Can conditional cash transfers serve as safety nets
to keep children at school and out of the labor market?

Alain de Janvry, Frederico Finan, and Elisabeth Sadoulet,
University of California at Berkeley,
and
Renos Vakis,
The World Bank
November 2004

Abstract

Conditional cash transfer (CCT) programs for education are known to be effective in increasing educational achievements among the rural poor. Using panel data from the Progresa experience with randomized treatment, we show that there is strong state dependence in school attendance. Short term shocks that take children out of school will consequently have long term consequences on their educational achievements. We show that idiosyncratic and covariate shocks do indeed push parents to take children out of school and to use child labor as risk coping instruments. However, CCT help protect children from these shocks, creating an additional benefit from these programs as effective safety nets with long term benefits.

I. School dropping out and child labor as elements of risk coping strategies

Poor people in rural communities tend to be exposed to a broad array of shocks. The unemployment or illness of an adult member of the household can imply loss of income. Illness of any family member requires unexpected health expenditures. Natural shocks such as droughts, floods, hurricanes, plagues, and earthquakes affect incomes from natural resources, either directly for the self-employed, or indirectly for workers in the fields of others and income earners in activities linked to agriculture. Responses to shocks to protect family consumption consist in a wide range of creative coping strategies including drawing down of liquid assets held by the household, use of credit, and risk pooling in informal insurance arrangements. Children can also be used as risk-coping instruments. When households have difficulties in sustaining consumption, children can be taken out of school and/or sent to work until the shock has been absorbed. Children can enter the labor market, work in home-based enterprises, or substitute for parent's time by doing household chores. The problem, however, is that children who leave school temporarily may be less likely to subsequently return to school. When this is the case, temporary shocks that induce parents to take their children out of school may have permanent effects on the children's human capital development and future earnings.

Conditional cash transfer (CCT) programs such as Progresa in Mexico, Bolsa Escola in Brazil, and many others around the world (Morley and Coady, 2003) have been used to induce poor parents to send their children to school and care more for their health. These programs have been shown to be effective in raising school achievements (Schultz, 2004) and improving health conditions (Gertler, 2004). However, this may happen not only because the CCT lowers the price of schooling, inducing a corresponding quantity response, but also because it prevents parents from responding to shocks through taking kids out of school as they would lose the transfer when it is most needed. This risk coping value of CCT programs, which has yet to be explored, is what we address in this paper.

We examine whether or not shocks adversely affect child schooling and increase child labor, and to what extent CCT programs can help mitigate these effects. Specifically, we analyze the effects of shocks on education and child labor outcomes using data from the evaluation component of the Progresa program. Our empirical analysis is divided into three parts. In the first part, we characterize the prevalence of shocks, the low and irregular attendance to school, and the importance of child and teenage labor. Data show that these phenomena are all very important in the poor rural communities observed. A very high percentage of households are affected by unemployment, illness, and natural shocks. A high percentage of children tend to come in and out of school, expectedly in response to shocks. And there is a high prevalence of work, often temporary, among children who have not graduated from junior high school.

In the second part, we add to past analyses of Progresa's impact on schooling and child labor by introducing state dependence in the enrollment decision, and by extending the analysis to periods when the control group was incorporated into the program. State dependence shows that children finishing their primary school are on average over the next three years 15% more likely to enroll when they are currently enrolled. For children already enrolled in secondary school, the likelihood of continued enrollment is 30% higher than for those who are not. Conversely, state dependence also means that children who fail to enroll in one semester are less likely to be subsequently enrolled, implying that the short run response to a shock via taking a child out of school will have long term consequences for the child's educational attainment. Although we cannot model state dependence for the decision to work due to insufficient data, we find that Progresa had a significant impact on child labor decisions: for boys ages 12-14, Progresa reduced the incidence of work by 21 percent. Another addition to past analyses of Progresa is that we evaluate the impact of the program using the 2000 data, after control households had become incorporated into the program. We find that girls that were deciding to enter or not into secondary school when the Progresa program started in November 1998 continue to enroll 11 percentage points more for the 2000/2001 school year than those from the control villages who became treated in that year. With baseline enrollment of 0.76, this represents an increase of 15 percent. In terms of child labor, we find that the impact of Progresa in 2000 is comparable with its impact in previous years. This suggests that children that did not go to school or went to work because they did not benefit from transfers in earlier years are difficult to recuperate in later years, evidencing again the existence of long term effects of short term decisions.

In the third part, we look at the effects of shocks on schooling and child labor decisions, and at the mitigating effect that Progresa transfers may have on how parents respond to shocks by taking children out of school or sending them to work. Results show that many shocks are important in pushing children out of school. This is particularly the case for household head unemployment and illness, and for natural disasters that hit the locality. Progresa does, however, largely or fully compensate for these shocks in keeping children in school. Evidence is not as strong for child labor, but several categories of children (12 to 14 years old girls, children of farm workers) respond to household shocks by working more, especially when the shock is due to head of household unemployment. Progresa also helps prevent these children from working more as elements of risk coping strategies.

CCT are thus seen to be effective in keeping children at school when their families are hurt by different kinds of shocks, both idiosyncratic and covariate. The policy implication of the results is that extending eligibility for CCT programs to households affected by observable shocks could be used to protect school age children from dropping out of school and joining the labor force. This would be a novel use of these programs as safety nets, giving them considerable social value additional to what has proven to be a successful approach for enhancing human capital formation among the children of the poor.

II. Exposure to shocks, dropping out of school, and child labor in recent studies

There is a well established conventional wisdom linking child labor to poverty. According to this view, child labor is associated with an income constraint on parents, not to their preference for child work. Basu and Van (1998) conceptualized this relation as the "luxury axiom" (see also López-Calva, 2001). Rising parents' income would allow them not to send their children to the labor market. Without this income, parents use child labor to tradeoff higher current income against lower future child income as it reduces children's human capital development, and sometimes compromises their future health as well. Poverty is, however, not sufficient for this relation to hold. It has to be associated with non-positive bequests and financial market imperfections that prevent parents from trading-off old-age income with current resources, leading them to produce too much child labor relative to the first best optimum that would hold with positive bequests or perfect financial markets (Baland and Robinson, 2000).

Developing financial institutions to remedy this liquidity constraint is, however, unlikely to be sufficient. Financial institutions will not provide the necessary long term credit for primary or secondary education as parents lack a commitment device that child education will pay for itself. The South African pension system, by injecting anticipated liquidity into poor households, has been shown to help increase children's schooling (Edmonds, 2004). CCT programs like Progresa can also serve this purpose. Because income effects are weak (including the "wealth paradox" according to which the children of households with productive assets may work more and study less than the children of less wealthy households), impact achieved on school enrollment is much greater by tying transfers to conditions on school assistance and health visits, transforming the transfer from an income into a price effect. By targeting transfers on children at risk of not meeting the condition without a transfer, CCT can be quite efficient in improving school achievements among the poor (Sadoulet and de Janvry, 2004).

In recent years, another determinant of erratic school attendance and of child labor has been analyzed: taking children out of school to reduce costs and using child labor as risk coping instruments when other instruments are insufficient to shelter consumption from income shocks. Using the ICRISAT

India panel data for rural households, Jacoby and Skoufias (1997) show how unanticipated income shocks and financial market failures result in an increase in child labor and a decline in school attendance. Child labor in turn leads to lower educational attainments, and hence to lower future child productivity. Short term self-insurance via taking children out of school and child labor is thus obtained at the cost of lower future income growth. They also show that the income shocks that result in lower school attendance are covariate (as opposed to idiosyncratic) and un-anticipated (as opposed to anticipated) shocks.

The Jacoby and Skoufias paper has been followed by several empirical studies measuring the impact of uninsured shocks and credit market failures on child labor and schooling. Duryea et al. (2003) show how in Brazil male household head unemployment increases child labor and decreases school advancement, particularly for 16 years old girls, thus reducing their future welfare. Guarcello et al. (2003) not only observe a similar response for households in Guatemala, but also point out that child labor creates state dependence in that children that are sent to work are subsequently less likely to return to school. They show that parent's access to credit and to medical insurance provide risk coping instruments that protect children from dropping out of school. Parker and Skoufias (2000) show that, in urban Mexico, idiosyncratic shocks such as parents' unemployment and divorce have no impact on boys' schooling, but reduce school attendance and school attainment among girls, creating long term effects on their human capital. Jensen (2000) and Beegle et al. (2003) look at agricultural shocks in Côte d'Ivoire and Tanzania, respectively. They show that these shocks increase child labor and reduce school attainment. Access to credit in Tanzania protects children from these shocks and keeps them at school. Economic crises have also been shown to lead to declines in school enrollment, especially among the poor and younger children. This has been evidenced by Funkhouser (1999) in response to the debt crisis in Costa Rica, by Thomas at al. (2003) in response to the financial crisis in Indonesia, and by Rucci (2003) in response to the Argentine economic crisis.

We show in this paper that CCT programs like Progresa are effective in sheltering recipient children from being taken out of school in response to shocks. Beneficiaries remain at school when there are idiosyncratic (unemployment and illnesses) and covariate (natural disasters) shocks. Girls and children of farm workers that receive cash transfers are also less likely to be sent to work when the household head is affected by an unemployment shock. This suggests that CCT programs can be used as safety nets in protecting investments in children's human capital from short run uninsured shocks. We discuss how these safety nets could be put into place in response to both idiosyncratic and covariate shocks.

