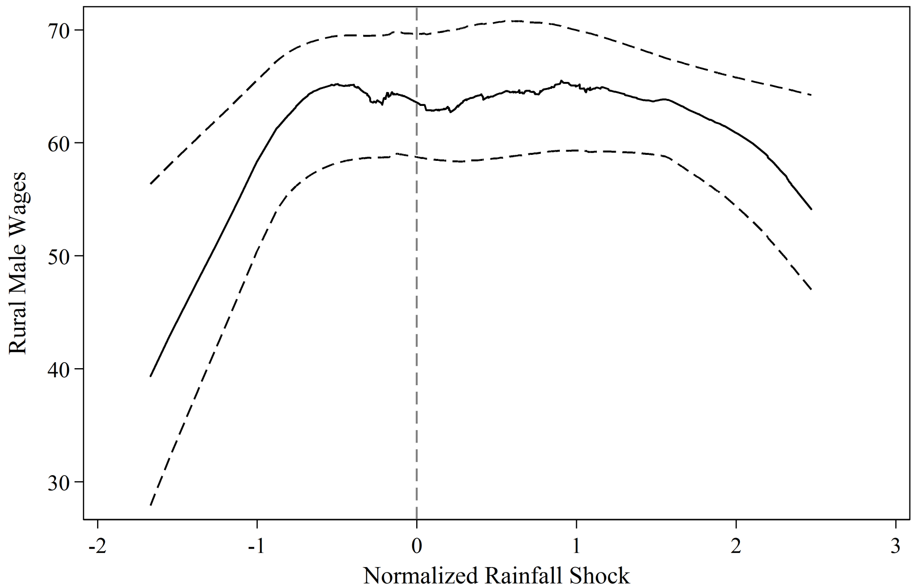
Figure 5 depicts the relationship, using lowess smoothing, between average male wages from the 2003 surveys and rainfall in that same year, normalized using the locality-specific 10-year historical mean and standard deviation. The clear inverted U-shape, which peaks at around zero, shows that wages are 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 negative 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. 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 in 1997, born in year t, their education or employment outcomes y_{islt} can be expressed as follows:

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

where R_{slt} represents a normal rainfall dummy, indicating that rainfall during the individual's

Figure 5: Locality Wages



Notes:
Dashed lines represent 95% confidence intervals, calculated from 1000 bootstrap replications.

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 x birth year fixed effects (μ_s x δ_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 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 2.5, 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 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,[16] 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.[17] In the Appendix, we show specifications that include interactions between the rainfall shock and each of the characteristics that are not balanced across treatment and control.

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 30% and 10% of the sample, respectively. Similarly, distance to secondary school and distance to bank are missing for 58% and 12% of localities, respectively. 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.[18] Similarly, distance to bank and distance to secondary

[16]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.

[17]DICONSA stores, operated by the Ministry of Social Development, are responsible for distributing the nutritional supplements that are part of the health component of *Progresa*.

[18]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.

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 measure potential heterogeneity in the effect of the investment shock on individuals with different endowments. The following specification adds an interaction term to measure precisely this heterogeneity:

