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# Ex-ante Evaluation of Conditional Cash Transfer Programs: the Case of Bolsa Escola[1]

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**Abstract:** Cash transfers targeted to poor people, but conditional on some behavior on their part, such as school attendance or regular visits to health care facilities, are being adopted in a growing number of developing countries. Even where ex-post impact evaluations have been conducted, a number of policy-relevant counterfactual questions have remained unanswered. These are questions about the potential impact of changes in program design, such as benefit levels or the choice of the means-test, on both the current welfare and the behavioral response of household members. This paper proposes a method to simulate the effects of those alternative program designs on welfare and behavior, based on micro-econometrically estimated models of household behavior. In an application to Brazil's recently introduced federal *Bolsa Escola* program, we find a surprisingly strong effect of the conditionality on school attendance, but a muted impact of the transfers on the reduction of current poverty and inequality levels.

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## 1. Introduction

During the 1990s, a new brand of redistribution programs was adopted in many developing countries. Although local versions varied, programs such as *Food for Education* in Bangladesh, *Bolsa Escola* in Brazil, and *Progresa* in Mexico are all means-tested conditional cash transfer programs. As the name indicates, they share two defining features, which jointly set them apart from most pre-existing programs, whether in developing or developed countries. The first of these is the means-test, defined in terms of a maximum household income level, above which households are not eligible to receive the benefit.[3] The second is the behavioral conditionality, which operates through the requirement that applicant households, in addition to satisfying the income targeting, have members regularly undertake some pre-specified action. The most common such requirement is for children between 6 and 14 years of age to remain enrolled and actually in attendance at school. In Mexico's *Progresa*, additional requirements applied to some households, such as obligatory pre- and post-natal visits for pregnant women or lactating mothers.

The implementation of these programs have generated considerable interest, both in the countries where they took place and in the international academic and policy-making communities. Accordingly, a great deal of effort has been placed in evaluating their impact. There are two types of approach for evaluating the effects of these programs on the various aspects of household welfare that they seek to affect. *Ex-post* approaches consist of comparing observed beneficiaries of the program with non-beneficiaries, possibly after controlling for selection into the first or the second group if truly random samples are not available. An important literature has recently developed on these techniques and many applications to social programs have been made in various countries.[4]

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[3] For verification and enforcement reasons, the means-test is often specified in terms of a score based on responses to a questionnaire and/or a home visit by a social worker. In some countries, the score is 'calibrated' to be approximately equivalent to a pre-determined level of household income per capita. See Camargo and Ferreira (2001) for a discussion of the Brazilian case.

[4] This literature relies heavily on matching techniques, and draws extensively on the early work by Rubin (1977) and Rubin and Rosenbaum (1985). For a survey of recent applications, see Heckman and Vytlacil (2002). For a study of the effects of the *Food for Education* program in Bangladesh, see Ravallion and

*Ex-ante* methods consist of simulating the effect of the program on the basis of some model of the household. These models can vary widely in complexity and coverage. Arithmetic simulation models simply apply official rules to determine whether or not a household qualifies for the program, and the amount of the transfer to be made, on the basis of data commonly available in typical household surveys. More sophisticated models include some behavioral response by households.

Ex-ante and ex-post evaluation methods are complements, rather than substitutes. To begin with, they have different objectives. Ex-post methods are meant to identify the *actual* effects of a program on various dimensions of household welfare, by relying on the direct observation of people engaged in the program, and comparing them with those same dimensions in a carefully constructed comparison group, selected so as to provide a suitable proxy for the desired true counterfactual: "how would participants have fared, had they not participated?". In some sense, these are the only "true" evaluations of a program.

Even when comparison groups are perfectly believable proxies for the counterfactual, however, ex-post evaluations leave some policy-relevant questions unanswered. These questions typically refer to how impact might change if some aspect of the program design – such as the level of the means-test; the nature of the behavioral conditions imposed; or the level of the transfer benefits - changes. It is difficult enough to obtain an actual control group to compare with a single program design in reality. It is likely to be impossible to "test" many different designs in experimental conditions. Ex-ante methods are valuable tools exactly because it is easier to experiment on computers than on people. These methods are essentially prospective since they rely on a set of assumptions about what households are likely to do when faced with the program. They also permit direct counterfactual analysis of alternative programs for which no ex-post data can be available. Thus, they are indispensable when designing a program or reforming existing ones.

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Wodon (2000). A number of important studies of *Progresa* were undertaken under the auspices of the International Food Policy Research Institute (IFPRI). See, in particular, Parker and Skoufias (2000) and Schultz (2000).

Simulation models of redistribution schemes based on micro data sets are widely used in developed countries, especially to analyze the effect of the numerous and often complex cash transfer instruments found in those countries. Given the progress of direct cash transfers in developing countries, building the same type of models in developing countries may become necessary.[5] However, the specific behavioral conditionality that characterizes these programs requires modifications, and a focus on different aspects of household behavior. The present paper takes a step in that direction by proposing a simple *ex-ante* evaluation methodology for conditional means-tested transfer programs. We apply the method to the new federal design of *Bolsa Escola*, in Brazil, and we are concerned with both dimensions cited by the program administrators as their objectives: (i) the reduction of current levels of poverty and inequality; and (ii) the provision of incentives for the reduction of future poverty, through increased school enrollment among poor children today.

The paper is organized as follows. Section 2 describes the *Bolsa Escola* program, as it was launched at the federal level in Brazil in 2001. Section 3 presents the simple econometric model used for simulating the effects of the program. Given the conditionality of *Bolsa Escola*, this model essentially deals with the demand for schooling and therefore draws on the recent literature on child labor. The estimation of the model is dealt with in Section 4, whereas the simulation of program effects and a comparison with alternative program designs are discussed in Section 5. Section 6 concludes.

## 2. Main features of the Bolsa Escola program

The Brazilian national *Bolsa Escola* program, created by a law of April 2001 within the broader context of the social development initiative known as *Projeto Alvorada*, is the generalization at the federal level of earlier programs, which were pioneered in the Federal District and in the city of Campinas (SP) in 1995, and later

[5] See, for instance, Harding (1996). On the need for and difficulties with building the same type of models in developing countries, see Atkinson and Bourguignon (1991).

extended to several other localities.[6] The law of April 2001 made these various programs uniform in terms of coverage, transfer amounts and the associated conditionality. It also provided federal funding. Yet, the monitoring of the program itself is left under the responsibility of municipal governments.

The rules of the program are rather simple. Households with monetary income per capita below 90 Reais (R$)[7] per month – which was equivalent to half a minimum wage when the law was introduced - and with children aged 6 to 15 qualify for the *Bolsa Escola* program, provided that children attend school regularly. The minimum rate of school attendance is set at 85 per cent and schools are supposed to report this rate to municipal governments for program beneficiaries. The monthly benefit is R$15 per child attending school, up to a maximum of R$45 per household. Transfers are generally paid to the mother, upon presentation of a magnetic card that greatly facilitates the monitoring of the whole program.

The management of the program is essentially local. Yet, control will be operated at two levels. At the federal level, the number of beneficiaries claimed by municipal governments will be checked for consistency against local aggregate indicators of affluence. In case of discrepancy, local governments will have to adjust the number of beneficiaries on the basis of income per capita rankings. At the local level, the responsibility for checking the veracity of self-reported incomes is left to municipalities.

It is estimated that some ten million children (in six million households) will benefit from this program. This represents approximately 17 percent of the whole population, reached at a cost slightly below 0.2 percent of GDP. The latter proportion is higher in terms of household disposable income: 0.45 percent when using household income reported in the PNAD survey and 0.3 per cent when using National Accounts. Of course, this figure is considerably higher when expressed in terms of targeted households. Even so, it amounts to no more than 5 percent of the income of the bottom two deciles.

[6] Early studies of these original programs include Abramovay et. al. (1998); Rocha and Sabóia (1998) and Sant'Ana and Moraes (1997). A comprehensive assessment of different experiences with *Bolsa Escola* across Brazil can be found in World Bank (2001). There is much less written on the federal program, for the good reason that its implementation in practice is only just beginning. The description given in this section draws on the official Ministério da Educação website, at http://www.mec.gov.brhome/bolsaesc.
[7] Approximately US$ 30, at August 2002 exchange rates.

## 3. A simple framework for modeling and simulating Bolsa Escola

The effects of such a transfer scheme on the Brazilian distribution of income could be simulated by simply applying the aforementioned rules to a representative sample of households, as given for instance by the *Pesquisa Nacional por Amostra de Domicílios* (PNAD), fielded annually by the Brazilian Central Statistical Office (IBGE). This would have been an example of what was referred to above as 'arithmetic' simulation. Yet, for a program which has a change in household behavior as one of its explicit objectives, this would clearly be inappropriate. After all, *Bolsa Escola* aims not only to reduce current poverty by targeting transfers to today's poor, but also to encourage school attendance by poor children who are not currently enrolled, and to discourage evasion by those who are. Any ex-ante evaluation of such a policy must therefore go beyond simply counting the additional income accruing to households under the assumption of no change in schooling behavior. Simulating *Bolsa Escola* thus requires some structural modeling of the demand for schooling. This section presents and discusses the model being used in this paper.

