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Do School Subsidies Promote Human Capital Investment among the Poor?

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M E X I C O

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Abstract

We investigate the hypothesis that conditioning transfers to poor families on school attendance leads to a reallocation of household resources enhancing the human capital of the next generation, via the effect of the conditionality on the shadow price of human capital. We estimate the price effect of conditional transfers to mothers on intra-household allocations using data from a social program in Mexico, and show that price effects are large and statistically significant. The estimates suggest that household resources beyond those directly subject to conditionality have been reallocated favorably to children's human capital.

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1 Introduction

The design of social policies which encourage human capital accumulation among the poor, thus breaking the transmission of poverty from one generation to the next, is a basic concern for development economists. Roughly speaking, these policies can be classified as either "supply-side" interventions, attempting to improve the infrastructure or quality of education, or "demand-side" interventions, attempting to provide incentives for poor parents to keep their children longer in school and engage in other activities bolstering human capital accumulation. A number of recent demand-side interventions, in countries like Mexico, Brazil, Colombia and Honduras, involve cash and in-kind transfers to parents that are conditional on the school attendance of their children. In this paper, we provide a model to study the effects of conditional transfers on intra-household allocations, and conduct an empirical analysis of the impact of a conditional transfer program in Mexico.

The model we propose combines elements of the household production approach pioneered by Becker [4, 21] and the collective household approach developed by Chiappori and his co-authors [11, 12, 13]. We treat schooling as one input in the production of children's human capital. Other important inputs are the time devoted by family members to children's human capital and the consumption of children. By subsidizing schooling, conditional transfers reduce the shadow price of human capital acquisition. The price or conditionality effect of transfers, then, involves not only an increase in schooling but also in the time devoted to school homework and in the consumption of children. The impact on these other inputs may very well precede the impact on schooling. Suppose, for instance, that conditional transfers induce families which already send their children to primary school to anticipate they will send their children also to secondary school. Then, independently of income effects, parents will devote more resources to children now if these resources are perceived to be complementary with more years of schooling in the production of human capital.

Income and price effects do not exhaust all implications of conditional transfers for intra-household allocation. Conditional transfers are typically paid to the mother of the family. A wide empirical literature (including the work of Thomas [31], Schultz [28], Lundberg, Pollak and Wales [19], Duflo [16]

and others) has shown that changes in the household income distribution, and in particular benefits oriented to the mother, may shift household expenditure patterns in directions favorable to children. Thus, we treat the household as a collective entity, and allow schooling subsidies to vary the weight of the mother's preferences in the household utility function. In the description of the model, we provide conditions under which a higher weight of the mother's preferences translates into a shift favorable to children.[1] If parents have CES preferences and the initial bargaining power of the mother is small, these conditions entail that the elasticity of substitution between own consumption and the consumption and human capital of the children cannot be much smaller than one. This, in turn, implies that price effects cannot be small.

Note that the price effect of conditional transfers on resources allocated to children is due to the impact of an expected or current increase in schooling on human capital acquisition. Price effects capture the mobilization of family resources toward human capital accumulation in response to a lower price for one input, schooling. On the other hand, income and bargaining effects may lead to a reallocation favorable to children even if conditional transfers have little effect on schooling or if schooling is not perceived by families to have a significant impact on human capital acquisition due to, say, low quality of schools available to the poor. Isolating the effects of conditionality on intra-household allocation from income and bargaining effects gives a good indication about the perception by beneficiaries of the impact of increased schooling on human capital acquisition and hence of the impact of a conditional transfer program on breaking the intertemporal poverty linkage. An evaluation relying on the impact of the program on schooling would need to be based on some assumption about similar returns to education across individuals.[2]

We use data from the evaluation of a recent conditional transfer program in Mexico, *Progresa*, to estimate the conditionality effect of the program on

[1] As opposed to what seems to be a common implicit assumption in the empirical literature, the assumption that the marginal utility of child goods is larger for the mother than for the father is not sufficient for this result. (See also Bergstrom [10].)

[2] Such an evaluation of conditional transfers in Mexico has been carried out by Schultz [29] and by Behrman et al. [8]. Ultimately, an evaluation of the effects of demand-side intervention will need to rely on the ex post impact on earnings of the cohort that benefited from the program, as in Duflo's [17] evaluation of a supply-side intervention in Indonesia.

intra-household allocation. *Progresa* provides monetary and in-kind transfers to mothers in very poor families in exchange for regular attendance of their children to school and periodic medical check-ups of children and adults. The objective of the empirical analysis is to distinguish the price or conditionality effect from the income and bargaining effects of the program on time devoted to children and household expenditure on child goods, which are in the model complementary with human capital accumulation, and on household expenditure on adult goods, which are substitutes.

Our empirical analysis aims at a sample of households for which the income and bargaining effects of the program are similar but which face different schooling subsidies at the margin. We take advantage of previous studies (Schultz [29], Sadoulet and de Janvry [27]), which have shown little impact of *Progresa* grants on enrollment at the primary level (due to the very high enrollment rates of children in primary school prior to the implementation of the program) but strong impacts at the secondary level. We thus clasify education grants for children in primary school as unconditional income, and education grants for children in secondary school as conditional income. We also take advantage of a design feature of *Progresa*, which limits total benefit amounts per family to a maximum level. We assume that the income and bargaining effects of the program are similar across families that could potentially receive the maximum level of benefits. Restricting attention to these families, we estimate how investment in inputs to child human capital vary with the proportion of *Progresa* benefits which are conditional to schooling. We make use of the experimental design of *Progresa* to difference out unobservable aspects of the family correlated with conditional income.