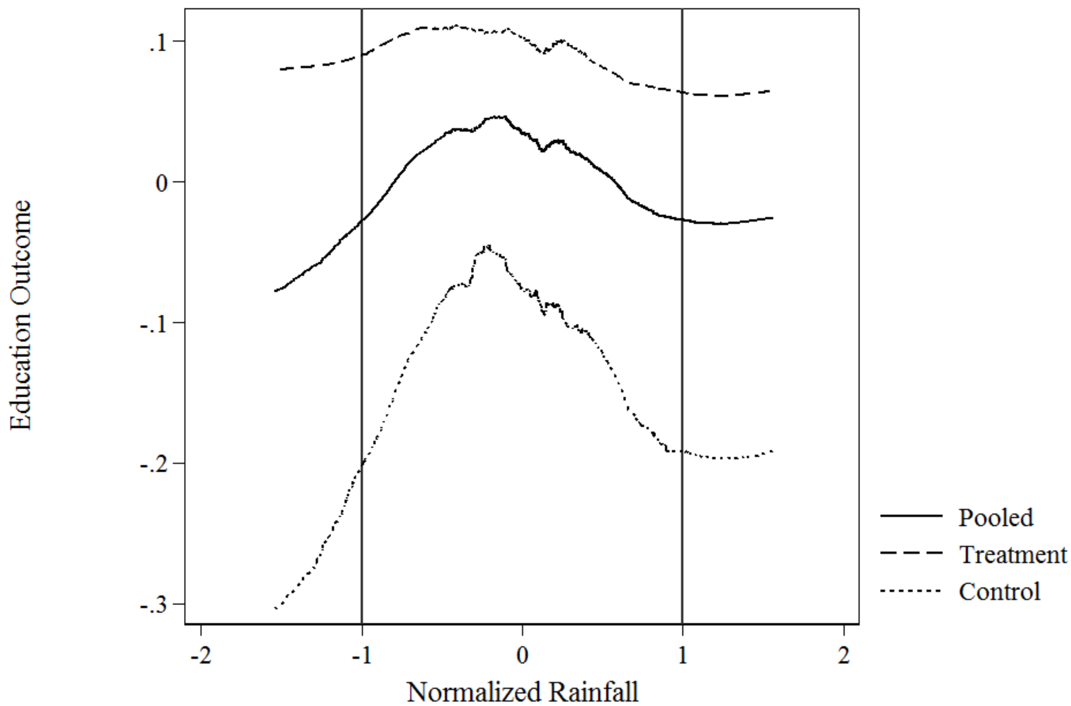
$$y_{islt} = \beta_1 R_{slt} + \beta_2 P_{slt} + \beta_3 R_{slt} P_{slt} + \alpha' X_{islt} + \mu_s \text{ x } \delta_t + \epsilon_{islt} \tag{2}$$

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.

4 Results

In this section, we report and discuss estimation results from the strategy discussed above. We begin with a graphical illustration of our results on education, which reflects the pattern found in the remainder of the empirical results. We then move on to present the results of the regression analysis for all outcomes, first discussing educational outcomes and then enrollment and employment outcomes for the oldest cohort of our sample. Finally, we discuss a number of checks to address concerns about selective fertility, attrition, and imbalance in the prevalence of rainfall shocks across treatment and control.

Figure 6: Years of Educational Attainment by Rainfall in Year of Birth



Notes:
All three lines represent the lowess-smoothed educational attainment residuals for the relevant group, calculated after regressing educational attainment on state by birth-year fixed effects and the control variables described in section 3. Vertical lines depict one standard deviation above and below the mean of normalized rainfall, which is trimmed at the 5th and 95th percentiles.

Figure 6 illustrates the intuition underlying our identification strategy, using lowess smoothing to depict the non-monotonic relationship between rainfall at birth and educational attainment across treatment and control households, as well as in the pooled sample. We first regress educational attainment 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 normalized rainfall 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, 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*. Second, the distance between the treatment and control lines is smallest around a normalized rainfall deviation of zero and grows larger in the tails (below and above one standard deviation, depicted by the vertical lines). Furthermore, the treatment line is essentially flat, as compared to the control line, indicating that *Progresa* exposure successfully mitigates the impacts of extreme rainfall at birth on educational attainment.

4.1 Education Results

The following tables 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.[19] 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 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).

[19]Because these results are very similar to those from a simplified specification that only includes the state-by-year fixed effects, gender, and household size, we only report results using the more complete set of controls.

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 completed 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 generally not significant at the 5% level. In column 3, we see that *Progresa* and normal rainfall have positive and significant impacts on 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 the investment shock to have heterogeneous impacts on individuals with different endowments. Panel B of Table 4 displays the results from specification 2. 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 column 1, the main effects of *Progresa* and normal rainfall are positive and significant while the interaction is negative and significant, all at the 5% level (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), indicating that educational outcomes respond less for children with relatively high endowments.

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 characteristics. Controls for parental language/education and locality distance include dummies for missing values

Looking at the specification with municipality fixed effects 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 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 here 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 endowments. 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.[20]

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 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. 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

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

Table 5: Interaction Effects on Schooling Completion by Grade

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		Primary School				*Junior High 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 characteristics. Controls for parental language/education and locality distance include dummies for missing values

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*.

