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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 social protection programme: the Bono Juancito Pinto (BJP). Exploiting the exogenous variation of the programme expansion, this paper examines the impact of the BJP on schooling and child labour. The analysis suggests that the transfer increases the likelihood of school enrolment and that it has no sizable effect on the incidence of child labour. The evidence is in line with theoretical models that predict that if leisure and schooling are substitutes, a school incentive may have either no or positive impact on child labour. Further, the findings support previous evidence that children's school and work participation are not perfectly substitutable.

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 also nutrition. This 'human development' approach to poverty reduction gives strong emphasis on tackling the inter-generational transmissions of poverty through human capital investment, see Niño-Zarazúa (2011), Levy (2006).

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Education policy plays an *instrumental* role, as it is assumed to enhance human capital formation and through schooling decisions, address some of the structural roots of poverty. Flagship cash transfer programmes such as Mexico's Progresa-Oportunidades (currently known as Prospera), Brazil's Bolsa Familia, and Colombia's Familias en Acción are leading examples of this antipoverty policy framework that has very distinctive design features, notably: i) a poverty focus that often relies on categorical criteria and complex systems of identification and selection of beneficiaries,[1] and ii) monetary incentives that aim to change behaviours and preferences of parents and school-age children in the utilisation of education services Masino and Niño Zarazúa (2016). These features, together with the scale of transfer programmes, their coverage, transfer size, regularity of transfers, and the duration and timing of support, play a critical role in explaining the direction and size effect cash transfer programmes on schooling, see Barrientos and Niño Zarazúa (2011), Barrera-Osorio et al. (2011), Parker et al. (2007).

Monetary incentives are particularly important, as they link income support with regular school attendance. This is done through explicit conditionalities that are monitored and enforced with varying degrees of effort and efficacy across countries.[2] 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 materialises 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).

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

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 objective of improving the 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

[1] Categorical criteria focus on groups that are regarded as vulnerable and therefore entitled to receive income support. These groups can be identified in terms of age (e.g. children, the elderly), ill-health and disabilities, and people in disadvantaged social conditions (e.g. orphans and ethnic minorities). In some contexts, vulnerable groups correlate with poverty. Systems of identification and selection of beneficiaries also include geographical methods of identification, self-selection, and means-tests or proxy means tests. For a discussion see Barrientos and Niño Zarazúa (2011) and Handa and Davis (2006).

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

in its coverage, as it covers 90 per cent of school-age children that are enrolled in public schools.

The programme began in 2006, providing income support of 200 Bolivianos per year (about $25 USD) to children enrolled in grade 1 to 5 of primary school. In subsequent years the government gradually expand support to include secondary education in its coverage, meaning that between 2006 and 2014 the number of beneficiaries increased from 1,084,967 to 2,132,393 school-age children. 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, to estimate the differences in outcomes between treatment and control groups before and after the programme implementation. Overall, we find positive and significant effects of BJP on schooling, driven by children living in rural areas, particularly girls.

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 organised as follows: Section 2 provides an review of the literature on schooling and child labour impacts of cash transfers. Section 3 provides an overview of Bono Juancito Pinto, highlighting its distinctive design features and characteristics, whereas Section 4 discusses the data and methodology adopted in the 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 Schooling and labour market impacts of cash transfer programmes

The literature on the impacts of cash transfers on schooling and child labour is considerable. School enrolment and school attendance are common proxies for schooling. While school enrolment is an imperfect proxy for attendance, grade progression, and school transitions, it remains a valid indicator in the context of the Bono Juancito Pinto, which provides yearly payments at the end of each school year, conditional upon at least 80 of proven attendance records during the school year.

Cash transfer programmes are often not designed with the explicit objective of reducing child labour. They have, however, proved to be under certain conditions, successful in

lowering children's participation in the labour markets ((Behrman et al. (2012), Skoufias et al. (2001), Schultz (2004), de Janvry et al. (2006)).

This is an important issue. Early entry into the labour markets can lead to school drop outs, which has long-term implications for future income and well-being of children Canelas (2015). In many developing countries, child labour can also be associated with hazardous employment. Reducing child labour can be in general a very positive step towards sustained efforts to reduce poverty and vulnerability.

