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Conditional Cash Transfers, Schooling, and Child Labor: Micro-Simulating Brazil's Bolsa Escola Program

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A growing number of developing economies are providing cash transfers to poor people that require certain behaviors on their part, such as attending school or regularly visiting health care facilities. A simple ex ante methodology is proposed for evaluating such programs and used to assess the Bolsa Escola program in Brazil. The results suggest that about 60 percent of poor 10- to 15-year-olds not in school enroll in response to the program. The program reduces the incidence of poverty by only a little more than one percentage point, however, and the Gini coefficient falls just half a point. Results are better for measures more sensitive to the bottom of the distribution, but the effect is never large.

During the 1990s many developing economies adopted a new type of redistribution programs. Programs such as Food for Education in Bangladesh, Bolsa Escola in Brazil, and PROGRESA (Programa de Educación, Salud y Alimentación) in Mexico are means-tested conditional cash transfer programs. As the name indicates, they share two defining features, which jointly set them apart from most other programs. First, these programs include means tests, defined in terms of a maximum household income level, above which households are not eligible to receive the benefit.[1] Second, they include a behavioral conditionality that requires that members of participating households regularly undertake some prespecified action. The most common such requirement is for children between 6 and 15 years of age to remain enrolled in and actually attend school. In Mexico's PROGRESA, additional requirements, such as obligatory pre- and postnatal visits for pregnant women or lactating mothers, apply to some households.

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

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Implementation of these programs has generated considerable interest, both in the countries in which they have been implemented and in the international academic and policymaking communities. Accordingly, a great deal of effort has been placed on evaluating their impact. Two types of approach have been used to evaluate the effects of these programs on the various aspects of household welfare they seek to affect. Ex post approaches consist of comparing observed beneficiaries of the program with nonbeneficiaries, possibly after controlling for selection as beneficiaries if truly random samples are not available. Important work has recently been done on these techniques, and they have been applied to social programs in various countries.[2]

Ex ante methods consist of simulating the effect of the program on the basis of a model of the household. These models can vary widely in complexity and coverage. Arithmetic simulation models simply apply official rules to determine whether a household qualifies for the program and to calculate the amount of the transfer to be made. They use 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. They attempt to do so by observing people participating in the program and comparing them with people in a carefully constructed comparison group, selected to provide a suitable proxy for the desired 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 the program's impact might change if some aspect of the program design—the level of the means test, the nature of the behavioral conditions imposed, the level of the transfer benefits—changed. 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, because 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

2. 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 Wodon (2000). Several 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).

of alternative programs for which no ex post data are available. They are thus indispensable when designing or reforming a program.

Simulation models of redistribution schemes based on micro data sets are widely used in industrial countries, especially to analyze the effect of the numerous and often complex cash transfer instruments found there. Given the progress of direct cash transfers in developing economies, building the same type of models there may become necessary.[3] However, the specific behavioral conditionality that characterizes these programs requires modifications and a focus on different aspects of household behavior.

This article takes a step in that direction by proposing a simple ex ante evaluation methodology for conditional means-tested transfer programs and applying the method to the new federal design of Bolsa Escola in Brazil. It addresses both objectives of the program: reducing current levels of poverty and inequality, and providing incentives for reducing future poverty, through increased school enrollment among poor children today.

The next section describes the Bolsa Escola program. Section II presents the simple econometric model used for simulating the effects of the program. Given the conditionality of Bolsa Escola, the model essentially deals with the demand for schooling and therefore draws on the recent literature on child labor. Section III deals with the estimation of the model. Section IV covers the simulation of program effects and compares the program with alternative program designs. Section V summarizes the article's main findings.

I. Main Features of the Bolsa Escola Program

The Brazilian national Bolsa Escola program was created in April 2001 within the broader context of the social development initiative known as Projeto Alvorada. The law of April 2001 made uniform in terms of coverage, transfer amounts, and associated conditionality programs pioneered in the federal district and in the city of Campinas (São Paulo) in 1995 and later extended to several other localities.[4] It also provided federal funding for the program. Responsibility for monitoring, however, was left to municipal governments.

The rules of the program are simple. Households with monetary income per capita of less than 90 Reais (R$) per month—equivalent to half the minimum wage when the law was introduced—and with children age 6–15 qualify for the

3. See, for instance, Harding (1996). On the need for and problems associated with building the same type of models in developing countries, see Atkinson and Bourguignon (1991).

4. Studies of these programs include Abramovay and others (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). Much less has been written on the federal program, for the good reason that its implementation in practice is only just beginning. The description here draws on the Web site of the Ministério da Educação (www.mec.gov.brhome/bolsaesc).

program, provided that children attend school regularly.[5] The minimum rate of school attendance is set at 85 percent, and schools are supposed to report attendance rates of program beneficiaries to municipal governments. 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 on presentation of a magnetic card that greatly facilitates the monitoring of the program.