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. The tests were administered to a sample of the population aged 15 to 21 in 2003. We find small effects tightly bound around 0 of *Progresa*, rainfall, and their interaction on these tests (see Appendix Table A7). This is consistent with previous literature (Behrman et al., 2009), which has found no main effect 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, almost 30% of the sample answered everything correctly (and over 50% only made 2 mistakes) in a test of 58 questions.

4.2 Employment Outcomes

We are also interested in whether the endowment and investment shocks we study have similar effects on longer-run labor 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,[21] 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. In this smaller sample, we estimate the effects of *Progresa*, birth year rainfall,

[21] We do not use the 2007 survey because of significant attrition problems. We also cannot use individuals who are older than 18 in 2003 as the fraction living outside of the original household and, accordingly, missing employment data, grows large after age 18. See section 2.1 for more details.

Table 6: Effects of *Progresa* and 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 Currenly Working	Enrolled or Worked this Year	Enrolled or Worked in Non-Laborer Job
Years of Progresa Exposure	**0.0126**	**0.0884**	**0.0798**	**0.0841**	**0.0811**	**0.0814**	**0.0853**
	(0.0135)	(0.0427)**	(0.0384)**	(0.0393)**	(0.0462)*	(0.0443)*	(0.0429)**
	[0.0124]	[0.0428]**	[0.0290]***	[0.0314]***	[0.0483]*	[0.0345]**	[0.0341]**
	{0.0128}	{0.0478}*	{0.0155}***	{0.0249}***	{0.0578}	{0.0273}***	{0.0218}***
No Rainfall Shock	**0.103**	**0.206**	**0.174**	**0.220**	**0.215**	**0.206**	**0.259**
	(0.0532)*	(0.146)	(0.128)	(0.128)*	(0.146)	(0.141)	(0.133)*
	[0.0531]*	[0.153]	[0.101]*	[0.0959]**	[0.157]	[0.110]*	[0.104]**
	{0.0474}**	{0.168}	{0.0585}***	{0.0719}***	{0.185}	{0.0853}**	{0.0628}***
No Shock x Exposure	**-0.0175**	**-0.0874**	**-0.0767**	**-0.0985**	**-0.0792**	**-0.0780**	**-0.0996**
	(0.0165)	(0.0442)**	(0.0391)*	(0.0395)**	(0.0464)*	(0.0444)*	(0.0416)**
	[0.0169]	[0.0434]**	[0.0304]**	[0.0312]***	[0.0491]	[0.0375]**	[0.0368]***
	{0.0158}	{0.0464}*	{0.0182}***	{0.0230}***	{0.0552}	{0.0311}**	{0.0252}***
Observations	1597	1147	1143	1143	1145	1139	1138
Mean of Dependent Variable	0.0607	0.502	0.532	0.354	0.563	0.587	0.414
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 characteristics. Controls for parental language/education and locality distance include dummies for missing values.
-These regressions restrict to individuals aged 18 in 2003.

and their interaction on a set of variables related to continuing education and employment after high school.

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 years of schooling. 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 low-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: both main effects are positive, while interaction terms are all negative. Some of the coefficients are imprecisely estimated, which is unsurprising given the much smaller sample sizes, but the overall pattern clearly suggests that the mitigative effects of *Progresa* are not limited to school-aged outcomes directly incentivized by the program. The results in columns 4 and 7 are particularly striking. Normal birth-year rainfall significantly increases the probability of an individual being employed in a non-laborer (i.e., higher skill and more stable) job, and *Progresa* also has a positive effect for individuals who experienced negative rainfall shocks. But the effect of *Progresa* is essentially zero for higher-endowment children. That is, *Progresa* has significant impacts on the probability of stable employment immediately following high school completion among disadvantaged children, but no impact on children with higher endowments. Taken in sum, these results illustrate the ability of investments in adolescence to offset the impacts of insults in early life and the higher return to investments for disadvantaged children.

4.3 Robustness Checks

4.4 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 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 section 4.4.3 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.

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.[22] In short, an important concern is whether trends over time varied for localities growing different types of

[22] 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).

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. Controlling for the age of an individual in the year their locality was certified,[23] we address concerns that correlations between PROCEDE's rollout and rainfall shocks might be confounding our estimates.

