Early and Middle Childhood Circumstances and Child Development: Exploring the Role of Time Poverty and Conditional Cash Transfer Programmes in Peru

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Declaration

I, Rolando Leiva-Granados, confirm that the work presented in my thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Abstract

The production of human capital is the subject of a growing body of literature that aims to understand how human capital accumulates from early childhood. Previous studies have identified facilitators and obstacles to human capital accumulation (Almond et al., 2018). However, many aspects of this process remain under explored. These include the identification of causal relationships between different inputs and child development; and the mechanisms through which inputs and public interventions affect child development. The primary objective of this thesis is to provide insights into how the circumstances and experiences of children in Peru, shape their human capital development.

I first study the effect of child time poverty, defined as insufficient discretionary time due to commitments such as work and household chores (Kalenkoski et al., 2011), on child development. Children experiencing time poverty may be unable to allocate adequate time to cognition-enhancing activities, impacting their cognitive development. Existing research has concentrated on adult time poverty, and little is known about the possible effect of child time poverty on children's human capital.

I then study the impact of a conditional cash transfer (CCT) programme on nutritional and cognitive outcomes over the short- and long-term. I also explore whether the programme's impact on nutrition mediates its effects on cognitive outcomes.

My results suggest a positive impact of time poverty on girls' verbal skills, a negative effect on girl's mathematical skills and a positive effect on boy's verbal skills. Moreover, the CCT programme had some negative effects on test scores and mixed effects on nutrition, both on the short- and long-terms. Additionally, in the short-term, some of the programme's negative impact on cognition were mediated by its negative effect on BMI. The impact of the CCT programme was most pronounced in girls and in children who were exposed at earlier ages.

Impact statement

The principal objective of this doctoral thesis is to examine the impact of time poverty and a CCT programme on child development in Peru. My results show disparities in time allocation and the prevalence of time poverty by gender. Moreover, I document gender-specific effects of time poverty across different measures of cognition. Additionally, the CCT programme exhibited gender- and cohort-specific impacts on child cognition and nutrition, including some negative effects among girls and children exposed at earlier ages.

Based on these results, I have highlighted potential avenues for academic research as well as implications for policy. Thus, my PhD thesis seeks to influence future academic research and inform the formulation of public policies and CCT programmes aimed at enhancing child cognition and nutrition and tackling gender disparities that emerge from early childhood.

In terms of academic insights, this thesis underscores the significance of studying children's time poverty. Research on time poverty typically focuses on adults, and the effects of time poverty on child cognition remains an underexplored domain. To the best of my knowledge, this thesis represents the first attempt to study this topic. Further research on children's time poverty and time allocation should be conducted to better understand its effects in settings with different cultural, economic and demographic characteristics. As discussed in the main body of this document, I emphasise the necessity for methodological advancements in measuring children's time allocation and time poverty. These advancements include the development of a more precise definition of what constitutes discretionary time for children and enhancing the tools used for its measurement.

These advancements should be incorporated into future cohort and longitudinal studies to assess the impact of children’s time poverty more accurately on a broader spectrum of human capital outcomes. Moreover, better time use data could encourage other researchers to study the relationship between time poverty and child development. I intend to continue working on these methodological advancements in my forthcoming academic work.

My findings have several implications for policymaking. As discussed before, I have documented gender disparities in time allocation and gender-specific effects of a CCT programme, with girls often experiencing more negative impacts on cognitive and nutritional outcomes than boys. This highlights the importance of complementing CCT programmes with policies focused on addressing potential implicit gender biases, improving the quality of education delivery and enhancing the human capital of parents. Such measures would ensure that children, regardless of their gender, receive improved support and investments from both the educational system and their parents.

Regarding dissemination, preliminary findings from this thesis have already been presented at three international academic conferences. My goal is to continue sharing my thesis results with both academic and non-academic audiences, including policymakers. This will be achieved through publication in peer-reviewed journals, conference presentations, student seminars, lectures, workshops and collaborations with non-governmental organisations, think thanks and advocacy groups.

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1. Introduction and rationale

1.1. Background

The production of human capital is the subject of a growing body of literature (Almond et al., 2018; Attanasio et al., 2022; Cunha & Heckman, 2008; Todd & Wolpin, 2007). It is a dynamic, lifelong process that starts at the prenatal stage (Almond et al., 2018; Currie, 2020). Early childhood is considered an extremely important period for human capital accumulation. Shocks in this period have been associated with negative effects on adults' cognitive, health and labour market outcomes (Almond et al., 2018; Duncan et al., 2010). Children's circumstances in early life, such as household resources, parental investments and health shocks, may affect the production of human capital at school-age and in adolescence, which are in turn two productive periods for child development (Attanasio, 2015).

Existing literature identifies key facilitators and barriers to human capital accumulation. Income poverty, child labour, natural disasters and health and economic shocks, have been identified as important barriers for child development (Alam, 2015; Almond et al., 2018; Apouey & Geoffard, 2013; Dickerson & Popli, 2016; Dooley & Stewart, 2007). Parental resources, parental investments child endowments at birth, and the quality of school, childcare and health care, are inputs that foster human capital production and accumulation in the short- and long-run (Attanasio, 2015; Attanasio, Meghir, et al., 2020; Bernal & Fernández, 2013; Brunello et al., 2016; Todd & Wolpin, 2007).

However, given that the production and accumulation of human capital is complex, it is not surprising that there are aspects of this process that are yet to be explored. These include the correct identification of causal relationships between different inputs and child development; how these different inputs interact, including their complementarity or substitutability at different points of time; and the mechanisms and channels through which inputs and public interventions affect child development (Almond et al., 2018; Attanasio, 2015; Heckman et al., 2013). An element often missed in much of the literature is the role of time as an input and as a meditating factor for other inputs in the production of human capital (Du & Yagihashi, 2017a, 2017b).

1.2. The role of time in human capital production

Time has been a key element in theoretical models of health production and human capital investment models since the 1960s (Becker, 1965; Ben-Porath, 1967; Grossman, 1972). However, contrary to the broad existing empirical research on the effects of quality and/or quantity of education, health investments and parental income on child development (Akee et al., 2018; Almond et al., 2018; Attanasio et al., 2017; Brunello et al., 2016; Currie, 2020), the effects of severe time limitations experienced by children, often called *time poverty,* on children's human capital has, to the best of my knowledge, not yet been studied.

Time poverty is defined as not having enough discretionary time because most time is devoted to committed activities such as paid and unpaid work, school and household chores (Kalenkoski et al., 2011; Merz & Rathjen, 2014). Most of the existing literature focuses on adults' time poverty and on their health-related outcomes. For instance, a review of 16 studies by Giurge et al. (2020) found that time poverty is associated with lower individual's well-being and poorer health outcomes in high income countries. In Australia, feeling "rushed or time pressed" has been associated with unhealthy dietary habits and poorer self-reported and mental health (Strazdins et al., 2016; Venn & Strazdins, 2017). In Canada, time poverty was found as an important barrier to participation in physical activities (Spinney & Millward, 2010).

Recent literature has examined the effect of parental and child time investments on human capital production (Attanasio, Cattan, et al., 2020; Bono et al., 2016; Del Boca et al., 2017). These results suggest that the effects of these investments are highly context dependent. For instance, Del Boca et al. (2014), using data from the United States, found that parental time investments may be more productive than monetary investments in the process of child development. Using the same data, Del Boca et al (2017) found that during adolescence children's own time investments were more productive than those made along with their parents. Using UK data, Bono et al. (2016) found that maternal time investments were crucial for the development of cognitive skills. Finally, Attanasio, Catan et al. (2020) studied the effect of an RCT in Colombia on parental material- and time-investments. Although the intervention increased quality time investments, it did not result in a positive impact on children's development. It is important to note that these studies focused on the productivity of the actual time spent on the aforementioned activities, and not on time poverty defined as severe time constraints in discretionary time.

This is particularly relevant in the context of low- and middle-income countries (LMICs) such as Peru, the country studied in this thesis. Owing to a lack of childcare programmes; a higher dependence on agricultural livelihoods in which children are relatively productive; and larger family sizes, children in LMICs often contribute to household tasks and care activities (Putnick & Bornstein, 2016), which may significantly reduce their discretionary time and thus their potential for investing time in human capital development. This dependence on child labour could be more pronounced in poorer households, suggesting a correlation between income poverty and child time poverty. Further, due to patriarchal gender roles still predominant in many LMICs, girls are usually expected to engage in more housework than boys (Putnick & Bornstein, 2016), suggesting also the potential existence of gender gradients in child time poverty.

1.3. The role of conditional cash transfer programmes for child development

Conditional cash transfer (CCT) programmes are social assistance initiatives implemented in many countries to improve children's outcomes in the short and long run. These programmes condition cash transfers on school attendance, health check-ups, or vaccinations, with the aim of reducing present poverty through the cash transfers and addressing future poverty by investing in children's health capital through the conditionalities (Bastagli et al., 2016; Millan et al., 2019). One such programme is Juntos[1], a large CCT implemented in Peru since 2005 (Sanchez et al., 2020).

Three previous studies have assessed the short-term impact of the Juntos programme on the cognitive and nutritional status of children aged 5 to 8 years old during the initial

[1] *Juntos* (together in Spanish) is a conditional cash transfer (CCT) program targeting households in the most disadvantaged municipalities in Peru. The program transfers a bimonthly fixed amount of 200 Peruvian Soles (60 US dollars) to the households meeting the conditionalities associated to the program. These conditionalities target children and pregnant women. Pregnant women must attend prenatal health check-ups. Children below 5 years must attend health care centres for vaccination and grow-up checks. School-age children must attend at least 85% of the school classes and must have a national ID card (Díaz & Saldarriaga, 2019; Sanchez et al., 2020).

phase of programme expansion (Andersen et al., 2015; Gaentzsch, 2020; Sanchez et al., 2020). These papers found heterogeneous effects of the programme on children's nutritional outcomes, with both positive and negative effects depending on the specific outcome and subgroup studied (for instance, girls versus boys). However, they have also consistently found that the programme does not lead to improvements in children's cognitive development; in some cases, negative effects on cognition have been documented (Gaentzsch, 2020).

In the Peruvian case, the non-positive results of the programme on cognitive outcomes contrast with existing evidence on child development. It is generally expected that improvements in nutrition should be associated with improvements in cognition (Alderman & Fernald, 2017; Andersen et al., 2015). However, these previous evaluations have treated nutrition and cognition outcomes as independent variables, and they have not formally tested the potential impact of nutrition on cognition outcomes due to programme participation.