There is a rather large literature on the demand for schooling in developing countries and the related issue of child labor. The main purpose of that literature is to understand the reasons why parents would prefer to have their kids working within or outside the household rather than going to school. Various motives have been identified and analyzed from a theoretical point of view,[8] whereas numerous empirical attempts have been made at testing the relevance of these motives, measuring their relative strength and evaluating the likely effects of policies.[9] The empirical analysis is difficult for various inter-related reasons. First, the rationale behind the decision on child labor or school enrollment is by itself intricate. In particular, it is an inherently intertemporal decision, and it will differ depending on whether households behave as a unitary model, or whether internal bargaining takes place. Second, it is difficult to claim exogeneity for most plausible explanatory variables, and yet no obvious instrument is available for

[8] See the well-known survey by Basu (1999) as well as the recent contribution by Baland and Robinson (2001).

[9] Early contributions to that literature include Rosenzweig and Evenson (1977), as well as Gertler and Glewwe (1990). For more recent contributions and short surveys of the recent literature see Freije and Lopez-Calva (2000), Bhalotra (2000). On policy see Grootaert and Patrinos (1999).

correcting the resulting biases. Third, fully structural models that would permit a rigorous analysis of policies are complex and therefore hard to estimate while maintaining a reasonable degree of robustness.

In light of these difficulties, our aims are modest and our approach is operational: rather than proposing a new, more complete structural model of the demand for schooling and intra-household labor allocation, we aim simply to obtain reasonable orders of magnitude for the likely effects of transfer programs of this kind. We thus make the choice to limit the structural aspects of the modeling exercise to the minimum necessary to capture the main effects of the program.

In particular, we make four crucial simplifying assumptions. First, we entirely ignore the issue of how the decision about a child's time allocation is made within the household. We thus bypass the discussion of unitary versus collective decision-making models of household. Instead, we treat our model of occupational choice as a reduced-form reflection of the outcome of whichever decision-making process took place within the household.[10] Second, we consider that the decision to send a child to school is made after all occupational decisions by adults within the household have been made, and does not affect those decisions. Third, we do not discuss here the issue of various siblings in the same household and the simultaneity of the corresponding decision. The model that is discussed thus is supposed to apply to all children at schooling age within a household. Fourth, we take the composition of the household as exogenous.

Under these assumptions, let $S_i$ be a qualitative variable representing the occupational choice made for a child in household i. This variable will take the value 0 if the child does not attend school, the value 1 if she goes to school *and* works outside the household and the value 2 if she goes to school and does not work outside the household. When $S_i=0$, it will be assumed that the child works full time either at home or on the market, earnings being observed only in the latter case. Similarly, $S_i=2$ allows for the possibility that the child may be employed in domestic activities at the same time he/she goes to school. The occupational choice variable $S_i$ will be modeled using the standard utility-maximizing interpretation of the multinomial Logit framework, so that:

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[10] For a discussion of how intra-household bargaining affects the occupational choice of members, see Chiappori (1992). See also Bourguignon and Chiappori (1994) and Browning et. al. (1994).

$$S_i = k \text{ iff } S_k(A_i, X_i, H_i; Y_{-i} + y_{ik}) + v_{ik} > S_j(A_i, X_i, H_i; Y_{-i} + y_{ij}) + v_{ij} \text{ for } j \neq k \qquad (1)$$

where $S_k(\ )$ is a latent function reflecting the net utility of choosing alternative k (=0, 1 or 2) for deciders in the household. $A_i$ is the age of the child I; $X_i$ is a vector of her characteristics; $H_i$, is a vector of the characteristics of the household she belongs to - size, age of parents, education of parents, presence of other children at school age, distance from school, etc.; $Y_{-i}$ is the total income of household members other than the child and $y_{ij}$ is the total contribution of the child towards the income of the household, depending on her occupational choice j. Finally, $v_{ij}$ is a random normal variable that stands for the unobserved heterogeneity of observed schooling/participation behavior. If we collapse all non-income explanatory variables into a single vector $Z_i$ and linearize, (1) can be written as:

$$U_i(j) = S_j(A_i, X_i, H_i; Y_{-i} + y_{ij}) + v_{ji} = Z_i.\gamma_j + (Y_{-i} + y_{ij})\alpha_j + v_{ij} \qquad (2)$$

This representation of the occupational choice of children is very parsimonious. In particular, by allowing the coefficients $\gamma_j$ and $\alpha_j$ to differ without any constraints across the various alternatives, we are allowing all possible tradeoffs between the schooling of the child and his/her future income, and the current income of the household. Note also that the preceding model implicitly treats the child's number of hours of work as a discrete choice. Presumably that number is larger in alternative 0 than in alternative 1 because schooling is taking some time away. This may be reflected in the definition of the child income variable $y_{ij}$ as follows. Denote the observed market earnings of the child as $w_i$. Assuming that these are determined in accordance with the standard Becker-Mincer human capital model, write:

$$\text{Log } w_i = X_i .\delta + m^*\text{Ind}(S_j=1) + u_i \qquad (3)$$

where $X_i$ is a set of individual characteristics – including age and schooling achieved - and where $u_i$ is a random term that stands for unobserved earnings determinants. Assumptions on that term will be discussed below. The second term on the right hand side takes into account the preceding remark on the number of hours of work. Children who attend school and are also reported to work on the market presumably have less time available and may thus earn less. Based on (3), the child's contribution to the household income, $y_{ij,}$ in the various alternative j is defined as follows:

$$y_{i0} = Kw_i \; ; \quad y_{i1} = M\, y_{i0} = MKw_i \; ; \; y_{i2} = D\, y_{i0} = D\, Kw_i \quad \text{with } M = Exp(m) \qquad (4)$$

where it is assumed that $y_{ij}$ covers both market and domestic child labor. Thus domestic income is proportional to actual or potential market earnings, $w_i$, in a proportion K for people who do not go to school. Going to school while keeping working outside the household means a reduction in the proportion 1-M of domestic and market income. Finally, going to school without working on the market means a reduction in the proportion 1-D of total child income, which in that case is purely domestic. The proportions K and D are not observed. However, the proportion M is taken to be the same for domestic and market work and may be estimated on the basis of observed earnings.

Replacing (4) in (2) leads to :

$$U_i(j) = S_j(A_i, X_i, H_i; Y_{-i} + y_{ij}) + v_{ji} = Z_i.\gamma_j + Y_{-i}\,\alpha_j + \beta_j.w_i + v_{ij}$$

$$\text{with : } \beta_0 = \alpha_0\, K \; ; \quad \beta_1 = \alpha_1 MK; \;\; \beta_2 = \alpha_2\, DK \qquad (5)$$

We now have a complete simulation model. If all coefficients $\alpha$, $\beta$, $\gamma$ are known, as well as the actual or potential market earnings, $w_i$ and the residual terms $v_{ij}$ , then the child’s occupational type selected by household i is:

$$k^* = \text{Arg max}[U_i(j)] \qquad (6)$$

Equation (5) represents the utility of household i under occupational choice j $[U_i(j)]$ in the benchmark case. If the *Bolsa Escola* program entitled all children[11] going to school to a transfer T, (5) would be replaced by:

$$U_i(j) = \; Z_i.\gamma_j + (Y_{-I} + BE_{ij}).\alpha_j + \beta_j.w_i + v_{ij} \quad \text{with } BE_{i0}=0 \text{ and } BE_{i1} = BE_{i2} = T \qquad (7)$$

Under the assumptions we have made, equation (7) is our full reduced-form model of the occupational choice of children, and would allow for simulations of the impact of *Bolsa Escola* transfers on those choices. All that remains is to obtain estimates of $\beta$, $\gamma$, $\alpha$, $w_i$ and the $v_{ij}$'s.

[11] It will prove simpler to discuss the estimation problem under this simplifying assumption. We reintroduce the means test, without any loss of generality, at the simulation stage.

*Estimation of the discrete choice model*

Assuming that the $v_{ij}$ are iid across sample observations with a double exponential distribution leads to the well-known multi-logit model. However, some precautions must be taken in this case. It is well known that the probability that household i will select occupational choice k is given by:

$$p_{ik} = \frac{Exp(Z_i.\gamma_k + Y_{-i}\alpha_k + w_i.\beta_k}{\sum_j Exp(Z_i.\gamma_j + Y_{-i}\alpha_j + w_i.\beta_j)} \tag{8}$$

Taking regime j = 0 as a reference, the preceding probability may be written as:

$$p_{ij} = \frac{Exp\left[Z_i.(\gamma_j - \gamma_0) + Y_{-i}.(\alpha_j - \alpha_0) + w_i(\beta_j - \beta_0)\right]}{1 + \sum_{j=1}^{2} Exp\left[Z_i.(\gamma_j - \gamma_0) + Y_{-i}.(\alpha_j - \alpha_0) + w_i(\beta_j - \beta_0)\right]} \quad \text{for } j = 1, 2 \tag{9}$$

and $p_{i0} = 1 - p_{i1} - p_{i2}$ .

