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Conditional Cash Transfers, Schooling and Child Labor: Micro-Simulating 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 15 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 has 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 is available. Thus, they are indispensable when designing a program or reforming existing ones.

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 was created by a law in April 2001, within the broader context of the social development initiative known as *Projeto Alvorada*. It 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

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[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.

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[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 in the 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

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[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), and Bhalotra (2000). On policy, see Grootaert and Patrinos (1999).

available for 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. The econometric literature on child labor and schooling often relies on reduced form models that permit to test the significance of particular variables but not always more structural hypotheses. Few existing models would allow for the ex-ante evaluation of a conditional transfer program like Bolsa Escola. [10]

In light of these difficulties, our aims are modest and our approach is operational. We do not attempt to estimate a fully structural model of the demand for schooling based on some representation of the intra-household labor allocation. We aim simply to obtain orders of magnitude for the likely effects of transfer programs of the Bolsa Escola type. In doing so, we make the choice to limit the structural aspects of the modeling exercise to the strict minimum, and thus to depart as little as possible from standard reduced form models of child occupation.

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. In particular, we 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.[11] 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 is thus 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.

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[10] This is even true for an explicit structural model like Gertler and Glewwe (1990).

[11] For a discussion of how intra-household bargaining affects labour supply behaviour by members, see Chiappori (1992) or Bourguignon and Chiappori (1994).

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[12], so that:

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

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 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 γ_j and α_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 on the one hand, and the current income of the household on the other. 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's income variable, y_{ij}, as follows. Denote the

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[12] Several authors model the joint labor/schooling decision for children as a binomial or sequential Probit rather than a multinomial logit – see for instance Canagarajah and Coulombe (1997) and Grootaert and Patrinos (1999). Because this specification has no direct utility maximizing interpretation, it is not convenient for the kind of simulation undertaken in this paper. A multinomial Probit would be more appropriate but its estimation is somewhat cumbersome.

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:

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

where X_i is the set of individual characteristics defined earlier – which includes standard Mincerian variables like age and schooling achieved - u_i is a random term that stands for unobserved earnings determinants and Ind( ) is the indicator function. 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 alternatives j is defined as follows:

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

where it is assumed that y_{ij} values the output of 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 still working in the market means a (proportional 1-M) reduction in 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, from equation (3).

Replacing (4) in (2) leads to :

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

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

We now have a complete simulation model. If all coefficients α, β, γ 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[13] going to school to a transfer T, (5) would be replaced by:

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

This simply adds a positive transfer amount T to the household's income term which is independent of the child's occupation (Y_{-i}), provided that the child is attending school (i.e in states j=1 or j=2, but not in state j=0). Note that this is what makes this transfer conditional: in solving its occupational problem, the household knows that T will only accrue if the household is in states 1 or 2 – i.e. if the child is going to school – and that the transfer will be zero otherwise. An unconditional transfer, conversely, would add to family income Y independently of state j.

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 β, γ, α, w_i and the v_{ij}'s.

Estimation of the discrete choice model

Assuming that the v_{ij} are i.i.d. 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. In this model, the probability that household i will select occupational choice k is given by:

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

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

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

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

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[13] 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.

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 α_0, α_1 and α_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 α_0, α_1 and α_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.[14] It then follows that:

$$
\alpha_1 = \frac{\hat{a}_1 - \hat{b}_1}{1 - M}, \quad \alpha_0 = \alpha_1 - \hat{a}_1, \quad \alpha_2 = \alpha_1 + \hat{a}_2 - \hat{a}_1 \text{ and } D = \frac{\hat{b}_2 + \alpha_0}{\alpha_2}
\tag{11}
$$

Of course, a test of the relevance of the identifying assumption is that α_0, α_1 and α_2 must be positive. One could also require that the value of D 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

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

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:

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

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 earnings equation simultaneously by maximum likelihood techniques. This is a rather cumbersome procedure.

We adopt a simpler approach, which has the advantages of transparency and robustness. It consists of estimating (3) by OLS, and then generating 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 earnings function for possible 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 is now known that Lee's procedure is justified and efficient only in a rather unlikely particular case.[15] 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 for selection using standard techniques and no convincing instrument led to rather implausible results.