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

It is estimated that some 10 million children (in 6 million households) will benefit from this program. This represents about 17 percent of the population, reached at a cost of less than 0.5 percent of gross domestic product (GDP) (0.3 percent based on national accounts and 0.45 percent based on household income reported in the National Household Survey, PNAD, the main annual household survey in Brazil). Of course, the 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.

II. A Simple Framework for Modeling and Simulating Bolsa Escola

The effects of such a transfer scheme on the distribution of income could be simulated by simply applying the program's rules to a representative sample of households (from the PNAD, for example). For a program that has a change in household behavior as one of its explicit objectives, however, such an arithmetic simulation would clearly be inappropriate. After all, Bolsa Escola aims not only to reduce current poverty by targeting transfers to the poor but also to encourage school enrollment by poor children not currently enrolled and to discourage school 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.

A large body of literature exists on the demand for schooling in developing economies and the related issue of child labor. The main purpose of that literature is to explain why parents might prefer their children to work, within or outside the household, rather than attend school. Various motives have been

5. R$90 is equal to about US$30, at August 2002 exchange rates.

identified and analyzed from a theoretical point of view,[6] and numerous empirical attempts have been made at testing the relevance of these motives, measuring their relative strength, and evaluating the likely effects of policies.[7]

The empirical analysis is difficult for various interrelated reasons. First, the rationale behind the decision to send a child to work or school 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 internal bargaining takes place. Second, it is difficult to claim exogeneity for most plausible explanatory variables, yet no obvious instrument is 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 the significance of particular variables, but not always more structural hypotheses, to be tested. Few models would allow for the ex ante evaluation of a conditional transfer program like Bolsa Escola.[8]

In light of these difficulties, the aims of this article are modest and the approach is operational. The article does not attempt to estimate a fully structural model of the demand for schooling based on some representation of the intrahousehold labor allocation. Instead, it seeks simply to obtain orders of magnitude for the likely effects of transfer programs of the Bolsa Escola type. The structural aspects of the modeling exercise are limited to the strict minimum, so as to depart as little as possible from standard reduced-form models of child occupation.

Four crucial simplifying assumptions are made. First, the model ignores the issue of how the decision about a child's time allocation is made within the household, bypassing the discussion of unitary versus collective decisionmaking models of the household. Instead, the model of occupational choice is treated as a reduced-form reflection of the outcome of whatever decisionmaking process took place within the household.[9] Second, the decision to send a child to school is assumed to be made after all adults within the household have made their occupational decisions, and it is assumed not to affect those decisions. Third, the issue of siblings in the same household and the simultaneity of the corresponding decision is not addressed. The model that is discussed is thus supposed to apply to all school-age children within a household. Fourth, the composition of the household is assumed to be exogenous.

6. See the well-known survey by Basu (1999) as well as Baland and Robinson (2001).

7. Early contributions to that literature include Rosenzweig and Evenson (1977) and Gertler and Glewwe (1990). For more recent contributions and short surveys of the recent literature, see Freije and Lopez-Calva (2001) and Bhalotra (2000). On policy see Grootaert and Patrinos (1999).

8. This is true even for an explicit structural model, such as Gertler and Glewwe (1990).

9. For a discussion of how intrahousehold bargaining affects labor supply behavior by members, see Chiappori (1992) or Bourguignon and Chiappori (1994).

Under these assumptions, let S_i be a qualitative variable representing the occupational choice made for a child in household i. This variable takes the value 0 if the child does not attend school, 1 if the child goes to school and works outside the household, and 2 if the child goes to school and does not work outside the household. When $S_i = 0$ the child is assumed to work full-time, either inside or outside the home, with earnings observed only for work done outside the household. Similarly, $S_i = 2$ allows for the possibility that children may be employed in domestic activities at the same time they attend school. The occupational choice variable S_i is modeled using the standard utility-maximizing interpretation of the multinomial logit framework,[10] so that

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

where $S_k()$ is a latent function reflecting the net utility of choosing alternative $k(= 0,1 \text{ or } 2)$ for decisionmakers in the household. A_i is the age of child i; X_i is a vector of the child's characteristics; H_i is a vector of the characteristics of the household the child belongs to (size, age of parents, education of parents, presence of other children at school age, distance from school, and so forth); Y_{-i} is the total income of household members other than the child; and y_{ij} is the total contribution of the child toward the income of the household, depending on the child's occupational choice j. v_{ij} is a random variable that stands for the unobserved heterogeneity of observed schooling/labor behavior. If all non-income explanatory variables are collapsed into a single vector Z_i and linearized, equation 1 can be written as

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

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, it allows all possible tradeoffs between the schooling of the child and the child's future income on one hand and the household's current income on the other. The model also 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 takes some time away. This may be reflected in the definition of the child's income variable, y_{ij}, as follows. Denote the observed market earnings of the child as w_i. Assume that these are determined in accordance with the standard Becker-Mincer human capital model. Then

10. 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 here. A multinomial probit would be more appropriate, but its estimation is cumbersome.