1.4. The goal of this research project

This thesis aims to investigate the impact of childhood circumstances on child development, with a particular focus on children's time use, child time poverty and nutrition in the context of CCT programmes in Peru. I will use data from the Young Lives (YL) study for Peru, a longitudinal study conducted by the University of Oxford in Peru, Ethiopia, Vietnam and India (Barnett et al., 2013).

This thesis will consist of three analytical chapters, where I will:

Explore the effect of time poverty on child development across difference age groups, ranging from school-aged children to adolescents (Chapter 5). In this chapter, I aim to investigate the previously unexplored dimension of the effects of severe time constraints on children's cognition in both the short- and long-term.

Revisit the short-term effects of the Juntos programme on child nutrition and cognition (Chapter 6). In contrast to previous studies, I hypothesise that even though the programme may not have a statistically significant *total* effect on cognition, as found in previous evaluations, it may have an *indirect effect* on cognition through its effects on child nutrition. To explore this indirect effect, I will use causal mediation analysis, combined with difference-in-differences (DID) methods.

Extend the previous short-term evaluations of the Juntos programme (covering ages between 5 and 8 years) to the long-term (encompassing ages between 5 and 15 years) (Chapter 7). This extended evaluation will also include two additional cohorts of children who were treated after the first expansion of the Juntos programme, a dimension not previously explored in those assessments of the programme's impact on cognitive and nutritional outcomes. I will also employ the recently developed Callaway and Sant'Anna's DID estimator (Callaway and Sant'Anna, 2021), which is designed to overcome potential limitations associated with the traditional Two-Ways Fixed Effects (TWFE) model when estimating treatment effects in the context of staggered adoption of an intervention.

The rest of this thesis is structured as follows: Chapter 2 presents a comprehensive review of the literature, while Chapter 3 outlines the objectives and aims of the study. In Chapter 4, I introduce a conceptual and methodological framework, alongside an overview of the Young Lives study. Chapter 5 explores the impact of time poverty on cognitive development, with Chapter 6 addressing the short-term effects of the Juntos programme on child nutrition and cognition, including the mediation analysis previously described. Chapter 7 investigates the long-term consequences of the Juntos programme for children's human capital. Finally, in Chapter 8, I provide a summary of the thesis, engage in a discussion of the overall findings, and propose potential directions for future research.

2. Literature review

In this section, I present a literature review that summarises the known determinants of child development, how discretionary time may impact child development and the known effects of CCTs programmes on human capital.

2.1. Parental material investments.

The role of parental investments, such as expenditure on clothing, food, medicines and education, has been studied in high and low- and middle-income countries (LMICs) (Attanasio et al., 2022; Helmers & Patnam, 2011; Todd & Wolpin, 2007). A study by Tood and Wolpin (2007) found that home inputs such as number of books or access to musical instruments, accounted for a significant proportion of the gaps in math and reading test scores between White students and those of Black- and Hispanic- minorities in the United States. Both Helmers and Patnam (2011) and Attanasio et al. (2020) found that parental investments such as expenditures in books and stationery, clothing, shoes and uniforms had a positive effect on the cognitive development of children at all ages in India. However, Attanasio et al. (2020) highlighted that those investments had a greater effect on children at younger ages.

Existing evidence has also shown that household resources affect child development. Parental income is positively correlated with child development and health in both High and LMICs (Akee et al., 2018; Almond et al., 2018; Apouey & Geoffard, 2013; Aughinbaugh & Gittleman, 2003). For instance, in the UK, Dickerson and Popli (2016) found that children born into poverty had lower cognitive test scores at ages 3, 5 and 7 years than their peers. In Canada, children experiencing early poverty had greater odds of not being ready for school at the ages of 5 and 7 years (Roos et al., 2019).

2.2. Parental and children's time investments.

In addition to household resources and material investments, the time parents and children spend engaging in activities such as playing, educational tasks such as reading and homework, and other similar interactions, can also impact child development. In the United Kingdom, Bono et al (2016) found that early maternal time investments were an important determinant of long-term cognitive outcomes. Gialamas et al. (2020), in Australia, showed that the time parents spent engaging in educational activities with their children at the age of 2 and 3 years improved vocabulary and behavioural skills at school entry. Del Boca et al. (2014) found that material investments were less productive than time investments for children's cognitive skills in the United States. It is worth noting, however, that this outcome has not been replicated in similar studies conducted in LMICs. For instance, Attanasio, Cattan et al. (2020) explored whether material and time investments served as mechanisms through which an RCT in Colombia impacted child cognition. While the intervention increased both types of investment, they found that only material investments led to improvements in child cognition.

Nicoletti et al. (2020), using Norwegian data, studied whether a reduction in maternal time inputs due to an increase in labour supply affected the development of children with mothers who worked during preschool years. Even though they found a direct negative effect of mother's working hours on test scores in adolescence, this effect was fully compensated by the rise in income. Moreover, Del Boca et al. (2017) studied the effect of both parental and children's own time investments in the United States. They found that in adolescence, children's own investments were more productive than mother's investments.

Considering the importance of both material and time investments in child development, it is essential to examine the obstacles that hinder these investments. In the preceding sub-section, I provided a brief overview of the adverse effects of income poverty on children. In the following section, I will discuss how time poverty can act as a barrier to time investments.

2.3. Time use and time poverty

Time is a scarce resource that individuals allocate between several activities such as paid and non-paid work, leisure, and other committed and discretionary activities (Kalenkoski et al., 2011). Time has gained recognition as a key component of a person’s well-being (Giurge et al., 2020; Seymour et al., 2017) and as a key input to human capital production as already described above.

An important concept in the study of time allocation is time poverty, first introduced by Vickery (1977) as an additional dimension in the study of poverty in the United States. Time poverty occurs when individuals have severe time constraints, leaving them little discretionary time to engage in activities that improve their own well-being (Kalenkoski et al., 2011). In terms of human welfare, time poverty may be as important as income poverty given its potential negative effects on subjective well-being, physical and mental health, and human relationships (Giurge et al., 2020). Yet, it has received little attention from policy makers (Giurge et al., 2020).

Different definitions of time poverty have been used in empirical research (Williams et al., 2016). One common way to identify an individual as “time-poor” is by considering the amount of discretionary time that they have relative to the population’s discretionary time (Kalenkoski et al., 2011). “Discretionary time” is the remaining time a person has after subtracting the time allocated to “necessary” activities (such as sleeping or personal care) and to “committed” activities (such as time for work or childcare). Thus, a person would be considered time-poor if they have less than 60% of the median of the population’s discretionary time. This definition is based on the income poverty line, which is estimated as 60% of the median household income (Eurostats, 2021; Merz & Rathjen, 2014). The 50% and 70% thresholds are also commonly used as robustness checks for time poverty. This definition of time poverty has been used by Kalenkoski et al (2011) and by Kalenkoski and Hamrick (2013) in the United States, by Seymour et al. (2019) in Bangladesh and by Merz and Rathjen (2014) in Germany. It is important to note that this time poverty is a relative measure of discretionary time constraints.

The definition of committed and discretionary activities in the literature depends on the quality and granularity of time use data available. For instance, Kalenkoski et al (2011) and Kalenkoski and Hamrick (2013), using data from the American Time Use Survey, divided time into 3 main activities: *personal care* (including sleeping and grooming); *committed activities* (such as housework, caring and helping household members and work related activities); and *discretionary activities*, which are a large set of activities such as leisure (sports, exercise, relaxing, eat, watching television, etc), time for education, volunteering, eating and drinking, and personal care services (such as doctor's appointment).

Most research on time allocation and time poverty to date analyses time use patterns among adults, frequently exploring gender inequalities in time use. Owing to a double burden of market work and household chores, it would be expected that women suffer more time poverty than men. This pattern has been confirmed by empirical research carried out in LMICs such as Mozambique (Arora, 2015), Guatemala (Gammage, 2010), Ghana (Orkoh et al., 2020) and Guinea (Bardasi & Wodon, 2010). Similar results have been found in high income countries including the United Kingdom (Chatzitheochari & Arber, 2012). One exception was found by Strazdins et al. (2016) in Australia, where the likelihood of time poverty was not statistically different between men and women. However, they found that women had higher odds (63% more likely) of reporting feeling rushed or pressed for time.

While the relationship between time poverty and gender is fairly well understood, the association between time poverty and income has been less well studied. There is empirical evidence correlating time poverty with income in Germany (Merz & Rathjen, 2014) and Ghana (Zacharias et al., 2018). However, research in the United States (Kalenkoski et al., 2011), Canada (Spinney & Millward, 2010), or among women in Mozambique (Arora, 2015) did not find significant correlations between time poverty and income. Other empirical evidence supports the hypothesis of a trade-off between income and time poverty (Orkoh et al., 2020), but with important features depending on the country studied: in Guatemala, women from the lowest two income quintiles were more likely to experience time poverty, while the opposite was true for men (Gammage, 2010). In the United Kingdom, individuals working in high-level high-income positions were more likely to be time poor on weekdays, while they compensated on weekends. However, women were at higher risk of time poverty in both cases (Chatzitheochari & Arber, 2012). This remains an area where greater study is required.

However, to the best of my knowledge, determinants of child time poverty and the effect of time poverty on children's outcomes has not yet been studied in any context**.** My thesis aims to fill these gaps in the existing knowledge base.

2.4. Discretionary time activities that foster human capital among children.

As previously discussed in Section 2.2, prior research has examined the significance of parental and child time investments in child development. Nevertheless, some of this literature does not distinguish between activities conducted within discretionary or committed time, as outlined in Section 2.3. Therefore, in this sub-section, I discuss the relevance of time investments that may fall under the “discretionary activities” categorisation, including playing and other leisure pursuits.

The importance of play for cognitive development has been highlighted in many pieces of research. For instance, according to Ginsburg (2007), play promotes children’s healthy brain development, cognitive skills and emotional strength. Gialamas et al. (2020) found that time spent playing during the early childhood period (between 2 and 5 years old) was associated with improved vocabulary and behavioural outcomes at school entry in Australia. Active leisure (such as physical and outdoor activities) was found to improve math tests performance among children and adolescents in the United States (Laidley & Conley, 2018).

Other pieces of research have found that “cultivated” activities such as reading, visits to museums, or high-arts participation were positively associated with math test scores and grade point averages among children and adolescents in the United States (Gaddis, 2013; Jæger, 2011).

Given the various causal links between cognition, socioemotional skills, health, and nutrition (Attanasio, 2015), play and leisure activities can also influence cognition through their impact on these variables. For instance, participation in activities outside school, such as sports, organisations and clubs, has been associated with greater social competences (Howie et al., 2010). Undirected playing also helps to develop non-cognitive abilities such as negotiation and conflict resolution skills (Ginsburg, 2007).

