Schooling and Labor Market Impacts of Bolivia's *Bono Juancito Pinto* Program

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OVER THE PAST 15 YEARS, cash transfer programs have become a core component of antipoverty policy strategies in the developing world. In Latin America in particular, cash transfer programs have adopted a multidimensional approach to poverty, whereby income support is provided together with simultaneous interventions in health, education, and nutrition. This "human development" approach to poverty reduction places a strong emphasis on tackling the intergenerational transmission of poverty through human capital investment (Levy and Schady 2013; Niño-Zarazúa 2011; Levy 2006). Mexico's *Progresa-Oportunidades-Prospera*, Brazil's *Bolsa Familia*, Colombia's *Familias en Acción*, and *Chile Solidario* are prominent examples of this antipoverty policy framework.

The incentive mechanisms that cash transfers generate for schooling decisions are instrumental in enhancing human capital formation and tackling the structural roots of poverty (Parker, Rubalcava, and Teruel 2007). Monetary incentives are particularly important, as they link income support with mandatory regular school attendance. This is done through explicit conditions that are monitored and enforced with varying degrees of effort and efficacy across countries.[1] Since cash transfers target the poor, monetary incentives can have both an *income effect* (contingent on the size of transfers, relative to household income) and a *substitution effect,* which materializes through a reduction in the shadow prices of education and which in turn can affect decisions on both schooling and child labor (Behrman, Parker, and Todd 2009; Bourguignon, Ferreira, and Leite 2003).

The empirical literature on the impacts of cash transfer programs on schooling and child labor has shown that overall, cash transfers can successfully raise school enrollment and attendance (Attanasio et al. 2010; Dammert 2009; Schady and Araujo 2006; Skoufias et al. 2001); under certain conditions, they can also delay or reduce child labor, both the likelihood of children working and the intensity of work for those who do work (Behrman et al. 2012; de Janvry et al. 2006; Ferro, Kassouf, and Levison 2010; Schultz 2004; Skoufias et al., 2001).

In this article, we investigate the schooling and labor market impacts of Bolivia's *Bono Juancito Pinto* (BJP), a cash transfer program launched by the Bolivian government with the explicit objective of improving enrollment, retention, and completion rates of pupils in public schools. Unlike other cash transfer programs in Latin America, BJP does not follow a strict poverty-targeting mechanism; instead, it is nearly universal in its coverage, as it covers 90 percent of school-age children who are enrolled in public schools.

The program began in 2006 by providing income support of 200 Bolivianos per year (about US$25) to children enrolled in grades 1–5 of primary school. In subsequent years, the government gradually expanded its coverage to include children in secondary education, raising the number of beneficiaries from nearly 1.1 million school-aged children in 2006 to 2.1 million in 2014.

Using data from the Bolivian National Living Standards Survey, we exploited the exogenous variation in the timing of the announcement of the program expansion, as well as the changing age eligibility criteria, for identification. More specifically, we resort to difference-in-differences (DD) estimators to measure the effect of the program on schooling and the incidence and intensity of child labor.[2]

This article contributes to the literature on cash transfer programs in three ways. This is the first study that estimates the impact of the BJP program among children in secondary school, the level at which important occupational transitions take place in the country. Additionally, while most studies have focused on the incidence of child labor, we also provide evidence of the impact of the BJP program on the intensity of child labor. Finally, our identification strategy—relying on eligibility—solves the problem of selection bias seen in previous studies.

Schooling and labor market impacts of cash transfer programs

In situations of poverty, where children's and adults' labor income are substitutable, child labor arises not because of parental exploitation, but because of the need for additional sources of income (Basu and Van 1998). Legal frameworks prohibiting child labor would only be effective if policy interventions were in place to reduce households' liquidity constraints and compensate them for the income loss from schooling. It is important to distinguish here between children's participation in the labor market and the intensity of their engagement. Patrinos and Psacharopoulos (1997) have pointed out that the allegedly mutually exclusive relationship between child labor and schooling is not linear, particularly when labor is part-time and acts not as a substitute for children's time in school, but rather as a complementary strategy that may in fact allow children to continue their education.

In the particular context of cash transfer programs, the literature has largely focused on short-term effects on schooling (Akresh, de Walque, and Kazianga 2013; Behrman, Parker, and Todd 2009; Dammert 2009; Filmer and Schady 2008; Lincove and Parker 2016; Maluccio and Flores 2005; Skoufias et al. 2001) and on child labor (Behrman et al. 2012; Edmonds and Schady 2012; Ferro, Kassouf, and Levison et al. 2010; Skoufias et al. 2001).[3] Cash transfer programs are conventionally not designed with the explicit objective of reducing child labor. They have, however, proven to be effective—under certain conditions—at lowering children's participation in the labor market (Behrman et al. 2012; de Janvry et al., 2006; Schultz 2004; Skoufias et al. 2001).

This is important, because early entry into the labor market can lead to school dropout, which has long-term implications for children's future income and well-being in adulthood (Canelas 2015). In several contexts, child labor can also be associated with hazardous employment, with its detrimental and long-term negative consequences (Anker 2000; Edmonds and Pavcnik 2005; Ide and Parker 2005). Thus, reducing child labor can be generally regarded as a positive contribution of cash transfers toward sustained efforts to reduce poverty and vulnerability.

In Colombia, for example, *Familias en Acción* led to a significant reduction in domestic work in rural areas, particularly among children ages 8–13 (Attanasio et al. 2010). Similar effects were found in Nicaragua's *Red de Protección Social* for children in the same age-group (Barrientos and Santibañez 2009) and among beneficiary children of Ecuador's *Bono de Desarrollo Humano* (Schady and Araujo 2006).

Similarly, a study of Brazil's *Programa de Erradicação do Trabalho Infantil*, or PETI (Child Labor Eradication Program) found that the program increased children's time in school, improved academic success, and reduced labor participation and hazardous work (Yap, Sedlacek, and Orazem 2009). In Mexico, Rawlings and Rubio (2005) found small but significant reductions in child labor among beneficiaries of *Progresa-Oportunidades*, although no significant reduction was found for boys aged 16–17, a finding linked to the increasing opportunity cost of schooling. In Costa Rica, *Superémonos* increased school attendance and educational attainment among poor children, but there was no evidence of a reduction in child labor (Duryea and Morrison 2004).