Our results are supportive of the hypothesis that larger schooling subsidies at the margin lead families to spend a larger share of resources on their children, beyond what is directly required to satisfy conditionality. In particular, price effects seem to account for a large fraction of the total impact of the program on the increase in the expenditure share of girls' clothing and on the decrease in boys' labor force participation. Moreover, while the total effect of the program is negative on time spent doing (school) homework, the price effect is either positive or close to zero, suggesting that it is strong enough to counter for a change in the composition of the sample of school children, toward children with a larger opportunity cost of time.

A number of empirical papers have been motivated recently by *Progresa*. Attanasio and Lechene [3] and Rubalcava, Teruel and Thomas [25] use the program database to provide further evidence of the existence of bargaining effects of transfers directed to the mothers. We differ from them by trying to ascertain the importance of the conditionality effects of the program. Davis et al. [15, 26] focus on the distinction between the effects of *Progresa* versus *Procampo*, an agricultural subsidy program in which the beneficiary is very rarely the mother. Attanasio, Meguir and Santiago [2] and Todd and Wolpin [33] have done structural estimations of the impact of *Progresa* on schooling in dynamic models in which the family acts as a single agent. Allocation of family resources to market goods is not a concern in either work. Todd and Wolpin [33], however, perform a counterfactual exercise comparing conditional and unconditional transfers which concludes that the effect of conditionality on schooling is very strong.

In previous work [20], we analyzed the welfare effects for household members of school subsidies under the assumption of Nash bargaining. Our current framework is not restricted to Nash bargaining, allows for changes in the bargaining power of adults as a result of subsidies, and incorporates the household production of human capital. Apps and Rees [1] and Chiappori [14] have combined before the domestic production and the collective household approaches, but they do not deal with household "public goods" such as children's consumption and human capital. Finally, Behrman et al. [6] develop a model where the human capital for the child is produced domestically using maternal time, child time and school goods under some simplifying assumptions, such as loglinear utilities and perfect complementarity in production. The focus there is on the possibility of maternal schooling augmenting the effect of maternal time on the production of human capital.

2 The Model

2.1 Household Production and Bargaining

Consider a household consisting of a mother, a father and a child, respectively A, B, and C. The adults' utility functions are separable in four basic commodities, A's consumption (Z_A), B's consumption (Z_B), the child's current

consumption (Z_C), and the child's human capital (H). The adults' utility functions are given by

$$U_A = U_A(Z_A, Z_C, H) \text{ and } U_B = U_B(Z_B, Z_C, H).$$

Note that child's consumption and human capital are "public goods" from the point of view of the adults.

There are m market goods. Each household member's consumption is produced domestically using a vector of market goods and a fraction of his or her time:

$$Z_A = Z_A(x_A, t_A),\ Z_B = Z_B(x_B, t_B),\ Z_C Z_C(x_C, t_C),$$

where $x_A \in \Re_+^m$ is the vector of market goods used in the production of Z_A and t_A is the time devoted by A to the production of Z_A, and similarly for the other terms.

The child's human capital, in turn, is produced domestically using market goods and the time of each household member:

$$H = H(x_H, h_A, h_B, h_C, e),$$

where x_H is the vector of market goods used in the production of H, h_M is the time devoted by $M = A, B, C$ to the production of human capital, and e is the time spent by the child in formal education.

The endowment of each household member is T units of time, that can be devoted to the activities mentioned before or to earn a wage in the labor market. (Note that this recognizes the existence of child labor.) We normalize to one the prices of market goods and the wages that the members of the household can earn in the labor market. The household receives an unconditional transfer s_0 and a subsidy rate $0 \leq s < 1$ to formal education. Thus, the household budget constraint is given by

$$\begin{aligned} &1 \cdot (x_A + x_B + x_C + x_H) \leq \\ &\quad (T - t_A - h_A) + (T - t_B - h_B) + (T - t_C - h_C - e) + s_0 + se. \end{aligned}$$

As in the collective household approach, we assume that the household decision is the result of maximizing

$$\mu(s_0, s)U_A + (1 - \mu(s_0, s))U_B$$

subject to the household budget constraint. The term μ is the "bargaining power" of A, which is a function of s_0 and s. In principle, it depends not only on transfers and subsidies but also on other prices and wages, but we keep these constant throughout. More generally, if we introduce in the model unearned income, we can allow bargaining power to be sensitive to the unearned income of A and B. The function μ is nondecreasing in both arguments; the idea is that transfers and subsidies are paid to the mother so they cannot possibly reduce and may actually increase her bargaining power.

Under the assumption that the production of the basic commodities exhibits constant returns to scale, we can define "commodity prices" π_A, π_B, π_C, and $\pi_H(s)$ to be equal to the unitary cost of production of Z_A, Z_B, Z_C and H, respectively. We write π_H as a function of s because the unitary cost of production of human capital depends on the school subsidy:

$$\begin{aligned} \pi_H(s) \quad = \quad & \min_{x_H, h_A, h_B, h_c, e} x_H + h_A + h_B + h_c + (1-s)e \\ & \text{s.t.} \;\; H(x_H, h_A, h_B, h_c, e) = 1. \end{aligned}$$

Note that the commodity prices depend on the household production functions, and thus they are potentially different across families. Provided that in every solution of the household decision problem each member devotes some time to the labor market, we can reduce the household problem to

$$\begin{aligned} & \max_{Z_A, Z_B, Z_C, H} \mu(s_0, s) U_A(Z_A, Z_C, H) + (1 - \mu(s_0, s)) U_B(Z_B, Z_C, H) \\ & \text{s.t.} \;\; \pi_A Z_A + \pi_B Z_B + \pi_C Z_C + \pi_H(s) H \leq 3T + s_0. \end{aligned}$$

Assuming that the utility functions of the parents are continuous, quasiconcave, and strictly increasing in own consumption and (jointly) in the consumption and human capital of the child, and that the marginal utility of each argument grows unboundedly as the value of the argument goes to zero, the program has an interior solution. We assume further that the solution is unique, that is, that family demand functions exist for any (strictly positive) commodity price vector.

