A Social Umbrella: PROGRESA & Precipitation
Can a cash transfer program perform the same functions as insurance for the rural poor?

by

Daniel Walker Kenefick

Jon M. Bakija, Advisor

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Abstract

This paper investigates the effects of precipitation shocks on poor rural children, and the potential mitigating effects of PROGRESA / Oportunidades, a cash transfer program in Mexico. I find that negative shocks, or droughts, lower school attendance for both boys and girls. How shocks change labor participation varies with the kind of precipitation shock and the gender of the child. Boys are less likely to work after a negative shock, and more likely to work after a positive shock, whereas girls are less likely to work after a positive shock, and more likely to work during a negative shock. PROGRESA / Oportunidades treatment has a clear treatment effect on boys and girls for keeping them out of work and in school. Furthermore, PROGRESA / Oportunidades almost entirely mitigates the effects of shocks for boys. There is less evidence that PROGRESA / Oportunidades reduces the effects of shocks on girls. I also find substantial biological consequences of precipitation shocks in the year after birth, and that treatment by PROGRESA/ Oportunidades in early childhood almost entirely mitigates these effects.

Introduction

This paper evaluates the capacity of a conditional cash transfer program to mitigate the effects of shocks on poor rural children. Specifically, I compare the effects of precipitation shocks on the recipients of PROGRESA / Oportunidades, a conditional cash transfer program in Mexico[1], and a control group that did not receive the program benefits. The ability to respond to shocks is linked with insurance and credit markets. Poor areas might not formally have these markets, and have to make do without them. A conditional cash transfer program, like PROGRESA, might act as a form of insurance or credit for these poor families, and mitigate the effects of negative shocks.

The analysis of this paper is divided into two parts. The first examines responses to year–to–year variation in rainfall, focusing on how households will treat their children differently in response to the shock. The second explores the consequences of early life shocks, and investigates PROGRESA's capacity to mitigate their effects. These two categories of rainfall shocks can have tremendous long-term consequences for both the shocked individuals and the communities they comprise. Conceptually, we would expect cash transfer programs that target children to be an effective solution to these problems. I begin with a brief overview of the PROGRESA program, then the literature on insurance and credit markets, and then the literature on

[1] Referred to PROGRESA for short. Halfway through the experimental design (in 2000) the program changed its name to Oportunidades,

PROGRESA itself. Following that is an introduction to the precipitation and survey data, and an outline of the econometric specification. I then describe the econometric specification, and present the results. I then conclude the paper.

The PROGRESA Program

PROGRESA is an instance of a Conditional Cash Transfer program, a redistribution mechanism that is becoming increasingly popular in developing countries. The underlying mechanism is simple: the governments redistribute cash to the poor. As straightforward as they are, the programs vary dramatically from instance to instance. First, these programs can differ in their target population. Some programs redistribute to the poorest 20% of the entire population, and some to only a tiny fraction of a percent in certain areas. Families with children might receive the benefits in one program, and the elderly in another. Programs can also differ in whom they redistribute from, although this is more a question of tax structure. Another key difference is how much money is transferred, which can range to mere pennies in countries in Africa to hundreds of dollars in Ecuador. Many cash transfer programs are "conditional", requiring the recipients of the transfer to conform to certain criteria in order to receive benefits. Program goals also vary, but often the focus is on families with children, so as to address the intergenerational perpetuation of poverty. Hanlon, Barrientos, Hulme's *Just Give your Money to the Poor* (2010) gives an excellent overview of these programs, and shows how they are similar, how they are different, and what makes them work.

Cash transfer programs are interesting as a category, but my interest is in the Mexican program specifically. PROGRESA targets very poor families in very poor communities in rural Mexico. Transfers are conditional on several criteria, including community work done by females in each household, attendance at nutrition talks, and a child's attendance at school. The payment schedule is tailored to grade and gender, with primary school children receiving, in 1998, from $70 per year in 3rd grade to $135 per year in 6th grade, and secondary school children receiving from $200 per year for boys in first grade and $210 per year for girls, to $220 per year for boys in third grade and $255 per year for girls. These numbers are taken from the PROGRESA database documenting monetary transfers to each household in the program. [2] These cash transfers account for 20% of household income on average. Other transfers included multivitamins, easier access to health care, and a number of other elements that focused on the health of the recipients (National Institute for Public Health of Mexico, 2005). As a result, we would expect the effects of PROGRESA to be much more than the income effect of the cash transfers.

Certain characteristics of the PROGRESA program make it ideal for econometric analysis. From the onset, PROGRESA was designed as a randomized experiment for the purposes of evaluation. The program

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[2] The numbers were originally in pesos, but have been inflation adjusted to 2010 dollars, using Mexico's inflation adjustment index and the 1998 average exchange rate.

consisted of randomly selected treatment and control groups, as well as periodic intensive surveys to capture changing household characteristics. The program was organized by locality. Mexico is made up of 31 states, three of which are included in the initial survey. Each state is made up of municipalities, which are in turn made up of localities. Localities are individual towns, which vary in size from 20 to 10,000 people in the PROGRESA sample. In 1997, about 6,000 rural localities were identified as potential recipients of PROGRESA benefits, and 506 of these were selected to participate in a pilot program. These 506 communities[3] were randomly assigned to treatment and control groups, 320 in the treatment, and 186 in the control. A baseline survey, capturing the socioeconomic characteristics of the households in the communities, was conducted in 1997. Beginning to 1998, the treatment communities began to receive PROGRESA benefits. Follow up surveys were taken bi-annually in both groups in 1998, 1999 and 2000. Beginning in the year 2000, both the treatment and control groups began receiving benefits, invalidating the experimental design of the program. In 2003 a matched comparison group was introduced for the purposes of evaluation. This matched comparison group consisted of 306 of the communities from the original 6000 that were selected to closely resemble the original treatment and control groups. This group was surveyed in 2003 and retroactively back to 1997 for important characteristics such as school attendance and labor participation. We will see

[3] The words community and locality are used interchangeably. Mexico is made up of states, which contain municipalities, which contain communities (or localities).

later, however, that this matched control group is slightly different from the original treatment and control groups, and requires special econometric treatment. The 2003 survey was much more extensive. It included biological information, socioeconomic evaluation of the localities, and a much more thorough baseline survey. This experimental setup is summarized in figure 1.

Year	Treatment	Control	Matched comparison group
1997 (Baseline)	x	x	*
1998	x	x	*
	x	x	
1999	x	x	*
	x	x	
2000	x	x	*
	x	x	
2001	*	*	*
2002	*	*	*
2003 (most recent)	x	x	x

Figure 1 - Treatment and survey schedule. Shaded grey indicates that the group received PROGRESA benefits during that time. An x indicates that survey data was collected during that time, * indicates that retrospective data was collected. Note that surveys were taken twice a year during the beginning of the program.

Insurance for the Poor

The poor in developing countries have a hard time dealing with risk. Exogenous shocks can be devastating to poor households trying to get by on what little they have, since even a small negative shock can have dramatic consequences if a family is living near subsistence. These shocks can come in any form, but some common examples are sickness, natural disaster, and death of a family member. Many of these shocks can have consequences that far outlast the shocks themselves. For example, a natural disaster could disrupt a family's ability to feed a young child or send him or her to school. We'd expect this child to be less productive as an adult because of disrupted health and education. This exemplifies the connection between risk and intergenerational poverty. If a household cannot take advantage of insurance-like mechanisms, then the household cannot efficiently manage risk, and the next generation might suffer. To help the next generation, there must be efficient mechanisms for families to confront risk, respond to negative shocks differently, or avoid shocks altogether.

In developed countries, insurance markets allow individuals to redistribute risk across a population. The mechanism is a simple one: a group of people pays into a pool, and if a shock strikes one member, the pool helps pay for some or all of the related expenses. The end result is that risk is shared (or hedged) across many people and everyone is better off. For example, if it is known that P people out of a population of N will suffer some shock that costs $x to treat, such a system should charge a premium of (P/N)x, the average expense, to each member of the population. If N is reasonably large, the premiums should offset the payouts to the affected individuals. Random events may cause the total losses due to shocks and the total collected from premiums to differ slightly. But if the population is large enough, this discrepancy will be tiny and will even out over time. If we assume diminishing marginal utility of consumption then expected utility is greater paying the small insurance premium over time than with uninsured exposure to large negative shocks. This creates the demand for insurance, and substantially increases welfare across a population.

Risk coping strategies do not always involve insurance markets, either formal or informal. Many of us have substantial savings in case of shocks. Even if we don't have enough saved to cover the shock, there is always the possibility of borrowing money. This strategy is not ideal: for an individual, saving or borrowing only pools risk over time. If negative shocks are correlated though time, or a shock reduces lifetime income (perhaps through injury) then smoothing across your lifetime will still leave you worse off because of the shock. Insurance markets pool risk across people and time, so it is possible to totally protect against this sort of risk.

Puzzlingly, insurance markets rarely exist in poor developing areas. Uninsured risk causes dramatic welfare loss for the poor in developing countries, so there is clearly cause for these markets to exist. Natural disasters, sickness, economic and agricultural shocks are all examples of problems that are insurable in developed areas, but plague the poor in developing countries. The question is, why do formal insurance markets tend to be absent in poorer areas? We would expect the model of insurance outlined earlier to work regardless of the wealth of the population or the type of shock in question. The reason is not immediately clear, but there are several economic explanations for the absence. First, these poor families might not demand the optimal amount of insurance. Even if these families are vulnerable to shocks, they might not fully recognize the risks involved, or they might not fully understand the concept of insurance. For example, Cole *et al*. (2010) demonstrate that a lack of financial literacy is a major barrier to efficient consumption of insurance. On top of not understanding insurance, it is possible that the purchasers do not fully trust the providers: a poor family might have a hard time giving their money to an insurer in exchange for a promise for protection against a future shock. This is especially true if there is a precedent of insurance companies not appropriately compensating their customers, or if governments do not consistently enforce insurance contracts. Trust between parties and contract enforcement are public goods. If an insurance provider violates a contract or his purchaser's trust, even the well-meaning insurers will suffer from decreased demand. These arguments are fleshed out in Pauly *et al*., (2006).

There are other explanations. These families might be systematically less risk averse, and be demanding an optimal (but small) amount of insurance. This smaller demand might be served by less formal markets for insurance, which we outline later. High administrative costs, asymmetric information, or other hurdles might decrease the demand from those families that are less risk averse, especially if the premium affects average risk. Higher costs for insurance providers are observed in poor areas, for reasons I outline now.

Most of these costs of insurance are a consequence of two major barriers: information asymmetry and moral hazard. If the two parties have asymmetrical information about the risks being insured, then it is difficult to determine the efficient price of insurance. For example, a person might know himself to be sickly, but might misrepresent his health to an insurer in order to get a lower price and a net payout. Insurance providers can incur great costs trying to reduce this information asymmetry, and these costs can prevent the provider from entering a market. Also, if customers vary in their individual level of risk, and it is too costly to determine each individual customer's risk, the provider of insurance might set the price to reflect the average risk of its customer pool. This will cause the less risky customers to not buy insurance (provided they know their own risk and are rational), which will further drive the price up, and so on.

Moral hazard comes into play when a person is already insured. If there is a smaller cost of failure in an insurable endeavor, a person might not try as hard to succeed, placing both the insurer and the buyer at risk. Unfortunately, neither of these is a satisfactory explanation to why such a market does not exist for some kinds of shocks, like rainfall. There is little possibility for moral hazard with precipitation (a farmer cannot cause it to rain less after he has insured himself against drought) and information is relatively symmetric to both the farmers and the insurers with regard to precipitation. This suggests that the explanations for lack of insurance (e.g., lack of understanding, lack of trust, high administrative costs) are more likely to explain absence of drought insurance. There are many possible explanations, but the fact remains that several poor areas of the world lack a formal insurance market.

Without a steady insurance market, poor individuals have two strategies for dealing with risk: risk management, which is done in anticipation of a shock, and risk coping, which is done after the fact (Dercon, 2003). Risk management is the hedging of activities in response to anticipated risk. This is analogous to the payment of a monthly insurance premium. Hedging activities typically involve some cost, so they represent an individual's efforts to smooth their consumption across all of the possible states of the world. For example, poor rural households might hedge risk by planting drought resistant crops with their normal crops. This provides some protection against a drought shock, at the cost of forgoing some land for their normal crops, which are presumably more profitable. However, risk management strategies are intrinsically limited. First, an individual can only hedge his activities against what he can anticipate. Very rare occurrences, such as one-in-a-lifetime natural disasters, might be difficult for an individual to foresee. A formal insurance firm might have a broader perspective, and economies of scale when researching potential risks (Besley, 1994). Second, an individual cannot hedge his activities against any and every potential shock. For example, it might be infeasible to plant a collection of crops that is resistant to flood, drought, frosts, heat, and changes in consumer tastes all at the same time. Since they deal in only cash and contracts, an insurance market can cover against any number of different shocks, without the need to specially prepare for the type of shock. Finally, certain events might be foreseeable, but there might not be an activity that effectively hedges against it. The death of a primary breadwinner might be unavoidable, but very difficult to hedge against if there are no other capable wage earners in a household. A cash payout from an insurance firm could easily solve this problem. Preemptive risk-management is a limited strategy for mitigating risk, but without a formal insurance market it is often the best a household can do.

The other strategy for dealing with risk is risk coping, which happens after the shock. For example, the community often acts like an informal insurance market for its constituent households. If one individual becomes sick, then all members of the community pitch in to help that one individual. In this sense, risk coping is analogous to an insurance payout: after the shock, the victim receives some compensation to alleviate the shock. While these informal markets can be effective, they lack certain characteristics that make more formal markets more effective. (Dercon and De Weerdt 2006) First, it is possible for an individual to leech off of such a system. Without an enforceable contract, an individual might never help a community member in need, or the community might refuse to help a particular household for social reasons. Presumably a formal insurance firm would be legally bound to help, and the payer similarly bound to pay. Also, shocks can be highly correlated in small communities, making it difficult for community members to help each other. Disease and natural disaster might affect every person in the community, in which case there will be no one to provide help, and everyone will be in need of it. A larger insurance firm can sell insurance to people who are susceptible to different kinds of shocks, thereby reducing the probability of a totally correlated shock (Getler and Grueber, 2002). Informal insurance mechanisms can be effective for small communities, but might not be as effective as a more formal market.