III. Theoretical model of school enrollment choice

Adapting a model proposed by Hyslop (1999) that represents labor market participation decisions when there are search costs, we develop a simple dynamic model of school enrollment decision under uncertainty in which re-entry to school after a discontinuity requires additional effort and cost on the part of the student. This model generates an enrollment decision that depends on the past enrollment state.

Consider a household with a single child, with period utility *u* a function of consumption C_t, the binary enrollment status S_t of the child, and household characteristics Z_t. With a rate of time preference ρ, the discounted value of expected utility over an infinite time horizon is written:

(1) $$U_t = \sum_{s=0}^{\infty} \frac{1}{(1+\rho)^s} E_t u(C_{t+s}, S_{t+s}, Z_{t+s}) .$$

In addition to its contribution to current utility, schooling contributes to the accumulation of human capital H_t. We assume human capital to be a function of accumulated schooling, with return to schooling decreasing and falling asymptotically to zero, so that H_t is bounded:

(2) $$H_t = g\left(\sum_{\tau=1}^{t-1} S_\tau\right).$$

The wage that the child is able to secure on the labor market is assumed proportional to his human capital, $w_t H_t$.

A key assumption of the model is that re-entry to school after a discontinuity is more difficult than just continuing school. Difficulties are of many types. The utility for going to school may be lower when the child remains behind his cohort of classmates, the child has learned to appreciate other ways of life or lost studying skills, he may have forgotten the specific material that is taught in school, etc. In this simple model, we summarize all of these aspects in an additional cost c_t of schooling. Assuming that there is neither saving nor borrowing, the period *t* budget constraint of the household is written as:

(3) $$C_t + w_t H_t S_t + c_t\left(1 - S_{t-1}\right) S_t = y_t + w_t H_t \bar{L} ,$$

where $\bar{L} = 1$ is total time available for work and school (school time is set to unity), and y_t the autonomous income in the household.

The household's optimal choice of schooling and consumption is the solution to the maximization of (1) under the contemporary budget constraint (3). Assuming that y_t, w_t, and c_t are iid random variables, the corresponding value function is stationary. Given the state variables H_t and S_{t-1} at the beginning of period *t* and the observed values for Z_t, y_t, w_t, and c_t, the value function is:

$$V\left(H_t, S_{t-1}\right) = \max_{S_t}\left[u^{S_{t-1}S_t} + \frac{1}{1+\rho} E_t V\left(H_{t+1}, S_t\right)\right],$$

where $u^{S_{t-1}S_t} = u\left(y_t + w_t H_t - w_t H_t S_t - c_t\left(1 - S_{t-1}\right) S_t, S_t, Z_t\right)$ is the current period utility given the past and current periods' schooling.

Since schooling S_t is a binary variable, the maximization problem consists in choosing the maximum of two values:

(4) $$V\left(H_t, S_{t-1}\right) = \max\left[u^{S_{t-1}0} + \frac{1}{1+\rho} E_t V\left(H_t, 0\right), u^{S_{t-1}1} + \frac{1}{1+\rho} E_t V\left(H_{t+1}, 1\right)\right],$$

with $H_{t+1} = g\left(g^{-1}\left(H_t\right)+1\right)$.

Consider first the case where the child was not enrolled in the previous period, $S_{t-1} = 0$. From (4), the threshold wage w_{0t}^* that keeps the child indifferent between enrolling and not enrolling is the solution to:

(5) $$u^{00} - u^{01} = \frac{1}{1+\rho}\left(E_t V\left(H_{t+1}, 1\right) - E_t V\left(H_t, 0\right)\right).$$

The child does not enroll in school if the LHS expression is larger than the RHS expression, and enrolls if it is smaller. The LHS is unambiguously increasing in wage since:

(6) $$u_1^{00} - u_1^{01} = u_1\left(y_t + w_t H_t, 0, Z_t\right) \geq 0,$$

where u_1 represents the partial derivative of *u* with respect to consumption. The child thus enrolls in school only for wage offers that are below his threshold wage, $w_t < w_{0t}^*$. School enrollment is deterred by high opportunity costs.

Next, consider the decision to enroll in school given that the child had been enrolled in the previous period, $S_{t-1} = 1$. As before, we define the threshold wage w_{1t}^* at which this child is indifferent between continuing school and dropping out as the solution to:

(7) $$u^{10} - u^{11} = \frac{1}{1+\rho}\left(E_t V\left(H_{t+1}, 1\right) - E_t V\left(H_t, 0\right)\right).$$

As the LHS expression is increasing in wage, the child enrolls in school if $w_t \leq w_{1t}^*$

Note that the RHS of equations (5) and (7) are identical. This is because the re-entry cost only has a short-term effect on utility but does not affect the future beyond its influence on current enrollment and thus human capital. Therefore, w_{0t}^* and w_{1t}^* are related by the expression $u^{00} - u^{10} = u^{01} - u^{11}$, or:

$$u\left(y_t + w_{0t}^* H_t, 0, Z_t\right) - u\left(y_t + w_{1t}^* H_t, 0, Z_t\right) = u\left(y_t - c_t, 1, Z_t\right) - u\left(y_t, 1, Z_t\right).$$

A linear approximation to the left and right-hand sides of this equation around $y_t + w_{1t}^* H_t$ and y_t, respectively, gives:

$$u_1\left(y_t + w_{1t}^* H_t, 0, Z_t\right)\left(w_{0t}^* - w_{1t}^*\right) H_t \cong -c_t u_1\left(y_t, 1, Z_t\right),$$

further implying,

$$w_{1t}^* \cong w_{0t}^* + c_t \frac{u_1\left(y_t, 1, Z_t\right)}{u_1\left(y_t + w_{1t}^* H_t, 0, Z_t\right) H_t}.$$

Therefore, conditional on current human capital level H_t and all current and expected future realizations of the exogenous variables $\left(Z_t, y_t, w_t, c_t\right)$, the school enrollment decision can be characterized by:

(8) $$S_t = 1\left(w_t \quad w^*_{0t} + \gamma S_{t-1}\right),$$

where $\gamma = c_t \dfrac{u_1\left(y_t, 1, Z_t\right)}{u_1\left(y_t + w^*_{1t} H_t, 0, Z_t\right) H_t}$ is the positive state dependence effect. The state of current enrollment is thus function of the state of last period's enrollment and of the current level of human capital.

IV. Progresa and the data

To analyze the role of shocks on school and child labor decisions, we use the data set collected for the evaluation of Progresa, a CCT program in rural Mexico. Progresa was introduced in 1997 to offer cash transfers to poor mothers in marginal rural communities, conditional on their children using health facilities on a regular basis and attending school between third grade of primary and third grade of secondary. Children cannot miss more than three days of school per month without losing the transfer, and will not receive the transfer if they have not visited a health center. The Program was recently renamed Oportunidades, and expanded to sixth grade of secondary and to peri-urban areas. In 2003, it serviced 4 million families at an annual cost of US$2.2 billion. The payment schedule is tailored to grade and gender, with primary school children receiving, in 1998, from $70/year in 3rd grade to $135 in 6th grade, and secondary school children receiving from $200/year for boys in first grade and $210 for girls, to $220 for boys in third grade and $255 for girls.

The data consist of a census of households in 506 rural localities, with information in November 1997, and then every 6 months until November 2000[1]. Of these 506 localities, almost two thirds were randomly chosen to be incorporated in the CCT program in May 1998, while the others were kept as control localities until early 2000. Since only households classified as poor according to a constructed welfare index are eligible for the CCT program, we restrict our analysis to the children of poor households.

We are interested in the school and labor choices of children 8 to 17 years old at any point in time during the period of analysis. Our total sample thus consists in the 52,719 poor children that were 5 to 17 years old in November 1997. Although there are many missing values in the database, the school enrollment status is recorded in each of the 7 rounds. The work status in the week prior to the survey for children at least 8 years old is recorded in 6 of the rounds (the question was not included in the March 1998 round).

[1] The seven rounds of survey took place in November 1997, March and November 1998, May and November 1999, and May and November 2000. Transfers were in place by the time of the November 1998 round. The control villages had become incorporated into the treatment by the time of the May and November 2000 rounds.

V. Empirical evidence on shocks, attendance to school, and child labor

5.1. Prevalence of shocks

Exposure to shocks is very high among the rural poor. Table 1 reports the prevalence of different types of shocks at the household and community levels. We consider three types of idiosyncratic shocks at the household level: unemployment of the head of household, illness of the head of household, and illness of the younger siblings. The first two shocks are causes for income loss. The two illness shocks are potential causes for special expenses or need of help at home to take care of the sick. Information on the employment status of the head of household is not observed in round 2 (March 1998), and illness shocks are not reported in either rounds 3 or 7 (November 1998 and 2000). The frequencies reported in Table 1 show a high exposure to risk. Almost one in every four households has experienced unemployment of its head at least once over the six rounds of observation, and about 10% have experienced unemployment more than once. Almost one household in five has experienced illness of its head at least once in 5 rounds of observation. An even more frequent but probably less severe shock is the illness of younger siblings.