Moreover, active playing and leisure activities have been associated with better health and nutritional outcomes, such as lower BMI and obesity risk (McCurdy et al., 2010), and better mental health (Vella et al., 2017). Furthermore, the amount of time available for lunch was associated with improvements in dietary intake among children in primary and secondary schools in the United States (Cohen et al., 2016).

Therefore, the significance of play and leisure activities conducted during children's discretionary time emphasises the need to investigate time poverty as a possible barrier to child development.

2.5. Endogeneity of parental investments

A recurrent finding in the literature is that parental investments are endogenous, i.e., parents react to the current stocks of children's health and cognitive skills when they make investment decisions (Fan & Porter, 2020; Frijters et al., 2013; Grätz & Torche, 2016; Nicoletti & Tonei, 2020), Yet, these findings are not conclusive in terms of the direction of the reaction. For instance, Nicoletti and Tonei (2020) found that parents in Australia compensated for low cognitive skills by increasing the time that children spent in learning activities. In the United States, on the contrary, Gratz & Torche (2016) found that parents from higher socioeconomic status provided more cognitive stimulation to higher ability children. However, in Peru and Ethiopia, Attanasio et al. (2017) found that parental investments were not determined by child health or cognition.

In countries with high gender preferences, parental investments interact with the gender of children. In India, Barcellos et al. (2014) studied a sample of families with children between 0 and 15 months of age. They found that boys were breastfed longer and received more childcare and vitamins than girls, which led to a gender gap in weight and height. In the same country context, Jayachandran and Pande (2017) found the unequal allocation of parental investments increased with children's age: the first-born children received more resources than latter-born children, which led to a birth-order gradient in children's height.

The endogeneity of parental investments poses an important issue for the correct estimation of human capital production functions and the identification of the effect of parental and children's inputs.

2.6. Conditional cash transfers and child development

Conditional Cash Transfers (CCTs) are social protection programmes aimed at alleviating poverty amongst vulnerable populations. Most of these programmes have a dual objective of reducing short term poverty through cash transfers and long term poverty through human capital investment (Millan et al., 2019; Parker & Todd, 2017). To achieve these aims, they usually condition the cash transfers on meeting goals in education, healthcare, and nutrition.

The evidence of the effect of CCTs on education, healthcare utilisation and nutrition is mixed. While they seem to be an effective tool for increasing the utilisation of health preventive services (Lagarde et al., 2007), the evidence of their effects on nutritional or cognitive outcomes is heterogeneous. For instance, a meta-analysis of 21 papers by Manley et al. (2013) found a positive but not statistically significant effect of 17 CCTs on Height-for-age (HAZ) z-scores.

Furthermore, the evidence of the effect of CCTs on children's cognition is more limited. A literature review of ten CCTs found that most of the programs had positive effects on schooling, but fewer had a positive impact on cognitive outcomes (Millan et al., 2019). The review from Bastagli et al. (2016), found that across 42 studies, only five provided overall estimates of cognitive outcomes. Among those studies, three found improvements and two found non-significant effects of CCTs on child cognition.

The evidence of the effect of CCTs on children's time allocation has primarily focused on the effect on child labour. A literature review by de Hoop and Rosati (2014) found some evidence that CCTs reduced child labour. Similar results were found by Kabeer & Waddington (2015), particularly for boys. However, Attanasio et al. (2010) evaluated a large CCT programme in Colombia and found a positive effect of the programme on school enrolment but no effect on income-generating activities. As pointed out by the authors, time spent at work and at school may not be perfect substitutes, and part of the increase in time at school may be taken out of leisure time. This stresses the importance of considering the effect of CCTs on time allocation to other categories of activities besides child labour and schooling. To the best of my knowledge, there is no research on the effect of CCTs on child time poverty.

For the specific case of Peru, evaluations of the Juntos CCT programme have found evidence of a positive effect on prenatal care utilisation (Díaz & Saldarriaga, 2019), linear growth among boys (Andersen et al., 2015), school enrolment, completing primary school and transition to secondary school (Gaentzsch, 2020), children's nutritional status (Sanchez et al., 2020), and short term reductions in labour supply for mothers (Fernandez & Saldarriaga, 2014).

Despite some positive impacts of the programme on nutritional status and schooling achievements demonstrated by the literatures, the evidence on cognitive outcomes is not encouraging. This result is surprising given evidence of the positive association between nutritional improvement and cognitive development of children (Alderman & Fernald, 2017; Andersen et al., 2015). Andersen et al. (2015) found no association between programme participation and grade attainment or receptive vocabulary. Similarly, Gaentzsch (2020) found no effect on vocabulary development and a negative effect on mathematical test scores among primary and secondary children. Sanchez et al. (2020) found a positive effect on language scores, but only among children initially exposed to the programme during their first four years of life. However, this positive effect became non-significant when further robustness checks were carried out.

However, these previous evaluations have treated nutrition and cognition outcomes as independent variables, and they have not formally tested the potential impact of nutrition on cognition outcomes due to programme participation. To address this gap in the existing literature, a mediation analysis will be conducted in Chapter 6.

Moreover, it is also important to highlight that these previous studies were short-term evaluations, specifically limited to two periods (before and after the introduction of Juntos) when children were between 5 and 8 years old. Therefore, the long-term effects of the programme remain understudied and will be a focal point of Chapter 7.

2.7. Summary of findings and gaps from this literature review

From this review, the following key findings and gaps in the existing literature have been identified:

- Parental financial and material investments play a crucial role in child development during early and middle childhood, as well as throughout adolescence. Poverty and material deprivation act as significant barriers to child development through their impact on parental material investments.
- The literature exploring the role of time investments has emphasised the significance of parental and children's time investments in child development. Some of these activities, like play and active leisure, fall under the category known as discretionary time. This term refers to the time left after subtracting the time dedicated to committed activities, as detailed above. Time poverty is thus defined as a severe constraint on the amount of discretionary time available to a person.
- While *time poverty* is being increasingly studied, the focus is on determinants of adult time poverty and its effects on well-being outcomes. The determinants of *child time poverty* and its effects on human capital production have not been explored.
- Previous evaluations of the Juntos CCT programme in Peru have identified some positive effects of the programme on children's nutrition but not on cognitive outcomes. Furthermore, these evaluations have not formally identified intermediate mechanisms through which the programme may influence child cognition, including the nutritional channel. Moreover, these evaluations have been only in the short-term (for children aged between 5 and 8 years old), during the first expansion of the programme between 2005-2009. Long-term effects, especially among the cohorts of children treated after 2009, present an unexplored area for research.

This thesis will contribute to our understanding of these important elements of human capital production as described in the next chapter.

3. Aim and objectives.

This thesis aims to investigate the impact of childhood experiences and circumstances on human capital development in Peru, with a particular focus on children's time use, child time poverty and the impact of a CCT programme. Using the Young Lives longitudinal study for Peru, it will explore the following questions:

Research question 1: How do time constraints affect human capital accumulation, specifically cognitive development, among children aged between 5 and 15 years in Peru?

Research objectives:

- **a)** Estimate the rate of child time poverty in Peru.
- **b)** Examine the effect of child time poverty on cognitive development.
- **c)** Investigate whether the effect of child time poverty on cognitive development differs among children of different age groups and by gender.

Research question 2: Do the short-term effects of Juntos on child nutrition mediate its impact on child cognition? Do these intermediate effects depend on children's gender?

Research objectives:

- **a)** Determine whether the effects of the programme on child nutrition measured by stunting status, BMI-for-age z-scores and Heigh-for-age z-scores, mediate an intermediate effect of the programme on child cognition.
- **b)** Investigate whether the intermediate effects vary by gender.

Research question 3: What are the long-term effects of Juntos on child cognition and child nutrition? Do these effects vary by children’s gender?

Research objectives:

- **a)** Extend the evaluation of the impact of the Juntos programme on child nutrition and cognition to encompass the age range of 5 to 15 years old.
- **b)** Investigate whether the long-term effects of Juntos on these outcomes vary by children’s gender.

4. Conceptual and methodological framework

This chapter presents a conceptual framework for the production of children's human capital. After that, it will present a methodological framework in which I will review how such a production function can be estimated. Special emphasis will be placed on how to address the potential endogeneity of children's time use and parental investments, with children's innate unmeasured ability.

4.1. Conceptual framework for the study of time investments in the production of human capital

Following Attanasio (2015), I assume that the production of human capital in time t+1 is a dynamic process depending on the initial values of human capital $H_{i,t}$, investments in human capital $I_{i,t}$, background variables $Z_{i,t}$ (such as parental and socioeconomic variables), and a vector of random shocks $e_{i,t}$:

$$H_{i,t+1} = f(H_{i,t}, I_{i,t}, Z_{i,t}, e_{i,t}) \tag{1}$$

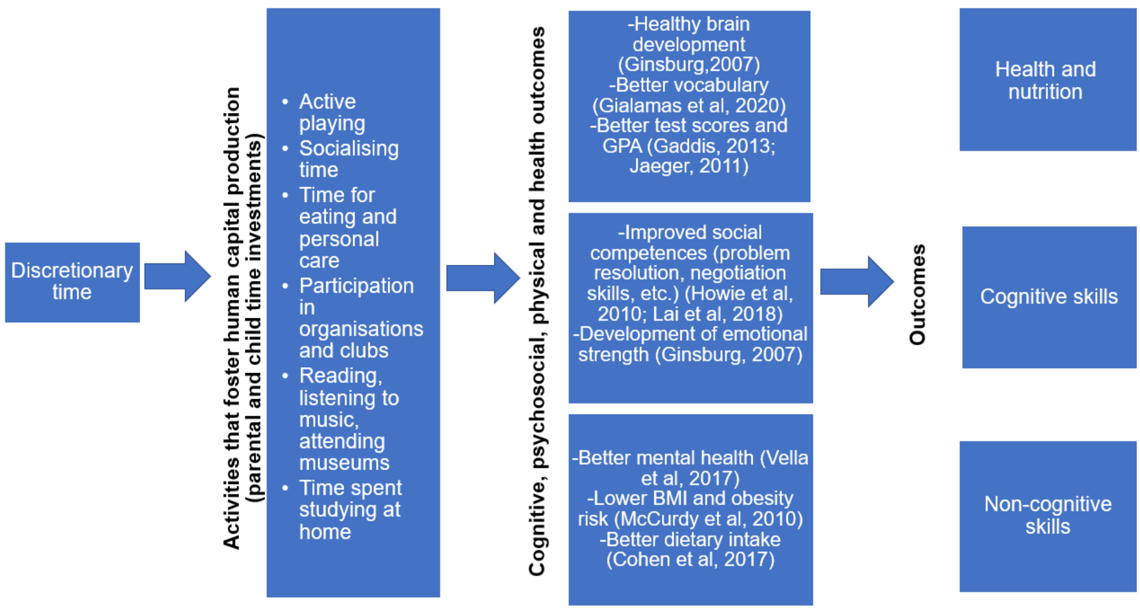
Human capital is composed of cognitive skills $\theta^c_{i,t}$, socio-emotional skills $\theta^s_{i,t}$ and health $\theta^h_{i,t}$. Investments are classified into two main categories: material investments $I^M_{i,t}$ and time investments $I^T_{i,t}$:

$$H_{i,t} = \{\theta^c_{i,t}, \theta^s_{i,t}, \theta^h_{i,t}\}$$
$$I_{i,t} = \{I^M_{i,t}, I^T_{i,t}\}$$

The focus of this thesis is the study of children's discretionary time, which is essential for time investments $I^T_{i,t}$. However, I will not focus on the productivity of discretionary time activities per se as there is already literature on how these activities impact human capital (see Section 2.4). Rather, I will consider the effect of time constraints on child cognition. Following the literature that studies income poverty as a barrier for material investments; I will study how *time poverty*, a barrier for discretionary time investments, affects children's human capital.