Credit market failure is another major issue in development economics. The poor (especially the rural, agricultural poor) often have very little steady income, which is essential to paying back a loan. By definition, the poor have very little collateral. Also, the poor might be less careful with money that has been lent to them than if the money was theirs to begin with. (Hanlon, Barrientos, Hulme, 2010). This mirrors the moral hazard problem with insurance mentioned earlier. A credit market might also suffer from adverse selection. Credit markets suffer from a problem of limited liability if borrowers have insufficient collateral to repay an outstanding debt. This provides a kind of insurance for the borrower, and makes him less likely to repay. This is especially troublesome for people with little wealth. As a result, the lender must charge a risk premium in the interest rate as compensation for providing the insurance. If the risk premium in the interest rate is set based on the average level of risk, the people with the lowest risk (those most likely to repay, and therefore least likely to need the insurance) will drop out of the market. This forces the interest rate up, and so on (Stiglitz and Weiss, 1981).

All of these factors make loan granting institutions a rarity in poor places. Without a population to lend to, dependable banks are almost non-existent in poor areas as well. This in turn affects a family's ability to save. Instead of saving, a family might put their extra money towards an animal, which they can sell or eat when times get tough. But again, correlated disasters might mean everyone sells their animals at once, which reduces their value. Similarly, a natural disaster might reduce the value of eating an animal: if food is scarce for people, it is likely that food is scarce for animals. Problems with the credit market in poor areas prevent the poor from saving or borrowing efficiently, and reduce the ability for a population to deal with shocks.

If poor families cannot preemptively deal with shocks, and cannot borrow or depend on their community for recovery, they must find other ways of coping. More often than not, the poor get though tough times by "borrowing" from the next generation. If a shock hits, a parent might be unable to afford school fees, and be forced to take a child out of school in order to eat (Chetty and Looney, 2007). Or, to save money, parents might purchase less nutritious food for their young children, stunting their development (Hoddinott and Skoufias, 2004). Both of these actions improve the poor family's current welfare, but at the cost of reducing the productivity of the next generation. This behavior is especially worrisome because it leads to a perpetuation of poverty between generations. Insurance and credit market failures, then, could be a major contributor to the poverty trap.

It is possible that a government program, such as social insurance or a cash transfer program like PROGRESA, could be an economically efficient solution to the problems listed above. A government has access to a much wider pool of risk bearers than a smaller insurance provider, and taxes could be an effective way to redistribute and hedge risk across a nation's citizens.

Government intervention could be efficient even if administrative costs are prohibitively high for a standard insurance market. Government action could benefit from economies of scale in administration. If administrative costs are too high, simple redistribution might be more efficient than social insurance or government provided credit. For example, if cash is given to the poor with no strings attached, the moral hazard problem is avoided because they will have the proper incentives regarding what to do with it. Neither is there a problem of information asymmetry. Without any of these interventions, a family might "borrow" from the next generation to cope with risk. Investments in children for which the present value of benefits exceed the costs may be sacrificed to cope with current risks. This creates an opportunity for an improvement in efficiency through social insurance or cash transfers.

This thesis will study these insurance market failures for poor rural areas in Mexico. I will examine how poor rural households respond to random shocks, and how they smooth consumption when these shocks strike. Specifically I will be interested in how a family's response to a shock affects the next generation. I will also investigate the efficacy of one public intervention in the insurance market, a cash transfer program called PROGRESA. By guaranteeing a small amount of cash every month to a poor household, PROGRESA provides them with some guaranteed protection against shocks to their wellbeing. This allows them to send their children to school, keep them in school, and feed them better when a shock occurs. In other words, PROGRESA should mitigate any change in behavior associated with a random shock.

Early life shocks

Children are incredibly sensitive to their "initial conditions". Social, genetic, and economic endowment at birth are good predictors of outcomes later in life. For a few examples n the literature, see Birnie *et al.* (2010) or Maccini and Yang (2008). As a corollary, bad luck early in life can have a lasting impact on both the individual and the society he or she belongs to. Malnutrition or sickness in infancy, for example, have been known to affect health, test scores, and a number of characteristics throughout the individual's life (Alderman, Hoddinott, Kinsey, 2006). These maladies can affect an individual's physical and mental productivity, which affects themselves and the society investing in them. Shocks early in life, like the ones investigated in this paper, are therefore of central importance to economics. It is not only important to identify which shocks can have a lasting impact on an individual, but also to identify effective countermeasures (Grimm, 2011). If negative early life shocks can have a disproportionate effect on later life outcomes, then mitigating these shocks will be a good investment.

In wealthier areas, we might expect that early life shocks can be mitigated by insurance and credit markets. If insurance and credit markets were perfect, parents could insure the early years of their child's life or borrow to cover losses. PROGRESA could mitigate these just as it mitigates year-to-year shocks, by guaranteeing a small amount of cash that can cushion the shock. PROGRESA could combat these shocks through other channels as well, such as its distribution of multivitamins or emphasis on nutrition in early life. Regardless, we would expect PROGRESA to reduce the effects of early life shocks.

PROGRESA literature

The data associated with the PROGRESA program is very rich. As such, it has been studied extensively in the literature. Early papers, such as the paper by Coady and Parker (2004), focus on program evaluation. They find that PROGRESA , even in its early years, was effective at getting children to attend school. They show that secondary school enrollment went up by 8 percentage points for boys and 11 percentage points for girls in PROGRESA communities relative to non-PROGRESA communities in the first three years of the program. They also show that PROGRESA was a cost-effective way of getting children to attend school relative to building more schools. Later papers, such as Behrman *et al*. (2008) evaluate the long-term outcomes in communities receiving PROGRESA benefits. They find considerable improvements in education based outcomes, such as attendance, child / teacher ratio, and percentage of students failing.

A paper by de Brauw and Hoddinott (2008) explored whether cash transfer programs need to be conditioned by exploiting a natural experiment in the PROGRESA program. Recall that children in PROGRESA were required to attend school in order to receive a part of their transfer. Each child had to bring a form to school in order for it to be signed by their teacher, and this form was checked at the end of each month. In a few communities, however, this form was not delivered or there were not enough copies. The authors used this information to demonstrate that the conditioning of a cash transfer program was not strictly required for a cash transfer program to get children to attend school.

The above papers investigate the average effects of PROGRESA through both good times and bad. The focus of this paper is different: I investigate whether the program is successful at changing behavior when times are tough. It might be true that shocks cause temporary changes in behavior that have long term consequences, and cash transfers are unsuccessful at mitigating these sub-optimal responses to shocks. For example, PROGRESA might increase school attendance on average over the course of seven years, but this attendance might be flighty, where many of these marginal children drift in and out of school with good and bad times. Non- consecutive years of school are not as effective as regular, consecutive years of school, so the eleven percent figure might overestimate the effectiveness of PROGRESA. If cash transfer programs have an especially large causal effect on school attendance during shocks, we might enhance their effectiveness by treating them more like the insurance substitute they are, by transferring less money when times are good and more when times are bad. Identifying how the recipients of PROGRESA benefits behave during shocks is key to understanding the best way to provide welfare.

Two papers have heavily influenced my approach. The first is "Can Conditional Cash Transfer Programs Serve as Safety Nets?" by de Janvry, Finan, Sadoulet, and Vakis (2006), which examines the effects of self-reported shocks on children's school and work habits in PROGRESA and control communities. The second is "Under the Weather" by Maccini and Yang (2008), a paper that investigates the effects of early life rainfall shocks on Indonesian adults. I briefly outline the results of these papers, and explain their relevance.

The de Janvry *et al*. paper examines the effects of random shocks on a child's decision to attend school and work. The authors develop a theoretical model of the decision to work and/or attend school, incorporating a high cost of school reentry, and the idea that work and school are not mutually exclusive. They empirically examine the effects of many kinds of shocks, including sickness of the head of the household, unemployment of the head of household, sickness of a small child, and self- reported natural disaster shocks, on school attendance and labor force participation. They find that PROGRESA almost entirely mitigated the effects of these shocks on school enrollment, but did not mitigate the effects of shocks on labor participation. They find that some shocks, including droughts, do not reduce school attendance, so there was no effect to mitigate. Their econometric specification was split into two parts: labor participation regressions and school attendance regressions. For the schooling regressions, the predictors are lagged school enrollment, a vector of self reported shocks, treatment by PROGRESA, round fixed effects, individual fixed effects, and treatment interacted with the self-reported shocks. This last set of interacted variables is the set of predictors the authors are interested in: a significant coefficient on any of these indicates that PROGRESA has some additional effect after a particular shock. This specification is then first differenced, and then estimated using Arellano-Bond techniques. Their labor participation regressions are similar, but they do not include lagged schooling as a predictor, eliminating the need for Arellano – Bond estimation.

My specification builds on the de Janvry specification. First, I control for age and level of schooling attained through a vector of dummy variables. Age is a proxy control for unobservable productivity in the labor market (a 6 year old will be a less productive famer than a 16 year old) and including the level of schooling attained controls for critical transition periods, for example between primary and secondary school. Past literature has shown that both of these components are important in school and labor decisions, see for example Coady and Parker (2004) or Behrman, Parker, and Todd (2009). I also include lagged schooling in my labor participation regression since I find that it has a large effect on the results. The consequences of this inclusion are discussed in the Data and Methodology section.

The shocks investigated in the de Janvry paper warrant some discussion. The authors used data from the surveys themselves to determine whether a village was exposed to an environmental shock. The PROGRESA survey included an item for natural shocks, like earthquakes, droughts, floods, etc. during three of the seven rounds. However, the reporting of shocks within each community was not universal. In fact, the average number of people reporting a particular shock in each locality was only around 30%, even in relatively small communities. An individual might be more or less aware of a particular environmental characteristic depending on their vocation: for example, a farmer might be more likely to notice a drought than a shopkeeper. However, the shopkeeper's livelihood might be just as affected by precipitation as the farmer's if these communities are closed economies. The authors find that floods or droughts had no effect on school attendance or child labor in either treatment or control areas. Their explanation was that drought was so common that households had developed "ex-ante risk coping strategies" to cope with rain shocks. However, the scope of their investigation is limited to three survey rounds over two years, and uses only self-reported data. The fact that only 30% reported a given shock is evidence that the self-reported shock involves a great deal of measurement error. If the measurement error is random, then this should bias the estimated effects of the shock towards zero. The climatology data gives much more precise precipitation for all rounds of the PROGRESA data, including the survey round before treatment began. This removes any measurement error bias from using self-reported shocks, and allows me to use many more rounds of PROGRESA data. Additionally, the use of real precipitation data allows me to have a non-linear effect of rainfall on my variables of interest, which is important.

Maccini and Yang's "Under the Weather" has also been influential to my analysis. The authors investigate the effects of early life rainfall on long-term outcomes for individuals born in Indonesia. They match real weather station data with the year and place of birth for each individual in a data set of 2000 adults. They find that women with 20% higher rainfall during their year and location of birth are 3.8 percentage points less likely to self report poor health status as adults, are .57 cm taller, attain .22 more years of schooling, and score higher on an asset index. They do not find that early life rainfall affects men's outcomes.

As the measure of rainfall shocks, the authors use the difference between the log of the average rainfall in the locality and the log of the rainfall in that locality during the year of birth. (log(birth precipitation) - log(avg. precipitation)). Their final specification includes a biological outcome as the dependent variable. Explanatory variables include their measure of the precipitation shock, a separate fixed effect for each district-season combination (i.e., two fixed effects for each district), and a separate time trend for each district-season combination. They also use a time dummy for each year-season combination. The authors break the analysis up based into seasons because agricultural activity is centered on them, and they worry that different kinds of parents have children during the wet or dry season. Mexico, however, does not have a clear wet and a dry season, and agriculture takes place year round in treatment and control communities. I also find that the month of birth is independent of the prior six months' rainfall, see table A2. This eases the worry that different kinds of parents are having children at different points in the year as a reaction to rainfall.

Maccini and Yang's paper demonstrates that early life conditions can have a lasting impact on the health and productivity of individuals later in life. My analysis replicates and builds on this conclusion. First, I show that these early life shocks have lasting effects for children's outcomes in Mexico. Secondly, I test PROGRESA's capacity to mitigate these early life shocks for the young children born at the beginning of the PROGRESA program.

PROGRESA and Rainfall: A Theoretical Perspective

Consider a negative rainfall shock, or a drought. Assume that a household pools its resources, and collectively makes decisions about school attendance and child labor. Also assume that a child's education is a normal good, or that when income goes up, demand goes up. We can think of drought of having two effects. First, it acts as an income shock to the entire household. The PROGRESA communities are relatively isolated agricultural communities, so we would expect that the productivity of many vocations depend on the productivity of agriculture within the community. Lower rainfall in agricultural based communities should lower income for all households, and therefore the demand for child's schooling. This is an income effect.

Reduced rainfall is also a shock to the opportunity set for children. We expect a household to be making a choice between sending children to school, sending children to work, or allowing children leisure time.[4] Drought

[4] Rainfall might also change the opportunity cost of leisure, perhaps by reducing the utility of jumping in puddles.

might lead to a smaller harvest, which leads to less demand for child labor, and therefore a lower wage. If drought reduces the productivity of child labor, it reduces the opportunity cost of school and leisure. However, this effect is reduced if the child's best employment opportunity is insulated from rainfall. A drought could therefore shift children from work to school or leisure, depending on the relative utility of the two. This is a substitution effect.

The income and substitution effects of rainfall move in opposite directions for children who are employed in agriculture. The income effect of a drought reduces the likelihood of attending school, while the substitution effect of a drought increases it. However, there would be less substitution effect for children who are employed in sectors that are insulated from rainfall, to the extent that labor is not easily substitutable across agricultural and non-agricultural jobs. Boys and girls, for example, might be involved in different lines of work, and therefore react differently to drought. From the PROGRESA data, we know that 24% of boys report working, and of these boys 42% work in agriculture or horticulture. Only 9% of girls report working, and15% of them work in agriculture or related activities. So, for every 100 boys in PROGRESA communities, 9 are working in agriculture, compared to 1.5 out of 100 girls. From this we would expect girls to respond differently than boys to rainfall shocks.