2.2 Price, Income, and Bargaining Effects

In the framework described above, consider two households that are identical in every respect, except that one faces the social policy s_0', s' and the

other faces the policy s_0'', s''. The difference between the allocations chosen by the first and the second households can be split into three components, corresponding to a "bargaining effect," an "income effect" and a "price effect." The bargaining effect corresponds to a movement along the budget hyperplane of the first family as a result of substituting the "new" household preferences, given by $\mu(s_0'', s'')$ for the "old" household preferences, given by $\mu(s_0', s')$. The magnitude of the bargaining effect is directly related to the impact of change in social policy on the bargaining power of the mother. The empirical work of Schultz [28], Thomas [31], and others suggests that an increase in the power of the mother within the household has a positive impact on spending on women's and children's consumption. As noted by Bergstrom [10], to obtain a positive bargaining effect on the family public goods (Z_C and H) requires more than the marginal utility of the public goods being higher for A than for B. The examples in subsection 2.2 illustrates a set of circumstances under which the bargaining effects are as conjectured.

The price effect is a movement along an indifference curve given by the "new" household preferences. Since the subsidy reduces the price of human capital, an increase in the subsidy rate will have a positive price effect on H. It seems likely that the price effect will be positive on the consumption of the child and negative on the consumption of the adults. The reason for this conjecture is that child consumption and human capital may very well be complementary from the viewpoint of the preferences of both adults in the household. Mulligan [22] argues that parents who devote more resources to children at present become more altruistic toward them and more willing to make further sacrifices toward their future welfare. Turning this argument around, if parents expect children to be better off in the future, they will have added incentives to become altruistic. Or, after Becker [5], parents who expect children to have more human capital and hence a larger income in the future will devote more resources to them at present to foster altruism toward parents in them.

The income effect is a movement along the income expansion path associated to the new household preferences and the new commodity prices, given by $\pi(s'')$. The income effect is likely to be positive on all basic commodities, as it seems natural to expect own consumption, consumption of the child and human capital to be normal goods for both adults.

The objective of the empirical analysis in the next section is to estimate the price effect as a function of the the subsidy rate. This is done by considering a sample of households which face different subsidy rates, but for which the maximum possible benefit is constant. In terms of our model, benefits offered to these households satisfy the constraint that $s_0 + sT'$ is constant, where $T' < T$ is the maximum time that the child can dedicate to formal schooling. This is only an approximation to the extent that the maximum refers to monthly payments while the model deals with lifetime benefits, which may differ for households with children of different age. Also, we ignore possible issues arising from the fact that the households in the sample have several children, as we cannot distinguish empirically between resources allocated to different children, save for the child's time use.

The idea of the empirical analysis is the following. Consider a household for which the subsidy rate is large enough for it to dedicate T' units of time to formal schooling. As s is reduced and s_0 increased to keep $s_0 + sT'$ constant, it is reasonable to expect the bargaining power of the mother to either remain approximately constant or to increase, since unconditional income is substituted for conditional (potential) income. Moreover, a reduction in s compensated by an increase in s_0 that keeps the older allocation (in terms of market commodities and time use) in the budget constraint is equivalent to estimating the (Slutsky) compensated price effect for reductions in the subsidy rate. This is a good local approximation, though it underestimates the "true" (Hicks) price effects for normal goods. Thus, our empirical exercise can be considered a conservative estimation of the price effect for resources allocated to children and for resources allocated to the father of the family, to the extent that in both cases the price and the bargaining effect operate in the same direction. For large variations of the subsidy rate, our exercise most likely underestimates price effects for every household commodity due to the imperfect account of the income effect.

2.3 Examples

A Cobb-Douglas Family. Let the preferences of the parents be given by

$$U_A(Z_A, Z_C, H) = (1 - \delta_A) \log Z_A + \delta_A \log \min\{Z_C, H\},$$
$$U_B(Z_B, Z_C, H) = (1 - \delta_B) \log Z_B + \delta_B \log \min\{Z_C, H\},$$

with $0 < \delta_B < \delta_A < 1$, and let $T = 1/3$. Suppose the social policy changes from s_0', s' to s_0'', s''. Price and income effects are easily calculated and have the appropriate signs. In particular, the price effect on the consumption of the child and on human capital is

$$\frac{\delta(s_0'', s'')}{\pi_C + \pi_H(s'')} \left(1 - \left(\frac{\pi_C + \pi_H(s'')}{\pi_C + \pi_H(s')} \right)^{1-\delta(s_0'', s'')} \right) (1 + s_0'),$$

where $\delta(s_0, s) = \mu(s_0, s)\delta_A + (1 - \mu(s_0, s))\delta_B$. The above expression is positive if and only if $s'' > s'$.

