



Breaking the poverty cycle? Conditional cash transfers and higher education attainment

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ABSTRACT

This paper analyses the effects of the Peruvian 2005 Juntos Conditional Cash Transfer, program on higher education attainment and by gender. Based on the Young Lives Survey and using matching techniques, we find that Juntos has a positive effect on higher education attainment. Recipients are 8.5 percentage points more likely to attain technical studies, and this positive result remains regarding the matching technique used. Moreover, after controlling for community and cognitive test variables, recipients are 11.4 percentage points more likely to attain university studies. The positive effect of Juntos, however, is only for men and not for women revealing a gender gap in higher education attainment among Juntos recipients.

1. Introduction

Over the past few decades there has been a rise in inequality across developing countries characterized by unprecedented levels of wealth and increasing vulnerability and poverty especially among women and girls (DESA, 2018; UNICEF, 2019). As a result, social protection programs across the developing world have been created to meet some of the challenges associated with enduring poverty and the inability of individuals to adapt to increasingly precarious conditions. Within this context, social protection programs aim to ameliorate the risks and vulnerabilities felt by individuals at different points and transitions in their life course including childhood, youth, adulthood, and old age (UNICEF, 2019; ILO, 2017). Among state-led social protection programs, Conditional Cash Transfers (CCTs) have been widely adopted across Latin America to provide targeted income transfers to qualifying households with the purpose of breaking the intergenerational cycle of poverty by building human capital through policy interventions (Garcia and Saavedra, 2017; Barrientos and Hulme, 2009; DESA, 2018), during the re/productive stage of women's life course and the pre-adulthood stage of the educational life course of their offspring.

To build human capital and break the poverty cycle, most CCT programs require that poor women in their reproductive gender roles as mothers and caregivers meet program conditionalities, including self and child periodic health and nutrition check-ups and milestones as well as to ensure that their children meet primary and/or secondary

educational program requirements. As designated program recipients, program participation provides poor women access to income transfers so long as they make sustained and prolonged efforts to meet school enrollment and attendance program conditionalities; thus, enhancing the human development of their offspring. From a poverty-alleviation perspective, CCT educational conditionalities reflect the emphasis governments place on education early in a child's life course as an essential catalyst for intergenerational change and social mobility. As such, potential short and longer-term educational outcomes and intergenerational transitions associated with CCT program conditionalities are tightly linked to the efforts made by women caregivers.

While much has been learned from scholarly analyses of CCTs related to primary and secondary children's educational enrollment, attendance, performance and wellbeing (Baird et al., 2013; Fiszbein and Schady, 2009; Garcia and Saavedra, 2017; Jones and Samuels, 2015; Millán et al., 2020), less is known about the effect of CCTs on educational attainment in higher education and by gender. Given that CCTs constitute a government intervention in the educational life of program recipients, it is important to consider the effects of CCT programs on higher educational attainment of boys and girls since primary and secondary education attainment is often insufficient to break the cycle of intergenerational poverty. This is because CCT programs may affect higher educational attainment even after program participation is concluded not only by enforcing primary and secondary school attendance and by emphasizing the value of education for social mobility

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(Barrera-Osorio et al., 2019), but also by raising educational aspirations of parents and children (Chiapa et al., 2012; García et al., 2019), and their perceptions regarding the returns on education (Barrera-Osorio et al., 2019). From an economic perspective, studies find that cash transfer programs help alleviate financial strains for disadvantaged families relaxing some of their liquidity constraints (García et al., 2019). Consequently, we expect that changes in behavior related to valuing educational attainment do not stop at the primary or secondary school levels, but that household aspirations and the easing of economic constraints will have longer-term effects on higher educational attainment. Within the context of the poverty gender gap across the life course (Boudet et al., 2018; DESA, 2018) and because these government programs are premised and justified on their potential to break the intergenerational cycle of poverty, examining the effects of CCT programs on higher education by gender sheds light on whether CCT education-related interventions benefits boys and girls equal; therefore, providing an indication of their potential to short-circuit the intergenerational transmission of poverty among women.

This article contributes to the literature on educational attainment and gender within the context of government educational interventions by assessing the effect of Peru's 2005 National Program of Direct Help to the Poorest or *Juntos*, a means-tested program targeting poor households, on higher education attainment. Within the context of poverty alleviation, the *Juntos* program seeks to influence household behavior by providing incentives to poorer households to increase human capital via educational attainment to improve future opportunities of household members and to generate higher incomes (Grey et al., 2018). Therefore, we focus on higher education attainment (i.e., technical and university studies) since reaching this level of education can potentially provide more opportunities for social mobility. Within the context of Peru, figures from the 2017 Census show that low educational attainment is a pressing problem given that only 25.7% of the Peruvian population over 24 years of age held a higher education degree despite having a population with 94.9% primary and 74.5% secondary school education. Because *Juntos* has an educational component that is tightly linked to governmental intergenerational poverty alleviation strategies, this study contributes to the literature by examining longer-term outcomes for higher education attainment and by gender providing an opportunity to examine the program's limitations and opportunities.

To the best of our knowledge, this study is the first to explore the effect of *Juntos* on higher education attainment and by gender using information of the educational trajectories of former beneficiaries beyond program completion and tracking them for 14 years. As such, our sample dataset and its analysis contribute to the literature by following program participants across time while they were in the program and later in life after they left the program.

Akin to our study, there are two recent publications, Millán et al. (2020) and Barrera-Osorio et al. (2019) exploring the effect of randomized cash transfer programs on higher education in specific regions of Honduras and Colombia, respectively. We build on these works by examining *Juntos*, which is a non-randomized program with a larger geographic scope covering nearly all of Peru's poorest districts and by separating university and technical studies attainment. Making this differentiation is important to reflect not only the educational pathways available for poorer students but also as an indication of their potential prospects in the labour market. This study also builds on previous studies on CCTs and educational attainment by providing evidence of the effects of *Juntos* on higher education attainment by gender and the higher education gender gap. From a policy perspective, while CCTs are intended as a short-term government intervention, the rapid worldwide uptake of such programs and the onus placed on poor women to meet program conditionalities require consideration of the intergenerational and life course effects CCT programs have on higher education attainment by boys and girls, particularly within the context of the poverty gender gap.

We rely on Young Lives Survey (YLS) data of *Juntos* household

beneficiaries to examine the effect of the program on higher education attainment defined as technical or university studies. Using three matching techniques, we estimate several specifications of a multinomial probit model to examine higher education attainment of individuals in *Juntos* households and their *non-Juntos* counterparts. We find statistical significance that *Juntos* increases the likelihood of attaining technical education but only for men and not for women. *Juntos*, however, has no statistically significant effect on attaining university studies. We also find that the program helps offset the potential negative effects of having a less educated mother when it comes to attaining technical studies. Additionally, we consider other individual and household characteristics to shed light on whether *Juntos* has heterogeneous effects on higher education attainment influenced by specific social markers. Finally, based on two placebo tests we verify that our results are not driven by a spurious correlation.

The paper is organized as follows: in Section 2 we describe the *Juntos* program within the broader context of social protection programs and highlight the links between program conditionality and intergenerational poverty alleviation and provide an overview of the *Juntos* program. Section 3 briefly reviews the literature on CCTs and educational attainment with a focus on the Peruvian context. Section 4 describes the data and presents the descriptive findings and in Section 5, we explain the identification strategy and econometric models. We conclude by highlighting the main results and provide a brief discussion of the policy implications.