Now consider a positive rainfall shock, or what happens when there is abundant rainfall. Agricultural productivity increases, which in turn boosts incomes. As a result more children attend school. However, abundant rainfall increases the productivity of child labor in agricultural sectors, increasing the gains from work.. Children in other sectors might be insulated from this effect. Alternatively, too much rainfall might lead to flooding, which could have the same consequences as a drought, perhaps by destroying crops and property. This possibility is discussed in the Data and Methodology section.

Now consider the effects of PROGRESA. The program provides a substantial income effect: cash transfers accounted for approximately 20% of household income (Skofias, 2003). I expect that this income effect would transfer children from work to leisure and/or school. The program also provides a substitution effect, since additional benefits were provided to families whose children consistently attend school. This effectively reduces the opportunity cost of school.[5] PROGRESA increases school attendance by both increasing incomes and incentives to go to school.

PROGRESA should interact with rainfall, making school attendance more likely during a drought. PROGRESA might relieve credit constraints, enabling a household to keep their child in school when a negative income shock would have otherwise forced them to take the child out. We'd also expect the effect of PROGRESA on school attendance to be largest for the children who are at the margin between attending and not attending. For example, kids in families that are suffering from drought, or kids in situations

[5] Although the large injection of capital into a poor community by PROGRESA might increase the productivity of labor, if the recipients are able to make efficient investments with their new money.

where agricultural labor is unusually productive are more likely to be at the margin between attending and not attending school, but PROGRESA might tip the balance towards attendance.

I expect the effects of early-life rainfall on biological outcomes to be limited to income effects, since there is no substitution effect of labor for very young children.

Data and Methodology

PROGRESA Data

The PROGRESA data set is available freely online to anyone who registers with the program. The data are panel data, following specific households and people throughout a seven-year time frame. Each survey round consisted of a long survey that collected information on each individual in each household in the program. Survey items included school habits, labor and vocation, household inventories, overviews of health, and a number of other categories. For the most part the surveys were identical each year, but many items are not constant across all waves. Often questions are phrased differently, or entirely absent during certain waves. In the final round of data (in 2003) additional surveys recorded health information, data about schools in each locality, and information about the locality itself. The dataset required translation into English, since it was initially in Spanish. In this paper, I use the normal survey data for each round, and the additional health information collected in the final round. Tables 1 and 2 give some basic statistics of some important variables for the analysis. Table 1 separates the statistics by gender, and table 2 separates the statistics by treatment for the original treatment and control groups. There are no statistically significant differences between these two. Differences between the matched control groups and the other groups are shown later, when I discuss the propensity score re-weighting used in my analysis. Table 3 gives means for some summary statistics for biological outcomes for younger children, aged either 0 to 6 or 0 to 2 depending on the measure.

"Attending school" is a binary indicator for school attendance, "years of schooling completed" and "age" are self explanatory, "Sickness of the head of household" is a binary indicator for the head of household being sick enough not to participate in daily activities in the past 4 weeks, "family member sick" is a binary indicator for another family member (not including the head of household) being sick in the past 4 weeks. "Working" is a binary indicator for participation in the labor force, which in this instance means working for a wage or helping in a family owned business or farm. In table 3, height and weight are self explanatory, and "number of words recognized" is a test given during the final round of PROGRESA to test a child's mental development.

In addition to the data provided by the PROGRESA program, I also make use of data on precipitation. Precipitation was a natural choice when searching for a source of totally exogenous shocks. It is randomly "assigned" to communities, and not linked to PROGRESA treatment. The rainfall data is from the Center for Climatic Research at the University of Delaware (Willmont and Matsuura, 2009). It originates from real weather station data recorded back until the early 1900's and was then interpolated to a 0.5 degree by 0.5 degree of latitude/longitude grid, where the grid nodes are centered on 0.25 degree. Standard meteorological interpolation was performed on the monthly station differences to obtain this gridded difference field, which was used to construct the final precipitation grid. This interpolation was accomplished with the spherical version of Shepard's algorithm. The specifics of this algorithm are not important for the purposes of this paper, but it does provide a measure of confidence for each point. This confidence level is influenced by the number of stations, year to year variability, and a number of other factors, but the confidence is good in the areas of Mexico that I examine. Some econometric techniques, described later, address the impreciseness of this data.

Precipitation Data

In order to evaluate the effects of rainfall shocks, I matched monthly precipitation data to each Mexican locality from 1973 through 2003. This required the geographic location of each locality. If the municipality that contained the locality in question was sufficiently small, the centroid of that municipality was used as the geographic location for the locality. If the municipality that contained the locality was large, then the centroid of the locality itself was used as the geographic location. In this context, "large" means that the centroid of the municipality containing the locality would select different precipitation grid points than the centroid of the locality itself. The municipalities used in PROGRESA were overwhelmingly "small." -- 96% of localities were in "small" municipalities. Figure 2 gives a visual representation of the precipitation grid. Green diamonds are the points on the precipitation grid, and the red dots are a subsample of the PROGRESA localities. This subsample was chosen randomly to show the distribution of localities without cluttering the map.

The mean rainfall in each locality is calculated over a period of 30 years to smooth any variation that might have occurred during the relatively short seven-year evaluation period of PROGRESA. A histogram of the averages for each locality is given for treated communities in figure 4 and non-treated communities in figure 5. There are no statistically significant differences between these two groups.

A central challenge is that the precipitation from the external data set is interpolated with error. The five precipitation grid points closest to the geographic location of each locality were used in the final analysis. Each of these points was broken into 4 categories: less than 30% below average rainfall, between 30% and 10% below average rainfall , between 10% and 30% above average rainfall, and greater than 30% above average rainfall. Baseline rainfall for the community is between 10 percent below and 10 percent above average rainfall, and is the omitted category for each point. Each of these categories is interacted with a binary variable indicating treatment by PROGRESA, and a binary variable indicating that the community is not treated by PROGRESA. This produces eight categories for each point in each year for each locality. In the final specification each of the eight categories for the closest point to each locality is instrumented by the corresponding categories in each of the next four closest points. Thus, each year-locality combination has 32 instruments for precipitation.

A histogram for these categories of the precipitation point closest to each locality is given in Figure 3, and a table of the frequencies of these shocks, separated by PROGRESA treatment, is given in table 4.

For the rest of the text, I describe rainfall shocks above the average as "positive" rainfall shocks, and shocks below the average as "negative" rainfall shocks. I do so because the PROGRESA localities are heavily dependent on local agriculture, and we would expect that the entire economy of the locality to be somewhat dependent on local precipitation. Therefore, if in the past year there was less than average rainfall in the locality, we expect a lower productivity on farms and other dependent activities, such as shops or transport. On the other hand, a positive precipitation shock would increase productivity for the entire locality. One worry is that too large a positive shock might be a negative shock because of flooding. I test this by examining the relationship between the >30% rainfall category and self reported floods, which were recorded in some (but not all) of the PROGRESA survey rounds.

Table 1 - Summary statistics for children aged 8-18, separated by sex.

	Males				Females			
Variable	Mean	Std. D	Min	Max	Mean	Std. D	Min	Max
Attending school	0.683	0.465	0	1	0.654	0.475	0	1
Annual precip, cm	103.525	43.354	18.88	354.61	103.3	43.09	18.88	354.6
Treatment	0.625	0.483	0	1	0.628	0.483	0	1
Years schooling	6.293	0.689	0	12	6.293	0.713	0	12
Age	12.839	3.142	8	18	12.906	3.158	8	18
Household head sick	0.053	0.291	0	1	0.052	0.29	0	1
Family member sick	0.087	0.296	0	1	0.085	0.293	0	1
Head unemployed	0.062	0.289	0	1	0.059	0.285	0	1
Working	0.257	0.437	0	1	0.164	0.370	0	1

Table 2 - Summary statistics for children aged 8-18, separated by PROGRESA treatment

	Treated localities				Non-Treated localities			
Variable	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Attending school	0.680	0.467	0	1	0.698	0.459	0	1
Years of schooling completed	6.313	0.677	0	12	6.220	0.653	0	12
Age	12.902	3.159	8	18	12.803	3.129	8	18
Sickness of head of household	0.033	0.490	0	1	0.064	0.292	0	1
Family member sick	0.065	0.493	0	1	0.108	0.298	0	1
Head of household unemployed	0.050	0.486	0	1	0.063	0.290	0	1
Working	0.212	0.409	0	1	0.154	0.361	0	1

Table 3 - Summary statistics of biological outcome variables for young children

	males			females			
Variable	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Ages of Children
Height (cm)	3485	97.1	9.0	3484	96.1	8.9	0 to 6
Weight (kg)	3497	15.4	3.0	3505	14.9	3.0	0 to 6
Words recognized	771	62.3	30.1	744	66.0	28.8	0 to 2

Figure 2 - the Precipitation Grid, and the Mexican Localities. The green diamonds are each of the points in the .5 by .5 grid. The red dots are a sampling of the PROGRESA communities.

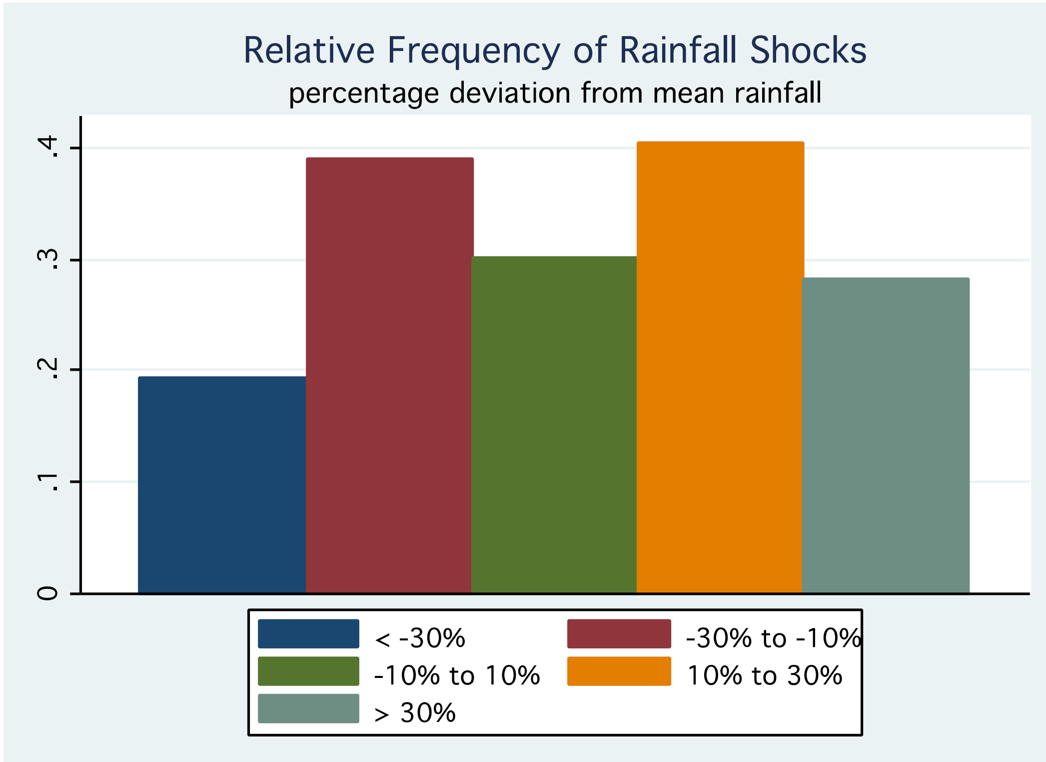

Figure 3 - relative Frequency of rainfall categories for PROGRESA localities.

Table 4 - Frequency of different categories of rainfall shocks in treated and non treated areas.

	Variable	Mean	Std.
not treated	< -30%	0.121	0.271
	-30% to -10%	0.272	0.337
	-10% to 10%	0.228	0.301
	10% to 30%	0.262	0.327
	> 30%	0.117	0.298
treated	< -30%	0.128	0.318
	-30% to -10%	0.244	0.438
	-10% to 10%	0.201	0.331
	10% to 30%	0.265	0.450
	> 30%	0.162	0.388

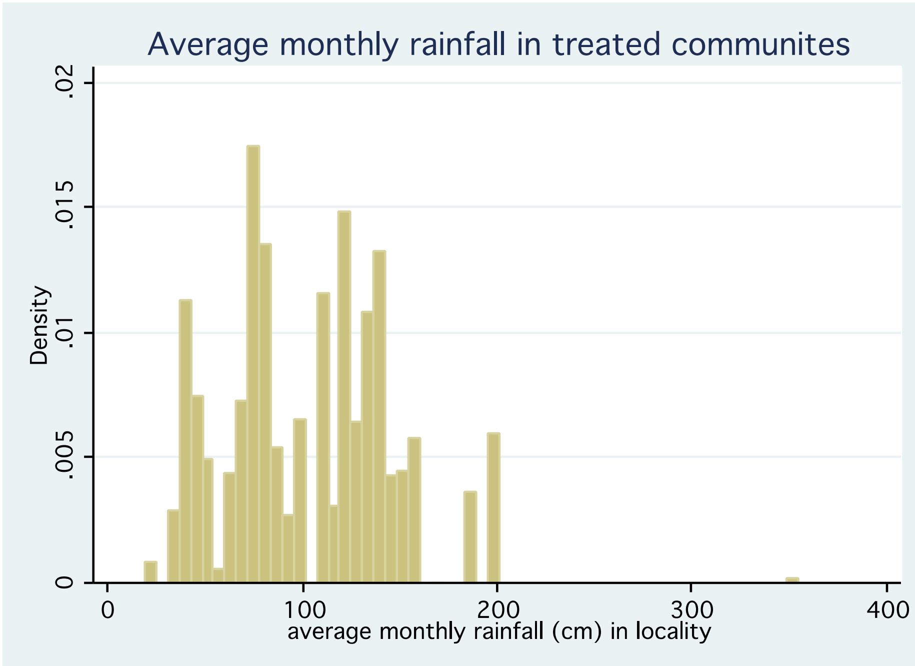

Figure 4 - Average monthly precipitation in treated communities.

