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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.

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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 (Cunha et al., 2006; Doyle et al., 2009; Heckman, 2006); 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.

Several influential studies argue that there is very little scope for catch-up when it comes to nutritional deficiencies that occur before a child's second birthday (Martorell et al., 1994; Victora et al., 2008, 2010), but more recent evidence from longitudinal data suggests that catch-up on both physical and cognitive dimensions is still possible after age 2 (Crookston et al., 2010, 2013; Lundeen et al., 2014; Prentice et al., 2013). Evidence on whether later-life investments can help generate this catch-up is much more limited. Some recent work has explored whether early schooling investments have heterogeneous effects across the physical and cognitive ability distributions (Bitler et al., 2014; Cueto et al., 2016), but due to non-trivial empirical difficulties, causal relationships have proved difficult to pin down.

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

[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); Bleakley (2007, 2010); Gould et al. (2011); Hoddinott et al. (2008); Maccini and Yang (2009); Majid (2015); Maluccio et al. (2009)).

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 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 affects physical health. We then show that Mexican youth born during periods of adverse rainfall have worse educational attainment and employment outcomes than those born in normal rainfall periods. Exposure to adverse rainfall in the year of one's birth – a crucial period for the determination of long-term health and human capital – decreased years of completed 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 almost 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,[2] 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] 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.

Adverse rainfall in the year of birth leads to a reduction of 17 percentage points in the probability of working, but each additional year of PROGRESA exposure offsets nearly 8 percentage points of this impact. At 2 years of program exposure, 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 have higher educational attainment (about 0.5 grades more). But program exposure increases educational attainment 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.

To study the mechanisms underlying the remediation we find, we build a simple extension to the canonical model of schooling choice and lifecycle consumption that incorporates heterogeneity in the initial endowment and a role for conditional transfers that change the price of schooling. The model makes clear that there are two primary mechanisms driving remediation. First, the value of the PROGRESA transfer represents a larger proportion of forgone wages for low endowment individuals as compared to high endowment individuals, as low endowment individuals have lower earning potential than their high endowment counterparts, leading to a larger schooling response to the PROGRESA incentive among low endowment individuals. Second, because high endowment individuals obtain more schooling than do their low endowment counterparts, in the absence of the PROGRESA incentive, it is more difficult for a program like PROGRESA to increase the schooling of high-endowment individuals (vis-a-vis low-endowment individuals) due to effort costs being convex in schooling levels.

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, 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 benefit 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 (Behrman et al., 2009a; Chetty et al., 2016; Gould et al., 2011; Heckman et al., 2010, 2013; Hoddinott et al., 2013a,b, 2008; Hoynes et al., 2016; Lavy et al., 2016; Maluccio et al., 2009), we know little about how large are those benefits compared to those of similar interventions 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 benefits 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 most responsive to incentives for later-life investments, producing scope for remediation. 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; Wolpin, 1982). 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. Our empirical strategy is described in section 3 and results are discussed in section 4. In section 5, we outline a theoretical

[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 most disadvantaged among a low-income population, as both PROGRESA and Head Start already target poor households.

schooling model to shed light on what may be driving our empirical results. Section 6 concludes.

1 Program Background

1.1 Description of Program

In 1997, the Mexican government began a conditional cash transfer program called the Programa de Educación, Salud y Alimentación (PROGRESA), aimed at alleviating poverty and improving the health, education and nutritional status of rural households, particularly children and mothers. The program provided cash transfers to poor families (mothers, specifically), conditional on certain education and health-related requirements. Since then, the program has been expanded to urban areas and renamed, first to *Oportunidades* in 2002 and most recently to *Prospera* in 2014.

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). Initially ranging from 60 to 205 pesos in 1997, subsidies were adjusted every year for inflation. In each year, 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.[5] 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 health and nutrition component of the program involved conditional cash transfers intended to incentivize healthy behaviors. For instance, in order for a household to receive a cash grant for food, all members were required to visit the health facility a specified number of times per year and to attend nutrition and health education lectures. The required number of visits varied by age and gender, with pregnant women and infants required to go every 1-3 months, while anyone aged 5 and older only required to attend 1 or 2 times per year. The program also provided nutrition supplements and other preventative care for pregnant and lactating mothers and young children, and supported the improved

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

provision of primary health care services in PROGRESA localities.

Table 1: Monthly Amount of Educational Transfers to Beneficiary Households

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

Notes:

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

PROGRESA was targeted toward poor households in poor localities. To determine which localities would receive the program, a set of marginalized localities was identified using data from the 1990 and 1995 censuses. Within these selected localities, household-level eligibility for PROGRESA was determined based on the results of an income survey administered to all households in each locality. First, household per capita income (excluding child income) was calculated, and households were categorized as above or below a poverty line. Then, separately for each region, the program identified the household characteristics that were the best predictors of poverty status, which were then used to construct the index that ultimately classified households as poor (eligible for PROGRESA) or nonpoor.[6]

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

Like the existing literature, we take advantage of the random assignment and treat PROGRESA as

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

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 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 most previous studies do, on this subset of the population in our analysis.

1.2 Previous Literature on PROGRESA Effects

An enormous body of research has explored the effects of PROGRESA on a wide array of outcomes (Parker et al., 2017). In Appendix Table A3, we attempt to summarize the key findings of this literature, categorizing studies based on the age of the analysis sample – specifically, how old they were during the years of PROGRESA being used to identify its effects. We also classify studies as education-related, health-related, cognitive or behavioral, and consumption-related; it is clear that PROGRESA was successful at improving outcomes across all of these dimensions. For school-aged children, however, the main effect of PROGRESA was educational. In the first panel of Table A3, we show that existing work on school-aged children has focused almost exclusively on education outcomes: in the short and medium term, PROGRESA has been found to have improved educational attainment, grade progression, and other measures of schooling success. That the benefits of PROGRESA for school-aged children were primarily educational is not surprising: this age group was the only one directly affected by the schooling subsidies and was too old to benefit from the main health benefits targeted toward much younger children. Consistent with this, Table A3 shows that most of the effects that PROGRESA had on health were concentrated among much younger (or much older) samples.

As outlined in the introduction, the main question we seek to answer in this paper is whether later-life investments can help remediate for disadvantage generated very early in life, and we are interested in PROGRESA as an exogenous shock to later-life investments. We are therefore interested in studying the outcomes of children who were school-aged when the program was rolled out, for whom there is experimental variation in exposure to the schooling grant and for whom we observe schooling outcomes

[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.

past primary school. When interpreting our results, therefore, we view the education subsidy channel as the main driving mechanism behind the results we find, not the health component or the actual cash received.[8] This is consistent with what has been documented in the literature – large education effects for school-aged children but virtually no evidence of health effects for this age group – and with the design of the program.

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2 Data

2.1 PROGRESA Data

The data collected for the evaluation of 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.

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

[8]Though Appendix Table A3 shows that significant consumption effects have been documented, these are on the whole relatively small in magnitude (Parker et al., 2017).

2 year differences, we use the birth year reported in the 1997 wave.

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] While survey respondents (usually mothers or grandmothers) are still asked some questions about non-resident individuals, these responses are likely to introduce greater measurement error, potentially correlated with our regressors of interest. To avoid this issue, which is particularly problematic for our employment outcomes (which are missing for non-resident household members), we exclude individuals over 18 years old.

This issue is also what limits our use of the 2007 survey, during which our sample individuals were aged 16 to 22. Specifically, attrition is too high for us to continue to follow our sample individuals and use their 2007 outcomes.[10] However, for some of our supporting analysis, we use a number of child development measures collected for younger children in 2007 (along with other physical, cognitive, and behavioral outcomes measured during the 2003 survey for various sub-samples that do not overlap with our sample of interest).

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. Restricting to this 20 kilometer radius ensures that we are using data from rainfall stations very close to our localities of interest. In fact,

[9] As Figure A1 shows, the proportion of 19-year-olds not living in the household is over 40%, and this proportion continues to grow with age.

[10] We lose over half of our 2003 sample, partially due to household-level attrition, but primarily due to individual migration (no proxy information is collected for those no longer living in the originally surveyed household) – likely to be endogenous. This unfortunate feature of the 2007 data has resulted in its limited use in the literature: the few studies that do use the 2007 data (for example, Behrman et al. (2008) and Fernald et al. (2009)) focus exclusively on PROGRESA's health effects on a much younger cohort, for whom migration is less of an issue.

the average distance between a locality's coordinates and its nearest rainfall station is 7 kilometers.[11] 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.

2.3 Outcome Variables

Our main education outcome variables include educational attainment (in years), a dummy for grade progression, and a dummy for having completed the appropriate number of grades for one's age. Given the fairly young age restrictions of our sample, the latter two variables are used as potentially more appropriate variables for individuals who have yet to complete their schooling. Educational attainment is constructed using information on the last grade-level achieved in 2003.[12] "Grade progression" is a binary variable equal to 1 if an individual progressed at least five complete grades between 1997 and 2003. We also define an indicator for age-appropriate grade completion. This is equal to 1 if an individual completed the appropriate number of grades for their age. For an individual who is 7 years old, we expect them to have completed one grade, for an 8 year-old, two grades, and so on. In order to study differential effects by grade, we also use 12 dummy variables, each indicating whether the individual completed at least 3, 4, and up to 12 grades of school.

For individuals who are 18 years old in 2003, we also look at continued enrollment and employment outcomes.[13] 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).

[11] The high density of rainfall stations, combined with the fact that we are using inverse distance weighting instead of nearest neighbor matching, means that our data is not as prone to the problems with spatially misaligned data as the examples explored in Pouliot (2015).

[12] Students with complete primary education have a maximum of 6 years of educational attainment; 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 and graduate work an additional one. We do not count years in preschool and kindergarten.

[13] As shown in Figure A1, attrition is higher among this cohort, but we show in column 4 of Table 11 that attrition for this cohort is not significantly affected by PROGRESA status or birth year rainfall.

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. 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. We obtain this by first calculating the number of months, dividing by 12, and rounding to the nearest year (because there is some ambiguity about the precise month in which treatment households began receiving benefits).[14]

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

2.5 Rainfall Shock Variable

For our early life shock, we use rainfall as an exogenous shock to income during a child's first year of life. Our interest is not in the absolute level of rainfall, but rather a measure of rainfall that maps best to household incomes at the time of birth (and therefore to a child's biological endowment). Specifically, we define a shock as a level of annual rainfall that is one standard deviation above or below the locality-specific mean (calculated over the 10 years prior to the birth year). We use this relative measure instead of an absolute measure of rainfall in order to capture the fact that the same amount of rainfall may have different consequences for different regions based on average rainfall levels. As we discuss in detail in section 3, both previous literature as well as our own data show that defining the shock variable in this way captures the contemporaneous relationship between rainfall and agricultural wages: normal years are associated with better outcomes than shock years. Importantly, defining the shock based on

[14]Benefits should have begun in May 1998, but some households appear to have been initiated earlier (Skoufias, 2005) while others appear to have experienced delays (Hoddinott and Skoufias, 2004).

comparisons to *locality-specific* means ensures that we are not simply comparing areas that typically get a lot of rainfall to areas with more moderate rainfall, as these areas could be substantially different on a number of dimensions. Instead, identification relies on comparing locality-years that received a lot more (or a lot less) rainfall than is typical for that specific locality. The regressions in Appendix Table A4, discussed in more detail in section 3, offer support for the exogeneity of these rainfall shocks.

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 rainfall in an individual's calendar year of birth in their locality of residence in 1997.[15] 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.[16]

In general, however, we recognize that it is difficult to pinpoint precise critical periods of development, and we do not claim to be doing this in this paper. For example, rainfall shocks in one year could in theory affect the income-generating abilities of households in subsequent years, which means that our rainfall variable should be more broadly interpreted as a shock to a child's early-life endowment rather than a shock in any specific period.[17] To answer our research question, all we require is that rainfall shocks exogenously affect a child's endowment, and we discuss evidence for this in section 4.1.

It is 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.[18]

[15] 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.

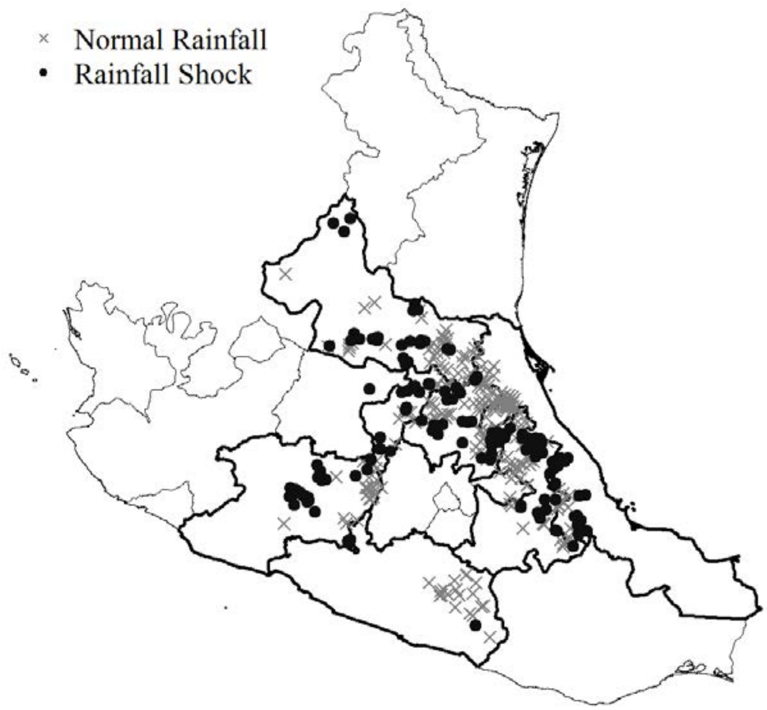
[16] In robustness checks (not shown here but available on request), we calculate annual sums based on the month of birth reported in the 1997 survey and find that rainfall summed over the 6 months prior to and 6 months after birth appear to be the most important.