2. Social protection programs: the conditionality mechanism and life course CCT interventions

Across Latin America, anti-poverty social protection programs spread rapidly since the mid-1990s resulting from structural adjustment policies (Barrientos et al., 2008; Barrientos 2013; Barrientos and Santibáñez, 2009; Ham and Michelson, 2018). Structural shocks associated with public sector cuts and market liberalization demanded the recalibration of the role of the state through the extension of a broad range of state-led poverty alleviation interventions including universal as well as targeted programs aimed at vulnerable and at-risk populations, especially women and children. The transformative potential of government interventions, however, largely depends on scope and coverage as well as the nature of individual claimable entitlements or whether they are universal or means-tested and targeted. While universal and residual approaches to poverty alleviation need not be mutually exclusive, they have different implications for the individual's life course as well as meeting longer-term government poverty alleviation goals. From a life course perspective, it is recognized that for social protection programs to be effective, an integrated approach that considers support across the life course is necessary (UNICEF, 2019). As such, the conditionality mechanism associated with social protection programs including CCTs have important implications on the susceptibility to risk and vulnerability as well as the potential for intergenerational social mobility especially for women and girls during key transitions in their life course.

When analyzing CCT programs, gender analysts point out that the mechanism of conditionality places additional burdens on poor women during the re/productive life course phase since they alone are held responsible for meeting program requirements related to health, nutrition,¹ and the education of their offspring (Cookson, 2016; Razavi, 2012; Staab, 2012). From this perspective, gender analysts argue that means-tested and conditional government interventions such as CCTs are 'residual' programs that may have limited transformative potential

¹ The health and nutritional aspects of the program are beyond the scope of this study, but these include periodic health and nutritional requirements (i.e., pre- and post-natal controls, vaccinations, growth development controls for children up to 5 years of age, etc), while meeting the program's educational conditionalities requires comparatively prolonged effort over time.

(Devereux and Sabates-Wheeler, 2004), and instead exacerbate gender inequality.² From an education perspective, however, studies focusing on the life course of children have shown that conditionalities have had a positive effect on children's primary and/or secondary school enrollment (Baird et al., 2011; De Brauw and Hoddinott, 2011), and that in some cases conditionalities have had positive effects on girls in particular. For instance, Baird et al. (2011) find that in Malawi, CCTs decreased dropout rates and improved reading performance among school-aged girls compared to unconditional programs. For Peru and Kenya Jones and Samuels (2015) find that CCT conditionalities lowered absenteeism and dropping out of school, particularly for girls.

Therefore, what we highlight here is that program conditionalities may have dissimilar effects depending on where individuals are in their life course (i.e., re/productive, childhood and youth) and that these interventions have different implications on the short and longer-term outcomes on risk and vulnerability and poverty alleviation. And, while the study presented here does not focus on the impacts of CCT conditionality on women as mothers and caregivers, we acknowledge that the life course interventions associated with this form of social protection relies on the re/productive life course and gendered roles of poor women to effect children's educational attainment. Hence, *Juntos* presents an opportunity to examine the nexus of government educational interventions, higher education attainment and gender within the context of intergenerational change and poverty alleviation strategies.

As other CCT government programs across Latin America, *Juntos* aims to reduce poverty by providing income transfers to poor households according to geographic location to break the intergenerational poverty cycle by building human capital through education and health improvements (PCM, 2010; MIDIS, 2019). In 2015, women accounted for around 95% of *Juntos* recipients (MIDIS, 2015), and in their traditional gender roles as caregivers they are not only de facto held responsible for meeting program conditionalities but are considered accountable for its success (*Juntos en cifras*, 2014).³ According to *Juntos* policymakers, the program has a differential impact on women by encouraging their financial inclusion, household decision-making, increasing their access to educational and health services and strengthening and valuing their role within the home and the community (ibid., pp. 51–52). From this perspective, cash transfers increase women's bargaining power within the home which may allow them to assign more resources to their children's education.

Geographically, *Juntos* originally covered 70 districts in the southern highlands, the poorest region of Peru and by the end of 2016, it had expanded to 1224 (or 65%) districts in the country, with approximately 46% poor households participating in the program nationwide (Info-JUNTOS, 2020). The bulk of beneficiary households are concentrated in the highlands and the northern and southern jungle regions where poverty rates are higher and where the provision of quality education tends to be more heterogeneous than in more affluent and urban areas (Leon and Valdivia, 2015). There's a less pronounced presence of *Juntos* in the coastal region since it has a lower poverty rate (See Fig. 1).

This spatial differentiation in the spread of the *Juntos* program reflects government directives that beneficiaries live in the poorest districts of Peru as identified in the poverty map by the National Institute of

² The nexus between CCT program conditionality and gender inequality is related to program design and administration (Molyneux, Jones and Samuels, 2016), which places women in traditional gender roles as mothers and caregivers and de facto enforcers of not only the daily practices that ensure program compliance but also implementers of government poverty alleviation goals (Molyneux, 2006).

³ Studies of *Juntos* show that program conditionalities require women to regularly obtain health check-ups and related services for themselves and their children and to ensure that children are enrolled and attend school. However, women often face challenges in accessing these services and facilities, especially if they live in remote areas (Cookson, 2019).

Statistics and Informatics (INEI). Within districts, households are selected through the national household targeting system by the Ministry of Development and Social Inclusion (MIDIS 2014). The household criteria is then verified by community level representatives (PCM, 2010). Targeted households must have at least one child under 14 years old or a pregnant woman living in poverty (MEF, 2017; MIDIS, 2019; PCM, 2010).

Since the inception of the program in 2005, women continue to receive a cash transfer of S/100 per month equivalent to 22% and 12% of the minimum wage in 2005 and 2016, respectively (PCM, 2010; MIDIS, 2019), so long as they fulfill program conditionalities. In the area of education, children between 6 and 14 years old must be enrolled in school and have an 85% attendance rate minimum. Meeting *Juntos'* conditionalities is verified by program officers and the Ministry of Education every three months. If women are found to be non-compliant for the first time, the cash transfer is suspended for three months and if they are found to not comply a second time the income transfer is terminated and cannot be reinstated.⁴

3. CCT programs and education attainment

CCT programs like *Juntos* have become a commonly used policy intervention across the world. And while they aim to increase primary and secondary school enrollment and attendance among the poor and extreme poor by providing conditional income transfers (Baird, McIntosh, and Özler, 2011; Behrman et al., 2011; García et al., 2019; Sánchez et al., 2020; Millán et al., 2020), they do so in a variety of ways depending on program design and implementation. Thus, it is important to consider that CCT program design and implementation is largely contingent and heterogeneous, and this may partially explain outcome variation when it comes to the processes, target populations and life course transitions involved in educational attainment.