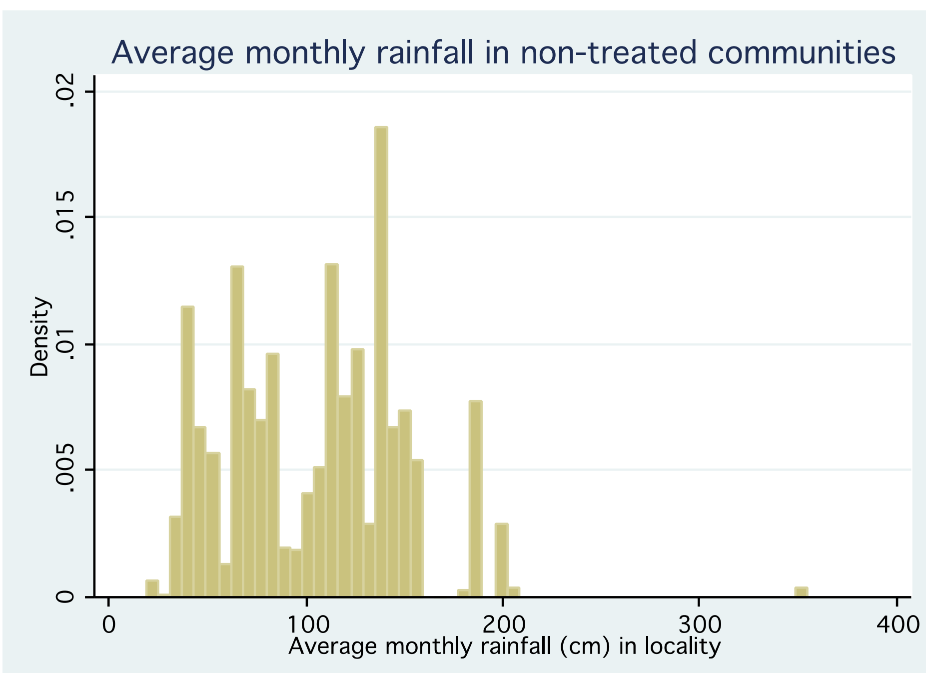

Figure 5 : Average monthly precipitation in non-treated communities. Note the similarity in distribution between these and the treated communities, including the outliers between 300 and 400 cm/month.

I find that the percentage of households reporting a flood is 12% for households experiencing rainfall in the >30% category, 6% for households experiencing rainfall in the 10% to 30% category, 1% in the -10% to -30% category, and essentially zero in the <-30% category. Floods are relatively rare, and for the most part we can think of positive rainfall shocks as good shocks and negative rainfall shocks as bad shocks.

Econometric Specification

The econometric analysis of this paper is divided into two major sections. The first concerns the effects of concurrent rainfall shocks on school attendance and labor force participation, and whether PROGRESA mitigates the effects of these shocks. The second concerns the effects of early life rainfall shocks, and PROGRESA's capacity to mitigate these effects. The analysis for these two is substantially different, so we describe them separately.

Concurrent Shocks

First, I construct an econometric model that will estimate how families treat their children differently in response to shocks, and whether PROGRESA mitigates these effects. The variables of interest are changes the changes in child's school attendance, and changes in the child's participation in the work force. School attendance is defined as meeting the school attendance requirements for receiving PROGRESA benefits, and is a binary variable. Labor participation is defined as working for a wage or consistently helping with a family operated farm or business in the last four weeks. Both of these items are included in each wave of the PROGRESA surveys. Shocks are incorporated from the precipitation data set. Precipitation shocks are measured by the categories described in the preceding section, over the 12 months prior to the survey.

The econometric specification for the schooling equation is given as:

$$S_{i\,j\,t} = \alpha_0 + \alpha_1 S_{i\,j\,t-1} + \boldsymbol{\alpha_2 p}_{j\,t} T_{i\,j\,t} + \boldsymbol{\alpha_3 p}_{j\,t}(1-T)_{i\,j\,t} + \alpha_4 T_{i\,j\,t} + \boldsymbol{\alpha_5 X}_{i\,j\,t} T_{i\,j\,t} + \boldsymbol{\alpha_6 X}_{i\,j\,t}(1-T)_{i\,j\,t} + \boldsymbol{\alpha_7 L}_{i\,j\,t} + \boldsymbol{\alpha_8 R}_{i\,j\,t} + \boldsymbol{\alpha_9 A}_{i\,j\,t} + C_i + \epsilon_{i\,j\,t}$$

Equation 1

Where $S_{i\,j\,t}$ whether child i in community j is attending school in year t, according to PROGRESA's criteria for school attendance[6] $\mathbf{p_{j,t}}$ is vector of dummies for different levels of precipitation shocks for child i in community j at in the year leading up to t, and $T_{i\,j\,t}$ is an indicator variable for the treatment of PROGRESA for child i in community j in the year leading up to t. 1- $T_{i\,j\,t}$ is the negation of T, and is an indicator for non-treatment. $\mathbf{X_{i\,j\,t}}$ is a vector of control shocks for child in community j in the year leading up to t, and includes sickness of the head of household, sickness of another family member, and unemployment of the head of household. Each of these is 1 if the shock is experienced in the year leading up to t, and zero otherwise. $\mathbf{L_{i\,j\,t}}$ is a vector of dummies for the maximum year of schooling attained by the child in the year leading up to t (i.e. , one dummy for the first year of public school, one for the second, etc.) to control for students who have already completed school or are at a critical transition period. $\mathbf{R_{i\,j\,t}}$ is a vector of

[6] Defined as missing fewer than 5 days of school for non-health related reasons in the past 6 weeks

round dummies for each PROGRESA survey, C_i is the child fixed effect, $\mathbf{A}_{i\,t}$ is a vector of age dummies for each individual i in the year leading up to t, and epsilon$_{i\,j\,t}$ is the error.

The final specification requires a number of controls to make it robust. First, we control for other, non-precipitation shocks in $\mathbf{X}_{i\,j\,t}$. Again, these include illness of the head of household, illness of another family member and unemployment of the head of household. It is important to control for past attendance behaviors when investigating current school attendance and work habits. It has been demonstrated many times in the literature, both within the PROGRESA dataset and elsewhere that a child rarely re-matriculates to school once he or she has exited. See for example Morley 2003), Patrinos (2005), and Schady (2006). For this reason, we control for the child's prior enrollment status, or school attendance a year ago.

I include child fixed effects to allow for unobserved time-invariant heterogeneity and a time fixed effect to absorb all context variables common to all children. The sample size is approximately 50 thousand children aged 8 to 18 over the lifetime of the program. Each of these children belonged to families that were identified as being poor enough to receive PROGRESA benefits, in both treatment and control communities. The above specification is first differenced, which cancels out the time-invariant child-specific characteristics, effectively controlling for them. This yields:

$$\Delta S_{i\,j\,t} = \beta_0 + \beta_1 \Delta S_{i\,j\,t-1} + \boldsymbol{\beta_2} \Delta \boldsymbol{p}_{j\,t} T_{i\,j\,t} + \boldsymbol{\beta_3} \Delta \boldsymbol{p}_{j\,t} (1-T)_{i\,j\,t} + \beta_4 T_{i\,j\,t} + \boldsymbol{\beta_5} \Delta \boldsymbol{X}_{i\,j\,t} T_{i\,j\,t} + \boldsymbol{\beta_6} \Delta \boldsymbol{X}_{i\,j\,t} (1-T)_{i\,j\,t} + \boldsymbol{\beta_7} \Delta \boldsymbol{L}_{i\,j\,t} + \boldsymbol{\beta_8} \Delta \boldsymbol{R}_{i\,j\,t} + \boldsymbol{\beta_9} \Delta \boldsymbol{A}_{i\,j\,t} + e_{i\,j\,t}$$

Equation 2

The lagged dependent variable, S_{ijt-1} is correlated with the error term e_{ijt}if there is autocorrelation in $e_{i\,j\,t}$. Unobserved characteristics could be correlated with one or more of the explanatory variables, leading to biased coefficient estimates. To address the endogeneity of the lagged change in S, I use a two stage least squares approach for the whole equation.

Excluded instruments for S_{ijt-1} include the level of time t-2 school attendance, and t-1 and t-2 levels of PROGESA treatment. I also instrument with past shocks, such as time t-1 and t-2 changes in rainfall catagories and time t-1 and t-2 of **X** , the vector of other shocks. All of the shocks are intereacted with the indicator for treatments and the negation of treatment.

I also instrument $\mathbf{p}_{\mathbf{j\,t}}$ with the next four closest points on the precipitation grid as described earlier. Each of the eight categories of rainfall for the closest point is instrumented by the corresponding categories in each of the next four closest points. Each year-locality combination therefore has 32 total instruments for precipitation.

The parameters of interest from the second stage equation are $ß_2$, the effects of shocks in treated communities, and $ß_3$, the effects of shocks in non-treated communities. I allow the effects of precipitation shocks and other shocks to differ across treated and non-treated communities to test whether PROGRESA mitigates the effects of shocks. If precipitation has some effect on our dependent variable, we would expect $ß_3$ to be non-zero, and if

PROGRESA has some mitigating effect, we would expect the absolute value of ß$_2$ to be smaller than the absolute value of ß$_3$.

In addition to estimations for school attendance, we estimate labor participation. The final econometric specification for this estimation is given as:

$$\Delta W_{i\,j\,t} = \zeta_0 + \zeta_1 \Delta S_{i\,j\,t-1} + \boldsymbol{\zeta_2} \Delta \boldsymbol{p}_{j\,t} T_{i\,j\,t} + \boldsymbol{\zeta_3} \Delta \boldsymbol{p}_{j\,t}(1-T)_{i\,j\,t} + \zeta_4 T_{i\,j\,t} + \boldsymbol{\zeta_5} \Delta \boldsymbol{X}_{i\,j\,t} T_{i\,j\,t} + \boldsymbol{\zeta_6} \Delta \boldsymbol{X}_{i\,j\,t}(1-T)_{i\,j\,t} + \boldsymbol{\zeta_7} \Delta \boldsymbol{L}_{i\,j\,t} + \boldsymbol{\zeta_8} \Delta \boldsymbol{R}_{i\,j\,t} + \boldsymbol{\zeta_9} \Delta \boldsymbol{A}_{i\,j\,t} + o_{i\,j\,t}$$

Equation 3

Where $W_{i\,j\,t}$ is a binary variable indicating participation in the labor force, and all other variables have meanings identical to equation 2, except for the error term $o_{i\,j\,t}$. Note that the lagged change in schooling is also included in this specification. This follows de Janvry's paper: a change in school attendance in prior years usually indicates a change in child labor in the current year. This is a two stage least squares estimation using the same set of instruments as the Schooling equations. The outcomes of interest are Zeta's 2 and 3, and have interpretations similar to ß$_2$ and ß$_3$. Both specifications (equations 2 and 3) are separately estimated on males and females, since past literature has found different effects for these two subsamples.

Both PROGRESA treatment and rainfall shocks are randomly assigned, which reduces the scope for correlation between them and omitted variables that influence school attendance or work. There is also a substantial amount of attrition in the PROGRESA data. Only half of the children identified in 1997 are present in all seven of the PROGRESA surveys. If attrition is the result of time invariant child characteristics, there is no concern since all specifications effectively control for child fixed effects. However, it is feasible that receiving the PROGRESA treatment makes attrition more or less likely, and that without PROGRESA intervention rain shocks make attrition more or less likely. For example, families in treatment communities might be more likely to be successively surveyed because completing the survey was required in order for them to receive their benefits. Families in control communities had no such incentive, and might avoid a drain on their time by not taking the survey. On the other hand, PROGRESA might remove the need for migrant labor by providing money migrant labor would have. Drought or flood might also prevent or encourage migration from the community regardless of PROGRESA treatment, removing those families that might be most affected by the rainfall. These families might also be those who are most likely to take their children out of school or send their children to work, which would bias my results. If the types of families that would tend to take their kids out of school in response to a drought or avoid a survey are also the ones who tend to move out of the area after a drought, then I will underestimate the effect of a drought on school attendance. If PROGRESA makes it more likely that families will migrate when a drought hits, and the types of families who migrate are also the types who would take their kids out of school, then I'll overestimate the degree to which PROGRESA mitigates the effect of a drought on school attendance.

Another concern is that the matched control group added in 2003 has systematically different observable characteristics from the original treatment and control groups from the beginning of the PROGRESA program. This is in fact the case -- see columns one, two and three of table 5 . These give means of characteristics of individuals in 1997 for the treatment, control and matched control groups. The differences between treatment and control groups are negligible, but the differences between these two groups and the matched control group is large and statistically significant. Many investigations of PROGRESA, including Behrman, Parker, and Todd (2009) have demonstrated that the matched control group is significantly different from the other two. If the matched control group is better off than the other groups, then any evaluation of the welfare components of PROGRESA could be substantially underestimated. For example, a wealthier group of people might be more resistant to rainfall shocks, which could bias my estimations towards zero.

To address these concerns, I use a technique called propensity score reweighting. The idea of propensity score reweighting is to reweight the observations in all groups and all survey rounds to balance the mean characteristics of individuals in that group and the treatment group in the base year. Propensity scores are estimated using a logit regression where the dependent variable is a binary variable equal to one if the individual was in the 1997 treatment group. Explanatory variables include all of the items in table 5. The idea is to include many exogenous household variables to ensure that households with similar observable characteristics to those in the original treatment group in 1997 get the most weight in the regression. In most cases, I use base year (1997) values of the observable characteristics in the logit. For example, I include household missing and family member missing in the base year to address concerns about migration. Note this variable is "missing in the base year only" otherwise I might be weighting people according to factors that could be altered by PROGRESA, which could bias my results. If people with certain observable base-year characteristics are systematically more likely to leave the sample, and if these observable characteristics are correlated with the outcome of interest, the reweighting will correct the bias that would otherwise occur. I also include number of individuals in each household, parental education from each year, and from the base year daily wage, indoor plumbing, sickness of the head of household, sickness of a family member, and unemployment of the head of household. For each group – year combination, I limit the sample to the 1997 treatment group and the other group-year combination, and perform the logit. In total, I perform 21 logit regressions, one for each of the 7 survey rounds and three groups. The average predicted probability of being in the treatment group in the base year from these logits, for each group and time period, are given in Table 6. Then, following Rosenbaum (1987), the propensity scores from these regressions were used to reweight the observations in the rest of the survey rounds. These weights are the relative odds of being in the target group given the set of observable characteristics included as explanatory variables in the logit. The actual weight used is the predicted probability divided by (1- the predicted probability). This technique ensures that I am only comparing similar individuals across the three different groups and the 7 survey rounds.