Appendix Table A8 addresses all of these concerns by running 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.

4.4.1 Selective Fertility

In Table 7 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 x 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 7 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

[23] 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 7: 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 variables, 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.

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 very small in magnitude, we interpret these results as finding little evidence in support of selection bias.

4.4.2 Attrition

As in any longitudinal study, we must consider the extent to which selective attrition may be confounding our results. In Table 8, we show that although attrition between the baseline and

Table 8: Effects of *Progresa* and 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	-0.00415	-0.00342	0.00343	-0.0328
	(0.00889)	(0.00384)	(0.00373)	(0.0379)
No Rainfall Shock	-0.0336	-0.0297	0.00168	-0.129
	(0.0383)	(0.0228)	(0.0192)	(0.135)
No Shock x Exposure	0.00652	0.00492	0.0000278	0.0393
	(0.00757)	(0.00450)	(0.00369)	(0.0400)
Observations	14525	12917	12159	1646
Mean of Dependent Variable	0.889	0.941	0.973	0.697
Ages	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).
-"No rainfall shock" = 1 for individuals whose birth-year rainfall was within one standard deviation of 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.

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 positive rainfall 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 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 of course to 18-year-olds in column 4). We do not find any evidence of either.

4.4.3 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 further 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 for rainfall measures in every year, or no shocks in any year, over the study period. As Tables A3, A4, and A5 show, our results are virtually identical to the full sample results.[24]

Second, we conduct a robustness exercise regarding the unbalanced demographic characteristics discussed in section 2 above and identified in previous studies. Table A6 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.

5 Conclusion

In this paper, we leverage the combination of two sources of exogenous variation – in early life circumstance and investments during childhood – to study whether (and the extent to which) it is possible to mitigate the impact of early life shocks, a question that is usually confounded 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 and employment outcomes.

We find that better early-life circumstance and more investments generate greater schooling and employment probabilities. Moreover, the coefficient on the interaction between *Progresa* exposure and normal rainfall is negative and significant across most outcome measures,

[24]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.

indicating that remediation of early-life shocks is possible through investments. Put differently, the positive impact of *Progresa* exposure on educational outcomes is largest for individuals with low endowment realizations due to adverse early-life conditions.

The magnitude of the interaction term is telling: in most cases, it is over half of the size of the main effect of *Progresa*, suggesting that cash transfer programs like *Progresa* have the potential to offset almost entirely the inequality generated by early life circumstances. We find similar patterns when studying continued education and employment outcomes in a sub-sample of older individuals. That is, longer-run post-schooling labor outcomes exhibit the potential for remediation as well.

Our study contributes to the large literature evaluating *Progresa* and conditional 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 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