[17] Serial correlation in rainfall shocks would also make it difficult to precisely identify the effects of shocks in consecutive periods, but – like other papers that test for serial correlation in rainfall shocks (Kaur, 2014; Shah and Steinberg, 2017) – we do not find that our rainfall shocks are serially correlated over time.

[18] 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.

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 by state fixed effects and accordingly identifies using within birth year variation. In the Appendix, Figure A2 uses rainfall from all birth years.

Figure 1: PROGRESA Localities by Rainfall Shock in 1987



Since we ultimately care about the interaction between rainfall and PROGRESA exposure, it is also important to note that for both treatment and control villages, we see still substantial variation in rainfall shock status, even within small geographic areas, as shown in Figure 2. Another question related to the interaction of these two variables is whether rainfall shocks in an individual's year of birth could affect the likelihood of that individual being eligible for PROGRESA, by affecting their household's long-run income-generating capabilities, for example. We do not find any significant differences in the likelihood of being categorized as poor (and therefore eligible for PROGRESA) across individuals born during rainfall shock years compared to normal years.

2.6 Summary Statistics

Table 2 reports summary statistics for individual-level variables from the 2003 survey for our sample of interest: individuals aged 12 to 18 (and for employment outcomes, only those aged 18) living in house-

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

	Full Sample	*Treatment Villages*	*Control Villages*	*Treatment - Control Differences*
12 to 18-year-olds				
Educational Attainment	6.79	6.85	6.69	0.15***
	(2.11)	(2.09)	(2.13)	(0.040)
Grade Progression	0.58	0.59	0.56	0.030***
	(0.49)	(0.49)	(0.50)	(0.0096)
Appropriate Grade Completion	0.46	0.48	0.44	0.037***
	(0.50)	(0.50)	(0.50)	(0.0094)
Number of individuals	11829	7193	4636	
Number of localities	420	257	163	
18-year-olds				
Currently Enrolled w/ HS Degree	0.061	0.058	0.064	-0.0057
	(0.24)	(0.23)	(0.25)	(0.012)
Worked this Week	0.50	0.51	0.48	0.029
	(0.50)	(0.50)	(0.50)	(0.030)
Worked this Year	0.53	0.54	0.52	0.028
	(0.50)	(0.50)	(0.50)	(0.030)
Worked in Non-Laborer Job	0.35	0.36	0.35	0.0051
	(0.48)	(0.48)	(0.48)	(0.029)
Number of individuals	1597	942	655	
Number of localities	368	218	150	

Notes:

Standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). We do not cluster standard errors in these summary statistics but cluster at the municipality-level in all main results.

Variable definitions:

-Educational attainment: number of grades completed

-Grade progression: 1(progressed 5 grades between 1997 and 2003)

-Appropriate grade completion: 1(completed the age-appropriate grades of schooling, eg: 1 for age 7, 2 for age 8, etc)

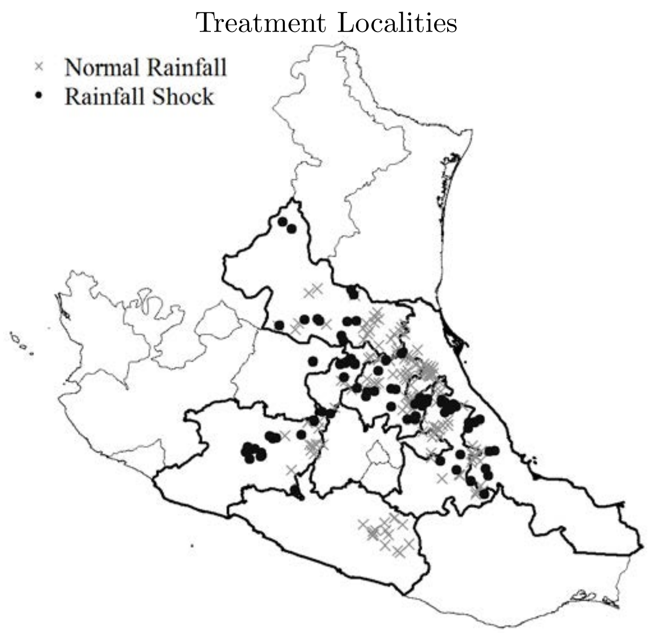
-Currently enrolled w/ HS degree: 1(still enrolled in school after having received a high school degree)

-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)

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



holds eligible for PROGRESA.[19] Average educational attainment is 6.8 years for the pooled sample, with individuals in treatment villages completing on average 0.15 more grades 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 school grades 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 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 by 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 dif-

[19]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.

Table 3: Summary Statistics for Shock Variables

	Full Sample	*Treatment Villages*	*Control Villages*	*Treatment - Control Differences*
A. Full Sample				
Years of PROGRESA exposure	4.84	5.57	3.69	1.88***
	(1.17)	(0.73)	(0.72)	(0.030)
Annual rainfall	1182.4	1180.6	1185.3	-4.75
	(644.3)	(654.8)	(628.0)	(26.3)
Normalized rainfall	-0.070	-0.054	-0.096	0.042
	(0.81)	(0.79)	(0.84)	(0.033)
Rainfall Shock	0.24	0.22	0.27	-0.048***
	(0.43)	(0.42)	(0.45)	(0.017)
Number of locality x birth-year observations	2519	1536	983	
Number of localities	420	257	163	
B. Trimmed Sample				
Years of PROGRESA exposure	4.81	5.58	3.71	1.87***
	(1.17)	(0.72)	(0.71)	(0.031)
Annual rainfall	1181.1	1171.1	1195.5	-24.4
	(644.0)	(654.8)	(628.0)	(28.1)
Normalized rainfall	-0.067	-0.051	-0.089	0.038
	(0.84)	(0.83)	(0.86)	(0.037)
Rainfall Shock	0.28	0.27	0.29	-0.028
	(0.45)	(0.44)	(0.46)	(0.020)
Number of locality x birth-year observations	2170	1282	888	
Number of localities	344	203	141	

Notes:

Standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). We do not cluster standard errors in these summary statistics but cluster at the municipality-level in all main results.

Variable definitions:

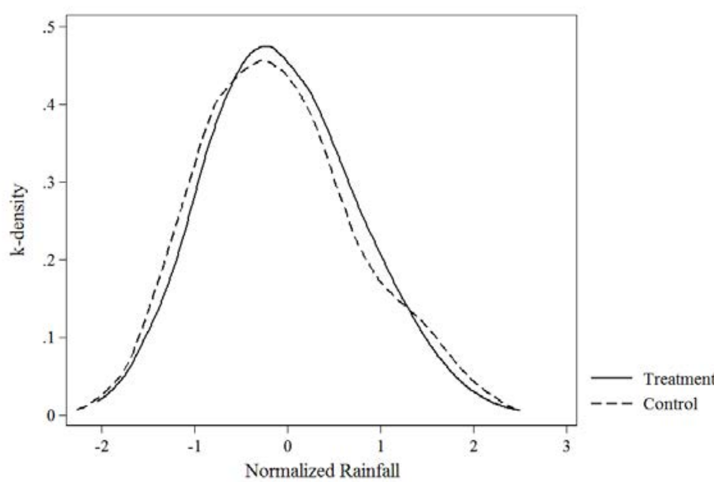
-Annual rainfall: Total annual rainfall in mm

-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) ference 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 control group, the spatial distribution of rainfall shocks are similar across the two groups (and both quite disperse).

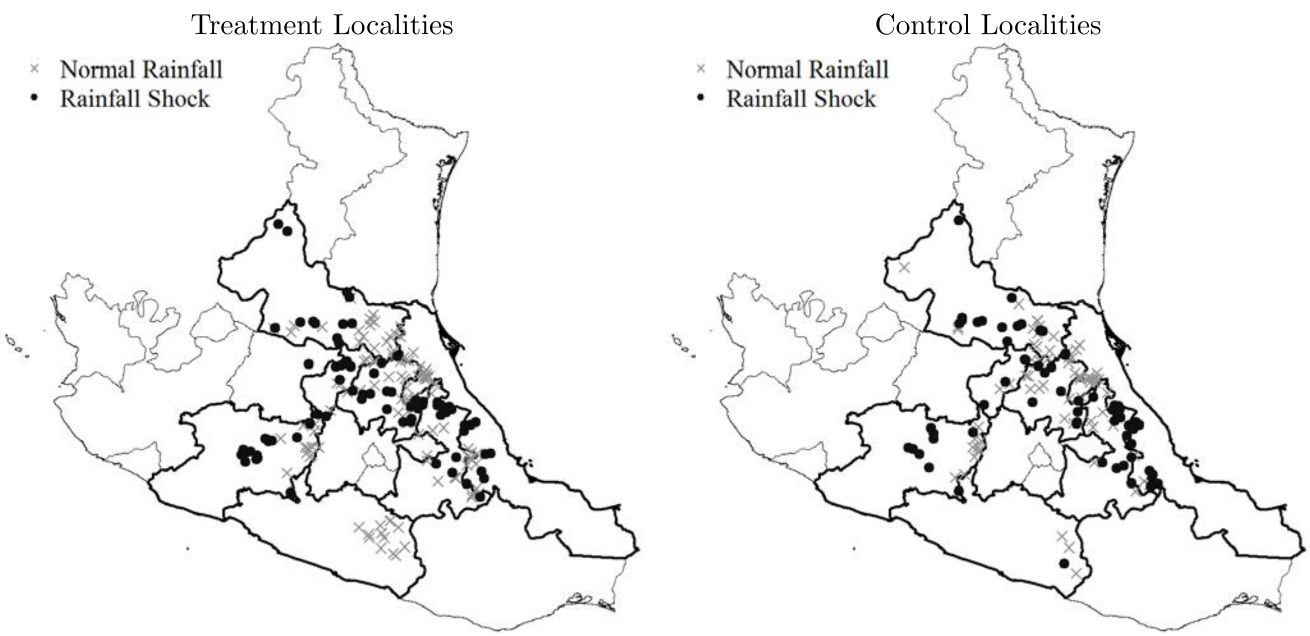
Figure 3: Normalized Rainfall Distributions in Treatment and Control Villages



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

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 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.

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



Despite the randomized nature of the PROGRESA experiment, previous literature has found that some household-level and locality-level characteristics are not fully balanced across treatment and control villages (Behrman and Todd, 1999). For this reason, in keeping with empirical methods used in previous studies of PROGRESA impacts, we include a rich set of controls that are summarized in Appendix Table A2. At the household level, the sample is fairly balanced across the groups with the exception of household head age, several household composition variables, two parental education variables, and father's language. At the locality-level, access to a public water network as well as garbage disposal techniques are significantly different across treatment and control villages, at the 10% level. We control for all of these household 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.[20]

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 Indonesian villages in Maccini and Yang (2009), the PROGRESA villages are also rural, suggesting that rainfall also serves as an important income shock to these communities. Bobonis (2009) confirms that negative rainfall shocks have a large negative impact on household expenditures in our exact same setting 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), but answering a very different question,[21] 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.

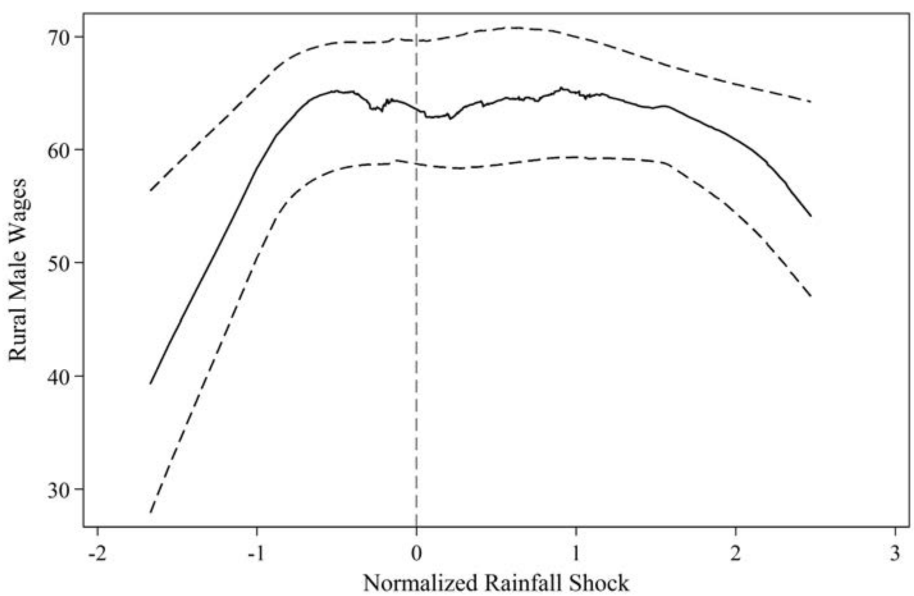
Our investment shock, which is the total number of years of PROGRESA exposure, also depends on

[20]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.

[21]Bobonis (2009) compared the effects of these rainfall shocks (exogenous drivers of overall household income) to the effects of PROGRESA transfers (exogenous drivers of female-specific income) to test for Pareto optimality in intrahousehold allocation decisions.

year of birth and locality of residence during the PROGRESA program. This years of exposure variable, the rainfall shock described above, and their interaction form the basis of our empirical specification. For the interpretation of our results, it is important that these variables truly be exogenous to the endowment and investments. To provide support for the exogeneity of these variables, we check whether individuals appear to be observably different across PROGRESA treatment and control villages, as well as normal rainfall versus rainfall shock groups. In Table A4, we regress each of the individual, household, and village-level characteristics that we use as control variables on our dependent variables of interest: years of exposure to PROGRESA and the rainfall shock . Across a total of 80 coefficients, only 11 are significant at the 10 percent level (and only 6 at the 5 percent level), which is approximately the number of significant coefficients we would expect to see by chance. Importantly, the vast majority of coefficients are small in magnitude relative to the means.[22]

Figure 5: Locality Wages



Notes:
Dashed lines represent 95% confidence intervals, calculated from 1000 bootstrapped samples.