Simulating programs of the Bolsa Escola type

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[15] See Schmertmann (1994), Bourguignon et al. (2001), Dahl (2002)

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:

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

The conditions associated with modalities 1 and 2 stand for the means test, $Y°$ being the income threshold. Note that these conditions are defined in terms of monetary income, which explains why the contribution of the child to domestic production in the case $S=2$ is not taken into account.

As mentioned above, only the differences between the utilities corresponding to the three cases matter, so that one only needs to know the differences $(\beta_j-\beta_0)$, $(\gamma_j-\gamma_0)$ and $(v_{ij}- v_{i0})$ – but all three coefficients α_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 $Y°$ and the transfer T could be made dependent on characteristics of either the household (H) or the child (X). 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 estimation 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 model the effects (on the occupational choice) of the ceiling of R$45 on transfers to any single household. The reason is that by ignoring multi-children interactions in the

model, it is as though we had effectively assumed that all households consisted of a 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 if there were more than one child in 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[16]. 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.[17] At the simulation stage, however, transfers are 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 consistently declines – and work increases – with age. Whereas only 2.6% of

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

[17] 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.

ten year-olds are out of school, the figure for fifteen year-olds is 13.6%. Whereas some 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 oldest children.

It is important to stress the PNAD contains data on school enrollment but not on actual school attendance. We are therefore unable to model the Bolsa Escola's minimum 85% attendance condition as a separate constraint to enrolment. Our results would no longer be valid if a significant number of enrolled children had attendance rates regularly below 85%. The latest administrative data from the Secretaria do Programa Nacional de Bolsa-Escola (the agency that runs the federal program) indicates that fewer than 3% of all beneficiaries had failed to meet the 85% frequency requirement, in the latest quarter for which data is available (July-September, 2002). Whether this is also true for non-beneficiaries is the assumption we are forced to make in the absence of the relevant data.

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, and proportionately most common in rural areas. Interestingly, households where children both work and go to school are generally in an intermediate position between those whose children specialize, but are often 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. With age-specific averages ranging from around R$80 to R$130 per month, children's earnings represent approximately half the minimum wage, an order of magnitude that seems rather reasonable. These amounts are much above the R$15 transfer that is granted by the *Bolsa Escola* program for children enrolled in school. Note, however, that observed earnings are not a good measure for the opportunity cost of schooling, since school attendance is evidently consistent with some amount of market work. We return to this issue below.

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. This allows us to take fully into account the interaction between a child's age, her last grade completed and, by subtraction, age out of school. This specification allows for considerably more flexible estimation of the age effects than the simple introduction and interaction of dummy variables. The simulations reported in the next section rely on the age-specific models, but in this section we report only the joint estimation results, 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), for the pooled sample.[18] Geographical variables, race and gender have the expected signs, and the same qualitative effect as for adults, although the racial dummy is less significant. The coefficient on the logarithm of the (drop-out) median earnings of children of a given age in his or her state is positive, and both statistically and economically significant. This is in fact an important variable, which is included as a proxy for the spatial variation in the demand for child labor of different ages. It is constructed as the median of the distribution of earnings for children with exactly 10 (or 11, 12, 13, 14, 15, as appropriate) years of age, in her state in Brazil, excluding the child herself, provided there are at least two elements in this vector.[19] This variable is our identifying instrument, and will not appear in the multinomial logit model (12). The intuition is that demand conditions in the age and spatially specific labor market facing the child affect her occupational decision only through her potential earnings variable.

It is also the fact that median earnings are computed for age-specific distributions in each state which explains why the linear experience term (Age) in Table 3 is insignificant. In an alternative (unreported) specification for the pooled sample which omitted the "median earnings by state" variable, an additional year of age increased earnings by approximately 40 per cent. But there was a clear non-linearity in the way age

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[18] Analogous results for the 10, 11, 12, 13, 14 and 15 year-old samples are available from the authors on request.

[19] Whenever there were fewer than three working children of a certain age in the 1999 PNAD sample for the state, the drop-out median was taken in the region (North, Northeast, Southeast, South, Centre-West).

affected earnings, which is reflected in changes in the coefficient estimates when the model is separately estimated. Indeed, these non-linearities and interactions between age and other determinants are the reason why the separate specification was preferred for the simulations using the model. Regional dummies were also all insignificant, and were dropped. The effect of previous schooling is positive and significant.

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 34 per cent less than a dropout, for the pooled sample. This seems like a reasonable order of magnitude.