Table 5 gives unweighted and weighted means of observable characteristics in each group. Column 1 gives summary statistics for children aged 8-18 for the original treatment in the year 1997. Column 2 gives unweighted means for the original control group in the year 1997. None of the variables are statistically significantly different from the original treatment group. Column 3 gives unweighted means for the matched control group in the year 1997. All of the means are statistically significantly different from the means for the other two groups, except for sickness of the head of household, family member sick, or head of household unemployed. Columns 4 and 5 give means for the weighted original control and matched control after weighting. The means are almost identical to those for the original treatment group, and none of the differences are statistically significant.

Arellano-Bond estimation is the typical tool for "large N, small T" panel datasets like PROGRESA, meaning a large number of observations and a small number of rounds, when there is a lagged dependent variable in the specification. I use a two stage least squares regression instead of Arellano

Bond estimation for two reasons. First, it is more accessible than Arellano Bond estimation, and second, a 2 stage least squares approach allows me to more easily reweight the data. I perform the Arellano – Bond estimations, as well as unweighted estimations as a robustness check (tables A6 and A7) and the estimates are very similar.

Table 5 - Means of individual and household characteristics for children aged 8-18 across different experimental groups, weighted and unweighted.

Variable	Original treatment group	Original control group	Matched control group	Original control, weighted	Matched control, weighted
Years of schooling completed	6.2665	6.22	7.001	6.2663	6.2663
Mother's education	4.009	4.021	4.502	4.008	4.009
Father's education	3.961	3.956	4.342	3.962	3.961
Age	12.852	12.803	12.342	12.83	12.85
Household size	5.81	5.76	5.22	5.81	5.81
Sickness of head of household (base)	0.0485	0.064	0.0312	0.0484	0.0485
Family member sick (base)	0.0865	0.108	0.0556	0.0864	0.0864
Head of household unemployed (base)	0.0565	0.063	0.0452	0.0569	0.0567
Daily wage, (base)	39.81	39.77	46.26	39.82	39.81
Member missing, (base)	0.007	0.012	0.005	0.007	0.007
Household missing, (base)	0.008	0.015	0.006	0.008	0.008
Indoor plumbing (base)	0.344	0.322	0.401	0.343	0.344
Household size	5.81	5.76	5.22	5.81	5.81

Table 6 – Average predicted probability of inclusion in 1997 treatment group from logits, by group and year. Note the high probability of inclusion in the treatment and control groups, and the lower probability in the matched comparison group.

Round /group	1997	1998	1999	1999	2000	2000	2001	2002	2003
Treatment	1	.912	.916	.902	.893	.888	.882	.878	.891
Control	.901	.891	.886	.880	.897	.872	.830	.841	.881
Matched comparison group	.682	.691	.698	.709	.711	.731	.740	.732	.756

Early Life Shocks

The second half of the analysis investigates the effects of early life rainfall on later life outcomes. The analysis is designed to answer two questions. First, does an early life rainfall shock have long-term biological or socioeconomic consequences? And second, does PROGRESA mitigate these consequences? Only a small fraction of children were born into a family that was being treated by PROGRESA (about 5% of all children surveyed in all waves) but the effectiveness of the program is still measurable.

As stated earlier, the final round of PROGRESA surveys occurred in 2003, and included a detailed biological survey as well as the standard socio-economic survey. This means that all of the children in the PROGRESA program had certain biological outcomes measured in 2003, which I will use as my variables of interest. Again, these include height, weight, and the number of words recognized on a test. Since there is only one observation of Biological outcomes, the data are no longer panel data. The econometric specification to investigate the effects of early life rainfall for person i in locality j born in year t can be written:

$$Y_{i\,j\,2003} = \delta_0 + \boldsymbol{\delta_1 p}_{k\,t} T_{i\,j\,t} + \boldsymbol{\delta_2 p}_{k\,t}(1 - T_{i\,j\,t}) + \boldsymbol{\delta_3 p}_{k\,t-1} T_{i\,j\,t-1} + \boldsymbol{\delta_4 p}_{k\,t-1}(1 - T_{i\,j\,t-1}) + \delta_5 T_{i\,j\,t} + \delta_6 T_{i\,j\,t-1} + \boldsymbol{\delta_8 W}_{i\,j\,t} + \delta_{10} YEARS_{i\,j\,t} + \boldsymbol{\delta_9 COM}_j + \boldsymbol{\delta_7} TREND_j + v_{i\,j\,t}$$

Equation 4

Where $Y_{i\,j\,2003}$ is the outcome of interest for person i in community j recorded in the final round of PROGRESA surveys, 2003. $\mathbf{p_{j\,t}}$ is the instrumented vector of binary indicator variables for different levels of precipitation, T_{ijt} is a binary indicator for treatment by PROGRESA, (1-T) is the complement of T (not treated), $TREND_j$ is a linear time trend specific to the community j, $\mathbf{W_i}$ is a vector of year of birth dummies (which effectively controls for age, since there is only one measurement of the outcome of interest, in 2003) , $\mathbf{COM_j}$ is a vector of community dummies, $YEARS_{ijt}$ is the number of years of PROGRESA treatment after age 1, and v_{ijt} is the error term. Precipitation and treatment in both the year following birth (time t) and the year prior to birth (time t-1) are included in the specification. The community fixed effects control for persistent effects of rainfall on the places (and households) in which children are born. Effects of persistent differences in rainfall across communities on long-run income of households will be common to all individuals born in the same area and so should be absorbed by these community fixed effects. I control for the number of years of PROGRESA treatment after age 1 to allow for the possibility that treatment might have some cumulative effect on certain biological outcomes. Like the schooling and labor regressions, the rainfall categories are instrumented by the next four closest points. The parameters of interest are δ_1 and δ_2 , which give the effects of year of birth rainfall on the outcome of interest in non treated and treated areas respectively, δ_3 and δ_4, which will give the effects of year prior to birth rainfall in both types of communities. Standard errors are clustered by the locality of birth.

Outcomes of interest include height, weight, and mental development, measured by the number of words recognized by a child. These measures are only taken in the final round of surveys, so each individual has only a single observation of each of these outcomes. Early life rainfall is defined as the average rainfall for the 12 months following the month of birth, including the month of birth. Shocks are measured in levels relative to the average rainfall in each community over the 1973-2003 period the same way they are for the earlier specifications.

This specification might be vulnerable to sample attrition, and/or differences between the three groups. To address this problem, I reweight this specification using the same methodology as specifications 2 and 3, described above. However, weighted and non-weighted estimations are very similar, compare tables 4.1 and A8, and tables 4.2 and A9.

We might also worry that the treatment and control cohorts of children are fundamentally different as a result of being treated. For example, if PROGRESA benefits help keep sickly children alive that would otherwise die, then PROGRESA communities might have sicklier individuals than non-PROGRESA communities. The same argument can be made for rainfall shocks in absence of PROGRESA treatment: a strong negative rainfall shock could remove some of the sicklier children from the population, and the remaining children might appear healthier on average. Since we expect to find that PROGRESA communities have healthier children than non-PROGRESA communities, and for PROGRESA to mitigate the effects of rainfall, either of these effects would bias the coeffiecients in δ_2 δ_4 δ_5 , and δ_6 on negative rainfall shocks towards zero, and could bias δ_1 and δ_3 on negative rainfall shocks to be more negative. Also, parents who have children during a drought might be different from those who wait for rainier weather. Although the propensity score re-weighting should help address these issues, I perform a separate robustness check for each of these concerns. I test whether there is any correlation between PROGRESA treatment or year of birth rainfall and maternal characteristics. Substantially different maternal characteristics might indicate the cohorts of children are fundamentally different depending on PROGRESA treatment or year-of-birth rainfall. This robustness check is addressed in the results section, but I do not find any statistically significant relationships. See appendix table A1.

These two categories of specifications, the early life shocks and the concurrent shocks, are a series of difference in differences analyses. Difference in differences analyses require four groups of data: treatment and control groups, and before and after treatment observations for both groups. In the early life regressions, the coefficient on PROGRESA treatment is a simple difference-in-differences comparison: the treatment group contains communities receiving PROGRESA and the control group contains communities not receiving PROGRESA. The before/after groups are cohorts born before/after the onset of treatment. For this analysis it is essential to have observations of children within each community who were born before and after the onset of PROGRESA treatment. If not, the effect of treatment would be perfectly collinear with the community dummies.

The coefficient on the interaction of rainfall with treatment and rainfall without treatment are also difference-in-differences comparisons, but they occur in two disjoint sets of communities. Within each type of community (receiving PROGRESA and not receiving), the treatment group consists of communities that suffered a shock, and the control consists of communities that did not suffer a shock. The "before" group is made up of children born before the drought, and the "after" group is made up of children born during the drought. To identify the effect of the interaction, it is not necessary to observe individuals in a given community who were born in years before and after treatment. I am simply allowing rainfall, which varies randomly from year-to-year, to have different effects in treatment and control communities. Here, the interaction between rainfall shock and time-invariant treatment status would not be perfectly collinear with the community dummies, even without before and after observations on treatment. This logic applies to the schooling and child labor regressions as well.

Results

Concurrent Shocks

First I report the results for the concurrent shocks regressions. These include the results of estimations of equations 2 and 3, the school attendance and child labor regressions. These are the fully specified two stage least squares regressions. The dependent variable for equation 2 is change in school attendance from period t-1 to t. School attendance is defined as meeting the school attendance requirements for receiving PROGRESA benefits, and is a binary variable. The dependent variable in equation 3 is change in child labor participation from period t-1 to t, and is defined as working for a wage or in a family owned shop or farm. The coefficients can be interpreted as the change in probability of school attendance or child labor from the last period to the current period. The precipitation shocks and some other shocks are interacted with “treatment”, a binary variable indicating that the individual was randomly selected to receive PROGRESA benefits in the current period, and “not treated”, indicating that the individual was not being treated in the current period. In each period, these two groups are disjoint subsamples.

To avoid perfect colinearity, I omit the -10% to 10% category for precipitation. This way we can compare the effects of rainfall categories to some measurement of “base” rainfall. The coefficients estimated from any other category of rainfall can be interpreted as the difference in the effect between that category and normal rainfall. The coefficient on the “treatment” variable should be interpreted as the effect of PROGRESA treatment on school attendance in this omitted category of rainfall. To get the total treatment effect of PROGRESA in any category of rainfall, then we add the coefficient on the treatment variable and the coefficient on the other category of rainfall.

Although they are estimated separately, I choose to report these sets of results side by side, since the results from one illuminate the results from the other. I report my results by gender, since PROGRESA and rainfall have different effects on boys and girls.

Concurrent Shocks – Boys

I present the effects of rainfall and PROGRESA treatment on school attendance and child labor for boys in table 7, and the effects of non-precipitation shocks in table 8. Not reported in these tables are the age fixed effects, the round fixed effects, or the level of schooling fixed effects. The age fixed effects are reported in the appendix (table A5).

For males in non-treated areas, school attendance and child labor are sensitive to precipitation shocks. Strong negative rainfall shocks make boys about 3.7 percentage points less likely to attend school relative to the omitted category, and 3.3 percentage points less likely to participate in the labor force. The school attendance result is consistent with an income effect of drought. When there is a drought, households have less income to send their children to school. Since the primary vocation for boys is agriculture, they are less likely to work during a drought as well, a substitution effect. These children might be engaged in leisure, or they might be occupied in some other productive activity not measured by the PROGRESA surveys.

A strong positive rainfall shock reduces the likelihood of school attendance by 3.3 percentage points, and increases the likelihood of labor participation by 2.9 percentage points. This is consistent with a substitution effect: abundant precipitation increases a child's productivity in the fields, and therefore increases the opportunity cost of school. Unemployment of the head of household makes boys 2.6 percentage points less likely to attend school and 1.9 percentage points more likely to be working, which suggests that boys leave school to replace lost income when the primary breadwinner is unemployed. We also see this effect when the head of household is sick, which makes boys 7.2 percentage points more likely to participate in child labor. Strangely, this shock has no effect on school attendance.

In treated areas, PROGRESA increases the likelihood of attending school for boys by 2.9 percentage points in the omitted category, and decreases the likelihood of child labor by 7 percentage points. PROGRESA treatment mitigates the effect of a strong positive rainfall shock for boys. In treated areas, the additional effect of rainfall 30% or more above average rainfall is small and statistically indistinguishable from zero. It is statistically significantly smaller than the effect for a similar shock in non-treated communities, suggesting that PROGRESA completely mitigates this kind of shock. A strong positive rainfall shock also makes them 4 percentage points less likely to work. This is consistent with the idea that PROGRESA has the biggest effect on child labor for the people at the margin of the child labor decision.

Negative shocks actually enhance PROGRESA's treatment effect on attending school for boys. PROGRESA has a bigger positive impact on probability of going to school for households suffering negative rainfall shocks. Rainfall 10% to 30% below average rainfall in PROGRESA communities contributes a 3.8 percentage point increase in the effect of PROGRESA on the likelihood of attending school, and a stronger negative shock contributes 6.6 percentage point increase. This is intuitive: I expect PROGRESA to have a larger effect on those who are most affected by outside shocks. The children being affected here are the marginal children, or those who are most likely to leave school because they are experiencing a negative shock, and most easily helped by an incentive and income transfer mechanism like PROGRESA. This pattern of coefficients can again be explained as PROGRESA having the biggest effect on child labor for those closest to the margin of participating in child labor, or those for whom child labor is relatively productive and benefit from high rainfall.

