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Schooling and labour market impacts of Bolivia's Bono Juancito Pinto

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Abstract: In 2006, the Bolivian government introduced a large-scale cash transfer programme, Bono Juancito Pinto (BJP). Exploiting the exogenous variation of the programme expansion, this paper examines the impact of BJP on schooling and child labour. The analysis suggests that the transfer increases the likelihood of school enrolment but has no sizeable effect on the incidence of child labour. The results are in line with theoretical models that predict that if leisure and schooling decisions are substitutes, a school incentive will have either positive or neutral effects on child labour. Our findings support previous evidence that schooling and work decisions are not perfect substitutes among children.

Keywords: Bolivia, child labour, conditional cash transfers, schooling
JEL classification: I25, I38, J13, J22, N36

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1 Introduction

Over the past 15 years, cash transfer programmes have become a core component of antipoverty policy strategies in the developing world. In Latin America in particular, cash transfer programmes 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 et al., 2007). Monetary incentives are particularly important, as they link income support with mandatory regular school attendance. This is done through explicit conditionalities 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* that materializes through a reduction in the shadow prices of education, which in turn can impact both schooling and child labour decisions (Behrman et al., 2009; Bourguignon et al., 2003).

The empirical literature on schooling and child labour impacts of cash transfer programmes has shown that, overall, cash transfers can successfully raise school enrolment and attendance (Attanasio et al., 2010; Dammert, 2009; Schady and Araujo, 2006; Skoufias et al., 2001), and under certain conditions, delay or reduce the propensity and intensity of child labour (Behrman et al., 2012; de Janvry et al., 2006; Ferro et al., 2010; Schultz, 2004; Skoufias et al., 2001).

In this paper, we investigate the schooling and labour market impacts of Bolivia's Bono Juancito Pinto (BJP), a cash transfer programme that was launched by the Bolivian government with the explicit objective of improving enrolment, retention, and completion rates of pupils in public schools. Different from other cash transfer programmes in Latin America, BJP does not follow a strict poverty-targeting mechanism, but instead is nearly universal in its coverage, as it covers 90 per cent of school-age children that are enrolled

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

in public schools.

The programme began in 2006, providing income support of 200 Bolivianos per year (about $25 (USD)) 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-age children in 2006 to 2.1 million in 2014.

Using data from the Bolivian National Living Standards Survey, we exploit the exogenous variation in the timing of the announcement of the programme expansion, as well as the age eligibility criteria, for identification. More specifically, we resort to difference-in-differences (DD) estimators to measure the effect of the programme on schooling and the incidence and intensity of child labour. Overall, we find evidence of a positive and significant effect of BJP on schooling decisions, although the effect is largely driven by children living in rural areas, particularly girls. However, We found no evidence of sizeable programme effects on labour market outcomes, which we attribute, at least partly, to the small size of the transfer, and the structure of the labour market and the school system in Bolivia.[2]

This paper contributes to the literature on cash transfer programmes in a number of ways. First, this is the first study that estimates the impact of BJP among children in secondary school, the level at which important occupational transitions take place in the country. Second, while most studies focus on the incidence of child labour, we also provide evidence of the impact of the programme on the intensity of child labour. Third, our identification strategy—relying on eligibility—solves the problem of selection bias found in previous studies.

The rest of the paper is organized as follows: Section 2 provides a review of the literature on schooling and child labour impacts of cash transfers. Section 3 provides an overview of BJP, highlighting its distinctive design features and characteristics, while Section 4 discusses the data and methodology adopted in this study. Section 5 presents the empirical findings with regard to the impact of BJP on schooling and work-related outcomes, and Section 6 concludes.

[2]The official school day in Bolivia last for only four hours, while market work lasts, on average, for five hours per day. Both activities are perfectly compatible and the interaction between the two can go in either direction, to support or be detrimental of schooling. In such contexts, the effect of cash transfers can, as we discuss in Section 5, be better captured by changes in labour intensity rather than changes in the incidence of child labour.