Figure 1 presents the conceptual map for the causal pathway from discretionary time to human capital outcomes. I list a non-exhaustive set of activities that children carry out using discretionary time (second column). As discussed in the literature review (Sections 2.2- 2.4), these activities have been found to impact some outcomes that are relevant to human capital (third column), such as brain development, academic performance, social competences and health, among others. Then, these outcomes can be classified into cognitive skills, socioemotional skills and health outcomes (fourth column). Chapter 5 of this thesis focusses mainly on the cognitive skills dimension.

Figure 1. Conceptual map for discretionary time and children's developmental outcomes



4.2. Conceptual framework for the study of the effect of conditional cash transfer programmes in the production of human capital

The mechanisms through which cash transfers influence human capital outcomes can also be examined using the investment function within this conceptual framework. A CCT programme may modify both time $I_{t,m}^{T}$ and material $I_{t,m}^{M}$ investments, but the direction of these investments and their effects on child cognition are, in principle, undetermined. Additionally, considering that outcomes like nutrition are influenced by both time and material investments, and they, in turn, may affect cognition (Alderman & Fernald, 2017; Andersen et al., 2015), nutrition can also be in the pathway from CCTs to child cognition.

Concerning ***material investments*** $\boldsymbol{I_{i,t}^{M}}$, one would anticipate that cash transfers would, in principle, have a non-negative effect on the amount of resources that families allocate to durable and non-durable goods that may enhance children's nutrition and cognition. These goods might include general food expenditures or expenditures on more specific items for children, such as books and stationery, school uniforms, medicines and tuition fees. Indeed, several studies have documented a positive impact of cash transfers on food expenditures or parental investments (Attanasio, Cattan, et al., 2020, 2020; Bastagli et al., 2016). Only a small number of evaluations have reported a reduction in food expenditures, possibly attributed to decreased labour supply and/or a preference for saving over consumption (Bastagli et al., 2016).

Cash transfers can also modify the allocation of both parental and children's time and ***time-related investments*** $\boldsymbol{I_{i,t}^{T}}$. The evidence in this area is more mixed compared to material investments. For instance, an evaluation conducted in Colombia found that a parenting intervention, implemented within the framework of a CCT programme, increased parental time investments, but this increase did not result in improvements in child cognition (Attanasio, Cattan, et al., 2020). Further, most of the evaluations found that CCTs did not increase child labour (de Hoop & Rosati, 2014). However, two recent studies found that cash transfer programmes may increase child labour as in the case of the Philippines (De Hoop et al., 2019) and among girls in Pakistan (Awaworyi Churchill et al., 2021).

The factor $\boldsymbol{Z_{i,t}}$ in the human capital equation (Equation 1) conveys various other inputs or material factors, such as parental education and school quality, that may influence the effectiveness of time and material investments in fostering child development, For instance, parental background may not only affect the type and intensity of parental investments but also the productivity of those investments (Brown, 2006). The quality of school inputs is another key factor that affects the effectiveness of time investments in child cognition (Wedel, 2021). This includes the productivity of both time spent at school and studying at home[2].

Figure 2. Conceptual map of the pathways from cash transfers to children's developmental outcomes

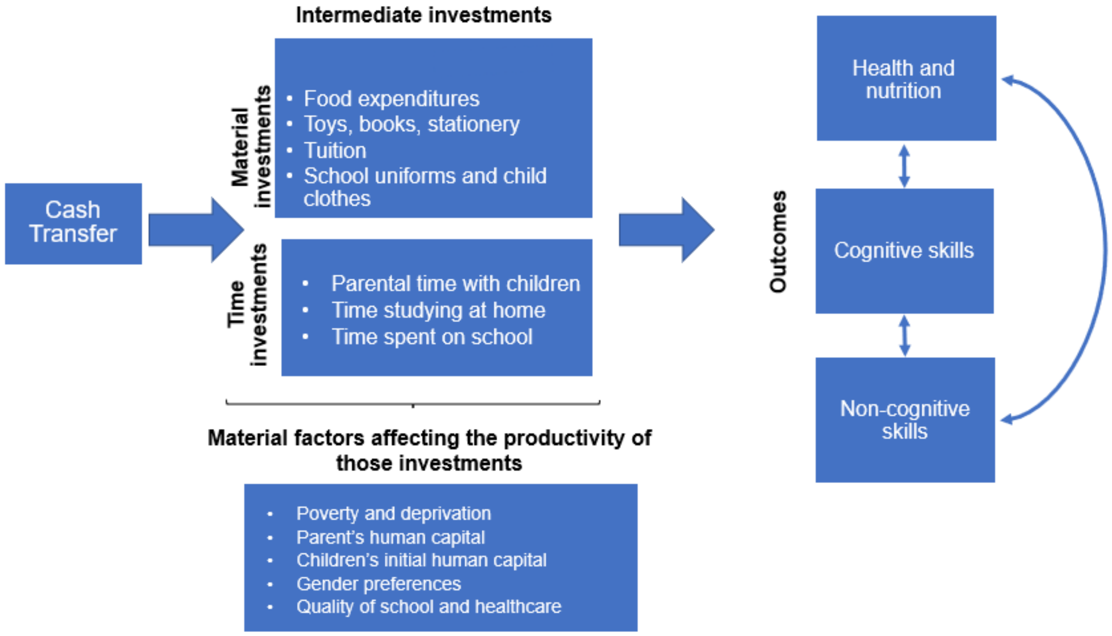


Figure 2 presents a conceptual map in which I summarise some of the causal pathways connecting CCTs to children's human capital accumulation. This conceptual map makes it more evident that a cash transfer may not directly result in improvements in children's human capital. It also influences intermediate investments, that may ultimately (indirectly) impact human capital. The productivity of those intermediate investments is affected by other material factors such as poverty and deprivation, parental human capital and quality of school and healthcare, among others.

Changes in the intermediate investments, as well as in the material factors, may require time, which can lead to different effects of the cash transfer on outcomes in the short- and long-term. This is usually referred to as a "distal process" in the context of causal mediation analysis (Shrout & Bolger, 2002), which will be explained in more detail in Section 6.2.2.

[2] The quality of the school can influence the productivity of children's time studying at home in various ways. For instance, the quality of inputs children receive at school may influence their ability for home learning.

Moreover, the three human capital outcomes shown in the conceptual map may influence each other. For example, as previously discussed, enhancements in nutrition are expected to correlate with improvements in cognition (Alderman & Fernald, 2017; Andersen et al., 2015). Consequently, nutrition can also serve as a pathway through which a CCT impacts cognition. This is the pathway that will be therefore explored in Chapter 6.

4.3. The Young Lives Study

The empirical estimation of a production function such as Equation (1) relies on the availability of suitable data. Before studying the econometric methodologies available, I will briefly introduce the dataset used for the analyses I conduct in this thesis.

The Young Lives (YL) study in Peru is part of a multi-country longitudinal study implemented by the University of Oxford in Ethiopia, Vietnam, Peru and India. Multiple published studies have used the YL data to study child development in LMICs (Attanasio et al., 2017; Attanasio, Meghir, et al., 2020; Keane et al., 2022; Sanchez et al., 2020).

The YL study followed two cohorts of children every 3-4 years from 2001 until 2016. In the first round of the surveys, the Younger Cohort was aged 1 year in 2002 (n = 2,052) and the Older Cohort, 5 years (n = 714). Only the Younger Cohort will be used in this thesis due to its larger sample size and due to missing information for the Older Cohort in Rounds 4 and 5[3]. By the 5th Round of the survey in 2016, the Younger Cohort was aged 15 years and the sample of remaining children was 1,860.

[3] For instance, information on household's consumption, an important variable for my empirical analyses, was not collected for the Older Cohort after Round 3.

Table 1. Sample and average age of the younger cohort children of the Youngs Lives study

Round	Year	Average age (years)
1	2002	1
2	2006	5
3	2009	8
4	2013	12
5	2016	15

The sampling strategy followed in the first round of the YL study chose 20 districts using a multistage, cluster-stratified random sampling. YL is a "pro-poor" study. The pro-poor sampling strategy was designed using the Peruvian Poverty Index Map of 2000. From the 1,818 districts in this map, the richest 5% were excluded, which allowed an oversampling of poor areas. The final sample consisted of 1 extremely poor, 4 very poor, 8 poor and 7 average districts (Escobal & Flores, 2008). Figure 3 maps the geographical location of the 20 districts.

Figure 3. Map of the districts included in the Young Lives study in Peru

Source: taken from Sanchez & Melendez (2015)

4.4. Variables

4.4.1. Measures of cognitive development

The Peabody Picture-Vocabulary Test (PPVT), a quantitative test and a mathematical test were conducted with children in each round of YL data collection. These indicators are commonly used in the child development literature as measures of child cognition, such as in previous evaluations of the Juntos programme (Andersen et al., 2015; Gaentzsch, 2020; Sanchez et al., 2020) as well as in other studies of child development (Attanasio et al., 2017; Attanasio, Meghir, et al., 2020; Clark et al., 2021; Keane et al., 2022).

In the PPVT test, children are asked to select the picture that best represents the word that is told to them (Cueto, Leon, et al., 2009). This test was administered continuously from Rounds 2 to 5 and adjusted to the age of the children in each round. The quantitative test administered in Round 2, when children were 5 years old, asked them to identify notions such as "few", "most" and "equal", among others, with statements such as "*Point to the plate that has a few cupcakes*" (Cueto, Guerrero, et al., 2009). From Rounds 3 to 5, the mathematical test evaluated children's ability to recognise numbers ("*Pleas, put your finger on number twenty-one*") and perform mathematical operations such as *"Jane has two apples and she receives three more apples. How many apples does she have now"*? The mathematical test was also adjusted to the age of the children in each round.