Still, we would expect the income effect of PROGRESA to have some additional effect when rainfall is very low. It might be that the money provided by the PROGRESA program creates employment opportunities for children when rainfall is scarce, creating a substitution effect for PROGRESA and scarce rainfall.

Table 7 - Effects of rainfall and PROGRESA on change in school attendance and change in child labor, boys

		School Attendance		Labor Participation	
		Treatment	Control	Treatment	Control
Precipitation shocks	< -30%	**0.066***** 0.002	**-0.037***** 0.008	0.002 0.001	**-0.033***** 0.01
	-30% to -10%	**0.038***** 0.002	**-0.027***** 0.006	0.003 0.003	0.0056 0.004
	10% to 30%	0.003 0.009	0.012 0.006	**-0.012***** 0.002	-0.006 0.007
	> 30%	-0.003 0.004	**-0.033***** 0.005	**-0.039***** 0.003	**0.0293***** 0.007
	Treatment	**0.029***** 0.006		**-0.070***** 0.006	
	Observations	98,244	98,244	102,659	102,659
	Individuals	24,302	24,302	22,906	22,906

*** p<0.01, ** p<0.05, * p<0.1

Not reported: age, round and schooling fixed effects

Standard errors (clustered by locality) below estimates

Table 8 - Effects of other shocks and PROGRESA on change in school attendance and change in child labor, boys

	School Attendance		Labor Participation	
	Treatment	Control	Treatment	Control
Attended School Last Year	**0.298***** 0.003	**0.298***** 0.003	**-0.177***** 0.001	**-0.177***** 0.001
sickness of head of household	0.022 0.019	0.026 0.017	**0.041***** 0.012	**0.072***** 0.015
Sickness of a family member	0.006 0.005	0.018 0.014	0.003 0.004	0.012 0.012
Unemployment head of household	-0.009 0.005	**-0.026***** 0.005	-0.002 0.003	**0.019***** 0.005
Observations	98,244	98,244	102,659	102,659
Individuals	24,302	24,302	22,906	22,906

*** p<0.01, ** p<0.05, * p<0.1

Not reported: age, round and schooling fixed effects

Standard errors (clustered by locality) below estimates

Concurrent Shocks – Girls

I present the effects of Rainfall and PROGRESA treatment on school attendance and child labor for girls in table 9, and the effects of non-precipitation shocks in table 10. Not reported in these tables are the age fixed effects, the round fixed effects, or the level of schooling fixed effects. The age fixed effects are reported in the appendix (table A5).

Girl's school attendance and labor participation are less responsive to rainfall than boys. Girl's attendance is only affected by a strong positive rainfall shock in control communities, which makes them 2.8 percentage points less likely to attend relative to the omitted category. A similar shock makes girls 2.9 percentage points less likely to work in control areas. The child labor result is expected: if heavy rain provides a positive income effect for households, then girls will be less likely to work after abundant rainfall, especially if their lines of work are non-agricultural, as demonstrated by the data. The school attendance results are more difficult to explain in an income/substitution effect framework: we would expect that the income effect would lead to more girls attending school. One explanation is that households value girl's leisure more than girl's school attendance, or that increased rainfall somehow enhances girl's leisure time. If this were the case, we would expect an income shock to shift girls from labor and school to leisure. Alternatively, the mother's agricultural productivity could rise when there is a positive rainfall shock, and then daughters are then "drafted" to assume some of the mother's household responsibilities.

Unlike boys, a strong negative rainfall shock makes girls 2.2 percentage points more likely to join the labor force. This makes sense given the different vocations of boys and girls: of a household is in need of money, and their boys are not productive in their lines of work, then a household should be more likely to send their girl to work.

In the control areas, sickness of a family member makes girls 3.5 percentage points less likely to attend school, and 2.4 percentage points less likely to be working. This implies that girls are staying home to care for sick members of the household. Sickness of the head of household makes girls 1.8 percentage points more likely to attend school, which is odd, given the last result. This effect is persistent in treated communities, as well.

Girls in treated communities are 3.8 percentage points more likely to attend school, and 5.7 percentage points less likely to participate in the labor force. However, there are no significant differences between the additional effects of shocks in treated communities and the effects of shocks in non-treated communities. This too is a strange result, but it might be that the coefficient on “treatment” is capturing the entire income effect of PROGRESA for girls, and the substitution effect is less important because girls are involved in different lines of work. Note that the treatment effect is more than large enough to offset all of the consequences of rainfall in treated communities.

PROGRESA does not mitigate the effects of the head of household being sick in treated and control communities: the numbers are statistically indistinguishable. However, the program does mitigate the effect of a family member being sick: girls are actually more likely to attend school after this shock in treated areas.

Table 9 - Effects of rainfall and PROGRESA on changes in school attendance and changes in Child Labor, girls

		School Attendance		Labor Participation	
		Treatment	Control	Treatment	Control
Precipitation shocks	< -30%	-0.017	-0.010	**0.0221*** **	**0.0223*** **
		0.027	0.007	0.007	0.006
	-30% to -10%	-0.002	0.001	0.0042	-0.005
		0.004	0.005	0.002	0.003
	10% to 30%	0.001	0.001	-0.008	0.001
		0.002	0.006	0.005	0.003
	> 30%	**-0.022*** **	**-0.028*** **	**-0.031*** **	**-0.029*** **
		0.005	0.006	0.003	0.004
	Treatment	**0.038*** **		**-0.0057*** **	
		0.004		0.005	
	Observations	98,244	98,244	99,519	99,519
	Individuals	24,302	24,302	21,839	21,839

*** p<0.01, ** p<0.05, * p<0.1

Not reported: age, round and schooling fixed effects

Standard errors (clustered by locality) below estimates

Table 10 - Effects of other shocks and PROGRESA on change in school attendance and change in child labor.

	School Attendance		Labor Participation	
	Treatment	Control	Treatment	Control
Attended school last year	**0.283*** **	**0.283*** **	**-0.212*** **	**-0.212*** **
	-0.005	-0.005	-0.002	-0.002
Sickness of head of household	**0.013*** **	**0.018*** **	0.006	0.008
	-0.003	-0.004	-0.08	-0.01
Sickness of a family member	**0.025*** **	**-0.035*** **	**-0.022*** **	**-0.024*** **
	-0.002	-0.008	-0.002	-0.006
Unemployment head of household	-0.003	-0.006	-0.003	-0.006
	-0.002	-0.004	-0.002	-0.005
Observations	98,244	98,244	99,519	99,519
Individuals	24,302	24,302	21,839	21,839

*** p<0.01, ** p<0.05, * p<0.1

Not reported: age, round and schooling fixed effects

Standard errors (clustered by locality) below estimates

The estimated effect of the schooling lag is large: attending school the year prior makes boys 17 and girls 21 percentage points less likely to work in the current year. Recall de Janvry *et al*. (2006) did not include lagged schooling in their labor participation regressions because of its small effect on their results. However, I find that estimating equation 4 without lagged schooling as a predictor causes significantly different estimates. The results of this regression are given in the appendix, in table A4.

As we can see, school attendance decisions have different elasticities depending on the situation. One implication for these findings would be to redistribute PROGRESA transfers. I find that the PROGRESA funds are most effective during large negative precipitation shocks for boys. Consequently, we might make the stipend smaller for boys when there is normal rainfall, and larger when there is a positive or negative rainfall shock. Similarly, we might redistribute to families with girls when there is a large negative shock in order to mitigate their lessened attendance. This is a solution specific to rainfall shocks, but it is easily expandable to other shocks. For example, we might increase PROGRESA benefits to families who have a sick head of household or a sick family member. This shift in benefits from easy to difficult times would make PROGRESA behave more like government insurance. As a corollary, expanding access to drought or health insurance might increase school attendance. Several developing countries are pursuing this goal, including Mexico. See, for example King *et al*. (2009).

I find that PROGRESA increases the likelihood of attending school by about 6 Percentage points. This number is a weighted average of my estimated treatment effects during different types of shocks and across boys and girls. This estimate is similar to other estimates of the treatment effect of PROGRESA. Behrman and Todd (2005) estimate a 4-5 %-point treatment effect for the first three years of data[7], or Patrinos, Harry A., Lopez-Calva, Luis Felipe and Bando (2005) who estimate a 6-7 %-point treatment effect across all waves of the data. This estimate is smaller than the 8-11 %-point increase in likelihood of attendance measured in the Coady and Parker paper mentioned in the literature review, but their analysis focused on different age groups and the first three years of data. Drop out rates tend to be higher for older children, especially around critical transition points, i.e. the transition from primary to secondary school.

In any instrumented equation, we need to make sure that our instruments are valid. To check, I use a Sargan test for over identifying restrictions. This test compares the estimates using different subsets of instruments**.** If the estimates do not change significantly, this is some evidence that I am using valid instruments.[8] For the boy's child labor regressions, the test yields a p-value of .48. For the boy's school attendance regressions, this test yields a p-value of .37. For girl's child labor and school attendance regressions, the p-values are .44 and .37 accordingly. Each of

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[7] Although they focus on a more narrow age group.

[8] This does not prove that the instruments are valid, it might just be that all of the instruments are bad.

these results suggests I cannot reject the null hypothesis that my over-identifying restrictions are valid, which is evidence that my instruments are valid. As an additional robustness check, I run a similar analysis using an Arellano-Bond specification, which uses more lags of certain instruments. I also run the analysis without reweighting the sample. The details of these specifications are in the appendix (tables A6 and A7). The results of these three analyses are very similar, which lends credence to my results. However, the weighted least squares picks up the largest magnitude effects of rainfall shocks and PROGRESA treatment. This suggests that the matched control group is better off than the other groups. Without weighting, we would be comparing PROGRESA beneficiaries to people who have a higher standard of living which should underestimate the effects of the welfare program. By reweighting the sample, I give similar individuals across the three categories a higher weight, which should increase the measured effect of PROGRESA.

Early Life Rainfall

Next, I investigate the ability of PROGRESA to mitigate the consequences of early life shocks, or equation 4. The outcome variables of interest are height in cm, weight in kg, and score on a "number of words recognized" test administered during the survey for those children younger than two. In each specification I control for the year of birth, fixed effects for each community, a linear time trend for each community, and the number of years of PROGRESA treatment. To address concerns about differences between the matched control group and the other groups, as well as concerns about migration and attrition, I use the propensity score reweighting technique outlined earlier. In all regressions, we are interested in the effects of pre-birth precipitation and post-birth precipitation on the outcome, as well as their interaction with PROGRESA treatment.

Year After Birth Precipitation

The results for the effects of rainfall in the year after birth are given in table 11. Recall that the data used to estimate these coefficients are no longer panel data: the outcome variables were only observed once in 2003. Not reported in the table are the community fixed effects, the community time trends, and year of birth (age) dummies. Non-weighted estimates are given in appendix tables A6 and A7.

In non-treated areas, height, weight, and words recognized are sensitive to and negatively affected by strong negative rainfall shocks (< 30% below average rainfall). These effects are close to zero in treated areas, suggesting they are completely mitigated, and boys actually end up taller after a strong negative shock. Girls end up .87 cm shorter after a strong positive shock in non-treated areas, which is strange, but could be explained by floods. This effect is completely mitigated in the treated areas.

Boys and girls receive some benefit from treatment in the year after birth: treatment causes boys to be .49 cm taller and girls .28 cm taller, boys

.18 kg heavier, girls .16 kg heavier, boys to recognize 11 and girls to recognize close to 11.5 more words. It appears that PROGRESA also has some cumulative effect for boys: each year treated increases male height by .37 cm, and male weight by .132 kg. Other effects are uncertain.

Other biological measures that were investigated using this specification include hemoglobin concentration, resting heart rate, and the number of days sick in the last month, but I find no statistically significant effects from either PROGRESA treatment or rainfall shocks on these outcomes. Generally the standard errors are large, so it is not clear whether there is no effect or I simply cannot measure one with the available data. These results suggest that the year after birth is a more critical time than the year before birth. This confirms the results from Maccini and Yang (2008), who only find effects from rainfall shocks in the year following birth.

Table 11- The effects of Precipitation shocks in the year after birth on later life outcomes

		Height		Weight		Words	
VARIABLES		Male	Female	Male	Female	Males	Females
Precip. shocks not treated	< -30%	**-1.302*** **	**-1.355*** **	**-0.424*** **	**-0.398*** **	**-14.080** **	**-16.812** **
		(0.565)	(0.576)	(0.149)	(0.153)	(7.008)	(8.109)
	-30% to -10%	0.088	0.065	0.084	0.011	3.468	0.052
		(0.348)	(0.364)	(0.144)	(0.156)	(4.737)	(4.018)
	10% to 30%	-0.790	0.164	0.019	-0.109	7.595	8.569
		(0.549)	(1.744)	(0.172)	(0.174)	(12.31)	(6.082)
	> 30%	1.588	**-0.867** **	0.828	0.191	19.987	-22.463
		(1.238)	(0.403)	(0.554)	(0.759)	(16.33)	(24.17)
Precip. shocks treated	< -30%	**2.147** **	1.793	0.440	-0.101	13.510	-30.464
		(1.100)	(1.355)	(0.527)	(0.580)	(21.48)	(20.89)
	-30% to -10%	0.381	0.163	0.300	-0.094	7.414	-3.960
		(0.550)	(0.550)	(0.238)	(0.229)	(7.744)	(8.421)
	10% to 30%	0.159	-0.269	-0.453	-0.241	10.776	2.441
		(0.638)	(0.598)	(0.291)	(0.244)	(9.700)	(10.02)
	> 30%	1.576	-1.376	1.236	-0.846	33.799	28.896
		(2.047)	(1.732)	(0.864)	(0.730)	(25.23)	(33.54)
	Treatment in the year after birth	**0.499*** **	**0.280*** **	**0.180** **	**0.155** **	**10.944** **	**11.574** **
		(0.195)	(0.11)	(0.079)	(0.723)	(5.012)	(5.134)
	Years treated After age 1	**0.373** **	0.224	**0.132***	-0.045	0.422	-0.819
		(0.191)	(0.306)	(0.091)	(0.096)	(2.398)	(2.873)
	Observations	2,500	2,504	2,506	2,513	733	709
	R-squared	0.603	0.600	0.427	0.415	0.276	0.306
	Ages of children	0 to 6	0 to 6	0 to 6	0 to 6	0 to 2	0 to 2

Robust standard errors (clustered b locality) in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Not reported: locality fixed effects, locality time trends, year of birth

Year Before Birth Precipitation

There are few statistically significant effects from precipitation shocks in the year before birth. In non-treated and treated areas, boys are .89cm shorter after a strong negative shock in the year before birth. This is not statistically distinguishable from the treated areas, so it is unknown whether this kind of shock is mitigated. Females treated in the year before birth are .16 kg heavier.

