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Household Vulnerability and Child Labor: The Effect of Shocks, Credit Rationing and Insurance

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Abstract

The theoretical literature has pointed at the importance of access to credit market in determining the household decisions concerning children's activities and the reaction of households to adverse shocks. In this paper we address these issues making use of a unique data set for Guatemala that contains information on credit rationing and shocks.

We address the potential endogeneity of the variable of interest using a methodology based on propensity scores and we use sensitivity analysis to assess the robustness of the estimates with respect to unobservables.

The results show the importance of access to credit markets and of shocks in determining children's labor supply.

JEL: D1, O1
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Household Vulnerability and Child Labor: the Effect of Shocks, Credit Rationing and Insurance

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Overview

The relationship between credit markets and child labor has been widely discussed in the literature. It is well known that in the absence of perfect credit markets, investment in human capital may be smaller than optimal. Moreover, capital markets are important to allow households to smooth the effect of shocks. If capital markets were perfect, households could insure themselves against idiosyncratic shocks. Human capital accumulation would then depend only on the relative benefits and costs, and its path over time would not be influenced by shocks. But we know that capital markets are far from perfect, especially in developing countries, and that this is truer for insurance markets, formal or informal.

It is important to assess to what extent capital market imperfections and the inability of households to "insure" themselves against risk are actually relevant for determining the supply of child labor. From a theoretical point of view, changes in the labor supply and investment in human capital are two of the possible responses to the presence of risks and to exposure to shocks. However, there is no established evidence on the extent to which children's labor supply is actually used as risk coping strategy and/or as a buffer against shocks (with the exception discussed below).

This has important policy implications. If the role of child labor as a buffer against uninsured shocks is substantial, then policies aimed at reducing household risk exposure might have a substantial bearing on children's labor supply.

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Despite the attention given in the literature to the issues of capital market imperfection and child labor (human capital accumulation), there is almost no evidence on the issue with the exception of the seminal paper of Jacoby et Skoufias (1997).

In this paper, we exploit a unique data set for Guatemala containing information on access to credit markets, occurrence of several kinds of shocks and presence of insurance programs. The next section will briefly outline the theoretical foundations of the work, Section 3 illustrates the data set used and defines the variables. The econometric methodology adopted is described in Section 4, and the empirical results are presented in the Section 5.

1. Credit Market Imperfection and Children's Work

The theoretical background of the paper rests on two sets of "classical" results about the role of credit markets in determining human capital accumulation.

Recent works, building on the seminal work of Becker and Tomes (1976), have shown that borrowing constraints may represent an important source of inefficiency in the allocation of household resources to human capital investment (Ranjan 2001, Baland and Robinson 200 and Cigno, Rosati, Tzannatos 2002). If households do not have access to capital markets, they might be resource constrained and under invest in the human capital of their children. Better access to credit might, therefore, contribute to a reduction in child labor.

In an uncertain world, perfect capital markets would allow households to base investment decisions, including those on human capital, only on the relative rate of returns. Because the completeness of capital markets allows to households "insure" themselves from the expected shocks, child labor would not be influenced by negative shocks.

Child work (as shown in a companion paper) shows a high degree of persistence, making transition back to school problematic. If households move children to the (internal or external) labor market to cope with shocks, the costs of "uninsured" shocks can therefore be quite high in terms of human capital accumulation.

Determining that credit market imperfection and shocks affect the household decision concerning children's labor supply would have far reaching implications in terms of policy. In particular, a whole set of policies aimed at promoting development of capital markets, and at improving risk coping and risk reduction mechanisms, would become relevant as instruments to reduce child work and increase human capital accumulation. The strategic relevance of such policies has recently been strongly stressed by the World Bank (Holzmann and Jorgensen, 2002, and World Bank 2001); this paper aims to offer further empirical support to such a policy approach.

Recent research has shown that income has a relatively small effect on the supply of child work (Cigno et al. 2002, Deb and Rosati 2001). Sustained income growth or large transfer programs would be necessary to substantially reduce child work. Moreover, it has been shown (Deb and Rosati, 2001) that different groups of households have very different propensities to invest in children's education, even if they have very similar sets of observable characteristics. Both findings are coherent with a potential role of credit rationing and the lack of "insurance" mechanisms, but they do not offer direct support to these hypotheses. The available evidence is, however, extremely scarce. Beyond the evidence contained in the seminal paper of Jacoby and Skoufias (1997), some results based on a cross section of countries (Dehejia and Gatti, 2002) indicate that credit market development does have an impact on child labor. A recent paper by Edmonds (2002) performs an indirect test of the relevance of credit constraints for child work by evaluating the effects of an expected changes in household income.

In this paper we use a unique dataset on Guatemala that contains information on shocks, access to credit, availability of insurance mechanism. We will be able, given this information, to assess the relative importance of credit market, risk and policies on child labor and human capital accumulation.

The theoretical basis on which our empirical estimates will rest is well known, and no new insight is gained by presenting a formal model. We will therefore just outline the reference theoretical model and refer to the literature cited above for further details.

We assume that households maximize a utility function defined over current consumption and future (children's) consumption. Parents supply inelastically labor, whose returns are used to finance current consumption. Children's time can be used either to further increase current consumption through work, to accumulate human capital, or for leisure (above the minimum level physiologically required). Human capital determines children's future consumption. The household can change the intertemporal allocation of consumption by changing the children's labor supply[1]. The presence of credit rationing restricts the budget set of the household and, if binding, will generate inefficiently low level of investment in human capital. Moreover, household income net of children contribution is not certain, but rather subject to shocks. If capital markets were complete, the realization of such shocks would not affect children's labor supply (and consumption), as they would be insured.

The class of models just described predicts four possible outcomes for children's activities: three corner solutions and one internal solution. A child can attend school full time, work full time, do neither or combine work and school. The decision of the household concerning the activities of their children will be guided by an unobservable utility index I :

$$I = f(Z, X, C, S)$$

where Z indicates set of household characteristics including household expected or "permanent" income net of children's contribution, X indicates a set of proxies for the rate of returns to child work and for cost and returns to schooling, C indicates a set of variable relating to credit rationing, access to, public or private, insurance mechanisms, and S indicates realized shocks.

2. Data Set and Variable Definitions

Information on poverty, household conditions and other variables was collected in Guatemala through the 2000 Living Standards Measurement Survey (ENCOVI, 2000). The survey followed a probabilistic survey design, covering 7,276 households (3,852 rural and 3,424 urban) . The survey is representative at the national and regional level as well as in urban and rural areas. ENCOVI included questions to elicit a unique level of detail (for a representative sample) on themes related to vulnerability. The survey included modules on risks and shocks; conflict, crime, and violence; social capital; and migration. The data set for Guatemala is also unique in containing information on access to credit, shocks and insurances. As most of our attention will be devoted to such variables, we now discuss their exact definition and present some summary statistics.

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[1] Several variations are possible within this class of models. For example, future consumption of parent's could be included, as well as fixed costs in participating to work or school etc. Nothing of substance would change in the results relevant to the present paper.

Credit rationing. The survey contains a set of questions related to access to credit. In particular, households are asked whether they have applied for credit and, in case of application, whether they were denied the credit. We define as "credit rationed" households that did not apply for credit for one of the following reasons: a) Institutions offering credit not available b) Does not know how to ask for credit c) Does not have the required characteristics d) Does not have collateral e) Interest rates too high f) Insufficient income g) Institutions do not give credit to household in that conditions. We also classify as credit rationed households that applied for, but were denied, credit (see appendix 3 for details of the questions).

Table 1 shows descriptive statistics for credit rationed household broken down by level of poverty[2]. About 50 per cent of the households in Guatemala are credit rationed according to our definition. The incidence rises with poverty, ranging from about 40 per cent for households above the poverty line to almost 70 per cent for extreme poor households. In absolute terms, lack of income, lack of collateral and household conditions are the most

Table 1: Distribution of Households Credit Rationed by Poverty

Reasons for not applying for Credit	**Extreme Poor**	**Poor**	**Non Poor**	**Total**
Institutions offering credit not available	5.13	1.98	1.86	2.39
Does not know how to ask for credit	5.92	4.78	3.05	4.2
Does not have the required characteristics	8.28	11.34	11.02	10.76
Does not have the collateral	12.23	12.5	8.43	10.7
Afraid of Loosing collateral	5.13	5.53	4.58	5.06
Interest rates to high	5.33	6.56	12.42	8.92
Insufficient income	34.12	36.82	37.85	36.87
Institutions do not give credit to household in that conditions	22.09	18.24	13.01	16.54
Other reasons	1.78	2.25	7.77	4.57
Total	**100**	**100**	**100**	**100**
Credit refused following application	**Extreme Poor**	**Poor**	**Non Poor**	**Total**
	14.43	14.47	10.71	12.28
	Extreme Poor	**Poor**	**Non Poor**	**Total**
Credit Rationed Households	67.84	58.65	39.78	49.41

Source: *Encuesta de Condiciones de Vida* (ENCOVI) 2000. *Instituto Nacional de Estadisticas* (INE) Guatemala

[2] The extreme poverty line is defined as yearly cost of a "food of basket" that provides the minimum daily caloric requirement, estimated in Q. 1,912. The "non-extreme" poverty line (poor) is defined as the extreme poverty line plus an allowance for non-food items, estimated in Q. 4,319

common reasons for not applying for credit. Credit rationing through interest rate adjustments mainly applies to non-poor households. The rate of rejection of credit applications is similar for poor and non-poor households.

