Assessing the Impact of School Subsidies in Bolivia: A Reduced Form Non-Parametric Approach

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Abstract

This paper assess the impact of Bolivia's school subsidy program, *Bono Juancito Pinto* (BJP), on school attendance. BJP is a relatively small cash transfer (less than 30 dollars per child per year) given conditional on being enrolled into a public school and on regular school attendance. Since there are no feasible alternatives of a control group, we use simple structural behavioral models to understand the school-work decision and derive counterfactuals of interest. Estimation is conducted using two dimensional kernel regression estimators. Our results sugget that BJP has been successful increasing school attendance only for young children - 6 to 8 years old, and particularly for girls. We conclude that BJP has only encourage households to enroll children to school at the proper age but has not give an additional incentive to attend to those already enrolled for the first time.

*The views and opinions expressed in this paper are those of the author and do not necessarily reflect the views and opinions of the ARU Foundation or any other institution to which he may be affiliated. A previous version of this paper circulated under the title of "An Ex-Ante Evaluation of *Bono Juancito Pinto*". Comments are welcome at whl@aru.org.bo

1 Introduction

This paper asses the impact of Bolivia's school subsidy program, *Bono Juancito Pinto* (BJP), on school attendance. The main objective of BJP is "to break the intergenerational poverty traps by providing incentives for regular school attendance". The subsidy is a relatively small cash transfer (less than 30 dollars) that is given annually to children conditional on: (1) being enrolled into a public school, and (2) having a *regular* school attendance - at least 80% of the regular days. The program started covering only the first five grades in 2006 (Presidential Decree No.28899, and was extended to the 6th grade in 2007 (Presidential Decree No.29321) and to the 7th and 8th grade in 2008 (Presidential Decree No.29652).

A number of reasons may be used to justify an impact assessment of BJP, but the most important one is that BJP is not a cheap program -at least for Bolivia. Given the program's high coverage rates (from 1.1 million children in 2006 to 1.8 million children in 2009), the investment in BJP have increased from 249 million of *bolivianos* to 380 million of *bolivianos*.

Evaluating the impact of *quasi*-universal programs is not an easy task with ex-post methods -if at all possible. Given the absence of a "comparable" control group within the country, one alternative would be to assume that the post-program outcomes under no-treatment are equal to the pre-program outcome under treatment and use*before-and-after* estimators. However, given the pro-poor growth observed during the period of implementation of the program this alternative seems not a good choice. Another alternative would be to use synthetic difference in difference methods (Abadie et.al. 2007), but the fact that most "control" countries had been implementing some kind of school attendance subsidies complicate the construction of a "synthetic" control country.

On the contrary, ex-ante evaluations can be performed even on the absence of a comparable control group. Structural behavioral model can be and has been used not only to understand the school-work decision but also to derive and estimate counterfactuals of interest. Tood and Wolpin (2006) has used ex-ante evaluation methods to asses the impact of school subsidies in Mexico. They found that -at least for girls, the predicted impacts are fairly similar to the experimental impacts, both in magnitude and in replicating age patters, with the larger impacts observed at higher ages. This document follows their approach closely.

The remaining of the document is organized as follows. Section 2 describes some simple behavioral models that help us understand the fundamentals of the school/work decision and derive the counterfactuals of interest. Section 3 presents the methods for estimation and inference. Section 4 presents the results. Section 5 concludes.

2 The Model

2.1 Case 1: Single child with exogenous wage offers

It is usually easier to understand the fundamentals of a model beginning with the simplest version. Here we present a version of the model with one child per family where wage offers are assumed to be exogenous. We describe three simple equilibrium varieties: (1) a no policy scenario, (2) a school subsidy conditional on attendance, and (3) an unconditional cash transfer per child.

2.1.1 No policy scenario

First, consider an economy where each household has only a single child and faces a single period decision about whether to send him to school or to work. Assume that household utility depends only on its consumption level and whether the child attends school. Let c be the household consumption, y its household income net of the child's earnings, and s an indicator of whether the child attends school or not, i.e.

$$s = \begin{cases} 1 & \text{if child attends school} \\ 0 & \text{if child works} \end{cases} \tag{1}$$

In this economy, the household utility maximization problem will be given by:

$$\begin{aligned} &\max_{s} U(c, s, \mu) \\ \text{s.t.: } &c = y + w(1 - s) \end{aligned} \tag{2}$$

In words, the household must decide whether send their child to school and derive utility from his attendance but forgone his contribution to household income; or send their children to work and obtain his wage as a contribution to the household income but forgone the utility derive from his attendance. Notice that in this simple model the optimal choice of the household will be given by a function of three parameters, the household income, the child wage and an unobserved heterogeneity parameter μ,

$$s^* = \phi(y, w, \mu) \tag{3}$$

2.1.2 A CCT Subsidy

Now consider the same economy with a policy that provides a cash subsidy conditional on school attendance. Let the amount of the subsidy be τ. Under this policy, the household utility maximization problem will be given by:

$$\begin{aligned} &\max_{s} U(c, s, \mu) \\ \text{s.t.: } &c = y + w(1 - s) + \tau s = (y + \tau) + (w - \tau)(1 - s) \end{aligned} \tag{4}$$