The difficulty is that the Multinomial logit estimation permits identifying only the differences ($\alpha_j$-$\alpha_0$), ($\beta_j$-$\beta_0$), and ($\gamma_j$-$\gamma_0$) for j = 1, 2. Yet, inspection of (6) and (7) indicates that – since the *Bolsa Escola* transfer is state-contingent, meaning that the income variable is asymmetric across alternatives - it is necessary to know *all three* coefficients $\alpha_0$ , $\alpha_1$ and $\alpha_2$ in order to find the utility maximizing alternative, k*.

This is where the only structural assumption made so far becomes useful. Call $\hat{a}_j$ and $\hat{b}_j$ the estimated coefficients of the multilogit model corresponding to the income and the child earning variables for alternatives j = 1, 2, the alternative 0 being taken as the default. Then (5) implies the following system of equations:

$$\begin{aligned}
&\alpha_1 - \alpha_0 = \hat{a}_1 \\
&\alpha_2 - \alpha_0 = \hat{a}_2 \\
&(\alpha_1 M - \alpha_0).K = \hat{b}_1 \\
&(\alpha_2 D - \alpha_0)K = \hat{b}_2
\end{aligned} \tag{10}$$

M is known from equation (3). It follows that arbitrarily setting a value for K or for D allows us to identify $\alpha_0$ , $\alpha_1$ and $\alpha_2$ and the remaining parameter in the pair (K,D). The identifying assumption made in what follows is that kids working on the market and not going to school have zero domestic production, i.e. K = 1. In other words, it is assumed that the observed labor allocations between market and domestic activities are corner solutions in all alternatives.[12] It then follows that :

$$\alpha_1 = \frac{\hat{a}_1 - \hat{b}_1}{1 - M} \text{ and } \alpha_2 = \alpha_1 + \hat{a}_2 - \hat{a}_1 \qquad (11)$$

Of course, a test of the relevance of the identifying assumption is that both $\alpha_1$ and $\alpha_2$ must be positive. One could also require that the value of D obtained from system (9) with K=1 be in the interval (0,1).

For completeness, it remains to indicate how estimates of the residual terms $v_{ij}$-$v_{i0}$ may be obtained. In a discrete choice model these values cannot be observed. It is only known that they belong to some interval. The idea is then to draw them for each observation in the relevant interval, that is: in a way consistent with the observed choice. For instance if observation i has made choice 1, it must be the case that :

$$Z_i.\gamma_1 + Y_{-i}.\ \hat{a}_1 + \hat{b}_1.w_i + (v_{i1}\text{-}v_{i0}) > \text{Sup}[0,\ Z_i.\gamma_2 + Y_{-i}.\ \hat{a}_2 + \hat{b}_2.w_i + (v_{i2}\text{-}v_{i0})]$$

The terms $v_{ij}$-$v_{i0}$ must be drawn so as to satisfy that inequality. All that is missing now is a complete vector of child earnings values, $w_i$.

*Estimation of potential earnings*

The discrete choice model requires a potential earning for each child, including those who do not work outside the household. To be fully rigorous, one could estimate both the discrete choice model and the earning equation simultaneously by maximum likelihood techniques. This is a rather cumbersome procedure. Practically, a multinomial probit would then be preferable to a multinomial logit in order to handle simultaneously the random terms of the discrete choice model and that of the earning equation. Integrating tri-variate normal distributions would then be required. Also, other issues which are already apparent with a simpler technique would not necessarily be solved.

[12] In effect, this assumption may be weakened using some limited information on hours of work available in the survey.

We adopt a simpler approach, which has the advantages of transparency and robustness. It consists of estimating (3) by OLS, and then to generate random terms $u_i$ for non-working kids, by drawing in the distribution generated by the residuals of the OLS estimation.

There are several reasons why correcting the estimation of the earning function for a selection bias was problematic. First, instrumenting earnings with a selection bias correction procedure requires finding instruments that would affect earnings but not the schooling/labor choice. No such instrument was readily available. Second, the correction of selection bias with the standard two-stage procedure is awkward in the case of more than two choices. Lee (1983) proposed a generalization of the Heckman procedure, but it has been shown that Lee's procedure was justified only in a rather unlikely particular case.[13] For both of these reasons, failing to correct for possible selection bias in (3) did not seem too serious a problem. On the other hand, trying to correct using standard techniques and no convincing instrument led to rather implausible results.

*Simulating programs of the Bolsa Escola type*

As mentioned in footnote 11, the model (6)-(7) does not provide a complete representation of the choice faced by households in the presence of a program such as *Bolsa Escola*. This is because it takes into account the conditionality on the schooling of the children, but not the means-test. Taking into account both the means-test and the conditionality leads to choosing the alternative with maximum utility among the three following conditional cases:

$$
\begin{aligned}
&U_i(0) = Z_i.\gamma_0 + \alpha_0 Y_{-I} + \beta_0 w_i + v_{i0} \\
&U_i(1) = Z_i.\gamma_1 + \alpha_1 (Y_{-I} + T) + \beta_1 w_i + v_{i1} \quad if\ Y_{-I} + Mw_i \leq Y^\circ \\
&U_i(1) = Z_i.\gamma_1 + \alpha_1 Y_{-I} + \beta_1 w_i + v_{i1} \quad if\ Y_{-I} + Mw_i > Y^\circ \\
&U_i(2) = Z_i.\gamma_2 + \alpha_2 (Y_{-I} + T) + \beta_2 w_i + v_{i2} \quad if\ Y_{-I} \leq Y^\circ \\
&U_i(2) = Z_i.\gamma_2 + \alpha_2 Y_{-I} + \beta_2 w_i + v_{i2} \quad if\ Y_{-I} > Y^\circ
\end{aligned}
\qquad (12)
$$

where Y° stands for the means test. Of course, as mentioned above, only the differences between the utility corresponding to the three cases matter, so that one only need to know

[13] See Bourguignon et al. (2001).

the differences ($\beta_j$-$\beta_0$), ($\gamma_j$-$\gamma_0$) and ($v_{ij}$- $v_{i0}$)– but the three coefficients $\alpha_j$. In this system, one can see how the introduction of *Bolsa Escola* might lead households from choice (0) – no schooling – to choices (1) or (2), but also from choice (1) to choice (2). In the latter case, a household might not qualify for the transfer T when the child both works and attends school, but qualifies if she stops working.

A wide variety of programs may be easily simulated using this framework. Both the means-test and the transfer T could be made dependent on characteristics of either the household or the child (X and H). In particular, T could depend on age or gender. Some examples of such alternative designs are simulated and discussed in Section 5.

Before presenting the model estimations results, we should draw attention to two important limitations of the framework just described. Both arise from the set of assumptions discussed in the beginning of this section. The first limitation is that we can not take into account the household transfer ceiling of R$45 per household. The reason is that by ignoring multi-children interactions in the model, it is as though we had effectively assumed that all households were single-child, from a behavioral point of view. In the non-behavioral part of the welfare simulations which are reported in Section 5 below, however, each child was treated separately, and the R$45 limit was applied.

The second limitation has to do with the exogeneity of non-child income $Y_{-I}$. This exogeneity would clearly be a problem when there are more than one child at schooling age. But it is also unrealistic even when only adult income is taken into account. It is clearly possible that the presence of the means-test might affect the labor supply behavior of adults, since there are circumstances in which it might be in the interest of the family to work slightly less in order to qualify for *Bolsa Escola*. Note, however, that this might not be so sharply the case if the means-test is based, not on current income, but on some score-based proxy for permanent income, as appears to be the case in practice.

## 4. Descriptive statistics and estimation results

The model consisting of equations (3) and (12) was estimated on data from the 1999 PNAD household survey. This survey is based on a sample of approximately 60,000 households, which is representative of the national population[14]. Although all children aged 6-15 qualify for participation in the program, the model was only estimated for 10-15 year-olds, since school enrollment below age 10 is nearly universal.[15] At the simulation stage, however, transfers are of course simulated for the whole universe of qualifying 6-15 year-olds.

Table 1 contains the basic description of the occupational structure of children aged 10-15 in Brazil, in 1999. In this age range, 77% of children report that they dedicate themselves exclusively to studying. Some 17% both work and study, and 6% do not attend school at all. This average pattern hides considerable variation across ages: school attendance declines – and work increases – monotonically with age. Whereas only 2.5% of ten year-olds are out of school, the figure for fifteen year-olds is 13%. Whereas 90% of ten year-olds dedicate themselves exclusively to studying, fewer than 60% of fifteen year-olds do so. From a behavioral point of view, it is thus clear that most of the action is to be found among the eldest children.

Table 2 presents the mean individual and household characteristics of those children, by occupational category. Children not going to school are both older and less educated than those still enrolled. As expected, households with school drop-outs are on average poorer, less educated and larger than households where kids are still going to school. Dropping out of school and engaging in child labor are relatively more frequent among non-whites and in the North-East. Both forms of behavior are least common in metropolitan areas, but proportionately more common in non-metropolitan urban areas than in rural areas. Interestingly, households where children both work and go to school

[14] Except for the rural areas of the states of Acre, Amazonas, Pará, Rondônia and Roraima.