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

where X_i is the set of individual characteristics defined earlier, including standard Mincerian variables such as age and schooling achieved; u_i is a random term that stands for unobserved earnings determinants; and Ind( ) is an indicator function that takes the value of 1 if children both attend school and work outside the household. The second term on the right-hand side takes into account the fact that the number of hours worked is likely to differ systematically across occupational categories 0 and 1. Children who attend school and work outside the household presumably have less time available and may thus earn less. Based on equation 3, the child's contribution to the household income, y_{ij}, in the various alternatives j is defined as

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

where y_{ij} is assumed to measure the value of 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 attend school. Going to school while still working outside the household means a (proportional $1 - M$) reduction in domestic and market income. Going to school without working outside the household 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 equation 4 in equation 2 yields

$$\text{(5)} \qquad U_i(j) = S_j(A_i, X_i, H_i; Y_{-i} + y_{ij}) + v_{ji} = Z_i.\gamma_j + Y_{-i}\alpha_j + \beta_j.w_i + v_{ij},$$

with $\beta_0 = \alpha_0 K; \beta_1 = \alpha_1 MK$; and $\beta_2 = \alpha_2 DK$.

If all coefficients α, β, and 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

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

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, equation 5 would be replaced by

$$\text{(7)} \quad U_i(j) = Z_i.\gamma_j + (Y_{-1} + BE_{ij}).\alpha_j + \beta_j.w_i + v_{ij} \text{ with } BE_{i0} = 0 \text{ and } BE_{i1} = BE_{i2} = T.$$

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

11. It is simpler to discuss the estimation problem under this simplifying assumption. The means test is reintroduced, without any loss of generality, at the simulation stage.

attending school (that is, 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 accrue only if the household is in states 1 or 2 (that is, the child attends school) and that the transfer will be 0 otherwise. An unconditional transfer, conversely, would add to family income Y independent of state j.

Under these assumptions, equation 7 is the full reduced-form model of the occupational choice of children. It allows 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}s are independently and identically distributed across sample observations with a double exponential distribution leads to the well-known multilogit 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

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

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

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

for $j = 1,2$ 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 equations 6 and 7 indicates that because 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) 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 equation 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 D allows one 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 children working outside the household and not attending school have zero domestic production, that is, $K = 1$. 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

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

Of course, a test of the relevance of the identifying assumption is that α_0,α_1, and α_2 are 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 known only 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

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

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

Estimation of Potential Earnings

The discrete choice model requires potential earnings 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, however.

We adopt a simpler approach, which has the advantages of transparency and robustness. It consists of estimating equation 3 by ordinary least squares (OLS) and then generating random terms u_i for nonworking children by drawing in the distribution generated by the residuals of the OLS estimation.

Correcting the estimation of the earnings function for possible selection bias was problematic for several reasons. First, instrumenting earnings with a selection bias correction procedure requires finding instruments that 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

12. This assumption could be weakened using some limited information on hours of work available in the survey.

generalization of the Heckman procedure, but that procedure is justified and efficient only in a rather unlikely particular case (see Schmertmann 1994, Bourguignon and others 2001, Dahl 2002). For both of these reasons, failing to correct for possible selection bias in equation 3 did not seem too serious a problem, whereas trying to correct for selection using standard techniques and no convincing instrument led to rather implausible results.

Simulating Programs of the Bolsa Escola Type

The model given in equations 6 and 7 does not provide a complete representation of the choice faced by households in the presence of a program such as Bolsa Escola, 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 following three conditional cases:

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

The conditions associated with modalities 1 and 2 stand for the means test, where Y° is the income threshold. 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 previously mentioned, what matters is the differences between the utilities corresponding to the three cases, so that one needs to know only $(\beta_j - \beta_0)$,$(\gamma_j - \gamma_0)$ 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 to move from choice 0 (no schooling) to choices 1 or 2, or from choice 1 to choice 2. A household might move from choice 1 to choice 2 if it did not qualify for the transfer T when the child both worked and attended school but qualified if the child stopped 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 IV.

Two important limitations of the framework are worth nothing, both arising from the set of assumptions. The first is that we cannot 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 interactions among children in the same household, the model effectively assumes that all households consist of a single child, from a behavioral point of view. In the nonbehavioral part of the welfare simulations (reported in section IV), however, each child is treated separately and the R$45 limit applied.