Information on climatic shocks was collected in rounds 3 to 6. Each household was asked whether it had experienced certain shocks (drought, earthquake, hurricane, flood, or plague), and whether it had either lost its land, its harvest, or an animal as a consequence of these climatic events. Table 1 reports these individual observations. There is a clear distinction between the very frequent shock of a drought which affect 60% of the households at least once over the course of these two years (and more than 25% of the households more than once), and the low frequency shocks (earthquake, hurricane, flood, or plague), although they still affect around 10% of the households over the four rounds. Regrouping the low frequency shocks under the collective name of natural disaster, prevalence is high with 25% of the households reporting having experienced a natural disaster at least once over 4 rounds. Since these shocks are really community level, we construct and use in the analysis a measure of intensity of two community shocks (drought and natural disaster) using the percentage of households in the community that declare having been affected by any of them in any specific round. The average intensity of these shocks in each round is 24% for drought and 7% for natural disaster. The idiosyncratic loss of a harvest follows closely the drought shocks.

While climatic shocks are clearly exogenous to a specific household, this is not necessarily the case for employment and health shocks, or even to a certain extent for loss of land, harvest, or animal, since these are partly determined by household behavior. In addition, by imposing regular health checkups as conditionality for transfers, Progresa may decrease the prevalence of illness shocks. We do observe a lower health shock frequency in the Progresa than in the non-Progresa villages. For unemployment, there could also be some effect of the Progresa program as it injects large amounts of resources in the communities, although confirming causality would require a more detailed study. On the other hand, drought just happens to have been 10% less frequent in the Progresa villages despite randomization of program placement, but frequency of natural disasters is not different across the two types of villages. In the econometric analysis that follows, we will argue that using child fixed effects controls for problems associated with the potential endogeneity of these shocks.

5.2. Low and irregular school attendance

A serious educational problem in rural Mexico that prompted creation of the Progresa program is low enrollment rates among school age children. Table 2 reports the percent of children not attending school by age. Focusing first on control villages, we see that, most 8 years old children are enrolled in school in fall semesters. However, 5% of the 11 years old are not attending school at each beginning of school year. These non-enrollment rates rise dramatically to 14%, 29% and 41% for the 12, 13, and 14 years old, with an additional 2–3% in spring semesters. The effect of the Progresa program is seen in the decline, but far from elimination, of these non-enrollment percentages starting in the 1998 school year in the treatment villages (November 1998), and in November 2000 in the control villages.

A related issue that can be observed with the panel data is high irregularity in school attendance, meaning children that interrupt their schooling for one or more semesters in the course of their education. Table 3 reports on this phenomenon. We qualify as transition into school the observation of a child enrolled in school, while the previous non-missing information was non-enrollment. And, symmetrically, we qualify as transition out of school observations of non-enrollment after observing enrollment. Column 1 reports on all 52,719 children in the database, column two on children without missing information in the middle of the sequence of 7 semesters, and columns 3-9 only on those children with complete school information over the seven semesters.[2] This second sub-sample includes children that either became too old during the survey period (above 16 or 18 depending on the rounds) to be asked about their schooling, or young children that had missing information before entering school for the first time. The striking number is the 8–11% of children that experience at least two transitions into or out of school. This corresponds to students that either drop out of school for a period but re-enter afterwards, or reciprocally children that go to school for a period but drop out again, and all this within a period of only seven semesters. There is no obvious contrast between boys and girls (columns 4 and 5), but there are very sharp differences between the younger and older children (columns 6 and 7). Children that were already more than 12 years old in 1997, not only quit school in large numbers (36%) during the period of observation but also experienced very large instability, with 19.5% of them moving in or out of school at least twice, and 6.8% at least three times. Comparisons of columns 8 and 9 shows that Progresa is effective in reducing both the drop-out rate and irregularity in school enrollment.

It is very likely that these interruptions have dear consequences on school achievements, with children lagging in age behind their cohort being a strong correlate of low performance and high

[2] Noting school participation by 0 (out), 1(in), or . (information is missing), examples of complete sequences are [1110111] for a child that temporarily dropped out of school in Spring 99 or [0011111] for a child that entered school in Fall 98. Examples of sequences without missing intermediate information are [..10111] for a child with no information in the year 97-98, or [1100…] for a child with no information from Fall 99 on. Finally, an example of sequence with missing intermediate information is [011..111].

probability of definitively dropping out of school. Establishing causality between instability and performance will, however, require proper control for selection effects.

Table 4 reports on the reasons given by survey respondents (usually mothers) for a child to drop out of school. We distinguish between children that we know return to school (as observed later in the data) and those that we don't observe coming back to school. This last group includes those that drop out of school indefinitely and those with truncated information that would eventually return, but after November 2000. Financial reasons or need for the child at work or at home, account for 50 to 60% of the responses, with numbers increasing with age of the child and higher among those that don't return to school. The distance to school is almost strictly related to entry into junior high school (all villages have their own primary school). A striking result is the high percentage of children of all ages that quit school simply because they don't like it or feel they don't learn anything. While this could simply represent a self-selected group of children that for idiosyncratic reasons do not perform well in school, this disturbingly high number likely reflects a serious problem with school quality. Surprisingly, this reason is also given by many children that will eventually overcome their dislike and return to school. Splitting the sample between Progresa beneficiaries and non-beneficiaries, shows (data not reported) that it is not because of Progresa that these children eventually return to school despite their reservations.

5.3. Evidence on child and teenage labor

A similar analysis of the work pattern of children indicates large numbers working at least intermittently during the period of observation. Work here is defined as engaging into productive activities, including wage work, unpaid work outside of home, and work in the family business or farm, in the week preceding the survey, and is recorded for all children 8 years of age and older in six of the seven rounds (there is no information in round 2). We, however, do not know the number of hours of work and hence cannot distinguish between part-time and full-time work. As seen in Table 5, and considering only children that have not yet graduated from 9^{th} grade, the percentage of children that declare working at least once over the 6 rounds increases with their age, from 11% for those 8-11 years old during the period of observation to 25% for the 11–14 years old, and to 51% for the 13-16 years old. More than half of these working children work intermittently, i.e., have at least 2 transitions into or out of work (e.g., (17+8.3)/39.8 = 63.6% for the 12-15 years old), except for the older group, and 10 to 18% of them experience at least 3 transitions. This high frequency of intermittent child labor suggests that their work may be used as a mechanism to cope with shocks or temporary needs.

One should not consider work as necessarily incompatible with school, especially in environments where the school day is short. However, only 2 to 3% of the children in fact do both (Table 6). The most surprising number here, again, is the large percentage of children that neither go to school nor work. At age 12, roughly the time of entry into secondary school, 10% neither work nor study, and this percentage rises to 31% by the age of 15.

VI. The econometric model

The empirical model we use is a reduced form specification of equation (8). Although the theoretical model was derived assuming that each child had one unit of time that he could spend either at work or at school, in reality, as seen above, many children are neither at school nor at work, while a few both attend school and work. We consequently estimate separately the decisions to enroll in school and to work using this framework.

Following the model developed in Section III, current school enrollment and child labor decisions depend on last period choices, conditional on human capital and on current and expected future values of the exogenous variables. The variables of interest are the Progresa treatment, shocks, and human capital relevant for schooling and work which includes both completed grade and age. We use child fixed effects to allow for unobserved time-invariant heterogeneity.

The empirical model corresponding to equation (8) can consequently be written as:

$$(9) \qquad y_{it} = \quad y_{it-1} + \alpha s_{it} + \beta s_{it} T + \delta_t T + \theta_t + \mu_i + \eta H_{it} + \varepsilon_{it} \,, \quad i = 1,\dots,N; t = 2,\dots,7 \,,$$

where y_{it} is a binary variable representing school or work participation for child *i* in period *t*, γ is the state dependence parameter, s_{it} represent shocks, *T* is an indicator for the treatment (Progresa) villages, δ_t the impact of the treatment in round *t*, θ_t a survey round fixed effect, μ_i a child fixed effect representing time invariant heterogeneity, H_{it} the child's human capital, and ε_{it} a time variant heterogeneity term. Because the treatment assignment was randomized, *T* is truly exogenous and orthogonal to ε_{it}. The mitigating effect of Progresa on shocks is captured by the interactive term $s_{it}T$.

With first round parameters normalized to 0, the estimation provides treatment effects δ_t relative to November 1997. The treatment effects are thus identified by double difference between treatment and control villages, before and after treatment. Since both rounds 1 and 2 are pre-treatment observations, we expect to find no treatment effect in round 2, and effective treatment afterwards. Recall also that the control villages were brought into the program in January 2000, so that one needs to be cautious when interpreting the "treatment" effect in rounds 6 and 7. The school and work participation decisions are estimated with the same model, although round 2 is missing for work participation.

While shocks could be correlated with unobserved characteristics of the child -- as there is likely correlation between the household head's average level of unemployment/illness and the schooling of children -- we assume that, conditional on child fixed effects, idiosyncratic shocks are truly exogenous. As for Progresa, the random assignment in 1997 insures that it is orthogonal to children's characteristics in 1997.