In this case, the household must decide whether send their child to school and not only derive utility but also receive some cash from their attendance but still forgone the wage he could earn in the labor market; or send his children to work and obtain his wage as a contribution to household income but forgone the utility derive from his attendance to school. It is important to notice that under the CCT subsidy the optimal choice of the household is a function of the same three parameters and the school subsidy.

$$s^{**} = \phi(\tilde{y}, \tilde{w}, \mu) = \phi(y + \tau, w - \tau, \mu) \tag{5}$$

Furthermore, it is worth to notice that the schooling choice for a family with income, y, child wage, w, and unobserved heterogeneity, μ under the CCT subsidy would be the same as the schooling choice for a family with income $\tilde{y}$, child wage $\tilde{w}$, and unobserved heterogeneity, μ, under the no policy regime. This generate two possibilities for predicting the potential affects of the CCT subsidy. One alternative would be to use data collected before the implementation of the policy and estimate the counterfactual,

$$\phi(y + \tau, w - \tau, \mu) - \phi(y, w, \mu) \tag{6}$$

Another alternative would be to use data collected after the implementation of the policy and estimate the counterfactual,

$$\phi(y, w, \mu) - \phi(y - \tau, w + \tau, \mu) \tag{7}$$

2.1.3 An UCT Subsidy

Now consider an economy with the same structure but with an alternative policy that provides a cash transfer per child to all families, i.e. an unconditional cash transfer. Let the amount of unconditional cash transfer be υ. Under this policy, the household utility maximization problem will be given by:

$$\begin{aligned} &\max_s U(c, s, \mu) \\ \text{s.t.: } c = y + w(1 - s) + \upsilon &= (y + \upsilon) + w(1 - s) \end{aligned} \tag{8}$$

In this case, the household receive some cash independent of their choice of sending or not child to school. Therefore, the optimal choice of the household is a function of the same three parameters and the UCT subsidy.

$$s^{***} = \phi(\check{y}, w, \mu) = \phi(y + \upsilon, w, \mu) \tag{9}$$

Again, it is worth to notice that the schooling choice for a family with income, y, child wage, w, and unobserved heterogeneity, μ under the UCT subsidy would be the same as the schooling choice for a family with income $\check{y}$, child wage w, and unobserved heterogeneity, μ, under the no policy regime. This give us two possibilities for predicting the potential affects of the UCT subsidy. One alternative would be to use data collected before the implementation of the policy and estimate the counterfactual,

$$\phi(y+\upsilon,w,\mu)-\phi(y,w,\mu) \tag{10}$$

Another alternative would be to use data collected after the implementation of CCT policy and estimate what would have happened if the policy would not require school attendance, i.e. counterfactual,

$$\phi(y,w,\mu)-\phi(y-\upsilon,w,\mu) \tag{11}$$

2.2 Case 2: Multiple child with endogenous wage offers

Although the model presented before can (and was) used for predicting the effect of school subsidies, it can be improved by extending it to the case of multiple child with endogenous wage offers. A more general version of the model will allow household to have n children and would take into account selectivity in the observed wages. under this scenario the utility maximizing problem of the household would be given by,

$$\max_{(s^1,s^2)} \quad U(c,s^1,...s^n\mu) \tag{12}$$

$$\text{s.t.:} \quad c=(y+n\tau)+(w-\tau)\textstyle\sum_{i=1}^{n}(1-s^i)$$

$$ln(w)=\mu_w+\epsilon \tag{13}$$

Assuming that ϵ is normally distributed with mean 0 and variance σ^2 the selectivity in observed wages can be taken into account.

$$ln(w) \quad =\mu_w+E[\epsilon|s=0]+\epsilon-E[\epsilon|s=0] \tag{14}$$

$$=\mu_w+E[\epsilon|U(y+\mu_w+epsilon,0)>U(y,1)]+u \tag{15}$$

3 Estimation and Inference

3.1 Average Intent-to-treat Effect

Assume that **conditional** on a vector of family characteristics, denoted by x, unobserved heterogeneity distribution is independent of both, household income and wages it is straightforward to use non-parametric techniques to estimate the counterfactuals of interest. More formally, assuming that,

$$f(\mu|y,w,x)=f(\mu|\tilde{y},\tilde{w},x) \tag{16}$$

then, the matching estimator of the average treatment effect for those offered the program (the so called “intent-to-treat” (ITT) estimator will be given by:

$$\frac{1}{n}\sum_{j=1:j,i\in S_p}^{n}\{E(s_i|w_i=w_j-\tau, y_i=y_j+\tau)-s_j(w_j,y_j)\} \tag{17}$$

where $s_j(w_j, y_j)$ denotes the school attendance decision for a child of family j with characteristics (w_j, y_j). Notice that the average can only be taken over the region of overlapping support S_p, which in this case is over the set of families j for which the values $w_j - \tau$ and $y_j + \tau$ lie within the observed support of w_i and y_i.