Shocks. ENCOVI 2000 contains a set of questions pertaining to the occurrence of shocks (See Appendix 3 for details). Shocks are divided in to two broad categories: collective and individual (idiosyncratic). Collective shocks include events like earthquakes, floods, fires etc. Individual shocks include loss of employment, death, etc[3]. Households can report more then one shock for each group. We have, however, classified a household as being hit by a shock if it reported at least one shock. In the analysis we used two dummies, one for each of the broad categories of shocks (collective and individual). Other classifications were also tried, but did not change the main results.

Table 2: Percentage of Households Surveyed Affected by Collective and Individual Shocks

Collective shock	Individual Shock				Shock	N° Hh	Percent
		Yes (%)	No (%)				
	Yes (%)	18	12		Individual	2769	38.06
	No (%)	20	50		Collective	2142	29.44
				Total 100	Total Households Surveyed	7276	

Source: *Encuesta de Condiciones de Vida* (ENCOVI) 2000. *Instituto Nacional de Estadisticas* (INE) Guatemala

About 50 percent of households surveyed reported experiencing one or more shock in year 2000; of these, 12 percent reported experiencing natural or economic shocks affecting the community, 20 percent shocks directly affecting the family and 20 percent affecting both. Of the 7,276 households surveyed, 38 percent were affected by individual (idiosyncratic) shocks and about 30 per cent by collective shocks (see Appendix 4 for additional details).

The most frequently reported collective shock is a general increase of prices. This could reflect a misperception of the economic environment or just a generic complaint about the cost of living. In any case, excluding this form of shock from the definition of the dummy variables does not change the results obtained.

[3] For a detailed description and analysis see Tesliue and Lindert 2002

Table 3: Percentage of Households Affected by Different Types of Collective and Individual Shocks

Individual Shock		Collective Shock	
	%		%
Loss of employment of any member	13.67	Earthquake	0.87
Lowered income of any member	17.42	Drought	6.32
Bankruptcy of a family business	2.55	Flood	2.33
Illness or serious accident of a working member of the household	15.64	Storms	3.28
Death of a working member of the household	2.19	Hurricane	1.66
Death of another member of the household	3.03	Plagues	16.69
Abandonment by the household head	1.67	Landslides	1.41
Fire in the house/business/property	0.27	Forest Fires	1.1
Criminal Act	4.79	Business Closing	0.81
Land Dispute	1.56	Massive lay offs	0.85
Family Dispute	1.82	General increase in price	63.01
Loss of cash or in-kind assistance	1.82	Public Protests	0.87
Fall in prices of products in the household business	7.54	Other Covariate Shocks	0.82
Loss of Harvest	24.95		
Other Idiosyncratic shocks	1.08		
Total	**100**	**Total**	**100**

Source: *Encuesta de Condiciones de Vida* (ENCOVI) 2000. *Instituto Nacional de Estadisticas* (INE) Guatemala

Risk reduction and risk coping mechanisms. The questionnaire allows us to identify whether an individual has medical insurance (public or private). A dummy variable was created, taking value of 1 if at least one member of the household has medical insurance (*Insurance*). Information was insufficient to identify whether households belonged to an informal social support network.

"Expected" expenditure. We computed expected expenditure by regressing household expenditure on a set of variables (age and sex of the household head, parents' education, parents' occupation and sector of employment, urban/rural area, regional dummies, household structure).

Child and household characteristics. We have employed a set of control variables to take into consideration individual and household characteristics.[4] The control variables include: the age of the child (*age*, age^2); a gender dummy (*Female*); a dummy variable taking

[4] The rationale for the use of these variables is well known in the literature on child work, see Cigno et al, 2001 and the literature cited therein.

value 1 if the child belongs to an indigenous household (*Indigenous*); the number of the household members (*Hhsize*); the number of children aged 0-5 in the household (*numkidsy*) and the number of school age children (*numkidso*); a dummy variable taking value 1 if the child is a girl and there are children aged 0-5 in the household (*femkidsy*); and a series of dummy variables for the education of the mother (*M_*) and of the father (*F_*).

3. Child Work in Guatemala

Child work is very common in Guatemala. Some 506,000 children aged 7-14 years, one-fifth of total children in this age group, are engaged in work. Most are employed on the family farm or in petty business and are located in rural areas. Guatemala ranks third highest in child work prevalence of the 14 Latin America and Caribbean (LAC) countries where data are available, behind only Bolivia and Ecuador. In terms of GDP per capita, on the other hand, the country ranks fifth lowest of the 14 countries. Guatemala's relative level of child work is therefore high compared to its relative level of income.

The decision to consider the age range 7 to 14 in order to define child work is based on several grounds. School starts at 7 in Guatemala and no significant amount of child labor is found below the age of 7. The basic cycle of education (*ciclo basico*) requires in most cases 9 years of study to be completed. It should be noted, on the other hand, that current legislation allow children to work legally as from the age of 14. We decided, however, to keep the age range coherent with the completion of the basic cycle of education, also to facilitate international comparison. Nothing of substance changes in the results if we define child work over the age range 7- 13.

The following table gives more detailed information on children's activities in Guatemala. It shows that a significant proportion of children – 17 percent – is reportedly neither working nor attending school. This group includes children (mainly girls) performing full time household chores, "hidden" workers and children for whom school attendance is too expensive or impossible due to lack of infrastructure, but that do not have opportunities to perform any productive activities. "Idle" children, a group almost as large as that of working children, also constitute an important policy concern; they not only do not go to school, but are at risk of becoming part of the labor force. This group is the most sensitive to changes in policy and in exogenous variables.

The table shows that gender differences in child activity status are important: boys are more likely to work, but girls are more likely to be neither working nor attending school. It also shows that children of indigenous households have a lower school attendance rate and a higher work participation rate than the rest of the population.

Table 4: Children Aged 7-14, by Sex, Type of Activity and Residence

Sex	Activity	Urban		Rural		Total	
		%	*No.*	*%*	*No.*	*%*	*No.*
Male	Work only	4.3	19,285	12.3	104,161	9.5	123,446
	Study only	73.9	334,299	53.9	455,964	60.9	790,263
	Work and study	10.1	45,587	19.7	166,924	16.4	212,511
	Total work*		64,872	32.0	271,085	25.9	335,957
	Total study**	78.2	379,886	73.6	622,888	67.3	1,002,774
	Neither	11.8	53,308	14.1	119,329	13.3	172,637
Female	Work only	4.1	17,820	6.8	54,249	5.9	72,509
	Study only	74.6	323,451	58.4	464,030	64.1	787,764
	Work and study	7.6	32,764	8.3	66,386	8.1	99,546
	Total work*	11.7	50,584	15.1	120,635	14.0	172,055
	Total study**	82.2	356,215	66.7	530,416	72.2	887,310
	Neither	13.8	59,770	26.5	210,491	22	270,371
Total	Work only	4.2	37,105	9.7	158,410	7.7	195,515
	Study only	74.2	657,750	56.1	919,994	62.4	1,577,744
	Work and study	8.8	78,351	14.2	233,310	12.3	311,661
	Total work*	13.0	115,456	23.9	391,720	20.0	507,176
	Total study**	83.0	736,101	70.3	1,153,304	74.7	1,889,405
	Neither	12.8	113,078	20.1	329,820	17.5	442,898

* 'Total work' refers to children that work only and children that work and study.