2 Schooling and labour market impacts of cash transfer programmes

In situations of poverty, where the substitutability between children's and adults' labour income exists, child labour arises not because of parental exploitation, but because of the need to find additional sources of income (Basu and Van, 1998). Legal frameworks prohibiting child labour would only be effective if policy interventions were in place to reduce households' liquidity constraints and compensate the income loss from schooling. It is important to distinguish here between children's participation in the labour market and the intensity of their engagement. Patrinos and Psacharopoulos (1997) have pointed out that the allegedly mutually exclusive relationship between child labour and schooling is not linear, particularly when the former is part-time and does not act 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 programmes, the literature has largely focused on short-term effects on schooling (Akresh et al., 2013; Behrman et al., 2009; Dammert, 2009; Filmer and Schady, 2008; Lincove and Parker, 2016; Maluccio and Flores, 2005; Skoufias et al., 2001) and child labour dimensions (Behrman et al., 2012; Edmonds and Schady, 2012; Ferro et al., 2010; Skoufias et al., 2001).[3] Cash transfer programmes are conventionally not designed with the explicit objective of reducing child labour. They have, however, proved to be effective—under certain conditions—at lowering children's participation in the labour market (Behrman et al., 2012; de Janvry et al., 2006; Schultz, 2004; Skoufias et al., 2001).

This is an important issue. Early entry into the labour market can lead to dropping out of school, which has long-term implications for children's future income and well-being in adulthood (Canelas, 2015). In several contexts, child labour 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 labour can be generally regarded as a positive contribution of cash transfers towards 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 aged between 8 and 13 (Attanasio et al.,

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

2010). Similar effects were found in Nicaragua's Red de Protección Social for children in the same age group Barrientos and Santibañez (2009), and also among beneficiary children of Ecuador's Bono de Desarrollo Humano Schady and Araujo (2006).

Similarly, a study of Brazil's Child Labour Eradication Programme (PETI), found that the programme increased children's time in school, improved academic success, and reduced labour participation and hazardous work (Yap et al., 2009). In Mexico, Rawlings and Rubio (2005) found small but significant reductions in child labour among beneficiaries of Progresa-Oportunidades, although no significant reduction was found for boys aged 16–17, which was 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 labour ((Duryea and Morrison, 2004). In Brazil, studies of Bolsa Familia found that the impact of the programme on child labour was small and in both directions (Barrientos and Santibañez, 2009).

The review by de Hoop and Rosati (2014) identified 30 studies worldwide, among which 23 focused on cash transfer programmes implemented in Latin America. None of the studies focused on Bolivia's BJP programme. Most studies cited in the review focused largely on the incidence of child labour; however, little attention was paid to the intensity of child labour, with a few exceptions, notably the work of Skoufias et al. (2001), Ferreira et al. (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 labour 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 enrolment in 2007, but had no effect on school attendance or child labour. The study relied on whether children enrolled in school in 2007 reported receiving the transfer in 2006. This creates a selection bias problem since children that reported in the 2007 survey as having received BJP in 2006 had already met the enrolment 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 enrolment and attendance, which in turn led to a lower incidence of child labour and poverty. Hernani-Limarino (2015) examined the effect of the programme covering the period 2005-2009, and found a positive effect on school enrolment for children aged 6-8 years old. More recently, Vera-Cossio (2017) looked at the effect of BJP on adult female labour supply from households with eligible children. He found that BJP increased adult female working hours by 8 per cent, which was largely explained by credit constraints and fixed costs of labour.

3 Background of Bono Juancito Pinto

The programme was introduced in 2006, initially with the objective of promoting enrolment, retention, and completion of the first five years of primary education in public educational institutions across the country. However, since 2007 programme eligibility has been expanded gradually, and by 2014 it covered all levels of primary and secondary education. Children of 6–19 years of age attending public schools are eligible to receive support from the programme. The transfer consists of a yearly payment of 200 Bolivianos (approximately US$25) conditional on proven 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 programme currently costs about 0.3 per cent of Bolivia's gross domestic product (GDP).