There remains, however, a problem with human capital as the current completed grade is an endogenous variable that is partly the result of the Progresa treatment. We consequently cannot include the observed completed grade in the model as it would capture an important part of the treatment effect. Only the grade in 1997 is orthogonal to the treatment, and it is absorbed in the fixed effect. In order to control for the changing human capital over time and its influence on the schooling/working decision, we divide the sample in cohorts of equal initial levels of human capital. For the schooling decision, we define cohorts by the completed grade in Fall 1997, which we consider even more important than age. We thus split the sample between children with less than grade 5 ("primary school" children), exactly grade 5 (and therefore most likely having to take the decision to enter secondary school in the Fall 1998 when Progresa came in), and more than grade 5 ("secondary school" children, meaning having already decided by the Fall 1998 whether they want to pursue secondary school or not). The underlying assumption is that the effect of grade progression on the schooling decisions over time is relatively homogenous within these cohorts. For the work decision, we consider that, at this low level of education, age is a more important determinant of human capital than education. This is in line with the low marginal returns to education for all grades below nine found in de Janvry et al. (2001). We thus define age cohorts, regrouping children of less than 12 years old, 12-14 years old, and more than 14 years old in Fall 1997.

We use a linear probability model for two reasons. First, despite the emerging literature on estimating dynamic binary probit or logit response models (Hyslop, 1999; Chay and Hyslop, 2000) linear probability models are far more tractable and more flexible in the handling of unobserved heterogeneity (Hyslop, 1999).[3] The second, and more substantive, reason is that, with fixed effect probit and logit, all observations of children that are either continuously in school or continuously out of school drop out of the sample. While this selection would pose no problem to identify the effect of shocks, it does for the impact of Progresa. This is because Progresa itself affects schooling, and thus increases the occurrence of complete schooling sequences and decreases the occurrence of complete out of school sequences. This selection would thus induce a downward bias in the measurement of the Progresa effect.

The dynamic model used for estimating schooling decisions thus becomes:

$$(10) \qquad y_{it} = \gamma y_{it-1} + \alpha s_{it} + \beta s_{it} T + \delta_t T + {}_t + \mu_i + \varepsilon_{it}\,,\ i = 1,\ldots,N; t = 2,\ldots,7$$

The value of the state dependence parameter γ carries information on the long-term effect of any variation in the current determinants of participation. If the endogenous variable were continuous, a one-time incorporation in the treatment in period t would generate a contemporaneous effect of δ_t and persistent effects of $\gamma\delta_t, \gamma^2\delta_t, \ldots$ over the following years, cumulating into a long run effect of $\dfrac{\delta_t}{1-\gamma}$. With a binary endogenous variable, whereby the treatment shock may induce y_{it} to switch from 0 to 1, small differences in one year may have long lasting effects on participation decisions.

Following Arellano-Bond, equation (10) is estimated by first differencing to eliminate the heterogeneity parameters μ_i. With shocks observed only in rounds 3 to 6, we simplify the treatment effect to an average treatment effect over the four rounds, which is eliminated by first differencing. We thus estimate the following model:

[3] There are also a few papers that estimate structural dynamic models of school and work decisions, where unobserved heterogeneity is captured by parameters characterizing a discrete number of types (see Eckstein and Wolpin (1999) and Canals-Cerdá and Ridao-Cano (2004)).

(11) $\Delta y_{it} = \gamma \Delta y_{it-1} + \alpha \Delta s_{it} + \beta \Delta s_{it} T + \Delta \theta_t + \Delta \varepsilon_{it}$, $i = 1, \ldots, N; t = 4, \ldots, 6$.

The parameters of interest in this equation are the instantaneous effect α of a shock s_{it} on enrollment probability and the mitigating effect β of the treatment. With a lagged variable and first differencing, we loose two rounds. As a result, only treatment effects for rounds 3 to 7, relative to round 2, can be identified. First differencing also creates a correlation between Δy_{it-1} and the error term $\Delta \varepsilon_{it}$. To address this problem, the Arellano-Bond estimator uses the lagged endogenous variables dated up to $t - 2$, $y_{i1}, \ldots, y_{it-2}$, as instruments for Δy_{it-1}.

While at this point we only estimate the average effect of state dependency on enrollment decisions, there expectedly exist significant sources of heterogeneity in the state dependence parameter. A "diploma" effect creates incentives to finish school cycles. One would thus expect state dependence to be stronger between grades within the same cycle and lower at the end of primary school and of secondary school. A second source of heterogeneity in the state dependence effect is an "end of grade" effect. Quitting school in the middle of a school year wastes the benefit of the whole school year. Entering school in the second semester is impossible. For this reason, children are more likely to finish a school year and to change their participation between two school years.

With missing information in round 2, the panel is too short for estimating the state dependence model for work participation. To estimate the effect of shocks on the work decision, we consequently resort to a special case of (10) without state dependence and with an average treatment effect:

(12) $y_{it} = \alpha s_{it} + \beta s_{it} T + \delta T + \theta_t + \mu_i + \varepsilon_{it}$, $i = 1, \ldots, N; t = 3, \ldots, 6$.

Note that, as there are no pre-treatment observations, β is identified in both estimations of equations (11) and (12) by simple difference between the effect of shocks in the treatment and control villages.

VII. Impact of Progresa on schooling and child labor

Before analyzing the impact of shocks on schooling and child labor, we estimate the simple impact of Progresa on children's schooling and work decisions. For both schooling and work, we measure the impact of the Progresa treatment using a static linear probability model with unobserved child heterogeneity in:

(13) $y_{it} = \delta_t T + \quad_t + \mu_i + \varepsilon_{it}$, $i = 1, \ldots, N; t = 1, \ldots, 7$.

We then extend this analysis to a dynamic model in order to account for the role of state dependence on the schooling decision by estimating:

(14) $\Delta y_{it} = \quad \Delta y_{it-1} + \Delta \delta_t T + \Delta \quad_t + \Delta \varepsilon_{it}$, $i = 1, \ldots, N; t = 3, \ldots, 7$.

7.1. Impact of Progresa on schooling

Table 7 reports the impact of Progresa on the decision to enroll in school for various children cohorts using equation (13). We compare enrollment rates among eligible households from treatment and control villages before and after the start of program to identify the program's impact. Justification for the counterfactual assumption underlying the difference-in-difference model stems from the randomization of villages into treatment and control. Table 7 presents Progresa's impact for each of the 6 rounds, with the November 1997 baseline census representing the excluded round. Implementation of the program starts in May of 1998. Hence, round 3 (November 1998) is the first year of treatment for the purpose of schooling decisions. The experimental design of the program ends in January of 2000 with inclusion of the control villages. There is, however, speculation that the control villages might have known that they be would included as early as November of 1999, thus potentially affecting school enrollment decisions before inclusion in the program. Columns 5 and 6 present the program's impact on the boys and girls who had completed 5th grade in 1997, and hence were ready to decide whether to continue in secondary school in Fall 1998 when Progresa started. Columns 3 and 4 report the estimates for the sample of boys and girls who had attained at least grade 5 in 1997, and columns 1 and 2 estimate the impact for children who had completed no higher than grade 4 in 1997.

Focusing on columns 5 and 6, results show that the impact of Progresa is higher for girls than for boys. This is consistent with both the design of the program, as it provides higher grants to girls than to boys, and previous evaluations of the programs (Skoufias and Parker, 2001; Schultz, 2004). An interesting point, however, and a contribution to the literature, is Progresa's impact on school enrollment in May and November of 2000, well after the control villages had been incorporated into the program. Compared to the control villages, girls in the Progresa villages enroll 11 percentage points more for the 2000/2001 school year (November 2000 treatment). With baseline enrollment of 0.76, this represents an increase of 15 percent. For boys, the November 2000 impact is positive, but attenuated and imprecisely measured. Note that despite the lack of a control group by 2000, we are still capturing the proper treatment effect. Indeed, many children of this age cohort from the control villages will have been out of school for two years, making it difficult for them to return to school.

The remaining columns report the effects of the program for secondary school and primary school children. Overall, the impacts are positive but smaller, an indication that the decision to enroll into secondary school is the biggest hurdle and the grade at which Progresa has its greatest effect. The March 1998 impact observed for secondary school boys could be due to announcement of the program, inducing future beneficiaries not to drop out of school in the second semester, in anticipation of transfers the following school year.

With the same sample specifications, Table 8 estimates a linear probability model that, in addition to controlling for unobserved time-invariant factors, allows for the possibility of state dependence in the enrollment decision (equation (14)). To account for endogeneity concerns imposed by a lagged dependent variable, we apply the Arellano-Bond estimator, which instruments lagged schooling with the enrollment history. As the lagged specification makes us lose one year of pre-treatment data, interpretation of the treatment parameters is relative to the spring semester of the pre-treatment year, March 1998. Moreover, these coefficients are not the marginal effects of the program because Progresa feeds back on enrollment through the lagged dependent variable.

As Table 8 points out, there is strong state dependence in enrollment decisions. Having been enrolled in the previous semester increases the probability of enrolling in the next period by 16 percentage points for boys who had completed 5^{th} grade in 1997. State dependence is higher in secondary school (30 percentage points for boys) than in primary school (6 percentage points for boys), and higher for secondary school girls (33 percentage points) than for boys. With state dependence, the long term impact of a temporary effect (i.e., one single year of Progresa transfer) would persist the following year for boys at 16% of the value of the short-term effect at entry into secondary school. In this case, the long-term effect of a permanent change (a lasting Progresa transfer) is 18% larger than the short-term effect. The estimated impacts are consistent with those found in the fixed-effects model in Table 7.