** 'Total study' refers to children that study only and children that work and study.

Source: *Encuesta de Condiciones de Vida* (ENCOVI) 2000. *Instituto Nacional de Estadisticas* (INE) Guatemala

4. Econometric Methodology: Propensity Scores, ATT and Sensitivity Analysis

The main econometric problem we face in estimating the effects of credit rationing, insurance and shocks is the potential endogeneity of these variables. To be credit rationed, to belong to an insurance scheme, or to be part of a social security system can all to a certain extent be endogenous. Even the occurrence of a shock cannot be treated as fully exogenous: if strong winds destroy the roof the outcome can partially depend on the way the roof was build. This in turn can be seen as a decision taken from the household not independently from those regarding children's labor supply and school attendance. Given the relevance of the endogeneity issue for the results presented in this paper, we discuss the matter at some length in the Appendix A in order to support the approach followed here which is based on propensity score matching methods and regression analyisis. Given that, as shown in many papers, analyses involving adjustments for unobservables tend to be quite subjective and very sensitive to distributional and functional assumptions and usually rely on the existence of a valid instrument, our analysis rests on the so-called unconfoundedness assumption, similar to the so-called selection on observables assumption: exposure to treatment is random within cells defined by observed variables X. We then use propensity score (i.e. the individual probability of receiving the treatment given the observed covariates) and regression methods to "adjust" the best possible way for all the pre-intervention covariates.

We now discuss how the propensity score will be specified and used for analysing the effects of shocks, insurance and credit rationing on child labor and school attendance.

Credit rationing, as well as shocks and insurance, is defined at the household level. A child is affected as long as the household to whom she belongs is also affected. This means that these treatment variables are assigned at the level of households, even if we want to analyse their effects on children. The clustered structure of the units of analysis (children) has some methodological implications. First of all, because the assignment is at the household level, assignment can be assumed ignorable (or even unconfounded) only if we condition on the households and their characteristics. In terms of propensity score modelling, the score must be defined at the household level, thus being the probability that a single household with a vector of characteristics, x, is credit rationed (or subject to a shock, or insured). In order to be consistent with the hypothesized assignment mechanism, the vector should also include summary characteristics of the children in each household (e.g. the number and age of the children).

Once the propensity score are estimated using households as units of analysis, the estimated propensity score for treated and non treated households can be used to check the degree of overlap between the two groups in terms of the distribution of their characteristics.

The propensity score can also be used to estimate the ATT using a matching strategy. Even if the outcome involves the children within the household, the outcome Y in this case must be defined at the household level. Summary measures of child labor or school attendance, such as the proportion of school-age children going to school, to work, etc. is appropriate. An explicit treatment of children as unit of analysis can only be appropriately done in a model such as the one introduced later.

As far as the matching procedure is concerned, in the paper we use a nearest neighbor matching, that for each of the N^T treated (e.g., rationed) households looks for the nearest neighbor matching sets in the group of control households, defined as:

$$C(i) = \min_j \left\| p_i - p_j \right\|$$

which usually contains a single control unit (household). Denoting the number of controls matched with treated observation i by N_i^C, then the matching estimator of ATT is

$$A\widehat{T}T = \frac{1}{N^T} \sum_{i \in T} \left[Y_i^1 - \sum_{j \in C(i)} \frac{1}{N_i^T} Y_j^0 \right].$$

An estimate of the variance of this estimator can be derived analytically or using bootstrap methods (see Becker, Ichino, 2001 for details).

A further complication of our analysis is that we are interested in at least three potentially endogenous variables, namely credit rationing, insurance and the occurrence of shocks. It cannot be determined from the questionnaire the order of these treatments. In principle we could define a treatment variable as the combination of the three, but that would render the propensity score based analysis, as well as the interpretation of the results, more complicated. We opted instead to analyse the propensity scores for each variable separately and derive separate estimates of their ATTs[5]. Eventual interactions among these variables are then captured and analysed in the model specified subsequently.

Finally, in order to test for the consequences that a violation of the hypothesis of unconfoundedness could have on our causal conclusion we have performed a sensitivity analysis. proposed by Rosenbaum and Rubin (1983) and extended here to a multinomial outcome. In particular this method allows us to assess the sensitivity of the causal effects with respect to assumptions about an unobserved binary covariate that is associated with both

[5] Some preliminary testing supported our decision, as they show conditional independence of the occurrence of the three variables considered

the treatments and the outcome. Details of the methodology and of the results are reported in Appendix 2.

5. Some Results

Propensity scores have been estimated as the probability that a household with characteristics X is credit rationed, insured or experienced a shock, respectively. In each case, specification of the propensity score was achieved by checking if the balancing property of the estimated propensity score was satisfied[6]. The estimated propensity score distributions are shown in Appendix 5. The distributions of the propensity scores for "treated" and "non treated" groups of households overlap to a large extent. ATT on several outcome variables have been derived using a nearest neighbor matching estimator and results appear in Tables 5 to 8.

The results obtained are very similar to those stemming from the regression analysis discussed in the next section. We leave, therefore, a detailed discussion for later and provide a short summary here.

Credit rationing reduces school attendance and increases, especially, the number of "idle" children; individual shocks significantly increase the proportion of "working" and "working and studying" children, while reducing the "studying only" children. Collective shocks have similar effects, although the effects seem to be smaller in absolute terms.

Table 5: Average Treatment Effects for "Credit Rationing"
Results from Matching Procedure using "Credit" as a Treatment Variable

Outcome variable	N. Treated	N. Control	ATT	Std. Err.	t
Proportion of children attending School	2078	1089	-0.044	0.017	-2.655
Proportion of children employed	2078	1089	-0.029	0.015	-1.936
Proportion of Children working only	2078	1089	-0.001	0.01	-0.082
Proportion of Children studying only	2078	1089	-0.016	0.018	-0.874
Proportion of Children working and studying	2078	1089	-0.028	0.012	-2.348
Proportion of Idle Children	2078	1089	0.045	0.014	3.286

[6] To do this we used the procedure implemented in Stata by Becker and Ichino (2001).

Table 6: Average Treatment Effects for “Individual Shock”
Results from Matching Procedure using "Individual Shock" as a Treatment Variable

Outcome variable	N. Treated	N. Control	ATT	Std. Err	T
Proportion of children attending School	1603	1011	-0.009	0.017	-0.521
Proportion of children employed	1603	1011	0.057	0.014	3.992
Proportion of Children working only	1603	1011	0.013	0.01	1.347
Proportion of Children studying only	1603	1011	-0.052	0.018	-2.842
Proportion Children working and studying	1603	1011	0.044	0.011	3.85
Proportion of Idle Children	1603	1011	-0.004	0.014	-0.277

Table 7: Average Treatment Effects for “Collective Shock”
Results from Matching Procedure using "Collective Shock" as a Treatment Variable

Outcome variable	N. Treated	N. Control	ATT	Std. Err.	T
Proportion of children attending School	1284	951	-0.001	0.018	-0.047
Proportion of children employed	1284	951	0.027	0.016	1.711
Proportion of Children working only	1284	951	-0.002	0.01	-0.181
Proportion of Children studying only	1284	951	-0.03	0.02	-1.495
Proportion Children working and studying	1284	951	0.029	0.013	2.248
Proportion of Idle Children	1284	951	0.003	0.015	0.208

Table 8: Average Treatment Effects for “Medical Insurance”
Results from Matching Procedure using "Insurance" as a Treatment Variable

Outcome variable	N. Treated	N. Control	ATT	Std. Err.	T
Proportion of children attending School	1130	743	0.026	0.019	1.365
Proportion of children employed	1130	743	-0.055	0.017	-3.227
Proportion of Children working only	1130	743	-0.021	0.011	-1.942
Proportion of Children studying only	1130	743	0.059	0.022	2.745
Proportion Children working and studying	1130	743	-0.033	0.014	-2.454
Proportion of Idle Children	1130	743	-0.005	0.016	-0.296

6. The Effects of Access to Credit, Shocks and Insurance on Children’s School Attendance and Labor Supply: a Multinomial Logit Analysis

As discussed in the previous section, we have computed the propensity scores relative to our proxies for credit rationing, insurance and for the occurrence of shocks. As shown in Appendix 5, the distribution of the propensity scores for “treated” and “non treated” groups of households overlap to a large extent, allowing us to draw causal inference from a regression model with reasonable confidence, i.e. we can be confident that, under the unconfoundedness assumption, the use of a regression model does not imply that the estimation of treatment effects relies on extrapolation. Because of similar covariates’ distributions for the treatment and control groups, model-based sensitivity should be very limited. Moreover, as reported in details in Appendix 2, the results obtained are robust with respect to the sensivity analysis carried out to assess the consequences of a violation of the unconfoundedness assumption. This gives us more confidence in the causal interpretation of our results.

We have used a multinomial logit[7] to model the household decisions concerning the four children's activities we consider (namely work only, work and study, study only, neither work nor study).

Table 9 presents the marginal effects[8] obtained by estimating the multinomial logit model (the results of the estimates are reported in Appendix 7.