In situations of poverty, where the substitutability between children's and adults' labour income exists, child labour would arise 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 of children's time in school, but rather, as a complementary strategy that may in fact allow children to continue their education.

In Colombia, Familias en Acción led to a significant reduction in child labour in rural areas, particularly amongst children aged between 10 and 13 (World Bank, 2006). 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 to 17, which was linked to the an 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 of 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 Skoufias et al. (2001), Ferreira et al. (2009), Attanasio et al. (2010), Gee (2010), and Del Carpio and Loayza (2012).

Evidence of the impact of the BJP on school enrolment and child labour is scant. The few studies available, while providing useful information, suffer either from selection bias or they focus only on child participation in the labour market and nothing is said concerning labour intensity. Grigoli and Sbrana (2013), using data from 1999-2007, found that being a recipient of the BJP in 2006 had a significant and positive effect on school enrolment in 2007, but it has not significant effect on either school attendance or child labour. However, the study relies on whether children enrolled in school in 2007 reported to have received the transfer in 2006. This creates a selection bias problem since children who reported in the 2007 survey as having received the stipend 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.

Yáñez (2012), using micro-simulation techniques with data from 2005, found that the BJP was structured so as to create the necessary incentives to induce 4 of every 100 children not attending school to enrol and attend classes. It also concluded the stipend would lead to a slight reduction of poverty and inequality and to lower child labour. Hernani-Limarino (2015) using kernel estimations with data from 2005-2009, found a positive effect on school enrolment for children aged 6-8 years old.

3 Background of Bono Juancito Pinto

The programme was introduced in 2006 with the initial objective of promoting enrolment, retention and completion of the first five years of primary education in public educational institutions across the country. Since 2007, programme eligibility has been expanded gradually and by 2014 it covered the entire primary and secondary education levels.

Children between six and nineteen years of age, attending public schools are eligible to receive support from the programme. The transfer consists of a yearly payment of 200 Bolivian pesos, approximately $25 USD, 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 in nationwide ceremonies conducted with the help of the armed forces. According to the official information, between 2006 and 2014 the number of beneficiaries passed from 1,084,967 to 2,132,393 school-age children with a public expenditure close to 0.3 percent of GDP.

Table 1 shows the programme design and roll-out. In 2006, the Bolivian Government announced the creation of the CCT and the payment of the transfer in December of that year to children enrolled from first to fifth grade of primary school who had complied

with the programme conditions. Thus, at the beginning of the 2007 school year, eligible children were those who have at most four years of schooling and were facing the decision to enrol into first to fifth grade of primary school. On October 2007, the first expansion of the programme took place and included as beneficiaries children enrolled in 6th grade of primary school, so eligible children were those with at most 5 years of schooling. In 2008, the programme was expanded once again and included children enrolled in 1st and 2nd years of secondary school. During the following years the coverage of the programme did not change until 2012, when the Government announced the inclusion of children enrolled in 3rd year of secondary school. Thus, at the beginning of the 2013 school year, eligible children were those who have completed at most 8 years of schooling (2nd year of secondary school). The coverage expansion continued until October 2014, when the programme covered the entire primary and secondary education levels.

Table 1: Coverage of Bono Juancito Pinto

Year	Eligible children beginning of school year	Educational levels covered end of school year	Announcement date	Payment
2006	-	1st-5th grade	October 2006	200 Bs.
2007	0-4th grade	1st-6th grade	October 2007	200 Bs.
2008	0-5th grade	1st-8th grade	July 2008	200 Bs.
2009	0-7th grade	1st-8th grade	October 2009	200 Bs.
2010	0-7th grade	1st-8th grade	October 2010	200 Bs.
2011	0-7th grade	1st-8th grade	October 2011	200 Bs.
2012	0-7th grade	1st-9th grade	October 2012	200 Bs.
2013	0-8th grade	1st-10th grade	October 2013	200 Bs.
2014	0-9th grade	1st-12th grade	October 2014	200 Bs.
2015	0-11th grade	1st-12th grade	-	200 Bs.

4 Data and Empirical Strategy

Data

The data used in this study comes from the Bolivian National Living Standards Survey MECOVI (Encuesta Nacional de Condiciones de Vida) for the period 2005-2013, which was conducted by the 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 report that they worked the previous week (whether paid or unpaid). We also include all children who report to be engage in any of the following activities: (1) working in agriculture activities or caring for animals, (2) helping in family business, (3) selling products, (4) making products to sell, and (5) providing services for paid (washing clothes, cutting hair, teaching, etc). The definition of schooling is based on information on whether children are enrolled in school.