7.2. Impact of Progresa on Child Labor

Table 9 considers the effects of Progresa on child labor in a static model. The sample consists of children at least 8 years old at any point during the observation period, and each specification controls for child fixed-effects. The dependent variable in this linear probability model takes on a value of one if the child worked in the previous week. We do not distinguish between part-time and full-time work. Distinguishing between boys and girls, columns 1 and 2 correspond to children younger than 11 years old in November 1997, columns 3 and 4 to ages 12-14, and columns 5 and 6 to ages 15-17. Identification of the program's impact is again based on a difference-in-difference model, and the impact is reported for each of 5 rounds, with November 1997 as the pre-treatment reference round. Unfortunately, with no information on child labor in the March 1998 survey, we cannot estimate the state dependence model for work decisions.

Progresa's most dramatic impact on child labor occurs among children who were 12-14 years old in 1997. Focusing on column 3, we see that Progresa reduces the probability that boys work by 6.5 percentage points on average. This impact, which represents a 21 percent decrease in the incidence of child labor, is consistent across all rounds, even after elimination of the control group in 2000. Progresa also has a larger absolute impact on boys than on girls (4.6 percentage points), which is not surprising given that girls work less. However, the relative impact on girls is larger, reducing labor by 50%.

These coefficients, while consistent with those reported by Skoufias and Parker (2001), are estimated to be slightly higher. Our analysis differs from their study in three respects. First, we control for child fixed-effects. Second, and more substantive, we estimate the effects of Progresa for each round, as opposed to each year. Finally, we also estimate the impact on the 2000 round, after the control group had already been incorporated.

VIII. Impact of shocks on schooling and child labor, and the mitigating effect of Progresa

8.1. The effects of shocks on school and Progresa's ability to mitigate them

We now add shocks and interactions of shocks with the Progresa treatment effect in the school enrollment equation. Note that we only have information on shocks in rounds 3 to 6. In addition the Arellano-Bond estimator requires differencing. Hence, results reported in Table 10 correspond to an estimated relationship between current enrollment and lagged enrollment, shocks, and mitigation by Progresa for rounds 4 to 6 (equation (11)). There is no pre-intervention observation among these rounds, and therefore the Progresa mitigating effect is identified by the simple difference in the effect of shocks between the treatment and control villages. This is sufficient given the random assignment of the program. Table 10 reports the impact of individual shocks (columns 1 to 6) and then of all shocks jointly in column 7. Column 8 reports an estimation of the model with child fixed effects and no state dependence (equation (12)).

Considering shocks one at a time, we see that an unemployment or illness shock for the head of household reduces the probability of enrollment of the child by an average 1.7–1.8 percentage points, but that Progresa almost completely (unemployment) or fully (illness) mitigates these negative effects. Illness of the younger siblings has no aggregate effect on schooling of the children in the family. Interestingly, despite its very damaging effect on income, drought has no measurable effect on schooling. This result is robust to various econometric specifications and sub-samples of children. A possible explanation for this result is that droughts are sufficiently frequent in Mexico that households have designed ex-ante risk coping strategies to account for these occurrences.[4] By contrast, natural disasters have a dramatic effect on schooling. A disaster that affects the whole community reduces enrollment by 3.2 percentage points, but this effect is completely mitigated by Progresa. The household's experience of a loss of land, harvest, or animal, has a smaller effect (0.5 percentage point) on schooling and it is completely mitigated by Progresa.

When all the shocks are considered together, we loose some precision in the estimation.[5] Column 7 shows that the two main shocks that affect schooling are unemployment of the head of household and natural disaster in the locality. While Progresa completely mitigates the natural disaster effect, it only partially compensates for the unemployment shock. Column 8 of Table 10 shows that results are similar with a fixed-effect linear probability model.

Table 11 analyzes the heterogeneity of effects of shocks on different sub-groups of children. Primary school children, boys, and children of agricultural workers are more affected by the unemployment shock of the head of household than secondary school children, girls, and children of non-agricultural workers, respectively, and are only partially protected by Progresa. The effect of natural disaster is severe on all categories of children, but particularly on secondary school children, girls, indigenous, and children of agricultural workers. For all categories of children, Progresa completely erases the negative effects of the natural disaster on schooling.

---

[4] Reardon et al. (1988) find a similar result for Burkina Faso.

[5] Correlations are 0.19 between head of household unemployment and illness, 0.11 between head of household and siblings illness, and 0.16 between drought and natural disaster.

In Table 12, we focus on secondary school children as this is the group for which there is a critical link between shocks and school. For many of them, going to school is costly because there is no secondary school in their village. In addition, their opportunity cost on the labor market is the highest among Progresa beneficiaries. Results show that the schooling of boys is affected by illness of the household head, that of girls by natural disasters in the locality, and that of the children of agricultural workers by both. They are, however, all completely protected from shocks by Progresa transfers.

Note that a temporary disaster has both an immediate effect in taking some children out of school, and a long-term impact through the state dependence effect. Even when the shock does not last over the next period, an effect equal to 17% of the initial short-term effect remains in the following semester (Table 10, column 7). This state dependence effect is highly robust across shocks. A natural disaster thus reduces the probability of enrollment by 5.1 percentage points immediately and by 0.9 percentage points the following semester. Given the frequency of such events, as seen in Table 1, all of these shocks, each with its long term effect cumulated over several years, can indeed seriously compromise the schooling of children when they are not protected. Table 11 shows that the state dependence is almost twice as large for girls than for boys, implying that any temporary event that takes a girl out of school has a more lasting effect. Conversely, on the positive side, any event that induces a girl to stay in school, such as receiving a Progresa transfer, has more lasting impact as well. Children of agricultural workers have a lower state dependence than other children, suggesting that their school attendance is more volatile than that of other children.

8.2. The effects of shocks on child and teenage labor and Progresa's ability to mitigate them

Estimations of the effects of shocks on child and teenage labor are reported in Table 13. As discussed above, with no information on child labor in round 2, we do not have enough data points to estimate the state dependence model. We thus report results from a fixed-effect model over four rounds of observations, from November 1998 to May 2000. The Progresa effect is here again identified by simple difference between control and treatment villages.

We do not expect to find a symmetrical effect of Progresa in mitigating the effect of shocks on school and child labor. This is because Progresa is a "price" subsidy to school, and not an income transfer. Hence, stepping out of school immediately induces a loss of the corresponding transfer, which is certainly the last thing a household would want to do in case of an income shortfall, while entering the labor market for part-time work does not preclude receiving the Progresa transfer. Hence, Progresa would mitigate entry in the labor market only through its income effect that reduces the need for additional income from child work, or through the difficulty of combining work and school.

Results show that a household head unemployment shock does not induce children to work. However, others shocks do. Child labor increases in response to illness of the household head, illness among young siblings, and more severe natural disasters in the locality. Progresa is, however, unable to prevent these child labor responses to shocks. There are two cases where Progresa mitigates the effect of shocks: impacts of droughts and losses as a consequence of natural disasters. In both cases, the shocks reduce opportunities for children to work. Progresa compensates for these effects by helping maintain children working. If the income effect of Progresa (either through other siblings or through general equilibrium effects in the community) helps reduce loss of land, harvests, and animals (as seen for crops in Table 1), this helps keep children at work.

Focusing on children 12 to 14 years old in Table 14, we observe that girls and especially children of agricultural workers dramatically increase their participation to the labor market when the head of household is hit by unemployment. In both cases, Progresa completely protects them from the shocks. The mitigating function of CCT programs in protecting child labor from being used as a risk coping instrument is thus verified in these two cases.

IX. Conclusions and policy implications

Using panel data for villages from the Mexican Progresa program with randomized treatment, we have shown that shocks are highly prevalent, that many children have irregular periods of school enrollment, and that child labor is very frequent. We extended the impact analysis of the conditional cash transfers to show that there is strong state dependence in the enrollment decision. Children taken out of school are less likely to subsequently return, implying long-term consequences from short term decisions. By observing control villages after they became incorporated in the treatment, we see that children that did not benefit from transfers during the experiment are harder to bring back to school, implying as well that short-term actions are difficult to reverse.

Shocks have strong effects in taking children out of school. This applies to unemployment of the household head, illness of the household head, and natural disasters in the community. In poor rural communities, children are indeed used as risk coping instruments in responding to these shocks. Strong state dependence implies that short run consumption smoothing gains for the household result in long term losses in human capital for children. The Progresa transfers, however, largely or completely compensate for these shocks. CCT thus have an important safety net role to play, protecting child education from a range of idiosyncratic and covariate shocks. Shocks also induce children to work, particularly girls and children of farm workers when their parents are affected by unemployment. Progresa transfers also fully shelter them from being sent to work. The conditionality on school attendance is thus effective in preventing use of their time as a risk coping instrument.

The Progresa experience shows that beneficiaries of CCT are effectively protected from the risk of shocks that induce them to take their children out of school. This result suggests another potential use of CCT programs. For non-beneficiaries, inclusion could automatically follow covariate shocks since these are easily verifiable.[6] In this case, all members of poor communities would be offered the CCT for the

---

[6] Extending the analysis to the 25% of non-poor households above the poverty line, we see that they differentially send their children more to school than the poor, but that their ability to protect the schooling of their children from shocks is no better than that of the poor. This would justify using a poverty threshold

duration of the shock. Idiosyncratic shocks are also easily verifiable through community participation, even if after some delay. In this case, a household that declares a shock would automatically be immediately included in the program for one semester to maintain children at school and thus avoid irreversibilities. Community verification would then be used to decide on subsequent permanence in the program for as long as the idiosyncratic shock is effective and the child at risk of being taken out of school. CCT programs could thus acquire an additional dimension relative to the ones they currently have: serve as flexible safety nets to prevent short run shocks from having long term consequences on the human capital formation of children when their parents lack access to other risk coping instruments.