A review by de Hoop and Rosati (2014) identified 30 studies worldwide, among which 23 were of cash transfer programs implemented in Latin America. None of these studies addressed Bolivia's BJP program. Most studies cited in the review focused largely on the incidence of child labor; however, little attention was paid to the intensity of child labor, with the notable exceptions of the work of Skoufias et al. (2001), Ferreira, Filmer, and Schady (2009), Attanasio et al. (2010), Gee (2010), and Del Carpio and Loayza (2012).

In the specific context of Bolivia, scholarly work on the impact of BJP on schooling and child labor is scant. The few studies available, while providing useful information, remain limited in their focus and methods. For instance, using household survey data for the period 1999–2007, Grigoli and Sbrana (2013) found that being a recipient of BJP in 2006 increased school enrollment in 2007 but had no effect on school attendance or child labor. The study relied on whether children enrolled in school in 2007 reported receiving the transfer in 2006. This created a selection bias, since children who were reported in the 2007 survey as having received BJP in 2006 had already met the enrollment and attendance conditions for 2006 and thus may have been predisposed to meet them again in 2007, with or without the stipend.

Using static microsimulation techniques with data for 2005, Yáñez (2012) found that BJP had a small effect on school enrollment and attendance, which in turn led to a lower incidence of child labor and poverty. Hernani-Limarino (2015) examined the effect of the program covering the period 2005–2009 and found a positive effect on school enrollment for children aged 6–8. More recently, Vera-Cossio (2017) looked at the effect of BJP on adult female labor supply from households with eligible children and found that BJP increased adult female working hours by 8 percent, a change largely explained by credit constraints and fixed costs of labor.

Background of BJP

The BJP program was introduced in 2006, initially with the objective of promoting school enrollment, retention in school, and completion of the first five years of primary education in public educational institutions across the country. However, since 2007, program eligibility gradually expanded, and by 2014 BJP covered all levels of primary and secondary education. Children 6–19 years of age attending public schools are eligible to receive support from the program. The transfer consists of a yearly payment of 200 Bolivianos (approximately US$25), conditional on proven school attendance during the school year. The transfer is paid in cash at the end of each school year directly to the children. It is distributed at ceremonies for that purpose, guarded with the help of the armed forces. According to official estimates, between 2006 and 2014, the number of beneficiaries increased from nearly 1.1 million to 2.1 million school-age children enrolled in public schools. The program currently costs about 0.3 percent of Bolivia's gross domestic product.

Table 1 shows the coverage and roll-out process of BJP. Relevant for our analysis is the *timing* of the public announcement of the program. The Bolivian government announced the creation of BJP in December 2006 to

TABLE 1 Coverage of *Bono Juancito Pinto* (BJP)

Year	Eligible children, beginning of school year	Educational levels covered, end of school year	Announcement date	Payment (Bolivianos)
2006	Na	Grades 1–5	October 2006	200
2007	Grades 0–4	Grades 1–6	October 2007	200
2008	Grades 0–5	Grades 1–8	July 2008	200
2009	Grades 0–7	Grades 1–8	October 2009	200
2010	Grades 0–7	Grades 1–8	October 2010	200
2011	Grades 0–7	Grades 1–8	October 2011	200
2012	Grades 0–7	Grades 1–9	October 2012	200
2013	Grades 0–8	Grades 1–10	October 2013	200
2014	Grades 0–9	Grades 1–12	October 2014	200
2015	Grades 0–11	Grades 1–12	–	200

SOURCE: Based on Decreto Presidencial No. 309 (2009) and Decretos Supremos No. 28899 (2006), 29321 (2007), 29652 (2008), 648 (2010), 1016 (2011), 1372 (2012), 1748 (2013), 2141 (2014).

initially cover, as discussed earlier, children who were enrolled in grades 1–5 of primary school and who had complied with the program conditions. Thus, at the beginning of the 2007 school year, eligible children were those who had at most four years of schooling and had the choice of enrolling (or not) in grades 1–5 of primary school.

A year later, in October 2007, the government announced the expansion of the program to include children enrolled in grade 6 of primary school. This meant that eligible children were those with at most five years of schooling by the time of the announcement. In July 2008, the government announced a further expansion of the program to include children enrolled up to grade 8 (i.e., the second year of secondary education). BJP remained unchanged until October 2012, when the government announced a further expansion to include children enrolled in grade 9 (i.e., the third year of secondary school). That meant that at the beginning of the 2013 school year, eligible children were those who had completed at most eight years of schooling, (i.e., up to the second year of secondary school). The progressive expansion of BJP continued until October 2014, when the program covered the entire range of primary and secondary education levels, including high school (see Table 1).

Data and empirical strategy

The data used in this study come from the Bolivian National Living Standards Survey (Encuesta Nacional de Condiciones de Vida) for the period 2005–2013, which was conducted by Bolivia's National Statistics Institute (Instituto Nacional de Estadística Bolivia [INE]). This is a nationally representative household survey of the Bolivian population. The survey collects detailed information on household demographic characteristics, health,

TABLE 2 Mean measures of children's labor participation, Bolivia, 2005–2006 and 2013

	2005–2006		2013	
Variable	Mean	SD	Mean	SD
Work participation	0.23	0.42	0.17	0.38
School enrollment	0.92	0.26	0.95	0.22
Hours of market work	5.65	13.65	4.47	12.46
No. of observations	8,974		7,425	

NOTE: SD = standard deviation.
SOURCE: Author calculations from Encuesta Nacional de Condiciones de Vida.

education, occupations and labor force participation, housing and asset ownership, household food and nonfood expenditures, and income, including contributions from social assistance. It also gathers information on whether the individual has participated in paid or unpaid market activities for a private and/or family business and the number of hours allocated to these activities. Unfortunately, it does not obtain information on domestic tasks and leisure time activities.

We focused our analysis on children aged 7–17 years, as this is the age cohort that is covered by primary and secondary education in Bolivia, and also the age range in which children are reported to have engaged in paid or unpaid work in the previous week in any of the following activities: (1) working in agricultural activities or caring for animals; (2) helping in the family business; (3) selling products; (4) making products to sell; and (5) providing services for payment (washing clothes, cutting hair, teaching, etc.).