The results from the estimation of the multinomial logit for occupational choice also appear eminently plausible. Marginal effects and the corresponding p-values for the pooled sample are reported in Table 4.[20] The reference category was "not studying" ($j = 0$), throughout. Once parental education is controlled for, household income (net of the child's) has a positive, but very small effect on the schooling decision, whereas the child's own (predicted) earnings have a negative effect. Household size reduces the probability of studying, compared to the alternatives.[21] Previous schooling at a given age has a positive effect. White children are more likely than non-white children to be studying and not working. Boys are less likely than girls to be studying only, but more likely to be working and studying, which suggests a possible pattern of specialization in domestic work by girls, and market work by boys. 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 α_1 and α_2, and $0 < D < 1$ - hold or not. For the pooled sample and using (11), we find that:

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[20] Analogous results for each of the age-specific models (for 10, 11, 12, 13, 14 and 15 year-olds) are available from the authors upon request.

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

$$\alpha_1 = \frac{0.0001 + 0.0120}{1 - Exp(-0.3444)} = 0.0415, \ \alpha_0 = .0414, \ \alpha_2 = 0.0417, \ D = \frac{-0.0101 + 0.0414}{0.0417} = 0.7510$$

The coefficients of income in the utility of alternatives $j = 1$ and 2 are thus positive, which is in agreement with the original model. But there are very close to each other, which suggests that income effects are likely to be small. According to the value obtained for parameter D, children who are going to school but do not work on the market are estimated to provide domestic production for approximately three quarters of their potential market earnings. This is very close to the estimated value for M [= Exp(-0.3444) = 0.709]. 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 rather similar 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.

The values implied for M and D, as well as for all α and β parameters, for each of the age-specific models, are reported in Table 5. There is some variation across age-groups, which is due at least in part to the lesser precision of the estimation in the smaller sub-samples. Apart from a value for D just greater than unit in the 11 year-old sample, all of the parameters conform to the theoretical restrictions. Overall, the estimates obtained both from the multinomial discrete occupational choice model and from the earnings equation seem therefore remarkably consistent with rational, utility-maximizing behavior. We may thus expect simulations run on the basis of these models and of 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 programs 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, without making strong assumptions about the future path of returns to schooling. 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.[22] 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.[23]

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.[24] 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 (α, β, γ, 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. System (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. This is done using the models estimated separately by age. Comparing the vector of

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[22] The evidence on educational outcomes, from an ex-post evaluation of a municipal *Bolsa Escola* program in Recife, is not conclusive. Lavinas and Barbosa (forthcoming) applied a maths test to control and treatment groups, and found that test scores were not statistically significantly different between them. There is also 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).

[23] See Coady and Morley (2003) for a brave – and sensible - attempt at estimating the present value of the gains arising from the additional education acquired due to conditional cash transfer programs.

[24] 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 has any impact on the children's occupational choices (or time allocation decisions).

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 choice $S_i = 2$. The corresponding transition matrix is shown in Table 6 for all children between 10 and 15, as well as for all children in the same age group living in poor households.[25] In interpreting Table 6, we should remember that the observed "original" vector corresponds to the actual situation in September 1999, prior to the introduction of the Federal Bolsa Escola program we are simulating. It is therefore an appropriate "control" sample for comparing with the counterfactual "treatment" population obtained from the simulations.[26]

Despite the small value of the proposed transfer, Table 6 suggests that four out of every ten children (aged 10-15) who are presently not enrolled in school would get enough incentive from Bolsa Escola to change occupational status and go to school. Among them, just over a third would enroll, but remain employed on the labor market. The other two thirds would actually cease work outside their household. This would reduce the proportion of children in that age range outside school from 6.0% to 3.7% - a rather sizable effect.

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 $S_i = 0$, the group of children both studying and working would actually grow in the simulated scenario, albeit marginally.

The impacts are even more pronounced among the poor – who are the target population for the program. According to the poverty line being used, the incidence of

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[25] 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).

[26] There were a number of similar municipal programs in operation at the time, such as the Recife Scholarship Program. There were few of them, however, and they were usually very small, so that the frequency of beneficiaries of these programs in the national 1999 PNAD sample would have been tiny. The Recife program, for instance, reached an estimated sixteen hundred families by December 1999 (see Lavinas and Barbosa, forthcoming). Additionally, a number of these local programs have continued in existence concurrently with the federal program, so that the inclusion of any income from them among "other incomes" in any family that might have been sampled in the PNAD 1999 is also appropriate in a comparison between the no-treatment control group, and the counterfactual treatment sample. The point is that treatment, defined as the *federal* design of the Bolsa Escola program, only came into being in April 2001.