The tests were administered in Spanish, Quechua, Aimara, Native from the Jungle or a combination of these languages as required. Nonetheless, in some cases, the assessments were conducted in a language that was not the child's native tongue. This occurred when the fieldworker did not share the same mother tongue as the child.

4.4.2. Children's time use in the Young Lives Study

From Round 2 when the children were 5 years old, to Round 5 when children were 15 years old, the YL study collected information about children's time use. Time use data was from children during Round 1 given that the children were only 1 year old at that time. The information was reported by the caregivers in Rounds 2 and 3, and by the children in Rounds 4 and 5. In both cases, they were asked to report the number of hours that children spent on eight activities during the last “typical” day, defined as a normal weekday, excluding holidays, festivals or days of rest (Briones, 2018). The categories of activities considered were the following: sleeping, caring for others, household chores, unpaid work (such as working in a family farm), paid work, time at school, studying at home, and time for playing and leisure (including time for eating and bathing). Unfortunately, data on parental time use was not gathered. Consequently, my analysis will focus solely on children's time allocation.

These last three categories can be a considered time investments in the conceptual framework from Section 4.1: time spent at school, time studying outside school and time for leisure and playing. The category of playing and leisure is close to what is considered as “discretionary time” in the studies by Kalenkoski et al. (2011), Kalenkoski and Hamrick (2013) and Seymour et al. (2019), all of which studied time use in adults. Nevertheless, Kalenkoski et al. (2011) and Kalenkoski and Hamrick (2013) also considered the time that adults devoted to education as discretionary time. While this might be accurate for adults, it is not clear whether the time that children spend in educational activities should be considered as “discretionary”.

In the case of children, time at school is a commitment that is outside of their direct control. The time that children spend at school is determined by the educational system. However, it can be argued that children (and their families) have a slightly higher level of freedom of choice over the time devoted to studying outside school. Time studying outside school cannot be totally equated to time at school, which is exogenously determined by the education system. While school can influence the time students dedicate to studying outside of the classroom through assignments, the ultimate decision to comply with these tasks lies in the hands of parents and children. Furthermore, families and children will also make their own choices when it comes to other forms of study, such as revision time. Moreover, time studying outside school cannot equated to labour time, which is determined by the economic needs of the household.

Therefore, I will consider as discretionary time only the category of "leisure and playing". This is the definition used in most of the empirical estimations in thesis. This definition will be further extended to include also "time studying outside school". for the measurement system model that will be describe in Section 4.5.4.1, where leisure and studying outside school will be considered as imperfect proxies for a latent discretionary time. This alternative definition will be used only as a robustness check in the estimations using a Dynamic Factors approach, as will be discussed in Section 4.5.4. Pooling these two categories would give us an aggregated measure close to what is considered as "children's time investments" in the work by Del Boca et al. (2017).

Having in mind these differences, I will follow the strategy used by Kalenkoski et al. (2011) to define time poverty (see Section 2.3). I will consider all the children who have less than 60% of the population's median discretionary time (measured by age-round) as "time poor". This procedure is inspired by the income poverty line estimated as the 60% of the median household income (Eurostats, 2021; Merz & Rathjen, 2014). Further estimations using thresholds of 50% and 70% of the median of the discretionary time will be carried out as robustness checks (Kalenkoski et al., 2011).

4.4.3. Parental investments and conditional cash transfers

The YL study collects data on household expenditure on a range of durable and non-durable goods, such as expenditure on books and stationery, school uniforms and children's clothes. These categories of spending have previously been employed as proxies for parental material investments in previous studies of human capital production (Attanasio et al., 2017; Attanasio, Meghir, et al., 2020). The YL dataset also includes information on participation in the Juntos CCT programme, which has been used previously to study the effect of this programme on children's nutritional and cognitive outcomes (Andersen et al., 2015; Gaentzsch, 2020; Sanchez et al., 2020).

4.4.4. Prices of goods, shocks and adverse conditions

The YL study contains data that is relevant for my modelling strategies aimed at dealing with the endogeneity of parental investments and children's time use, in the framework of the robustness checks carried out using an Instrumental Variables (IV) approach, which will be explained further in Section 4.5.3.

The first set of variables relates to economic shocks and adverse conditions experienced by households, which can affect both parental investments and children's time use (Datar et al., 2014; Duryea et al., 2007; Hupkau et al., 2023; Mendolia et al., 2019). These shocks include crime shocks (such as theft of cash, crops, livestock or other assets), economic shocks (such as loss of employment, increases in inputs prices or decreases in output prices and death of livestock), natural disasters (such as drought, flooding or crop pests), health shocks (such as illness within the family) and changes in the composition of the household (such as death or birth of a family member, or parental divorce).

The second set of variables consists of the prices of various durable and non-durable goods at the level of the community. These include prices for the investment goods identified previously, i.e., school uniforms, notebooks, clothes and shoes, as well as prices for essential items such as food, medicines and gas. These prices have been employed as instruments in the works of Attanasio, Meghir et al (2020) and Keane et al. (2022). Another set of instruments used are the wages at the level of the community for agricultural and non-agricultural jobs, as used in the study by Keane et al. (2022).

In Appendix 11.1, I provide a more detailed discussion of the use of these variables in the context of my instrumental variables estimation.

4.5. Methodological framework for the study of the production of human capital

In this section, I present a review of the most employed methods in the empirical literature for estimating a production function of human capital. This section is inspired by the works of Attanasio et al. (2017), Attanasio, Meguir et al. (2020) and Keane et al. (2022), who estimated human capital production functions using YL data. Additionally, I draw insights from the study by Dickerson & Popli (2016), who examined the effect of persistent poverty on children's cognition in the UK, and the earlier reviews and methodological developments by Cunha et al. (2010), Cunha & Heckman (2008) and Tood & Wolpin (2007).

Following Keane et al. (2022), I assume a linear production function for a set of observable measures of child cognition $Y_{i,t}$ (for instance, verbal and mathematical test scores):

$$Y_{i,t} = \beta_0 + \beta_1' X_{i,t} + \gamma_1 u_{i,t} + \rho_1 \mu_i + e_{i,t}$$

(2)

For now, let's assume that $X_{i,t}$ contains all the relevant observable inputs that may impact children's performance on the measure of cognition $Y_{i,t}$, such as material investments made by parents, child characteristics (such as health status and age), children's time use, time poverty status and parental characteristics such as parents' health, income and education. $e_{i,t}$ is an error term that captures any omitted input in Equation (2) and any measurement error of the skills tests (for now, I assume that it is uncorrelated to the other observed variables. This assumption regarding measurement errors will be relaxed later). μ_i is a measure of unobserved innate children's ability (also called unobserved heterogeneity), and $u_{i,t}$ are other unobservable inputs (such as parental cognition). The endogeneity problem arises since $u_{i,t}$ and μ_i are not directly observed. These factors may be correlated with the observed inputs, leading to biased estimates of the β's .

Fixed Effects (FE), Value-Added (VA) models, Instrumental Variables (IV) and Dynamic Latent Factor Models (DFM) are the most common methods used to address the potential endogeneity of Equation (2) (Attanasio, Meghir, et al., 2020; Dickerson & Popli, 2016; Keane et al., 2022). These models allow for the estimation of the effect of time poverty on cognitive skills under different assumptions, which will be elaborated upon in the next four subsections. Subsequently, I will provide a brief discussion of the similarities and differences between these models, as well as their suitability for the empirical estimations in my thesis.

4.5.1. Fixed Effects approach

Fixed effects (FE) is a widely used panel data technique to address endogeneity arising from unobserved heterogeneity. To illustrate this approach, let's consider a scenario with only two periods, 1 and 2. Following Keane et al. (2022), I can slightly modify Equation (2) and present it for Periods 1 and 2 as follows:

$$Y_{i,1} = \beta_1^1 X_{i,1} + \gamma_1^1 u_{i,1} + \rho_1 \mu_i + e_{i,1} \tag{3}$$

$$Y_{i,2} = \beta_2^2 X_{i,2} + \beta_1^2 X_{i,1} + \gamma_2^2 u_{i,2} + \gamma_1^2 u_{i,1} + \rho_2 \mu_i + e_{i,2} \tag{4}$$

Note that β_1^2 and γ_1^2 in Equation (4) represent the effects that $X_{i,1}$ and $u_{i,1}$, from the first period, have in the second period. FE will address the endogeneity problem under the assumption that the effect of innate ability is the same in each period, $\rho_1 = \rho_2 = \rho$. Computing the first difference between time 1 and time 2 (which can be generalised to times t and $t-1$), I have:

$$Y_{i,2} - Y_{i,1} = \beta_2^2 X_{i,2} + \beta_1^2 X_{i,1} - \beta_1^1 X_{i,1} + \gamma_2^2 u_{i,2} + \gamma_1^2 u_{i,1} - \gamma_1^1 u_{i,1} + \rho \mu_i - \rho \mu_i + e_{i,2} - e_{i,1}$$

Which is equivalent to:

$$Y_{i,2} - Y_{i,1} = \beta_2^2 X_{i,2} + X_{i,1}(\beta_1^2 - \beta_1^1) + [\gamma_2^2 u_{i,2} - u_{i,1}(\gamma_1^1 - \gamma_1^2) + e_{i,2} - e_{i,1}] \tag{5}$$

We can observe that the effect of children's unobserved ability μ_i is eliminated in Equation (5). However, it is important to note that the differentiation of $Y_{i,2} - Y_{i,1}$ does not automatically yield unbiased estimates of β, as the terms for unobserved inputs $u_{i,1}$ and $u_{i,2}$ are still present in Equation (5). Two further additional assumptions need to be imposed: 1) that the effects of the unobserved inputs are time invariant and

$\gamma_2^2 u_{i,2} = u_{i,1}(\gamma_1^1 - \gamma_1^2)$ or 2) that the term $\gamma_2^2 u_{i,2} - u_{i,1}(\gamma_1^1 - \gamma_1^2)$ is uncorrelated with the observed inputs. Value-Added Models (VA) offer a different solution to this issue.