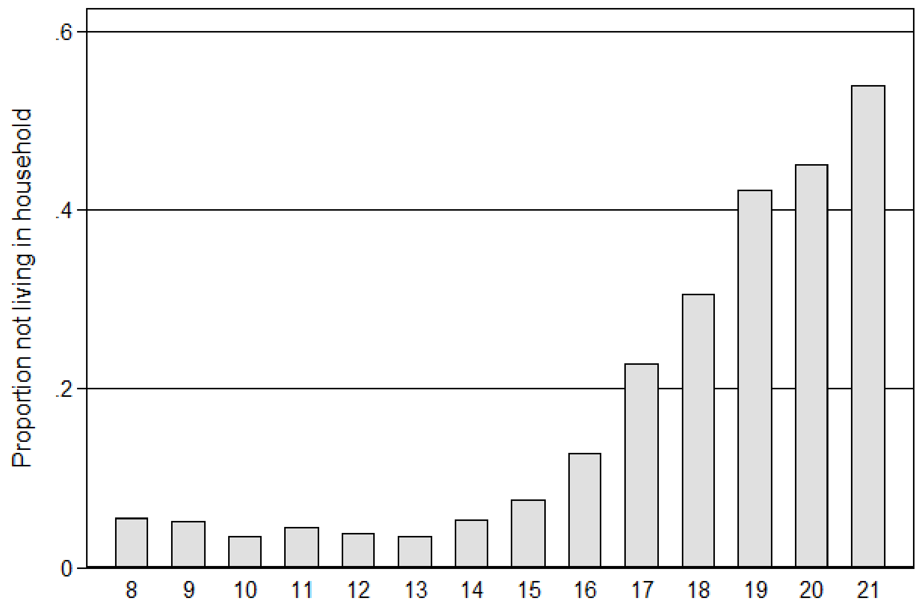
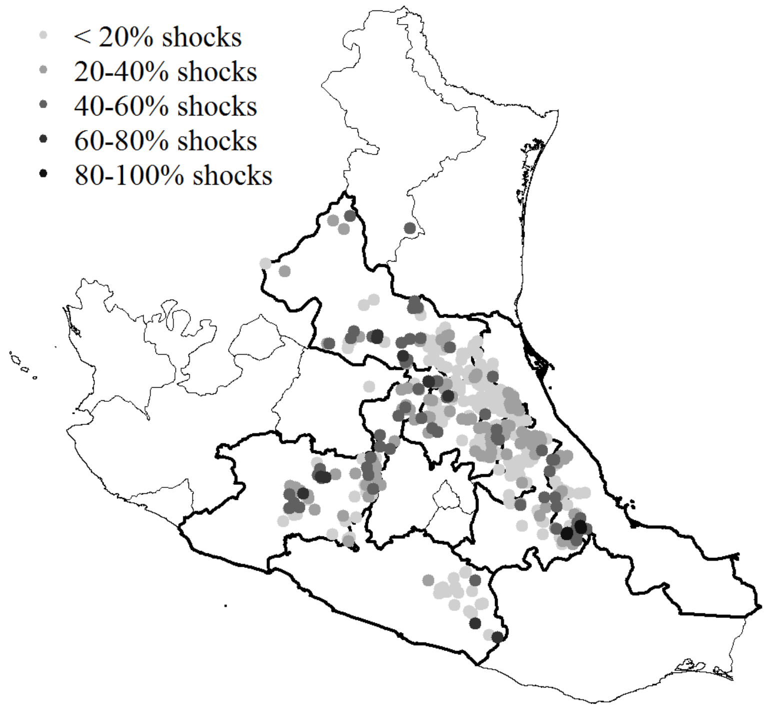


Table A1: Exposure to *Progresa*

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



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

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

Panel B: Locality-level

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

Notes:
Standard errors 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.

To verify that our results are not being driven by the imbalance in rainfall shock prevalence across treatment and control, 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 A3, A4, and A5 show, our results are virtually identical to the full sample results.

Table A3: 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	Birth year x state	Birth year x state, Municipality	Birth year x state, Municipality	Birth year x state, Municipality

Notes:
- Standard errors clustered at the municipality are reported 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 characteristics. Controls for parental language/education and locality distance include dummies for missing values

Table A4: Interaction Effects on Schooling Completion by Grade: Trimmed Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Primary School				*Junior High 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 are reported 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 characteristics. Controls for parental language/education and locality distance include dummies for missing values

Table A5: Effects of *Progresa* and Rainfall on Longer-Term Outcomes: Trimmed Sample

	(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 Currenly Working	Enrolled or Worked this Year	Enrolled or Worked in Non-Laborer Job
Years of Progresa Exposure	0.0101	0.0777	0.0816	0.102	0.0673	0.0857	0.105
	(0.0145)	(0.0462)*	(0.0407)**	(0.0396)**	(0.0485)	(0.0485)*	(0.0461)**
No Rainfall Shock	0.0958	0.162	0.191	0.315	0.145	0.221	0.351
	(0.0559)*	(0.163)	(0.137)	(0.129)**	(0.155)	(0.159)	(0.148)**
No Shock x Exposure	-0.0161	-0.0788	-0.0844	-0.123	-0.0655	-0.0859	-0.125
	(0.0178)	(0.0481)	(0.0419)**	(0.0408)***	(0.0486)	(0.0487)*	(0.0456)***
Observations	1320	970	966	966	969	963	962
Mean of Dependent Variable	0.0652	0.494	0.519	0.348	0.556	0.576	0.411
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).
-"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 characteristics. Controls for parental language/education and locality distance include dummies for missing values
-These regressions restrict to individuals aged 18 in 2003.