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:

[22] One exception is age, for which the coefficients are slightly larger in magnitude relative to the means. As we discuss in section 4.4.4 and show in Table A9, these age imbalances do not appear to be affecting our main results.

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

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

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 obtain more precise estimates of the treatment effects and account for some significant differences across treatment and control villages that exist despite the randomization. All of our specifications include controls for individual gender, household size, household head age, household head gender, household composition variables,[23] 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.[24] 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.[25] Similarly, distance to bank and distance to secondary school are set to zero for missing observations but missing dummies for each variable are added to the specification.

4 Results

In this section, we begin by discussing evidence that the rainfall shocks we have defined actually affect the early life endowment. We then report and discuss estimation results from the strategy discussed above, beginning with a graphical illustration of our educational attainment results. Finally, we discuss a number of checks to address concerns about selective fertility, attrition, migration, and imbalance in the prevalence of rainfall shocks across treatment and control.

[23]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.

[24]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.

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

4.1 Rainfall Shocks and Health

A central part of our empirical strategy is the use of birth-year rainfall shocks as an exogenous driver of the early life endowment. Tables 4 to 6 present evidence that helps validate our use of this variable. As Bobonis (2009) has established and as is reflected in Figure 5, rainfall shocks affect expenditures and wages. In Table 4, we show that this translates to effects on contemporaneous BMI (likely via nutrition) as well. To study effects on BMI, we pool all individuals for whom BMI was measured across the 2003 and 2007 surveys. Specifically, height and weight were measured for sub-samples of children aged 2 to 6 in 2003, adolescents aged 15 to 21 in 2003, infants aged 0 to 2 in 2007, children aged 8 to 10 in 2007, and adults aged 30 and older in 2007. For each individual, we calculate gender- and age-specific BMI z-scores using WHO tables,[26] and regress this variable on an indicator for a normal rainfall realization (within one standard deviation of the locality-specific historical mean) in the individual's locality of residence in the relevant survey year (controlling for state-by-survey-year fixed effects and a host of other individual and household-level controls, described in the table notes). In column 1, we see that positive rainfall shocks in the survey year have positive effects on BMI for the entire sample. This supports the idea that the higher wages and expenditures that result from good rainfall also translate into higher nutritional intake. In column 2, we show that this result persists (and is much larger) for children under two years old, for whom these measurements are a closer proxy to their initial health endowment. In other words, this provides us with evidence that rainfall around the time of birth affects the nutritional intake and therefore BMI of infants.

We next ask whether these contemporaneous nutrition effects have longer-term implications for child health. To answer this question, we use height data, collected for children aged 0-2 in 2007, aged 2-6 in 2003, and aged 8-10 in 2007. We calculate age- and gender-specific height z-scores (once again using WHO tables) and create an indicator for stunted children, with heights falling more than 2 standard deviations below their group-specific mean. We then regress this indicator on the rainfall shock variable that we use in our main analysis – a dummy for a normal rainfall realization in the individual's year of birth. In columns 2 and 3, we see that there is a significant negative relationship between good rainfall and stunting for children aged 2 and older. In other words, year of birth rainfall shocks have physical health effects that persist into early childhood.

Taking advantage of other measures of child development collected in 2003 (for 2-6 year-olds) and

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

Table 4: Effect of Contemporaneous Rainfall Shocks on BMI

	(1)	(2)
	BMI z-score	BMI z-score
Normal Rainfall	0.056	0.14
(in survey year)	(0.033)*	(0.079)*
Observations	9569	1157
Mean of Dependent Variable	0.56	0.54
Ages	All	0-1
Survey year(s)	2003; 2007	2007
Fixed Effects	Birth year, state, survey year x state	

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

Table 5: Effect of Birth-Year Rainfall Shocks on Stunting

	(1)	(2)	(3)
	Stunted	Stunted	Stunted
Normal Rainfall	0.00016	-0.042	-0.037
(in birth year)	(0.026)	(0.019)**	(0.019)**
Observations	1227	1978	1426
Mean of Dependent Variable	0.19	0.22	0.087
Ages	0-2	2-6	8-10
Survey year	2007	2003	2007
Fixed Effects		Birth year x state	

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Normal Rainfall" = 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

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

Results are reported in Table 6, where we use the cognitive z-score (from 2003), the behavioral z-score (from 2003), and multiple behavioral z-scores (from 2007) as our dependent variables, and run regressions identical to the ones in Table 5. We find that birth-year rainfall had no significant effects on cognitive or behavioral measures for 2 to 6 year-olds, but did (weakly) reduce the likelihood of behavioral problems (externalizing problems, in particular) later in childhood. That income shocks in the year of birth can affect non-cognitive development is consistent with the child development literature, which documents that socioeconomic disadvantage is associated with altered maternal responses to infant emotions (Kim et al., 2017) and, in general, with other reasons for negative mother-infant interactions that could lead to behavioral problems later in childhood (Goyal et al., 2010).

4.2 Education Results

Having established that rainfall shocks are indeed relevant to early life endowments, we move on to our main analysis. 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 and normalized rainfall on our full set of controls (state-by-birth year fixed effects, and all household and locality-level controls described in Section 3). We then plot non-parametrically the relationship between the educational attainment residuals on the y axis and the normalized rainfall residuals on the x axis. The solid line represents the relationship for the pooled sample, including both treatment and control villages, which had varying degrees of exposure to the PROGRESA experiment.

Table 6: Effect of Birth-Year Rainfall Shocks on Cognitive and Behavioral Outcomes in Childhood

	(1)	(2)	(3)	(4)	(5)	(6)
	Cognitive measure z-score	Personality measures z-score	Behavioral problems z-scores			
			Overall	Externalizing problems	Internalizing problems	Social problems
Normal Rainfall (in birth year)	-0.015 (0.039)	-0.0020 (0.023)	-0.083 (0.050)*	-0.12 (0.065)*	-0.017 (0.067)	-0.11 (0.071)
Observations	2032	2014	1488	1488	1488	1488
Mean of Dependent Variable	-0.052	0.034	0.061	0.014	-0.0084	-0.044
Ages	2-6	2-6	8-10	8-10	8-10	8-10
Survey year	2003	2003	2007	2007	2007	2007
Fixed Effects	Birth year x state					

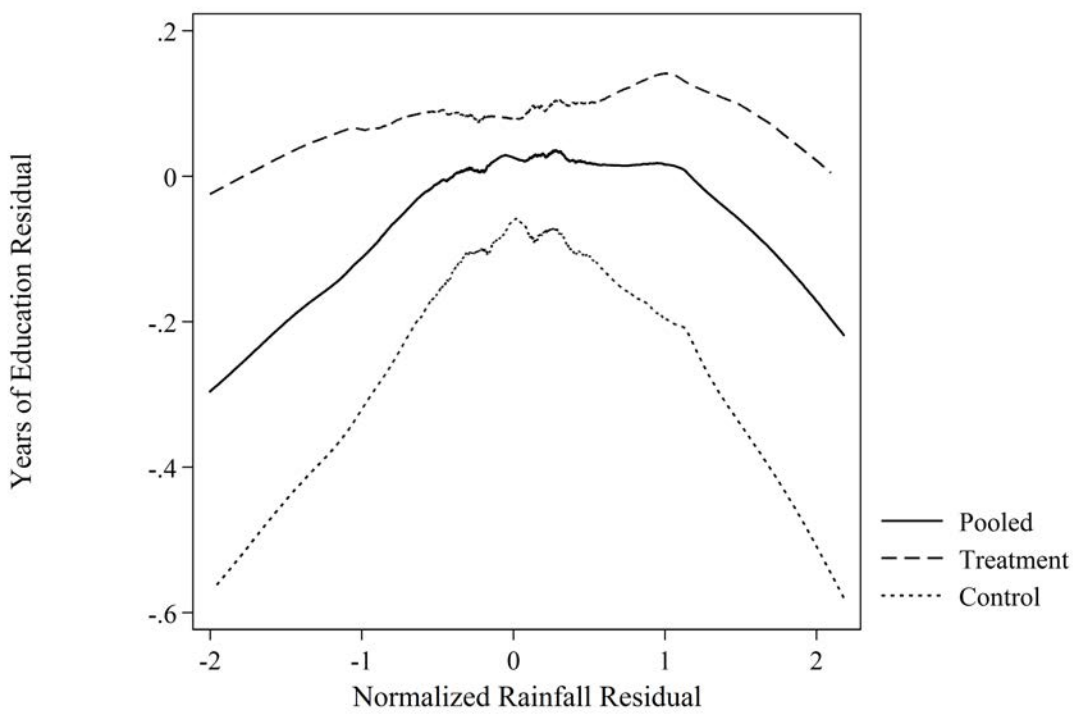
Notes:

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

-"Normal Rainfall" = 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

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

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. Furthermore, the treatment line is much flatter compared to the control line, indicating that PROGRESA exposure successfully mitigates the impacts of extreme rainfall at birth on educational attainment.

The following tables report parametric regression estimates analogous to the graphical analysis above. Before discussing the results of equation 1, we report in Panel A of Table 7 the results of regressions that include only the main effects of rainfall and PROGRESA exposure. The first three columns show the regression results from our base specification, which includes state-by-year fixed effects and household and locality controls.[27] For each coefficient of interest, we report three standard errors: first, clustered at the municipality level; second, allowing for spatial correlation using a 100km cutoff; and third, allowing for spatial correlation using a 500km cutoff. The results in column 1 show that one year of PROGRESA exposure leads individuals to complete 0.13 more grades of schooling on average: this effect is significant at the 5% level. Multiplying this coefficient by 1.5 years (the number of years between the treatment and control villages' first exposure to PROGRESA), we obtain a treatment effect of 0.2 years, which is consistent with previous work by Behrman et al. (2009b, 2011), which also estimated a treatment effect of 0.2 years using a slightly different sample.

Individuals who did not experience a negative rainfall shock at birth show a similarly sized boost in educational attainment of 0.10 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.

[27]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.

Table 7: Effects of PROGRESA and Rainfall on Education Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Educational Attainment	Grade Progression	Appropriate Grade Completion
Panel A: Main Effects Only						
Years of PROGRESA Exposure	**0.13**	**0.015**	**0.017**	**0.042**	**-0.0082**	**-0.0061**
	(0.037)***	(0.0096)	(0.0074)**	(0.046)	(0.012)	(0.011)
	[0.026]***	[0.0063]**	[0.0064]***	[0.033]	[0.0085]	[0.0082]
	{0.021}***	{0.0054}***	{0.0066}**	{0.031}	{0.0077}	{0.0092}
Normal Rainfall	**0.10**	**0.012**	**0.027**	**0.066**	**-0.00075**	**0.021**
	(0.056)*	(0.014)	(0.012)**	(0.054)	(0.014)	(0.011)*
	[0.062]*	[0.015]	[0.013]**	[0.050]	[0.012]	[0.012]*
	{0.068}	{0.015}	{0.015}*	{0.049}	{0.012}	{0.012}*
Panel B: Main Effects and Interaction						
Years of PROGRESA Exposure	**0.22**	**0.030**	**0.031**	**0.15**	**0.011**	**0.014**
	(0.055)***	(0.013)**	(0.011)***	(0.058)**	(0.015)	(0.014)
	[0.046]***	[0.011]***	[0.0097]***	[0.043]***	[0.011]	[0.011]
	{0.056}***	{0.012}**	{0.0091}***	{0.043}***	{0.011}	{0.011}
Normal Rainfall	**0.65**	**0.11**	**0.12**	**0.70**	**0.12**	**0.14**
	(0.28)**	(0.056)**	(0.051)**	(0.27)***	(0.057)**	(0.054)***
	[0.27]**	[0.058]*	[0.049]**	[0.23]***	[0.048]**	[0.048]***
	{0.34}*	{0.065}*	{0.047}**	{0.25}***	{0.046}**	{0.043}***
Normal Rainfall x Exposure	**-0.11**	**-0.020**	**-0.019**	**-0.13**	**-0.024**	**-0.025**
	(0.053)**	(0.011)*	(0.010)*	(0.051)**	(0.011)**	(0.011)**
	[0.053]**	[0.012]*	[0.010]*	[0.044]***	[0.0095]**	[0.0096]***
	{0.062}*	{0.013}	{0.0091}**	{0.045}***	{0.0086}***	{0.0081}***
Observations	11824	11216	11824	11824	11216	11824
Mean of Dependent Variable	6.79	0.58	0.46	6.79	0.58	0.46
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).

-"Normal Rainfall" = 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

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.

We allow for this in Panel B of Table 7 which displays the results from equation 1. Again, columns 1 to 3 show the results with the baseline set of controls, while columns 4 to 6 add the municipality fixed effects. As above, we report three sets of standard errors, which are generally quite similar. For educational attainment in column 1, the main 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). 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 educational attainment by 0.65 years in column 1 (our base specification); and although PROGRESA increases educational attainment for lower-endowment individuals by 0.22 years, it only increases educational attainment for higher-endowment individuals by 0.11 years (still positive and significant), indicating that educational outcomes respond less for children with relatively high endowments.