Table 1 shows the coverage and roll-out process of BJP. Relevant for our analysis is the *timing* of the public announcement of the programme. The Bolivian government announced the creation of BJP in December 2006 to initially cover, as discussed earlier, children enrolled in grades 1–5 of primary school, and who had complied with the programme 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 programme to include children enrolled in grade 6 of primary school. This meant 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 programme, to include children enrolled up to grade 8 (or the second year of secondary education). BJP remained unchanged until October 2012, when the government announced its expansion to include children enrolled in grade 9 (or the third year of secondary school). That means that at the beginning of the 2013 school year, eligible children were those who had completed at most eight years of schooling (up to the second year of secondary school). The progressive expansion of BJP continued until October 2014, when the programme covered the entire

Table 1: Coverage of Bono Juancito Pinto

Year	Eligible children beginning of school year	Educational levels covered end of school year	Announcement date	Payment (Bolivianos)
2006	–	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: authors, based on Decreto Presidencial No. 309 (2009), Decretos Supremos No. 28899 (2006), 29321 (2007), 29652 (2008), 648 (2010), 1016 (2011), 1372 (2012), 1748 (2013), 2141 (2014).

primary and secondary education levels, including high school (see Table 1). In the next section we discuss how we exploit this gradual expansion for the identification of causal effects of the programme on schooling and child labour.

4 Data and empirical strategy

The data used in this study come from the Bolivian National Living Standards Survey MECOVI (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). The MECOVI is a nationally representative household survey of the Bolivian population. The survey collects detailed information on household demographics, health, education, occupations and labour force participation, housing and asset ownership, household food and non-food expenditures, and income, including contributions from social assistance. It also collects 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 collect information on domestic tasks and leisure time.

We define child labourers as children aged 7–17 years who reported that they had undertaken paid or unpaid work in the previous week. We also include children who reported carrying out 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 also take 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 six. Education is free of tuition fees and, since 2009, compulsory throughout all primary and secondary levels. The school year starts in February and lasts until the end of October/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 labour from the age of ten has meant that about 20 per cent of children aged 7–14 years engage in labour activities (Bureau of International Labor Affairs, 2014). In rural areas in particular, child labour—especially related to agriculture—is embedded into normative aspects and tradition, whereby it is considered as part of children's instruction and skill development. The considerable high incidence of child labour is captured in Table 2, which shows basic statistics on school enrolment, work participation, and time allocation to income-generating activities during the week prior to the survey interview. While work participation of children 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 2: Sample statistics

	2005–2006		2013	
Variable	Mean	Std Dev.	Mean	Std Dev.
Work participation	0.23	0.42	0.17	0.38
School enrolment	0.92	0.26	0.95	0.22
Hours of market work	5.65	13.65	4.47	12.46
Observations	8,974		7,425	

Source: authors, based on MECOVI surveys.

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 is high, particularly at baseline in 2006. This can be explained to a certain extent by late school

[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 changed to six years each.

entry rates: 39 per cent of children aged 6–8 were not enrolled in school in 2006 and 45 per cent of children aged 9–11 were enrolled in a lower grade to the one corresponding to their age. In more recent years, some progress has been achieved in the basic education system. For example, by 2013, 68 per cent of school-age children were in the school grade corresponding to their age, while 26 per cent were falling behind, and only 4 per cent had dropped out of school altogether.

Table 3: Children status in the school system, by age

		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	0.39	0.60	0.00	0.00	0.05	0.94	0.00	0.01
Primary (4–6)	9–11	0.01	0.53	0.45	0.01	0.01	0.72	0.26	0.01
Secondary (1–3)	12–14	0.00	0.44	0.51	0.05	0.00	0.61	0.36	0.03
Secondary (4–6)	15–17	0.01	0.36	0.49	0.15	0.01	0.52	0.37	0.10
All	6–17	0.11	0.49	0.35	0.05	0.01	0.68	0.26	0.04

Note: panel rows add to 1.
Source: authors, based on MECOVI surveys.