All the coefficients for individual and household level characteristics are significant and have the expected sign. Holding expenditure and other characteristics constant, girls are less likely than boys to become part of the labor force. They are more likely to attend school, but especially to be "idle". This probably indicates that they are more likely than boys to be involved full time in household chores.

Indigenous children are more likely to be working than other children, and the probability to work increases by 8 percentage points. Parents' education (above primary education is the omitted category) has a negative effect on child labor and a positive effect on school attendance. A child belonging to those households whose father is not educated is about 5 percentage points more likely to work full time and 13 percentage points more likely to be idle than a child belonging to household with better educated father. In the case of Guatemala we do not observe large differences between the impact of mother and father education.

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[7] The multinomial logit model is even more flexible than the usual bivariate probit model, that takes account of the simultaneity of the decisions only through the correlation of the error terms. In fact, the covariates in the multinomial logit model may explicitly have a different effect on the probability of taking one of the four decisions. Also note that usual weakness of the conditional logit model, namely the Independence of Irrelevant Alternatives (IIA) property, does not apply when, as in our case, most or all the covariates are individual characteristics (as opposed to choice specific characteristics) and each of them has coefficients that are choice specific (i.e. each of them enter the underlying stochastic utilities with a different coefficient): in this case cross elasticities are not constant and including another alternative to the choice set does not leave the odds of the other alternatives unchanged.

[8] Computed at the mean.

Table 9: Multinomial Logit Model Marginal Effects

Variable	Work only		Study only		Work and Study		No Activities	
	dy/dx	*z*	*dy/dx*	*z*	*dy/dx*	*z*	*dy/dx*	*z*
Female	-0.022	-4.02	0.036	2.13	-0.078	-7.26	0.064	4.94
Age	-0.023	-2.48	0.178	7.09	0.094	5.51	-0.249	-13.5
age2	0.002	4.4	-0.010	-8.6	-0.003	-3.77	0.011	12.76
Indigenous*	0.013	3.02	-0.096	-7.19	0.065	7.07	0.018	1.8
Hh expenditure	-0.032	-5.01	0.152	7.95	-0.035	-2.95	-0.084	-5.41
Hhsize	-0.013	-5.89	0.052	7.99	-0.016	-3.74	-0.023	-4.61
Numkidsy	0.009	3.59	-0.013	-1.63	0.012	2.49	-0.007	-1.12
Numkidso	0.003	1.46	-0.018	-2.75	0.008	1.87	0.007	1.42
Femkidsy	-0.004	-1.8	0.003	0.36	-0.009	-1.64	0.010	1.64
M_none*	0.050	3.22	-0.155	-5.61	-0.014	-0.84	0.118	4.99
M_primary*	0.047	2.36	-0.092	-3.14	-0.006	-0.38	0.051	2
F_none*	0.048	3.72	-0.177	-7.13	-0.004	-0.27	0.132	5.7
F_primary*	0.023	2.47	-0.099	-4.53	0.002	0.19	0.073	3.8
Collective*	0.006	0.96	-0.055	-3.08	0.055	4.62	-0.005	-0.37
Individual*	0.015	2.51	-0.051	-3	0.039	3.65	-0.002	-0.17
Credit*	0.006	1.3	-0.066	-4.49	-0.002	-0.22	0.062	5.55
Insurance*	-0.014	-3.38	0.037	2.66	0.039	-4.94	0.016	1.37
Credit_Individual*	-0.006	-0.97	0.023	1.06	0.017	-1.34	0.000	0.01
Credit_Collectivet*	-0.010	-1.54	0.081	3.91	0.037	-3.18	-0.034	-2.11
Regional Dummies:								
Norte*	-0.008	-0.94	0.067	2.43	0.005	-0.22	-0.054	-3.06
Nororiente*	-0.009	-1.08	0.051	1.84	0.003	0.16	-0.045	-2.54
Suroriente*	-0.017	-2.44	0.088	3.32	0.021	0.94	-0.092	-6.73
Central*	0.008	0.72	0.035	1.25	0.050	2.1	-0.092	-6.66
Surroccidente*	-0.021	-3.21	0.113	4.62	0.017	0.81	-0.108	-8.1
Noroccidente*	-0.016	-2.08	0.106	4.15	-0.015	-0.78	-0.075	-4.47
Peten*	-0.003	-0.32	0.080	3.06	0.003	0.12	-0.080	-5.64

(*) For dummy variables, dy/dx is the effect of a discrete change from 0 to 1

Household expenditure reduces child labor and increases full time school attendance. At the mean, an increase of 10 per cent in income reduces the probability of a child to work only or work and study of about 7 percentage points.

The proxies for access to credit, shocks and insurance are not only significant, but also show strong effects on household decisions regard children's activities; in addition the results are consistent with those found in the propensity score based analysis.

Credit rationing strongly reduces school attendance: the probability that a child belonging to a credit rationed household attends school is about 7 percentage points lower compared to non rationed household. Children from credit rationed households are more likely to be out of school without participating in the labor force. This finding seems to indicate that credit rationing especially influences investment in the human capital of children. The alternative to school is not necessarily work. Credit-rationed households would send their children to school, if they could have access to credit. Hence, returns to education are at the margin higher than returns to work. If households value children leisure, or there are fixed costs to send children to work, in presence of low returns to child labor credit-rationed household will keep their children idle.

Idle children may lose twice: they do not obtain education, and they are also vulnerable to enter the labor force in presence of changing circumstances.

Households affected by shocks reduces children's full time school attendance, and increase child labor. Following a collective shock, children's participation increases by 5.5 percentage points. The largest part of these children are full time student, that start to work without dropping out of school.

Individual shocks have a similar overall effect with respect to the collective shocks. Child labor participation for households hit by such a shock is about 5 percentage points higher than average. Individual shocks, however, mainly affect children attending school and increase the probability of work full time (1.5 percentage points), while only marginally influencing idle children. About two thirds of the children that enter the labor force continue, however, to attend school also.

These results highlight the fact that inability to obtain credit significantly affects household investment decision in human capital, rather then children's labor force participation. Shocks, on the contrary, directly affect children's labor force participation, most likely because of the need to compensate for unexpected loss of resources. This result confirms the importance of credit rationing for investment in human capital, and indicate that better access to credit is not necessarily a powerful instrument to facilitate removal of children from the labor force. Children who do not attend school nor work are children at risk of becoming workers, and they may actually be in worse conditions than working children, as they might receive a smaller allocation of resources[9] and do not even benefit

[9] This seems to be confirmed by data on health status (see Cigno and Rosati, 2001, and tabulation available for many countries at www.ucw-project.org ,

from the increase in human capital from on-the-job training that their working children may receive.

Information on the availability of formal or informal insurance and "safety nets" mechanisms is scarce in the data set considered. As discussed above, we have utilized an indicator of whether any of the household members were covered by health insurance. The effect of this variable is far from negligible: children belonging to household where at least one member (usually the household head) is covered by health insurance are about 5 percentage points less likely to work only or to work and study. Such a large effect should not come to a surprise if one consider that about 15 per cent of the idiosyncratic shocks are linked to health conditions and that other kinds of shocks can be at least in part influenced by health conditions. The inference obtained from the use of this variable might be limited by the fact that holding an health insurance could proxy for income and education effects. Better-paid jobs might have attached to them such a scheme or more educated parents could be in a better position to evaluate the advantage of an insurance. However, the estimates are obtained controlling for income and parent's education. This gives further support to the conclusion that we are actually capturing differential effects on household behavior due to insurance coverage.

As mentioned above, credit rationing and shocks not only significantly influence child work and school attendance, but these effects are also relatively large. As a rough impression of the size of the effects of these variable, consider that in order to achieve an increase in school attendance equal to that due to the elimination of credit rationing, an income increase of 30 per cent would be required. To match the effects of eliminating the consequences of a negative individual shock on child work, an increase in income of about 20 per cent would be required. Similar figures can be obtained for the other variables.

Policies aimed at favoring access to credit markets and to providing safety nets, especially to poorer households, appear to be amongst the most powerful instruments for promoting school attendance and reducing child work. Moreover, the income equivalent needed to compensate for the effects of credit rationing and shocks also indicates that policies aimed at reducing risk are not only effective, but may prove to be also cost efficient in terms of use of resources.