We estimate the matched outcomes $E(s_i|w_i = w_j - \tau, y_i = y_j + \tau)$ non-parametrically using a tow dimensional kernel regression estimator. Letting $w_0 = w_j - \tau_j$ and $y_0 = y_j - \tau_j$, the estimator is given by:

$$E(s_i|w_i=w_j-\tau, y_i=y_j+\tau)=\frac{\sum_{j=1:j,i\in S_p}^{n} s_i K(\frac{w_i-w_0}{h_n^w})K(\frac{y_i-y_0}{h_n^y})1(x_1=x_0)}{\sum_{j=1:j,i\in S_p}^{n} K(\frac{w_i-w_0}{h_n^w})K(\frac{y_i-y_0}{h_n^y})1(x_1=x_0)} \tag{18}$$

where $K(.)$ denotes the kernel biweight function and h_n^w and h_n^y are the smoothing (or bandwidth) parameters[1]

3.2 Take-up Rates

Not all families will choose to participate of the subsidy program. The coverage rate is the probability that a family takes up the subsidy program. In this particular case, the probability that a family sends their child to school when the subsidy program is in place,

$$Pr(s(w-\tau, y+\tau)=1)=E(s(w-\tau, y+\tau)) \tag{19}$$

I estimate this probability using a non-parametric regression of the indicator variable s on w and y - only for families whose w and y values fall within the region of overlapping support, evaluated at the points $w - \tau$, $y + \tau$

3.3 Average Impact Effect on the Treated

Using the ITT estimate and the TR estimate, it is easy to obtain an estimate of the average treatment effect on the treated (ATT). Notice that the relationship between the ITT and the ATE for a family with characteristics w, y is given by:

$$\begin{aligned} ITT(w,y) = {} & Pr(\text{participates in program}|w,y)ATT(w,y) \\ & + Pr(\text{does not participate in program}|w,y)0 \end{aligned} \tag{20}$$

[1] $K(s) = \frac{15}{16}(s^2-1)^2 \text{if} |s| \leq 1$

Thus, to obtain an overall average estimate of ATT, we simple integrate over the distribution of w and y values that fall within the support region.

$$ATT(w, y) = \frac{ITT(w, y)}{E(s(w - \tau, y + \tau))} \tag{21}$$

Empirically, this is simply done by simply averaging over the ATE estimates for each individual families within the support region:

$$\frac{1}{n} \sum_{j=1:j,i \in S_p}^{n} \frac{E(s_i | w_i = w_j - \tau, y_i = y_j + \tau) - s_j(w_j, y_j)}{E(s_i | w_i = w_j - \tau, y_i = y_j + \tau)} \tag{22}$$

4 Predicting the Effects of BJP

4.1 Data

To assess the impact of BJP we use *Fundacion ARU*'s set of harmonized household surveys[2]. It is important to notice that the set of harmonized household surveys has used a uniform definition of variables and indicators - to the extent that it is possible, has restrained from using any kind of imputation or adjusting method and, most importantly, has corrected differences in sample design between different surveys constructed new sample weights using post-stratification methods (For further reference see Hernani-Limarino (2009) and *Fundacion ARU*, 2010).

A key future of our method is the use of data *before* and *after* the implementation of the policy. Therefore, we have conducted the estimation using the surveys one-year before and one-year after the implementation of BJP, i.e. we have used years 2005 and 2007 to evaluate the impact of BJP on *basic* primary education (grades 1 to 5) and years 2006 and 2008 to evaluate the impact of BJP on the first grade of *intermediate* primary education (grade 6); and years 2007 and 2009 to evaluate the impact of BJP on the second and third grades of *intermediate* primary education (grade 7 to 8).

From the household surveys, we use information on the age and gender of the child, the child's highest grade completed, whether the child is currently enrolled in school and the income of the parents. Total family income includes not only labor earnings but also other sources of non-income. Labor earnings include net salaries (after tax and social security discounts) plus in-kind labor income for employees and net-income (gross income minus production costs) for self-employed workers. Non-labor income includes includes social security payments, property income as well as transfers received from other families or the government.

[2] The harmonize set of surveys include the Living Standard Measurement Surveys (LSMS) from 1999 to 2002, the Income and Expenditure survey for years 2003 and 2004, the LSMS from 2005 to 2007, and the Social Stratification and Mobility Surveys (EMES) of years 2008 and 2009.