The second limitation has to do with the exogeneity of nonchild income Y_{-i}. This exogeneity would clearly be a problem if there were more than one school-age child. It is also unrealistic when only adult income is taken into account. The presence of the means test might affect the labor supply behavior of adults, because there are circumstances in which it might be in the family's interest to work slightly less to qualify for Bolsa Escola. Note, however, that this effect might be muted 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.

III. Descriptive Statistics and Estimation Results

The model (equations 3 and 12) was estimated on data from the 1999 PNAD household survey, a survey based on a sample of about 60,000 households representative of the national population.[13] Although all children age 6–15 qualify for participation in the program, the model is estimated only for 10- to 15-year-olds, because school enrollment below age 10 is nearly universal.[14] At the simulation stage, however, transfers are simulated for the universe of qualifying 6- to 15-year-olds.

Among 10- to 15-year-old children in Brazil in 1999, 77 percent were enrolled in school and did not work (table 1). About 17 percent both worked and were enrolled in school; 6 percent were not enrolled in school. These figures conceal considerable variation across ages: school attendance consistently declines—and work increases—with age. Just 2.6 percent of 10-year-olds, but 13.6 percent of 15-year-olds, were not in school. About 90 percent of 10-year-olds were enrolled in school and did not work, whereas fewer than 60 percent of 15-year-olds did 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 that the PNAD survey contains data on school enrollment, not school attendance. It is therefore not possible to model the Bolsa Escola's minimum 85 percent attendance condition as a separate constraint to enrollment. The results would no longer be valid if a significant number of enrolled children had attendance rates regularly below 85 percent. The latest administrative data from the Secretaria do Programa Nacional de Bolsa Escola

13. The survey does not cover rural areas in the states of Acre, Amapá, Amazonas, Pará, Rondônia, or Roraima.

14. Answers to schooling questions in the PNAD reveal that almost all children under the age of 10 attend school. Another reason to limit the estimation of the behavioral model to children age 10 and older is that the incidence of child labor at lower ages is probably measured with much greater error, because PNAD interviewers are instructed to pose labor and income questions only to people age 10 and older.

TABLE 1. Percent of Children Ages 10–15 Attending School, Working, or Both

Status	10	11	12	13	14	15	Total
Not attending school	2.6	2.3	3.4	5.9	8.5	13.6	6.1
Attending school and working	8.0	11.0	14.0	18.3	22.5	27.1	16.8
Attending school and not working	89.4	86.7	82.6	75.8	69.0	59.3	77.1

Source: National Statistical Office, National Household Survey (IBGE, PNAD) 1999, and authors' calculations.

(the agency that runs the federal program) indicate that less than 3 percent of beneficiaries failed to meet the 85 percent frequency requirement in the latest quarter for which data were available (July–September 2002). In the absence of the relevant data, the model assumes that this is also true for nonbeneficiaries.

The mean individual and household characteristics of children, by occupational category, reveal that children not going to school are both older and less educated than those enrolled (table 2). As expected, households with school dropouts are on average poorer, less educated, and larger than households in which children attend school. Dropping out of school and working are relatively more frequent among nonwhite children and children in the northeast. Both forms of behavior are least common in metropolitan areas and most common in rural areas. Interestingly, households in which 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 dropouts.

A remarkable feature of table 2 is the observed amount of children's earnings when they work and do not attend school. With age-specific averages ranging from about R$80 to R$130 per month, children's earnings represent about half the minimum wage, an order of magnitude that seems reasonable. These amounts are much higher than the R$15 transfer granted by the Bolsa Escola program for children enrolled in school. Note, however, that observed earnings are not a good measure of the opportunity cost of schooling, because school attendance is evidently consistent with some amount of market work, an issue addressed later.

Because of the great behavioral variation across age groups even within the 10–15-year range (as revealed, for instance, in table 1), the model is estimated separately for each age, as well as for the pooled sample of all 10- to 15-year-olds (tables 3 and 4). Doing so allows the interaction between a child's age, the last grade completed, and, by subtraction, the age out of school to be taken fully into account. 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; this section 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 2. Characteristics of Children Ages 10–15 and Their Households (Sample Means)

Characteristic	Not attending school	Working and attending school	Attending school	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 (R$)	87.7	110.5	203.4	180.75
Observed children's earnings by age (R$)				
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 more educated parent	3.1	3.9	6.3	5.72
Age of older parent (years)	46.0	46.3	44.9	45.18
Number of household members	5.8	5.9	5.2	5.39
White (%)	37.1	40.9	51.6	48.9
Male (%)	52.8	65.2	46.9	50.3
Region (%)				
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 nonmetropolitan area	47.5	37.9	53.0	50.1
Rural area	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: National Statistical Office, National Household Survey (IBGE, PNAD) 1999, and authors' calculations.