7. Conclusion

Recently a growing attention has been paid to policies aimed at reducing the vulnerability of households and at promoting risk reduction strategies. The World Bank has developed a Social Risk Management strategy (see the works already quoted) that is increasingly on more incorporated in the Bank's coming activities

Until now the Social Risk Management approach has focused mainly, but not exclusively, on targeting vulnerability to poverty as defined by consumption. Obviously there are other dimensions of household behavior that are important from the point of view of risk management and vulnerability especially in an dynamic setting. Human capital investment and child labor are not only important dimensions of household welfare, but they also influence future income vulnerability and current and future health. In this paper we have tried to assess whether risk and vulnerability are also relevant for the set of the decisions concerning children's school attendance and labor supply. In particular we have aimed to evaluate the effect of shocks, credit rationing and insurance on the households decisions concerning children's activities.

On the basis of a theoretical approach based on well known results relative to human capital investment decision and children's labor supply, we have developed an estimation strategy that allow us to assess the importance of a set of risk factors.

We have used a very rich data set from Guatemala that contains information on shocks, credit rationing and insurance. Because of the potential endogeneity of the variable of interest, we used a methodology based on propensity scores. The analysis of the distribution of propensity scores for the "treated" and "not treated" population for the population of interest allows us to conclude that, given the maintained hypothesis of unconfoundedness (selection on observables), we can safely draw causal inference from our estimates. The computed ATTs confirm the main results obtained through the regression analysis.

The main results indicate that credit rationing is extremely important in determining the household's decision to invest in the human capital of children. This variable is, however, less relevant in changing the household decision relative to children's labor supply. The main effects being linked to the decision to leave the children "idle" or to send them to school.

Even if it does not directly affect children's labor supply, credit rationing appears to be a very important determinant of children's vulnerability as "idle" children are particularly at risk of becoming workers and often face circumstances that are even harder than those of working children.

Shocks substantially alter household decisions and a negative shock substantially increases the probability that a child will work. Coupled with the evidence from other research that child labor shows a high degree of persistence, this indicates the importance that protection from shocks would have in reducing children's labor supply and increasing human capital investment.

Finally risk reduction schemes, proxied in our analysis by the availability of medical insurance also showed substantial effect on child work.

Note that not only the above mentioned variables are all significative, but their impact is quite large. For example, the same reduction in children labor supply determined by the elimination of negative shocks could be brought about by an increase on about 40 per cent of the income of the concerned household. Similar orders of magnitude are obtained for the other variables.

These results clearly illustrate how policies aimed at reducing the risks households face and at promoting better access to credit markets, can also have powerful effects on child labor. Such "general" measures do not appear to be less powerful than other targeted policies in the real of child labor prevention policies.

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APPENDIX 1:

Econometric Methodology

Empirical applications in economics often struggle with the question of how to accommodate (often binary) endogenous regressor(s) in a model aimed at capturing the relationship between the endogenous regressor(s) and an outcome variable.

Problems of causal inference involve “what if“ statements, and thus counterfactual outcomes and are usually motivated by policy concerns. They can be “translated” into a treatment-control situation typical of the experimental framework. The fact that the treatment is endogenous reflects the idea that the outcomes are jointly determined with the treatment status or, that there are variables related to both treatment status and outcomes. “Endogeneity” thus prevents the possibility of comparing “treated” and “non treated” individuals: no causal interpretation could be given to such a comparison because the two groups are different irrespective of their treatment status.

A growing strand of applied economic literature has tried to identify causal effects of interventions from observational (i.e. non experimental) studies, using the conceptual framework of randomised experiments and the so-called potential outcomes approach, that allows causal questions to be translated into a statistical model[10]. While it is possible to find some identification strategies for causal effects even in non experimental settings, data alone do not suffice to identify treatment effects. Suitable assumptions, possibly based on prior information available to the researchers, are always needed.

In this paper we will use the potential outcomes approach to causal inference, based on the statistical work on randomized experiments by Fisher and Neyman, and extended by Rubin (see Holland 1986). In recent years, many economists have accepted and adopted this framework[11] because of the clarity it brings to questions of causality.

This approach defines a causal effect as the comparison of the potential outcomes on the same unit measured at the same time: Y(0) = the value of the outcome

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[10] See for example Angrist and Krueger, 1999; and Heckman et al., 1999 for state-of-the-art papers.

[11] See for example Bjorklund and Moffit, 1987; Pratt and Schlaifer, 1988; Heckman, 1989; Manski, 1990; Manski et al., 1992; Angrist and Imbens, 1995, Angrist and Krueger, 1999

variable Y if the unit is exposed to treatment T = 0, and Y(1) = the value of Y if exposed to treatment T = 1. Only one of these two potential outcomes can be observed, yet causal effects are defined by their comparison, e.g., Y(1) - Y(0). Thus, causal inference requires developing inferences able to handle missing data. The focus of the analysis is usually that of estimating the average treatment effect ATT = E(Y(1) – Y(0)), or the average treatment effect for subpopulations of individuals defined by the value of some variable, most notably the subpopulation of the treated individuals ATT = E(Y(1) – Y(0) | T = 1).

The assignment mechanism is a stochastic rule for assigning treatments to units and thereby for revealing Y(0) or Y(1) for each unit. This assignment mechanism can depend on other measurements, i.e. P(T = 1|Y(0), Y(1), X). If these other measurements are observed values, then the assignment mechanism is ignorable; if given observed values involve missing values, possibly even missing Y's, then it is non-ignorable. Unconfoundedness is a special case of ignorable missing mechanisms and holds when P(T = 1|Y(0), Y(1), X) = P(T = 1| X) and X is fully observed. Unconfoundedness is similar to the so called "selection on observables" assumption (also exogeneity of treatment assignment), which states that the value of the regressor of interest is independent of potential outcomes after accounting for a set of observable characteristics X. This approach is equivalent to assuming that exposure to treatment is random within the cells defined by the variables X. Although very strong, the plausibility of these assumptions rely heavily on the amount and on the quality of the information on the individuals contained in X.

Under unconfoundedness one can identify the average treatment effect within subpopulations defined by the values of X:

$$E(Y(1) - Y(0)| X = x) = E(Y(1) \mid X = x) - E(Y(0) \mid X = x) =$$

$$= E(Y(1) \mid T = 1, X = x) - E(Y(0) \mid T = 0, X = x)$$

and also the overall ATT as :

$$E(Y(1) - Y(0)) = E(E(Y(1) - Y(0)| X = x))$$

where the outer expectation is over the distribution of X in the population. If we could simply divide the sample into subsamples, dependent on the exact value of the covariates X, we could then take the average of the within subsample estimates of the average treatment effects. Often the covariates are more or less continuous, so some smoothing techniques are in order: under unconfoundedness several estimation strategy can serve this purpose. One such strategy is regression modelling: usually a functional form for $E(Y(t) \mid X = x)$ is assumed, for example a linear function in a vector of functions of the covariates $E(Y(t) \mid X = x) = g(x)' b_t$. Estimates of the parameters' vectors b_t ($t = 0, 1$) are usually obtained by least squares or maximum likelihood methods. Causal effects are rarely estimated, especially if the model is non linear, by the value of some parameters, unless some restrictions are imposed on the b_t.[12]

Using regression models to "adjust" or "control for" pre-intervention covariates while being in principle a good strategy, it has some pitfalls. For example, if there are many covariates, it can be difficult to find an appropriate specification. In addition, regression modelling obscures information on the distribution of covariates in the two treatment groups. In principle, one would like to compare individuals that have the same values for all the covariates: unless there is a substantial overlap of the covariates' distributions in the two groups, with a regression model one relies heavily on model specification, i.e. on extrapolation, for the estimation of treatment effects.

Therefore it is crucial to check the extent of the overlapping between the two distributions, and the "region of common support" for these distributions. When the number of covariates is large, this task is not an easy one. An approach that can be followed is to reduce the problem to a one-dimensional one by using the propensity score, that is, the individual probability of receiving the treatment given the observed covariates $p(X) = P(T = 1 \mid X)$. In fact, under unconfoundedness the following results hold (Rosenbaum and Rubin, 1983a)

[12] For example imposing that the treatment effect is constant, i.e. excluding the interaction terms of the treatment with the other covariates

1. T is independent of X given the propensity score p(X)
2. Y(0) and Y(1) are independent of T given the propensity score

From (1) we can see that the propensity score has the so-called balancing property, i.e., observations with the same value of the propensity score have the same distribution of observable (and possibly unobservable) characteristics independently of the treatment status; from (2), exposure to treatment and control is random for a given value of the propensity score. These two properties allow us to a) use the propensity score as a univariate summary of all the X, to check the overlap of the distributions of X, because it is enough to check the distribution of the propensity score in the two groups, and b) use the propensity score in the ATE (or ATT) estimation procedure as the single covariate that needs to be adjusted for, as adjusting for the propensity score automatically controls for all observed covariates (at least in large samples). In this paper we will use the estimated propensity score to serve purpose a) to validate the regression results, and purpose b) by estimating the ATT with a propensity score based matching algorithm.