Bargaining effects also have the appropriate signs; in particular, the bargaining effect of the change in policy on the consumption of the child and on human capital is

$$\left(\frac{\delta_A - \delta_B}{\pi_C + \pi_H(s')} \right) (\mu(s_0'', s'') - \mu(s_0', s'));$$

this expression is positive if and only if $\mu(s_0'', s'') > \mu(s_0', s')$. ■

A CES Family. More generally, let the preferences of the parents be given by

$$U_A(Z_A, Z_C, H) = (1 - \delta_A) Z_A^q + \delta_A [\min\{Z_C, H\}]^q,$$
$$U_B(Z_B, Z_C, H) = (1 - \delta_B) Z_B^q + \delta_B [\min\{Z_C, H\}]^q$$

for $0 < q < 1$ and

$$U_A(Z_A, Z_C, H) = -(1 - \delta_A) Z_A^q - \delta_A [\min\{Z_C, H\}]^q,$$
$$U_B(Z_B, Z_C, H) = -(1 - \delta_B) Z_B^q - \delta_B [\min\{Z_C, H\}]^q$$

for $q < 0$, with the case $q = 0$ given by the Cobb-Douglas example above, with $0 < \delta_B < \delta_A < 1$ and $T = 1/3$. The signs of price and income effects correspond to those conjectured in the description of the model. Interestingly, however, increasing the bargaining power of the mother may *reduce* the consumption and human capital of the child. Note that the objective function of the family is also CES. Using the well-known CES demand function, we can obtain the consumption of the child and human capital as a function of the bargaining power of the mother for a given commodity price vector:

$$Z_C(\mu) = H(\mu) = (\mu\delta_A + (1 - \mu)\delta_B)^{1/(1-q)} (\pi_C + \pi_H))^{1/(q-1)} \Gamma(\mu)^{-1}$$

where

$$\begin{aligned}\Gamma(\mu) &= (\mu(1-\delta_A))^{1/(1-q)}\pi_A^{q/(q-1)} \\ &\quad +((1-\mu)(1-\delta_B))^{1/(1-q)}\pi_B^{q/(q-1)} \\ &\quad +(\mu\delta_A+(1-\mu)\delta_B)^{1/(1-q)}(\pi_C+\pi_H)^{q/(q-1)}.\end{aligned}$$

The relationship between child consumption (and human capital) and the bargaining power of the mother is inverted-U shaped if $q > 0$ and U-shaped if $q < 0$. To see this, differentiating with respect to μ, we obtain that the effect of a marginal increase in μ is positive when $q > 0$ if and only if

$$\mu < \left(1+\frac{\pi_B}{\pi_A}\frac{\delta_A}{\delta_B}\left(\frac{\delta_B/(1-\delta_B)}{\delta_A/(1-\delta_A)}\right)^{1/q}\right)^{-1}.$$

When $q > 0$, an increase in the bargaining power of the mother has a positive effect if the bargaining power of the mother in the initial situation is small enough. But when $q < 0$, the inequality is *reversed*, i.e. the bargaining effect is positive only if the initial bargaining power of the mother is large enough. If q is close to zero then the bargaining effect is positive for all μ except very close to 1 (if $q > 0$) or very close to 0 (if $q < 0$). But if q is negative and far from zero the inequality becomes more stringent. This means that an elasticity of substitution much smaller than one (implying small price effects) cannot be reconciled with the assumptions that the family chooses a Pareto allocation, that the initial bargaining power of the mother is small and that bargaining effects are positive on child consumption and human capital. ■

3 Empirical Analysis

3.1 Data

The data we use comes from the evaluation effort of *Progresa*, the main anti-poverty program of the Mexican Government. This program provides cash grants to poor families in exchange for these families sending children to school regularly and fulfilling a schedule of family health clinic visits. The cash grants are given directly to the mother of the family. Grant amounts for the first semester of 1999 are detailed in Table 1. On average, *Progresa*

benefits represent about 20% of the value of family consumption prior to the program (Skoufias [30]). In the original program design, *Progresa* provides grants linked to children in third through ninth grade. Since 2001, after the data we use was collected, *Progresa* (re-christened as *Oportunidades*) extended educational grants to the high school level. Maximum total monthly transfers per family were restricted in the first semester of 1999 to 695 pesos; this feature of the program design is crucial for our estimation procedure.

TABLE 1. TRANSFERS FROM PROGRESA[a]

Scholarships (monthly transfers per child)

Level	Boys	Girls
Elementary		
Third	75	75
Fourth	90	90
Fifth	115	115
Sixth	150	150
Junior High		
First	220	235
Second	235	260
Third	245	285

Nutrition grants (monthly transfers per family)[b]

115

[a] At first semester 1999. All amounts in Mexican pesos (10 pesos ≈ US$ 1). Maximum monthly transfer per household: 695. Actual mean monthly payment for families eligible for maximum transfer: 448.

[b] 'Nutrition grants' are conditional on family health clinic visits and there is no explicit or implicit monitoring of spending on food.

In 1997, at the start of the program, *Progresa* carried out a social experiment in which a random sample of 506 rural eligible communities were selected in the seven Mexican states where the program was first implemented. 320 communities were assigned to receive benefits (the treatment group) and the remaining 186 were assigned to a control group that would receive benefits about two years later, at the beginning of the year 2000. All households in the treatment and control communities (a total of 24,077) were interviewed prior to implementation of the program. The baseline household census (ENCASEH97) was collected in November 1997. Behrman and Todd [9] analyze the distribution of household characteristics between the control

and the treatment group, and conclude that there are some very small though statistically significant differences. Table 2 uses information from the baseline census to show that, prior to implementation, households potentially eligible for maximum monthly benefits in the control and treatment group were fairly similar. Note that all but two of the t-tests for differences between households in the two groups are insignificant at the 10% level.