We took a broader definition of schooling to measure children enrolled in school in the reported academic year. Formal education in Bolivia starts at the age of 6. Education is free of tuition fees and since 2009 is compulsory throughout all primary and secondary levels. The school year starts in February and lasts until the end of October or early November. Primary and secondary education consist of six years of education each. Each academic year lasts for about 40 weeks, five days per week and four hours per day.[4] Short school days and a lax legal framework that allows child labor from the age of 10 has meant that about 20 percent of children aged 7–14 engage in labor activities (Bureau of International Labor Affairs 2014).[5] In rural areas in particular, child labor—especially related to agriculture—is embedded in normative aspects and tradition, whereby it is considered as part of children's instruction and skill development (Fontana and Grugel 2015, 2017). The considerably high incidence of child labor is captured in Table 2, which shows basic statistics on school enrollment, work participation, and time allocated to income-generating activities during the week prior to the survey interview. While children's work participation has declined slightly between 2005–2006 and 2013, its incidence remains high and at a level twice that of the Latin American average (UNICEF 2017).

TABLE 3 Percent distribution of Bolivian children aged 6–17, by school status, according to grade level and year

		Panel A: 2006				Panel B: 2013			
Grade	Age	No school	In grade	Behind	Dropout	No school	In grade	Behind	Dropout
Primary (1–3)	6–8	39.3	60.4	0.0	0.2	5.0	94.3	0.00	0.7
Primary (4–6)	9–11	1.0	52.6	45.2	1.2	0.6	72.4	26.1	1.0
Secondary (1–3)	12–14	0.4	43.5	50.7	5.4	0.4	61	35.6	3.0
Secondary (4–6)	15–17	0.6	35.7	48.6	15.1	0.6	52.4	37.0	10.1
All	6–17	11.2	48.5	35.1	5.2	1.4	68.4	26.3	3.9

NOTE: Panel rows add to 100
SOURCE: Authors' calculations from Encuesta Nacional de Condiciones de Vida.

Table 3 shows the status of children in the school system between 2005–2006 and 2013. Retention rates were relatively high, although there was slow progress throughout the school grades. The proportion of children behind the corresponding grade for age was high, particularly at baseline in 2006. This can be explained to a certain extent by late school entry rates: 39 percent of children aged 6–8 were not enrolled in school in 2006, and 45 percent of children aged 9–11 were enrolled in a lower grade than the one corresponding to their age. In more recent years, the basic education system has achieved some progress. For example, by 2013, 68 percent of school-age children were in the school grade corresponding to their age, while 26 percent had fallen behind and only 4 percent had dropped out of school altogether.[6]

Identification strategy

BJP targets all children enrolled in public primary and secondary schools; while the transfer benefits all children independent of their socioeconomic status, coverage has expanded gradually over time. We exploited this variation in coverage to compare children who were eligible to receive the cash transfer (the treatment group) with those children who were just above the eligibility threshold at the same point in time and who therefore did not benefit fully from the program throughout the entire period covered in the analysis (the control group). A second source of variation comes from the *timing* of the announcement of the program expansion. We also exploited this exogenous variation to estimate the differences in outcomes between treatment and control groups before and after the program implementation, in a DD framework. The basic idea behind our identification strategy is illustrated in Table 4.

For the analysis, we focused on the last school grade covered by the program in the last available survey. We did so for several reasons. First, enrollment rates in primary school in Bolivia are relatively high. In fact, primary school is almost universal, so if the transfer is effective in increasing

TABLE 4 Identification strategy

Completed years of schooling	2005–2006	2013
0	B	T
1	B	T
2	B	T
3	B	T
4	B	T
5	B	T
6	B	T
7	B	T
8	B	T
9	B	C
10	B	C
11	B	C

SOURCE: Authors.
NOTE: T refers to the treatment group, C refers to the control group, and B refers to the baseline period. The shadowed cells capture the two groups that are analyzed in this study.

enrollment rates and school retention, this is more likely to be observed in secondary education, in which occupational transitions and school dropout rates are more common. Therefore, for our analysis, it was more relevant to test whether vulnerable groups who were more likely to drop out of school and work more intensively—due to an increasing opportunity cost of schooling—improved their schooling achievements relative to the pre-treatment period. Second, by using the last available survey and looking at the behavior of children and their schooling and work decisions in the last covered school year in the survey, we can take advantage of their cumulative exposure to the program, meaning that those children who were last covered by the program were also exposed to the cash transfer for a longer period of time.

By 2013, children who had completed at most eight years of schooling at the beginning of the 2013 school year were eligible to receive BJP. In this case, our treatment group consisted of children who had completed eight years of schooling, whereas the control group was made of children who had completed nine years of schooling and were not eligible to receive BJP at the end of that academic year. We note, however, that with the 2013 sample, the control group might have been exposed to the program in earlier years and therefore cannot be used to measure the average treatment effect of the program, but just the effect of the program in grade 9, the period in which a very important school–labor market transition occurs in Bolivia.

To get a better sense of the broader impact of the program, we also used data from the 2011 survey, where children with nine years of schooling had not been exposed to the program, and estimated the DD matching estimators, following the framework described below. The results, presented in Appendix Table A6, were very similar in terms of size and direction.[7] A potential source of concern for this strategy comes from the fact that the treatment and control groups in the postprogram period were from samples with a one-year gap. However, in the absence of any major shock that could have affected schooling decisions between these two groups, we are confident that children with nine years of schooling in 2011 are a good control group for the postprogram period. Nevertheless, while our matching estimator should minimize any remaining source of observed heterogeneity, we acknowledge this data limitation and caution concerning the policy interpretations of our findings.

Estimation strategy

We estimated the effect of the program on school enrollment and work participation using a DD approach. The DD equation took the following form:

$$Y_{igt} = \beta_0 + \beta_1 T_{ig} + \Upsilon T_{ig} * P_{it} + \sum_{j=1}^{J} X_{ij}\theta_j + \delta_t + \varepsilon_{igt}, \quad (1)$$

where Y was the outcome of interest (i.e., work participation or schooling), T was a dummy variable equal to 1 for eligible children (those with eight years of schooling) and 0 otherwise (those with nine years of schooling), P was a dummy variable equal to 1 for the years when the transfer was paid, and was the parameter of interest yielding the program treatment effect. X_i was a vector of socio demographic characteristics, including the child's age, sex, and ethnicity, the household head's age and education level, the household size, the number of household members working, and housing conditions, including piped water, a toilet connected to the sewerage system, and access to electricity. We also included in X_i controls for rural households and geographic dummies for the nine departments in Bolivia, whereas δ_t controlled for potential time-varying effects of each round of data. The specification included robust standard errors clustered at the household level.