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 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.[28]

Table 8: Effects of PROGRESA and Rainfall on Schooling Completion by Grade

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Primary School				*Junior High School*			*High School*		
	3 grades	4 grades	5 grades	6 grades	7 grades	8 grades	9 grades	10 grades	11 grades	12 grades
Years of PROGRESA Exposure	**0.0022**	**0.012**	**0.018**	**0.023**	**0.055**	**0.054**	**0.065**	**0.019**	**0.0056**	**0.014**
	(0.0038)	(0.0051)**	(0.0071)**	(0.0087)**	(0.016)***	(0.016)***	(0.017)***	(0.010)*	(0.0078)	(0.014)
	[0.0037]	[0.0049]**	[0.0063]***	[0.0078]***	[0.012]***	[0.012]***	[0.013]***	[0.010]*	[0.0081]	[0.013]
	{0.0041}	{0.0060}**	{0.0072}**	{0.0090}**	{0.012}***	{0.012}***	{0.015}***	{0.0081}**	{0.0050}	{0.014}
Normal Rainfall	**-0.012**	**0.0090**	**0.031**	**0.036**	**0.20**	**0.23**	**0.25**	**0.065**	**0.072**	**0.10**
	(0.020)	(0.028)	(0.038)	(0.047)	(0.070)***	(0.072)***	(0.083)***	(0.052)	(0.037)*	(0.054)*
	[0.019]	[0.028]	[0.034]	[0.042]	[0.064]***	[0.069]***	[0.076]***	[0.044]	[0.035]**	[0.056]*
	{0.021}	{0.034}	{0.035}	{0.048}	{0.064}***	{0.067}***	{0.089}***	{0.042}	{0.029}**	{0.053}*
Normal Rainfall x Exposure	**0.0020**	**-0.0025**	**-0.0052**	**-0.0047**	**-0.032**	**-0.040**	**-0.046**	**-0.010**	**-0.0059**	**-0.018**
	(0.0040)	(0.0054)	(0.0072)	(0.0090)	(0.014)**	(0.013)***	(0.016)***	(0.011)	(0.0071)	(0.017)
	[0.0037]	[0.0053]	[0.0066]	[0.0080]	[0.013]**	[0.014]***	[0.015]***	[0.0096]	[0.0078]	[0.018]
	{0.0040}	{0.0061}	{0.0067}	{0.0087}	{0.012}***	{0.013}***	{0.017}***	{0.0086}	{0.0055}	{0.017}
Observations	11824	11824	11824	11824	10068	8285	6618	5002	3231	1592
Mean of Dependent Variable	0.97	0.93	0.88	0.78	0.56	0.52	0.45	0.14	0.097	0.058
Ages	12 to 18	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	16 to 18	17 to 18	18
Fixed Effects	Birth year x state									

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

In Table 8, we look at schooling completion by grade. We create separate dummy variables for the completion of 3 to 8 grades of school and estimate specification 1 using these dummies as the dependent variables. We start with 3 years of school because this is the youngest grade directly affected by the conditional cash transfers. In each column, we restrict the sample to individuals old enough to have completed the number of grades used in the dependent variable, which means that we are looking at smaller samples starting in column 5.

In columns 2 to 9, we see that the impact of PROGRESA on completing grades 4 to 11 is positive and significant, though the coefficient magnitudes are very small (especially relative to the means) up until the beginning of secondary school. The size of this main effect is largest in magnitude for the 7th grade of schooling, which Behrman et al. (2011) highlight as a critical transition period (between primary and

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

secondary school) during which many children drop out. Previous literature has documented that the positive effects of PROGRESA on educational outcomes are concentrated among children who were in late primary school when the program began (Behrman et al., 2009b; Schultz, 2004), which is consistent with these by-grade results. Taken together, these suggest that our main results in Table 7 are being driven by the older children in the sample.

Similarly, the main effect of normal rainfall only becomes positive and significant starting in 7th grade. Prior to this, the high completion rates suggest that endowments may not matter much during this period, as the vast majority attend school and pass. Also starting in 7th grade, we see significant negative interaction coefficients that offer support for the potential for interventions to mitigate the effects of early life shocks by encouraging the completion of secondary schooling among those hit by these shocks. As in Table 7, 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 as a potential proxy for ability. The tests were administered to a sub-sample of individuals 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 A5). This is consistent with previous literature (Behrman et al., 2009b), 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 – sitting in a classroom does not automatically imply improvements in cognitive ability. The null effects of PROGRESA and rainfall on cognitive scores may be an indication that cognitive ability is not the main component of the ability endowment that is driving our empirical results, which would be consistent with our finding that physical health and non-cognitive skills seem to be more affected by rainfall shocks than cognitive measures (see Table 6). That being said, these null effects could also be a result of certain features of the data. For instance, the tests may have been unable to capture sufficient variation in cognitive ability. In the letter-word identification test, for example, almost 30% of the sample answered everything correctly (and over 50% only made 2 mistakes) in a test of 58 questions. In addition, the fact that these tests were only administered to a sub-sample of the original study population means smaller sample sizes that could make it more difficult to detect effects. More important, however, is the fact that treatment status is significantly negatively related to the probability of an individual being in this sample at all (i.e., having a non-missing test score). Though it is unclear what is driving this selection,[29] it is very clear that these cognitive score results should be interpreted with caution.

4.3 Employment Outcomes

Table 9: 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 Currently Working	Enrolled or Worked this Year	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	**0.013**	**0.088**	**0.080**	**0.084**	**0.081**	**0.081**	**0.085**
	(0.014)	(0.043)**	(0.038)**	(0.039)**	(0.046)*	(0.044)*	(0.043)**
	[0.012]	[0.043]**	[0.029]***	[0.031]***	[0.048]*	[0.035]**	[0.034]**
	{0.013}	{0.048}*	{0.016}***	{0.025}***	{0.058}	{0.027}***	{0.022}***
Normal Rainfall	**0.10**	**0.21**	**0.17**	**0.22**	**0.21**	**0.21**	**0.26**
	(0.053)*	(0.15)	(0.13)	(0.13)*	(0.15)	(0.14)	(0.13)*
	[0.053]*	[0.15]	[0.10]*	[0.096]**	[0.16]	[0.11]*	[0.10]**
	{0.047}**	{0.17}	{0.059}***	{0.072}***	{0.19}	{0.085}**	{0.063}***
Normal Rainfall x Exposure	**-0.017**	**-0.087**	**-0.077**	**-0.099**	**-0.079**	**-0.078**	**-0.100**
	(0.017)	(0.044)**	(0.039)*	(0.040)**	(0.046)*	(0.044)*	(0.042)**
	[0.017]	[0.043]**	[0.030]**	[0.031]***	[0.049]	[0.037]**	[0.037]***
	{0.016}	{0.046}*	{0.018}***	{0.023}***	{0.055}	{0.031}**	{0.025}***
Observations	1597	1147	1143	1143	1145	1139	1138
Mean of Dependent Variable	0.061	0.50	0.53	0.35	0.56	0.59	0.41
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).
-"Normal Rainfall" = 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.

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,[30] but the oldest cohort – who were 18 at the time of the 2003 survey – were just old enough to be graduating

[29] Because treatment status does not affect overall attrition, as we show in Table 11, it must be related specifically to sub-sample selection or individuals' availability for test-taking.

[30] 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.

from high school and pursuing either further education or formal employment. In this smaller sample, we estimate the effects of PROGRESA, birth year rainfall, 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 grades of school. In columns 2 and 3, we create dummies for employment in the week of survey and in the past year. Column 4 attempts to separate those employed in 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.

Table 10: Effects of PROGRESA and Rainfall on Fertility

	(1)	(2)	(3)	(4)	(5)	(6)
	Locality-Level	Individual-Level				
	Total number of children	Number of younger siblings	Birth spacing (in days) between younger sibling	Mother's ideal number of children	Mother wants more children	Number of additional children desired
Years of PROGRESA Exposure		0.023*	-13.8	-0.12	-0.0040	-0.073
		(0.013)	(13.7)	(0.12)	(0.017)	(0.052)
Normal Rainfall	0.090	0.10	-29.1	0.067	-0.089	-0.18
	(0.16)	(0.066)	(80.0)	(0.52)	(0.074)	(0.17)
Nornal Rainfall x Exposure		-0.020	6.37	-0.018	0.022	0.046
		(0.013)	(15.5)	(0.10)	(0.015)	(0.034)
Observations	0.090	11686	7230	2057	2027	2091
Mean of Dependent Variable	(0.16)	1.98	1107.9	4.34	0.091	0.076
Fixed Effects	Birth year x state	Birth year x state				

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

4.4 Robustness Checks

4.4.1 Selective Fertility

In Table 10 we investigate how PROGRESA and rainfall shocks may have affected fertility, which could lead to potential selection issues. One concern might be that negative rainfall shocks during a year may affect the number of children that are born and/or survive to school-aged years. If this were the case, the composition of individuals in our sample who were born in shock years would be different from those in our sample born in regular years. In order to check this, we collapse to the locality by birth year level and count the total number of children born in a particular year in each locality. We then use this constructed panel to regress the total number of children born that year on our rainfall shock. Column 1 of Table 10 reports results from this regression. We find no evidence of selective fertility or selective child mortality.

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

In addition to investigating effects on actual fertility, we also ask whether PROGRESA or rainfall affected planned or expected fertility. To answer this question, we use questions on expected and desired fertility for the mothers of our sample children who were part of a detailed fertility questionnaire subsample. We do not find that rainfall shocks (or PROGRESA) affected the total number of desired children or the desire for additional children, as we show in the last three columns of Table 10.

4.4.2 Attrition

Table 11: 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	-0.0042	-0.0034	0.0034	-0.033
	(0.0089)	(0.0038)	(0.0037)	(0.038)
Normal Rainfall	-0.034	-0.030	0.0017	-0.13
	(0.038)	(0.023)	(0.019)	(0.14)
Normal Rainfall x Exposure	0.0065	0.0049	0.000028	0.039
	(0.0076)	(0.0045)	(0.0037)	(0.040)
Observations	14525	12917	12159	1646
Mean of Dependent Variable	0.89	0.94	0.97	0.70
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).
-"Normal Rainfall" = 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.

As in any longitudinal study, we must consider the extent to which selective attrition may be confounding our results. In Table 11, we show that although attrition between the baseline and 2003 surveys was sizeable, it appears to be uncorrelated with our regressors of interest. In this table, we simply regress various attrition indicators on years of PROGRESA exposure, the 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 Migration

In addition to selective fertility and attrition, selective migration in response to rainfall shocks could also be a concern. In particular, we might worry that permanent household-level migration responds to rainfall shocks, which would mean that year-of-birth rainfall shocks might affect the probability of an individual showing up in our PROGRESA localities in the first place. Unfortunately, we cannot study the migration behavior of households that never made it into our sample. What we can do is check whether rainfall shocks in 1997 affect the probability of a household participating in the 2003 survey: this six-year gap between the shock and migration outcome would capture more delayed migration responses, similar to what might have occurred for individuals whose household migrated before they started school. Although migration is not the only reason a household could be missing from the sample in 2003, it is likely the main one, and as we show in Table A6, we find no effects of 1997 rainfall shocks on this migration-related outcome.

Another potential issue is that rainfall shocks during an individual's year of birth might affect the temporary (rather than permanent) migration decisions of their parents. If a child grows up without a father in the household as a result, this could generate effects on their development separate from the mechanisms we have focused on in this paper. Therefore, in Table A6, we check to see whether rainfall shocks at birth affect the likelihood of a child's father being present in the household during the 1997 survey (the survey closest to the time of birth), and find no evidence of this.[31]

[31]Of course, this variable does not fully capture a fathers presence throughout the child's life, particularly in the early years, which could still have been affected by year-of-birth rainfall shocks. Even if this were the case, however, we do not view an absent father as a threat to identification but rather another potential mechanism through which adverse rainfall

4.4.4 Balance

We investigate further the implications of the small but statistically significant imbalance in rainfall shock prevalence across PROGRESA treatment and control villages in our baseline sample. First, to test whether our results are being driven by this imbalance, we repeat our analysis using the trimmed sample described in Section 2, in which rainfall shock prevalence is the same across treatment and control villages. This sample omits localities exhibiting shocks in every year, or no shocks in any year, over the study period. As Table A7 shows, our results are virtually identical to the full sample results.[32]

Another way to approach this issue is by comparing our original estimates to results obtained from a weighted regression, where we use inverse probability weighting to re-weight the observations so that the distribution of rainfall shocks is the same across treatment and control groups. As shown in Table A8, the point estimates are almost identical to the results in Tables 7, 8, and 9, suggesting that the statistically significant imbalance was too small in magnitude to substantially affect our original estimates.

We also conduct a robustness exercise regarding the unbalanced demographic characteristics across treatment and control villages in Appendix Table A2, which we discuss in section 2 and which have been identified in previous studies. Table A10 reports the results of regressions on our main outcomes of interest, additionally controlling for interactions between the rainfall shock variable and each of the control variables that are not balanced across treatment and control groups. The results are once again very similar to the main results reported above.

Finally, we address the imbalance in age across treatment villages and rainfall shock groups, which was revealed by the balance checks conducted in Table A4. Because we include birth year fixed effects in our regressions, we are not concerned about these imbalances affecting the estimation of the PROGRESA and rainfall shock main effects. However, these imbalances could be affecting the estimation of the interaction effect, if there is any heterogeneity by age. To address this problem, we use inverse probability weighting to re-weight our sample so that the age distributions are balanced across treatment and control as well as across individuals born during normal and shock years. The results in Table A9,[33] if anything, are stronger after using this weighting procedure, which suggests that our results were not a spurious

shocks could have affected later-life educational outcomes, likely much less important than the biological mechanisms discussed in section 4.1.

[32]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 table and for the remainder of this section.

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

consequence of this age imbalance.

4.4.5 Other Programs

One potential threat to validity is the rollout of other programs during the period between the birth years of our sample individuals and our survey year, 2003. In particular, though we argue that the occurrence of a rainfall shock is random, it is possible that a rainfall shock in a given year affects the probability of a household or locality being the target of another program in subsequent years. This of course is more of a concern in situations where localities are hit by repeated shocks, which are more likely to affect future agricultural activity than a single shock. To this end, the exercise conducted in Table A7 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.[34] In short, an important concern is whether trends over time varied for localities growing different types of crops. To address this concern, we create indicators for whether a locality reports corn, kidney beans, or sugar as one of their top three crops, and interact these indicators with individual birth year dummies.

Finally, we also control for the rollout of a land certification program (PROCEDE) that essentially eliminated the link between land use and land rights in communally farmed agricultural communities called *ejidos*. PROCEDE has been found to have affected migration decisions (De Janvry et al., 2015)

[34] 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).

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,[35] we address concerns that correlations between PROCEDE's rollout and rainfall shocks might be confounding our estimates.

Appendix Table A11 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.