In relation to CCTs educational outcomes and gender, studies show mixed results. Pertaining enrolment, Schultz (2004) finds that the impacts of PROGRESA in Mexico are larger for rural children in secondary school in comparison to those in primary school and more concentrated among girls. Similarly, for Brazil de Brauw et al. (2015) find that the CCT Bolsa Familia increases school enrollment and grade progression especially for girls and has no impact on boys 6–17 years old. On the other hand, Cahyadi et al. (2020) and Ham and Michelson (2018) find that CCTs programs in Indonesia (Program Keluarga Harapan – PKH) and in Honduras (Programa de Asignación Familiar – PRAF), respectively increase high school enrollment for both girls and boys. For Ecuador, Schady et al. (2008) show that the CCT Bono de Desarrollo Humano also increases school enrollment for girls and boys between 6 and 17 years old without difference. Cahyadi et al. (2020), however, find that PKH increases the probability to complete secondary school only for 18–21-year old young men.

Behrman et al. (2011) also find that beneficiaries of PROGRESA are more likely to complete more grades of school, but the impacts are larger for men than women between 9 and 12 years of age and the contrary occurs for those who are between 13 and 15 years old. For attendance, Dammert (2009) finds that the CCT Red de Protección Social in Nicaragua has larger positive impacts for 7–13 year old boys compared to girls whereas for Indonesia Cahyadi et al. (2020) find that a CCT increases attendance for girls and boys. The impacts of CCTs for dropouts are also unclear, De Brauw et al. (2015) find that Bolsa Familia in Brazil has no effect for girls but only for boys whereas Mo et al. (2013) based on a CCT experiment in China find that the decrease on school dropout is

⁴ Studies show that even when women manage to meet official program conditionalities, they face a range of 'shadow' or unofficial requirements that they must comply with if they want their households to remain in the program (Cookson, 2018). Thus, women endeavor to meet both official and unofficial program conditionalities (ibid., 2019).

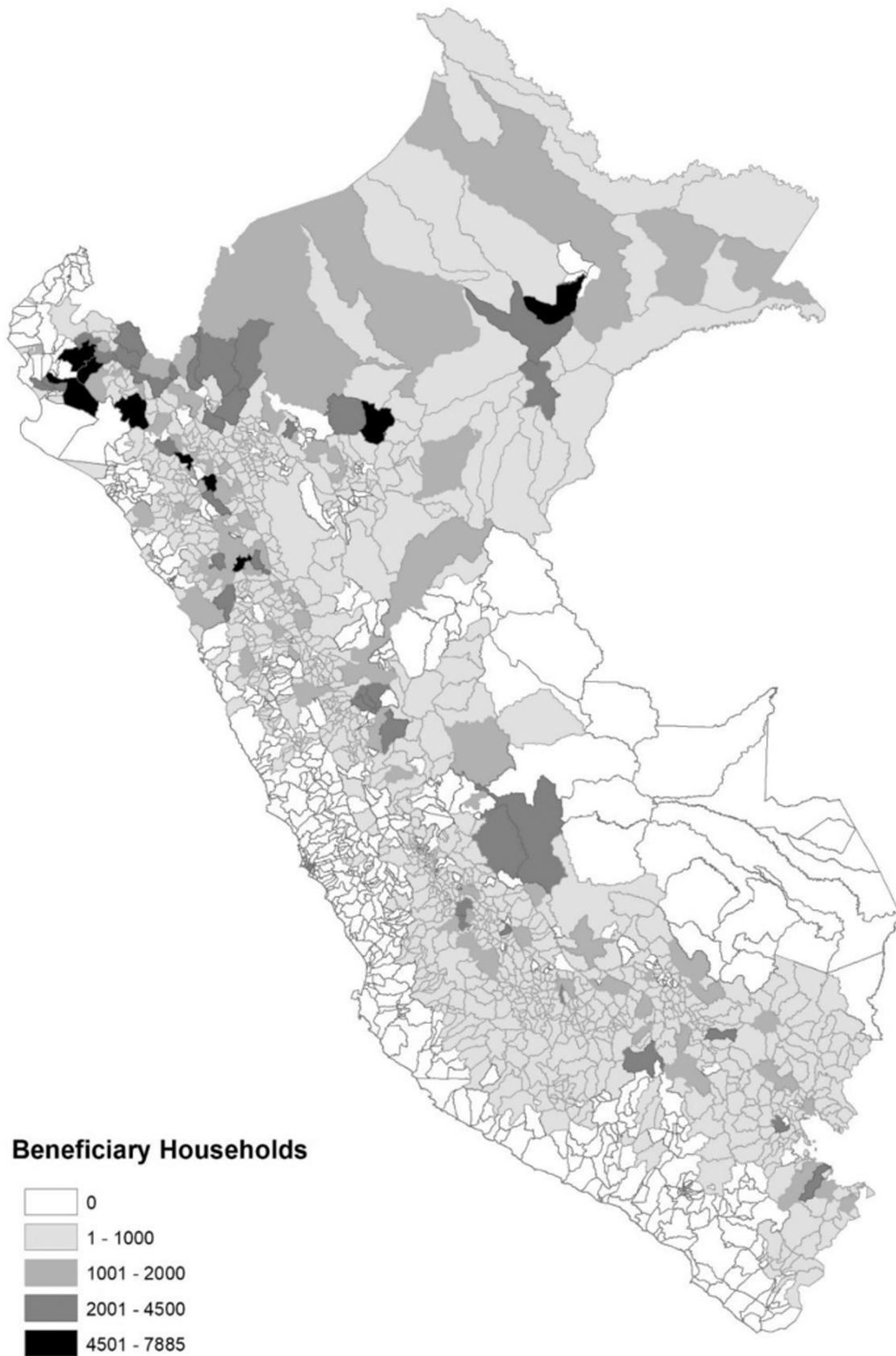


Fig. 1. *Juntos* program coverage, 2016.
Source: Base de datos distrital. InfoJUNTOS (n.d.). Own elaboration.

larger for girls than boys. Therefore, these studies suggest that CCTs have gendered effects to consider, especially if CCTs are to remain a preferred targeted life course policy intervention with an emphasis on poverty alleviation through educational attainment.

In Peru, there are also mixed findings related to the effect of *Juntos* on educational attainment at the primary and secondary school levels with few studies considering gender. Regarding attendance and enrollment, Perova and Vakis (2012) find that after 5 years *Juntos* has no effect on school enrollment and yet the program increases school attendance. Gaentzch (2018) finds that *Juntos* increases the probability of primary school enrollment, years of schooling and has a positive effect on the transition to secondary school for children over 12. A qualitative and comparative analysis of various CCTs on attendance and child wellbeing also shows that *Juntos* had a positive impact on attendance on primary and secondary school children particularly for girls (Jones and Samuels, 2015). These authors suggest that the cash transfer might relieve pressures on girls to have to be absent from school due to expectations that they engage in paid employment and domestic care work.⁵

In relation to test score achievement, some studies find that *Juntos* has no effect on the vocabulary test scores nor on the school grade achieved for children among 7 and 8 years old, regardless of gender (Andersen et al., 2015). Other studies find that *Juntos* improves PPVT scores only for children aged 4 or younger but not for those between 5 and 8 years old (Sánchez, Meléndez, and Behrman, 2020). Alternatively, Gaentzch (2018) finds that *Juntos* has a negative effect on math scores for those aged between 14 and 15, but that there is a ‘catching up’ effect whereby *Juntos* participants level up with their *non-Juntos* peers age-for-grade. While the catching up effect might reflect attendance conditionalities (Sánchez, Meléndez, and Behrman, 2020), it might also have to do with the educational aspirations of children and parents. Focusing on the nexus between CCTs and educational aspirations, studies on Mexico’s PROGRESA (Chiapa et al., 2012) and Colombia’s Familias en Acción (García, Harker, and Cuartas, 2019) show that exposure to these programs has a positive impact on parental aspirations for higher education attainment of their offspring regardless of whether they are boys or girls, and that children’s aspirations to attain higher education are also raised.