To capture changes in the intensity of child labor, we also estimated the effect of BJP on the number of hours children spent on market work, using the following specification:

$$H_{igt} = \beta_0 + \beta_1 T_{ig} + \Upsilon T_{ig} * P_{it} + \sum_{j=1}^{J} X_{ij}\theta_j + \delta_t + \varepsilon_{igt}, \quad (2)$$

where H accounted for the number of hours per week allocated to income-generating activities (i.e., market work). We also provided robust standard errors clustered at the household level. We used data for children who had completed the second and third years of secondary school (i.e., those aged

13–16) and then estimated separate models for children living in rural areas, children living in urban areas, boys, and girls.

The DD estimates would provide unbiased treatment effects of the program in ninth grade under the assumption of "parallel trends"—that is, in the absence of the treatment, the outcomes of the two groups would have followed similar trends. As noted by Attanasio et al. (2010), while this assumption cannot be tested formally, it is useful for comparing trends in outcomes between the treatment and control groups before the program started. If they are similar, it is likely they would have been the same in the posttreatment period in the absence of the program. We tested this formally using data from the pretreatment period (2005–2006). The results, presented in Appendix Table A9, indicate that time trends were similar for treatment and comparison groups.

Another possible source of bias arises from the presence of an unbalanced distribution of observed characteristics between the treatment ($Z_i = 1$) and control ($Z_i = 0$) groups, which would then affect the outcomes of interest Y_{it}. To address this threat of bias, we followed Blundell and Dias (2009) and first matched treatment and control observations using a kernel propensity score matching, imposed a common support, and then calculated a DD matching (DDM) estimator, as follows:

$$\begin{aligned} DDM = &\left\{E\left(Y_{it=1}|D_{it=1} = 1, Z_i = 1\right) - w^c_{it=1} * E\left(Y_{it=1}|D_{it=1} = 0, Z_i = 0\right)\right\} \\ &-w^t_{it=0} * \{E\left(Y_{it=0}|D_{it=0} = 0, Z_i = 1\right) \\ &- w^c_{it=0} * E\left(Y_{it=0}|D_{it=0} = 0, Z_i = 0\right)\right\}, \end{aligned} \tag{3}$$

where D_{it} was the treatment indicator equal to 1 for the treatment group in the follow-up period and 0 otherwise, and $w^c_{it=0}$, $w^c_{it=1}$, and $w^t_{it=0}$ were the kernel weights for the control and treatment groups in the baseline ($t = 0$) and follow-up ($t = 1$) periods, respectively. The common support was composed of members of the treatment group for whom a counterfactual was found in each of the control samples.[8]

Appendix Tables A1–A5 show the characteristics of matched and unmatched samples at baseline and the different tests concerning the balancing property of the different groups. In general, the matching substantially improved the quality of the comparison, as shown by both the reduction in the mean absolute standardized bias and in the pseudo R^2 of the probit model for the selection of treated children. For reference, we also present the p values of the mean differences for each of the observed characteristics we controlled for. We note, however, that t-tests and other statistical tests of hypothesis are influenced by the sample size, and therefore we expected few significant differences between the treated and controls to remain after the matching for the subsamples under analysis.

TABLE 5 Impact of the BJP program on school enrollment

	National sample	Rural	Urban	Boys	Girls
Effect	0.052**	0.108*	–0.006	0.029	0.082**
	(0.019)	(0.046)	(0.022)	(0.026)	(0.029)
Observations	2,472	727	1,734	1,235	1,210

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
NOTE: Coefficients were estimated using kernel propensity score matching using a DD approach. In all specifications, we used control variables and time- and department-fixed effects. Robust standard errors clustered at the household level are in parentheses.

TABLE 6 Impact of the BJP program on work participation

	National sample	Rural	Urban	Boys	Girls
Effect	−0.062	−0.097	−0.002	−0.039	−0.078
	(0.047)	(0.099)	(0.043)	(0.066)	(0.065)
Observations	2,472	727	1,734	1,235	1,210

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
NOTE: Coefficients were estimated using kernel propensity score matching using a DD approach. In all specifications, we used control variables and time- and department-fixed effects. Robust standard errors clustered at the household level are in parentheses.

TABLE 7 Impact of the BJP program on hours worked

	National sample	Rural	Urban	Boys	Girls
Effect	−1.275	−3.692	0.584	−2.130	−0.870
	(1.108)	(2.348)	(1.250)	(1.722)	(1.422)
Observations	2,389	703	1,671	1,183	1,179

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.
NOTE: Coefficients were estimated using kernel propensity score matching using a DD approach. In all specifications, we used control variables and time- and department-fixed effects. Robust standard errors clustered at household level are in parenthesis.

Finally, given the nature of the outcome variables (two dichotomous and one censored at 0), we should ideally have performed the estimation using nonlinear models (i.e., probit and tobit); however, as pointed out by Greene (2010), while the marginal effects of the interaction terms can be computed, testing their statistical significance is not possible. We therefore carried out the estimations using ordinary least squares (OLS). A few remaining concerns about identification are addressed in Appendix B.

Results

We report here the results first for the full sample and then for different subpopulations. Tables 5 and 6 show the effect of BJP on the probability of school enrollment and labor force participation among children in the ninth grade, while Appendix Table A6 presents the results for the 2011 sample, which captures the average treatment effects of the program. Table 7 presents results on the intensity of child labor.

Here, the idea is that while the transfer size is too small to alter labor force participation of children, it may still affect the number of hours children spend working during the week.