References

Baland, Jean-Marie, and James Robinson. 2000. "Is Child Labor Inefficient?" *Journal of Political Economy* 108(4): 663-679.

Basu, Kaushik, and Phan HoangVan. 1998. "The Economics of Child Labor". *American Economic Review* 88(3): 412-27.

Beegle, Kathleen, Rajeev Dehejia, and Roberta Gatti. 2003. "Child Labor, Income Shocks, and Access to Credit". World Bank Policy Research Working Paper 3075.

Canals-Cerdá, José, and Cristóbal Ridao-Cano. 2004. "The Dynamics of School and Work in Rural Bangladesh." World Bank Policy Research Working Papers 3330.

Chay, Kenneth, and Dean R. Hyslop. 1999. "Identification and Estimation of Dynamic Binary Response Panel Data Models: Empirical Evidence Using Alternative Approaches". Economics Department, University of California at Berkeley, http://emlab.Berkeley.EDU/users/kenchay/index.shtml.

de Janvry, Alain, Frederico Finan, and Elisabeth Sadoulet. 2001. "How Effective are Educational Subsidies Programs for the Rural Poor? Progresa in Mexico". ARE: University of California at Berkeley.

Duryea, Suzanne, David Lam, and Deborah Levison. 2003. "Effects of Economic Shocks on Children's Employment and Schooling in Brazil". University of Michigan: Population Studies Center.

Eckstein, Zvi, and Kenneth Wolpin. 1999. "Why Youths Drop Out of High School: The Impact of Preferences, Opportunities, and Abilities". *Econometrica* 67(6):1295-1339.

Edmonds, Eric. 2004. "Does Illiquidity Alter Child Labor and Schooling Decisions? Evidence from Household Responses to Anticipated Cash Transfers in South Africa". Department of Economics, Dartmouth College.

Funkhouser, Edward. 1999. "Cyclical Economic Conditions and School Attendance in Costa Rica". *Economics of Education Review* 18(1): 31-50.

---

which is higher for inclusion in the CCT program in response to shocks than the threshold used for normal-times inclusion.

Gertler, Paul. 2004. "Do Conditional Cash Transfers Improve Child Health? Evidence from Progresa's Control Randomized Experiment". *The American Economic Review: Papers and Proceedings* 94(2): 336-342.

Guarcello, L., F. Mealli, and F. Rosati. 2003. "Household Vulnerability and Child Labor: The Effects of Shocks, Credit Rationing, and Insurance". *Understanding Children's Work, An Inter-Agency Research Cooperation Project* (ILO, UNICEF, World Bank Group).

Hyslop, Dean R. 1999. "State Dependence, Serial Correlation, and Heterogeneity in Intertemporal Labor Force Participation of Married Women". *Econometrica* 67(6):1255-1294.

Jacoby, Hanan, and Emmanuel Skoufias. 1997. "Risk, Financial Markets, and Human Capital in a Developing Country". *Review of Economic Studies* 64(3): 311-335.

Jensen, Robert. 2000. "Agricultural Volatility and Investments in Children". *American Economic Review* 90(May): 399-404.

López-Calva, Luis Felipe. 2001. "Child Labor: Myths, Theories, and Facts". *Journal of International Affairs* 55(1): 1083-1119.

Morley, Samuel, and David Coady. 2003. *From Social Assistance to Social Development: Targeted Education Subsidies in Developing Countries*. Washington D.C.: International Food Policy Research Institute.

Parker, Susan, and Emmanuel Skoufias. 2000. "Parental Unemployment, Marital Status Changes, and Child Human Capital: Evidence from Urban Mexico". Washington D.C.: International Food Policy Research Institute.

Reardon, Thomas, Peter Matlon, and Christopher Delgado. 1988. "Coping with Household-level Food Insecurity in Drought-affected Areas of Burkina Faso". *World Development* 16(9): 1065-74.

Rucci, Graciana. 2003. "Macro Shocks and Schooling Decisions: The Case of Argentina". Economics Department, University of California at Los Angeles. http://econweb.sscnet.ucla.edu/placement/2003/Rucci.pdf

Sadoulet, Elisabeth, and Alain de Janvry. 2004. "Making Conditional Cash Transfer Programs More Efficient". ARE: University of California at Berkeley.

Schultz, T. Paul. 2004. "School Subsidies for the Poor: Evaluating the Mexican Progresa Poverty Program". *Journal of Development Economics* 74(1): 199-250.

Skoufias, Emmanuel, and Susan Parker. 2001. "Conditional Cash Transfers and their Impact on Child Work and School Enrollment: Evidence from the Progresa Program in Mexico". Economia 2(1): 45-96.

Thomas, Duncan, Kathleen Beegle , Elizabeth Frankenberg, Bondan Sikoki , John Strauss, and Graciela Teruel. 2003. "Education in a Crisis." *Journal of Development Economics* 74(1): 53-85

Table 1. High prevalence of shocks

	Progresa villages	Control villages	Test of difference
Number of households	6,764	4,091	
Percentage of household having experienced a shock:			
Head of household unemployed at least once in 6 rounds	22.5	24.2	–
More than once	9.7	11.9	– –
Head of household ill at least once in 5 rounds	17.2	20.3	–
More than once	2.9	3.6	–
Children 0-5 years old ill at least once in 5 rounds	42.7	44.5	
More than once	24.3	25.7	
Drought at least once in 4 rounds	59.3	61.9	– –
More than once	25.5	28.6	– –
Harvest lost at least once in 4 rounds	58.6	61.7	– –
More than once	26.9	30.0	– –
Low frequency shock at least once in 4 rounds			
Earthquake	9.3	8.0	+
Hurricane	8.0	9.2	–
Flood	11.5	11.6	
Plague	1.5	1.2	
Natural disaster (earthquake, hurricane, flood, or plague)	25.7	24.7	
Community shocks intensity (percentage of households reporting the shock, average per round)			
Drought	22.6	25.2	– –
Natural disaster (earthquake, hurricane, flood, or plague)	6.9	6.7	

Shocks significantly higher/lower in Progresa villages at 5% (+/–), 1%(++/– –).

Head of household employment observed in 6 rounds (not March 98), head of household illness in 5 rounds (Nov-98 to Nov-00), drought, harvest loss, and natural disasters in 4 rounds (Nov-98 to May-00).

Table 2. School non-attendance rate by age

Age in fall semester	Percent of children not enrolled							Average over Novembers
	Nov-97	Mar-98	Nov-98	May-99	Nov-99	May-00	Nov-00	
Children from control villages								97-98-99
8	1.0	2.5	0.9	3.0	2.3	5.2	0.6	1.4
9	1.5	2.3	1.4	3.2	2.6	4.5	1.1	1.8
10	2.8	2.7	2.2	4.6	3.6	5.9	1.4	2.9
11	4.9	4.8	4.2	7.1	5.8	7.1	3.1	5.0
12	13.7	15.9	14.4	16.8	14.1	17.2	9.3	14.1
13	29.4	27.5	24.2	26.5	25.6	29.3	18.4	26.4
14	41.2	37.2	38.3	39.0	34.3	36.5	31.7	37.9
15	58.8	55.7	55.5	52.7	56.2	59.3	51.1	56.8
Number of observations	10,402	8,400	9,962	9,255	9,783	9,245	9,100	30,147
Children from Progresa villages								98-99-00
8	1.2	2.3	1.1	2.4	1.4	3.6	0.6	1.0
9	1.6	2.4	1.2	1.8	1.5	3.7	0.8	1.2
10	2.9	2.5	1.2	2.7	1.9	4.3	1.4	1.5
11	4.7	5.5	3.2	4.9	3.4	5.4	2.0	2.9
12	15.4	14.6	9.0	11.7	9.3	12.6	8.4	8.9
13	25.4	25.4	19.0	20.3	17.8	22.6	16.7	17.8
14	39.1	36.1	28.5	29.3	29.3	32.0	25.9	27.9
15	55.3	50.4	47.8	49.2	46.1	48.8	46.8	46.9
Number of observations	16,713	13,226	16,060	14,807	15,188	14,500	14,370	45,618

Excluding observations with missing information on enrollment.
Progresa villages were incorporated in the program in May 1998, and control villages in January 2000.

Table 3. Irregularity in school attendance

	All observations	Children w/o missing intermediate observations	Children with complete schooling data only						
						Age in 1997		Poor	
			All	Boys	Girls	≤ 12 years	> 12 years	Control	Progresa
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Number of observations	52,719	27,002	16,981	8,798	8,178	13,026	3,955	4,475	7,463
No transition into or out of school	74.4	77.6	71.6	71.5	71.7	80.4	42.6	71.6	74.6
Out of school	18.0	13.4	6.7	6.8	6.7	0.7	26.8	5.3	5.2
In school	56.4	64.2	64.9	64.8	65.0	79.8	15.7	66.3	69.4
One transition	17.2	14.3	17.8	18.0	17.6	11.7	38.0	16.8	15.8
Quit school after Nov-97	15.1	12.5	16.4	16.9	16.0	10.6	35.8	15.7	14.1
Enter school after Nov-97	2.2	1.8	1.4	1.2	1.6	1.2	2.1	1.1	1.8
Two transitions or more	8.3	8.0	10.6	10.4	10.7	7.8	19.5	11.6	9.6
Two transitions	6.4	5.8	7.6	7.4	7.8	6.0	12.7	8.6	7.0
Three or more transitions	2.0	2.2	3.0	3.0	2.9	1.8	6.8	3.0	2.6

Sample constituted of all children ages 5 to 16 in November 1997, observed over 7 semesters from November 1997 to November 2000.