TABLE 2. HOUSEHOLD CHARACTERISTICS

Households eligible for maximum benefits in control and treatment group (1997)

	Control		Treatment		t-Test
	Mean	Std. dev.	Mean	Std. dev.	Pr(equality)
Total income	1454	1397	1484	1643	0.694
School attendance:					
children aged 8 to 11	0.989	0.102	0.983	0.129	0.195
children aged 12 to 16	0.644	0.479	0.671	0.470	0.085
Land owned	2.12	3.00	2.25	5.06	0.566
Water	0.06	0.23	0.06	0.24	0.634
Electricity	0.72	0.45	0.70	0.46	0.275
Dirt floor	0.68	0.47	0.67	0.47	0.641
Head's age	45.37	9.70	44.96	9.68	0.401
Spouse's age	36.79	13.69	36.96	13.69	0.814
Head's years of schooling	2.24	2.09	2.37	2.13	0.235
Spouse's years of schooling	1.88	2.11	1.99	2.22	0.336
Household size	8.52	2.02	8.63	2.11	0.299
Indigenous head	0.35	0.48	0.33	0.47	0.300
Indigenous spouse	0.31	0.47	0.30	0.47	0.691
Boys aged 0-2	0.22	0.45	0.25	0.50	0.108
Boys aged 3-5	0.36	0.55	0.35	0.54	0.587
Boys aged 6-8	0.47	0.61	0.50	0.62	0.328
Boys aged 9-11	0.58	0.64	0.58	0.65	0.973
Boys aged 12-14	0.68	0.67	0.69	0.70	0.741
Boys aged 15-18	0.64	0.67	0.69	0.71	0.150
Boys aged 0-18	2.95	1.46	3.07	1.52	0.126
Girls aged 0-2	0.21	0.45	0.25	0.51	0.095
Girls aged 3-5	0.30	0.52	0.29	0.50	0.864
Girls aged 6-8	0.47	0.62	0.46	0.59	0.889
Girls aged 9-11	0.64	0.68	0.61	0.67	0.369
Girls aged 12-14	0.69	0.68	0.68	0.67	0.870
Girls aged 15-18	0.63	0.71	0.68	0.72	0.162
Girls aged 0-18	2.93	1.50	2.97	1.55	0.541

Households in the treatment group began to receive benefits in March 1998. Follow-up interviews (ENCEL) have been carried out every semester; the October 1998, May 1999, and October 1999 rounds are available at present. We use the May 1999 round because, unlike the other two, it has information on time use and does not coincide with the beginning of the school year. Patterns of spending at the beginning of the school year are likely to be distorted because spending on child clothing includes school uniforms, which may be mandatory in some schools. Moreover, at the beginning of the school year children in primary receive in-kind supplies, whereas secondary school children are given a fixed cash amount to be used to buy school supplies. Table 3 provides descriptive statistics of the variables of interest for the control group in 1999, which we assume in the analysis below can represent pre-program levels for the treatment group.

TABLE 3. DESCRIPTIVE STATISTICS

Households eligible for maximum benefits in control group (1999)

	Mean	Std. dev.
Expenditure shares (percent)		
Food	67.85	18.28
Boys' clothing	2.46	3.41
Girls' clothing	2.27	3.30
Men's clothing	1.61	2.64
Women's clothing	1.29	1.99
Transport	3.73	8.44
Time devoted to homework (daily minutes)		
Children aged 8 to 11	58.80	36.12
Children aged 12 to 16	64.48	43.25
School attendance (percent)		
Children aged 8 to 11	0.92	0.27
Children aged 12 to 16	0.70	0.46
Labor force participation (percent)		
Boys aged 8 to 11	0.03	0.18
Boys aged 12 to 16	0.27	0.44
Girls aged 8 to 11	0.02	0.12
Girls aged 12 to 16	0.08	0.28

3.2 Empirical Specification

The objective of the empirical tests is to estimate the impact of school subsidies on resources allocated to different family members for households that are potentially eligible for maximum monthly benefits. As argued in the previous section, both income and bargaining effects should be roughly similar for households in the treatment group in this sample, so the estimation should capture the price effect of subsidies.

To compute a proxy for the school subsidy at the margin of decision of the family, we take the nutrition grants and the grants for children at the primary level to be "unconditional income," and then define the difference between unconditional income and the total maximum benefit, which is due to secondary school grants, as "conditional income." We use conditional income as a proxy for school subsidies. Dealing with primary grants as unconditional income is consistent with previous studies of the program impact, which have shown little or no effect of *Progresa* on enrollment at the primary level (where enrollment is very high anyway), and large effects on enrollment at the secondary level. Note that both conditional and unconditional income refer to potential benefits and not to actually collected benefits. We calculate conditional income using the ages and schooling levels of children in 1997, just prior to program implementation. For instance, children who would be eligible for a secondary school grant in our year of analysis are defined as those children who had between five and seven years of completed schooling in the fall of 1997, and thus could potentially be enrolled in secondary school (7th through 9th grade) in May 1999. In this way, our definition of conditional income is exogenous to the program.

As proxies for household resources allocated to the different family members, we use spending on boys' and girls' clothing and on adult clothing, time devoted by children to school related homework, and child labor. Clothing is an example of a market good whose allocation inside the household is easily discernible, so that variations in spending on child clothing, in adult female clothing, and in adult male clothing can serve as proxies for variations in the vector of market goods used in the production of Z_C, Z_A, and Z_B, respectively. Given the assumed complementarity between Z_C and H, we expect the price effect of school subsidies to be positive on spending in child clothing and negative on spending in adult clothing. Using data on

expenditure shares, rather than spending, has the advantage of reducing the importance of income effects. With homothetic preferences (as in the examples above), and due also to the assumption of constant returns to scale in home production, income effects on expenditure shares are actually zero. In terms of time use, if there is not much substitution between the inputs in the production of human capital, we expect time devoted to the children's human capital to move jointly with H. Thus, the prediction is that the price effect is positive on schooling and time devoted to school related homework, and in consequence negative on child labor.