The first columns of Tables 5 and 6 report the DDM estimates on the full sample. Overall, we found an increase in the likelihood of school enrollment of 5 percent in ninth grade. The effect was nearly identical when the full average treatment effect of the program was measured using the 2011 sample (see Appendix Table A6). This is quite significant, given the important occupational transitions that usually occur at that age in Bolivia. Unsurprisingly, the program had no sizable impact on child labor, either at the extensive or intensive margin. In general, our results are consistent with previous work on cash transfer programs in Latin America, including those of Schultz (2004) in Mexico, Macours and Vakis (2009) in Nicaragua, and Attanasio et al. (2010) in Colombia.

The urban-rural dichotomy

Rural-urban differences in living standards are marked in Bolivia. In 2006, 76.5 percent of the rural population were poor[9] (that is, eight in every 10 persons); in urban areas, this proportion amounted to 50.3 percent. Differences in *extreme* poverty levels were even more striking, with rates of 62.3 percent in rural areas and 23.4 percent in urban areas. The incidence of child labor is also high. The labor participation rate was 64.9 percent among rural children and 17.0 percent among urban children. In this context, it is expected that the transfer will have different impacts according to the geographic location of the household.

Columns 2 and 3 of Tables 5 and 6 and Appendix Table A6 present the results of the DDM estimates by area of residence. The transfer had a significant positive effect on school enrollment in rural areas, but not in urban areas. While the coefficients of work participation and work intensity both had the desired negative sign in both rural and urban areas, the estimates were not statistically significant. Bolivia's educational system allows children to work, since the school day lasts on average only four hours. As a result, an important percentage of children combine work and schooling. This fact, coupled with the small amount of the transfer, can explain, in our judgment, the nonsignificant effect of the program on child labor.

In 2008, a study on child labor in Bolivia carried out by Bolivia's National Statistical Institute and the International Labour Organization revealed that the monthly average salary of children aged 14–17 was 633 Bolivianos in urban areas and 657 Bolivianos in rural areas.[10] This means that in 2008 the BJP represented on average just about 2.5 percent of children's income in both urban and rural areas.

Gender differences

Bolivia does not have a significant gender gap with regard to school attendance. Regarding child labor, however, it is more common to find boys working in productive activities, paid or unpaid, while girls are mostly confined to household chores. Columns 4 and 5 of Tables 5 and 6 and Appendix Table A6 present the gender results by focusing on girls and boys separately. Similarly, to the previous estimations, we found statistically significant results only for school enrollment, although the likelihood of schooling increased only for girls.

In the absence of time-use data on domestic activities and leisure time, we were unable to account for the substitution effects between different activities. In the case of girls in particular, the traditional division of labor led us to infer that the increase in school enrollment reduced the time allocated to household chores. Unfortunately, we were unable to test whether this was the case.

The results for child labor remained virtually unchanged irrespective of sex. Once again, the monetary value of the transfer, which was too low to compensate for the opportunity cost of schooling, seemed to provide a sensible explanation for absence of impact. Bolivia's National Statistical Institute (INE, 2010) reported that boys aged 14–17 years earned on average 715 Bolivianos per month. Their salary was also 1.6 times higher than that of girls (457 in urban areas and 427 in rural areas). In this context, the BJP transfer accounted for only two percent of a boy's monthly earnings.

Spillover effects

Finally, in this section, we tested whether the positive effect of the program on schooling is robust by controlling for spillover effects at the household level. Appendix Table A7 presents the results of Equation (4). The coefficient of interest α captures the spillover effects of the transfer in 2013. If significant, spillover effects cannot be rejected. As shown in Table A7, the results were robust to spillover effects at the household level for all specifications.

Conclusion

Different from other cash transfer programs in Latin America, BJP is nearly universal, with coverage of about 90 percent of school-aged children who are enrolled in public schools in Bolivia. By adopting a DDM approach, we assessed the effect of the program on schooling and child labor decisions at an important juncture in the life of young people.

Overall, we found evidence that the program has been successful in increasing school enrollment rates, which is consistent with previous scholarly work; however, we observed no evidence of program effects on child labor. There are at least two potential explanations for this result. First, the monetary value of the transfer may have been too low to compensate for the increasing opportunity cost of schooling, particularly among children aged 13–16, the period in which important school–labor market transitions occur in Bolivia. Second, the structure of the educational system, together with high poverty rates, normative factors, and a lax legal framework that regulates child labor in the country, allowed children to combine schooling with income-generating activities.

One immediate implication of our findings is that parents are likely to substitute other uses of their children's time, such as leisure. So, in the presence of child labor, an increase in school participation may come at the expense of a reduction in children's leisure time, including playing and recreational activities, with important consequences for their cognitive, emotional, and physical development. This is an important area for future research.

Notes

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1 For a discussion and systematic literature review on the effect of conditions of cash transfers, see Baird et al. (2013).

2 The official school day in Bolivia lasts for only four hours, while market work lasts for five hours per day, on average. Both activities are compatible, and the interaction between the two can go in either direction, to support schooling or have a detrimental effect on it. In such contexts, the effect of cash transfers may be better captured by changes in labor intensity rather than changes in the incidence of child labor.

3 For reviews of the literature, see Baird et al. (2013); Barrientos and Niño-Zarazúa (2010); Bastagli et al. (2016).

4 Until 2010, the school system in Bolivia was organized as eight years of primary school and four years of secondary school. Since 2011, the system has been changed to six years each.

5 In July 2014, the Bolivian government passed a new Law—Ley 548, Código Niña, Niño y Adolescente—lowering the minimum working age from 14 to 10 (Gaceta Oficial del Estado Plurinacional de Bolivia 2014).

6 See Figure A2 in the Appendix.

7 We focus on grade eligibility rather than on program take-up. This means that our results measure the intent-to-treat program effects on the targeted population.

8 See Blundell and Dias (2009) for more details on the estimation and Villa (2016a) for a software implementation.

9 Official figures from Bolivia's National Institute of Statistics.

10 See INE (2010) for further details.

11 For a more technical discussion, see Miguel and Kremer (2005) and Villa (2016b). 10Also see Appendix Figures A1 and A2.