Few studies have examined to what extent CCT recipient aspirations for higher education are materialized or what the longer-term effects of the CCTs programs might be on technical or university studies attainment by gender. A related study by Millán et. al (2020) estimate the intent-to-treat effect of a CCT program in Honduras (PRAF) and find that men and women who received it are more likely to start university studies. This study differs from ours in that it does not separate university from technical studies and does not use panel data to observe higher education attainment of program beneficiaries. Instead, Millán et. al (2020) conduct a cohort analysis and use municipality information to identify potential beneficiaries. Separating university from technical studies is important not only because this might reflect issues related to quality of education and cognitive preparedness but also signal a potential range of labor market opportunities available to program participants. Another similar study on the effects on higher education by Barrera-Osorio et al. (2019) analyzes a city-based CCT program in Colombia focusing on the relationship between higher education enrollment and cash transfer payment structures. The study separates higher education institutions between university and vocational schools and find that CCT secondary school recipients are more likely to enroll in higher education, but this effect is triggered by CCT payment structure. While our focus is not on payment structures, our study differs from

those cited above in relation to geographic scope (all of Peru), demographic characteristics (poor households in the poorest Peruvian districts), and in that our study accounts for the effects of CCT government educational intervention on gender.

4. Data and descriptive findings

We use survey data from the Young Lives Study (YLS) that collects information for two cohorts of children in Peru, Ethiopia, India and Vietnam. For our analysis, we use data from the older cohort in Peru that follows children (YL child hereafter) who were between 7 and 8 years of age in 2002 (round 1). Four additional rounds were undertaken in 2006 (round 2), 2009 (round 3), 2013 (round 4), and 2016 (round 5). YL children from the older cohort during the last round were aged around 21 and 22 years and this allows us to observe their higher education attainment at R5 after graduating from secondary school.⁶

Although we use R3 to identify YL children who live in *Juntos* recipient households,⁷ information from R1 is used as the baseline and R5 as the follow-up.⁸ The complete dataset includes 587 children out of which 93 children live in *Juntos* recipient households (16%) and 494 live in *non-Juntos* recipient households (84%). There is an attrition rate of 13.4% during the period analysed 2002 (R1) – 2016 (R5), but this rate is similar for both *Juntos* (13.9%) and *non-Juntos* (13.3%) samples. There are some differences in baseline characteristics (R1) between YL children who were interviewed at R5 from those who were not interviewed at R5⁹ but their differences were not significant across *Juntos* and *non-Juntos* recipient households with exception of the characteristic ‘presence of preschool in the community’ (see Online Appendix Table A1). This suggests that attrition is not likely to lead to significant biases.¹⁰

For descriptive purposes, we present differences between *Juntos* and *non-Juntos* households using the complete dataset and we use the *Juntos* sample to examine differences in higher education attainment between men and women. For the econometric analysis, we use matching methods to select a comparable group to *Juntos* households. Table 1 shows that there are some statistically significant differences in educational attainment used to construct our variable of interest: higher education. Our variable of interest, higher education, is defined as: (1) no higher education, (2) technical studies which includes incomplete and complete studies, and (3) university studies which includes incomplete and complete studies. YL children in *Juntos* households show a lower education attainment in comparison to those in *non-Juntos* households.

There is a higher percentage of children in *Juntos* households with less than higher education (56%) in comparison to their *non-Juntos* counterparts (46%). For technical studies, a higher percentage of YL children in *Juntos* households have incomplete studies (19%) in comparison to those in *non-Juntos* households (14%); and a higher

⁶ During R5, children from the younger cohort are still attending high school, and we cannot observe their post-secondary educational attainment.

⁷ Information on the *Juntos* program in the YLS dataset was available in R3 onward.

⁸ R1 is undertaken in 2002, three years before the *Juntos* program started. We do not use R2 since it is undertaken in 2006, one year after the *Juntos* program started.

⁹ There are three differences in the *Juntos* sample: mother’s education, number of parents, and presence of preschool in the community. In the case of *non-Juntos* sample, there are five differences: age turned in first year at school, number of household members who are 65 years or over, household size, wealth index, and residing in the mountain region.

¹⁰ In *Juntos* households there is a higher proportion of children who live in households located in communities with preschools that were interviewed at R5 compared to those who were not interviewed, whereas this difference is not observed in *non-Juntos* households. Thus, if there is any systematic difference due to attrition, children in *Juntos* households have a slightly greater opportunity to attend preschool which can favor their educational trajectory; however, we do not observe that difference for primary and secondary schooling.

⁵ Another finding is that the income transfer from *Juntos* ameliorated the employment out migration of fathers giving them the opportunity to be more involved in their children’s education, and this may have a possible knock-on effect on children’s school performance and wellbeing (Jones and Samuels, 2015).

Table 1
Educational attainment, by *Juntos* and *Non-Juntos* households.

Educational attainment at R5	All Sample			Juntos recipient households			Non-Juntos recipient households			z-test ^{a/}
	Obs.	Mean	Std. Dev.	(A)			(B)			
				Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	(B)- (A)
Less than post-secondary and not attending	252	0.429	0.495	48	0.516	0.502	204	0.413	0.493	-1.84 *
Less than post-secondary but attending	27	0.046	0.210	4	0.043	0.204	23	0.047	0.211	0.15
Incomplete technical and not attending	40	0.068	0.252	9	0.097	0.297	31	0.063	0.243	-1.19
Incomplete technical but attending	45	0.077	0.266	9	0.097	0.297	36	0.073	0.260	-0.79
Complete technical	56	0.095	0.294	5	0.054	0.227	51	0.103	0.305	1.49
Incomplete university and not attending	17	0.029	0.168	5	0.054	0.227	12	0.024	0.154	-1.55
Incomplete university but attending	137	0.233	0.423	12	0.129	0.337	125	0.253	0.435	2.59 * **
Complete university	13	0.022	0.147	1	0.011	0.104	12	0.024	0.154	0.81
Total	587			93			494			

Note. a/ The values displayed for z-tests are the differences in the mean across the groups of households. * ** p < 0.01, * * p < 0.05, * p < 0.10.

percentage of individuals in *non-Juntos* households completed technical studies (10%) in comparison to those in *Juntos* households (5%). Finally, a higher percentage of individuals in *non-Juntos* families obtained a university degree (2%) or have incomplete university studies but are still attending university (25%) in comparison to YL children in *Juntos* households (1% and 13%, respectively). In addition, a higher percentage of YL children in *Juntos* households dropped out of university (5%) in comparison to those in *non-Juntos* households (2%). This indicates that *Juntos* participants are lagging behind in higher education attainment compared to their *non-Juntos* counterparts.

A higher percentage of individuals in *non-Juntos* households has university studies in comparison to individuals in *Juntos* households, regardless of gender (see Fig. 2). Nevertheless, for technical studies, there are gender differences between these two types of households with a higher percentage of men in *Juntos* households (35%) with technical education compared to 15% of women and 21% of men in *non-Juntos* households with technical education as opposed to 27% of women. For those without higher education, there is a lower percentage of men in *Juntos* households (46%) who did not have higher education compared to 66% of women and 50% of men and 41% of women in *non-Juntos* households.