We estimate the following regression at the household level:

$$S_{hk} = \alpha_{0k} + \alpha_{1k}P_h + \alpha_{2k}D_h + \alpha_{3k}P_hD_h + \textstyle\sum_j \beta_{jk}X_{hj} + \epsilon_{hk},$$

where S_{hk} refers to indicator k (spending shares of adult male clothing, adult female clothing, boys' clothing and girls' clothing) for family h, P_h represents the proportion of conditional income for household h, D_h is an indicator of whether the household is in the treatment group, X_{hj} represents the control variable j for household h (household demographics and household expenditure), and ϵ_{hk} is an error component reflecting unobserved characteristics. We estimate a similar regression at the children level, substituting i for h. In this case, S_{ik} refers to indicator k (time devoted to school homework, school attendance, child labor) for individual i, P_i represents the proportion of conditional income for i's household, D_i is an indicator of whether i's household is in the treatment group, and X_{ij} represents the control variable j for i's household. The treatment group dummy intends to capture program effects that have not been explicitly modelled; for instance, women are required to attend monthly health lectures, which seem to have had an impact on expenditure in food.

Note that α_{3k}, the coefficient of interest, is a double difference estimator. It allows us to test whether the effect of potential conditional income is different for households in the treatment group than for those in the control group. If our sample included only households in the treatment group, the coefficient on conditional income might be capturing unobservable variables also correlated with our indicators of interest. For instance, households with a larger proportion of conditional income may have children of different abilities as evidenced by their previous overall achievement (and thus years of

completed schooling) in school. We carried out similar regressions restricting our attention to the treatment group. The estimated effects are similar to (and often larger than) those reported in the text.

3.3 Results

We begin by demonstrating that, in fact, the effects of *Progresa* on enrollment at the primary school level are much smaller than those at the secondary level. Table 4 replicates the double difference estimators carried out by Schultz [29] for two groups, children aged 8 to 11 and children aged 12 to 16, corresponding approximately to primary school (3rd through 6th grade, which is when grants can be received in primary) and secondary school (7th through 9th grade).

Table 4. Program impact on enrollment

	Pre-program level	Impact (percent)	
	Nov. 1997	Nov. 1998	Nov. 1999
All households			
Children aged 8 to 11	0.972	0.013	0.01
[9788]		(0.004)***	(0.004)***
Children aged 12 to 16	0.597	0.073	0.08
[9698]		(0.014)***	(0.017)***
Households eligible for maximum benefits			
Children aged 8 to 11	0.975	0.011	0.004
[2757]		(0.006)*	(0.007)
Children aged 12 to 16	0.537	0.063	0.054
[3819]		(0.021)***	(0.028)**

Observations in brackets
Standard errors in parenthesis
*Significant at 10%; **Significant at 5%; ***Significant at 1%

Table 4 confirms higher impacts on secondary school enrollment for both the entire sample and the sample of households eligible for the maximum

benefit. Note that, at the secondary level, impacts are somewhat lower for households eligible for the maximum benefit, which is consistent with the idea that families react to the reduced marginal incentive to send additional children to school.

We turn now to our empirical tests. Table 5 presents the effect of the proportion of conditional income on various categories of expenditures shares for families with children aged 8 to 17. If our hypotheses are correct, conditional income should show differing effects on spending on goods which are plausibly substitutes and complements with investment in children's human capital.

TABLE 5. IMPACT OF CONDITIONAL INCOME ON EXPENDITURE SHARES

Households eligible for maximum benefits with children aged 8 to 17

	Food	Boys' clothing	Girls' clothing
conditional income	-0.06421	0.01624	0.02018
× treatment group	(0.04842)	(0.01078)	(0.01045)*
conditional income	0.0052	-0.00271	-0.00904
	(0.04213)	(0.00938)	(0.00909)
R^2	0.08	0.06	0.11

	Women's clothing	Men's clothing	Transport
conditional income	0.00712	0.00182	0.00372
× treatment group	(0.00655)	(0.00817)	(0.02183)
conditional income	-0.00864	-0.002	0.01017
	(0.00570)	(0.00711)	(0.01900)
R^2	0.04	0.04	0.05

Observations: 1596
Standard errors in parenthesis
*Significant at 10%; **Significant at 5%; ***Significant at 1%

Regressions include a dummy for treatment group and controls for age, sex, and education of household head and spouse, a dummy indicating whether they speak an indigenous language, number of boys and girls by age groups (0-2, 3-5, 6-8, 9-11, 12-14, 15-18), dummies for water and electricity access and for dirt floor, land (has.), and total household expenditures and its square.

The results are somewhat supportive of strong price effects. The estimated coefficient of conditional income on the proportion of resources dedicated to spending on both boys' and girls' clothing is positive and significant for girls. An increase in the share of conditional income of about 10% (corresponding to an increase of about 35 pesos or $ 3.5US to conditional income and a corresponding decrease in unconditional income) would increase the share of spending on girls' clothing by 0.2, or an increase of 9% from pre-program levels. The results on adult male and female clothing are positive but insignificant.

Turning to child level variables, we consider first the effect of conditional income on time spent by children doing school homework. We distinguish here between children of primary and secondary school age. The effects may be different for these two groups, since *Progresa* may change the composition of children attending school, given the large impact on enrollment in secondary school. If those children who return to or continue their schooling with *Progresa*, but who otherwise would have dropped out, are "worse" students who spend less time studying, then one might observe that *Progresa* reduces the average time spent doing homework.[3] Table 6 presents both OLS and tobit estimations, although more than 90% of school children report doing homework. For primary school children, the results show a positive and significant effect. According to the estimations, an increase of 10% in the proportion of conditional income would correspond to an approximate increase of 2 minutes per day doing homework. In terms of pre-program levels, this would be an increase of about 3.4%. The results for secondary school children are negative though insignificant, quite possibly reflecting the composition change in this group described above.