4.4.6 Alternate Variable Definitions

We investigate the robustness of our results to other methods of defining our two main independent variables of interest. First, we show that our results are robust to replacing our PROGRESA exposure variable (rounded to the nearest year) with a simple treatment village indicator (Table A12) and a years of exposure variable that is not rounded to the nearest year (Table A13). Our pattern of results is preserved across both variables and all outcomes. In Table A12, the effects are slightly weaker for some outcomes, which indicates that the additional cohort-level variation linked to the schooling incentive is important.

In our main results, we generate rainfall shocks by calculating historical means (and standard deviations) for each locality birth-year observation using locality-level rainfall over the 10 years prior to each relevant year. In Table A14, we instead calculate these values using rainfall over the 20-year period centered around the median birth year in the sample, from 1978 to 1998. Our results are robust to the use of this longer time-frame that is consistent across all years.

We have chosen the simple rainfall indicator that we use in the main results because it is parsimonious, captures non-linearities, and easy to interpret. However, we can certainly allow for greater flexibility in this specification, which we do in Table A15. Here, we replace our simple indicator with four dummy variables for normalized birth-year rainfall below the 20th percentile ("droughts"), between the 20th and 40th percentile ("below normal"), between the 60th and 80th percentile ("above normal"), and above the 80th percentile ("floods") of the normalized rainfall distribution. Rainfall around the median – 40th to 60th percentile – is the omitted category.[36] Consistent with our main results, we see

[35] 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.

[36] Droughts and floods are roughly (though not exactly) equivalent to using the one-standard-deviation cutoff that we

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

We focus on rainfall shocks in an individual's year of birth, specifically, because we are interested in shocks that affect a child's endowment very early in life. A shock to the endowment during the year of birth should provide the cleanest and earliest source of exogenous variation, but it is of course possible that shocks during early childhood could also affect later-life outcomes. To investigate whether shocks in other years of life had similar positive effects on later-life outcomes, and similar interactions with PROGRESA, we add additional rainfall shock variables to our regressions and report the results in Table A16. Specifically, we add indicators for normal rainfall realizations during an individual's first, second, and third year of life, along with their interactions with PROGRESA exposure. Consistent with Maccini and Yang (2009), we find that year of birth rainfall is the only one that has consistently large and significant effects across all of our education outcomes. Accordingly, PROGRESA's ability to remediate is only apparent with respect to birth-year rainfall and not rainfall in any other year. Like in Table A15, the inclusion of a number of additional regressors to the employment regressions leads to a general lack of precision in the last three columns, but it is important to note that – across both employment and education outcomes – our three main coefficients of interest are almost identical in magnitude to the estimates in Tables 7, 8, and 9.

5 Model and Interpretation

5.1 Model of Endogenous Schooling

Having established empirically that PROGRESA was able to help remediate for the disadvantage generated by a negative early life shock, we now use an endogenous schooling choice model to explore what

use for our main normal rainfall dummy.

might be generating this remediation. We extend the model in Card (2001) by allowing individuals to have heterogeneous initial endowments that affect future earnings. We study how the optimal level of schooling changes with the initial endowment and with exogenous education policies, like PROGRESA, that attempt to reduce the opportunity cost of schooling.

We assume that individuals have an infinite time horizon: they attend school during the first S periods of life and work full-time for the rest of it. Individuals have an initial level of endowment, ω, that affects the earnings function in each period. While in school, the utility in period t depends on the level of consumption, $u(c(t))$,[37] and the effort cost for the t-th year of schooling, $\phi(t)$: specifically, in-school utility is equal to $u(c(t)) - \phi(t)$. Out of school, the utility is $u(c(t))$. Finally, individuals discount future flows at a rate ρ.

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

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

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

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

We now introduce two simple assumptions, similar to those used by Card (2001), that help us characterize the optimal level of schooling.

Assumption 1. $y(\omega, S, t) \equiv f(\omega, S) h(t - S)$, where $h(0) = 1$ and $f(\cdot)$ is increasing with respect to ω and S, and concave with respect to S, for all ω.

A1 assumes that the log of earnings is additively separable in years of post-schooling experience, and a function of an individual's education and initial endowment. We also assume that the earnings are increasing with respect to the initial endowment and the level of schooling, and exhibit decreasing

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

marginal returns to schooling for all levels of the initial endowment.

Assumption 2 $\phi(t)$ is increasing and convex with respect to t.

When $u(c(t)) = \log c(t)$ (as in Card (2001)), we show in the appendix (section B.2) that the optimal level of schooling, S^* is uniquely defined by[38]

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

where $\Gamma(\omega, S) \equiv \frac{f_S(\omega,S)}{f(\omega,S)}$, which is the marginal return to schooling. Let $d(x,\omega,S) \equiv \frac{1}{H(R)} + \frac{T-P-x}{f(\omega,S)H(R)} + \rho\phi(S) e^{-\rho S}$, be the marginal cost of schooling standardized by lifetime earnings.[39] The following lemma describes how PROGRESA and the initial endowment affect the optimal level of schooling.

Lemma 1. Suppose (A1) and (A2) hold.

(*i*) The optimal level of schooling is increasing with respect to PROGRESA x, i.e., $\frac{\partial S^*}{\partial x} > 0$.

(*ii*) If $f(\cdot)$ is log-supermodular with respect to (S, w),[40] the optimal level of schooling is increasing with respect to the initial endowment, ω, i.e., $\frac{\partial S^*}{\partial \omega} > 0$.[41]

5.2 What drives remediation?

We now study how PROGRESA affects the optimal level of schooling, S^*, differently for individuals with different levels of the initial endowment. We explore what factors determine whether we see reinforcement (larger PROGRESA effects for higher-endowment individuals) or remediation (larger PROGRESA effects for lower-endowment individuals). In other words, we study under what conditions $\frac{\partial S^*}{\partial\omega\partial x}$ is positive or negative in equilibrium. In Appendix section B.3, we explore remediation and

[38]In order to simplify the discussion we follow Card (2001) and impose two additional assumptions: first, we assume that tuition cost minus part-time earnings minus PROGRESA subsidy are constant over time. Second, we assume that tuition costs minus PROGRESA are small relative to lifetime earnings.

[39]Similar to Card (2001) we assume that the marginal cost of schooling standardized by the life time earnings is increasing with S, i.e., $d_S = -\frac{(T-P-x) f_S(\omega,S)}{f(\omega,S)H(R)} + \rho \frac{\partial}{\partial S}\left(\phi(S) e^{-\rho S}\right) > 0$.

[40]In the proof of Lemma 1 we show that (*ii*) holds even if $f(\cdot)$ is not log-supermodular with respect to (S, w), e.g., suppose that $f(\cdot)$ is log-submodular with respect to (S, w). In this case, we need the marginal cost with respect to the endowment to be negative and sufficiently large in absolute value, to offset the negative effect of the endowment on the marginal return of schooling, i.e., $d_\omega(x,\omega,S) + \frac{\partial}{\partial\omega}\left(\frac{f_S(\omega,S)}{f(\omega,S)}\right) > 0$. For simplicity we assume for the rest of the paper that $f(\cdot)$ is log-supermodular with respect to (S, w).

[41]In the appendix, Lemma 2 presents a similar result to Lemma 1 related to the optimal income function in equilibrium, $f(\omega, S^*)$.

reinforcement in terms of equilibrium income, y^*.

Whether PROGRESA generates reinforcement or remediation depends on,[42]

$$\frac{\partial S^*}{\partial\omega\partial x} = \alpha_1\Gamma_{S\omega}\left(\omega, S^*\right) - \Theta\left(x,\omega,S^*\right) + \alpha_3\left(\Gamma_{SS} - \rho\frac{\partial^2}{\partial^2 S}\left(\phi\left(S^*\right)e^{-\rho S^*}\right)\right), \tag{3}$$

where $\Gamma_{S\omega}\left(\omega,S\right) = \frac{\partial^2}{\partial\omega\partial S}\Gamma\left(S,x,\omega\right)$, $\Theta\left(x,\omega,S^*\right) \equiv \alpha_4\left(\frac{f_\omega(\omega,S)}{f(\omega,S)H(R)}\right) + \alpha_2\left(\frac{f_S(\omega,S)}{f(\omega,S)H(R)}\right)$, and, $\alpha_1 \equiv \frac{\Lambda_x}{\Lambda_S^2}$, $\alpha_2 \equiv \frac{\Lambda_\omega}{\Lambda_S^2}$, $\alpha_3 \equiv -\frac{\Lambda_x\Lambda_\omega}{\Lambda_S^3}$ and $\alpha_4 \equiv -\frac{1}{\Lambda_S}$ where $\Lambda \equiv \Gamma\left(\omega,S\right) - d\left(x,S\right)$. Note that $\alpha_i > 0$ for $i \in \{1,2,3,4\}$. Similarly, $\Theta\left(x,\omega,S^*\right) > 0$, since $f\left(\omega,S\right)$ is increasing with respect ω and S.

Proposition 1. Suppose (A1)-(A2) hold in equilibrium. If the expression in (3) is negative, PROGRESA generates remediation – larger PROGRESA effects (on the optimal level of schooling, S^*) for lower-endowment individuals. If this expression is positive, PROGRESA generates reinforcement – larger effects for higher-endowment individuals.

To shed light on what factors may be driving the remediation that we find in our empirical analysis, we discuss each term. We begin with the second term, $\Theta\left(x,\omega,S^*\right)$ – the weighted sum of two positive terms – which enters the $\frac{\partial S^*}{\partial\omega\partial x}$ expression (3) negatively and is therefore a key factor contributing to remediation. This term captures how PROGRESA changes the cost of schooling (relative to foregone earnings) differently for high-endowment and low-endowment individuals. The fact that this term is positive means that PROGRESA decreases the relative cost of schooling more for lower-endowment individuals, and therefore generates larger increases in optimal schooling for these lower-endowment individuals compared to higher-endowment individuals. We explore the reasons for this in appendix section B.1.1. Intuitively, PROGRESA has a larger effect on lower-endowment individuals because the nominal value of the transfer is a larger share of their earnings compared to higher-endowment individuals, who have higher foregone earnings.

We now move on to the third term in the $\frac{\partial S^*}{\partial\omega\partial x}$ expression (3), $\Gamma_{SS} - \rho\frac{\partial^2}{\partial^2 S}\left(\phi\left(S^*\right)e^{-\rho S^*}\right)$. The first part, Γ_{SS}, depends on the third derivative of $\log f\left(\cdot\right)$, which is difficult to interpret. However, the second term, $\rho\frac{\partial^2}{\partial^2 S}\left(\phi\left(S^*\right)e^{-\rho S^*}\right)$, is positive due to the convexity of the cost function (assumption A2) and therefore has a negative effect on the entire expression. Importantly, this identifies convex effort costs as another potential reason for the remediation we find in our empirical analysis. Because high

[42]To simplify the notation, we follow Card (2001) and assume that savings during school are close to zero i.e., $(T - P - x) \approx 0$. In the appendix we discuss the more general case in which $(T - P - x)$ is different from 0.

ability individuals obtain more schooling, absent the PROGRESA incentive, it is more difficult for a program like PROGRESA to increase the schooling of a high-endowment individual (compared to a low-endowment individual) due to the effort cost curve being steeper at the higher level of schooling of higher-endowment individuals. It is important to note, however, that we cannot make any statements about how *changes* in the convexity will affect the overall sign and magnitude of the expression in equation 3 because the weights α_i also depend on the convexity of the cost function.

These two dimensions of the cost of schooling are the main reasons for the remediation we are finding in this empirical context. However, the shape of the earnings function is also involved in the first term of (3). Specifically, $\Gamma_{S\omega}(\omega, S)$ measures how the curvature of the log earnings function, with respect to schooling, changes with the initial endowment. To better understand the sign of $\Gamma_{S\omega}(\omega, S)$, suppose that $f(\cdot)$ can be approximated by a Constant Elasticity of Substitution (CES) function, i.e., $f(\omega, S) = A \cdot [\lambda\omega^{\kappa} + (1-\lambda) S^{\kappa}]^{\frac{1}{\kappa}}$, where $\lambda \in [0, 1]$.[43] In the appendix (section B.1.2), we show that $\Gamma_{S\omega}(\omega, S) < 0$ if $\kappa < 0$, given that the contribution of both inputs are similar. That is, the first term of (3) is negative if schooling and endowments are complements.[44] Similarly, $\Gamma_{S\omega}(\omega, S) = 0$ if $\kappa = 0$, that is, $\Gamma_{S\omega}(\omega, S) = 0$ if $f(\cdot)$ is a *Cobb-Douglas* function.

Although we can make these claims about the sign of the first term, $\Gamma_{S\omega}(\omega, S)$, it is important to note that the expression in (3) does not become more negative (or positive) as κ approaches to $-\infty$, as the other terms of (3) also depend on κ. That is, we cannot claim any monotonicity in the relationship between $\frac{\partial S^*}{\partial\omega\partial x}$ and κ, the degree of complementarity or substitutability between the endowment and schooling in the earnings function. Remediation – that is, larger PROGRESA effects for lower-endowment individuals – is possible whether schooling and endowments are complements or substitutes in the earnings function. As we have shown here, the level of schooling is the result of an optimization problem, not a direct output of some "production" function – which means that complementarity (or substitutability) in the earnings function does not imply reinforcement (or remediation). Similarly, remediation itself does not necessarily imply complementarity or substitutability in the earnings function.

[43] κ determines the elasticity of substitution between the level of schooling and the initial endowment in the earnings function, which is defined by $\frac{1}{1-\kappa}$, and A is a factor-neutral productivity parameter. Under this technology, $\kappa \in [-\infty, 1]$; as κ approaches 1, the level of schooling and the initial endowment become perfect substitutes and as κ approaches $-\infty$, the factors become perfect complements.