For deeper insights, we use *Juntos* household and community characteristics including mother’s education, the presence of parents and siblings, and access to secondary schools on higher education attainment and found some important gender-based differences. For mother’s education, our descriptive findings indicate that for YL children who lived in *Juntos* beneficiary households with mothers without higher education, 48% of men did not attain higher education compared to 66% of women. With respect to the presence of parents, YL children (men and women) who lived with both parents attained university studies (18% in both cases), but 33% of men attained technical studies compared to 18% of women. Similarly, for YL children who lived without both parents, 33% of men attained university studies as opposed to 29% of women. For those who lived without both parents, 17% of men did not attain higher education as opposed to 71% of women. As for the presence of

siblings, we find that for those who lived without older siblings, 29% of men attained university studies as opposed to 36% of women and for those who lived with younger siblings, 45% of men did not attain higher education as opposed to 54% of women. For those who lived in a community without a secondary school, 29% of men did not attain higher education compared to 69% of women. Finally, for those in the lowest wealth quintile, 45% of men did not attain higher education compared to 65% of women.

The differences in education attainment can be driven by a selection problem since *Juntos* and *non-Juntos* households could differ according to individual or household characteristics whereby individuals participating in *Juntos* are also those who attain less education. To address this problem, we use a matching approach to find an adequate control group (i.e., *non-Juntos* households) to compare individual higher education attainment to that of an individual in the treatment group (i.e., *Juntos* households).

5. Identification strategy

To avoid a biased estimation of *Juntos* on higher education attainment due to systematic differences between beneficiary (i.e., *Juntos*) and non-beneficiary (i.e., *non-Juntos*) households, we use matching techniques to construct a *non-Juntos* sample comparable to our *Juntos* sample conditional on observed characteristics, which allow us to some extent estimate *Juntos*’ unbiased impacts (Black and Smith, 2004; Caliendo and Kopeinig, 2008). Since our variable of interest, higher education attainment is measured only at R5, we cannot use a difference-in-difference approach to control for unobserved characteristics. We include, however, several specifications to contrast the results obtained for *Juntos*’ estimates. In addition, we use a Rosenbaum bounding approach to test the sensitivity of our propensity-matched results due to unobserved factors (Becker and Caliendo, 2007).

Eq. (1) presents the basic model to estimate the influence of *Juntos* program on higher education attainment as the dependent variable, using the matched sample:

$$Y_{iR5} = \beta_0 + \beta_1' Juntos_i + \beta_2' X_i + z_g + e_{ig} \tag{1}$$

Y_{iR5} represents higher education attainment for YL child i at R5 that has three categories: $Y_{iR5} = 0$ if the YL child does not attain higher education, $Y_{iR5} = 1$ if the YL child attends technical studies, and $Y_{iR5} = 2$ if the YL child attends university studies. Given the nature of the dependent variable, we use a multinomial probit model to estimate Eq. (1). *Juntos* is the independent variable of interest that equals 1 if the YL child i lives in a *Juntos* household, and 0 otherwise. The coefficient of interest is β_1 , which estimates the influence of *Juntos* on YL child’s higher education attainment. A statistically significant estimate for β_1 implies that *Juntos* program is relevant to explain higher education attainment, a positive estimate suggests that *Juntos* increases the probability to attain higher education.

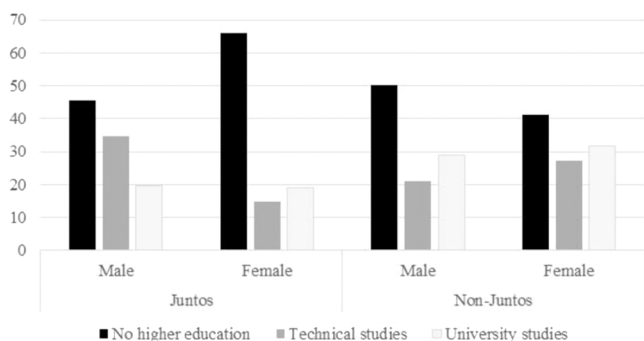


Fig. 2. Higher education by sex and *Juntos* program participation.

To improve the estimator's efficiency, we add a vector X_i that includes YL child, mother, household, and community control variables at the baseline.¹¹ For individual characteristics, we include gender, type of school that equals 1 if the YL child attended a private school at least one year, age turned in first year at school, attendance to preschool, and the Raven's Colored Progressive Matrices (Raven) test as a proxy for ability. The Raven test is administered at R1 when the individual aged between 7 and 8 years and measures abstract reasoning ability. This is a visual test where an individual must complete a matrix of figures. Thus, the Raven score is not influenced by formal education at school or university unlike standardized tests for mathematics and reading (Pasquier-Doumer and Brandon, 2015). Mother characteristics include mother's age and education that equals 1 if the mother has secondary studies or higher education.¹² Household characteristics include the number of family members with higher education, the number of younger and older siblings, the number of family members who are over 64 years old, the household size and the wealth index. Community variables include the presence of higher education institutions and the presence of secondary schools to control for potential differences driven by community development that can facilitate the pursue of tertiary education. In addition, we include a vector z_g of geographical region fixed effects to control for factors at the region level that could influence participation in *Juntos* and higher education attainment. Finally, we cluster the standard errors at the community level.

To assess the robustness of the estimated effects, additional estimations were performed using the matched sample. First, to observe potential heterogeneous effects of *Juntos*, we estimate Eq. (1) with interaction terms between *Juntos* and the following characteristics: gender, type of school, having an own child, education of mother, presence of both parents, presence of younger and older siblings, and wealth index quintile. Thus, we can identify whether *Juntos* has a different effect based on those characteristics. Second, Eq. (1) is estimated by adding a vector of standardized tests at R2 that includes the Peabody Picture of Vocabulary (PPV) test and a Math Achievement (MATH) test.¹³ Finally, we implement two placebo tests using education level from the oldest sibling (including half-siblings) and the mother at R1 (2002) as placebo outcomes. The education level at R1 should not be affected by the *Juntos* program since the program started in 2005, three years after R1 was undertaken. Therefore, we must find a non-statistically significant effect of *Juntos* on these placebo outcomes. Since 57% of the oldest siblings and 47% of mothers at R1 have less than secondary education, the placebo outcome equals 1 if they attain secondary or tertiary education and 0 if they attain less than secondary education. The placebo tests re-estimate Eq. (1) by using a probit model and a matched sample with the placebo outcome as the dependent variable. For the matched sample, we also estimate the probability to participate in *Juntos*, but the outcome is now the education level of the oldest sibling and mother. For the mothers, we do not use their education as a control variable since it is now the outcome variable.

¹¹ We mostly used information from R1, but to construct the variable 'type of school' (private or public) and 'number of own children' we used information from R4. At that round, YL children should have completed their school education which allows us to ascertain the type of school they attended, and by the age in R4 they are more likely to have their own children. To maximize power, we imputed missing values in R1 with information from other rounds. More specifically, for mother's age we used 5 observations from R2, 2 observations from R3, and 1 observation from R5; and for mother's education we used 5 observations from R2, 1 observation from R3, and 1 observation from R5. Estimates do not change due to the imputation (see Table 3).