The analysis of the propensity score alone can be very informative because it reveals the extent of the overlap in the treatment and comparisons groups in terms of pre-intervention variables. The conclusion of this initial phase may be that treatment and control groups are too far apart to produce reliable estimates without heroic modelling assumptions.

The propensity score itself must be estimated: if the treatment is binary, any model for binary dependent variables can be used, although the balancing property should be used to choose the appropriate specification of the model, i.e. how the observed covariates enter the model. Some specification strategies are described in Becker and Ichino (2001) and Rubin (2002). Propensity score methods can be extended to include multiple treatments (Imbems, 2000; Lechner 2001).

The assumption that the treatment assignment is ignorable, or even unconfounded, underlies much of the recent economic policy intervention evaluation strategies (Jalan, Ravallion, 2001), so that one might have the impression that researchers no longer pay much attention to unobservables. The problem of the analyses involving adjustments for unobserved covariates, such as the Heckman's type corrections (Heckman, Hotz, 1989), is that they tend to be quite subjective and very sensitive to distributional and functional specification. This has been shown in a series of theoretical and applied papers (Lalonde, 1986; Dehejia and Wahba, 1999; Copas and Li, 1997). The adjustment for unobserved variables, however, strongly relies on the existence of valid instruments, i.e. on variables that are correlated with T but are otherwise independent of the potential outcomes. If such variables exist, they can then be used as a source of exogenous variation to identify causal effects (Angrist, Imbens, 1995; Angrist, et al., 1996); the validity of a variable as an instrument, i.e., the validity of the exclusion restrictions, cannot be directly tested. In observational studies such variables are usually very hard to find, although there are some exceptions (see Angrist and Krueger, 1999, for some examples).

Thus, despite the strength of the unconfoundedness assumption, that, nevertheless, cannot be tested, it is very hard not to use it in observational studies: it is then crucial to adjust the "best" possible way for all observed covariates. Propensity score methods can help achieve this. The issue of unobserved covariates should then be addressed using models for sensitivity analysis (e.g. Rosenbaum and Rubin, 1983b) or using non parametric bounds for treatment effects (Manski, 1990; Manski et al., 1992).

APPENDIX 2:
Sensitivity Analysis

Our analysis of the effects of credit rationing, insurance and the occurrence of shocks is based on the critical assumption of unconfoundedness; as in all observational studies, our results might be subjects to dispute since this assumption rules out the role of the unobservables. In order to check how robust our causal conclusions are, we now apply a method for sensitivity analysis, proposed by Rosenbaum and Rubin (1983) and extended here to a multinomial outcome. In particular this method allow us to assess the sensitivity of the causal effects with respect to assumptions about an unobserved binary covariate that is associated both with the treatments and with the response.

The unobservables are assumed to be summarized by a binary variable in order to simplify the analysis, although similar techniques could be used assuming other distributions for the unobservables. Note however that a Bernoulli distribution can be thought of as a discrete approximation to any distribution, and thus we believe that our distributional assumption will not severely restrict the generality of the results.

Suppose that treatment assignment is not unconfounded given a set of observable variables X, i.e.,

$P(T = 1|Y(0), Y(1), X)$ is not equal to $P(T = 1| X)$

but unconfoundedness holds given X and an unobserved binary covariate U, that is

$P(T = 1|Y(0), Y(1), X, U)$ is equal to $P(T = 1| X, U)$.

We can then judge the sensitivity of conclusions to certain plausible variations in assumptions about the association of U with T, Y(0), Y(1) and X. If such conclusions are relatively insensitive over a range of plausible assumptions about U, then our causal inference is more defensible.

Since Y(0), Y(1) and T are conditionally independent given X and U, we can write the joint distribution of (Y(t), T, X, U) for t = 0, 1 as

$$\Pr(Y(t), T, X, U) = \Pr(Y(t)| X, U) \Pr(T| X, U) \Pr(U| X) \Pr(X)$$

where, in our analysis, we assume that

$$\Pr(U = 0|X) = \Pr(U = 0) = \pi$$

$$\Pr(T = 0| X, U) = (1+\exp(\gamma' X + \alpha U))^{-1}$$

$$\Pr(Y(t) = j| X, U) = \exp(\beta'_j X + \tau_j T + \delta_{tj} U) (1 + \Sigma_i \exp(\beta'_i X + \tau_i T + \delta_{ti} U))^{-1}$$

j=(Working only:W, Studying only: S, Working and Studying: WS, Idle Children: I)

π represents the proportion of individuals with U=0 in the population, and the distribution of U is assumed to be independent of X. This should render the sensitivity analysis more stringent, since, if U were associated with X, controlling for X should capture at least some of the effects of the unobservables. The sensitivity parameter α captures the effect of U on treatment receipt (e.g., credit rationing), while the δ_{ti},'s are the effects of U on the outcome.

Given plausible but arbitrary values to the parameters π , α and δ_{ti}, we estimated the parameters γ and β_j by maximum likelihood and derived estimates of the ATT as follows:

$$A\widehat{T}T = \frac{1}{N^T} \sum_{i \in T} \left[\hat{Y}_i^1 - \hat{Y}_i^0 \right]$$

where

$$\hat{Y}_i^t = \hat{\Pr}(Y(t) = j \mid X) = p\, \hat{\Pr}(Y(t) = j \mid X, U = 0) + (1-p) \hat{\Pr}(Y(t) = j \mid X, U = 1)$$

These estimates of the ATT are comparable to the ones based on the propensity score based matching procedure and they are very similar to the marginal effects obtained.

Table 2.1 - Average Treatment Effects for "Credit Rationing" for Different Values of the Sensitivity Parameters

ATT	$\alpha=0$ $\delta_{0W}=\delta_{1W}=0$ $\delta_{0S}=\delta_{1S}=0$ $\delta_{0WS}=\delta_{1WS}=0$	$\pi=0.1$, $\alpha=0.1$ $\delta_{0W}=\delta_{1W}=-0.1$ $\delta_{0S}=\delta_{1S}=0.1$ $\delta_{0WS}=\delta_{1WS}=0.1$	$\pi=0.5$, $\alpha=0.5$ $\delta_{0W}=\delta_{1W}=-0.1$ $\delta_{0S}=\delta_{1S}=0.1$ $\delta_{0WS}=\delta_{1WS}=0.1$	$\pi=0.1$, $\alpha=0.1$ $\delta_{0W}=\delta_{1W}=0.5$ $\delta_{0S}=\delta_{1S}=0.5$ $\delta_{0WS}=\delta_{1WS}=0.5$	$\pi=0.5$, $\alpha=0.5$ $\delta_{0W}=\delta_{1W}=-0.5$ $\delta_{0S}=\delta_{1S}=0.5$ $\delta_{0WS}=\delta_{1WS}=0.5$
Working only	0.011	0.011	0.012	0.011	0.018
Studying only	-0.049	-0.050	-0.052	-0.053	-0.060
Working and Studying	-0.028	-0.023	-0.028	-0.028	-0.031
Idle Children	0.066	0.062	0.067	0.070	0.073

In Table 2.1 the estimates of the ATT for credit rationing and different combinations of values for π , α and δ_{ti} are reported. The X's are the same used in the estimation of the multinomial logit model and the propensity score method. As can be observed the results are not very sensitive to a range of plausible assumptions about U. Note that an α or δ_{ti} of 0.5 almost doubles the odd of receiving the treatment or the odd of a certain value of the outcome. In addition these values are larger than most of the coefficients of the estimated multinomial logit. Setting the values of the association parameter to bigger numbers may change the obtained results. However, given the number of observed covariates already included in the models, the existence of a residual unobserved covariate so highly correlated with T and Y appears implausible. Sensitivity of ATT estimates for individual and collective shocks as well as for insurance gave similar results and are available upon request from the authors.