4.1 Identification strategy

BJP targets all children enrolled in public primary and secondary schools, and while the transfer benefits all children independently of their socioeconomic status, it has, as described in Section 3, expanded gradually the coverage of school grades over time. We exploit this variation in coverage to compare children that were eligible to receive the cash transfer (treatment group) with those children that were just above the eligibility threshold, and therefore did not benefit from the programme (control group). A second source of variation comes from the *timing* of the announcement of the programme expansion. We also exploit this exogenous variation to estimate the differences in outcomes between treatment and control groups before and after the programme implementation in a DD framework. The basic idea behind our identification strategy is illustrated in Figure 1.

For the analysis, we focus on the last school grade covered by the programme in the last available survey. We do so for several reasons. First, enrolment rates in primary school in Bolivia are relatively high. In fact, primary school is almost universal, so if the transfer is effective in increasing enrolment rates and school retention, this is more likely to be observed in secondary education, in which occupational transitions and school drop-out rates are manifested. Therefore, for us it is more relevant to test whether traditionally vulnera-

Figure 1: 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.

ble groups that are more likely to drop out of school and work more intensively—due to an increasing opportunity cost of schooling—have improved their schooling achievements relative to the pre-treatment period. Second, by using the last available survey and looking at the behaviour of children and their schooling and work decisions in the last covered school year in the survey, we can take advantage of the cumulative exposure to the programme, meaning that those children who were last covered by the programme 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 consists of children who had completed eight years of schooling, whereas the control group was made of children who had completed nine years of schooling but had not been exposed to the programme. There is a concern about our choice. It is conceivable that children with nine years of schooling could modify their behaviour, given the expectations the programme could generate. If that was the case, schooling and work outcomes in the control group in the post-treatment period would not be comparable to outcomes of the treatment group in the absence of treatment. We argue, however, that this is highly unlikely, given the fact that the previous expansion of the programme before 2012 took place in 2008; therefore, any expectation about further programme expansions between these years was indeed minimal.[5]

4.2 Estimation strategy

We estimate the effect of the programme on school enrolment and work participation using a DD approach. The DD equation takes the following form:

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

where Y is the outcome of interest (i.e. work participation or schooling), T is a dummy variable equal to 1 for eligible children (eight years of schooling) and 0 otherwise (nine years of schooling), P is a dummy variable equal to 1 for the years when the transfer was paid, and γ is the parameter of interest yielding the programme treatment effect. X_i is a vector of socio-demographic characteristics including the age, gender, and ethnicity of the child, the age and education level of the household head, household size, the number of household members working, and housing conditions including piped water, toilet connected to the sewerage system, and access to electricity. We also include in X_i controls for rural households, and geographical dummies for the nine departments in Bolivia, whereas δ_t controls for potential time-varying effects of each round of data. The specification includes robust standard errors clustered at the household level.

In order to capture changes in the intensity of child labour, we also estimate the effect of BJP on the amount of hours children spent on market work, using the following specification:

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

where H accounts for the number of hours per week allocated to income-generating activities (i.e. market work). We also provide 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. aged 13–16), and then estimated separate models for children living in

[5] We focus on grade eligibility rather than on programme take-up. This means that the results presented in Section 5 measure the intent-to-treat programme effects or, more generally, the programme effect on the targeted population.

rural areas, children living in urban areas, boys, and girls.

The DD estimates would provide unbiased average treatment effects of the programme 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 to compare trends in outcomes between treatment and control groups before the programme started. If they are similar, it is likely they would have been the same in the post-treatment period in the absence of the programme. We test this using data from the pre-treatment period (2005–2006). The results presented in Table A.8 in the Appendix suggest that time trends are similar for treatment and comparison groups.

Another possible source of bias arises from the presence of an unbalance 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 follow Blundell and Dias (2009) and first match treatment and control observations using a kernel propensity score matching, impose a common support, and then calculate a DD-matching (DDM) estimator as follows:

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

where D_{it} is the treatment indicator equal to 1 for the treatment group in the follow-up period, and 0 otherwise, $w^c_{it=0}$, $w^c_{it=1}$, and $w^t_{it=0}$ are the kernel weights for the control and treatment groups in the baseline ($t = 0$) and follow-up ($t = 1$) periods, respectively. The common support is composed of members of the treatment group for whom a counterfactual is found in each of the control samples.[6]

Tables A.1–A.5 in the Appendix, 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 improves substantially 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.