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. Each academic year last for about 40 weeks, five days per week, and four hours per day. The school year starts in February and lasts until the end of October-early November. Primary and secondary education consist of six years of study each.[3]

Bolivia's educational system allows children to work, since the school day lasts on average only 4 hours. As a result, an important percentage of children combine work and schooling. In particular, in rural Bolivia, agricultural child labour is more of a cultural aspect and it is considered part of children's development. Table 2 presents average statistics on school enrolment, work participation, and time allocation to income generating activities during the week previous the survey interview, for different sample years.

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	

Table 3 shows the status of children in the school system in 2006 and 2013. Retention rates are relatively high, but there is slow progress through school grades. The proportion

[3]Until 2010 the school system was organise as follows: eight years of primary school and four years of secondary school. Since 2011, the system changed to six years each.

of children behind the corresponding grade for age is quite high, particularly in 2006. This can be explained to a certain extend by late entrance into the school system: 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 that the one corresponding to their age. However, important progress has been recently achieved in the basic education system. By 2013, 68 per cent of children were in the grade corresponding to normal progress, 26 per cent were falling behind, and only 4 per cent have dropped out school.

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

Panel rows add to one.

Identification strategy

The design features of the programme, that aims to cover all school children enrolled in primary and secondary public education, makes it difficult to identify a control group for the analysis; however, while the transfer benefits all school age children independently of their socioeconomic status, it has gradually covered different grades of the school system. This variation in coverage allows us to compare the group of children who are eligible to receive the transfer (treatment) with those who are just above the school-grade eligibility threshold, and therefore cannot benefit from it (control). A second source of variation comes from the timing of the programme's coverage announcement. Essentially, we estimate the differences in outcomes between treatment and comparison groups before and after the programme implementation. The basic idea behind the identification strategy is illustrated in Figure 1.

For the estimation, we focus on the last grade covered by the programme in the last available survey for a variety of 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 should be more visible in secondary school where drop out rates become higher. For the same reason, it is more interesting for us to see whether traditionally vulnerable groups that are more likely to drop out school

and work full time given their opportunity cost of time, have improved their schooling outcomes compare to the pretreatment period. Second, by using the last available survey and looking at the behaviour of children in the last covered school year in that survey, we can take advantage of an individual's exposure to the programme; meaning that those children that were last covered by the programme, have also been exposed to it for a longer period of time.

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

Figure 1: Identification strategy

The last available survey is 2013, by this year children who have completed at most 8 years of schooling at the beginning of the 2013 school year were eligible to receive the transfer. Therefore, our treatment group consist of children who have completed 8 years of schooling and our control group of children who have completed 9 years of schooling and who have not been exposed to the programme.

There is one concern regarding our choice, it is possible that children with 9 years of schooling could modify their behaviour given expectations regarding the expansion of the programme. If this would be the case, outcomes in control groups in the post-programme period would not be representative of outcomes in treatment groups in the absence of the programme. We argue that this is highly unlikely given that the last expansion of the programme before the one in 2012 took place in 2008, and therefore any expectation of expansion between these years should be minimal if any.

Note that we focus on grade eligibility rather than on actual take-up. Basically, we estimate the intent-to-treat effects of the programme or the effects of the BJP on the entire targeted population.

Estimation strategy

We estimate the effect of the programme on school enrolment and work participation using a difference-in-differences approach. The estimated equation is the following:

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

where Y is the outcome of interest, i.e. work participation or schooling. T is an indicator variable equal to one for eligible individuals (8 years of schooling) and zero otherwise (9 years of schooling), P is an indicator variable equal to one for the years when the transfer was paid, and γ is the parameter of interest yielding the treatment effect. X_i is a vector of sociodemographic 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, three household assets that serve as wealth proxies (piped water, toilet connected to sewage, and electricity), a control for rural households, and geographic dummies for the nine Bolivian departments. δ_t controls for potential time varying effects of each round of data. The specification includes robust standard errors clustered at household level.