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 6 shows that dropouts are much more frequent among them – 8.9 instead of 6.0 per cent for the whole population. It also shows that *Bolsa Escola* is more effective in increasing their school enrollment. The fall in the proportion of dropouts is of almost 60%, rather than 40%. As a result, the simulation suggests that *Bolsa Escola* could increase the school enrollment rate among the poor by approximately 5.2 percentage points. Once again, this increase comes at the expense of the "not studying category", whose numbers are more than halved, rather than of the "working and studying" category, which actually becomes marginally more numerous.

That the impact of the program is stronger among the poor simply reflects the binding nature of the means test. Families which report monthly per capita incomes greater than R$90 simply do not qualify to receive the transfer T. Nothing changes in the equations in system (12) that are relevant to them, and they thus do not respond to the program in any way. Therefore, all children changing occupational status in Table 6 live in households with incomes lower than that threshold. Since the poverty line is approximately R$75, most of them are poor.

This being said, a 60% 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 in the 70-75% range. 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$25 as the true monthly 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$10 (and less than R$25). Those who do not, must on average value education at less than that.

Because our simulations suggest that *Bolsa Escola*, as currently formulated, would still leave some 3.7% of all 10-15 year-olds outside school, it is interesting to investigate the potential effects of changing some of the program parameters. This indeed was one of the initial motivations for undertaking this kind of *ex-ante* counterfactual analysis. Table 7 shows the results of such a comparative exercise in terms of occupational choice, by reporting the factual and counterfactual occupational distributions, once again both for all children and then separately for poor households only. Table 8 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 latter 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 five 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 means-test remains unchanged, but transfer amounts and the total household ceiling are quadrupled – i.e. doubled from Scenario 1. In scenario 3, 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. In addition, the household ceiling is removed.[27] In scenario 4, transfer amounts were unchanged, but the means-test was raised from R$90 to R$120. Scenario 5 simulated a targeted transfer exactly as in Bolsa Escola, but with no conditionality: every child in households below the means-test received the benefit, with no requirement relating to school enrollment.

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[27] The means-test remains at R$90.

Table 7 gives rise to three main results. First of all, a comparison of Scenario 5 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 5 are almost identical to the original data (i.e. no program). This is consistent with the very small marginal family income effect reported in Table 4, and 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 shown in the *Bolsa Escola* column.

Second, scenarios 1 and 2 reveal that the occupational impact of the program is reasonably elastic with respect to the transfer amount. The proportion of un-enrolled children drops by almost one percentage point (i.e. some 25%) in response to a doubling of the transfers in Scenario 1, and then another 25% as transfers double again from Scenario 1 to Scenario 2. This effect is even more pronounced among poor families, where the R\$60 transfers in Scenario 2 cause a reduction in the un-enrolled to 0.6%, from 3.7% under the current program design. Scenario 3 suggests that it doesn't matter much, in aggregate terms, whether this increase in transfers is uniform across ages, or rises with the age of the child. Finally, scenario 4 suggests that occupational effects are less sensitive to rises in the means-test than to the transfer amounts.

Results are considerably less impressive in terms of the program's first stated objective, namely the reduction in current poverty (and inequality) levels. Table 8 suggests that the program, as currently envisaged, would only imply a 1.3 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 8 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 - 3). Quadrupling the transfers to R\$60 per child, up to a ceiling of R\$180

per family, for instance, would further reduce the Brazilian poverty headcount by 4.2 percentage points.[28] But program costs would climb from R\$2billion to R\$8.5billion, that is from .2 to .85% of GDP. An increase in the means-test would not help much, as indicated by Scenario 4. 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 poor 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 in that 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). The more sizable poverty reductions simulated under our scenario 2, in which transfers are more generous, point in the same general direction.

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

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[28] The simulated 2.2 percentage-point decline in the P(2) is also quite respectable.