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Appendix A

TABLE A1 Characteristics across matched and unmatched samples, group 1

	Unmatched sample			Matched sample		
Variable	**Treatment**	**Control**	$p > t$	**Treatment**	**Control**	$p > t$
Age of child	14.63	15.41	0.00*	14.67	14.69	0.72
Male child	0.53	0.49	0.16	0.53	0.53	0.91
Indigenous child	0.40	0.38	0.49	0.40	0.42	0.44
No. of household members working	2.45	2.35	0.26	2.44	2.37	0.43
Head's no. of years of education	6.92	7.35	0.06	6.92	7.04	0.55
Age of head	44.50	45.94	0.02*	44.50	44.31	0.74
Female household head	0.20	0.23	0.26	0.20	0.20	0.95
Rural area	0.36	0.25	0.00*	0.35	0.38	0.40
Has piped water	0.28	0.33	0.06	0.28	0.28	0.91
Has toilet connected to sewerage	0.30	0.39	0.00	0.31	0.31	0.95
Has electricity	0.77	0.87	0.00*	0.78	0.77	0.44
Household size	5.89	5.84	0.67	5.90	5.78	0.31
Chuquisaca	0.07	0.07	0.72	0.06	0.05	0.57
Cochabamba	0.14	0.16	0.59	0.14	0.12	0.15
Oruro	0.10	0.11	0.33	0.10	0.11	0.54
Potosi	0.11	0.08	0.15	0.11	0.12	0.67
Tarija	0.09	0.09	0.63	0.09	0.09	1.00
Santa Cruz	0.18	0.16	0.28	0.18	0.19	0.68
Beni	0.08	0.10	0.49	0.09	0.09	1.00
Pando	0.03	0.03	0.97	0.03	0.04	0.56
Mean absolute bias		11.4			2.6	
Median absolute bias		6.4			2.5	
Pseudo R^2		0.11			0.004	

*$p < 0.05$.
NOTE: Pseudo R^2 of probit model for the selection of treated households. Group 1 refers to the sample at the national level.

TABLE A2 Characteristics across matched and unmatched samples, group 2

	Unmatched sample			Matched sample		
Variable	**Treatment**	**Control**	$p > t$	**Treatment**	**Control**	$p > t$
Age of child	14.61	15.42	0.00	14.70	14.77	0.42
Indigenous child	0.42	0.40	0.71	0.42	0.47	0.12
No. of household members working	2.53	2.21	0.01	2.50	2.46	0.71
Head's no. of years of education	6.88	7.14	0.42	6.88	7.01	0.65
Age of head	44.02	46.50	0.01	44.21	44.64	0.57
Female household head	0.18	0.24	0.08	0.19	0.18	0.77
Rural area	0.36	0.24	0.00	0.35	0.44	0.01*
Has piped water	0.29	0.35	0.09	0.29	0.22	0.03*
Has toilet connected to sewerage	0.30	0.38	0.03	0.30	0.25	0.10
Has electricity	0.77	0.90	0.00	0.78	0.73	0.10
Household size	6.05	5.82	0.17	6.05	5.84	0.16
Chuquisa	0.08	0.05	0.22	0.08	0.08	0.89
Cochabamba	0.14	0.16	0.47	0.14	0.16	0.47
Oruro	0.09	0.13	0.12	0.09	0.08	0.51
Potosi	0.11	0.09	0.38	0.11	0.11	0.91
Tarija	0.08	0.08	0.88	0.08	0.06	0.32
Santa Cruz	0.18	0.18	0.90	0.18	0.20	0.64
Beni	0.09	0.08	0.92	0.09	0.10	0.61
Pando	0.03	0.04	0.84	0.04	0.04	1.00
Mean absolute bias		14.40			6.70	
Median absolute bias		10.50			4.90	
Pseudo R^2		0.14			0.02	

*$p < 0.05$.
NOTE: Pseudo R^2 of probit model for the selection of treated households. Group 2 refers to the sample of boys.

TABLE A3 Characteristics across matched and unmatched samples, group 3

	Unmatched sample			Matched sample		
Variable	**Treatment**	**Control**	***p* > *t***	**Treatment**	**Control**	***p* > *t***
Age of child	14.64	15.40	0.00*	14.65	14.69	0.66
Indigenous child	0.38	0.36	0.60	0.38	0.35	0.37
No. of household members working	2.36	2.49	0.34	2.36	2.53	0.18
Head's no. of years of education	6.97	7.55	0.07	6.99	6.67	0.27
Age of head	45.03	45.40	0.69	45.00	44.71	0.74
Female household head	0.22	0.21	0.84	0.22	0.21	0.85
Rural area	0.37	0.26	0.01*	0.36	0.38	0.57
Has piped water	0.26	0.30	0.29	0.26	0.22	0.20
Has toilet connected to sewerage	0.31	0.41	0.01*	0.31	0.30	0.74
Has electricity	0.78	0.84	0.10	0.78	0.77	0.71
Household size	5.72	5.87	0.41	5.73	5.90	0.29
Chuquisa	0.05	0.09	0.08	0.05	0.05	1.00
Cochabamba	0.15	0.15	0.97	0.15	0.13	0.43
Oruro	0.10	0.10	0.85	0.10	0.07	0.09
Potosi	0.10	0.08	0.26	0.10	0.11	0.70
Tarija	0.11	0.09	0.57	0.11	0.13	0.34
Santa Cruz	0.18	0.14	0.16	0.18	0.25	0.04*
Beni	0.08	0.11	0.31	0.09	0.09	0.89
Pando	0.03	0.03	0.90	0.03	0.01	0.03*
Mean absolute bias		11.70			6.40	
Median absolute bias		8.50			6.00	
Pseudo R^2		0.11			0.02	

*$p < 0.05$.
NOTE: Pseudo R^2 of probit model for the selection of treated households. Group 3 refers to the girls sample.