The results of the OLS estimation of the earnings function (equation 3) for the pooled sample reveal that the geographical variables, race, and gender have the expected signs and the same qualitative effect as for adults; the racial dummy is less significant (table 3).[15] The coefficient on the log of the (dropout) median earnings of children of a given age in their state is positive and both statistically and economically significant. This is an important variable, included as a proxy for the spatial variation in the demand for child labor of different ages. It is

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

TABLE 3. Log Earnings Regression for Reported Earnings of Children Ages 10–15

Item	Coefficient	SE	$P > \|z\|$
Number of observations	2,431	n.a.	n.a.
R^2	0.35	n.a.	n.a.
Dummy (Working and Studying)	−0.3444	0.0360	0.0000
Age	−0.0571	0.0539	0.2900
Years of schooling	0.2528	0.0515	0.0000
Age minus years of schooling squared	0.0106	0.0025	0.0000
Male	0.2002	0.0304	0.0000
White	0.0588	0.0305	0.0540
Urban nonmetropolitan	−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

n.a., Not applicable.

Source: National Statistical Office, National Household Survey (IBGE, PNAD) 1999, and authors' calculations.

TABLE 4. Occupational Structure Multinomial Logit Model: Marginal Effects and *p*-Values for Children Ages 10–15

	Working and attending school		Attending school	
Item	Marginal effect	$P > \|z\|$	Marginal effect	$P > \|z\|$
Total household income	0.0000	0.0920	0.0000	0.0090
Children's earnings (predicted)	−0.0004	0.0000	−0.0004	0.0000
Household size	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 minus years of schooling squared	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
Years of schooling of most educated parent	−0.0085	0.0000	0.0162	0.0000
Age of oldest parent	−0.0009	0.0800	0.0018	0.3270
Number of children below age 5	0.0006	0.0000	−0.0166	0.0000
Rank of child (oldest to youngest)	0.0199	0.0690	−0.0217	0.0000
Urban nonmetropolitan	0.0569	0.3960	−0.0810	0.0000
Rural	0.2282	0.0000	−0.2606	0.0000

Note: Pseudo $R^2 = 0.1903$; number of observations = 43,296.

Source: National Statistical Office, National Household Survey (IBGE, PNAD) 1999, and authors' calculations.

constructed as the median of the distribution of earnings for children exactly 10 (or 11, 12, 13, 14, 15, as appropriate) years old, in their state in Brazil, excluding the child studied, provided there are at least two elements in this vector.[16] This variable is the 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 the child's occupational decision only through the potential earnings variable.

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 that omits the *median earnings by state* variable, an additional year of age increases earnings by about 40 percent. But there is clear nonlinearity in the way age affects earnings, reflected in changes in the coefficient estimates when the model is estimated separately. Indeed, these nonlinearities and interactions between age and other determinants are the reason why the separate specification was preferred for the simulations using the model. All 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 and works outside the household reduces total earnings relative to a comparable child who only works. If one interprets this coefficient as reflecting fewer hours of work, then a child going to school works on average 34 percent 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 plausible. Marginal effects and the corresponding p-values for the pooled sample are reported in table 4.[17] The reference category is *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 with the alternatives.[18] Previous schooling at a given age has a positive effect. White children are more likely than nonwhite children to be attending school and not working. Boys are less likely than girls to be in school 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

16. When fewer than three working children of a particular age were included in the 1999 PNAD sample for the state, the dropout median in the region (north, northeast, southeast, south, center-west) was used.

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

18. To the extent that household size reflects a larger number of children, this is consistent with Becker's quantity–quality tradeoff.

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 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. Using equation 11 for the pooled sample yields

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

The coefficients of income in the utility of alternatives $j = 1$ and 2 are thus positive, consistent with the original model. They are very close to each other, however, suggesting that income effects are likely to be small. According to the value obtained for parameter D, children who are in school but do not work outside the household are estimated to provide domestic production for about three-quarters of their potential market earnings. This is very close to the estimated value for $M[= \mathrm{Exp}(-0.3444) = 0.709]$. Because 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 nonmarket work by children studying (and not working outside the household) is similar to the market value of work by those studying (and working outside the household). If there were little selection on unobservables into market work, this is exactly what one would expect.