APPENDIX 3:

Questions used to define the some of the variables used in the estimation

Questions used to Define Credit Rationed Households
What is the principal reason that no one applied for a loan? In the community no one offer loans…………………………………...1 Do not know how to apply for a loan………………………………...2 They ask for too many requirements………………………………....3 Don't have the goods to give guarantees……………………………..4 Fear of losing the guarantees…………………………………….……...5 Interest rate is too high……………………………………………….6 Prefer to work with own resources…………………………….…..7 Do not have opportunity to invest……………………………………8 There was no need……………………………………………………9 Insufficient income…………………………………………………10 They don't give loans to people like us……………………………...11 Other what? …………………………………………………………12 Did they approve any loan that was applied for? Yes ……………………………………………………1 No ……………………………………………………..2

Questions used to Define the Collective and Individual Shocks	
Collective Shocks	***Individual Shocks***
In the last 12 months, has the households been affected by any of the following general types of problems? Earthquake………………………1 Drought…………………………..2 Flood……………………………..3 Storms…………………………....4 Hurricane………………………...5 Plagues…………………………...6 Landslides ………………………7 Forest Fires………………………8 Business Closing…………………9 Massive lay offs ……………..…10 General increase in price.………11 Public Protests ………..……..12 Other …………………… …....13	In the last 12 months, has the households been affected by any of the following problems? Loss of employment of any member.……….…………...1 Lowered income of any member………………………....2 Bankruptcy of a family business………………………....3 Illness or serious accident of a working member of the….4 household.……………………………………………..5 Death of a working member of the household …………..6 Abandonment by the household head Fire in the house/business/property.………………………………....7 Criminal Act ……………………………………………..8 Land Dispute.…………………………………………….9 Family Dispute.…………………………………………10 Loss of cash or in-kind assistance.……………………...11 Fall in prices of products in the household business.…...12 Loss of Harvest …………………………………………13 Other ……………………………………………………...14

Questions used to Define the "Health Insurance" and "Social Security" Variables	
Health Insurance	***Social Security***
Is [NAME] affiliated or covered by : Private Health or illness insurance …..1 IGSS ……………………………………2 IGSS and private..…………………....3 Other, what …………………………..4 None ……………………………………5	Do you pay a quota to social security (IGSS) for the work that you do as (…..)? Yes ……………………………… 1 No ……………………………….2

APPENDIX 4
Detailed Descriptive Statistics on Shocks
Table A4.1 Shocks that Resulted in a loss of Income, Inheritance or none of them

Collective Shocks	Loss of Income normally received		Loss of Inheritance		Loss of Income and Inheritance		None		Total	
	%	*No.*	*%*	*No.*	*%*	*No.*	*%*	*No.*	*%*	*No.*
Earthquake	20.1	4166	32.0	6625	7.4	1524	40.6	8407	100	20722
Drought	41.2	62231	8.6	12933	6.5	9749	43.8	66118	100	151031
Flood	29.5	16405	14.8	8240	7.7	4293	48.0	26673	100	55611
Storms	33.4	26186	14.4	11248	3.3	2554	48.9	38310	100	78298
Hurricane	37.1	14663	17.3	6835	9.8	3886	35.8	14179	100	39563
Plagues	48.9	195039	7.4	29469	5.8	23077	38.0	151401	100	398986
Landslides	33.1	11125	12.6	4237	15.3	5137	39.0	13115	100	33614
Forest Fires	13.0	3396	12.8	3346	7.5	1960	66.8	17473	100	26175
Business Closing	54.7	10545	2.1	409	6.8	1301	36.4	7021	100	19276
Massive lay offs	72.9	14861	0.0	0	7.3	1485	19.8	4046	100	20392
General increase in price	90.5	1363135	2.6	38430	2.4	36066	4.6	68490	100	1506121
Public Protests	35.5	7401	0.6	132	1.4	289	62.5	13011	100	20833
Other	39.3	7706	13.7	2694	11.1	2177	35.9	7029	100	19606
Total	72.7	1736859	5.2	124598	3.9	93498	18.2	435273	100	2390228

Note: the totals exceed the total number of households because of multiple answers

Table A4.2 Shocks that Resulted in a Loss of Income, Inheritance or none of them

Individual Shocks	Loss of Income Normally Received		Loss of Inheritance		Loss of Income and Inheritance		None		Total	
	%	*No.*	*%*	*No.*	*%*	*No.*	*%*	*No.*	*%*	*No.*
Loss of employment of any member	93.3	166753	2.18		1.9	3394	2.62	4680	100	178727
Lowered income of any member	93.53	213037	2.18	4963	2	4545	2.3	5230	100	227775
Bankruptcy of a family business	83.36	27794	5.11	1705	9.39	3130	2.14	713	100	33342
Illness or serious accident of a working member of the household	85.88		2.75	5620	5.41	11060	5.96		100	204548
Death of a working member of the household	87.75	25103	0.3	86	8.5	2431	3.45	986	100	28606
Death of another member of the household	55.02	21814	2.95	1171	1.71	679	40.32	15987	100	39651
Abandonment by the household head	63.93	14000	0.79	172	8.55	1872	26.74	5855	100	21899
Fire in the house/business/property	17.04	604	65.6	2325	17.35	615	0	0	100	3544
Criminal Act	69.93	43795	10.84	6786	8.6	5386	10.64	6661	100	62628
Land Dispute	29.56	6047	3.83	783	5.12	1048	61.5	12582	100	20460
Family Dispute	31.65	7513	2.96	702	3.05	725	62.34	14798	100	23738
Loss of cash or in-kind assistance	81.62	19412	0.66	156	8.62	2051	9.1	2165	100	23784
Fall in prices of products in the household business	79.16	78046	0.65	645	16.44	16208	3.74	3691	100	98590
Loss of Harvest	76.67	250179	8.82	28788	11.39	37182	3.12	10168	100	326317
Other	83.54	11835	1.52	216	0.88	125	14.05	1991	100	14167
Total	81.18	1061603	4.44	58018	6.92	90451	7.47	97704	100	1307776

Note: the totals exceed the total number of household because of multiple answers

APPENDIX 5:
Comparison of the Distributions of Propensity Scores for Treated and Control Groups

Fig A5.1: Propensity Scores Comparison for "Credit Rationing "

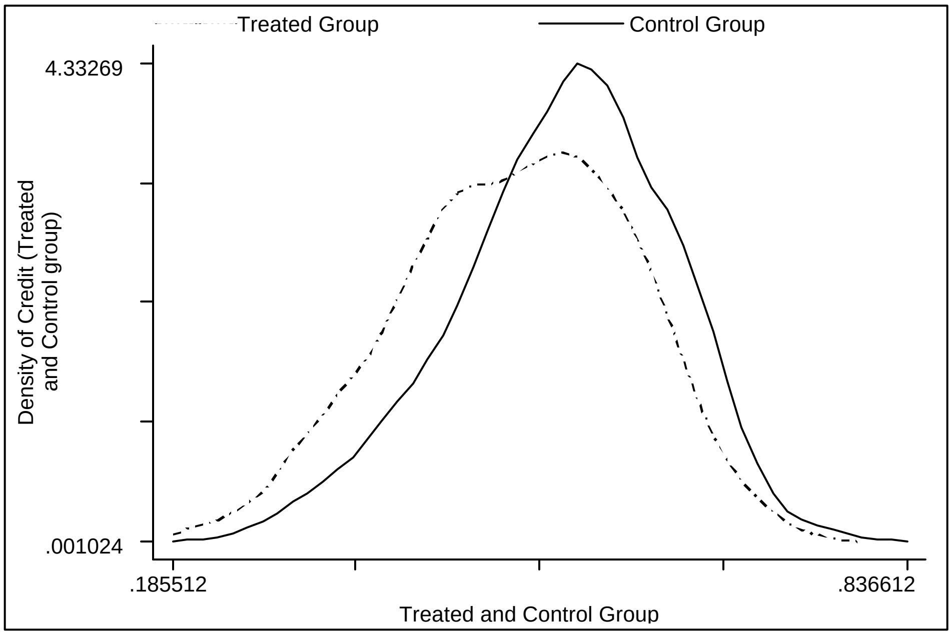


Fig A5.2: Propensity Scores Comparison for "Individual Shocks"

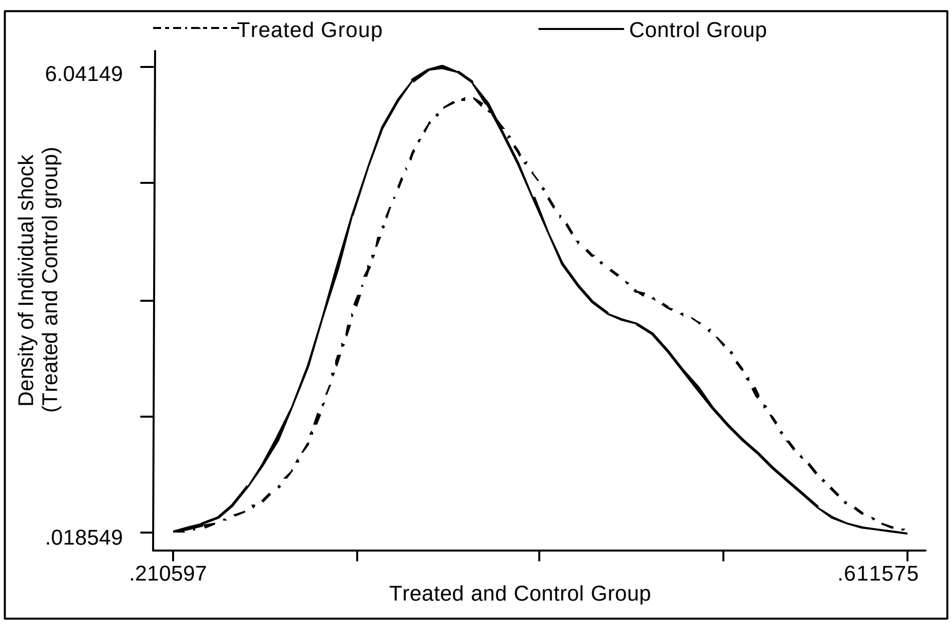


Fig A5.3: Propensity Scores Comparison for “Collective Shock”