¹² We do not include fathers' characteristics due to the high number of non-responses ($n = 143$) representing 21% of the final sample. In addition, 94% of mothers reported to be their children's primary caregiver during their children's school years.

¹³ These standardized tests were not taken at the baseline in R1.

6. Results

Before presenting the main results for *Juntos* program, we first need to determine if the matching procedure has served to construct comparable samples between control and treatment groups. The probit model used to estimate the propensity score is showed in the Online Appendix (Table B1).¹⁴ We also present the characteristics of *Juntos* and *non-Juntos* households according to each matching technique used: Kernel matching (Table B2), 5-nearest neighbours matching (Table B3), and radius matching (Table B4). We find that there are only three significant differences in observable characteristics between *Juntos* and *non-Juntos* matched samples using Kernel matching, and there are no significant differences using 5-nearest neighbours or radius matching techniques. In the case of the Kernel matched sample, a higher proportion of YL children in the *non-Juntos* sample lives in households with a higher wealth index, in communities with more health facilities and in the Coast in comparison to YL children in the *Juntos* sample. This means that YL children in *non-Juntos* households have a slightly better economic situation and live in communities with more infrastructure; therefore, we would have a lower bound estimate for *Juntos* as these three variables are positively correlated with educational attainment. In addition, the mean and median bias has decreased for all the matching techniques used. These results suggest that *Juntos* and *non-Juntos* households based on matching techniques are comparable.

Turning to the main results, Table 2 presents the effect of *Juntos* on higher education attainment using the matched sample based on the Kernel matching technique.¹⁵ This is the only table that shows the results for all the control variables included in the analysis. In this table, we present the results for the three categories of our dependent variable explained in the 'Identification strategy' section, and all the control variables in the vector X_i with region fixed effects.

Our main results presented in Table 2 show that there exists a positive relationship between *Juntos* and higher education attainment. Particularly, the program is associated with a statistically significant 10.2 percentage points decrease in the likelihood that individuals attain less than higher education, and a statistically significant 8.5 percentage points increase in the likelihood to attain technical studies. The *Juntos* program, however, is not statistically significant to explain university studies. Similar results are found for technical studies if we use samples based on alternative matching techniques and a sample without imputation but has no significant effect on the probability to obtain less than higher education if we use 5-nearest neighbour or radius matching as shown in Table 3 (Panel I). The estimates, however, are no longer significant if we use a sample without matching; this suggests that the effect of *Juntos* is masked if a not comparable sample is used.

Note that these results are robust to the inclusion of standardized tests controlvariables (see Table 3 – Panel II). Thus, our findings support that *Juntos* program increases higher education attainment. All the alternative specifications used show that YL children who live in *Juntos*

¹⁴ To clarify, *Juntos* has three household eligibility rules. The first rule states that for households to be eligible, they must be located in districts identified by the INEI as below the poverty line. Once prospective program participants have met this requirement, households are selected by the MIDIS through the national household targeting system. The third and final step is for households to be verified by community leaders. To account for how this eligibility criteria may affect the PSM estimates, we include characteristics at the community level, such as: number of banks, number of health facilities, presence of preschool, presence of primary school and presence of secondary school. We use presence rather than number of preschools, primary and secondary schools since in those communities there is only one of each. In addition, the standard errors are clustered at the community level. We also include other variables specifically related to children: 'age turned in first year at school' and 'attend preschool' to have a finer control of observable characteristics.

¹⁵ We present the results using matched samples based on 5-nearest neighbours and radius matching techniques in Table 3.

Table 2
Average marginal effects: Multinomial probit estimation for higher education attainment.

	No higher education	Technical studies	University studies
Young Live Child			
Live in a <i>Juntos</i> receiving-household (= 1)	-0.102 * (0.058)	0.085 * (0.048)	0.017 (0.046)
Male (= 1)	-0.031 (0.074)	0.060 (0.054)	-0.029 (0.037)
Age turned in first year at school	0.037 (0.061)	0.047 (0.069)	-0.084 * * (0.038)
Attend preschool (= 1)	-0.024 (0.067)	0.180 * * * (0.038)	-0.156 * * * (0.059)
Private school at least one year (= 1)	-0.046 (0.093)	0.126 (0.097)	-0.081 * (0.048)
Number of own children	0.419 * * * (0.084)	-0.114 (0.079)	-0.304 * * * (0.074)
Ravens test	-0.010 * * * (0.002)	0.004 (0.003)	0.006 * * * (0.002)
Mother			
Age of mother	-0.001 (0.005)	0.001 (0.004)	-0.000 (0.003)
Mother has secondary or tertiary education (= 1)	-0.128 (0.095)	0.106 (0.088)	0.021 (0.067)
Household			
Number of parents	-0.118 (0.086)	0.040 (0.061)	0.078 (0.070)
Number of members with post-secondary	-0.089 * * (0.045)	0.034 (0.047)	0.056 * * (0.027)
Number of younger siblings	0.060 (0.044)	-0.040 (0.033)	-0.020 (0.035)
Number of older siblings	0.014 (0.035)	0.014 (0.027)	-0.028 (0.026)
Number of members 65 or over	0.090 (0.074)	-0.111 (0.068)	0.022 (0.058)
Household size	0.005 (0.021)	0.008 (0.014)	-0.013 (0.018)
Wealth index	-0.037 (0.213)	-0.497 * * * (0.157)	0.535 * * * (0.173)
Community			
Has higher education institutions (= 1)	0.052 (0.063)	0.065 (0.050)	-0.117 * * * (0.039)
Has secondary schools (= 1)	0.048 (0.069)	0.009 (0.053)	-0.057 (0.052)
Region fixed effects	Yes	Yes	Yes
Wald Chi2 (p-value)	1867.28 (0.000)		
Observations	587		

Note. Standard errors, in parentheses, are clustered at the community level. * * * p < 0.01, * * p < 0.05, * p < 0.10.

recipient household are more likely to attain technical studies.

6.1. Heterogeneous effects

Table 4 summarizes the results of *Juntos* using the Kernel matched sample after we include interaction terms in Eq. (1) to evaluate potential heterogeneous effects.¹⁶ We use differences in the average marginal effects to statistically compare the effect of *Juntos* by the characteristics evaluated.

Our findings show that *Juntos* is not statistically significant to explain higher education attainment for women but for men, regardless the matching technique used. Men who lived in *Juntos* households are more likely to attain technical studies and less likely to obtain less than higher education compared to women. Moreover, for technical studies, the effect of *Juntos* is larger for men than women if we use the Kernel and 5-nearest neighbours matched but not for the radius matched samples. This suggests that men benefit more than women from participating in

¹⁶ In the Online Appendix, we present the results for heterogeneous effects using 5-nearest neighbours and radius matching techniques (see Tables C1 and C2, respectively).

the *Juntos* program. This difference may reflect the hurdles women face during their life course like becoming teen mothers which may deter their educational progression.