[15] We know that school enrolment is nearly universal from answers to schooling questions in the PNAD. An additional reason to limit the estimation of the behavioral model to children aged ten or older is that the incidence of child labor at lower ages is probably measured with much greater error, since PNAD interviewers are instructed to pose labor and income questions only to individuals aged ten or older.

are in an intermediate position, along all dimensions, between those whose children specialize, but are generally closer to the group of drop-outs.

A remarkable feature of Table 2 is the observed amount of children's earnings, when they work and do not study. Ranging from around R$80 to R$120 per month, children's earnings represent approximately half the minimum wage, an order of magnitude that seems rather reasonable. These amounts compares with the R$15 transfer that is granted by the *Bolsa Escola* program for children enrolled in school. Note, however, that the R$90 figure is not a good measure for the opportunity cost of schooling, since school attendance is evidently consistent with some amount of market work.

Tables 3 and 4 contain the estimation results. Because of the great behavioral variation across ages even within the 10-15 range - as revealed, for instance, in Table 1 - we estimated the (identically specified) model separately for each age, as well as for the pooled sample of all 10-15 year-olds. The simulations reported in the next section rely on the age-specific models, but in this section we focus on the joint estimation, both for ease of discussion and because the larger sample size allowed for more precise estimation in this case.

Table 3 shows the results of the OLS estimation of the earnings function (3), both for the pooled sample and for the 15 year-old group.[16] Geographical variables[17], race and gender have the expected sign, and the same qualitative effect as for adults. So does (the logarithm of) the average earnings of children in the census cluster, which is included as a proxy for the spatial variation in the demand for child labor. The effect of previous schooling is best described as insignificant. Even though the coefficient of the squared term is positive and significant, the influence of the (negative and insignificant) linear term implies that earnings decline with schooling in the range relevant for 10-15 year-olds. It should be noted that our separate specifications mask the main determinant of earnings for children, namely age. In an alternative (unreported) specification for the pooled sample, when age was included as an explanatory variable, an additional year of

[16] Analogous results for the 10, 11, 12, 13 and 14 year-old samples are available from the authors on request.

[17] With the South being insignificantly different from the reference Southeast region, as expected.

age increased earnings by approximately 40 per cent. But there was a clear non-linearity in the way age affected earnings, which is reflected in changes in the coefficient estimates when the model is separately estimated. These non-linearities and interactions between age and other determinants are the reason why the separate specification was preferred.

The estimate for m – the coefficient for “dummy WS” in Table 3 – reveals that, as expected, the fact that a child goes to school at the same time as she works outside the household reduces total earnings in comparison with a comparable child who dedicates herself exclusively to market work. If one interprets this coefficient as reflecting fewer hours of work, then a child going to school works on average 40 per cent less than a dropout (for the pooled sample), or just under a quarter less for fifteen year-olds. These seem like reasonable orders of magnitude.

The results from the estimation of the multinomial logit for occupational choice also appear eminently plausible. They are reported in Table 4 (for the pooled sample) and Tables 4a and 4b for 10-12 and 13-15 year-olds, respectively. The reference category was “not studying” (j = 0), throughout. As expected, household income (net of the child’s) has a positive effect on schooling, whereas the child’s own (predicted) earnings have a negative effect. Household size reduces the probability of studying, compared to the alternatives.[18] Previous schooling at a given age has a positive (but concave) effect. Race has an insignificant effect on occupational choice, unlike gender which reflects the usual asymmetry between market work for males and domestic work for females. Parents' education has the expected positive effect – on top of the income effect - on children's schooling.

In view of this general consistency of both the earnings and the discrete occupational choice models, the question now arises of whether the structural restrictions necessary for the consistency of the proposed simulation work – positive $\alpha_1$ and $\alpha_2$, and $0 < D < 1$ - hold or not. For the pooled sample and using (11), we find that:

$$\alpha_1 = \frac{\hat{a}_1 - \hat{b}_1}{1 - M} = \frac{0.0004 + 0.0075}{1 - Exp(-0.4118)} = 0.023 \text{ and } \alpha_2 = \alpha_1 + \hat{a}_2 - \hat{a}_1 = .024$$

[18] To the extent that household size reflects a larger number of children, this is consistent with Becker’s quantity-quality trade-off.

The coefficients of income in the utility of alternatives j = 1 and 2 is thus positive, which is in agreement with the original model. This is also true of the utility of alternative j =0 since it may be computed that $\alpha_0 = 0.023$. The value of the parameter D may also be derived. Under the identifying assumption that K =1, it is given by :

$$D = \frac{\hat{b}_2 + \alpha_0}{\alpha_2} = \frac{-0.0074 + 0.023}{0.024} = 0.6609$$

This figure means that children who are going to school but do not work on the market are estimated to provide domestic production for approximately two-thirds of their potential market earnings. Note that this is almost identical to the estimated value for M [= Exp (-0.4118) = 0.6625]. Since M denotes the average contribution to household income from children both studying and working, as a share of their potential contribution if not studying, this implies that the estimated value of non-market work by children studying (and not working in the market) is approximately equal to the market value of work by those studying (and working in the market). If there was little selection on unobservables into market work, this is exactly what one would expect.

Overall, the estimates obtained from the multinomial discrete occupational choice model and the earning equation seem therefore remarkably consistent with rational, utility-maximizing behavior. We may thus expect simulations run on the basis of these models and the identifying structural assumptions about the parameter K to yield sensible results. We can now turn to our main objective: gauging the order of magnitude of the effects of programs such as *Bolsa Escola*.

## 5. An ex-ante evaluation of Bolsa Escola and alternative program designs

Bolsa Escola – and many conditional cash transfer schemes like it – are said to have two distinct objectives: (i) to reduce current poverty (and sometimes inequality) through the targeted transfers, and (ii) to reduce future poverty, by increasing the incentives for today's poor to invest in their human capital. Later on in this section, we will turn to the first objective. We begin by noting, however, that, as stated, the second objective is impossible to evaluate, even in an ex-ante manner. Whether increased school enrollment translates into greater human capital depends on the trends in the quality of the educational services provided, and there is no information on that in this data set.[19] Finally, whether more "human capital", however measured., will help reduce poverty in the future or not, depends on what happens to the rates of return to it between now and then. This is a complex, general equilibrium question, which goes well beyond the scope of this exercise.

What we might be able to say something about is the intermediate target of increasing school enrollment. While the preceding remarks suggest that this is not sufficient to establish whether the program will have an impact on future poverty, it is at least necessary.[20] An *ex-ante* evaluation of impact on this dimension of the program thus requires simulating the number of children that may change schooling and working status because of it.

This is done by applying the decision system (12) - with behavioral parameter values ($\alpha$, $\beta$, $\gamma$, M and D) estimated from (9) - (11), and policy parameter values (T and $Y^0$) taken from the actual specification of Bolsa Escola - to the original data. Equation (12) is then used to simulate a counterfactual distribution of occupations, on the basis of the observed characteristics and the restrictions on residual terms for each individual child. Comparing the vector of occupational choices thus generated with the original, observed vector, we see that the program leads to some children moving from choice $S_i$= 0 to choices $S_i$=1 or 2, and from $S_i$= 1 to choices $S_i$= 2. The corresponding transition matrix is shown in table 5 for all children between 10 and 15, as well as for all children in the same age group living in poor households. [21]

Despite the small value of the proposed transfer, Table 5 suggests that one in every three children (aged 10-15) who are presently not enrolled in school would get

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[19] There is limited information in other data sets, such as the Education Ministry's Sistema de Acompanhamento do Ensino Básico (SAEB), but not for sufficiently long periods of time. See Albernaz et. al. (2002).

[20] One could argue that it is not even necessary, since the transfers might, by themselves, alleviate credit constraints and have long-term positive impacts, e.g. through improved nutrition. We focus on whether the conditional nature of these transfers actually have any impact of the children's occupational choices (or time allocation decisions).

[21] A household was considered poor if its (regionally price-deflated and imputed rent-adjusted) per capita income was less than R$74.48 in the reference month of the 1999 PNAD survey. For the derivation of the poverty line, see Ferreira et al. (forthcoming).

enough incentive from Bolsa Escola to change occupational status and go to school. Among them, just over a quarter would enroll, but remain employed on the labor market . The other three quarters would actually cease work outside their household. This would reduce the proportion of children outside school from 5.8% to 3.9%.

The impact on those currently both studying and working would be much smaller. Barely 2% of them would abandon work to dedicate themselves exclusively to their studies. As a result of this small outflow, combined with an inflow from occupational category 1, the group of children both studying and working would actually grow in the simulated scenario, albeit marginally.