In order to capture changes in work intensity, we also estimate the impact of the BJP programme on the amount of hours children spend 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 provide robust standard errors, clustered at household level. We use the data for children who have completed second and third year of secondary school (i.e aged 13-16), and we estimate separate models for children living in rural areas, children living in urban areas, boys, and girls.

The difference in differences approach would provide unbiased estimates under the assumption of 'parallel trends', i.e. in the absence of the treatment, the outcomes of the two groups would have followed parallel 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 that they would have been the same in the post-treatment period in absence of the programme. We test this using data from the pretreatment period 2005-2006. The results

presented in Table A.1 in the Appendix, suggest that time trends are the same for treatment and comparison groups.

Another possible source of bias comes from the presence of an unbalance distribution of observables between treatment ($Z_i = 1$) and control ($Z_i = 0$) groups affecting the outcomes of interest Y_{it}. To address this concern, we first match treatment and control observations using a kernel propensity score matching, impose common support, and then calculate the difference in differences estimators. Following Blundell and Dias (2009), the matching-DID estimator with repeated cross-section is specified as follows:

$$\begin{aligned} DID = \{&E(Y_{it=1}|D_{it=1} = 1, Z_i = 1) - w^c_{it=1} \text{ x } E(Y_{it=1}|D_{it=1} = 0, Z_i = 0)\} \\ &- w^t_{it=0} \text{ x } \{E(Y_{it=0}|D_{it=0} = 0, Z_i = 1) - w^c_{it=0} \text{ x } E(Y_{it=0}|D_{it=0} = 0, Z_i = 0)\} \end{aligned} \quad (3)$$

where D_{it} is the treatment indicator equal to one for the treated group in the follow-up period and zero 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. Note that, the common support is composed of the treated to whom a counterfactual is found in each of the three control samples.[4]

Tables **??-??** in the Appendix, show the characteristics for matched and unmatched samples at baseline and 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 standardised bias and the decrease 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 observables characteristics we are controlling for. We note, however, that t-tests and other statistical tests of hypothesis are influenced by 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 zero, 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 by OLS.

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

Identification concerns

The first concerns arise 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 sample. 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 errors at household level, we now explicitly control for this by adding to the specification the number of eligible children in the household and its interaction with treatment years, see Miguel and Kremer (2004) and Villa (2016b).

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

5 Results

This section reports several sets of regression results according to the estimation strategy over the full sample and for different sub-populations. In Tables 4 and 5 we report the effect of the cash transfer in the probability of both school enrolment and children's labour force participation. In Table 6, we look at the impact of the transfer on work intensity. The idea is that while the transfer amount may be too small to significantly change the likelihood of a child being employed, it may have an impact on the number of hours of work performed by a child during the week.

The first column of the tables, reports the estimates on the full sample. Overall, we find an increase in the likelihood of school enrolment of five per cent. This is a quite significant result given the sensible age of children in this grade, i.e. 14 years old. The results also show that the transfer has no statistically significant effect on child labour, neither at the extensive nor at the intensive margin.

In general, our results on schooling are consistent with previous research on cash transfer programmes in the Latin America region. In particular, they are similar to 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 difference-in-differences approach. In all specifications we use control variables, 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*

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 difference-in-differences approach. In all specifications we use control variables, 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*

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 difference-in-differences approach. In all specifications we use control variables, 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*

5.1 *The urban-rural dichotomy*

Rural-urban differences in living standards are marked in Bolivia. At 2006, poverty incidence in rural areas attained 76.47 per cent of the population,[5] that is 8 of every 10 persons, while in urban areas it attained 50.27 per cent. Differences on extreme poverty levels are even more striking, with rates that attained 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 the tables present the results of the estimation by area of residence. The transfer has a significant positive effect in school enrolment in rural areas but not in urban areas. While the coefficients of work participation and work intensity have both 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 4 hours. As a result, an important percentage of children combine work and schooling. In particular, in rural Bolivia, agricultural child labour is more of a cultural aspect and it is considered part of children's development. This fact, coupled with the small amount of the transfer, can easily explain the no statistically significant effect of the programme on child labour.

The 2008 study on child labour in Bolivia carried out by the Bolivia's National Statistical Institute and the ILO,[6] revealed that the monthly average salary of children aged 14-17 years was 633 Bolivian pesos in urban areas and 657 Bolivian pesos in rural areas. This means that the BJP, in 2008, represented in average 2.5 per cent of children's income in both urban areas and rural areas.