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

For reference, we also present the p values of the mean differences for each of the observed characteristics we are controlling 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 sub-samples under analysis.

Finally, given the nature of the outcome variables, two dichotomous and one censored at 0, we should ideally perform the estimation using non-linear 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 carry out the estimations using ordinary least squares (OLS).

4.3 Some concerns about the identification strategy

The first concern about our identification strategy comes from the fact that the transfer is directed only to children enrolled in public schools, which correspond to 90 per cent of all school-age children in the country. If the transfer becomes an incentive for children in private schools to switch to public schools, our results will be biased. We argue that given the small amount of the transfer, this situation is highly unlikely. The second concern comes from the number of eligible children within the households. While this has been controlled for, to a certain extent, in the previous specification by clustering standard errors at the household level, we now explicitly control in Equation 4 for the number of eligible children in the household and its interaction with treatment years as follows:[7]

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

5 Results

In this section, we report the results first for the full sample and then for different sub-population groups. In Tables 4 and 5 we report the effect of BJP on the probability of school enrolment and child labour force participation, while Table 6 presents the results on the intensity of child labour. The idea is that while the transfer size is too small to alter labour force participation of children, it may still affect the number of hours children spend working during the week.

[7]For a more technical discussion, see Miguel and Kremer (2004) and Villa (2016b).

The first column of Tables 4 and 5 reports the DDM estimates on the full sample. Overall, we find an increase in the likelihood of school enrolment of 5 per cent at grade 8 (14 years old). This is quite significant, given the important occupation transitions that usually occur at that age in Bolivia. Unsurprisingly, we also find that the programme has no sizeable impact on child labour, either at the extensive or intensive margin. In general, our results are consistent with previous work on cash transfer programmes in Latin America, including those found by Schultz (2004) in Mexico, Macours and Vakis (2009) in Nicaragua, and Attanasio et al. (2010) in Colombia.

Table 4: Impact of the BJP programme on school enrolment

	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

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

Source: authors.

Table 5: Impact of the BJP programme 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

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

Source: authors.

5.1 The urban–rural dichotomy

Rural–urban differences in living standards are marked in Bolivia. In 2006, poverty incidence in rural areas reached 76.47 per cent of the population,[8] that is eight in every ten

[8] Official figures from Bolivia's National Institute of Statistics.

Table 6: Impact of the BJP programme 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

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

Source: authors.

persons; in urban areas it reached 50.27 per cent. Differences in extreme poverty levels are even more striking, with rates of 62.25 per cent in rural areas and 23.36 per cent in urban areas. The incidence of child labour is also high. The participation rate was 64.85 per cent for rural children and 16.96 per cent for 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 4 and 5 present the results of the DDM estimates by area of residence. The transfer has a significant positive effect on school enrolment in rural areas but not in urban areas. While the coefficients of work participation and work intensity both have the desired negative sign in both rural and urban areas, the estimates are 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 judgement, the insignificant effect of the programme on child labour.

In 2008, a study on child labour in Bolivia carried out by Bolivia's National Statistical Institute and the International Labour Organisation[9] revealed that the monthly average salary of children aged 14–17 years was 633 Bolivianos in urban areas and 657 Bolivianos in rural areas. This means that BJP, in 2008, represented on average about 2.5 per cent of children's income in both urban and rural areas.

[9] See INE (2010) for further details.

5.2 Gender differences

Bolivia does not have a significant gender gap with regard to school attendance. Regarding child labour, 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 4 and 5 present the gender results by focusing on girls and boys separately. Similarly to the previous estimations, we find statistically significant results only for school enrolment, 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 labour leads us to infer that the increase in school enrolment led to a reduction in time allocated to household chores. Unfortunately, we were unable to test whether this was the case.