4.2 Results

Table 1 presents the ex-ante predictions of the effect of BJP disaggregated by age (rows) and sex (panels). Column (1) present the sample sizes, column (2) presents the % of overlapping support, column (3) present the predicted effect and its standard error based on 500 bootstrap replications. Our results sugget that BJP has been successful increasing school attendance only for young children and particularly for girls. In the case of boys, the effect on school attendance is 6.2 percentage points (and significant at 1% level) for 6 years old boys, 4.0 percentage points (and significant at 5% level) for 7 years old boys and 1.9 percentage points (and significant at 10% level) for 8 years old boys. All remaining ages show no effect on the school attendance of boys. In the case of girls, the effect on school attendance is 8.2 percentage points (and significant at 1% level) for 6 years old girls, 6.1 percentage points (and significant at 5% level) for 7 years old girls and 3.3 percentage points (and significant at 5% level) for 8 years old girls. Again, all remaining ages show no effect on the school attendance of girls.

Table 2 present the ex-post predictions of the effect of BJP disaggregated by age and sex. As was the case of ex-ante predictions, our results suggest that BJP has been successful increasing school attendance only for younger children and particularly for girls. In the case of boys, the ex-post effect on school attendance is 5.0 percentage points (and significant at 1% level) for 6 years old boys, 2.4 percentage points (and significant at 5% level) for 7 years old boys and 1.8 percentage points (and significant at 10% level) for 8 years old boys. In the case of girls, the effect on school attendance is 6.0 percentage points (and significant at 1% level) for 6 years old girls, 4.0 percentage points (and significant at 5% level) for 7 years old girls and 2.0 percentage points (and significant at 5% level) for 8 years old girls. All remaining ages show no effect on the school attendance of both, boys and girls. The similar patters of ex-ante and ex-post predictions can be interpreted as a sign of the robustness of our method.

5 Conclusions

This paper attempts to assess the impact of Bolivia's school subsidy program, *Bono Juancito Pinto* (BJP), on school attendance and child work. BJP is a relatively small cash transfer of less than 30 dollars per year that is given *to children* conditional on being enrolled into a public school and their school attendance. Since there are no possibilities for a control group we use simple structural behavioral models to understand the school-work decision and derive counterfactuals of interest. Estimation is conducted using two dimensional kernel regression estimators. Both ex-ante and ex-post predictions suggest that BJP has been successful increasing school attendance only for 6 to 8 years old children, and particularly for girls. All remaining ages show no effect on the school attendance of both, boys and girls. The similar patters of ex-ante and ex-post predictions can be interpreted as a sign of the robustness of our method.

We conclude that BJP has only encourage households to enroll children to school at the proper age but has not give an additional incentive to attend to those already enrolled for the first time.

Table 1: Ex-Ante Predictions of the Impact of BJP

	Panel A. Boys			Panel B. Girls			Panel C. Boys and Girls		
Ages	Sample Size	% Support	Effect	Sample Size	%Support	Effect	Sample Size	% Support	Effect
6	175	85	0.062 ***	211	87	0.082 ***	386	86	0.073 ***
7	233	89	0.040 **	213	84	0.061 **	446	87	0.050 **
8	216	92	0.019 *	200	89	0.033 **	416	91	0.026 **
9	209	95	0.001	207	95	0.001	416	95	0.001
10	202	93	0.002	218	94	0.001	420	94	0.002
11	210	91	0.003	181	93	0.005	391	92	0.004
12	218	94	-0.005	216	92	-0.005	434	93	-0.005
13	207	89	0.025	188	91	0.001	395	90	0.014
14	199	87	-0.003	207	89	0.024	406	88	0.011
15	208	83	0.003	220	82	-0.009	428	83	-0.003

Table 2: Ex-Post Predictions of the Impact of BJP

	Panel A. Boys			Panel B. Girls			Panel C. Boys and Girls		
Ages	Sample Size	% Support	Effect	Sample Size	%Support	Effect	Sample Size	% Support	Effect
6	168	83	0.050 ***	167	86	0.060 ***	335	85	0.055 ***
7	172	86	0.024 **	159	88	0.040 **	331	87	0.032 **
8	191	82	0.018 *	210	89	0.020 **	401	86	0.019 **
9	170	92	0.001	168	84	-0.001	338	88	0.000
10	180	94	0.000	190	90	0.002	370	92	0.001
11	154	87	0.005	205	94	0.003	359	91	0.004
12	195	94	-0.004	171	92	0.005	366	93	0.000
13	175	91	0.003	187	86	0.025	362	89	0.014
14	191	90	0.005	185	95	0.003	376	93	0.004
15	184	86	0.007	160	89	0.003	344	88	0.005