[44] This might seem counter-intuitive because complementarity in the production function is often assumed to imply reinforcement – larger positive effects of an intervention on high-endowment individuals. However, as we discuss below, the level of schooling is the result of an optimization problem, and not a direct output of the earnings or "production" function. Here, complementarity between schooling and endowments in the production function mean that the marginal benefit of schooling, *relative to foregone earnings*, will increase more for low endowment than high endowment individuals due to the lower value of their foregone earnings.

On the other hand, convex effort costs and lower foregone wages for low-endowment individuals are primary factors driving the remediation we find in our empirical context irrespective of the shape of the earnings function.

6 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 higher schooling attainment and employment probabilities. Moreover, the coefficient on the interaction between PROGRESA exposure and normal rainfall is negative and significant across most outcome measures, indicating that remediation of early-life shocks is possible through investments.

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.

This study contributes to the large literature evaluating PROGRESA, and more specifically, to our knowledge about the program's ability to mitigate shocks. Two studies investigate the ability of PROGRESA to mitigate for contemporaneous weather shocks and find mixed results: De Janvry et al. (2006) find that PROGRESA protected child enrollment from declining due to contemporaneous weather shocks, but Aguilar and Vicarelli (2011) find that the health component of the program did not mitigate the negative impact of a 1999 El Niño shock on child development. Unlike both of these studies, we look at shocks that took place over a decade prior to the beginning of the program and find strong evidence for remediation with respect to educational outcomes.

Our results also speak to the literature on cash transfer programs more generally (Behrman et al., 2011; Blattman et al., 2013; Haushofer and Shapiro, 2013; Schultz, 2004). While most evaluations of such programs tend to focus on average effects, we compare impacts across individuals with different unobserved endowments, exploiting rainfall shocks as our source of exogenous variation in this unobservable. PROGRESA 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

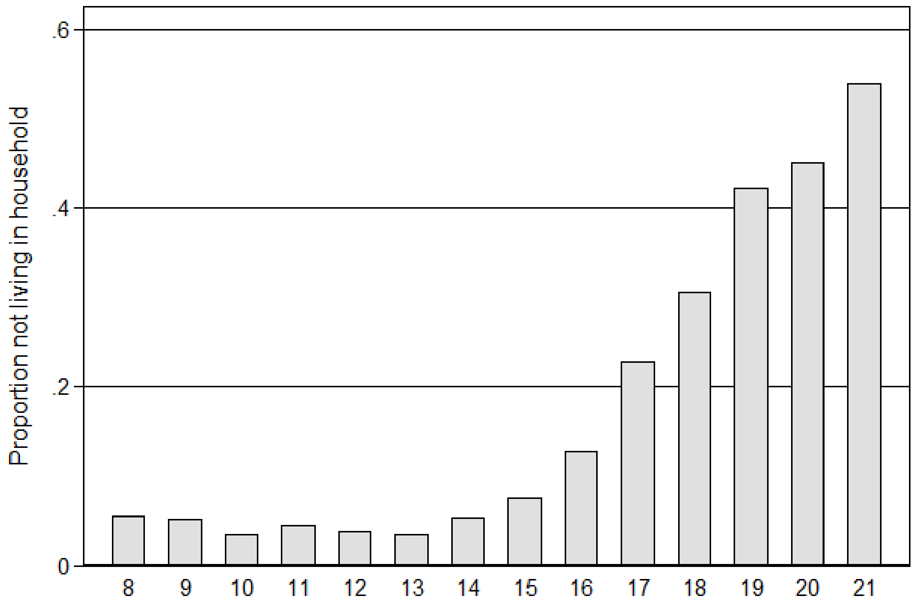
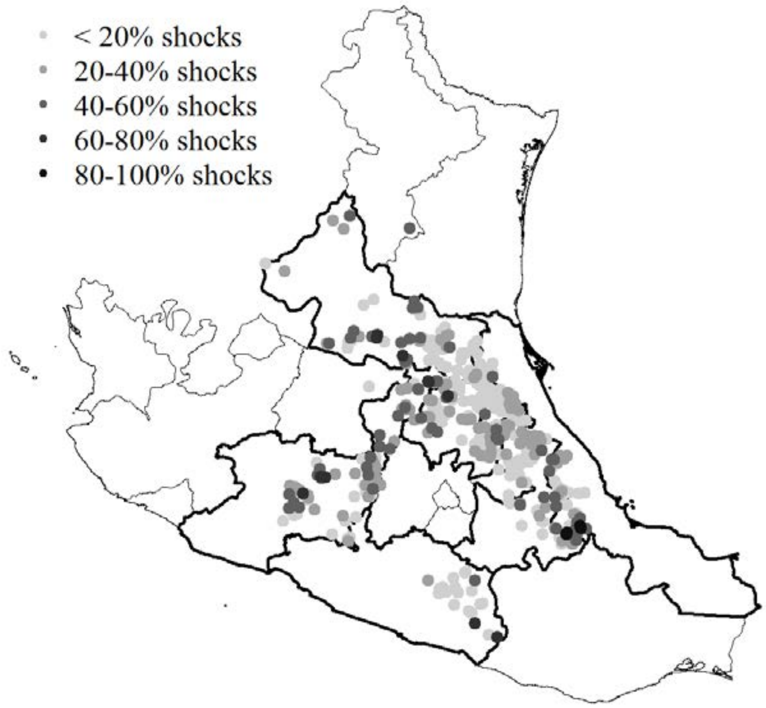


Table A1: Exposure to PROGRESA

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

Panel B: Locality-level

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

Notes:
Standard 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.

Table A3: Summary of Related PROGRESA Literature

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

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

Table A4: Rainfall Shocks, PROGRESA, and Baseline Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
	Mother's education	Father's education	Mother primary only	Mother secondary	Father primary only	Father secondary	Mother Education Missing	Father's Education Missing	Household head age	Female household head	Number of children aged 0-2	Number of children aged 3-5	Number of boys aged 6-7	Number of boys aged 8-12	Number of boys aged 13-18	Number of girls aged 6-7	Number of girls aged 8-12	Number of girls aged 13-18	Number of women aged 19-54	Number of men aged 55 and over
Treatment Village	-0.0017	0.15	0.0029	-0.0042	0.026	0.013	-0.024	-0.0072	-0.81	-0.00061	0.0015	0.0034	-0.0026	0.0047	0.00049	0.00095	-0.0022	-0.0015	-0.00088	-0.00088
	(0.13)	(0.15)	(0.028)	(0.0070)	(0.029)	(0.0085)	(0.028)	(0.024)	(0.42)*	(0.0068)	(0.0029)	(0.0030)	(0.0025)	(0.0035)	(0.0027)	(0.0023)	(0.0041)	(0.0025)	(0.0019)	(0.0015)
Normal Rainfall	-0.051	-0.032	0.000057	-0.0018	-0.017	0.0044	0.016	-0.017	0.45	0.0033	0.00097	-0.0014	-0.0040	0.0032	0.0042	-0.0031	-0.0010	0.00033	-0.0019	0.00026
	(0.057)	(0.066)	(0.013)	(0.0049)	(0.015)	(0.0051)	(0.016)	(0.013)	(0.24)*	(0.0055)	(0.0020)	(0.0021)	(0.0024)*	(0.0027)	(0.0020)**	(0.0019)	(0.0034)	(0.0021)	(0.0015)	(0.0010)
Observations	7998	8424	7998	7998	8424	8424	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159
Mean of Dep. Var.	3.93	3.98	0.29	0.037	0.30	0.050	0.34	0.31	41.7	0.057	0.073	0.10	0.052	0.12	0.070	0.051	0.12	0.066	0.16	0.019
Fixed Effects										Birth year x state										

	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)	(33)	(34)	(35)	(36)	(37)	(38)	(39)	(40)
	Number of women aged 55 and over	Mother speaks indigenous language	Father speaks indigenous language	Mother's language missing	Father's language missing	Community well	Well spring	Public water network	Bury garbage	Public dumpster	Public drainage	Public phone	Hospital or health center	Distance to health center	DICONSA store	Distance to bank	Distance to bank missing	Distance to secondary school	Distance to secondary school missing	Age
Treatment Village	-0.0019	-0.016	-0.025	-0.0047	0.0032	-0.023	0.052	-0.083	0.059	-0.027	-0.017	-0.0025	-0.041	1.58	0.095	0.56	0.0012	0.79	0.026	-0.10
	(0.0015)	(0.073)	(0.072)	(0.0056)	(0.0079)	(0.061)	(0.065)	(0.050)	(0.042)	(0.018)	(0.020)	(0.062)	(0.065)	(2.30)	(0.056)*	(5.32)	(0.042)	(1.63)	(0.071)	(0.046)**
Normal Rainfall	0.0018	0.083	0.088	0.0029	-0.0036	-0.027	0.083	-0.025	0.026	-0.010	0.0077	0.028	0.00059	0.93	-0.0024	0.29	-0.013	0.078	-0.0031	0.50
	(0.0011)*	(0.027)***	(0.027)***	(0.0043)	(0.0072)	(0.025)	(0.029)***	(0.017)	(0.022)	(0.0092)	(0.0081)	(0.031)	(0.023)	(1.01)	(0.024)	(1.84)	(0.018)	(0.58)	(0.026)	(0.17)***
Observations	12159	11663	10995	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159	12159
Mean of Dep. Var.	0.017	0.38	0.39	0.041	0.096	0.36	0.51	0.14	0.16	0.015	0.037	0.57	0.20	12.6	0.30	33.6	0.11	5.55	0.57	15.0
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).
-"Normal Rainfall" = 1 for individuals whose birth-year rainfall was within one standard deviation of the 10-year historical locality-specific mean
-"Treatment village" = 1 for individuals in villages assigned to PROGRESA in the first wave (1998).

Table A5: Effects of PROGRESA and Rainfall on Woodcock-Johnson Test Scores

	(1)	(2)	(3)	(4)
	Letter Word Identification	Applied Problems	Dictation	Average Score
Years of PROGRESA Exposure	-0.052	0.014	0.060	0.0079
	(0.051)	(0.050)	(0.060)	(0.045)
Normal Rainfall	-0.13	0.15	0.18	0.064
	(0.23)	(0.25)	(0.28)	(0.21)
Normal Rainfall x Exposure	0.056	-0.00039	-0.033	0.0046
	(0.048)	(0.052)	(0.057)	(0.044)
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).
-"Normal Rainfall" = 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 18
- Scores are standardized by test type, and the average score in column 4 takes the average across all three z-scores.

Table A6: Effects of Rainfall on Migration-Related Variables

	(1)	(2)	(3)	(4)
	Father living in household in 1997	Father living in household in 1997	Household found in 2003	Household found in 2003
Normal Rainfall (in birth year)	0.0025	0.0059		
	(0.0070)	(0.0074)		
Normal Rainfall (in 1997)			-0.0050	-0.021
			(0.014)	(0.016)
Observations	12156	12156	6684	6684
Mean of Dependent Variable	0.91	0.91	0.88	0.88
Fixed Effects	None	Birth year x state	None	Birth year x state

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Normal Rainfall" = 1 if relevant rainfall value fell within one standard deviation of the 10-year historical locality-specific mean.
-Columns 3 and 4 are household-level regressions

Table A7: Effects of PROGRESA and Rainfall on Education and Employment Outcomes: Trimmed Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.23	0.031	0.035	0.061	0.060	0.071	0.010	0.10	0.11
	(0.058)***	(0.014)**	(0.011)***	(0.016)***	(0.016)***	(0.017)***	(0.014)	(0.040)**	(0.046)**
Normal Rainfall	0.71	0.12	0.14	0.21	0.25	0.27	0.096	0.32	0.35
	(0.29)**	(0.058)**	(0.053)**	(0.071)***	(0.072)***	(0.085)***	(0.056)*	(0.13)**	(0.15)**
Normal Rainfall x Exposure	-0.12	-0.022	-0.021	-0.033	-0.043	-0.048	-0.016	-0.12	-0.12
	(0.056)**	(0.011)*	(0.011)**	(0.014)**	(0.013)***	(0.016)***	(0.018)	(0.041)***	(0.046)***
Observations	10236	9713	10236	8689	7160	5684	1320	966	962
Mean of Dependent Variable	6.78	0.59	0.47	0.56	0.51	0.45	0.065	0.35	0.41
Ages	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Normal Rainfall" = 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 A8: Effects of PROGRESA and Rainfall on Education and Employment Outcomes, Re-weighted on Shock Probability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.22	0.030	0.031	0.055	0.055	0.066	0.013	0.084	0.086
	(0.054)***	(0.013)**	(0.011)***	(0.016)***	(0.016)***	(0.017)***	(0.014)	(0.039)**	(0.043)**
Normal Rainfall	0.65	0.11	0.12	0.20	0.23	0.25	0.10	0.22	0.26
	(0.28)**	(0.056)*	(0.051)**	(0.070)***	(0.071)***	(0.083)***	(0.053)*	(0.13)*	(0.13)*
Normal Rainfall x Exposure	-0.11	-0.020	-0.019	-0.032	-0.040	-0.047	-0.018	-0.099	-0.10
	(0.053)**	(0.011)*	(0.010)*	(0.014)**	(0.013)***	(0.016)***	(0.017)	(0.040)**	(0.042)**
Observations	11824	11216	11824	10068	8285	6618	1597	1143	1138
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Ages	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Normal Rainfall" = 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 are weighted to produce a sample that is balanced on rainfall shocks across treatment and control groups.

Table A9: Effects of PROGRESA and Rainfall on Education and Employment Outcomes, Re-weighted on Age

	(1)	(2)	(3)	(4)	(5)	(6)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades
Years of PROGRESA Exposure	0.26	0.041	0.034	0.057	0.056	0.068
	(0.060)***	(0.013)***	(0.0100)***	(0.016)***	(0.016)***	(0.018)***
Normal Rainfall	0.93	0.17	0.13	0.22	0.24	0.28
	(0.31)***	(0.052)***	(0.044)***	(0.071)***	(0.073)***	(0.088)***
Normal Rainfall x Exposure	-0.16	-0.030	-0.019	-0.035	-0.042	-0.050
	(0.059)***	(0.010)***	(0.0090)**	(0.014)**	(0.014)***	(0.017)***
Observations	11824	11216	11824	10068	8285	6618
Mean of Dependent Variable	6.76	0.57	0.46	0.56	0.51	0.44
Ages	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18
Fixed Effects	Birth year x state					

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

-"Normal Rainfall" = 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 are weighted to produce the same age distributions across the four groups defined by treatment status and rainfall type.