TABLE A4 Characteristics across matched and unmatched samples, group 4

	Unmatched sample			Matched sample		
Variable	**Treatment**	**Control**	***p* > *t***	**Treatment**	**Control**	***p* > *t***
Age of child	14.85	15.53	0.00*	14.88	15.03	0.20
Male child	0.53	0.47	0.32	0.53	0.48	0.28
Indigenous child	0.53	0.60	0.17	0.53	0.48	0.21
No. of household members working	3.02	3.23	0.30	3.02	2.85	0.30
Head's no. of years of education	5.54	6.22	0.07	5.59	6.27	0.03*
Age of head	46.16	47.34	0.34	46.21	43.82	0.02*
Female household head	0.15	0.17	0.57	0.15	0.10	0.08
Has piped water	0.09	0.07	0.60	0.09	0.08	0.52
Has toilet connected to sewerage	0.05	0.06	0.72	0.05	0.08	0.27
Has electricity	0.47	0.59	0.03*	0.47	0.52	0.33
Household size	6.03	5.96	0.75	6.00	5.94	0.74
Chuquisa	0.09	0.07	0.51	0.10	0.08	0.53
Cochabamba	0.16	0.19	0.36	0.16	0.19	0.29
Oruro	0.09	0.13	0.14	0.09	0.07	0.51
Potosi	0.13	0.07	0.10	0.13	0.13	1.00
Tarija	0.08	0.09	0.82	0.08	0.03	0.01*
Santa Cruz	0.14	0.13	0.77	0.14	0.13	0.69
Beni	0.08	0.02	0.03*	0.07	0.14	0.01*
Pando	0.06	0.07	0.87	0.06	0.05	0.44
Mean absolute bias		13.6			10.5	
Median absolute bias		10.5			9.2	
Pseudo R^2		0.13			0.05	

*$p < 0.05$.
NOTE: Pseudo R^2 of probit model for the selection of treated households. Group 4 refers to the rural sample.

TABLE A5 Characteristics across matched and unmatched samples, group 5

	Unmatched sample			Matched sample		
Variable	**Treatment**	**Control**	***p* > *t***	**Treatment**	**Control**	***p* > *t***
Age of child	14.50	15.37	0.00	14.54	14.64	0.23
Male child	0.53	0.50	0.28	0.53	0.56	0.31
Indigenous child	0.33	0.31	0.53	0.33	0.36	0.32
No. of household members working	2.12	2.06	0.44	2.13	2.22	0.31
Head's no. of years of education	7.70	7.73	0.93	7.65	6.77	0.00*
Age of head	43.55	45.47	0.01	43.68	42.96	0.29
Female household head	0.23	0.24	0.59	0.23	0.26	0.27
Has piped water	0.38	0.41	0.41	0.38	0.38	0.89
Has toilet connected to sewerage	0.45	0.50	0.10	0.45	0.37	0.01*
Has electricity	0.95	0.96	0.26	0.95	0.93	0.31
Household size	5.81	5.80	0.94	5.81	5.87	0.64
Chuquisa	0.05	0.07	0.21	0.05	0.01	0.00*
Cochabamba	0.14	0.14	0.82	0.14	0.18	0.10
Oruro	0.10	0.11	0.84	0.11	0.10	0.58
Potosi	0.10	0.09	0.64	0.09	0.09	0.91
Tarija	0.10	0.09	0.43	0.10	0.08	0.25
Santa Cruz	0.21	0.17	0.15	0.21	0.30	0.00*
Beni	0.09	0.12	0.13	0.09	0.11	0.31
Pando	0.01	0.02	0.46	0.01	0.01	0.76
Mean absolute bias		9.30			8.30	
Median absolute bias		5.30			6.80	
Pseudo R^2		0.12			0.05	

*$p < 0.05$.
NOTE: Pseudo R^2 of probit model for the selection of treated households. Group 5 refers to the urban sample.

TABLE A6 Impact of the BJP program on school enrollment, 2011 sample

	National sample	**Rural**	**Urban**	**Boys**	**Girls**
Effect	0.045***	0.084**	−0.001	0.030	0.068***
	(0.017)	(0.039)	(0.023)	(0.024)	(0.024)
Observations	2,249	762	1,689	1,219	1,215

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
NOTE: Coefficients Were estimated using kernel propensity score matching using a difference-in-differences approach. In all specifications, we used control variables, time, and department fixed effects. Bootstrapped standard errors clustered at household level, 1,200 repetitions.

TABLE A7 Impact of the BJP program on school enrollment: spillover effects

	National sample	Rural	Urban	Boys	Girls
No. of eligible children in household,* 2013	−0.010	−0.004	−0.012	−0.020	−0.009
	(0.009)	(0.020)	(0.009)	(0.021)	(0.016)
No. of eligible children in household	0.006	0.008	0.016*	−0.004	0.020
	(0.006)	(0.014)	(0.008)	(0.012)	(0.012)
Observations	2,472	727	1,734	1,235	1,210

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
NOTE: Coefficients were estimated using kernel propensity score matching, using a DD approach. In all specifications, we used control variables and time- and department-fixed effects. Robust standard errors clustered at the household level are in parentheses.

TABLE A8 Impact of the BJP program on work participation: spillover effects

	National sample	Rural	Urban	Boys	Girls
No. of eligible children in household* 2013	0.015	0.006	0.034	−0.002	0.043
	(0.022)	(0.038)	(0.021)	(0.041)	(0.038)
No. of eligible children in household	0.036	0.018	−0.006	0.060*	0.020
	(0.014)	(0.027)	(0.014)	(0.028)	(0.024)
Observations	2,472	727	1,734	1,235	1,210

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
NOTE: Coefficients were estimated using kernel propensity score matching using a DD approach. In all specifications, we used control variables and time- and department-fixed effects. Robust standard errors clustered at the household level are in parentheses.

TABLE A9 Preprogram time trends in schooling, work, and hours worked

	School enrollment	Work participation	Hours worked
Treatment group * 2006	0.034	−0.044	0.639
	(0.033)	(0.066)	(1.584)
Observations	1,228	1,228	1,180