The impacts are even more pronounced, as one would expect, among the poor – who are the target population for the program. According to the poverty line being used, the incidence of poverty in Brazil is 30.5%. However, because there are more children in poor households – this being one of the reasons why they are poor – the proportion of 10-15 children in poor households is much higher: 42%. The second panel in Table 5 shows that dropouts are much more frequent among them – 9.1 instead of 5.8 per cent for the whole population. It also shows that *Bolsa Escola* is more effective in increasing school enrollment. The fall in the proportion of dropouts is one-half, rather than one-third. As a result, the simulation suggests that *Bolsa Escola* could increase the school enrollment rate among the poor by approximately 4.4 percentage points. Once again, this increase comes at the expense of the "not studying category", whose numbers are halved, rather than of the "working and studying" category, which actually becomes marginally more numerous.

A 50% reduction in the proportion of poor children outside school is by no means an insubstantial achievement, particularly in light of the fact that it seems to be manageable with fairly small transfers (R$15 per child per month). This is partly due to the fact that the value of the current contributions of children who are enrolled in school is a sizable proportion of their potential earnings when completely outside school. Those proportions are exactly the interpretation of the parameters M (for those who work on the market as well as study) and D (for those who work at home as well as study), which we estimated to be of the order of 0.66. Applying that factor to R$100, as a rough average of the earnings of children in category j = 0 (see Table 2), we are left with some R$33 as the true opportunity cost of enrolling in school. Consequently, those children who change occupation from that category in response to the R$15 transfer must have average personal present valuations of the expected stream of benefits from enrolling greater than R$18. Those who don't, must on average value education at less than that.

Because our simulations suggest that *Bolsa Escola*, as currently formulated, would still leave some 4% of all 10-15 year-olds (4.7% among the poor ones) outside school, it is interesting to investigate the potential effects of changing some of the program parameters. This was, after all, one of the initial motivations for undertaking this kind of *ex-ante* counterfactual analysis. Table 6 shows the results of such a comparative exercise in terms of occupational choice, using transition matrices analogous to those in Table 5, once again both for all children and then separately for poor households only. Table 7 compares the impact of each scenario with that of the benchmark program specification, in terms of poverty and inequality measures. Four standard inequality measures were selected, namely the Gini coefficient and three members of the Generalized Entropy Class: the mean log deviation, the Theil-T index and (one half of) the square of the coefficient of variation. For poverty, we present the three standard FGT (0, 1, 2) measures, with respect to the aforementioned Ferreira et. al. (forthcoming) poverty line. This later table allows us to gauge impact in terms of the first objective of the program, namely the reduction of current poverty (and possibly inequality).

In both tables, the simulation results for six alternative scenarios are presented. In scenario 1, the eligibility criteria (including the means test) are unchanged, but transfer amounts (and the total household ceiling) are both doubled. In scenario 2, the uniform R$15 per child transfer is replaced by an age-contingent transfer, whereby 10 year-olds would receive R$15, 11 year-olds would receive R$20, 12 year-olds would receive R$25, 13 year-olds would receive R$35, 14 year-olds would receive R$40, and 15 year-olds received R$45.[22] In scenario 3, transfer amounts were unchanged, but the means-test was raised from R$90 to R$120. Scenario 4 combines scenarios 1 and 3: the transfer was doubled, and the means-test raised to R$120. Scenario 5 combines scenarios 2 and 3 in the same way: an age-progressive transfer with a R$120 means-test. Scenario 6 simulated a targeted transfer exactly as in Bolsa Escola, but with no conditionality: every child in

[22] The household ceiling was also doubled to R$90 in this case.

households below the means-test received the benefit, with no requirement relating to school attendance.

Table 6 gives rise to three main results. First of all, a comparison of Scenario 6 and the actual *Bolsa Escola* program suggests that conditionality plays a crucial role in inducing the change in children's time-allocation decisions. The proportions of children in each occupational category under Scenario 6 are almost identical to the original data (i.e. no program). This suggests that it is the conditional requirement to enroll in order to receive the benefit – rather than the pure income effect from the transfer - which is the primary cause of the extra demand for schooling evident in the *Bolsa Escola* column.

Second, scenario 1 reveals that the occupational impact of the program is reasonably elastic with respect to the transfer amount. The proportion of un-enrolled children drops another percentage point (i.e. some 25%) in response to a doubling of the transfers. The proportion of children in the "studying only" category rises by the same percentage point. Scenario 2 suggests that it doesn't matter much, in aggregate terms, whether this increase in transfers is uniform across ages, or made to become increasing in the age of the child. Finally, scenario 3 (and the combinations in scenarios 4 and 5) suggest that occupational effects are less sensitive to the means-test than to the transfer amount.

Results are considerably less impressive in terms of the program's first stated objective, namely the reduction in current poverty (and inequality) levels. Table 7 suggests that the program, as currently envisaged, would only imply a one percentage point decline in the short-run incidence of poverty in Brazil, as measured by P(0). However, there is some evidence that the transfers would be rather well targeted, since the inequality-averse poverty indicator P(2) would fall by proportionately more than P(0), from 8% to 7%. This is consistent with the inequality results: whereas the Gini would fall by only half a point as a result of the scheme, measures which are more sensitive to the bottom, such as the mean log deviation, fall by a little more. Overall, however, the evidence in column 2 of Table 7 falls considerably short of a ringing endorsement of *Bolsa Escola* as a program for the alleviation of current poverty or inequality.

The situation could be somewhat improved by increases in the transfer amounts (scenarios 1 and 2). Nevertheless, even a doubling of the transfer amount to R$30 per month would only shave another 1.3 percentage points off the headcount.[23] An increase in the means-test would not help much, as indicated by Scenario 3. This is consistent with our earlier suggestion that the program already appears to be well-targeted to the poor. If it fails to lift many of them above the poverty line, this is a consequence of the small size of the transfers, rather than of the targeting.

These results contrast with the arithmetic simulations reported by Camargo and Ferreira (2001), in which a somewhat broader, but essentially similar program would reduce the incidence of poverty (with respect to the same poverty line and in the same sample) by two-thirds, from 30.5% to 9.9%. This was despite the fact that the absence of a behavioral component to the simulation weakened its power, by excluding from the set of recipients those households whose children might have enrolled in response to the program. The reason is simple: Camargo and Ferreira simulate much higher transfer levels, ranging from R$150 to R$220 per household (rather than child).

## 6. Conclusions

In this paper, we proposed a micro-simulation method for evaluating and experimenting with conditional cash-transfer program designs, ex-ante. We were concerned with the impacts of the Brazilian *Bolsa Escola* program, which aims to reduce both current and future poverty by providing small targeted cash transfers to poor households, provided their children are enrolled in and in actual attendance at school. We were interested in assessing two dimensions of the program: its impact on the occupational choice (or time-allocation) decisions of children, and the effects on current poverty and inequality.

For this purpose, we estimated a discrete occupational choice model (a multinomial logit) on a nationally representative household-level sample, and used its estimated parameters to make predictions about the counterfactual occupational decisions of children, under different assumptions about the availability and design of cash transfer programs. These assumptions were basically expressed in terms of different values for

[23] The simulated one-percentage-point fall in P(2) is, once again, more respectable.

two key policy parameters: the means-test level of household income; and the transfer amount.

Because predicted earnings values were needed for all children in the simulation, this procedure also required estimating a Mincerian earnings equation for children in the sample, and using it to predict earnings in some cases. Also, because the income values accruing to each household were not symmetric across different occupational choices, standard estimation procedures for the multinomial logit were not valid. An identification assumption was needed, and we chose it to be that children not enrolled in school work only in the market, and have a zero contribution to domestic work. Under this assumption, the estimation of the model generated remarkably consistent results: marginal utilities of income were always positive, and very similar across occupational categories. Time spent working by those enrolled in school, as a fraction of time spent working by those not enrolled, was always in the (0, 1) interval and was basically identical – and equal to two-thirds - whether work was domestic or in the market.

When this estimated occupational choice model was used to simulate the official (April 2001) design of the federal Brazilian *Bolsa Escola* program, we found that there was considerable behavioral response from children to the program. About one third of all 10-15 year-olds not currently enrolled in school would – according to the model – enrol in response to the program. Among poor households, this proportion was even higher: one half would enter school. The proportion of children in the middle occupational category ("studying and working in the market") would not fall. In fact, it would rise, marginally.

Results in terms of the reduction of current poverty, however, were less heartening. As currently designed, the federal *Bolsa Escola* program would reduce poverty incidence by one percentage point only, and the Gini coefficient by half a point. Results were better for measures more sensitive to the bottom of the distribution, but the effect was never remarkable.

Both the proportion of children enrolling in school in response to program availability and the degree of reduction in current poverty turn out to be rather sensitive to transfer amounts, and rather insensitive to the level of the means-test. This suggests that the targeting of the Brazilian *Bolsa Escola* program is adequate, but that poverty reduction through this instrument, although effective, is not magical. Governments may be transferring cash in an intelligent and efficient way, but they still need to transfer more substantial amounts, if they hope to make a dent in the country's high levels of deprivation.