Finally, we consider child labor. We restrict our attention to boys aged 12 to 16, since labor force participation for boys aged 8 to 11 and for girls in general is quite low, as illustrated by Table 3. The probit results in Table 7 show that conditional income has a negative and statistically significant effect on the probability that boys in secondary school age participate in the labor market. An increase in 10% in the proportion of conditional income would reduce the labor force participation of this group by about 2.2 percentage points, implying a decrease of about 8.7% from pre-program levels.

[3] A similar observation is made by Behrman et al. [7].

Table 6. Impact of conditional income on time devoted to school homework

Children aged 8 to 11 in households eligible for maximum benefits (I)

	OLS	Tobit			
		Latent variable	Uncond. exp. value	Cond. on censored	Prob. uncensored
conditional income × treatment group	0.19728 (0.09785)**	0.19288 (0.10338)*	0.1828	0.1551	0.0006
conditional income	0.03671 (0.08297)	0.05318 (0.08756)	0.0504	0.0428	0.0002
R^2	0.03				

Children aged 12 to 16 in households eligible for maximum benefits (II)

	OLS	Tobit			
		Latent variable	Uncond. exp. value	Cond. on censored	Prob. uncensored
conditional income × treatment group	-0.02603 (0.11098)	-0.02594 (0.11790)	-0.0246	-0.0210	-0.0001
conditional income	0.18206 (0.09917)*	0.20652 (0.10532)*	0.1960	0.1668	0.0006
R^2	0.05				
Test I=II					
p-value	0.12996	0.16196			

Observations: 1524 (I) and 1395 (II)
Standard errors in parenthesis
*Significant at 10%; **Significant at 5%; ***Significant at 1%

Regressions include same controls as Table 5 (treatment group dummy, household characteristics, household expenditure) and age of the child.

Table 7. Impact of conditional income on labor force participation

Boys aged 12 to 16 in households eligible for maximum benefits

conditional income	-0.00221
× treatment group	(0.00134)*
conditional income	0.00357
	(0.00120)***

Observations: 1601
Standard error in parenthesis
*Significant at 10%; **Significant at 5%; ***Significant at 1%
Regression includes same controls as Table 6.

3.4 Price Effects versus Total Effects

How large is the conditionality effect relative to the total program effect? To answer this question, we estimate the total program effect on household expenditure and children's time use using the sample of households eligible for the maximum amount of benefits. The results are reported in Tables 8, 9 and 10, which are comparable with Tables 5, 6 and 7, respectively. Looking first at Table 8, the total effect of the program on the household expenditure share of boys' and girls' clothing is about 0.9. Recall that our estimates suggested that an increase of 10 percentage points in the proportion of conditional income would increase spending on girls' clothing by about 0.2. Conditionality then would seem responsible for a large percentage of the impact.

TABLE 8. PROGRAM IMPACT ON EXPENDITURE SHARES

Households eligible for maximum benefits with children aged 8 to 17

	Food	Boys' clothing	Girls' clothing
treatment group	1.01661	0.79126	0.88445
	(0.89209)	(0.19659)***	(0.19087)***
R^2	0.06	0.06	0.11

	Women's clothing	Men's clothing	Transport
treatment group	0.32166	0.19167	-0.38230
	(0.11973)***	(0.14938)	(0.40413)
R^2	0.03	0.03	0.02

Observations: 1596
Standard errors in parenthesis
*Significant at 10%; **Significant at 5%; ***Significant at 1%

Regressions include same controls as in Table 5, except for household expenditure and its square.

With respect to time spent on school related homework, Table 9 shows that the total effect of the program is insignificant for children aged 8 to 11 and negative and significant for children aged 12 to 16. This is consistent with the composition change described in the the previous subsection. Children who would not have enrolled without *Progresa* are likely to have a larger opportunity cost of time, in relation to the expected returns of education, and hence spend less time studying. Thus, the impact estimate of *Progresa* includes both the direct effect of the program and a (negative) composition effect. It is remarkable that, in spite of the compositional bias downward revealed in Table 9, the conditionality effect was positive.

Finally, Table 10 reports that the labor force participation of boys aged 12 to 16 was reduced by 6 percentage points as a result of the program, according to a probit regression. Again, the conditionality effect seem to explain a large proportion of this effect. Recall that our estimates suggested that an increase in 10 percent in the proportion of conditional income would reduce the labor force participation of boys by about 2 percentage points.

Table 9. Program impact on time devoted to school homework

Children aged 8 to 11 in households eligible for maximum benefits (I)

	OLS	Tobit			
		Latent variable	Uncond. exp. value	Cond. on censored	Prob. uncensored
treatment group	-0.74701	-1.49986	-1.4211	-1.2054	-0.0046
	(1.78013)	(1.88338)			
R^2	0.02				

Children aged 12 to 16 in households eligible for maximum benefits (II)

	OLS	Tobit			
		Latent variable	Uncond. exp. value	Cond. on censored	Prob. uncensored
treatment group	-3.07389	-3.89418	-3.7004	-3.1555	-0.0106
	(2.08396)	(2.21482)*			
R^2	0.04				
Test I=II					
p-value	0.39367	0.42719			

Observations: 1524 (I) and 1395 (II)
Standard errors in parenthesis
*Significant at 10%; **Significant at 5%; ***Significant at 1%

Regressions include same controls as in Table 6, except for household expenditure and its square.

Table 10. Program impact on labor force participation

Boys aged 12 to 16 in households eligible for maximum benefits

treatment group	-0.06432
	(0.02563)**

Observations: 1601
Standard error in parenthesis
*Significant at 10%; **Significant at 5%; ***Significant at 1%

Regression includes same controls as previous table 6, except for household expenditure and its square.