4.5.2. Value-Added Models (VA)

VA models are widely used in estimating human capital production functions, as evidenced in the early review by Todd & Wolpin (2007) and the recent empirical works by Clark et al. (2021), Fiorini & Keane (2014) and Keane et al. (2022) among others. Let's assume the following production function in the second period:

$$Y_{i,2} = \rho Y_{i,1} + \beta_2^2 X_{i,2} + (\gamma_2^2 u_{i,2} + \epsilon_{i,2}) \tag{6}$$

In this case, I do not explicitly include the child's innate ability μ_i in the second period. However, it can be demonstrated that I control for it by including the previous cognitive score $Y_{1,i}$. Expanding Equation (6), we get:

$$Y_{i,2} = \rho[\beta_1^1 X_{i,1} + \gamma_1^1 u_{i,1} + \rho_1 \mu_i + e_{i,1}] + \beta_2^2 X_{i,2} + (\gamma_2^2 u_{i,2} + \epsilon_{i,2})$$

Which is equivalent to:

$$Y_{i,2} = \beta_2^2 X_{i,2} + \rho\beta_1^1 X_{i,1} + \rho\gamma_1^1 u_{i,1} + \rho\rho_1 \mu_i + \rho e_{i,1} + \gamma_2^2 u_{i,2} + \epsilon_{i,2} \tag{7}$$

Following Keane et al. (2022) and Todd & Wolpin (2007), it can be shown that Equation (7) is equivalent to Equation (4) under the following assumptions:

(a) The unmeasured initial child ability depreciates at a constant rate ρ, so that $\rho\rho_1 = \rho_2$.

(b) The effect of the lagged observed and unobserved inputs $X_{i,1}$ and $u_{i,1}$ depreciates also at the constant rate ρ, making $\rho\beta_1^1 = \beta_1^2$ and $\rho\gamma_1^1 = \gamma_2^1$.

(c) The error term $\epsilon_{i,2}$ satisfies $\epsilon_{1,2} = e_{i,2} - \rho e_{i,1}$. Alternatively, it can be assumed that current and past error terms are uncorrelated with the included inputs and with the previous test scores.

Assumption (b) can be partially relaxed using a Cumulative Value-Added (CVA) model, which will explicitly include previous inputs $X_{i,t-1}$ in Equation 7. For instance, Dickerson & Popli (2016), include all the past poverty states to study the long-term effects of persistent poverty. In my estimations using this model, I will follow a similar approach by including all past time poverty states and parental investments at each age of the children[4].

4.5.3. Instrumental Variables (IV)

Instrumental Variables (IV) is another approach to dealing with endogeneity in the child development literature [for instance, in Buonomo-Zabaleta (2011) and Keane et al. (2022)]. Assuming the following production function:

$$Y_{i,t} = \rho Y_{i,t-1} + \beta_n X_{i,t,n} + \psi_m T_{i,t,m} + (u_i + \vartheta_{i,t}) \tag{8}$$

where $T_{i,t,m}$ is the vector of m variables suspected to be endogenous (time poverty, children's time use and parental investments in my case). I need to find a vector of j instruments $Z_{i,t,j}$ satisfying the following three conditions:

a) **Enough instruments**: the number of instruments must be greater than or equal to the number of endogenous variables $m \leq j$.
b) **Relevance**: the instruments must have an impact on $T_{i,t,m}$. For instance, wages at the level of the community could serve as an instrument for time poverty. Higher wages may increase child labour, reducing discretionary time and thereby increasing time poverty.
c) **Exclusion restriction**: the impact of $Z_{i,t,j}$ on $Y_{i,t}$ must be only through its impact on $T_{i,t,m}$. In other words, the instrument does not directly affect child cognition, only time use or parental investments. In my case, the effect of wages on child cognition must be only by crowding out the available time children have for educational or other cognitive-enhancing activities.

[4] For instance, for the estimation at age 15, I will include parental investments and time poverty for ages 12, 8 and 5.

If my instruments satisfy all these three assumptions, I can estimate the effect of $T_{i,t,m}$ on $Y_{i,t,j}$ by a Two-Stage Least Squares estimation. In a first stage, a regression of $T_{i,t,j}$ on $Z_{i,t,j}$ and controls is performed. The predicted values for $\widehat{T_{\iota,t,J}}$ are then used in a regression of $Y_{i,t,j}$ on $\widehat{T_{\iota,t,J}}$:

$$Y_{i,t} = \rho Y_{i,t-1} + \beta_n X_{i,t,n} + \psi_m \widehat{T_{\iota,t,m}} + (u_i + \vartheta_{i,t}) \tag{9}$$

Given that children's innate ability and the error term $u_i + \vartheta_{i,t}$ are orthogonal to $\widehat{T_{\iota,t,J}}$ and $X_{i,t,n}$, the set of estimated coefficients β_n and ψ_m will be unbiased.

As highlighted by Cunha & Heckman (2008), implementing an instrumental variables procedure in the estimation of a human capital production function requires a substantial number of instruments. In most cases, researchers face the challenge of having more endogenous variables than instruments. Moreover, as observed by Keane et al. (2022), few studies have implemented IV strategies for children's time use and all those that have, suffered from weak instruments.

In my study I have five potentially endogenous variables: time poverty status, three variables of time use [sleeping time, children's work (all categories) and time for education (at school and at home)], and parental investments. To satisfy condition (a), I need to find at least five instruments, which then would need to satisfy conditions (b) and (c). In Appendix 11.1, I discuss the instruments used for the estimation of Equation (9) in more detail.

4.5.4. Dynamic Factors Models (DFM)

Finally, another stream of the literature uses Dynamic Factor Models (DFM) to estimate human capital production functions, such as in Attanasio et al. (2017), Attanasio, Meghir et. al (2020), and Dickerson and Popli (2016). This literature heavily relies on the methodological contributions made by Cunha et al. (2010), Cunha & Heckman (2008).

This framework relies on the assumption that information on parental investments, time poverty and cognitive outcomes (such as vocabulary or mathematical test scores) are likely to be measured with error. This relax the previous assumption that measurement errors are included in the general error term of Equation (2), and that they are uncorrelated with the observed inputs. Not accounting for measurement errors will likely induce additional endogeneity issues. Furthermore, these variables (cognitive outcomes, time poverty and parental investments) can only be considered as imperfect proxies for the true latent child cognition, time poverty and parental investments. Hence, it is necessary to establish a system of equations for producing error-corrected variables (step 1) that will be employed in the empirical estimations (step 2), as explained in the following subsections.

4.5.4.1. Measurement system

As before, I assume a linear model of human capital accumulation:

$$\theta_{i,t} = \beta_{0t} + \gamma_{1t}\lambda_{i,t} + \varphi_{1t}\delta_{i,t} + \pi_t X_{i,t,n} + \tau_{i,t} \tag{10}$$

Where $\theta_{i,t}$ is the cognition of child i in period t, $\lambda_{i,t}$ are parental investments and $\delta_{i,t}$ is a dichotomous indicator for time poverty. All these variables are measured with error, thus they are endogenous. $X_{i,t,n}$ is a vector of child and family characteristics, which are assumed exogenous. Finally, τ_t is a "well-behaved" error term. Child cognition, parental investments and child time poverty are assumed to be latent, depending on some exogenous covariates $X_{i,t,n}$:

$$\theta_{i,t} = \rho_t X^{\theta}_{i,t,n} + r_t \tag{11}$$

$$\lambda_{i,t} = \omega_t X^{\lambda}_{i,t,n} + v_t \tag{12}$$

$$\delta_{i,t} = \vartheta_t X^{\delta}_{i,t,n} + d_t \tag{13}$$

Where r_t, v_t and d_t are well-behaved error terms. The goal of this approach is to obtain error-corrected versions of these three variables, which will then be used to estimate Equation (10). For this purpose, I set a measurement system model, using the imperfect proxies for cognition, time poverty and parental investments.

For instance, following Dickerson & Popli (2016), let's set the following model for cognition:

$$Y^{\theta}_{i,j,t} = u_{j,t} + \alpha^{\theta}_{j,t}\theta_{i,t} + \varepsilon^{\theta}_{j,t} \tag{14}$$

Where $Y^{\theta}_{ij,t}$ is the jth available measure for latent cognition $\theta_{i,t}$, with $j = 1,2 \dots, m^{\theta}_{t}$, such that $m^{\theta}_{t} \geq 2$. The coefficients $\alpha^{\theta}_{j,t}$ are called "factor loadings", and identify how much information about cognition each measure contains. The same model can be used for parental investments and time poverty. Following Attanasio, Meghir et al. (2020) in their study using Indian YL data, I will use test scores for mathematical abilities and the Peabody Picture Vocabulary Test (PPVT) scores as measures of cognition; and the amounts spent on books, clothing and school uniforms as measures for parental investments. Note that one of the primary distinctions of the DFM approach from the previous ones is that, in those methodologies, separate analyses are conducted for each measure of child cognition (math and verbal test scores). In contrast, in this case, a composite measure of cognition derived from the combination of both test scores is used as the outcome variable.

The measurement system for time poverty is more complicated and, to the best of my knowledge, there are no previous examples. Given that child time poverty is estimated based on children's discretionary time, I will set a measurement system for discretionary time rather than for the time poverty indicator. As discussed previously, the best proxy for children's discretionary time is collected in the YL as one single category named "leisure and playing". However, to be able to estimate Equation (14) for discretionary time, I need at least two proxies for discretionary time.

Therefore, I use time for studying at home as another measure of discretionary time. As earlier explained, this choice is based on the assumption that time studying at home is more flexible compared with other time categories such as time spent at school, and it is discretionary at the margin. As outlined in Section 4.4.2 the approach of merging time for playing and leisure with time for studying at home aligns with the methodology employed by Del Boca et al. (2017), who referred to it as "children's time investments."

4.5.4.2. Identification of the factor loadings and the error-corrected variables

The next step is to identify the factor loadings $\alpha_{ij,t}^{\theta}$ using the approach developed by by Cunha et al. (2010), Cunha & Heckman (2008). It uses the covariances between the different measures and between periods. Assuming we have two measures for cognition ($Y_{i1,t}^{\theta}, Y_{i2,t}^{\theta}$), I need to identify at least $\alpha_{1,t}^{\theta}$ and $\alpha_{2,t}^{\theta}$. For this, I need to normalise one of the factor loadings to one. Without loss of generality, let's assume $\alpha_{1,t}^{\theta} = 1$. Hence, according to Cunha & Heckman (2008) :

$$cov(Y_{i1,t-1}^{\theta}, Y_{i1,t}^{\theta}) = cov(\theta_{i,t-1}, \theta_{i,t}) \tag{15}$$

$$cov(Y_{i2,t-1}^{\theta}, Y_{i1,t}^{\theta}) = \alpha_{2,t-1}^{\theta} cov(\theta_{i,t-1}, \theta_{i,t}) \tag{16}$$

$$cov(Y_{i1,t-1}^{\theta}, Y_{i2,t}^{\theta}) = \alpha_{2,t}^{\theta} cov(\theta_{i,t-1}, \theta_{i,t}) \tag{17}$$

Given that $Y_{i1,t}^{\theta}$ and $Y_{i2,t}^{\theta}$ are known in each period t, I can estimate the left hand sides of Equations 15-17.This allow the identification of $\alpha_{2,t}^{\theta}$ by the ratio of Equations 17 and 15. Similarly, taking the ratio of 16 and 15, $\alpha_{2,t-1}^{\theta}$ can be identified. This procedure is similar for the rest of latent variables.