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Table 1: School enrollment and occupation of children by age (10-15 years old)

| | 10 | 11 | 12 | 13 | 14 | 15 | Total |
|---|---|---|---|---|---|---|---|
| Not Studying | 2.5% | 2.3% | 3.3% | 5.6% | 8.0% | 13.0% | 5.8% |
| Working and Studying | 8.1% | 10.9% | 14.0% | 18.3% | 22.6% | 27.3% | 16.9% |
| Studying | 89.4% | 86.8% | 82.7% | 76.1% | 69.4% | 59.6% | 77.3% |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |

Source: PNAD/IBGE 1999 and author's calculation

Table 2 : Sample means. Characteristics of children and the household they belong to (10-15 years old only)

| | Not studying | Working and Studying | Studying | Total |
|---|---|---|---|---|
| Age | 13.5 | 13.2 | 12.3 | 12.51 |
| Years of schooling | 2.9 | 3.9 | 4.1 | 3.97 |
| Household per capita income | 80.9 | 104.5 | 202.0 | 178.25 |
| Earning's children (observed) | | | | |
| 10 | 118.4 | 34.2 | - | 38.0 |
| 11 | 98.3 | 44.6 | - | 50.4 |
| 12 | 100.7 | 50.8 | - | 57.0 |
| 13 | 76.8 | 66.9 | - | 68.5 |
| 14 | 100.5 | 83.8 | - | 87.8 |
| 15 | 127.6 | 109.3 | - | 113.9 |
| Years of schooling of the most educated parent | 3.2 | 4.0 | 6.4 | 5.79 |
| Age of the oldest parent | 46.4 | 46.1 | 44.5 | 44.89 |
| Number of household members | 5.8 | 5.9 | 5.2 | 5.39 |
| Race (White) | 36.9% | 40.9% | 51.6% | 48.9% |
| Gender (Male) | 53.0% | 65.2% | 46.9% | 50.3% |
| North | 6.1% | 5.6% | 6.0% | 5.9% |
| Northeast | 40.4% | 45.6% | 29.9% | 33.2% |
| Southeast | 32.8% | 26.1% | 43.5% | 39.9% |
| South | 14.1% | 15.9% | 13.7% | 14.1% |
| Center-West | 6.6% | 6.7% | 6.9% | 6.9% |
| Metropolitan area | 18.2% | 12.8% | 30.9% | 27.1% |
| Urban non metroplitan | 34.0% | 49.2% | 16.0% | 22.7% |
| Rural areas | 47.8% | 38.0% | 53.0% | 50.2% |
| Proportion of universe | 6.1% | 16.8% | 77.1% | 100.0% |
| Population | 1,208,313 | 3,345,075 | 15,329,237 | 19,882,625 |

Source: PNAD/IBGE 1999 and author's calculation

Table 3: Log earnings regression (10-15 years old children reporting earnings)

| | 10 to 15 years old | | | 15 years old | | |
|---|---|---|---|---|---|---|
| | Coeficient | Std | P>\|z\| | Coeficient | Std | P>\|z\| |
| n obs | 2444 | | | 1010 | | |
| $R^2$ | 0.43 | | | 0.54 | | |
| Dummy WS | -0.4118 | 0.0324 | 0.0000 | -0.2285 | 0.0385 | 0.0000 |
| Years of schooling | -0.0136 | 0.0198 | 0.4920 | -0.0409 | 0.0244 | 0.0930 |
| Years of schooling$^2$ | 0.0110 | 0.0021 | 0.0000 | 0.0077 | 0.0025 | 0.0020 |
| Male | 0.1746 | 0.0283 | 0.0000 | 0.1349 | 0.0355 | 0.0000 |
| White | 0.0658 | 0.0295 | 0.0260 | 0.0600 | 0.0355 | 0.0910 |
| North | -0.2329 | 0.0447 | 0.0000 | -0.1515 | 0.0748 | 0.0430 |
| Northeast | -0.2054 | 0.0379 | 0.0000 | -0.1529 | 0.0472 | 0.0010 |
| *South* | -0.0461 | 0.0422 | 0.2750 | -0.0165 | 0.0475 | 0.7290 |
| Center-West | -0.1082 | 0.0426 | 0.0110 | -0.0801 | 0.0490 | 0.1020 |
| Urban non metroplitan | -0.0284 | 0.0408 | 0.4870 | 0.0472 | 0.0538 | 0.3810 |
| Rural | 0.0042 | 0.0327 | 0.8980 | 0.0507 | 0.0393 | 0.1970 |
| Log of means earnings by cluster | 0.3788 | 0.0148 | 0.0000 | 0.4756 | 0.0199 | 0.0000 |
| Intercept | 3.5266 | 0.0751 | 0.0000 | 2.7600 | 0.1176 | 0.0000 |

Source: PNAD/IBGE 1999 and author's calculation

Table 4: Multinomial Logit Coefficients

| | | | Working and Studying | | | Studying | | |
|---|---|---|---|---|---|---|---|---|
| | Pseudo-$R^2$ | #obs | Coeficient | Std | P>\|z\| | Coeficient | Std | P>\|z\| |
| **10 to 15 years old** | 0.1586 | 42153 | | | | | | |
| Total household income | | | 0.0004 | 0.0001 | 4.6300 | 0.0006 | 0.0001 | 7.8200 |
| Earning's children (What) | | | -0.0075 | 0.0018 | -4.2100 | -0.0074 | 0.0015 | -4.9300 |
| Total people by household | | | -0.0343 | 0.0169 | -2.0300 | -0.1751 | 0.0157 | -11.1400 |
| Years of schooling | | | 0.6635 | 0.0407 | 16.3100 | 0.8338 | 0.0378 | 22.0500 |
| Years of schooling$^2$ | | | -0.0383 | 0.0051 | -7.5300 | -0.0837 | 0.0048 | -17.6000 |
| White | | | 0.0138 | 0.0628 | 0.2200 | 0.1613 | 0.0566 | 2.8500 |
| Male | | | 0.7447 | 0.0567 | 13.1400 | -0.1841 | 0.0503 | -3.6600 |
| Max parent's education | | | 0.0371 | 0.0104 | 3.5700 | 0.1300 | 0.0093 | 13.9100 |
| Max parent's age | | | -0.0035 | 0.0027 | -1.2900 | 0.0023 | 0.0024 | 0.9600 |
| Number of children below 7 | | | -0.0108 | 0.0362 | -0.3000 | 0.0875 | 0.0332 | 2.6400 |
| Rank of child | | | 0.6433 | 0.0538 | 11.9500 | 0.9099 | 0.0504 | 18.0500 |
| North | | | 0.5673 | 0.1113 | 5.1000 | 0.0980 | 0.0998 | 0.9800 |
| Northeast | | | 0.7086 | 0.0789 | 8.9800 | 0.2854 | 0.0717 | 3.9800 |
| South | | | 0.1901 | 0.0867 | 2.1900 | -0.3569 | 0.0778 | -4.5800 |
| Center-West | | | 0.2757 | 0.0977 | 2.8200 | -0.1013 | 0.0869 | -1.1700 |
| Urban non metroplitan | | | 1.1803 | 0.0807 | 14.6200 | -0.4999 | 0.0728 | -6.8700 |
| Rural | | | 0.2030 | 0.0735 | 2.7600 | -0.1835 | 0.0628 | -2.9200 |
| Means of earnings by cluster | | | 0.0118 | 0.0073 | 1.6200 | 0.0022 | 0.0057 | 0.4000 |
| Intercept | | | -2.3104 | 0.2071 | -11.1600 | 0.4412 | 0.1846 | 2.3900 |