These results have important implications for PROGRESA. Both boys and girls are vulnerable to early life fluctuations in environmental conditions, specifically rainfall, and the PROGRESA program effectively mitigated the impacts of these fluctuations. The consequences of early-life shocks on biological outcomes even a few years later should be considered in the decision of when and where to target PROGRESA benefits. They should also be considered when evaluating the program, since it is clear that the benefits of PROGRESA for children go far beyond decreased labor and increased school attendance. These results provide additional justification for interventions in the first few years of life, especially after an environmental shock. A program like PROGRESA might increase its effectiveness by providing weather and social insurance to protect children from the consequences of these early life shocks.

Table 12 - The effects of Precipitation shocks in the year before birth on later life outcomes

VARIABLES		Height Male	Height Female	Weight Male	Weight Female	Words Males	Words Females
Precip shocks not treated	< -30%	**-0.897****	-0.068	0.170	-0.258	-3.939	1.218
		(0.456)	(0.500)	(0.221)	(0.220)	(11.358)	(8.169)
	-30% to -10%	-0.137	-0.239	-0.061	-0.195	-0.624	1.121
		(0.322)	(0.319)	(0.146)	(0.141)	(5.173)	(5.041)
	10% to 30%	-0.651	-0.740	-0.305	-0.253	8.790	-5.705
		(0.499)	(0.460)	(0.220)	(0.202)	(7.681)	(7.820)
	> 30%	0.233	1.729	-0.027	0.703	-1.242	-8.956
		(1.331)	(1.520)	(0.644)	(0.671)	(16.096)	(35.567)
Precip shocks treated	< -30%	-0.616	-0.469	0.299	-0.396	-4.545	2.523
		(0.538)	(0.967)	(0.389)	(0.423)	(12.547)	(12.361)
	-30% to -10%	-0.420	-1.092	-0.161	-0.325	-6.281	-1.582
		(0.628)	(0.855)	(0.291)	(0.288)	(4.913)	(4.938)
	10% to 30%	-0.500	0.602	-0.213	0.263	9.321	-2.738
		(0.580)	(0.657)	(0.263)	(0.290)	(10.430)	(8.499)
	> 30%	0.324	1.225	0.513	0.222	-24.735	3.302
		(1.380)	(1.420)	(0.463)	(0.299)	(27.230)	(30.635)
	Treatment in the year before birth	-0.673	-0.526	0.034	**0.161***	1.880	0.373
		(0.428)	(0.479)	(0.208)	(0.091)	(5.032)	(5.161)
	Years treated After age 1	**0.373****	0.224	**0.132***	-0.045	0.422	-0.819
		(0.191)	(0.306)	(0.091)	(0.096)	(2.398)	(2.873)
	Observations	2,500	2,504	2,506	2,513	733	709
	R-squared	0.603	0.600	0.427	0.415	0.276	0.306
	Ages of children	0 to 6	0 to 6	0 to 6	0 to 6	0 to 2	0 to 2

Robust standard errors (clustered by locality) in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Not reported: locality fixed effects, locality time trends, year of birth

Robustness Checks

Now I outline the results of some robustness checks. One source of worry, mentioned earlier, is that precipitation shocks or treatment might fundamentally alter the composition of the cohorts that I am examining. For example PROGRESA might keep sicklier children alive, and a strong rain shock might kill them. This would bias any measurement of the benefit of PROGRESA towards zero: I would be comparing a treated community with more sickly children against a control community with fewer sickly children. To address this issue, I test whether the characteristics of a child's mother are different if the child was born during a rainfall shock or was treated by PROGRESA when they were born. If the cohorts of children are fundamentally different, we would expect certain maternal characteristics, such as years of schooling, age when the child was born, parental weight, years living in the locality, and the month of birth, to be different as well. For example, we might expect mothers with lower weight or lower education to give birth to sicklier children. I find no evidence that precipitation shocks or treatment has any effect on these parental characteristics. This check also addresses the concern that different kinds of parents might time their births to coincide with certain categories of rainfall, or seasonal births. Again, there is little evidence for this phenomenon. The results of this analysis are given in the appendix (table A1). Migration represents another cause for concern. We might expect precipitation shocks to drive people away from their locality, or that PROGRESA treatment might make attrition more or less likely by providing a family with additional funds. I check whether individuals or households are more likely to be missing or return to the panel depending on PROGRESA treatment and rainfall shocks. I find no statistically significant effects. The results are given in the appendix (table A2).

It is important to note that the height, weight, and words recognized effects are only measured when children are at most 6 years old. It could be that the effects measured in the above analysis do not represent permanent changes in outcomes, but rather the shocks retard growth relative to similarly aged children who did not experience a shock. I test this by investigating the effects of rainfall shocks in the year of birth on adult outcomes (ages 22-32). The results are given in table A10. Generally, the effects are similar to the estimations for children – rainfall more than 30 percent below average has large negative statistically significant effects on adult height and weight -- the point estimates are only slightly smaller. This implies that the effects of rainfall both retard development and have permanent lasting effects.

The above robustness check does not address the effects of PROGRESA, since adults in 2003 did not have PROGRESA intervention in their year of birth. To fully test the effect of PROGRESA's mitigating power on early life shocks, I would need later-life observations of the children who were born during the PROGRESA program.

Conclusion

Using panel data from the Mexican PROGRESA program and an external source of precipitation data, I find that shocks have large effects on school attendance for both boys and girls. This applies to the precipitation shocks that were the focus of the analysis and also shocks such as unemployment of the household head, sickness of the head of household, and sickness of another family member. Shocks also induce children to change their labor participation habits, although how they respond is a function of the gender of the child and the type of shock. We see that rainfall has both income and substitution effects: the income leads boys to to attend school less often after a drought, and the substitution effect leads them to participate in child labor less often. After a positive rainfall shock, the substitution effect dominates: boys attend school less, and work more. Girls respond differently to rainfall shocks. A strong negative rainfall shock increases the likelihood of girls working and a strong positive shock decreases their likelihood of working, which is consistent with an income effect if girls are in different lines of work than boys.

Attendance and labor are sensitive to other shocks, like sickness or unemployment of a family member. Boys respond primarily to changes in the head of household's condition, whereas girls tend to respond to sickness of a family member. Boys leave school and attend work to offset the loss of income from the incapacitation of the head of household, and girls tend to leave work and school to address sickness.

PROGRESA makes boys and girls more likely to attend school, and less likely to engage in child labor. The program also provides separate income and substitution effects for children in different categories for rainfall. PROGRESA has a larger benefit on marginal boys in treated communities, or those who are most affected by drought. The effects of PROGRESA do not change with rainfall in treated communities relative to control communities for girls. PROGRESA mitigates the effects of all other shocks for boys, but not always for girls.

I also find that precipitation shocks in the year after birth can have lasting biological impacts for children. Boys and girls who are exposed to negative rainfall shocks in their year of birth end up shorter, weight less, and recognize fewer words than those who are not exposed to a shock. PROGRESA treatment almost entirely mitigates these effects. There are no significant effects of rainfall shocks occurring in the year after birth. It is unknown from this analysis whether PROGRESA mitigates these effects in the long run. To ascertain this, I would require measurement of Biological outcomes later in life from people who were born during PROGRESA.

Cash transfer programs, therefore, play an important role in eliminating intergenerational poverty and child labor. The transfer of a small amount of money during difficult times can almost entirely mitigate the effects of negative shocks. After investigating the behavior of the program during precipitation shocks, we see that increasing benefits after bad shocks and decreasing benefits when times are easy could enhance the effectiveness of the program. We must also not limit the evaluation of cash transfer programs like PROGRESA to its effects on contemporary shocks: I have demonstrated that PROGRESA has important mitigating effects on early life shocks, which could have significant impact on long run outcomes. The rural poor lack formal insurance markets, so creating programs that serve the same purpose as insurance providers could greatly enhance welfare.

Appendix

Table A1 – Maternal characteristics

The following table gives the results of the "maternal characteristics" robustness check. The specifications for the first 4 variables are identical to equation 5, except the outcome variable is a maternal characteristic, and "years treated" is not included. The "month of birth" outcome is the actual month of birth of the child, and the precipitation shocks are measured in the past 6 months as opposed to the past year. This is to control for the possibility of seasonal births or a "wet" and "dry" season, which we find no evidence for. Note there are no statistically significant effects in any specification.

Table A1 – The effects of year of birth precipitation on maternal characteristics at the time of birth of the child

	VARIABLES	Years of schooling	Parental age at child birth	Parental weight	Years living in locality	Month of birth
pre birth	< -30%	1.147	1.986	0.272	0.110	0.111
		(2.015)	(2.153)	(0.399)	(0.560)	(0.096)
	-30% to -10%	0.322	1.408	0.115	0.180	0.009
		(1.348)	(1.303)	(0.241)	(0.339)	(0.058)
	10% to 30%	0.253	2.212	0.144	0.453	0.096
		(0.1753)	(1.562)	(0.289)	(0.406)	(0.696)
	> 30%	0.147	2.304	0.164	0.608	-0.201
		(0.659)	(2.435)	(0.451)	(0.634)	(0.186)
	Treatment	0.042	0.077	0.037	0.098	-0.064
		(0.536)	(0.348)	(0.229)	(0.393)	(0.816)
post birth	< -30%	0.435	-7.431	-0.819	-2.179	-0.244
		(0.5514)	(7.324)	(0.970)	(3.466)	(0.5946)
	-30% to -10%	-0.351	-4.796	-0.955	-1.355	0.155
		(2.126)	(4.943)	(0.917)	(1.286)	(0.2206)
	10% to 30%	-0.473	-5.420	-0.878	-1.897	-0.005
		(2.648)	(5.794)	(0.971)	(1.503)	(0.258)
	> 30%	0.045	-3.442	-0.750	-0.825	-0.039
		(0.729)	(3.437)	(2.602)	(0.950)	(0.6262)
	Treatment	0.012	0.056	0.022	0.083	0.044
		(0.554)	(0.438)	(0.112)	(0.443)	(0.116)
	individuals	76,880	78,234	77,978	78,231	82,844

Standard errors, clustered by locality, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2- Migration Robustness Check

The following table reports the results of the migration robustness check. The specification is identical to equation 1, without controlling for lagged schooling. A strong positive shock seems to make a member returning less likely, but we would expect one of the variables to be significant by chance.

Table A2 – The effects of precipitation shocks on migration patterns during the PROGRESA program

	VARIABLES	household missing	household returns	member missing	member returns
not treated	< -30%	0.006	-0.000	0.004	-0.002
		(0.005)	(0.005)	(0.005)	(0.004)
	-30% to -10%	0.007	0.007	0.008	0.003
		(0.009)	(0.009)	(0.006)	(0.002)
	10% to 30%	-0.004	-0.011	-0.003	-0.000
		(0.003)	(0.021)	(0.003)	(0.002)
	> 30%	-0.000	0.000	-0.004	**-0.004****
		(0.003)	(0.003)	(0.003)	(0.002)
treated	< -30%	0.007	0.009	0.007	0.006
		(0.005)	(0.008)	(0.005)	(0.004)
	-30% to -10%	0.001	0.001	0.002	0.000
		(0.001)	(0.001)	(0.003)	(0.001)
	10% to 30%	0.001	0.005	-0.002	0.002
		(0.001)	(0.012)	(0.001)	(0.001)
	> 30%	-0.002	-0.003	0.001	0.004
		(0.001)	(0.018)	(0.001)	(0.003)
	treatment	-0.000	-0.008	-0.002	-0.008
		(0.002)	(0.010)	(0.002)	(0.007)

Standard errors, clustered by locality, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A4 – Importance of including a lag of school attendance in the specification

The following regression compares two specifications for the labor participation portion of my analysis. The first column is the results from equation 3, the coefficient estimates reported in the results section. The second column is a 2SLS estimation of equation 3 without lagged schooling, and its instruments, as a dependent variable. This mimics the specification used in de Janvry *et al*. (2006). Note that the 2SLS approach uses less information in the instrument set than the Arellano Bond estimation, which can use all lags available to it as instruments. To ensure that the differences in estimation are not due to a different sample, I restrict the sample to only those observations used in the original weighted 2SLS estimation. The dependent variable is again, a binary indicator of participation in the labor force.

The magnitude, direction, and significance of these variables are very different across the two specifications, indicating that the inclusion of some lagged schooling as a predictor is important. Compare the differences between these two specifications with the differences between the 2SLS estimates and the Arellano-Bond estimations in table A6 and A7. The estimates are very similar.

Table A4 – A comparison of specifications for predicting labor participation for Males, aged 8-18, one including a two year lag of school attendance, and one including no lagged school attendance.

	VARIABLES	Included lag Weighted 2SLS	No lag Weighted 2SLS
not treated	< -30%	**-0.033***** (0.010)	0.003 (0.021)
	-30% to -10%	0.0056 (0.004)	**0.071***** (0.006)
	10% to 30%	-0.006 (0.007)	0.008 (0.006)
	> 30%	**0.0293***** (0.007)	**-0.022***** (0.007)
treated	< -30%	0.002 (0.001)	**0.026**** (0.013)
	-30% to -10%	0.0030 (0.003)	**0.020*** (0.012)
	10% to 30%	**-0.012***** (0.002)	0.012 (0.014)
	> 30%	**-0.039***** (0.003)	**0.020***** (0.006)
	Lagged Schooling	**-0.177***** (0.001)	
	individuals	24,906	24,906

Standard errors, clustered by locality, in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Regressions include but table does not report: treatment, aged fixed effects, locality FE, level of schooling FE, control shocks.