For each of the age-specific models, the values implied for M and D, as well as for all α and β parameters, reveal some variation across age groups, due at least in part to the loss of precision of the estimation in the smaller subsamples (table 5). With the exception of a value for D just greater than 1 in the 11-year-old sample, all of the parameters conform to the theoretical restrictions. Overall, the estimates obtained from both the multinomial discrete occupational choice model and the earnings equation thus seem remarkably consistent with rational

TABLE 5. Implied Values for Structural Parameters in Occupational Choice Models (Pooled and Age-Specific)

Age (years)	M (percent)	α_0	α_1	α_2	D (percent)	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: National Statistical Office, National Household Survey (IBGE, PNAD) 1999 and authors' calculations.

utility-maximizing behavior. The simulations run on the basis of these models and the identifying structural assumptions about the parameter K can thus be expected to yield sensible results.

IV. An Ex Ante Evaluation of Bolsa Escola and Alternative Program Designs

Bolsa Escola and many conditional cash transfer programs like it aim to reduce current poverty (and sometimes inequality) through targeted transfers and to reduce future poverty by increasing the incentives for the poor to invest in human capital. Their success in reducing future poverty 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, information that is not included in this data set.[19] Moreover, whether more human capital, however measured, will reduce poverty in the future 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.[20]

The results may reveal something about the intermediate target of increasing school enrollment. Although this is not sufficient to establish whether the program will have an impact on future poverty, it is at least necessary.[21] 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 to the original data the decision system 12—with behavioral parameter values (α,β,γ,M, and D) estimated from equations 9–11—and policy parameter values (T and Y^0) taken from the actual specification of Bolsa Escola. 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 child. This is done using the models estimated separately by age. Comparing the vector of occupational choices thus generated

19. The evidence on educational outcomes from an ex post evaluation of a municipal Bolsa Escola program in Recife is not conclusive. Applying a maths test to control and treatment groups, Lavinas and Barbosa (forthcoming) found that test scores of the two groups are not statistically significantly different. In addition, the Education Ministry's Sistema de Acompanhamento do Ensino Básico (SAEB) includes some information on outcomes, but the period covered is insufficiently long (see Albernaz and others 2002).

20. See Coady and Morley (2003) for a brave—and sensible—attempt at estimating the present value of the gains from the additional education acquired as a result of conditional cash transfer programs.

21. One could argue that it is not even necessary, because the transfers might, by themselves, alleviate credit constraints and have long-term positive impacts (through improved nutrition, for example). The focus here is on whether the conditional nature of these transfers has any impact on children's occupational choices (or time-allocation decisions).

with the original observed vector reveals that the program leads to some children moving from $S_i = 0$ to $S_i = 1$ or 2 and from $S_i = 1$ to $S_i = 2$. The corresponding transition matrix is shown in table 6 for all children ages 10–15, as well as for all children in the same age group living in poor households.[22] In interpreting table 6, it is important to remember that the observed original vector corresponds to the actual situation in September 1999, before the introduction of the federal Bolsa Escola program being simulated. It is therefore an appropriate control sample for comparing with the counterfactual treatment population obtained from the simulations.[23]

Despite the small value of the proposed transfer, table 6 suggests that 4 of every 10 children (ages 10–15) currently not enrolled in school would receive

TABLE 6. Simulated Effect of Bolsa Escola on Schooling and Work Status of Children Ages 10–15 (percent)

	Simulated			
Status Actual	Not attending school	Attending school and working	Attending school and not working	Total
All households				
Not attending school	60.7	14.0	25.3	6.0
Attending school and working	—	97.8	2.2	16.9
Attending school and not working	—	—	100.0	77.1
Total	3.7	17.3	79.0	100.0
Poor households				
Not attending school	41.3	21.7	37.0	8.9
Attending school and working	—	98.9	1.1	23.1
Attending school and not working	—	—	100.0	68.1
Total	3.7	24.7	71.6	100.0

Source: National Statistical Office, National Household Survey (IBGE, PNAD) 1999, and authors' calculations.

22. 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 and others (2003).

23. Several similar municipal programs, such as the Recife Scholarship Program, were in operation at the time. 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 1,600 families by December 1999 (see Lavinas and Barbosa forthcoming). Several of these local programs remain in operation 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, came into being only in April 2001.

enough incentive from Bolsa Escola to change occupational status and enroll. Among them, slightly more than one-third would enroll in school but remain employed outside the household. The other two-thirds would cease work outside the household. The program would reduce the proportion of 10- to 15-year-old children outside school from 6.0 percent to 3.7 percent—a sizable effect.

The impact on children currently both studying and working would be much smaller. Barely 2 percent of these children 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 number of children both studying and working would actually increase in the simulated scenario, albeit marginally.