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. When looking at the heterogeneous effects of the transfer by gender, given the traditional division of labour and gender roles documented since early ages, it is important to keep in mind that for this study, we do not have time use data on domestic activities and therefore, we are only able to see the effect of the transfer on market work.

[5] Official figures, Bolivia's National Institute of Statistics.

[6] See INE (2010)

Columns 4 and 5 on the tables present further results focusing on girls and boys separately. Similarly to the previous estimations, we find statistically significant results only for school enrolment. We also find, that the likelihood of schooling increases only for girls.

As mentioned above, the absence of time use data on domestic activities and leisure time, does not allow us to take into account substitution effects between different activities. In the case of girls, given the traditional division of labour, it is likely that the increase in the probability of school enrolment comes along with a reduction of time allocated to household chores; unfortunately, we are unable to test whether this is true or not in our sample.

The results for work related activities remains virtually unchanged irrespective to gender. Once again, the monetary value of the transfer, which is to too low to compensate the forgone income from a reduction of child labour, seems to provide a feasible explanation for the results. The report from the Bolivias National Statistical Institute, INE (2010), revels that boys aged 14-17 years earn on average 715 Bolivian pesos 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*

To check whether the positive effects of the programme found in schooling are robust, we control for spillover effects at household level. Table 7 shows the results from Equation 4. The coefficient of interest α captures the spillover effects of the transfer in 2013. If it is significant, spillover effects cannot be rejected. As shown in table, the results are robust to spillover effects at household level for all specifications.

Table 7: Impact of the BJP programme on school enrolment: spillover effects

	National sample	Rural	Urban	Boys	Girls
No. eligible children in hh x 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 difference-in-differences approach. In all specifications we use control variables, time and department fixed effects. Robust standard errors clustered at household level in parenthesis. *Significance level at *p<0.10, **p<0.05, ***p<0.01*

Table 8: Impact of the BJP programme on work participation: spillover effects

	National sample	Rural	Urban	Boys	Girls
No. eligible children in hh x 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 difference-in-differences approach. In all specifications we use control variables, time and department fixed effects. Robust standard errors clustered at household level in parenthesis. *Significance level at *p<0.10, **p<0.05, ***p<0.01*

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

	National sample	Rural	Urban	Boys	Girls
No. eligible children in hh x 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 difference-in-differences approach. In all specifications we use control variables, time and department fixed effects. Robust standard errors clustered at household level in parenthesis. *Significance level at *p<0.10, **p<0.05, ***p<0.01*

6 Conclusion

Different from other cash transfer programmes in Latin America, the Bono Juancito Pinto is nearly universal in terms of coverage, as about 90 per cent of school-age children in the country are enrolled in public schools and are, de facto, eligible to receive the transfer. By using a difference in differences approach, this paper studies the impact of the BJP on schooling and child labour in Bolivia.

Overall, there is clear evidence of the programme success in increasing school enrolment rates, which is consistent with previous research on cash transfer programmes in developing countries; however, there is no statistically significant evidence of spillover effects on child labour. There are at least two potential explanations for this result: first, the monetary value of the transfer may be too low to compensate the forgone income from a reduction of child labour stemming from an increase in school enrolment, in particularly among children aged 13-16. Second, Bolivia's educational system allows children to work, since the school day lasts on average only 4 hours and therefore, an important percentage of children can perfectly combine work and schooling.

Given the increased school participation and the not statistically significant effect on child labour, one immediate implication of these findings is that parents are substituting

other uses of their children's time, such as leisure. Indeed, an increase in school participation comes at the expense of a reduction in other activities, i.e. work or leisure. Unfortunately, in our case, it is not possible to distinguish whether the increase in school enrolment comes from a reduction in the time allocated to domestic chores, to leisure, or both.

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Appendix

Table A.1: Preprogramme time trends in schooling, work, and hours worked

	School enrolment	Work participation	Hours worked
Treatment group x 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 difference-in-differences approach. In all specifications we use control variables, time and department fixed effects. Bootstrapped standard errors clustered at household level, 1200 repetitions.
*Significance level at *p<0.10, **p<0.05, ***p<0.01*