Table 4. Reasons for dropping out of school

Age in November	Number of observations	No money Needed at work/home	School too far	Does not like/ learn at school	Too old	Other reasons
				Percentages		
Children that return to school by Nov-00						
8	66	27.3	3.0	19.7	0.0	50.0
9	66	31.8	6.1	31.8	1.5	28.8
10	120	36.7	1.7	32.5	2.5	26.7
11	186	48.9	4.8	29.0	0.0	17.2
12	424	51.7	10.4	26.4	0.7	10.8
13	465	58.5	11.6	27.5	1.5	0.9
14	565	56.3	6.9	25.5	1.6	9.7
15	585	55.9	7.9	23.6	1.9	10.8
Children that do not return to school by Nov-00						
8	0	–	–	–	–	–
9	2	50.0	0.0	50.0	0.0	0.0
10	14	50.0	7.1	14.3	0.0	28.6
11	40	55.0	7.5	30.0	2.5	5.0
12	229	52.8	18.3	22.7	1.7	4.4
13	304	53.3	8.9	29.9	0.3	7.6
14	309	56.0	10.4	23.3	1.0	9.4
15	495	59.6	8.1	22.4	2.0	7.9

Sample of children dropping out of school, in the semester when they leave school.

Table 5. Prevalence of work among children not having graduated from 9th grade

Cohorts: Age over	Number of	Distribution of children by number of rounds in which they work							Percent of children with transitions into/out of work	
1997–2000	children	At least 1	1	2	3	4	5	6	2	3 or more
8–11	3,291	10.6	9.6	0.8	0.2	0.0	0.0	0.0	6.9	0.6
9–12	3,122	13.9	12.1	1.4	0.3	0.0	0.0	0.0	8.0	0.9
10–13	3,366	18.5	14.8	2.6	0.6	0.1	0.0	0.0	9.9	1.5
11–14	3,024	25.4	18.4	5.0	1.6	0.5	0.3	0.0	11.7	3.1
12–15	2,437	39.8	23.0	10.4	5.2	2.6	1.1	0.3	17.0	8.3
13–16	1,912	51.3	24.3	12.9	9.7	5.8	2.9	0.6	21.5	11.2
14–17	1,574	61.5	21.5	13.5	13.3	11.1	6.8	2.0	25.5	14.9
15–18	1,376	72.7	18.2	13.6	13.9	12.6	11.8	5.3	25.3	12.9

Observation in 6 rounds from Fall 1997 to Fall 2000 (Spring 1998 missing).

Table 6. School and work

Age in Fall semester	Number of observations	School only	Work only	School and Work	Neither
		Percentages			
8	17,557	96.5	0.2	1.6	1.7
9	19,053	96.2	0.2	1.8	1.8
10	19,084	95.2	0.3	2.0	2.4
11	18,942	93.1	0.8	2.4	3.7
12	19,062	84.7	2.3	3.0	10.1
13	18,692	74.5	5.5	3.1	16.9
14	17,829	63.5	11.1	3.3	22.2
15	16,453	45.2	20.9	3.1	30.8

Data on 6 rounds from Fall 97 to Fall 00 (Spring 98 missing).

Table 7. The effect of Progresa on schooling - Static model
Dependent variable: child at school

		Cohorts of children					
		Primary School		Secondary School		Entry into Secondary School	
		Boys	Girls	Boys	Girls	Boys	Girls
Rounds		(1)	(2)	(3)	(4)	(5)	(6)
2	March 1998 Treatment	0.006	0.006	0.016	-0.017	0.051	-0.016
		[0.007]	[0.007]	[0.013]	[0.013]	[0.029]+	[0.028]
3	November 1998 Treatment	0.014	0.014	0.049	0.062	0.098	0.115
		[0.006]*	[0.006]*	[0.013]**	[0.012]**	[0.027]**	[0.027]**
4	May 1999 Treatment	0.023	0.026	0.05	0.066	0.114	0.148
		[0.006]**	[0.006]**	[0.013]**	[0.013]**	[0.027]**	[0.027]**
5	November 1999 Treatment	0.029	0.031	0.032	0.061	0.073	0.135
		[0.006]**	[0.006]**	[0.012]**	[0.012]**	[0.027]**	[0.026]**
6	May 2000 Treatment	0.033	0.033	0.039	0.058	0.078	0.112
		[0.006]**	[0.007]**	[0.012]**	[0.012]**	[0.028]**	[0.027]**
7	November 2000 Treatment	0.014	0.015	0.017	0.039	0.029	0.113
		[0.006]*	[0.006]*	[0.012]	[0.012]**	[0.028]	[0.027]**
Observations		56,260	51,508	32,124	30,869	6,911	6,452
Number of children		10,377	9,621	5,378	5,175	1,063	994
Mean value of schooling		0.920	0.926	0.620	0.609	0.739	0.755
R-squared (within)		0.020	0.020	0.130	0.090	0.160	0.160

Robust standard errors in bracket; + significant at 10%; * significant at 5%; ** significant at 1%.
Linear probability model. All models include round and child fixed-effects. Excluded round is November 1997.
Primary, secondary, and entry into secondary school cohorts of children are defined as having graduated from less than, exactly, or at least 5th grade in November 97.

Table 8. The effect of Progresa on schooling - Dynamic model
Dependent variable: child at school

Cohorts of children	Primary School Boys (1)	Primary School Girls (2)	Secondary School Boys (3)	Secondary School Girls (4)	Entry into Secondary School Boys (5)	Entry into Secondary School Girls (6)
Lagged Schooling	0.062	0.078	0.302	0.328	0.155	0.135
	[0.023]**	[0.025]**	[0.025]**	[0.026]**	[0.046]**	[0.052]**
November 1998 Treatment	0.013	0.022	0.029	0.083	0.043	0.146
	[0.005]*	[0.006]**	[0.015]+	[0.016]**	[0.033]	[0.034]**
May 1999 Treatment	0.018	0.028	0.013	0.058	0.050	0.144
	[0.006]**	[0.006]**	[0.014]	[0.014]**	[0.030]+	[0.031]**
November 1999 Treatment	0.025	0.044	-0.003	0.049	0.008	0.128
	[0.007]**	[0.008]**	[0.017]	[0.017]**	[0.033]	[0.034]**
May 2000 Treatment	0.028	0.037	0.012	0.053	0.012	0.094
	[0.008]**	[0.009]**	[0.017]	[0.017]**	[0.035]	[0.035]**
November 2000 Treatment	0.008	0.026	-0.018	0.039	-0.041	0.113
	[0.009]	[0.009]**	[0.018]	[0.018]*	[0.034]	[0.035]**
Observations	31,360	28,623	16,990	16,364	4,369	4,086
Number of children	8,150	7,399	5,048	4,868	1,053	985

Robust standard errors in bracket; + significant at 10%; * significant at 5%; ** significant at 1%.
All models include round and child fixed-effects. Treatment effects are relative to the March 98 pre-treatment round. Linear probability model estimated with the Arellano-Bond estimator.
Primary, secondary, and entry into secondary school children are defined as having graduated from less than, exactly, or at least 5th grade in November 97.

Table 9. The effect of Progresa on child labor - Static model
Dependent variable: child at work

Cohorts	Age ≤11 in Nov-97 Boys (1)	Age ≤11 in Nov-97 Girls (2)	Ages 12-14 in Nov-97 Boys (3)	Ages 12-14 in Nov-97 Girls (4)	Ages 15-17 in Nov-97 Boys (5)	Ages 15-17 in Nov-97 Girls (6)
November 1998 Treatment	-0.017	-0.020	-0.066	-0.041	-0.001	0.028
	[0.008]*	[0.006]**	[0.019]**	[0.014]**	[0.025]	[0.022]
May 1999 Treatment	-0.026	-0.006	-0.063	-0.033	-0.006	0.007
	[0.008]**	[0.006]	[0.020]**	[0.015]*	[0.026]	[0.024]
November 1999 Treatment	-0.030	-0.011	-0.065	-0.063	0.01	-0.006
	[0.008]**	[0.006]*	[0.020]**	[0.015]**	[0.026]	[0.024]
May 2000 Treatment	-0.052	-0.040	-0.073	-0.066	-0.043	-0.004
	[0.008]**	[0.006]**	[0.020]**	[0.015]**	[0.027]	[0.026]
November 2000 Treatment	-0.031	-0.019	-0.058	-0.024	-0.011	-0.002
	[0.008]**	[0.006]**	[0.021]**	[0.016]	[0.029]	[0.027]
Observations	40,742	39,118	18,070	15,716	11,357	10,209
Number of children	9,377	8,977	3,506	3,227	2,491	2,419
Mean value of work	0.046	0.021	0.304	0.091	0.647	0.190
R-squared	0.01	0.01	0.07	0.01	0.06	0.01

Robust standard errors in bracket; + significant at 10%; * significant at 5%; ** significant at 1%.
Linear probability model. All models include round and child fixed-effects. Excluded round is November 1997
Observations on children 8 years and older.