Table A10: 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)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.22	0.030	0.031	0.055	0.053	0.061	0.022	0.081	0.090
	(0.055)***	(0.013)**	(0.011)***	(0.016)***	(0.016)***	(0.016)***	(0.014)	(0.051)	(0.050)*
Normal Rainfall	0.51	0.14	0.070	0.17	0.16	0.24	0.20	0.56	0.63
	(0.36)	(0.084)*	(0.077)	(0.096)*	(0.10)	(0.12)**	(0.086)**	(0.36)	(0.38)*
Normal Rainfall x Exposure	-0.11	-0.019	-0.019	-0.032	-0.039	-0.041	-0.027	-0.096	-0.11
	(0.054)**	(0.011)*	(0.010)*	(0.014)**	(0.013)***	(0.015)***	(0.018)	(0.052)*	(0.051)**
Observations	11824	11216	11824	10068	8285	6618	1597	1143	1138
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Ages	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects	Birth year x state								

- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Normal Rainfall" = 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 A11: Effects of PROGRESA and Rainfall on Education and Employment Outcomes, Controlling for Other Government Programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.21	0.029	0.031	0.053	0.052	0.065	0.0091	0.094	0.089
	(0.054)***	(0.013)**	(0.011)***	(0.015)***	(0.016)***	(0.016)***	(0.012)	(0.038)**	(0.040)**
Normal Rainfall	0.64	0.10	0.12	0.20	0.22	0.26	0.082	0.25	0.26
	(0.28)**	(0.054)*	(0.050)**	(0.071)***	(0.073)***	(0.084)***	(0.047)*	(0.12)**	(0.12)**
Normal Rainfall x Exposure	-0.11	-0.019	-0.019	-0.031	-0.040	-0.048	-0.013	-0.11	-0.10
	(0.054)**	(0.011)*	(0.010)*	(0.014)**	(0.013)***	(0.016)***	(0.015)	(0.037)***	(0.039)***
Observations	11734	11135	11734	9992	8225	6575	1587	1134	1131
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.060	0.35	0.41
Ages	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Normal Rainfall" = 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 A12: Effects of PROGRESA and Rainfall, Using Treatment Village Dummy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Treatment Village	0.29	0.046	0.062	0.099	0.10	0.11	0.025	0.17	0.17
	(0.11)***	(0.027)*	(0.022)***	(0.035)***	(0.036)***	(0.040)***	(0.027)	(0.079)**	(0.086)**
Normal Rainfall	0.15	0.026	0.050	0.072	0.068	0.063	0.068	0.023	0.059
	(0.094)	(0.022)	(0.019)***	(0.026)***	(0.029)**	(0.033)*	(0.024)***	(0.061)	(0.062)
Normal Rainfall x Treatment	-0.080	-0.025	-0.040	-0.050	-0.069	-0.062	-0.035	-0.20	-0.20
	(0.11)	(0.025)	(0.023)*	(0.033)	(0.034)**	(0.040)	(0.033)	(0.079)**	(0.083)**
Observations	11824	11216	11824	10068	8285	6618	1597	1143	1138
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Ages	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Normal Rainfall" = 1 for individuals whose birth-year rainfall was within one standard deviation of the 10-year historical locality-specific mean
-"Treatment village" = 1 for individuals in villages assigned to PROGRESA in the first wave (1998).
-All specifications include gender, household head gender and age, household size, household composition variables, parental education, parental language, and locality characteristics. Controls for parental language/education and locality distance include dummies for missing values

Table A13: Effects of PROGRESA and Rainfall, Using Exact Years of Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.26	0.036	0.037	0.067	0.066	0.079	0.015	0.10	0.10
(not rounded to the nearest year)	(0.068)***	(0.017)**	(0.014)***	(0.019)***	(0.019)***	(0.021)***	(0.016)	(0.047)**	(0.051)**
Normal Rainfall	0.71	0.12	0.13	0.23	0.26	0.29	0.12	0.32	0.36
	(0.32)**	(0.066)*	(0.061)**	(0.084)***	(0.085)***	(0.100)***	(0.069)*	(0.17)*	(0.17)**
Normal Rainfall x Months	-0.13	-0.023	-0.021	-0.039	-0.049	-0.056	-0.021	-0.12	-0.12
	(0.065)*	(0.014)*	(0.013)*	(0.017)**	(0.017)***	(0.020)***	(0.020)	(0.047)**	(0.050)**
Observations	11824	11216	11824	10068	8285	6618	1597	1143	1138
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Ages	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Normal Rainfall" = 1 for individuals whose birth-year rainfall was within one standard deviation of the 10-year historical locality-specific mean
-The PROGRESA exposure variable in this regression is calculated by dividing months of exposure by 12 (not rounding to the nearest year, as we do in our main results).
-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 A14: Effects of PROGRESA and Rainfall, Using Alternate Historical Rainfall Calculations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.18	0.026	0.035	0.050	0.046	0.056	0.0028	0.071	0.066
	(0.054)***	(0.014)*	(0.012)***	(0.015)***	(0.016)***	(0.018)***	(0.017)	(0.041)*	(0.046)
Normal Rainfall	0.26	0.056	0.12	0.12	0.13	0.15	0.055	0.19	0.20
	(0.26)	(0.064)	(0.049)**	(0.061)**	(0.067)*	(0.083)*	(0.061)	(0.12)	(0.14)
Normal Rainfall x Exposure	-0.060	-0.014	-0.023	-0.025	-0.028	-0.033	-0.0070	-0.082	-0.077
	(0.051)	(0.013)	(0.0099)**	(0.012)**	(0.013)**	(0.016)**	(0.018)	(0.042)*	(0.046)*
Observations	11824	11216	11824	10068	8285	6618	1597	1143	1138
Mean of Dependent Variable	6.79	0.58	0.46	0.56	0.52	0.45	0.061	0.35	0.41
Ages	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

Notes:
- Standard errors clustered at the municipality level are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
-"Normal Rainfall" = 1 for individuals whose birth-year rainfall was within one standard deviation of the 20-year locality-specific mean from 1978-1998 (centered around the median birth year)
-All specifications include gender, household head gender and age, household size, household composition variables, parental education, parental language, and locality characteristics. Controls for parental language/education and locality distance include dummies for missing values

Table A15: Effects of PROGRESA and Rainfall, Using Flexible Definition of Rainfall Shock

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

Notes:

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

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

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

Table A16: Effects of PROGRESA and Rainfall in Year of Birth and Subsequent Childhood Years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Educational Attainment	Grade Progression	Appropriate Grade Completion	Completed 7 grades	Completed 8 grades	Completed 9 grades	Currently Enrolled w/ HS Degree	Worked in Non-Laborer Job	Enrolled or Worked in Non-Laborer Job
Years of PROGRESA Exposure	0.22	0.039	0.042	0.056	0.062	0.071	-0.0025	0.088	0.075
	(0.074)***	(0.018)**	(0.015)***	(0.019)***	(0.020)***	(0.023)***	(0.025)	(0.067)	(0.074)
Normal Rainfall in year of birth	0.65	0.10	0.12	0.22	0.26	0.30	0.091	0.15	0.15
	(0.31)**	(0.060)*	(0.055)**	(0.075)***	(0.077)***	(0.088)***	(0.061)	(0.18)	(0.19)
Normal Rainfall in year of birth x Exposure	-0.12	-0.019	-0.020	-0.037	-0.048	-0.056	-0.014	-0.070	-0.061
	(0.059)**	(0.012)	(0.011)*	(0.014)**	(0.014)***	(0.017)***	(0.019)	(0.051)	(0.056)
Normal Rainfall in first year	0.18	0.066	0.050	0.030	0.012	-0.0067	-0.021	0.14	0.11
	(0.16)	(0.043)	(0.033)	(0.042)	(0.043)	(0.049)	(0.048)	(0.11)	(0.12)
Normal Rainfall in first year x Exposure	-0.042	-0.014	-0.011	-0.0065	-0.00043	0.0024	0.0057	-0.052	-0.039
	(0.030)	(0.0085)	(0.0065)*	(0.0079)	(0.0085)	(0.0099)	(0.014)	(0.033)	(0.035)
Normal Rainfall in second year	-0.24	-0.036	-0.0091	-0.10	-0.039	-0.086	-0.024	-0.0067	0.029
	(0.20)	(0.048)	(0.040)	(0.046)**	(0.056)	(0.054)	(0.049)	(0.11)	(0.12)
Normal Rainfall in second year x Exposure	0.053	0.0066	0.0033	0.019	0.0093	0.019	0.014	-0.00057	-0.0052
	(0.040)	(0.0096)	(0.0085)	(0.0095)**	(0.012)	(0.011)*	(0.014)	(0.033)	(0.036)
Normal Rainfall in third year	0.069	0.046	0.016	0.072	0.064	0.083	-0.0088	0.027	0.0043
	(0.20)	(0.044)	(0.037)	(0.047)	(0.051)	(0.055)	(0.051)	(0.12)	(0.12)
Normal Rainfall in third year x Exposure	-0.0058	-0.0073	-0.0038	-0.0100	-0.0094	-0.017	0.0020	-0.015	-0.0077
	(0.038)	(0.0086)	(0.0075)	(0.0095)	(0.010)	(0.011)	(0.015)	(0.034)	(0.035)
Observations	10679	10127	10679	9033	7418	5846	1398	992	989
Mean of Dependent Variable	6.76	0.58	0.47	0.56	0.51	0.45	0.058	0.36	0.41
Ages	12 to 18	12 to 18	12 to 18	13 to 18	14 to 18	15 to 18	18	18	18
Fixed Effects					Birth year x state				

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

B Model Appendix

B.1 Investigating $\frac{\partial S^*}{\partial \omega \partial x}$

B.1.1 Costs

We explore why the second term of expression (3), $\Theta(x, \omega, S^*)$, is positive. To see why this is the case, note that the first term, $\alpha_4 \left(\frac{f_\omega(\omega,S)}{f(\omega,S)H(R)} \right)$, measures how PROGRESA changes the marginal effect of the endowment on the following term: $\frac{T-P-x}{f(\omega,S^*)H(R)}$ (from equation 2), which represents the cost of education (expressed as a fraction of foregone earnings), ignoring effort costs. Figure 3A plots this cost of education as a function of the endowment, separately for high PROGRESA exposure and low PROGRESA exposure, and shows that it is decreasing in the initial endowment (because higher-endowment individuals have higher lifetime earnings and therefore a larger denominator). This figure also demonstrates that PROGRESA shifts this cost curve down for all levels of the initial endowment but with a bigger shift for lower levels of the endowment. This fact is made more apparent in Figure 3B, where we see that PROGRESA decreases the magnitude of the negative slope (and therefore increases the value of the slope) for all levels of the initial endowment. Intuitively, PROGRESA has a larger effect on low-endowment individuals because the cost reductions are a larger share of their lifetime income compared to high-endowment individuals, who have higher foregone earnings. A similar interpretation applies to the second term, $\alpha_2 \left(\frac{f_S(\omega,S)}{f(\omega,S)H(R)} \right)$.

B.1.2 Parametric Example of Earnings Function

Suppose that

$$f(\omega, S) = A \cdot \left[\lambda \omega^\kappa + (1-\lambda) S^\kappa\right]^{\frac{1}{\kappa}}, \tag{4}$$

where $\kappa \in [-\infty, 1]$ and $\lambda \in [0,1]$. Note that, $f(\cdot)$ is increasing with respect to ω and S, and log-concave with respect to S. Moreover, $f(\cdot)$ is log-supermodular (log-submodular) with respect to (S, w) if $\kappa < 0$ ($\kappa > 0$). Note that

$$\Gamma_{S\omega} = \kappa \cdot \Xi(\omega, S) \frac{\Delta(\omega,S) + 2(1-\lambda) S^\kappa f(\omega,S)^\kappa}{\left[\lambda \omega^\kappa + (1-\lambda) S^\kappa\right]^4}, \tag{5}$$

where $\Xi(\omega, S) \equiv \lambda(1-\lambda) S^{\kappa-2} \omega^{\kappa-1}$ and $\Delta(\omega, S) \equiv (\kappa - 1)\left[(\gamma_S S^\kappa)^2 - (\gamma_\omega \omega^\kappa)^2\right]$. If the contribution

Figure 3A: $\frac{T-P-x}{f(\omega,S^{*})}$

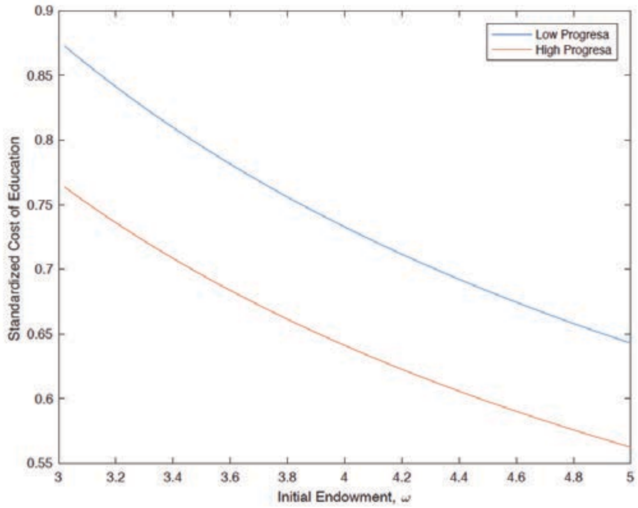


Figure 3B: $\frac{\partial}{\partial\omega}\left(\frac{T-P-x}{f(\omega,S^{*})}\right)$

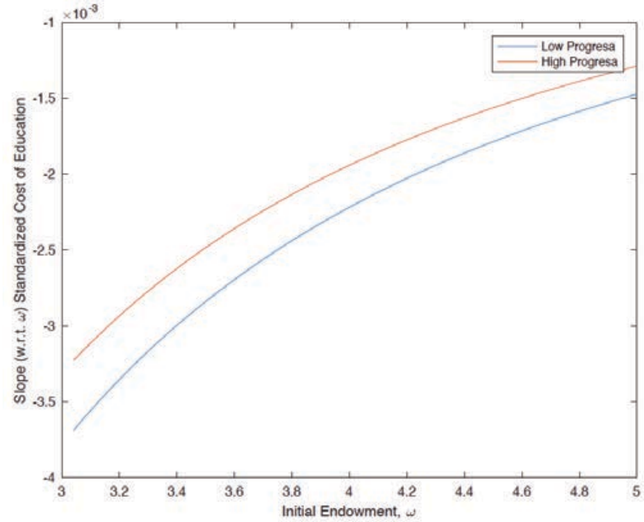


Figure (3A) shows how the standardized cost of schooling, $\frac{T-P-x}{f(\omega,S^{*})}$, change when PROGRESA increases. Figure (3B) shows how the slope (with respect to the initial level of endowment) of the standardized cost of schooling change when PROGRESA increases.

of the endowment and schooling are similar in the earning function, i.e., $\Delta(\omega,S)\approx 0$, $\Gamma_{S\omega}$ is negative if $\kappa<0$.