It is important to highlight that Equations 15-17 require the use of lags of the proxies for the variables. This means that I can identify the factor loadings only from Round 3 (age 8), as I need to use data from Round 2 (age 5), which is the initial round providing information on math and verbal test scores.

The next step is to obtain the error-corrected latent variables using these factor loads. Let's define $\widehat{\theta_{i,t}}$ as the error-corrected cognitive ability for children i. It can be obtained using the following procedure (Cunha, 2011; Dickerson & Popli, 2016):

$$\widehat{\theta_{i,t}} = \sum_{j=1}^{m_t^{\theta}} w_{j,t} Y_{ij,t}^{\theta}$$

(18)

$$\text{where } w_{j,t} = \frac{(\widehat{\alpha_{j,t}^{\theta}})^2}{\sum_{j=1}^{m_t^{\theta}} (\widehat{\alpha_{j,t}^{\theta}})^2}$$

(19)

Once the error-corrected latent variables are obtained using the same procedure for the remaining variables, I can substitute them into Equation (10) and estimate the following model:

$$\widehat{\theta_{i,t}} = \beta_{0t} + \gamma_{1t}\widehat{\lambda_{i,t}} + \varphi_{1t}\widehat{\delta_{i,t}} + \pi_{nt}\mathrm{X}_{i,t,n} + \tau_{i,t}$$

(20)

4.5.4.3. Further sources of endogeneity

As discussed by Cunha et al. (2010), the model in Equation (20) still assumes that the error term is uncorrelated with any of the observable and unobservable inputs. However, other sources of endogeneity such as reserved causality due to unobserved child ability may still exist.

I can relax this assumption by allowing for endogenous regressors in Equation (20) and implement additional techniques. One approach is to combine the Dynamic Factor Model with the Cumulative Value-Added approach, as done by Dickerson & Popli (2016). Following their approach, I can estimate the following equation:

$$\widehat{\theta_{\iota,t}} = \beta_{0t} + \beta_{1t}\widehat{\theta_{\iota,t-1}} + \gamma_{1t}\widehat{\lambda_{\iota,t}} + \gamma_{2t}\widehat{\lambda_{\iota,t-a}} + \varphi_{1t}\widehat{\delta_{\iota,t}} + \varphi_{2t}\widehat{\delta_{\iota,t-a}} + \pi_{nt}\mathrm{X}_{ni,t} + \tau_{i,t} \tag{21}$$

Where $\widehat{\theta_{\iota,t-1}}$, as explained in the previous section, would control for unobserved children's innate ability. a is the number of previous periods, so $t-a$ are the previous realisations of time poverty and parental investments. For instance, for age 15, it will be time poverty and parental investments in ages 8 and 12.

Another approach to address the endogeneity of parental investments, which also depend on children's innate ability, is to model parental investments as a separate equation. This suggestion by Cunha et al. (2010) has been followed by Atanassio, Meghir et al. (2020) and Dickerson and Popli (2016). In this approach, parental investments will depend on children's cognition as well as on other individual and household-level determinants $X_{i,t,n}^{\lambda}$, such as parental background and children's health:

$$\widehat{\lambda_{\iota,t}} = \sigma_{1,t}\widehat{\theta_{\iota,t}} + \sigma_{2,t}X_{i,t,n}^{\lambda} + \sigma_3 R_{i,t} + \varsigma_{i,t} \tag{22}$$

Where $\varsigma_{i,t}$ is an error term assumed not to be correlated with $\widehat{\theta_{\iota,t}}$ and $X_{i,t}^{\lambda}$. $R_{i,t}$ is a variable or set of variables that will impact parental investments, but not correlated with children's cognition. As discussed by Dickerson & Popli (2016), $R_{i,t}$ must influence parental desire and ability to invest in their children, but will not directly influence cognitive abilities. In that sense, it is similar to the relevance and inclusion restrictions from the IV model. $R_{i,t}$ must be also included only in Equation (22) but not in Equation (21). As in Attanasio et al. (2017) and Attanasio, Meghir et al. (2020), I use prices at the level of the community and household resources as measured by a wealth index.

The predicted error term $\widehat{\varsigma_{\iota,t}}$ from the Investment Equation 22 will be included in the Equation (21) to further control for the endogeneity of parental investments Attanasio et al. (2017). The inclusion of $\widehat{\varsigma_{\iota,t}}$ makes the error term $\kappa_{i,t}$ and parental investments orthogonal (Wooldridge, 2015):

$$\widehat{\theta_{\iota,t}} = \beta_{0t} + \beta_{1t}\widehat{\theta_{\iota,t-1}} + \gamma_{1t}\widehat{\lambda_{\iota,t}} + \gamma_{2t}\widehat{\lambda_{\iota,t-a}} + \varphi_{1t}\widehat{\delta_{\iota,t}} + \varphi_{2t}\widehat{\delta_{\iota,t-a}} + \pi_{nt}\mathrm{X}_{ni,t} + \partial_{i,t}\widehat{\varsigma_{\iota,t}} + \kappa_{i,t} \tag{23}$$

Therefore, Equation 23 is the final functional form for the estimation of children's human capital in the Dynamic Factor Approach.

4.6. Similarities between these methodological approaches and sources of variation.

As mentioned previously, all these approaches have been used in the applied child development literature. However, the studies by Keane et al. (2022), Borga (2019) (2019), Attanasio et al. (2017) and Attanasio, Meghir et al. (2020) are particularly relevant as they have utilised the YL data. Both Attanasio et al. (2017) and Attanasio, Meghir et al. (2020) focused on the use of DFM, while Keane et al. (2022) used CVA, IV and FE strategies, with CVA being their preferred strategy. Borga (2019) also used CVA and FE models.

Other recent studies, not limited to those involving YL data, have also employed these modelling strategies. For instance, Clark et al. (2021), Del Boca et al. (2017), and Fiorini & Keane (2014), have employed CVA models. IV strategies can be found in Buonomo-Zabaleta (2011), Fitzsimons & Vera-Hernandez (2022) and Nicoletti & Tonei (2020), while FE methods were applied in Del Boca et al. (2017) and Nicoletti & Tonei (2020). Finally, DFM were also used in the studies by Attanasio, Cattan et al. (2020) and Dickerson & Popli (2016).

As can be observed, some authors favour the adoption of certain methods over others. Their choice depends heavily on the characteristics of the available data. These characteristics include the nature of the data (for instance, longitudinal versus cross-sectional), the number of available data rounds in the case of longitudinal data, the type and number of measures for child cognition, and the availability of potential variables to be used as instruments. On the other hand, authors like Borga (2019), Fiorini & Keane (2014) and Keane et al. (2022) prefer to employ and compare various methods, discussing the general patterns observed across different models and methodologies.

It also is important to note that all of the methods listed here rely on different sources of variation to estimate the effects of key independent variables. These diverse approaches can lead to variations in the estimated effects and their associated confidence intervals. It is important to consider these differences when interpreting and comparing results across studies.

For instance, as discussed by Keane et al. (2022), an FE model will use only within-child variation in the inputs between periods. This model is suitable to study the changes in time poverty between rounds, but not to study the longer-term effects of time poverty, such as the effect of experiencing time poverty at age 5 on cognition at age 15. The CVA model will use, conditional on the controls, all the remaining variation on the inputs. Moreover, The CVA will utilise the between-child variation in time poverty, allowing for the inclusion of past realisations of time poverty.

The IV model uses the variation induced by the instruments to estimate the effects of endogenous inputs on child cognition. The strength of instruments is crucial in the IV approach, to ensure a strong source of variation in the instrumented variables. This variation is necessary to identify a statistically significant effect, if one exists. However, even with strong instruments, the CVA model is more efficient than FE and IV approaches if the respective assumptions hold, as it includes more sources of variation (Keane et al, 2022).

As outlined in Appendix 11.1, strong instruments were not found within the data available for this study, an issue also faced by Keane et al. (2022) in their study of child work in YL countries, including Peru. For that reason, the results from the IV approach are included only as an appendix to this thesis.

Equation (23) above offers a combination of a DFM and CVA model, in the framework of a control function approach. In this approach, the variation in parental investments is not conditioned on the variation induced by the instruments, as in the IV model. Conditional on the inclusion of the error term from the investment equation (Equation 22), the error term of Equation 23 is orthogonal to parental investments. Additional sources of endogeneity due to unobserved child innate ability are assumed to be controlled by the lag of the cognitive score.

However, there are two potential limitations of the DFM approach worth discussing in the context of this study. First, it will not allow the identification of different effects of time poverty on mathematical and verbal skills, but only on the composite measure for child cognition, as explained in Section 4.5.4.1. However, previous analyses identified potential heterogenous effects of inputs on different measures of child cognition, including heterogenous effects of time inputs (Borga, 2019; Keane et al., 2022).

In my case, depending on the determinants of the constraints on discretionary time, time poverty may have heterogenous effects on mathematical and verbal skills. For instance, consider a scenario where time poverty is driven by increased participation in a family business, such as a restaurant. The interaction with adults may contribute to improved verbal skills but may not necessarily enhance mathematical abilities if children are not actively involved in tasks requiring mathematical reasoning.

Second, this model can only estimate the effects of time poverty from ages 8 to 15 (Rounds 3 to 5) since information from Round 2 is used to identify the factor loadings (Equations 15-17 in Section 4.5.4.2) and the composite measure of cognition. Consequently, I will not be able to estimate the effects of experiencing time poverty during the pre-primary stage of early childhood (age 5) on middle childhood and adolescence. As discussed before, early childhood is widely recognised as one of the key windows of opportunity for children, significantly shaping their subsequent development (Almond et al., 2018; Duncan et al., 2010, 2017). The identification of the potential effects of time poverty at this age is of high value to my analysis.