Table A6: Effects of *Progresa* and Rainfall on Education and Employment Outcomes, Controlling for Rainfall Shock Interactions with Unbalanced Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Years of Education	Grade Progression	Appropriate Grade Completion	Currently Enrolled w/ HS Degree	Worked this Week	Worked this Year	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of Progresa Exposure	0.215	0.0297	0.0315	0.0225	0.102	0.0861	0.0810	0.0901
	(0.0553)***	(0.0134)**	(0.0110)***	(0.0142)	(0.0507)**	(0.0477)*	(0.0513)	(0.0505)*
No Rainfall Shock	0.513	0.144	0.0705	0.203	0.00861	0.262	0.563	0.629
	(0.356)	(0.0843)*	(0.0770)	(0.0856)**	(0.372)	(0.364)	(0.360)	(0.375)*
No Shock x Exposure	-0.110	-0.0194	-0.0185	-0.0269	-0.102	-0.0843	-0.0961	-0.105
	(0.0537)**	(0.0110)*	(0.0102)*	(0.0178)	(0.0524)*	(0.0481)*	(0.0515)*	(0.0507)**
Observations	11824	11216	11824	1597	1147	1143	1143	1138
Mean of Dependent Variable	6.787	0.579	0.465	0.0607	0.502	0.532	0.354	0.414
Ages	12 to 18	12 to 18	12 to 18	18	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).
-"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 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 A2).

Table A6 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. It should be noted that the main effect can no longer be interpreted as an overall endowment shock, as these specifications include a number of interactions that need to be summed in order to obtain the total effect of rainfall. What is important to note is that the years of exposure and interaction coefficients remain very similar to the main results reported in the body of this paper.

Table A7: Effects of *Progresa* on Woodcock-Johnson Test Scores

	(1)	(2)	(3)	(4)
	Letter Word Identification	Applied Problems	Dictation	Average Score
Years of Progresa Exposure	-0.0515	0.0145	0.0596	0.00792
	(0.0513)	(0.0500)	(0.0605)	(0.0448)
No Rainfall Shock	-0.133	0.146	0.182	0.0643
	(0.229)	(0.252)	(0.281)	(0.210)
No Shock x Exposure	0.0557	-0.000391	-0.0333	0.00456
	(0.0481)	(0.0517)	(0.0569)	(0.0440)
Observations	1593	1586	1581	1571
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 characteristics. Controls for parental language/ education and locality distance include dummies for missing values
-Sample includes individuals aged 15 to 21
-Scores are standardized by test type, and the average score in column 4 takes the average across all three z-scores.

In 2003, Woodcock-Johnson dictation, word identification, and applied problems tests were administered to a sub-sample of individuals aged 15 to 21. Table A7 reports the results of regressions on these standardized test scores.

Table A8: Effects of *Progresa* and Rainfall on Education and Employment Outcomes, Controlling for Other Government Programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Years of Education	Grade Progression	Appropriate Grade Completion	Currently Enrolled w/ HS Degree	Worked this Week	Worked this Year	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of Progresa Exposure	0.207	0.0292	0.0308	0.00914	0.0998	0.0877	0.0942	0.0893
	(0.0542)***	(0.0126)**	(0.0107)***	(0.0122)	(0.0436)**	(0.0382)**	(0.0376)**	(0.0401)**
No Rainfall Shock	0.636	0.104	0.121	0.0819	0.247	0.197	0.245	0.263
	(0.283)**	(0.0539)*	(0.0502)**	(0.0467)*	(0.155)	(0.126)	(0.118)**	(0.122)**
No Shock x Exposure	-0.108	-0.0189	-0.0193	-0.0127	-0.0985	-0.0869	-0.111	-0.104
	(0.0535)**	(0.0106)*	(0.0100)*	(0.0149)	(0.0454)**	(0.0394)**	(0.0374)***	(0.0390)***
Observations	11734	11135	11734	1587	1138	1134	1134	1131
Mean of Dependent Variable	6.786	0.579	0.464	0.0605	0.500	0.532	0.353	0.412
Ages	12 to 18	12 to 18	12 to 18	18	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).
-"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 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).

Table A8 reports the results of regressions on our main outcomes of interest, taking into account other contemporaneous programs and policies, including PROCAMPO, PROCEDE, and crop-specific agricultural policies.