The impacts are even more pronounced among the poor, the program's target population. According to the poverty line used, the incidence of poverty in Brazil is 30.5 percent. However, because there are more children in poor households—this being one of the reasons why they are poor—the proportion of 10- to 15-year-old children in poor households is much higher (42 percent). The second panel in table 6 shows that dropouts are much more frequent among poor children (8.9 percent versus 6.0 percent for the whole population). It also shows that Bolsa Escola is more effective in increasing their school enrollment. The decline in the proportion of dropouts is almost 60 percent, far higher than the 40 percent figure for the sample as a whole. The simulation thus suggests that Bolsa Escola could increase the school enrollment rate among the poor by about 5.2 percentage points. This increase comes at the expense of the *not attending school* category, whose numbers are more than halved, rather than of the *attending school and working* category, which actually becomes marginally larger.

That the impact of the program is stronger among the poor simply reflects the binding nature of the means test. Families that report monthly per capita incomes greater than R$90 do not qualify to receive the transfer T. Nothing changes in the equations in system 12 that is 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. Because the poverty line is about R$75 a month, most of them are poor.

That said, a 60 percent 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 achievable with fairly small transfers (R$15 per child per month). This relatively large impact of small transfers is partly due to the fact that the value of the current contributions of children enrolled in school is a sizable proportion of their potential earnings when not attending school at all. Those proportions are exactly the interpretation of the parameters M (for those who work outside the household as well as study) and D (for those who work at home as well as study), estimated to be in the 70–75 percent range. Applying that factor to R$100, as a rough average of the earnings of children in category

$j = 0$ (see table 2), leaves about R$25 as the true monthly opportunity cost of enrolling in school. Consequently, the children who change occupation from not studying to studying 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 less than that.

Because the simulations suggest that Bolsa Escola, as currently formulated, still leaves some 3.7 percent of all 10- to 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. The exercise identifies the factual and counterfactual occupational distributions for all children and separately for poor households only (table 7). The impact of each scenario is then compared with that of the benchmark program specification in terms of poverty and inequality measures (table 8). Four standard inequality measures were selected: the Gini coefficient, the mean log deviation, the Theil-T index, and (one half of the square of) the coefficient of variation. For poverty, the three standard FGT (0, 1, 2) measures are reported, with respect to the poverty line used by Ferreira and others (2003). The results allows us to gauge impact in terms of the first objective of the program, namely, reducing current poverty (and possibly inequality).

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 (that is, doubled from scenario 1). In scenario 3 the uniform R$15 per child transfer is replaced by an age-contingent transfer in which 10-year-olds receive R$15, 11-year-olds R$20, 12-year-olds R$25, 13-year-olds R$35, 14-year-olds R$40, and 15-year-olds R$45. In addition, the household ceiling is removed.[24] In scenario 4 transfer amounts remain unchanged, but the means test is raised from R$90 to R$120. Scenario 5 simulates a targeted transfer exactly as in Bolsa Escola but with no conditionality: every child in households below the means test receives the benefit, with no requirement to enroll in or attend school.

Three main results emerge from the analysis. First, comparison of scenario 5 with 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 (that is, 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 school in order to receive the

24. The means test remains R$90.

TABLE 7. Alternative Specifications of Conditional Cash Transfer Program: Simulated Effects on Schooling and Work Status of Children Ages 10–15 (percent)

Status	Original	Bolsa Escola	Scenario 1	Scenario 2	Scenario 3	Scenario 4	Scenario 5
All households							
Not attending school	6.0	3.7	2.9	2.2	2.8	3.2	6.0
Attending school and working	16.9	17.3	17.4	17.4	17.4	17.5	16.8
Attending school and not working	77.1	79.0	79.7	80.3	79.8	79.3	77.2
Poor households							
Not attending school	8.9	3.7	1.9	0.6	1.8	3.6	8.9
Attending school and working	23.1	24.7	25.1	25.4	25.2	24.9	23.0
Attending school and not working	68.1	71.6	72.9	74.0	73.0	71.4	68.2

Note: Scenario 1: Transfer = R$30, maximum per household = R$90, means test = R$90. Scenario 2: Transfer = R$60, maximum per household = R$180, means test = R$90. Scenario 3: Different values for each age, no household ceiling, means test = R$90. Scenario 4: Transfer = R$15, maximum per household = R$45, means test = R$120. Scenario 5: Bolsa Escola without conditionality.

Source: National Statistical Office, National Household Survey (IBGE, PNAD) 1999, and authors' calculations.