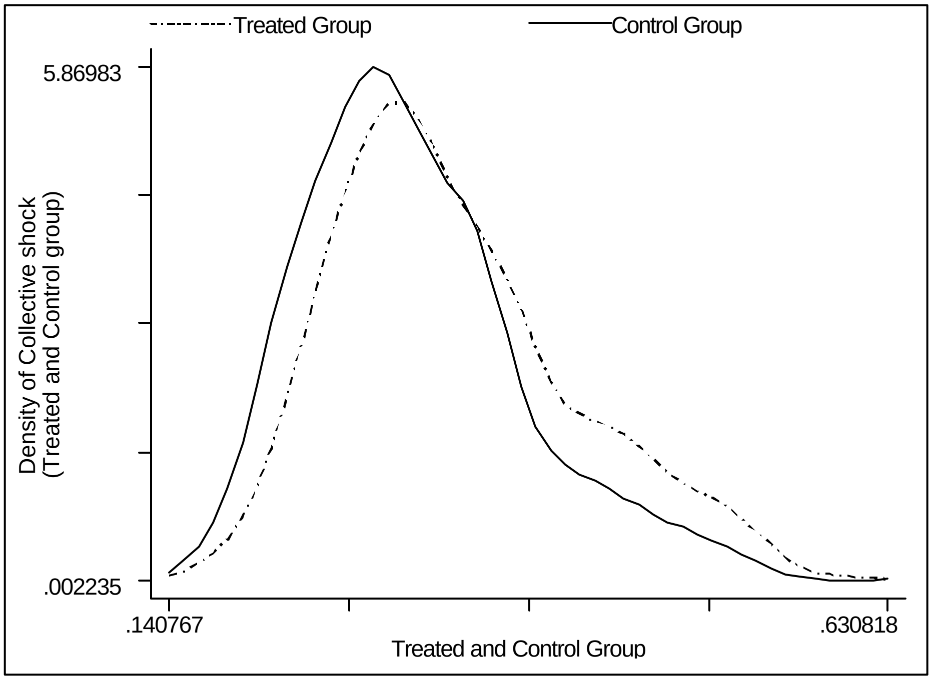
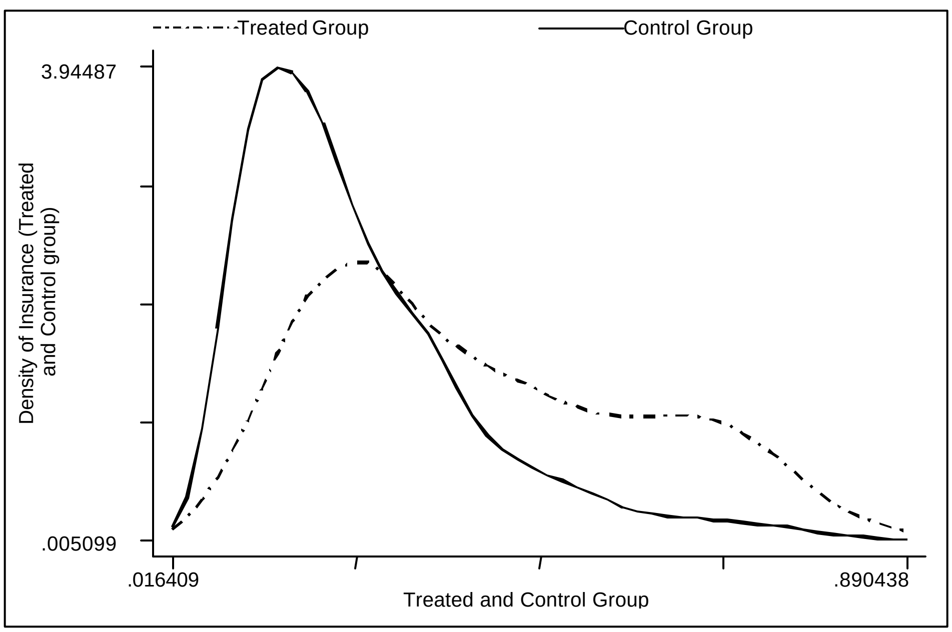


Fig A5.4: Propensity Scores Comparison for “Insurance ”



APPENDIX 6:

Variable Definitions

Child Activities:

Working:	1 if individual currently works, 0 otherwise
Attending school:	1 if individual currently attends school, 0 otherwise
Work only:	1 if individual currently works and do not attend school
Study only:	1 if individual currently attends school and do not work
Work and Study:	1 if individual currently works and attends school
Neither:	1 if individual currently neither works nor attends school

Other Variables:

Female:	1 if female, 0 otherwise
Household expenditures:	logarithm of per capita household expenditure

Father's Education:

F_None:	1 if he has no completed education, 0 otherwise
F_Primary:	1 if he has completed primary education, 0 otherwise

Mother's Education:

M_None:	1 if she has no completed education, 0 otherwise
M_ Primary:	1 if she has completed primary education, 0 otherwise

Secondary or higher education is the comparison group

Indigenous:	1 if a child is indigenous, 0 otherwise

Shocks:

Collective	1 if a household reported experiencing at least a collective shock, 0 otherwise
Individual	1 if a household reported experiencing at least a idiosyncratic shock, 0 otherwise

Social Risk Indicator:

Insurance	1 if at least one member of the household has a medical insurance, 0 otherwise

Credit Rationing Indicator:

Credit	1 if a household is credit rationed, 0 otherwise

APPENDIX 7:

Results from Multinomial Logit Estimates

Reference Group: Children neither Working nor Studying

Variable	Work only		Study only		Work and Study	
	Coef.	***Z***	***Coef.***	***Z***	***Coef.***	***Z***
Female	-1.03	-6.11	-0.39	-3.65	-1.20	-8.27
Age	1.06	3.85	1.94	13.04	2.60	11.62
age2	-0.02	-1.96	-0.09	-12.77	-0.11	-10.22
Indigenous*	0.22	1.68	-0.26	-3.17	0.47	4.18
Hh expenditure	-0.31	-1.5	0.78	6.29	0.23	1.34
Hhsize	-0.19	-2.82	0.23	5.61	0.01	0.13
Numkidsy	0.29	3.6	0.03	0.59	0.17	2.44
Numkidso	0.03	0.5	-0.07	-1.8	0.03	0.5
Femkidsy	-0.19	-2.4	-0.07	-1.28	-0.16	-2.19
M_none*	0.51	1.13	-1.03	-5.36	-0.94	-3.76
M_primary*	0.74	1.65	-0.46	-2.45	-0.39	-1.59
F_none*	0.30	1.01	-1.07	-6.78	-0.84	-4.09
F_primary*	0.13	0.45	-0.63	-4.14	-0.47	-2.37
Collective*	0.18	1.01	-0.04	-0.37	0.52	3.55
Individual*	0.40	2.25	-0.06	-0.5	0.38	2.65
Credit*	-0.26	-1.74	-0.52	-5.63	-0.45	-3.42
Insurance*	-0.52	-3.36	-0.05	-0.6	-0.52	-4.12
Credit_Individual*	-0.18	-0.78	0.03	0.21	-0.18	-0.93
Credit_Collectivet*	-0.03	-0.13	0.36	2.39	-0.16	-0.78
Regional Dummies:						
Norte*	0.18	0.54	0.52	2.76	0.38	1.4
Nororiente*	0.07	0.2	0.42	2.29	0.39	1.4
Suroriente*	0.26	0.76	0.96	5.11	1.04	3.85
Central*	1.00	3.36	0.85	4.96	1.23	4.95
Surroccidente*	0.25	0.8	1.13	6.43	1.13	4.5
Noroccidente*	0.10	0.3	0.75	4.09	0.46	1.73
Peten*	0.63	1.96	0.82	4.43	0.74	2.72
_cons	-7.19	-2.94	-13.35	-9.53	-16.10	-8.12