For technical studies, results in Table 4 show that the effect of *Juntos* is larger among children in public schools. For YL children who attended a public school, *Juntos* is associated with a 10.8% points higher probability to attain technical studies while it has no effect on children who attended private schools. Results using 5-nearest neighbours and radius matched samples (see Online Table C1 and C2, respectively) show that for technical studies, *Juntos* benefits children in public schools more than those in private schools, whereas for university studies, the effect of *Juntos* is larger among children in private schools. Thus, for YL children who attended a private school, *Juntos* decreases the likelihood to attain technical studies but increases the likelihood to attain university studies. We can argue that despite low quality education in public schools, the results suggest that *Juntos* offsets the potential negative effects of attending a public school related to technical studies and using the 5-nearest neighbours and radius matched samples results suggest that *Juntos* is associated with positive effects of private school related to attending university. Our results also show that for university studies the effect of *Juntos* is larger for children with less educated mothers if we use the Kernel or radius matched sample. YL children with less educated mothers who lived in a *Juntos* beneficiary household are more likely to attain university studies. These results suggest that *Juntos* helps to offset the potential negative effects of having a less educated mother. As such, it is possible that in situations where children attend a public school and have a mother without higher education, these limitations may be overcome by participating in CCT programs (Baird, McInthosh, and Özler, 2011; Dammert, 2009; Garcia and Saavedra, 2017).¹⁷

Regarding the presence of younger and older siblings, we find that for technical studies the effect of *Juntos* is only larger among those children with younger siblings, regardless of the matching technique used. For those who lived with younger siblings, *Juntos* is associated with a higher probability to attain technical studies. Additionally, we find that the effect of *Juntos* does not vary by the presence of both parents or household wealth quintile regardless of the matching technique used. Likewise, our results show that there is no difference in the effect of *Juntos* whether or not the YL participant is now a parent.

6.2. Placebo test

Table 5 shows the results for the placebo outcomes estimated. Each of the placebo tests finds no statistically significant effect of *Juntos* on the educational level of the oldest sibling or mother, regardless of the matching technique used. In contrast to our main results, the oldest sibling who would later belong to a *Juntos* recipient household did not exhibit a higher education level than those in *non-Juntos* recipient households. Nor was there a significant effect on education level for mothers who later belong to *Juntos* recipient households. Thus, the placebo tests fail to find evidence that our main results are driven by a spurious correlation.

Although, our results are consistent regardless the specification estimated, there still exists the possibility that unobserved factors influence selection into the treatment group and produce biased estimators of *Juntos* on higher education attainment. Therefore, we show in the

¹⁷ Nevertheless, for individuals whose characteristics reflect multiple disadvantages and within the context of limited universal social protection, *Juntos* may provide a narrow pathway to higher education attainment, but this does not preclude policymakers from relying on broader educational policies to address issues of poverty and inequality.

Table 3
Average marginal effects of *Juntos* program using alternative samples.

	No higher education		Technical studies		University studies		Obs.
	Ave. Marg. Effect	Std. Err.	Ave. Marg. Effect	Std. Err.	Ave. Marg. Effect	Std. Err.	
Participation in Juntos							
Panel I							
No imputation ^{a/}	-0.106 **	(0.058)	0.091 *	(0.047)	0.015	(0.047)	580
5-nearest neighbours matching	-0.104	(0.076)	0.106 *	(0.059)	-0.002	(0.048)	204
Radius matching	-0.081	(0.066)	0.102 *	(0.053)	-0.021	(0.050)	552
No matching	-0.093	(0.060)	0.074	(0.059)	0.018	(0.054)	587
Panel II - Include standardized tests							
No imputation ^{a/}	-0.181 ***	(0.053)	0.126 ***	(0.047)	0.055	(0.048)	563
Kernel matching	-0.176 ***	(0.052)	0.120 **	(0.047)	0.055	(0.048)	570
5-nearest neighbours matching	-0.182 **	(0.070)	0.141 **	(0.059)	0.042	(0.050)	199
Radius matching	-0.165 ***	(0.062)	0.144 ***	(0.053)	0.021	(0.052)	536
No matching	-0.173 ***	(0.051)	0.090	(0.061)	0.083	(0.062)	570

Note. a/ The sample is constructed using Kernel matching technique. Standard errors are clustered at the community level. Results for each sample are estimated using a multinomial probit. All estimations include the same control variables used in Table 2. *** p < 0.01, ** p < 0.05, * p < 0.10.

Table 4
Heterogeneous effects of *Juntos* using Kernel matched sample.

	No higher education		Technical studies		University studies		Obs.
	Ave. Marg. Effect	Std. Err.	Ave. Marg. Effect	Std. Err.	Ave. Marg. Effect	Std. Err.	
By gender							
Female	-0.011	(0.090)	0.006	(0.074)	0.004	(0.079)	587
Male	-0.184 **	(0.072)	0.155 ***	(0.056)	0.029	(0.046)	
Difference	-0.173	(0.109)	0.149 *	(0.090)	0.025	(0.085)	
By type of school							
Public	-0.114 *	(0.060)	0.108 **	(0.051)	0.007	(0.047)	587
Private	0.037	(0.248)	-0.231	(0.145)	0.194	(0.141)	
Difference	0.151	(0.250)	-0.339 **	(0.150)	0.187	(0.145)	
Subgroups by having a child							
No kids	-0.121 *	(0.063)	0.098 *	(0.050)	0.023	(0.054)	587
At least one kid	0.018	(0.096)	0.007	(0.094)	-0.024 *	(0.014)	
Difference	0.139	(0.104)	-0.091	(0.097)	-0.047	(0.058)	
Subgroups by mother with secondary or higher education							
No	-0.111	(0.070)	0.041	(0.066)	0.070	(0.047)	587
Yes	-0.115	(0.134)	0.220 **	(0.100)	-0.105	(0.078)	
Difference	-0.004	(0.155)	0.179	(0.138)	-0.175 **	(0.079)	
Subgroups by presence of both parents							
No	-0.213 *	(0.127)	0.115	(0.109)	0.098	(0.088)	587
Yes	-0.085	(0.060)	0.081	(0.052)	0.004	(0.049)	
Difference	0.128	(0.131)	-0.034	(0.122)	-0.094	(0.092)	
Subgroups by presence of younger siblings							
No	-0.090	(0.107)	-0.021	(0.076)	0.111	(0.114)	587
Yes	-0.107	(0.081)	0.127 **	(0.052)	-0.020	(0.054)	
Difference	-0.017	(0.147)	0.148 *	(0.085)	-0.131	(0.132)	
Subgroups by presence of older siblings							
No	-0.209 **	(0.090)	0.114	(0.071)	0.095	(0.072)	587
Yes	-0.058	(0.073)	0.080	(0.072)	-0.021	(0.060)	
Difference	0.151	(0.117)	-0.034	(0.117)	-0.116	(0.098)	
Subgroups by wealth index quintile							
Lowest	-0.118	(0.072)	0.090	(0.071)	0.028	(0.049)	587
Highest	-0.037	(0.103)	0.058	(0.053)	-0.021	(0.090)	
Difference	0.081	(0.123)	-0.032	(0.087)	-0.049	(0.097)	

Note. Standard errors are clustered at the community level. Results for each subgroup are estimated using a multinomial probit. All estimations include the same control variables used in Table 2. *** p < 0.01, ** p < 0.05, * p < 0.10.

Online Appendix (Table D1) the Rosenbaum bounds to test the sensitivity of our results to unobserved factors.¹⁸ Positive selection bias would occur if children in *Juntos* beneficiary households are more likely to attain higher education even if they do not belong to the *Juntos* program. Results are robust against unobserved selection bias; up to $\Gamma = 3$ results show that there are no hidden biases due to unobserved factors ($Q^+ = 6.885, p = 0.000$). Thus, our results indicate that the estimates of

Juntos are not sensitive to potential bias from unobserved factors.