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 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 which are not enrolled in school work only in the market, and make no 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 in the 0.70-0.75 range, independently of 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 forty percent of 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: sixty percent 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 just over 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 going to school	2.6%	2.3%	3.4%	5.9%	8.5%	13.6%	6.1%
Going to school and working	8.0%	11.0%	14.0%	18.3%	22.5%	27.1%	16.8%
Going to school and not working	89.4%	86.7%	82.6%	75.8%	69.0%	59.3%	77.1%
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 of the households they belong to (10-15 years old only)

	Not Studying	Working and Studying	Studying	Total
Age	13.6	13.2	12.3	12.51
Years of schooling	2.9	3.9	4.1	3.97
Household per capita income	87.7	110.5	203.4	180.75
Earning's children (observed)				
10	118.4	34.2	0.0	38.04
11	98.3	44.6	0.0	50.51
12	100.7	51.0	0.0	57.20
13	78.5	66.9	0.0	68.72
14	101.1	83.9	0.0	87.97
15	128.3	109.1	0.0	113.93
Years of schooling of the most educated parent	3.1	3.9	6.3	5.72
Age of the oldest parent	46.0	46.3	44.9	45.18
Number of household members	5.8	5.9	5.2	5.39
Race (White)	37.1%	40.9%	51.6%	48.9%
Gender (Male)	52.8%	65.2%	46.9%	50.3%
North	6.1%	5.6%	6.0%	5.9%
Northeast	40.3%	45.6%	29.9%	33.2%
Southeast	32.8%	26.1%	43.5%	39.9%
South	14.2%	15.9%	13.7%	14.1%
Center-West	6.7%	6.7%	6.9%	6.9%
Metropolitan area	18.2%	12.8%	30.9%	27.1%
Urban non metroplitan	47.5%	37.9%	53.0%	50.1%
Rural areas	34.3%	49.3%	16.1%	22.8%
Proportion of universe	6.1%	16.8%	77.1%	100.0%
Population	1,199,252	3,335,102	15,265,102	19,799,456

Source: PNAD/IBGE 1999 and author's calculation

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

	10 to 15 years old		
	Coefficient	S.E.	P>|z|
n obs	2431		
R^2	0.35		
Dummy WS	-0.3444	0.0360	0.0000
Age	-0.0571	0.0539	0.2900
Years of schooling	0.2528	0.0515	0.0000
$(Age-Years of schooling)^2$	0.0106	0.0025	0.0000
Male	0.2002	0.0304	0.0000
White	0.0588	0.0305	0.0540
Urban non metroplitan	-0.1020	0.0374	0.0060
Rural	-0.1089	0.0455	0.0170
Log of median of earnings by State	0.5984	0.0424	0.0000
Intercept	0.5325	0.3573	0.1360

Source: PNAD/IBGE 1999 and author's calculation

Table 4: Occupational Structure Multinomial Logit Model: Marginal Effects and p-values

	Pseudo-R^2	#obs	Working and Studying		Studying	
			ME	P>|z|	ME	P>|z|
10 to 15 years old	0.1903	43296				
Total household income			0.0000	0.0920	0.0000	0.0090
Earning's children (What)			-0.0004	0.0000	-0.0004	0.0000
Total people by household			0.0076	0.0000	-0.0038	0.0000
Age			0.0045	0.0000	-0.0705	0.1160
Years of schooling			0.0543	0.0000	0.0013	0.6880
$(Age-Years of schooling)^2$			0.0024	0.0000	-0.0014	0.7470
White			-0.0066	0.6370	0.0076	0.0000
Male			0.1238	0.0000	-0.1103	0.0000
Max parent's education			-0.0085	0.0000	0.0162	0.0000
Max parent's age			-0.0009	0.0800	0.0018	0.3270
Number of children below 5			0.0006	0.0000	-0.0166	0.0000
Rank of child			0.0199	0.0690	-0.0217	0.0000
Urban non metroplitan			0.0569	0.3960	-0.0810	0.0000
Rural			0.2282	0.0000	-0.2606	0.0000

Source: PNAD/IBGE 1999 and author's calculation

Table 5: Implied Values for the Structural Parameters in the Occupational Choice Models (pooled and age-specific)