Source: PNAD/IBGE 1999 and author's calculation

**Table 4a: Multinomial Logit Coefficients**

| | Pseudo-$R^2$ | #obs | Working and Studying | | | Studying | | |
|---|---|---|---|---|---|---|---|---|
| | | | Coeficient | Std | P>\|z\| | Coeficient | Std | P>\|z\| |
| **10 years old** | 0.2393 | 6853 | | | | | | |
| Total household income | | | 0.0001 | 0.0004 | 0.8570 | 0.0006 | 0.0003 | 0.0760 |
| Earning's children (What) | | | -0.0711 | 0.0273 | 0.0090 | -0.0460 | 0.0251 | 0.0670 |
| Total people by household | | | -0.0072 | 0.0769 | 0.9250 | -0.0766 | 0.0679 | 0.2590 |
| Years of schooling | | | 2.5342 | 0.2466 | 0.0000 | 2.8347 | 0.2138 | 0.0000 |
| Years of schooling$^2$ | | | -0.4023 | 0.0599 | 0.0000 | -0.4993 | 0.0513 | 0.0000 |
| White | | | -0.2006 | 0.2611 | 0.4420 | -0.1311 | 0.2375 | 0.5810 |
| Male | | | 0.6865 | 0.2057 | 0.0010 | -0.2596 | 0.1803 | 0.1500 |
| Max parent's education | | | 0.0235 | 0.0396 | 0.5530 | 0.0621 | 0.0343 | 0.0710 |
| Max parent's age | | | -0.0030 | 0.0094 | 0.7460 | -0.0037 | 0.0079 | 0.6430 |
| Number of children below 7 | | | 0.1721 | 0.1294 | 0.1840 | 0.0682 | 0.1145 | 0.5510 |
| Rank of child | | | 0.1935 | 0.1354 | 0.1530 | 0.0982 | 0.1179 | 0.4050 |
| North | | | 1.8948 | 0.4854 | 0.0000 | 0.7064 | 0.4214 | 0.0940 |
| Northeast | | | 1.7310 | 0.3279 | 0.0000 | 0.8418 | 0.2865 | 0.0030 |
| South | | | 0.5136 | 0.3755 | 0.1710 | -0.2513 | 0.3263 | 0.4410 |
| Center-West | | | 1.5302 | 0.4621 | 0.0010 | 0.8179 | 0.4202 | 0.0520 |
| Urban non metroplitan | | | 3.1158 | 0.3732 | 0.0000 | 0.5128 | 0.3077 | 0.0960 |
| Rural | | | 1.0942 | 0.3324 | 0.0010 | 0.1258 | 0.2610 | 0.6300 |
| Means of earnings by cluster | | | 0.3847 | 0.1175 | 0.0010 | 0.1872 | 0.1090 | 0.0860 |
| Intercept | | | -3.4075 | 0.7173 | 0.0000 | 1.4643 | 0.5963 | 0.0140 |
| **11 years old** | 0.2610 | 7022 | | | | | | |
| Total household income | | | -0.0001 | 0.0002 | 0.7180 | 0.0002 | 0.0002 | 0.3690 |
| Earning's children (What) | | | -0.0247 | 0.0313 | 0.4310 | 0.0481 | 0.0296 | 0.1050 |
| Total people by household | | | 0.1202 | 0.0750 | 0.1090 | 0.1143 | 0.0698 | 0.1020 |
| Years of schooling | | | 1.8700 | 0.2440 | 0.0000 | 1.9526 | 0.2194 | 0.0000 |
| Years of schooling$^2$ | | | -0.2545 | 0.0500 | 0.0000 | -0.2714 | 0.0451 | 0.0000 |
| White | | | 0.0327 | 0.2585 | 0.8990 | 0.0935 | 0.2424 | 0.7000 |
| Male | | | 0.3583 | 0.2115 | 0.0900 | -0.4660 | 0.1970 | 0.0180 |
| Max parent's education | | | 0.0057 | 0.0416 | 0.8910 | 0.0850 | 0.0381 | 0.0260 |
| Max parent's age | | | -0.0061 | 0.0094 | 0.5180 | 0.0020 | 0.0085 | 0.8180 |
| Number of children below 7 | | | -0.1829 | 0.1392 | 0.1890 | -0.2591 | 0.1310 | 0.0480 |
| Rank of child | | | -0.0341 | 0.1468 | 0.8160 | -0.2566 | 0.1372 | 0.0610 |
| North | | | 1.2805 | 0.4554 | 0.0050 | 0.5387 | 0.4270 | 0.2070 |
| Northeast | | | 0.8725 | 0.3029 | 0.0040 | -0.1828 | 0.2794 | 0.5130 |
| South | | | 1.4466 | 0.4633 | 0.0020 | 0.3018 | 0.4463 | 0.4990 |
| Center-West | | | 0.4704 | 0.3925 | 0.2310 | -0.5806 | 0.3546 | 0.1020 |
| Urban non metroplitan | | | 1.6909 | 0.3100 | 0.0000 | -0.0622 | 0.2874 | 0.8290 |
| Rural | | | -0.0171 | 0.2962 | 0.9540 | -0.1303 | 0.2621 | 0.6190 |
| Means of earnings by cluster | | | 0.0277 | 0.0313 | 0.3750 | -0.0778 | 0.0313 | 0.0130 |
| Intercept | | | -2.4141 | 0.6731 | 0.0000 | 1.6659 | 0.6096 | 0.0060 |
| **12 years old** | 0.2258 | 7196 | | | | | | |
| Total household income | | | 0.0000 | 0.0002 | 0.8790 | 0.0003 | 0.0002 | 0.1610 |
| Earning's children (What) | | | -0.0093 | 0.0084 | 0.2680 | -0.0150 | 0.0084 | 0.0730 |
| Total people by household | | | -0.0005 | 0.0581 | 0.9940 | -0.0769 | 0.0554 | 0.1650 |
| Years of schooling | | | 1.3963 | 0.1728 | 0.0000 | 1.5883 | 0.1572 | 0.0000 |
| Years of schooling$^2$ | | | -0.1405 | 0.0305 | 0.0000 | -0.1787 | 0.0278 | 0.0000 |
| White | | | 0.1590 | 0.2030 | 0.4330 | 0.2339 | 0.1907 | 0.2200 |
| Male | | | 0.9392 | 0.1726 | 0.0000 | 0.0547 | 0.1580 | 0.7290 |
| Max parent's education | | | 0.0072 | 0.0319 | 0.8220 | 0.0795 | 0.0289 | 0.0060 |
| Max parent's age | | | -0.0023 | 0.0089 | 0.8010 | 0.0001 | 0.0082 | 0.9920 |
| Number of children below 7 | | | -0.0121 | 0.1164 | 0.9170 | 0.0150 | 0.1082 | 0.8900 |
| Rank of child | | | 0.6002 | 0.1712 | 0.0000 | 0.4909 | 0.1601 | 0.0020 |
| North | | | 1.2716 | 0.3599 | 0.0000 | 0.6064 | 0.3377 | 0.0720 |
| Northeast | | | 0.8998 | 0.2481 | 0.0000 | 0.3845 | 0.2312 | 0.0960 |
| South | | | 0.0463 | 0.2760 | 0.8670 | -0.5530 | 0.2496 | 0.0270 |
| Center-West | | | -0.0045 | 0.3113 | 0.9890 | -0.2569 | 0.2818 | 0.3620 |
| Urban non metroplitan | | | 2.5243 | 0.2654 | 0.0000 | 0.1413 | 0.2319 | 0.5420 |
| Rural | | | 1.0872 | 0.2437 | 0.0000 | 0.2634 | 0.2035 | 0.1960 |
| Means of earnings by cluster | | | 0.0214 | 0.0184 | 0.2440 | -0.0046 | 0.0188 | 0.8080 |
| Intercept | | | -4.0732 | 0.6442 | 0.0000 | -0.1458 | 0.5756 | 0.8000 |