7. Conclusions and policy implications

The past few decades have been marked by enduring poverty and increasing inequality within and across world regions. As a response developing countries have adopted or expanded social protection programs to meet the specific challenges of increased poverty and vulnerability arising at particular points of the life course of individuals. CCTs have become a predominant short-term and targeted policy intervention providing income transfers to women during their re/productive life course to incentivize the use of health and nutritional services and to encourage school enrollment and attendance early in the life course of

¹⁸ To calculate Rosenbaum bounds, we use the dichotomous outcome variable that equals 1 if the individual attains higher education since this test does not allow using more than two categories for the outcome variable (Becker and Caliendo, 2007).

Table 5
Average marginal effects of placebo tests.

	Secondary or tertiary education at R1	
	Oldest sibling (1)	Mother (2)
Belongs to Juntos		
Kernel matching	-0.043 (0.061) [370]	-0.024 (0.040) [587]
5-nearest neighbours matching	-0.029 (0.060) [150]	-0.018 (0.043) [200]
Radius matching	0.006 (0.050) [336]	-0.027 (0.039) [545]
Oldest sibling' characteristics ^{a/}	Yes	No
Mother's characteristics ^{b/}	Yes	Yes
Young Live child characteristics	Yes	Yes
Household characteristics	Yes	Yes
Community characteristics	Yes	Yes
Region fixed effects	Yes	Yes

Note. a/ Oldest sibling' characteristics include age and sex. b/ Mother's characteristics do not include education for the estimation of column (2). Standard errors, in parentheses, are clustered at the community level. Columns (1) and (2) are estimated using a probit model. Number of observations are in brackets. All estimations include the control variables in Table 2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

children. While a variety of studies show that some gains have been made with respect to school enrollment and others point to mixed results related to attendance and cognitive achievement (see Fiszbein and Schady, 2009; Glewwe and Muralidharan, 2016), there is limited evidence regarding the impact of CCT programs on higher education attainment and by gender. One reason for this limitation is the lack of data following CCT recipients over time making it difficult to analyse CCT effects later in the life course of program recipients. From this perspective, our study contributes to the literature by examining a large-scale CCT program and its effects on higher education attainment by considering gender and differentiating technical from university studies.

Our key finding shows that *Juntos* is not statistically significant to explain higher education attainment for women but for men. This suggests that while *Juntos* has a positive effect on higher education attainment, a gap in higher education by gender remains. For policy makers, this finding poses a challenge for poverty alleviation strategies and gender-based intergenerational transformation given that women and girls are disproportionately affected by poverty, risk and vulnerability across their life course. From this perspective, for poverty alleviation goals to be realized, there is a need to consider the gender higher education gap and to examine in what ways program design and implementation must change to prevent girls who participate in the *Juntos* program from growing up to become recipients like their mothers. These findings present an opportunity for *Juntos* policy makers to consider increasing and extending income transfers specifically for girls to encourage not only primary and secondary schooling but higher education attainment. In this manner, the *Juntos* program could be modified to counteract gender inequalities later in the life course of girls by providing early incentives and tailored pathways for them to attain higher education.

When we separate university from technical studies, our results show that the program increases the probability to attain technical studies regardless of the model specification whereas the effect on university studies is not statistically significant. Unlike universities, technical institutions do not require an entry exam that measure individual knowledge which may explain why *Juntos* only increases technical studies. This result is consistent with *Juntos*' negative effect or no effect on cognitive tests (Andersen et al., 2015; Gaentzsch, 2018; Sánchez

et al., 2020), which may affect progress toward university studies. Similarly, although there is evidence that CCT programs increase enrolment and attendance to comply with transfers conditionalities (see Behrman et al., 2011; Cahyadi et al., 2020; Dammert, 2009; among others), educational quality may not be sufficient to allow beneficiaries to successfully pass the entry exam to university. From a structural perspective and within the context of Peru, our results may reflect a lack of access to quality primary and secondary education among poorer and marginalized populations (Arteaga and Glewwe, 2019; Leon and Valdivia, 2015). In addition, this might also reflect economic constraints whereby at the time *Juntos* recipients reach higher education and they are no longer receiving the cash transfer, they are unable to afford high university costs and instead opt for technical studies since this is a comparatively less expensive and more flexible alternative.

We also find heterogeneity in the effects of *Juntos* on higher education. Compared to their *non-Juntos* counterparts, our findings show the program has a positive effect on higher education, and this was significant for those individuals who attended public school or had a less educated mother. Our results also show that the presence of younger siblings can explain the influence of *Juntos* on higher education when compared to *non-Juntos* households. For those who live with younger siblings, *Juntos* increases the probability to attain technical studies. These results suggest that when considering their *non-Juntos* counterparts, *Juntos* may facilitate the attainment of higher education for disadvantaged individuals in relation to their non-disadvantaged counterparts, which resonates with empirical findings from other CCTs programs (Ham and Michelson, 2018; Ham, 2014).

When we examine *Juntos* household and individual characteristics, our descriptive findings show gender-based differences whereby men living with or without both parents with older or younger siblings or in the lowest wealth quintile benefit more when it comes to higher education attainment than women. Together, these findings point to the need for policy makers to consider not only the structural barriers for higher education attainment but also account for household composition as well as individual and community characteristics and gender norms and practices. From a gender perspective, the tension between the structural constraints poor households face coupled with gender norms and practices that place girls at a disadvantage highlights the need to consider gender-sensitive policy interventions across the life course of women and girls to narrow the gender gap in higher education. Therefore, for CCTs to potentially have a transformative effect, a more integrative and universal approach to poverty alleviation (Devereux and Sabates-Wheeler, 2004), across individual's life course is required.

Although we recognize that our analysis is based on a relatively small sample, our results have shown consistency among the different model specifications estimated. Equally important is that we use YLS data that follows individuals for a period of 14 years (2002–2016), which allows us to make a unique contribution by observing the long-term effects of *Juntos* after the individual exits the program. Future research in this area would require policy makers and program administrators to track the educational pathways of program recipients well beyond the duration of the program and by gender to gain deeper insights into the interaction between individual and social change within the context of CCT anti-poverty interventions.

CCT conditionalities do not necessarily guarantee better learning outcomes or narrow achievement gaps among marginalized populations at the intersection of class and ethnicity (Arteaga and Glewwe, 2019; Cueto et al., 2009; García, Harker, and Cuartas, 2019; Reimers et al., 2006). This is also the case for gender and the gender higher education attainment gap. Therefore, broader social and educational needs and inequalities must be addressed. From this perspective, even small gains in the educational trajectories of poor children as well as higher education attainment by *Juntos* men beneficiaries should not be diminished. However, it is important to emphasize that short-term CCT programs do not constitute an adequate alternative for long-term and integrated national poverty alleviation goals. Instead, social, and educational

policies and structures that provide poor and marginalized populations a sustained way to leverage intersectional inequalities aiming upend the poverty cycle must be considered.

Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at [doi:10.1016/j.ijedudev.2022.102612](https://doi.org/10.1016/j.ijedudev.2022.102612).

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