	M	α_0	α_1	α_2	D	β_0	β_1	β_2
10-15	70.9%	0.0414	0.0415	0.0417	75.1%	0.0414	0.0294	0.0313
10	33.6%	0.0548	0.0547	0.0552	84.6%	0.0548	0.0184	0.0467
11	61.3%	0.0960	0.0958	0.0960	102.4%	0.0960	0.0587	0.0983
12	52.3%	0.0300	0.0300	0.0302	98.5%	0.0300	0.0157	0.0297
13	73.3%	0.0848	0.0850	0.0851	85.9%	0.0848	0.0623	0.0731
14	75.3%	0.0683	0.0685	0.0686	80.7%	0.0683	0.0516	0.0554
15	71.5%	0.0418	0.0420	0.0421	64.1%	0.0418	0.0301	0.0270

Source: PNAD/IBGE 1999 and author's calculation

Table 6: Simulated effect of Bolsa Escola on schooling and working status (all children 10-15 years old)

All Households			
	Not going to school	Going to school and working	Going to school and not working	Total
---	---	---	---	---
Not going to school	60.7%	14.0%	25.3%	6.0%
Going to school and working	-	97.8%	2.2%	16.9%
Going to school and not working	-	-	100.0%	77.1%
Total	3.7%	17.3%	79.0%	100.0%

Poor Households			
	Not going to school	Going to school and working	Going to school and not working	Total
---	---	---	---	---
Not going to school	41.3%	21.7%	37.0%	8.9%
Going to school and working	-	98.9%	1.1%	23.1%
Going to school and not working	-	-	100.0%	68.1%
Total	3.7%	24.7%	71.6%	100.0%

Source: PNAD/IBGE 1999 and author's calculation

Table 7: Simulated effect on schooling and working status of alternative specifications of conditional cash transfer program (all children 10-15 years old)

	Original	Bolsa escola's program	Scenario 1	Scenario 2	Scenario 3	Scenario 4	Scenario 5
All Households							
Not going to school	6.0%	3.7%	2.9%	2.2%	2.8%	3.2%	6.0%
Going to school and working	16.9%	17.3%	17.4%	17.4%	17.4%	17.5%	16.8%
Going to school and not working	77.1%	79.0%	79.7%	80.3%	79.8%	79.3%	77.2%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
Poor Households							
Not going to school	8.9%	3.7%	1.9%	0.6%	1.8%	3.6%	8.9%
Going to school and working	23.1%	24.7%	25.1%	25.4%	25.2%	24.9%	23.0%
Going to school and not working	68.1%	71.6%	72.9%	74.0%	73.0%	71.4%	68.2%
Total	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

Source: PNAD/IBGE 1999 and author's calculation

note: Scenario 1: transfer equal R$30, maximum per household R$90 and means test R$90

Scenario 2: transfer equal R$60, maximum per household R$180 and means test R$90

Scenario 3: diferent values for each age, no household ceiling and means test R$90

Scenario 4: transfer equal R$15, maximum per household R$45 and means test R$120

Scenario 5: Bolsa escola without conditionality

Table 8. Simulated distributional effect of alternative specifications of the conditional cash transfer program

	Original	Bolsa escola's program	Scenario 1	Scenario 2	Scenario 3	Scenario 4	Scenario 5
Mean Income per capita	254.2	255.4	256.5	258.8	256.4	255.6	255.3
Inequality measures							
Gini coefficient	0.591	0.586	0.581	0.570	0.581	0.585	0.586
Mean of logarithmic deviation	0.692	0.659	0.636	0.601	0.639	0.658	0.660
Theil index	0.704	0.693	0.682	0.663	0.684	0.691	0.693
Square coefficient of variation	1.591	1.573	1.556	1.522	1.558	1.570	1.574
Poverty measures							
Poverty headcount	30.1%	28.8%	27.5%	24.6%	27.7%	28.8%	28.9%
Poverty gap	13.2%	11.9%	10.8%	8.8%	10.9%	11.9%	12.0%
Total square deviation from poverty line	7.9%	6.8%	5.9%	4.6%	6.0%	6.8%	6.8%
Annual cost of the program (million Reais)		2076	4201	8487	3905	2549	2009

Source: PNAD/IBGE 1999 and author's calculation

note: Scenario 1: transfer equal R$30, maximum per household R$90 and means test R$90
Scenario 2: transfer equal R$60, maximum per household R$180 and means test R$90
Scenario 3: diferent values for each age, no household ceiling and means test R$90
Scenario 4: transfer equal R$15, maximum per household R$45 and means test R$120
Scenario 5: Bolsa escola without conditionality

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