It is noteworthy that our estimates of the total effect of the program are generally in agreement with those of previous studies, though previous studies have dealt with all households rather than with the sample of households eligible for the maximum benefit. For instance, with respect to children's clothing, Hoddinot et al. [18] report an overall increase in expenditures on children's clothing of about 49% in 1999. This corresponds to an increase of 1.1 in the expenditure share of children's clothing. With respect to labor force participation, Parker and Skoufias [23] report that the program reduces labor force participation for boys aged 8 to 17 in 1999 by about 2.5 percentage points, with the largest effects on boys aged 14 to 15 at 6 percentage points.

3.5 Robustness

Since the proportion of conditional income is correlated with the proportion of children in secondary school, it is conceivable that we overestimate price effects if, somehow, income and bargaining effects of the program are larger for secondary school than for primary school children. In fact, the correlation between our indicator of conditional income and the proportion of secondary school aged children is high (0.88), but not perfect, because grants vary according to school grade and sex, and because of the cap to maximum benefits. As a robustness test, we re-run the regressions reported in Tables 5 through 7, including additional variables measuring the proportion of secondary school aged children and its interaction with the treatment group dummy. In spite of the high co-linearity, we continue to estimate significant effects of conditional income for both spending on child clothing (positive) as well as child labor (negative). The relevant coefficient estimates in Tables 11 and 13 are slightly larger than those reported in Tables 5 and 7. Note that we obtain negative (though insignificant) impacts of conditional income on the share of spending on adult clothing. In the case of time use, reported in Table 12, we are unable to identify separately the effect of conditional income from the proportion of children eligible for secondary. Overall, these additional results suggest that, if at all different, bargaining and income effects are stronger for families with a larger proportion of children in primary so that our previous results may underestimate conditionality effects.

Table 11. Impact of conditional income vs children in secondary school age on expenditure shares (clothing)

Households eligible for maximum benefits with children aged 8 to 17

	Boys	Girls	Women	Men
conditional income	0.04898	0.03129	-0.01001	-0.01264
× treatment group	(0.02383)**	(0.02314)	(0.01449)	(0.01806)
children in secondary	-0.03319	-0.01125	0.01725	0.01480
× treatment group	(0.02139)	(0.02077)	(0.01300)	(0.01621)
R^2	0.07	0.11	0.04	0.04

Observations: 1596
Standard errors in parenthesis
*Significant at 10%; **Significant at 5%; ***Significant at 1%

Regressions include conditional income, proportion of children in secondary school age, a dummy for treatment, and same controls as in Table 5.

Table 12. Impact of conditional income vs children in secondary school age on school homework

	Children in households eligible for maximum benefits:	
	Age 8 to 11	*Age 12 to 16*
conditional income	0.07406	0.07989
× treatment group	(0.20111)	(0.24479)
children in secondary	0.14365	-0.11197
× treatment group	(0.20527)	(0.23137)
R^2	0.03	0.05

Observations: 1524 children aged 8 to 11 and 1395 children aged 12 to 16
Standard errors in parenthesis
*Significant at 10%; **Significant at 5%; ***Significant at 1%

Regressions include conditional income, proportion of children in secondary school age, a dummy for treatment, and same controls as in Table 6.

Table 13. Impact of conditional income vs children in secondary school age on labor force participation

Boys aged 12 to 16 in households eligible for maximum benefits

conditional income	-0.00542
× treatment group	(0.00311)*
children in secondary	0.00328
× treatment group	(0.00279)

Observations: 1601
Standard error in parenthesis
*Significant at 10%; **Significant at 5%; ***Significant at 1%
Regression includes same controls as Table 6.

4 Conclusions

In this paper, we propose a model to analyze the impact of school subsidies for the poor on intrahousehold allocations. If schooling is an input in the production of human capital, then school subsidies will reduce the shadow price of human capital accumulation and henceforth lead to a reallocation of household resources toward this activity. Thus, the model suggests that in order to assess the impact of school subsidies on human capital accumulation it may be useful to consider not only the direct impact on schooling but also the impact on household expenditure patterns and on the time use of family members. However, since programs of school subsidies for the poor typically involve monetary transfers to the mother of the family, the impact of the subsidies on household expenditures and time use patterns may be due to the increased bargaining power of the mother rather than to the impact of the school subsidies on the shadow price of human capital. This means that it is important to isolate bargaining from price effects of school subsidies. A simple CES example shows that, surprisingly enough, positive bargaining effects coupled with the assumption of efficient bargaining in the family imply strong price effects.

We use data from the evaluation of *Progresa*, a program of school subsidies for the poor in Mexico, and estimate the price effects of the program. Ideally, distinguishing between price and bargaining effects could be done by comparing the impact of the program on two groups of similar households, one receiving conditional transfers and one receiving unconditional transfers. Since that was not the social experiment conducted, we have tried to approximate it by exploiting the fact that the program establishes a cap to total monthly benefits per household that is binding for a sample of about 1600 families. We also exploit the fact that pre-program enrollment in primary school was already very high, so the relevant margin of decision for beneficiary families is whether to send (more of) their children to secondary school. Thus, we treat primary school grants as unconditional income, and we define conditional income as the remainder of the maximum benefit. Our estimates suggest a strong effect of the proportion of unconditional income to the maximum possible benefits over household expenditure patterns and time allocation of children. We interpret this as evidence that the school subsidies have had an impact on the shadow price of human capital, and that household resources beyond those directly subject to conditionality have been reallocated favorably to children's human capital.

Some of the important effects of school subsidies may have to do with intertemporal incentives for household allocation. For instance, it is hard to explain otherwise the positive and significant effect of conditional income on time spent by primary school children doing homework. A proper consideration of intertemporal incentives requires a dynamic framework beyond what we have attempted in this paper, and it seems an exciting avenue of research both theoretically and empirically.

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