Source: PNAD/IBGE 1999 and author's calculation

**Table 4b: Multinomial Logit Coefficients**

| | Pseudo-$R^2$ | #obs | Working and Studying | | | Studying | | |
|---|---|---|---|---|---|---|---|---|
| | | | Coeficient | Std | P>\|z\| | Coeficient | Std | P>\|z\| |
| **13 years old** | 0.1813 | 7077 | | | | | | |
| Total household income | | | 0.0003 | 0.0002 | 0.1390 | 0.0004 | 0.0002 | 0.0280 |
| Earning's children (What) | | | -0.0211 | 0.0078 | 0.0070 | -0.0143 | 0.0078 | 0.0660 |
| Total people by household | | | 0.0422 | 0.0434 | 0.3310 | -0.0561 | 0.0402 | 0.1630 |
| Years of schooling | | | 0.7544 | 0.1192 | 0.0000 | 0.9879 | 0.1135 | 0.0000 |
| Years of schooling$^2$ | | | -0.0431 | 0.0184 | 0.0190 | -0.0737 | 0.0176 | 0.0000 |
| White | | | 0.0422 | 0.1606 | 0.7930 | 0.1379 | 0.1492 | 0.3560 |
| Male | | | 0.8550 | 0.1365 | 0.0000 | -0.0430 | 0.1270 | 0.7350 |
| Max parent's education | | | 0.0097 | 0.0250 | 0.6990 | 0.0798 | 0.0232 | 0.0010 |
| Max parent's age | | | -0.0022 | 0.0064 | 0.7260 | -0.0020 | 0.0059 | 0.7300 |
| Number of children below 7 | | | -0.1093 | 0.0921 | 0.2350 | -0.0676 | 0.0856 | 0.4300 |
| Rank of child | | | 0.1376 | 0.1495 | 0.3570 | 0.0841 | 0.1422 | 0.5540 |
| North | | | 0.7935 | 0.2676 | 0.0030 | 0.4388 | 0.2477 | 0.0760 |
| Northeast | | | 1.0844 | 0.1923 | 0.0000 | 0.7627 | 0.1812 | 0.0000 |
| South | | | 0.4987 | 0.2313 | 0.0310 | -0.2157 | 0.2142 | 0.3140 |
| Center-West | | | 0.4728 | 0.2452 | 0.0540 | 0.1034 | 0.2218 | 0.6410 |
| Urban non metroplitan | | | 1.1527 | 0.2210 | 0.0000 | -0.5803 | 0.2061 | 0.0050 |
| Rural | | | 0.2636 | 0.2008 | 0.1890 | -0.1524 | 0.1824 | 0.4030 |
| Means of earnings by cluster | | | 0.0342 | 0.0131 | 0.0090 | -0.0090 | 0.0138 | 0.5140 |
| Intercept | | | -2.4040 | 0.5116 | 0.0000 | 0.3448 | 0.4679 | 0.4610 |
| **14 years old** | 0.1795 | 7052 | | | | | | |
| Total household income | | | 0.0002 | 0.0002 | 0.2150 | 0.0004 | 0.0001 | 0.0060 |
| Earning's children(What) | | | -0.0029 | 0.0039 | 0.4590 | 0.0077 | 0.0049 | 0.1190 |
| Total people by household | | | 0.0431 | 0.0362 | 0.2350 | -0.0256 | 0.0349 | 0.4630 |
| Years of schooling | | | 0.4374 | 0.0924 | 0.0000 | 0.7161 | 0.0946 | 0.0000 |
| Years of schooling$^2$ | | | -0.0041 | 0.0132 | 0.7530 | -0.0385 | 0.0132 | 0.0040 |
| White | | | -0.0286 | 0.1310 | 0.8270 | 0.1265 | 0.1233 | 0.3050 |
| Male | | | 0.6975 | 0.1151 | 0.0000 | -0.2034 | 0.1092 | 0.0630 |
| Max parent's education | | | 0.0369 | 0.0233 | 0.1130 | 0.1091 | 0.0218 | 0.0000 |
| Max parent's age | | | -0.0137 | 0.0060 | 0.0220 | -0.0024 | 0.0056 | 0.6750 |
| Number of children below 7 | | | -0.1234 | 0.0769 | 0.1090 | -0.1285 | 0.0750 | 0.0870 |
| Rank of child | | | -0.1313 | 0.1638 | 0.4230 | -0.2028 | 0.1582 | 0.2000 |
| North | | | 0.6328 | 0.2363 | 0.0070 | 0.4337 | 0.2236 | 0.0520 |
| Northeast | | | 0.9830 | 0.1634 | 0.0000 | 0.8621 | 0.1573 | 0.0000 |
| South | | | 0.0849 | 0.1802 | 0.6380 | -0.5569 | 0.1678 | 0.0010 |
| Center-West | | | 0.5093 | 0.2100 | 0.0150 | 0.2439 | 0.1995 | 0.2220 |
| Urban non metroplitan | | | 0.9129 | 0.1687 | 0.0000 | -0.7278 | 0.1599 | 0.0000 |
| Rural | | | 0.2720 | 0.1529 | 0.0750 | -0.1551 | 0.1388 | 0.2640 |
| Means of earnings by cluster | | | 0.0016 | 0.0051 | 0.7620 | -0.0397 | 0.0078 | 0.0000 |
| Intercept | | | -1.4708 | 0.4538 | 0.0010 | -0.0760 | 0.4350 | 0.8610 |
| **15 years old** | 0.1549 | 6953 | | | | | | |
| Total household income | | | 0.0002 | 0.0001 | 0.0180 | 0.0004 | 0.0001 | 0.0000 |
| Earning's children (What) | | | -0.0029 | 0.0028 | 0.2860 | -0.0049 | 0.0032 | 0.1290 |
| Total people by household | | | 0.0752 | 0.0294 | 0.0110 | 0.0195 | 0.0291 | 0.5040 |
| Years of schooling | | | 0.2210 | 0.0719 | 0.0020 | 0.3994 | 0.0735 | 0.0000 |
| Years of schooling$^2$ | | | 0.0109 | 0.0087 | 0.2130 | -0.0052 | 0.0088 | 0.5510 |
| White | | | -0.1459 | 0.1070 | 0.1730 | 0.1201 | 0.1015 | 0.2370 |
| Male | | | 0.6201 | 0.0949 | 0.0000 | -0.1786 | 0.0903 | 0.0480 |
| Max parent's education | | | 0.0503 | 0.0173 | 0.0040 | 0.1109 | 0.0162 | 0.0000 |
| Max parent's age | | | 0.0103 | 0.0050 | 0.0400 | 0.0214 | 0.0049 | 0.0000 |
| Number of children below 7 | | | -0.2800 | 0.0669 | 0.0000 | -0.2619 | 0.0638 | 0.0000 |
| Rank of child | | | - | - | - | - | - | - |
| North | | | 0.3019 | 0.1848 | 0.1020 | 0.3707 | 0.1741 | 0.0330 |
| Northeast | | | 0.6628 | 0.1291 | 0.0000 | 0.6156 | 0.1260 | 0.0000 |
| South | | | -0.0736 | 0.1440 | 0.6090 | -0.5285 | 0.1384 | 0.0000 |
| Center-West | | | 0.1186 | 0.1635 | 0.4680 | -0.0937 | 0.1538 | 0.5420 |
| Urban non metroplitan | | | 0.4465 | 0.1439 | 0.0020 | -0.7331 | 0.1403 | 0.0000 |
| Rural | | | -0.1145 | 0.1298 | 0.3780 | -0.3243 | 0.1216 | 0.0080 |
| Means of earnings by cluster | | | 0.0048 | 0.0038 | 0.2030 | -0.0100 | 0.0050 | 0.0440 |
| Intercept | | | -2.2590 | 0.3516 | 0.0000 | -1.4724 | 0.3444 | 0.0000 |

Source: PNAD/IBGE 1999 and author's calculation

Table 5: Simulated effect of Bolsa Escola on schooling and working status (all children 10-15 years old)

| | All Households | | | |
|---|---|---|---|---|
| | Not Studying | Working and Studying | Studying | Total |
| Not Studying | 66.6% | 9.0% | 24.4% | 5.8% |
| Working and Studying | - | 98.1% | 1.9% | 16.9% |
| Studying | - | - | 100.0% | 77.3% |
| Total | 3.9% | 17.1% | 79.1% | 100.0% |
| | **Poor Households** | | | |
| | Not Studying | Working and Studying | Studying | Total |
| Not Studying | 52.0% | 13.4% | 34.6% | 9.1% |
| Working and Studying | - | 99.0% | 1.0% | 23.7% |
| Studying | - | - | 100.0% | 67.2% |
| Total | 4.7% | 24.7% | 70.6% | 100.0% |

Source: PNAD/IBGE 1999 and author's calculation

Table 6: Simulated effect on schooling and working status of alternative specifications of conditional cash transfer program (all children 10-15 years old)

| | **All Households** | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Original | Bolsa escola's program | Scenario 1 | Scenario 2 | Scenario 3 | Scenario 4 | Scenario 5 | Scenario 6 |
| Not going to school | 5.8% | 3.9% | 2.8% | 2.8% | 3.6% | 2.4% | 2.3% | 5.8% |
| Going to school and working | 16.9% | 17.1% | 17.1% | 17.1% | 17.2% | 17.2% | 17.2% | 16.8% |
| Going to school and not working | 77.3% | 79.1% | 80.1% | 80.1% | 79.3% | 80.4% | 80.4% | 77.4% |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |
| | **Poor Households** | | | | | | | |
| | Original | Bolsa escola's program | Scenario 1 | Scenario 2 | Scenario 3 | Scenario 4 | Scenario 5 | Scenario 6 |
| Not going to school | 9.1% | 4.7% | 2.5% | 2.5% | 4.7% | 2.5% | 2.5% | 9.0% |
| Going to school and working | 23.7% | 24.7% | 25.1% | 25.1% | 24.8% | 25.3% | 25.3% | 23.6% |
| Going to school and not working | 67.2% | 70.6% | 72.4% | 72.3% | 70.5% | 72.2% | 72.2% | 67.3% |
| Total | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% | 100.0% |

Source: PNAD/IBGE 1999 and author's calculation

Table 7. Simulated distributional effects of alternative specifications of the conditional cash transfer program

| | Original | Bolsa escola's program | Scenario 1 | Scenario 2 | Scenario 3 | Scenario 4 | Scenario 5 | Scenario 6 |
|---|---|---|---|---|---|---|---|---|
| Mean Income per capita | 253.9 | 255.0 | 256.1 | 255.8 | 255.2 | 256.5 | 256.3 | 255.1 |
| Inequality measures | | | | | | | | |
| Gini coefficient | 0.594 | 0.589 | 0.584 | 0.585 | 0.588 | 0.583 | 0.584 | 0.589 |
| Mean logarithmic deviation | 0.704 | 0.670 | 0.647 | 0.652 | 0.669 | 0.644 | 0.649 | 0.668 |
| Theil index | 0.710 | 0.700 | 0.690 | 0.692 | 0.699 | 0.687 | 0.689 | 0.699 |
| Generalized Entropy (2) | 1.605 | 1.589 | 1.572 | 1.575 | 1.585 | 1.565 | 1.569 | 1.587 |
| Poverty measures | | | | | | | | |
| Poverty headcount | 30.5% | 29.5% | 28.2% | 28.5% | 29.5% | 28.2% | 28.5% | 29.4% |
| Poverty gap | 13.5% | 12.4% | 11.2% | 11.5% | 12.4% | 11.2% | 11.5% | 12.3% |
| Total square deviation from poverty line | 8.1% | 7.1% | 6.2% | 6.4% | 7.1% | 6.2% | 6.4% | 7.0% |
| Annual cost of the program (million Reais) | | 1,668 | 3,228 | 3,000 | 1,944 | 3,984 | 3,720 | 1,668 |

Source: PNAD/IBGE 1999 and author's calculation