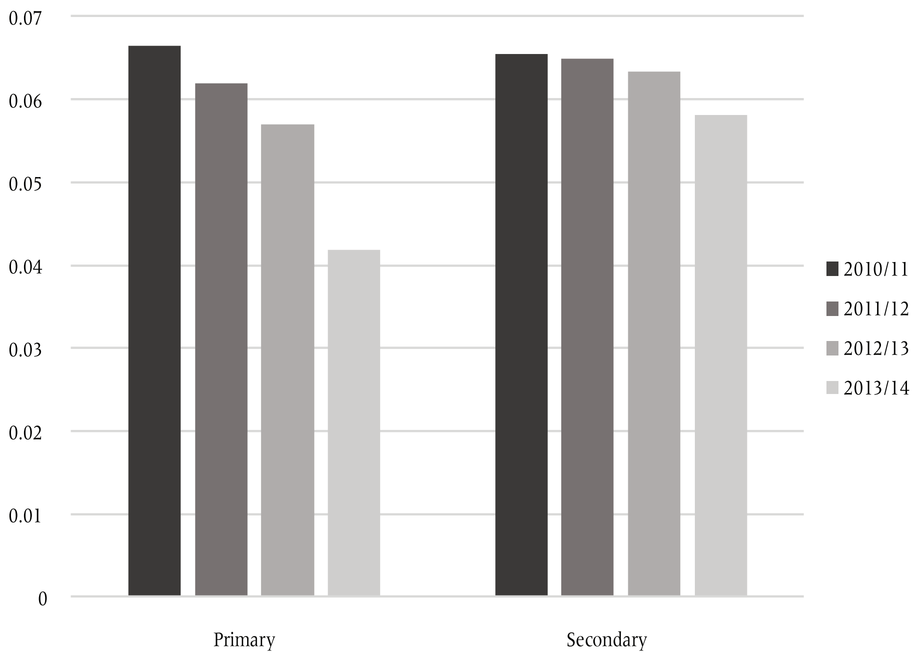
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
NOTE: Coefficients were estimated using kernel propensity score matching using a DD approach. In all specifications, we used control variables and time- and department-fixed effects. Bootstrapped standard errors were clustered at the household level, 1,200 repetitions.

TABLE A10 Impact of the BJP program on hours worked: spillover effects

	National sample	Rural	Urban	Boys	Girls
No. of eligible children in household* 2013	0.521	0.276	0.979	−0.737	1.550
	(0.513)	(1.026)	(0.683)	(0.039)	(0.905)
No. of eligible children in household	0.718*	0.471	0.001	1.747*	−0.035
	(0.338)	(0.671)	(0.484)	(0.724)	(0.587)
Observations	2,389	703	1,671	1,183	1,179

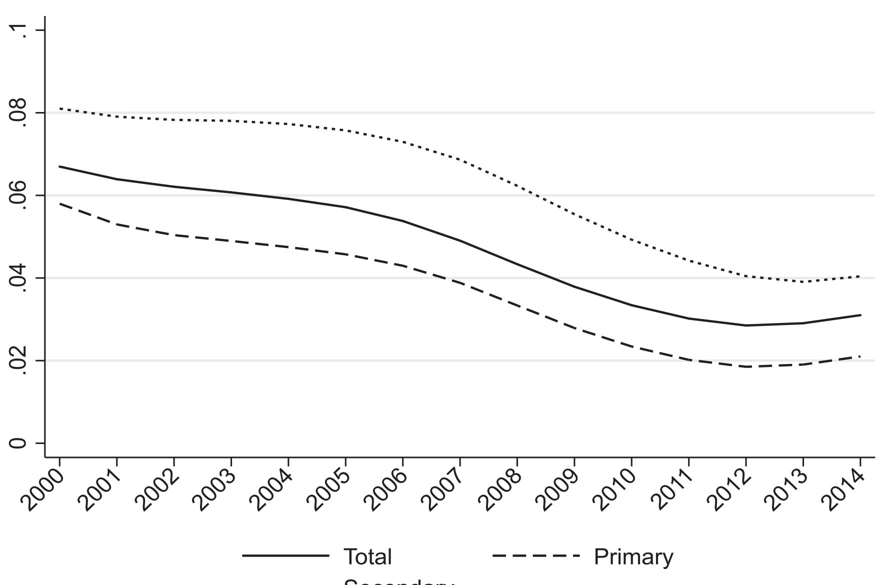
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$
NOTE: Coefficients were estimated using kernel propensity score matching using a DD approach. In all specifications, we used control variables and time- and department-fixed effects. Robust standard errors clustered at the household level are in parenthesis.

FIGURE A1 Grade repetition rates



SOURCE: Author calculations, based on the Ministry of Education data.

FIGURE A2 Dropout rates by education level



SOURCE: Authors based on the Ministry of Education figures.

Appendix B

Some remaining concerns about the identification strategy

There are a few remaining concerns about our identification strategy. The first relates to the fact that the transfer was directed only to children enrolled in public schools—90 percent of all school-age children in the country. If the transfer were to become an incentive for children in private schools to switch to public schools, our results would be biased. We argue that given the small amount of the transfer and the fact that children in private schools usually come from better-off families, this situation is highly unlikely.

A second concern comes from the number of eligible children within the households. While we controlled for this to a certain extent in the previous specification by clustering standard errors at the household level, we now explicitly controlled in Equation (4) for the number of eligible children in the household and its interaction with treatment years, as follows:[11]

$$Y_{igt} = \beta_0 + \beta_1 T_{ig} + \Upsilon T_{ig} * P_{it} + \rho N_i + \alpha_t N_i * P_{it} + \sum_{j=1}^{J} X_{ij}\theta_j + \delta_t + \varepsilon_{igt}. \quad (4)$$

The final concern relates to the mandatory aspect of education in Bolivia through all primary and secondary levels. This was first introduced in the Bolivian constitutional referendum of 2009 and was subsequently executed in 2010 with a new law—Ley de la Educación "Avelino Siñani-Elizardo Pérez." If such a compulsory schooling law were fully enforced, this could generate a confounding effect. However, while this law was an important policy reform, its enforcement has remained rather weak.

Previous studies have emphasized that efforts to keep children in school through legal instruments, without a good understanding of the economic and social constraints faced by the poor and vulnerable, can only have limited success (Jacoby and Skoufias 1997; Lleras-Muney 2002). Indeed, by 2018, almost 10 years after the introduction of the constitutional reform, there were still children out of school. Moreover, while school dropout rates have fallen steadily (see Appendix Figure A2), this decline began much earlier than 2009 (in fact, in around 2006), at the time BJP was introduced.

Nevertheless, universal education at both the primary and secondary levels remains a priority for the Bolivian government, and the BJP has become one instrument to achieve that goal (if not the main one). Previous analyses have attributed the decreasing dropout rates to the BJP (UNICEF 2011; Peredo-Videa 2013; UNESCO 2014; Ministerio de Educación 2015), including qualitative studies that interviewed school headmasters, teachers, and parents (Navarro 2012; Observatorio Social de Políticas Educativas de Bolivia 2011).