TABLE 8. Simulated Distributional Effects of Alternative Specifications of Conditional Cash Transfer Program

Item	Original	Bolsa Escola	Scenario 1	Scenario 2	Scenario 3	Scenario 4	Scenario 5
Mean income per capita (R$)	254.2	255.4	256.5	258.8	256.4	255.6	255.3
Inequality measures							
Gini coefficient	0.591	0.586	0.581	0.570	0.581	0.585	0.586
Mean of log deviation	0.692	0.659	0.636	0.601	0.639	0.658	0.660
Theil index	0.704	0.693	0.682	0.663	0.684	0.691	0.693
Square coefficient of variation	1.591	1.573	1.556	1.522	1.558	1.570	1.574
Poverty measures (%)							
Poverty headcount	30.1	28.8	27.5	24.6	27.7	28.8	28.9
Poverty gap	13.2	11.9	10.8	8.8	10.9	11.9	12.0
Total square deviation from poverty line	7.9	6.8	5.9	4.6	6.0	6.8	6.8
Annual cost of program (R$ million)	n.a.	2,076	4,201	8,487	3,905	2,549	2,009

n.a., Not applicable.

Note: Scenario 1: Transfer = R$30, maximum per household = R$90, means test = R$90. Scenario 2: Transfer = R$60, maximum per household = R$180, means test = R$90. Scenario 3: Different values for each age, no household ceiling, means test = R$90. Scenario 4: Transfer = R$15, maximum per household = R$45, means test = R$120. Scenario 5: Bolsa Escola without conditionality.

Source: National Statistical Office, National Household Survey (IBGE, PNAD) 1999, and authors' calculations.

benefit—rather than the pure income effect from the transfer—that is the primary cause of the extra demand for schooling.

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 unenrolled children drops by almost 1 percentage point (that is, some 25 percent) in response to a doubling of the transfers in scenario 1 and another 25 percent as transfers double again from scenario 1 to scenario 2. This effect is even more pronounced among poor families, among whom the R$60 transfers in scenario 2 reduce the percentage of unenrolled children from 3.7 percent under the current program design to 0.6 percent. Scenario 3 suggests that it does not matter much, in aggregate terms, whether this increase in transfers is uniform across ages or rises with the age of the child. Scenario 4 suggests that occupational effects are less sensitive to increases in the means test than to the transfer amounts.

Results are considerably less impressive in terms of the poverty (and inequality) reduction. As currently envisaged, the program implies only a 1.3-percentage-point decline in the short-run incidence of poverty in Brazil, as measured by $P(0)$ (table 8) However, there is some evidence that the transfers are well targeted, because the inequality-averse poverty indicator $P(2)$ falls proportionately more than $P(0)$, from 8 percent to 7 percent. This is consistent with the inequality results: Whereas the Gini coefficient falls only half a point as a result of the scheme, measures that 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 alleviating current poverty or inequality.

The situation could be somewhat improved by increasing 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 reduce the Brazilian poverty headcount by 4.2 percentage points.[25] But program costs would climb from R$2 billion to R$8.5 billion, that is, from 0.2 percent to 0.85 percent of GDP. An increase in the means test would not help much, as indicated by scenario 4. This result is consistent with the earlier suggestion that the program already appears to be well targeted to the poor. If the program fails to lift many of the poor above the poverty line, it is because of the small size of the transfers rather than 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 percent to 9.9 percent. These results held despite the fact that the absence of a behavioral component in that simulation weakened its power, by excluding from the set of

25. The simulated 2.2-percentage-point decline in the $P(2)$ is also quite respectable.

recipients 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 scenario 2 here, in which transfers are more generous, point in the same general direction.

V. Conclusions

This article proposes a microsimulation method for evaluating conditional cash transfer program designs ex ante. It simulates 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 enroll in and attend school. It assesses 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.

A discrete occupational choice model (a multinomial logit) is estimated on a nationally representative household-level sample. The estimated parameters are then used to make predictions about the counterfactual occupational decisions of children under different assumptions about the availability and design of cash transfer programs. These assumptions are 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. Because the income values accruing to each household are not symmetric across different occupational choices, standard estimation procedures for the multinomial logit are not valid. The identification assumption was made that children not enrolled in school work only outside the household and make no contribution to domestic work. Under this assumption, the estimation of the model generated remarkably consistent results: marginal utilities of income are always positive and very similar across occupational categories. As a fraction of time spent working by those not enrolled in school, time spent working by children enrolled in school is always in the (0, 1) interval and in the 0.70–0.75 range, whether work was done within or outside the household.

Using the estimated occupational choice model to simulate the official (April 2001) design of the federal Bolsa Escola program reveals considerable behavioral response from children to the program. About 40 percent of 10- to 15-year-olds not enrolled in school enroll in response to the program, according to the model. Among poor households this proportion is even higher (60 percent). The proportion of children in the middle occupational category (*studying and working in the market*) rises marginally.

The effect on current poverty reduction is less heartening. In its original design, the Bolsa Escola program reduces the incidence of poverty by only a little more than 1 percentage point, and the Gini coefficient falls just half a point. Results are better for measures more sensitive to the bottom of the distribution, but the effect is never large.

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, though 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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