Table 10. Impact of state dependency, shocks, and Progresa on school attendance
Dependent variable: Child at school

	Individual shocks						All shocks	
	AB-FE (1)	AB-FE (2)	AB-FE (3)	AB-FE (4)	AB-FE (5)	AB-FE (6)	AB-FE (7)	FE (8)
State dependency:								
Child at school last semester	0.164	0.168	0.174	0.173	0.171	0.172	0.170	
	[0.023]**	[0.023]**	[0.031]**	[0.023]**	[0.022]**	[0.022]**	[0.032]**	
Head of household unemployed	-0.018						-0.021	-0.017
	[0.009]+						[0.013]+	[0.009]*
* Progresa	0.012						0.008	0.005
	[0.109]						[0.015]	[0.011]
Head of household ill		-0.017					-0.005	-0.002
		[0.008]*					[0.011]	[0.008]
* Progresa		0.020					0.005	-0.009
		[0.010]*					[0.013]	[0.010]
Proportion of children age 0-5 ill			-0.006				-0.002	0.002
			[0.006]				[0.006]	[0.005]
* Progresa			0.001				-0.001	-0.006
			[0.007]				[0.007]	[0.006]
Drought severity in locality[1]				0.001			-0.005	-0.006
				[0.007]			[0.009]	[0.007]
* Progresa				-0.005			-0.004	-0.020
				[0.007]			[0.009]	[0.007]
Natural disaster severity in locality[1]					-0.032		-0.051	-0.052
					[0.009]**		[0.011]**	[0.010]**
* Progresa					0.040		0.053	0.042
					[0.010]**		[0.013]**	[0.013]**
Loss as consequence of natural disaster[2]						-0.005		
						[0.003]		
* Progresa						0.007		
						[0.004]+		
Number of observations	65,716	72,752	45,660	72,264	72,332	72,332	41,938	67,531
Number of children	23,588	24,483	17,014	24,599	24,621	24,621	16,291	24,069

Robust standard errors in bracket; + significant at 10%; * significant at 5%; ** significant at 1%.
All models include round and child fixed-effects. Dynamic model estimated with the Arellano-Bond estimator (AB-FE), static model with a fixed-effect specification (FE).

[1] Proportion of households in the locality reporting having experienced a drought or a natural disaster (earthquake, hurricane, flood, or plague) in the last 6 months.

[2] Loss of land, harvest, or animal. Average occurrence of these shocks are 7%, 25%, and 2% respectively.

Table 11. Heterogeneity in schooling vulnerability to shock
Dependent variable: Child at school

	Primary school[1]	Secondary school[1]	Boys	Girls	Indigenous	Non-indigenous	Children of agricultural worker	Children of non-ag. worker
State dependency:								
Child at school last semester	0.146**	0.304**	0.12**	0.22**	0.20**	0.16**	0.12*	0.64**
Head of household unemployed	-0.034*	0.001	-0.029+	-0.011	-0.026	-0.018	-0.029+	-0.027
* Progresa	0.02	-0.018	0.023	-0.011	0.017	0.003	0.031	0.000
Head of household ill	0.017	-0.041	-0.009	-0.000	0.002	-0.009	-.025+	0.014
* Progresa	-0.019	0.046	0.022	-0.015	-0.005	0.010	.028+	-0.015
Proportion of children age 0-5 years ill	0.002	-0.001	-0.001	-0.004	-0.010	0.002	-0.000	-0.002
* Progresa	-0.002	0.003	0.002	-0.001	-0.004	-0.000	-0.007	0.006
Drought severity in locality	0.004	-0.027	-0.003	-0.008	0.007	-0.013	0.001	-0.010
* Progresa	-0.013	0.017	0.002	-0.011	-0.019	0.007	-0.004	-0.017
Natural disaster severity in locality	-0.033**	-0.058*	-0.033*	-0.073**	-0.056**	-0.036*	-0.057**	-0.041+
* Progresa	0.034**	0.083**	0.034*	0.075**	0.057**	0.043**	0.057**	0.037
Number of observations	29,231	13,209	21,425	20,507	14,718	27,051	28,308	13,630
Number of children	10,788	5,222	8,301	7,987	5,614	10,609	13,466	8,775
Mean value of endogenous variable	0.932	0.682	0.841	0.828	0.858	0.822	0.842	0.819

Robust standard errors in bracket; + significant at 10%; * significant at 5%; ** significant at 1%.
All regressions include round and child fixed-effects. Linear probability model estimated with the Arellano-Bond estimator.
[1] Primary school includes all children having completed less than 5th grade in Fall 1997; secondary school children have completed 5th grade or more in Fall 97.

Table 12. Vulnerability of secondary school children to shocks - selected results
Dependent variable: Child at school

	Boys	Girls	Children of ag. worker
Head of household unemployed	-0.045	-0.052	-0.064
* Progresa	0.025	0.073+	0.016
Head of household ill	-0.059+	-0.018	-0.071*
* Progresa	0.083*	-0.001	0.082*
Natural disaster severity in locality	-0.002	-0.121**	-0.057+
* Progresa	0.003	0.168**	0.087*
Number of observations	6,516	6,689	8,681
Number of children	2,566	2,655	4,227

+ significant at 10% level; * significant at 5% level; ** significant at 1% level.

Arellano-Bond estimator. All models include round and child fixed effects, and the other shocks and interaction terms shock*Progresa, as in Table 11.

Table 13. Impact of shocks on child work and mitigation by Progresa
Dependent variable: Child works

	Individual shocks						All shocks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Head of household unemployed	-0.002						0.008
	[0.008]						[0.011]
* Progresa	-0.016						0.003
	[0.011]						[0.014]
Head of household ill		0.022					0.015
		[0.008]**					[0.010]
* Progresa		0.013					0.025
		[0.010]					[0.013]+
Proportion of children age 0-5 ill			0.023				0.021
			[0.006]**				[0.006]**
* Progresa			-0.005				-0.005
			[0.008]				[0.008]
Drought severity in locality				-0.075			-0.094
				[0.007]**			[0.009]**
* Progresa				0.019			0.028
				[0.007]*			[0.009]**
Natural disaster severity in locality[1]					0.048		0.045
					[0.011]**		[0.014]**
* Progresa					0.023		0.035
					[0.013]+		[0.017]*
Loss as consequence of natural disaster[2]						-0.019	
						[0.004]**	
* Progresa						0.017	
						[0.005]**	
Number of observations	87,631	90,224	59,586	90,276	90,276	90,276	57,798
Number of children	27,678	27,960	21,109	27,969	27,969	27,969	20,814
R-squared	0.01	0.02	0.01	0.02	0.02	0.02	0.02

Robust standard errors in bracket; + significant at 10%; * significant at 5%; ** significant at 1%.
Linear probability model. All equations include round and child fixed effects.
[1] Proportion of households in locality reporting having experienced a drought or a natural disaster (earthquake, hurricane, flood,
[2] Loss of land, harvest, or animal. Average occurrence of these shocks are 8%, 27%, and 2% respectively.

Table 14. Impact of shocks on work and mitigation by Progresa, children 12-14 years in 1997
Dependent variable: Child works

	All	Boys	Girls	Children of agricultural worker	Children of non-ag. worker
Head of household unemployed	0.023	-0.033	0.096	0.514	-0.044
	[0.028]	[0.044]	[0.033]**	[0.238]*	[0.043]
* Progresa	-0.023	0.042	-0.104	-0.580	0.047
	[0.036]	[0.056]	[0.043]*	[0.301]+	[0.057]
Head of household ill	0.008	-0.005	0.018	-0.018	0.013
	[0.027]	[0.042]	[0.031]	[0.040]	[0.047]
* Progresa	0.075	0.121	0.031	0.125	0.043
	[0.035]*	[0.055]*	[0.040]	[0.053]*	[0.064]
Proportion of children age 0-5 ill	0.029	0.020	0.041	0.027	0.063
	[0.017]+	[0.026]	[0.019]*	[0.021]	[0.039]
* Progresa	0.007	0.018	-0.007	0.008	-0.037
	[0.022]	[0.034]	[0.025]	[0.028]	[0.049]
Drought severity in locality[1]	-0.134	-0.143	-0.120	-0.077	-0.199
	[0.024]**	[0.037]**	[0.028]**	[0.031]*	[0.057]**
* Progresa	0.056	0.060	0.050	0.020	0.170
	[0.026]*	[0.040]	[0.030]+	[0.032]	[0.064]**
Natural disaster severity in locality[1]	0.038	0.052	0.026	0.021	0.105
	[0.036]	[0.056]	[0.043]	[0.046]	[0.102]
* Progresa	0.084	0.119	0.039	0.019	0.029
	[0.044]+	[0.068]+	[0.052]	[0.058]	[0.115]
Number of observations	13,340	7,054	6,284	8,403	4,937
Number of children	4,630	2,389	2,240	3,819	2,915
Mean value of endogenous variable	0.191	0.284	0.086	0.173	0.220
R-squared	0.04	0.05	0.03	0.04	0.05

Robust standard errors in bracket; + significant at 10%; * significant at 5%; ** significant at 1%.
Linear probability model. All equations include round and child fixed effects.
[1] Proportion of households in locality reporting having experienced a drought or a natural disaster (earthquake, hurricane, flood, or plague) in last 6 months.