The results for child labour remained virtually unchanged irrespective of gender. Once again, the monetary value of the transfer, which is too low to compensate for the opportunity cost of schooling, seems to provide a sensible explanation for absence of impact. Bolivias National Statistical Institute (INE, 2010) has reported that boys aged 14–17 years earned on average 715 Bolivianos per month. Their salary is also 1.6 times higher than that of girls (457 in urban areas and 427 in rural areas). In this context, the BJP transfer accounts for only 2 per cent of a boy's monthly earnings.

5.3 Spillover effects

Finally, in this section we test whether the positive effect of the programme on schooling is robust by controlling for spillover effects at the household level. Table A.6 in the Appendix 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 A.6, the results are robust to spillover effects at the household level for all specifications.

6 Conclusion

Different from other cash transfer programmes in Latin America, BJP is nearly universal, with coverage of about 90 per cent of school-age children who are enrolled in public schools in Bolivia. By adopting a DD with matching approach, we have assessed the effect of the programme on schooling and child labour decisions.

Overall, we find evidence that the programme has been successful in increasing school enrolment rates, which is consistent with previous scholarly work; however, we found no evidence of average treatment effects of the programme on child labour. There are at least two potential explanations for this result. First, the monetary value of the transfer is too low to compensate for the increasing opportunity cost of schooling, particularly among children aged 13–16 years, the period in which important school–labour 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 labour in the country, allows children to combine schooling with income-generating activities.

One immediate implication of our findings is that parents are likely to be substituting other uses of their children's time, such as leisure. So, in the presence of child labour, 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 the cognitive, emotional and physical development of children. This is an important area for future research.

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Appendix

Table A.1: Characteristics across matched and unmatched samples, group 1

	Unmatched sample			Matched sample		
Variable	Treated	Control	$p > t$	Treated	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
Number of household members working	2.45	2.35	0.26	2.44	2.37	0.43
Education years of head	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	

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

Source: authors.

Table A.2: Characteristics across matched and unmatched samples, group 2

	Unmatched sample			Matched sample		
Variable	Treated	Control	$p > t$	Treated	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
Number of household members working	2.53	2.21	0.01	2.50	2.46	0.71
Education years of head	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	

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

Source: authors.

Table A.3: Characteristics across matched and unmatched samples, group 3

	Unmatched sample			Matched sample		
Variable	Treated	Control	$p > t$	Treated	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
Number of household members working	2.36	2.49	0.34	2.36	2.53	0.18
Education years of head	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	

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

Source: authors.

Table A.4: Characteristics across matched and unmatched samples, group 4

	Unmatched sample			Matched sample		
Variable	Treated	Control	$p > t$	Treated	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
Number of household members working	3.02	3.23	0.30	3.02	2.85	0.30
Education years of head	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	

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

Source: authors.

Table A.5: Characteristics across matched and unmatched samples, group 5

	Unmatched sample			Matched sample		
Variable	Treated	Control	$p > t$	Treated	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
Number of household members working	2.12	2.06	0.44	2.13	2.22	0.31
Education years of head	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	

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

Source: authors.

Table A.6: Impact of the BJP programme on school enrolment: spillover effects

	National sample	Rural	Urban	Boys	Girls
No. eligible children in hh * 2013	–0.010	–0.004	–0.012	–0.020	–0.009
	(0.009)	(0.020)	(0.009)	(0.021)	(0.016)
No. eligible children in hh	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

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

Source: authors.

Table A.7: Impact of the BJP programme on work participation: spillover effects

	National sample	Rural	Urban	Boys	Girls
No. eligible children in hh * 2013	0.015	0.006	0.034	–0.002	0.043
	(0.022)	(0.038)	(0.021)	(0.041)	(0.038)
No. eligible children in hh	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

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

Source: authors.

Table A.8: Pre-programme time trends in schooling, work, and hours worked

	School enrolment	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

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

Source: authors.

Table A.9: Impact of the BJP programme on hours worked: spillover effects

	National sample	Rural	Urban	Boys	Girls
No. eligible children in hh * 2013	0.521	0.276	0.979	–0.737	1.550
	(0.513)	(1.026)	(0.683)	(0.039)	(0.905)
No. eligible children in hh	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

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

Source: authors.