Table A5 – Age fixed effects for attendance and labor regressions

Below are the coefficients on the age fixed effects for the school attendance and labor participation regressions. Note that school attendance peaks around 13 for boys and 12 for girls, and labor participation grows with age. This is consistent with other investigations of the effects of age within PROGRESA.

Table A5 – Coefficients on the age fixed effects for the school and labor regressions, from equations 2 and 3.

	School Attendance		Labor Participation	
Age	Male	Female	Male	Female
8	(omitted)	(omitted)	(omitted)	(omitted)
9	0.0512***	0.0602***	0.0022	.0022
	(.00102)	(.00119)	(0.0214)	(0.0019)
10	0.0805***	0.0902***	0.0142***	0.0023
	(.0199)	(.0234)	(0.0053)	(0.109)
11	0.0976***	0.125***	0.0216**	0.0006
	(.0202)	(.0322)	(0.0172)	(0.122)
12	0.109***	0.120***	0.0256***	0.0106***
	(.0133)	(.0303)	(0.0023)	(0.030)
13	0.137***	0.123***	0.0223**	0.0134***
	(.0404)	(.0341)	(0.0201)	(0.040)
14	0.0953***	0.152***	0.0578***	0.0352***
	(.0395)	(.0252)	(0.0025)	(0.082)
15	0.0532***	0.0641***	0.0944**	.0682***
	(1.50e-05)	(0.014)	(0.0399)	(0.0298)
16	-0.0306***	-0.0282***	0.166***	0.1321**
	(0.0100)	(0.008)	(0.0408)	(0.0432)
17	-0.142***	-0.121***	0.295***	0.236***
	(.0397)	(.0222)	(0.0299)	(.0682)
18	-0.252***	-0.193***	0.423***	.3182***
	(.0602)	(.0602)	(0.0402)	(.0411)

Clustered Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Tables A6 and A7 - Comparisons between Arellano–Bond, weighted and unweighted 2SLS

The following tables replicate the analysis under several different specifications as a robustness check. Additional specifications include unweighted 2SLS and Arellano-Bond specifications. The unweighted specification has the same functional form as the weighted equation. Arellano-Bond estimation is a two-stage regression technique designed to address the first order autocorrelation in predictors, in this case a one year lag of school attendance. It requires instruments for the endogenous dependent variable. The excluded instruments include all of the explanatory variables in equations 2 and 3, as well as additional lags of treatment, schooling, precipitation categories and other lagged shocks. Arellano – Bond estimation uses the lags of treatment and shocks from t-1 and earlier, and school attendance from t-2 and earlier, as instruments for the change in schooling between t-2 and t-1. This means that only the first period is omitted due to differencing, but for later periods more lags than one can be used as instruments. However, to make sure that differences in results are not driven by a different sample, I estimate the Arellano Bond specification on the same sample as the 2SLS estimations.

Note that the estimated effects are largest in the weighted 2SLS regressions, suggesting that the non-weighted specifications underestimate the effects of both rainfall shocks and PROGRESA. This might be due to the fact that the matched control group was substantially better off than either the original or matched control group: by weighting, we reduce the econometric consequences of this difference.

Not reported in any of these tables are the effects of the other shocks, the age, round, and schooling fixed effects.

Table A6 – The effects of precipitation shocks and PROGRESA treatment on change in school attendance for males and females age 8 to 18, across Arellano Bond, 2SLS, and weighted 2SLS specifications

	Specification	Arellano Bond		2 Stage least squares		2SLS weighted	
	VARIABLES	Males	Female	Males	Females	Males	Females
Precip shocks, not treated	< -30%	**-0.023****	-0.014	**-0.023*****	-0.015	**-0.037*****	-0.016
		(0.010)	(0.011)	(0.006)	(0.010)	(0.008)	(0.007)
	-30% to -10%	**-0.016****	0.000	**-0.017*****	0.000	**-0.027*****	0.001
		(0.008)	(0.006)	(0.006)	(0.005)	(0.006)	(0.005)
	10% to 30%	0.011	0.001	0.011	0.001	0.012	0.001
		(0.007)	(0.007)	(0.006)	(0.004)	(0.006)	(0.006)
	> 30%	**-0.025*****	**-0.015****	**-0.028*****	**-0.019*****	**-0.033*****	**-0.028*****
		(0.007)	(0.007)	(0.006)	(0.006)	(0.005)	(0.006)
Precip shocks, treated	< -30%	**0.052*****	-0.015	**0.058*****	-0.018	**0.066*****	-0.017
		(0.003)	(0.011)	(0.002)	(0.008)	(0.002)	(0.007)
	-30% to -10%	**0.025*****	-0.007	**0.026*****	-0.008	**0.038*****	-0.007
		(0.003)	(0.005)	(0.002)	(0.003)	(0.002)	(0.004)
	10% to 30%	0.003	0.001	0.003	0.001	0.003	0.001
		(0.011)	(0.003)	(0.009)	(0.002)	(0.009)	(0.002)
	> 30%	-0.003	**-0.014****	-0.004	**-0.017*****	-0.003	**-0.022*****
		(0.005)	(0.006)	(0.003)	(0.004)	(0.004)	(0.005)
	treatment	**0.020*****	**0.024*****	**0.021*****	**0.027*****	**0.029*****	**0.038*****
		(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.004)
	observations	98,244	98,922	98,244	98,922	98,244	98,922
	individuals	24,302	25,116	24,302	25,116	24,302	25,116

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parenthesis. These are clustered by locality in both of the 2 stage regression, and are robust standard errors in the Arellano bond specification.

Not reported: age, round and schooling level fixed effects, other shocks

Table A7 - Effects of precipitation shocks on child labor for males and females aged 8 to 18 across two stage least squares, weighted 2SLS, and Arellano bond specifications.

	Specification	Two stage least squares		2SLS Weighted		Arellano Bond	
	VARIABLES	Males	Females	Males	Females	Males	Females
Precip shocks not treated	< -30%	**-0.0299***** (0.008)	**0.018***** (0.007)	**-0.025***** (0.013)	**0.0172***** (0.008)	**-0.033***** (0.010)	**0.0223***** (0.006)
	-30% to -10%	**0.006** (0.004)	-0.005 (0.003)	0.0051 (0.006)	-0.005 (0.004)	0.0056 (0.004)	-0.005 (0.003)
	10% to 30%	-0.006 (0.005)	0.002 (0.004)	-0.006 (0.008)	0.002 (0.005)	-0.006 (0.007)	0.001 (0.003)
	> 30%	**0.022***** (0.005)	**-0.020***** (0.005)	**0.0204***** (0.008)	**-0.019***** (0.006)	**0.0293***** (0.007)	**-0.029***** (0.004)
Precip shocks treated	< -30%	0.002 (0.008)	0.021*** (0.007)	0.002 (0.001)	**0.0185***** (0.008)	0.002 (0.001)	**0.027***** (0.007)
	-30% to -10%	**0.003** (0.003)	0.004 (0.002)	0.0031 (0.005)	0.0040 (0.003)	0.0030 (0.003)	0.0042 (0.002)
	10% to 30%	-0.009*** (0.003)	-0.007 (0.005)	**-0.008***** (0.004)	-0.007 (0.006)	**-0.012***** (0.002)	-0.008 (0.005)
	> 30%	-0.039*** (0.003)	**-0.023***** (0.003)	**-0.034***** (0.005)	**-0.022***** (0.004)	**-0.039***** (0.003)	**-0.031***** (0.003)
	treatment	**-0.067***** (0.006)	0.008 (0.005)	**-0.062***** (-0.01)	0.0070 (-0.00)	**-0.070***** (0.006)	0.0077 (0.005)
	Observations	91,122	90,001	91,122	90,001	102,659	99,519
	Individuals	22,102	20,477	22,102	20,477	22,906	21,839

*** p<0.01, ** p<0.05, * p<0.1. Standard errors in parenthesis. These are clustered by locality in both of the 2 stage regression, and are robust standard errors in the Arellano bond specification. Not reported: age, round and schooling level fixed effects, other shocks

Tables A8 and A9 – Unweighted estimations of the early life specifications

The following tables give the results of the OLS estimation of the early life regressions, without reweighting. Note that the estimates are generally closer to zero, suggesting that this functional form underestimates the true effects of PROGRESA. Despite this, the estimates are remarkably consistent, varying only 5-10% across specifications. Most of these estimations are individually statistically insignificantly different from one another, but collectively they are larger with a p value of much less than one percent.

This result is important for two reasons. First, it shows these results are robust across specifications. Second, it shows the utility of reweighting the data. This technique provides a way to compare two samples that might be different on average, but contain a large set of similar individuals. By giving the individuals who are similar more weight than the others, I enhance my ability to measure the effects of PROGRESA. This technique has been used many times in the literature, see for example Davis (2010) and DiNardo (2002).

Table A8 - The effects of Precipitation shocks in the year after birth on later life outcomes, without weighting.

		Height		Weight		Words	
	VARIABLES	Male	Female	Male	Female	Males	Females
Precip shocks not treated	< -30%	-1.231***	-1.266***	-0.4***	-0.358**	-11.673**	-14.396**
		0.584	0.581	0.156	0.156	6.296	7.036
	-30% to -10%	0.084	0.064	0.082	0.01	3.393	0.05
		0.36	0.369	0.158	0.162	4.787	4.234
	10% to 30%	-0.767	0.159	0.018	-0.106	7.526	8.187
		0.606	1.87	0.179	0.178	12.833	6.742
	> 30%	1.536	-0.849**	0.804	0.179	19.489	-21.418
		1.321	0.405	0.58	0.825	17.254	25.511
Precip shocks treated	< -30%	2.044**	1.711	0.413	-0.096	13.509	-29.579
		1.203	1.393	0.528	0.617	21.676	22.78
	-30% to -10%	0.379	0.156	0.29	-0.088	7.072	-3.758
		0.606	0.57	0.265	0.252	7.846	8.68
	10% to 30%	0.151	-0.259	-0.452	-0.233	10.55	2.321
		0.665	0.603	0.293	0.266	10.576	10.054
	> 30%	1.488	-1.354	1.205	-0.808	32.132	27.366
		2.1	1.833	0.922	0.81	26.119	36.335
	Treatment in the year after birth	0.474***	0.245***	0.174**	-0.081	-0.773	10.996**
		0.180	0.117	0.82	0.237	5.93	5.557
	Years treated After age 1	0.323*	0.221	0.125*	-0.043	0.4	-0.783
		0.215	0.309	0.094	0.097	2.593	2.994
	Observations	2,500	2,504	2,506	2,513	733	709
	R-squared	0.603	0.600	0.427	0.415	0.276	0.306
	Ages of children	0 to 6	0 to 6	0 to 6	0 to 6	0 to 2	0 to 2

Robust standard errors (clustered by community) are below the estimates
*** p<0.01, ** p<0.05, * p<0.1
Not reported: locality fixed effects, locality time trends, year of birth

Table A9 - The effects of Precipitation shocks in the year before birth on later life outcomes, without weighting.

		Height		Weight		Words	
	VARIABLES	Male	Female	Male	Female	Males	Females
Precip shocks not treated	< -30%	**0.852***	0.068	0.17	0.258	3.939	1.218
		0.503	0.5	0.221	0.22	11.358	8.169
	-30% to -10%	0.134	0.239	0.061	0.195	0.624	1.121
		0.333	0.319	0.146	0.141	5.173	5.041
	10% to 30%	0.619	0.74	0.305	0.253	8.79	5.705
		0.501	0.46	0.22	0.202	7.681	7.82
	> 30%	0.232	1.729	0.027	0.703	1.242	8.956
		1.466	1.52	0.644	0.671	16.096	35.567
Precip shocks treated	< -30%	**0.889***	0.469	0.299	0.396	4.545	2.523
		0.585	0.967	0.389	0.423	12.547	12.361
	-30% to -10%	0.41	1.092	0.161	0.325	6.281	1.582
		0.667	0.855	0.291	0.288	4.913	4.938
	10% to 30%	0.484	0.602	0.213	0.263	9.321	2.738
		0.6	0.657	0.263	0.29	10.43	8.499
	> 30%	0.322	1.225	0.513	0.222	24.735	3.302
		1.466	1.42	0.463	0.299	27.23	30.635
	Treatment in the year before birth	0.656	0.526	0.034	**0.381**	1.88	0.373
		0.475	0.479	0.208	0.211	5.032	5.161
	Years treated After age 1	**0.323***	0.221	**0.125***	-0.043	0.4	-0.783
		0.215	0.309	0.094	0.097	2.593	2.994
	Observations	2,500	2,504	2,506	2,513	733	709
	R-squared	0.603	0.600	0.427	0.415	0.276	0.306
	Ages of children	0 to 6	0 to 6	0 to 6	0 to 6	0 to 2	0 to 2

Robust standard errors (clustered by community) re below the estimates
*** p<0.01, ** p<0.05, * p<0.1
Not reported: locality fixed effects, locality time trends, year of birth

Table A10 – The effects of Precipitation shocks on later life outcomes

I report the results of rainfall categories in the year of birth on adult outcomes. The specification is similar to equation 4 without any treatment variables included, since all of the people in this specification were too old to have been treated in the year of birth. Not reported are the year before birth estimates (there were no statistically significant effects) age dummies, locality dummies, and locality time trends.

Table A10 – The effects of rainfall categories in the year of birth on adult outcomes, ages 22-32

Specification	Height		Weight	
VARIABLES	Men	Women	Men	Women
< -30%	-1.179**	-0.981*	-0.411***	-0.356***
	(0.576)	(0.585)	(0.150)	(0.162)
-30% to -10%	0.0611	0.0510	0.0638	0.0079
	(0.351)	(0.399)	(0.154)	(0.167)
10% to 30%	-0.681	0.1207	0.0181	-0.096
	(0.556)	(1.767)	(0.188)	(0.184)
> 30%	1.2135	0.4077	0.7077	0.1660
	(1.317)	(0.415)	(0.589)	(0.787)
Observations	5,127	5,362	5,056	5,275

Robust standard errors (clustered by community) re below the estimates
*** p<0.01, ** p<0.05, * p<0.1
Not reported: locality fixed effects, locality time trends, year of birth, year before birth estimations

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