B.2 Proofs

Proof of Lemma 1. First order condition with respect to S is

$$-\phi(S)\,e^{-St}+\lambda\left[P(S)-T(S)+x(S)\right]e^{-RS}-y(\omega,S,S)\,e^{-RS}+ \int_{S}^{\infty}\frac{\partial y(\omega,S,t)}{\partial S}e^{-Rt}dt=0, \tag{6}$$

using (A1)

$$f_S(\omega,S)\,H(R)=f(\omega,S)+\left[T(s)-P(s)-x(S)\right]+\frac{\phi(S)\,e^{-(\rho-R)S}}{\lambda}. \tag{7}$$

From the first order conditions with respect to the consumption, together with the budget constraint

$$\frac{1}{\lambda\rho}=\int_{0}^{S}\left[P(t)-T(t)+x(t)\right]e^{-Rt}dt+f(\omega,S)\,H(R)\,e^{-RS}. \tag{8}$$

Using (8) and (7) and assuming that the tuition cost, part-time earnings and PROGRESA subsidy are constant over time, i.e., $T(t) - P(t) - x(t) = T - P - x$ for all t, in equilibrium, S is implicitly defined by

$$\frac{f_S(\omega, S^*)}{f(\omega, S^*)} = \frac{1}{H(R)} + \frac{T-P-x}{f(\omega, S^*)H(R)} + \rho\phi(S^*)e^{-\rho S^*}\left[1 - \rho\frac{(T-P-x)\left(e^{RS^*}-1\right)}{f(\omega, S^*)H(R)}\right]. \tag{9}$$

Next, we assume that tuition costs minus PROGRESA are small relative to lifetime earnings, thus, S^* is uniquely defined by

$$\Lambda(x, \omega, S^*) \equiv \Gamma(\omega, S^*) - \underbrace{\left\{\frac{1}{H(R)} + \frac{T-P-x}{f(\omega, S^*)H(R)} + \rho\phi(S^*)e^{-\rho S^*}\right\}}_{d(x,\omega,S^*)} = 0, \tag{10}$$

where $\Gamma(\omega, S) \equiv \frac{f_S(\omega,S)}{f(\omega,S)}$, which is the marginal return to schooling. Note that $\Lambda(x, \omega, S)$ is decreasing with respect to S. Moreover, note that $\Lambda(x, \omega, 0) > 0$ and as $S \to \infty$, $\Lambda(x, \omega, S) \to -\infty$. Thus, given $(x, \omega) \in X \times \Omega$, there exists a unique S^* that satisfies (10).

Note that

$$\frac{\partial \Lambda}{\partial x} = -d_x > 0; \quad \frac{\partial \Lambda}{\partial \omega} = \Gamma_\omega(\omega, S) - d_\omega > 0, \quad \frac{\partial \Lambda}{\partial S} = \Gamma_S(\omega, S) - d_S(x, \omega, S) < 0,$$

and that $d_x = \frac{-1}{f(\omega,S)H(R)} < 0$ and

$$d_S = -\frac{(T-P-x)f_S(\omega, S)}{f(\omega, S)H(R)} + \rho\frac{\partial}{\partial S}\left(\phi(S)e^{-\rho S}\right) > 0.$$

Thus, we conclude from the Implicit Function Theorem that

$$\frac{\partial S^*}{\partial x} = \frac{-d_x}{-\left[\Gamma_S(\omega, S) - d_S(x, \omega, S)\right]} > 0.$$

Similarly, note that $d_\omega = -\frac{(T-P-x)f_\omega(\omega,S)}{H(R)f(\omega,S)^2} < 0$ and that

$$\Gamma_\omega(\omega, S) = \frac{\partial}{\partial \omega}\left(\frac{f_s(\omega, S)}{f(\omega, S)}\right) = \frac{f_{s\omega}(\cdot)f(\cdot) - f_s(\cdot)f_\omega(\cdot)}{f(\cdot)^2},$$

is positive if $f(\cdot)$ is log-supermodular with respect to (S, w). Thus, we conclude that

$$\frac{\partial S^*}{\partial \omega} = \frac{\Gamma_\omega(\omega, S) - d_\omega}{-\left[\Gamma_S(\omega, S) - d_S(x, \omega, S)\right]} > 0.$$

Proof of Proposition 1. Using the implicit function theorem for a surface

$$\frac{\partial^2 S^*}{\partial \omega \partial x} = \frac{\left[\Lambda_x \Lambda_S \Lambda_{S\omega} + \Lambda_\omega \Lambda_S \Lambda_{xS} - \Lambda_x \Lambda_\omega \Lambda_{SS} - \Lambda_S^2 \Lambda_{x\omega}\right]}{\Lambda_S^3},$$

where

$$\Lambda_x = -d_x > 0, \quad \Lambda_\omega = \Gamma_\omega(\omega, S) - d_\omega > 0, \quad \Lambda_S = \Gamma_S(\omega, S) - d_S(x, S) < 0,$$

$$\Lambda_{S\omega} = \Gamma_{S\omega}(\omega, S) - d_{S\omega}, \ \Lambda_{x\omega} = -d_{x\omega},$$

$$\Lambda_{xS} = -d_{xS}, \quad \text{and}, \quad \Lambda_{SS} = \Gamma_{SS}(\omega, S) - d_{SS}(x, S).$$

Note also that

$$d_{S\omega} = -\frac{(T - P - x)}{H(R)} \Gamma_\omega(\omega, S) < 0,$$

$$d_{x\omega} = \frac{f_\omega(\omega, S)}{f(\omega, S) H(R)} > 0,$$

$$d_{xS} = \frac{f_S(\omega, S)}{f(\omega, S) H(R)} > 0,$$

and

$$d_{SS} = -\frac{(T - P - x)}{H(R)} \Gamma_S(\omega, S) + \underbrace{\rho \frac{\partial^2}{\partial^2 S}\left(\phi(S) e^{-\rho S}\right)}_{\geq 0} > 0.$$

Thus

$$\frac{\partial^2 S^*}{\partial\omega\partial x} = \left(\frac{\Lambda_x}{\Lambda_S^2}\right)\Gamma_{S\omega}(\omega,S) - \left\{\left(\frac{\Lambda_\omega}{\Lambda_S^2}\right)\left(\frac{f_S(\omega,S)}{f(\omega,S)\,H(R)}\right) + \left(-\frac{1}{\Lambda_S}\right)\left(\frac{f_\omega(\omega,S)}{f(\omega,S)\,H(R)}\right)\right\} \tag{11}$$

$$-\left\{\left(-\frac{\Lambda_x\Lambda_\omega}{\Lambda_S^3}\right)\left(-\frac{(T-P-x)}{H(R)}\Gamma_S(\omega,S) + \rho\frac{\partial^2}{\partial^2 S}\left(\phi(S)\,e^{-\rho S}\right)\right)\right\}$$

$$+\left(\frac{\Lambda_x}{\Lambda_S^2}\right)\left(\frac{(T-P-x)}{H(R)}\Gamma_\omega(\omega,S)\right) + \left(-\frac{\Lambda_x\Lambda_\omega}{\Lambda_S^3}\right)\left(\Gamma_{SS}(\omega,S)\right).$$

If savings during school are close to zero i.e., $(T-P-x)\approx 0$

$$= \left(\frac{\Lambda_x}{\Lambda_S^2}\right)\Gamma_{S\omega}(\omega,S) - \tag{12}$$

$$\left\{\left(\frac{\Lambda_\omega}{\Lambda_S^2}\right)\left(\frac{f_S(\omega,S)}{f(\omega,S)\,H(R)}\right) + \left(-\frac{1}{\Lambda_S}\right)\left(\frac{f_\omega(\omega,S)}{f(\omega,S)\,H(R)}\right)\right\}$$

$$+\left(-\frac{\Lambda_x\Lambda_\omega}{\Lambda_S^3}\right)\left(\Gamma_{SS}(\omega,S) - \rho\frac{\partial^2}{\partial^2 S}\left(\phi(S)\,e^{-\rho S}\right)\right)$$

Let $\alpha_1 \equiv \frac{\Lambda_x}{\Lambda_S^2} > 0$, $\alpha_2 \equiv \frac{\Lambda_\omega}{\Lambda_S^2} > 0$, $\alpha_3 \equiv -\frac{\Lambda_x\Lambda_\omega}{\Lambda_S^3} > 0$, and $\alpha_4 \equiv -\frac{1}{\Lambda_S} > 0$. Then we can write (12) as

$$\frac{\partial^2 S^*}{\partial\omega\partial x} = \alpha_1\Gamma_{S\omega}(\omega,S^*) - \Theta(x,\omega,S^*) + \alpha_3\left(\Gamma_{SS} - \rho\frac{\partial^2}{\partial^2 S}\left(\phi(S^*)\,e^{-\rho S^*}\right)\right), \tag{13}$$

where $\Gamma_{S\omega}(\omega,S) \equiv \frac{\partial^2}{\partial\omega\partial S}\Gamma(S,x,\omega)$ and $\Theta(x,\omega,S^*) \equiv \alpha_4\left(\frac{f_\omega(\omega,S)}{f(\omega,S)H(R)}\right) + \alpha_2\left(\frac{f_S(\omega,S)}{f(\omega,S)H(R)}\right)$.

B.3 Equilibrium Income

We explore the effects of PROGRESA, endowments, and their interaction, on equilibrium income, y^*.

Lemma 2. Suppose (A1) and (A2) hold in equilibrium.

(i) The equilibrium income is increasing with respect to PROGRESA x, i.e., $\frac{\partial y^*}{\partial x} > 0$.

(ii) If $f(\cdot)$ is log-supermodular with respect to (S,w), the equilibrium income is increasing with respect to the initial endowment, ω, i.e., $\frac{\partial y^*}{\partial \omega} > 0$.

Proof of Lemma 2. From the Implicit Function Theorem there are neighborhoods $U \subset \mathcal{S}$ of S^* and $W \subset X \times \Omega$ of (x, ω) on which equation (2) uniquely defines S^* as a function of (x, ω). That is, there is a function $\xi : W \to U$ such that $\Lambda(x, \omega, \xi(x, \omega)) = 0$ for all $(x, \omega) \in W$. Note that

$$y^* \equiv y(\omega, S^*, S^*) = f(\omega, \xi(x, \omega))$$

Then

$$\frac{\partial y^*}{\partial \omega} = f_S(\xi(x, \omega), \omega) \frac{\partial \xi(x, \omega)}{\partial \omega} + f_\omega(\xi(x, \omega), \omega) > 0$$

and

$$\frac{\partial y^*}{\partial x} = f_S(\xi(x, \omega), \omega) \frac{\partial \xi(x, \omega)}{\partial x}$$

thus the proof follows from Lemma 1.

Next, we study under what conditions there is remediation or reinforcement in the optimal of level of income. From Implicit Function Theorem there are neighborhoods $U \subset \mathcal{S}$ and $W \subset X \times \Omega$ of S^* and (x, ω), respectively, on which there is a function $\xi : W \to U$ such that $(x, \omega, \xi(x, \omega))$ satisfy (2) for all $(x, \omega) \in W$. We show in the next proposition that remediation on the optimal level of income depends on,

$$\left[\underbrace{f_{SS}(\xi(x, \omega), \omega)}_{<0} \underbrace{\frac{\partial \xi(x, \omega)}{\partial \omega}}_{>0} + f_{S\omega}(\xi(x, \omega), \omega)\right] \underbrace{\frac{\partial \xi(x, \omega)}{\partial x}}_{>0} + \underbrace{f_S(\xi(x, \omega), x, \omega)}_{>0} \underbrace{\frac{\partial \xi(x, \omega)}{\partial \omega \partial x}}_{<0} \qquad (14)$$

Proposition 2. Suppose (A1)-(A2) hold in equilibrium. If (14) is negative (positive) there is remediation (reinforcement) between PROGRESA and the initial level of endowment, in producing the optimal level of income, y^*.

To understand intuitively when there is remediation in the optimal of level of income we analyze each of the terms of equation 14. For the last term, we know from Proposition 1 that if there is remediation in the optimal level of schooling, $\frac{\partial \xi(x,\omega)}{\partial \omega \partial x} < 0$. The first term of the bracket is negative, due to the concavity of the earnings function, while the last term may be positive if the initial endowment and the level of schooling are direct complements in the earnings function. Thus, remediation on the optimal level of income depends on the size of the remediation effect on the optimal level of schooling, and on the total derivative of the marginal return of education on earnings, with respect the initial endowment weighted by the marginal effect of PROGRESA on the earnings function.