For these reasons, FE and CVA will be the primary estimation methods of the effects of time poverty on children's cognition in Chapter 5. FE will allow me to identify any short-term effects of time poverty on cognition i.e. the effect of changes in time poverty between rounds t and t-1 on current test scores (round t). CVA will allow me to study the longer-term effects of time poverty on test scores, for instance, the effect of experiencing time poverty in ages 5, 8 and 12 on test scores at age 15. **Thus, the results from the IV and DFM are presented in Appendices 11.1 and 11.2 and discussed in the main body of the thesis only as robustness checks.**

5. How does time poverty affect cognitive development of children in Perú?

5.1. Introduction

This chapter investigates the effect of child time poverty on human capital production and accumulation in Perú, addressing research question 1, as detailed in Chapter 3. I first study the prevalence and potential drivers of time poverty among children in Perú. Then, I explore the effect time poverty on cognitive development. I control for relevant child, parental, and household level characteristics.

As briefly discussed before, this chapter is aligned to the works of Attanasio, Meghir et al. (2020), Attanasio et al. (2017) and Keane et al. (2017). Attanasio, Meghir et al. (2020) and Attanasio et al. (2017) use the YL study from Peru, India and Ethiopia to study the effect of parental investments on child health and cognition. Keane et al, (2022), used data from the four countries in the YL study, including Peru, to examine the effect of child work on cognitive development. However, my research differs from that of Keane et al. (2022) by focusing on time poverty, which, as discussed in Section 2.3, represents constraints on discretionary time—the remaining category after deducting all other activities categorised as committed time. Under this definition, work time is classified as one of these committed time activities.

Therefore, this chapter contributes to the body of literature on child development that aims to understand how childhood circumstances such as poverty, child work, school inputs and parental investments, affect short- and long-term outcomes (Almond et al., 2018; Attanasio et al., 2022; Cunha et al., 2010). However, to the best of my knowledge, this chapter represents the first attempt to study the effect of children's time poverty on cognitive outcomes.

5.2. Data and variables

As presented in Chapter 4, I use the YL data from Round 2 (age 5) to Round 5 (age 15). My final working sample is 1,472 children (726 female and 746 male) for whom data on cognitive outcomes, parental investments, time use and other socioeconomic controls (see below) were collected. As dependent variables, I use age-standardised PPVT and mathematical test scores.

My key independent variable is time poverty status at the 60% level [5], that is, a child is considered time-poor if they have less than 60% of the median of the population's discretionary time. As discussed in Section 4.4.2, the category of "playing and leisure" from the YL time use data is considered as "discretionary time" for the FE, CVA and IV models. For the DFM model, the categories of "playing and leisure" and "studying at home" are considered as the proxies for discretionary time, in order to be able to identify the factor loadings of the DFM (see Section 4.5.4.2)

My measure of parental material investments is the sum of expenditure on children's clothes, books and stationery and school uniforms. I also control for a vector of children's time use, that is the number of hours spent sleeping, working for the household (including working on household chores and working for the family farm or business), paid work, and time for education (including time at school and time studying at home).

I also control for children's gender (1= female, 0 = male), attendance at pre-primary education (1= yes, 0=no), age of the child in months, child's ethnicity, stunting status and BMI as controls for children's health, an indicator for being the first child born in the household (1=yes, 0= no), age in months, education and gender of the household's head, household wealth measured as total expenditures, number of other children in the household, number of other members of the household aged 18+, receipt of conditional cash transfers, and area of residence (1= urban, 0=rural) and an indicator for whether the verbal or math test was conducted in the native language of the child. Additionally, to avoid outliers, I have excluded observations with BMI and HAZ values

[5] Additional models were run with the 50% and 70% levels, but the general conclusions from the main analysis did not change.

lower than -5 standard deviations or greater than 5 standard deviations (Anand et al., 2018). The same criteria have been applied to verbal and vocabulary scores.

5.3. Methodology.

As presented in Chapter 4, FE and CVA are the preferred estimation methods for this chapter.

For the FE estimation, I extend the model presented in Section 4.5.1 for t periods:

$$Y_{i,t} = \beta_0 + \beta_1\delta_{i,t} + \varphi'_{i,t}X_{i,t,n} + \rho\mu_i + \varepsilon_{i,t} \tag{24}$$

Where $Y_{i,t}$ are the standardised verbal and mathematical test scores (separate estimations for each variable), $\delta_{i,t}$ is a dichotomous indicator of time poverty, $X_{i,t,n}$ is a vector of controls (including parental investments), μ_i is the innate child ability and $\varepsilon_{i,t}$ = $(u_{i,t} + e_{i,t})$ is an error term composed of the unobserved inputs $u_{i,t}$ and the normal disturbance $e_{i,t}$. All other assumptions from Section 4.5.1 apply to Equation (24). Please note that in a FE approach, the vector $X_{i,t,n}$ contains only time-variant controls. All non-time variant controls (such as children's and parental gender and ethnicity) are removed by the model when estimating the first difference between t and $t-1$. This model is estimated using the observations from Round 2 to Round 5 (ages 5 to 15).

For the CVA approach, I also extend the model presented in Section 4.5.2 to t periods:

$$Y_{i,t} = \beta_0 + \rho Y_{i,t-1} + \beta_1\delta_{i,t} + \beta'_{t-a}\delta_{i,t-a} + \phi_t\lambda_{i,t} + \phi'_{i,t-a}\lambda_{i,t-a} + \varphi'_{i,t}X_{i,t,n} + \varepsilon_{i,t} \tag{25}$$

Where, as defined before, $Y_{i,t-1}$ is the lagged of the verbal or math test score. Contrary to the FE case, the CVA model allows for the inclusion of time-invariant controls in $X_{i,t,n}$.

In order to make explicit the “cumulative” nature of this model, I include variables for current and lagged time poverty, $\delta_{i,t}$ and $\delta_{i,t-a}$, respectively, and for current and previous parental investments, $\lambda_{i,t}$ and $\lambda_{i,t-a}$, where “a" is the number of lags for the previous rounds in each age. The rest of variables have been defined before.

Therefore, Equation (25) is estimated for each age-round, taking into account the test score from the previous round to control for innate child ability, and time poverty status and parental investments from all the previous age-rounds. Given that we have test scores since the age of 5, the CVA model was estimated for ages 8, 12 and 15 (age 5 is used to control for the lag of test scores at age 8).

As discussed in Section 4.6, results using IV and DFM approaches are presented and discussed in Appendices 11.1 and 11.2.

5.4. Descriptive statistics

5.4.1. Socioeconomic factors

Table 2 presents an overview of selected descriptive statistics for Round 5 (age 15) disaggregated by gender of the child. Gender differences were assessed with a t-test of equality of means.

Boys scored higher verbal test scores (PPVT scores) than girls in all data collection rounds. However, these differences were statistically significant only at the ages of 12 and 15, when the respondents were enrolled in middle school. A similar pattern was seen for math test scores: except for the age of 5, boys scored higher math test scores than girls. However, these differences between boys and girls in math test scores were statistically significant only at the age of 8 (primary school) and the age of 15 (secondary school).

Girls had a higher mean BMI-for-age z score than boys (0.471 versus 0.331, significant at the 1% level). However, these values fall within the normal range according to the

UN cut points for this indicator[6]. Stunting prevalence was higher among girls than boys (17% versus 13.8%, significant at the 10% level). Finally, I did not find statistically significant differences in parental investments, household expenditure per capita, number of children and adults in the household or the proportion of children whose mother tongue was Spanish.

Table 2. Selected descriptive statistics for Round 5 (age 15)

Variable	Girls (N=726)	Boys (N=746)	Difference (girls-boys)
Verbal score age 15 (standardised)	-0.069	0.086	-0.155***
Verbal score age 12 (standardised)	-0.071	0.091	-0.161***
Verbal score age 8 (standardised)	0.018	0.055	-0.037
Verbal score age 5 (standardised)	-0.035	0.011	-0.046
Math score age 15 (standardised)	-0.029	0.164	-0.193***
Math score age 12 (standardised)	0	0.07	-0.07
Math score age 8 (standardised)	-0.026	0.096	-0.121**
Math score age 5 (standardised)	0.059	0.009	0.05
Parental investments age 15 (in 100's)	9.041	9.081	-0.04
Parental investments age 12 (in 100's)	7.743	7.292	0.451
Parental investments age 8 (in 100's)	6.332	6.152	0.18
Parental investments age 5 (in 100's)	4.433	4.469	-0.037
Child language (Spanish = 1)	0.846	0.866	-0.02
BMI for age z-score	0.471	0.331	0.141***
Stunting status	0.17	0.138	0.032*
Expenditures per capita age 5 (in 100's)	1.625	1.565	0.06
Expenditures per capita age 8 (in 100's)	2.05	2.026	0.024
Expenditures per capita 12 (in 100's)	3.167	3.149	0.018
Expenditures per capita 15 (in 100's)	3.385	3.377	0.009
Number of children (<18)	1.536	1.523	0.013
Number of adults (18+)	2.675	2.687	-0.011

Notes:
Verbal and mathematical scores were standardised by age of the children.
P-values for the differences between girls and boys calculated from a t-test for equality of means between groups (* p<0.10, ** p<0.05, *** p<0.01)

[6] According to the UN cut points for z-scores, a z-score between -1 and 1 represents a normal BMI, a z-score between 1 and 2 is overweight and a z-score higher than 2 represents obesity. See more at https://www.who.int/tools/growth-reference-data-for-5to19-years/indicators/bmi-for-age

5.4.2. Time allocation and time poverty

Table 3 displays time allocation by gender and age. At age 5, girls spent 4.9 more minutes per day on household chores compared to boys, and slightly less time playing and at leisure (-2.9 minutes), although this last difference was not statistically significant.

At age 8, girls spent on average 17.9 fewer minutes per day playing and at leisure, and 7.3 more minutes studying outside school than boys. Considering the total time for education at school and outside of school, girls spent 10.6 more minutes per day in educational activities than boys. All these differences were statistically significant at the 1% level.

At ages 12 and 15, gender differences in time allocation became more pronounced. At the age of 12, girls spent more time caring for others (+5.9 minutes, p value < 0.1) and undertaking household chores (+7.1 minutes, p value < 0.05). Boys devoted more time to unpaid work (+9.5 minutes, p value <0.01). Girls also slept less than boys (-6.6 minutes, p value < 0.05). No difference was found for leisure and playing.

At age 15, girls again spent more time in household chores (+15.2 minutes, p-value < 0.01) and studying outside school (+12.4 minutes, p-value < 0.01). However, they spent less time sleeping (-12.1 minutes, p-value < 0.01), in unpaid work at the household (-6.2 minutes, p-value < 0.1), in paid work (-11.3 minutes, p-value < 0.01) and in playing and at leisure (-9.8 minutes, p-value < 0.05) than boys. Total education time was also higher for girls than boys (+ 21 minutes, p-value < 0.01).

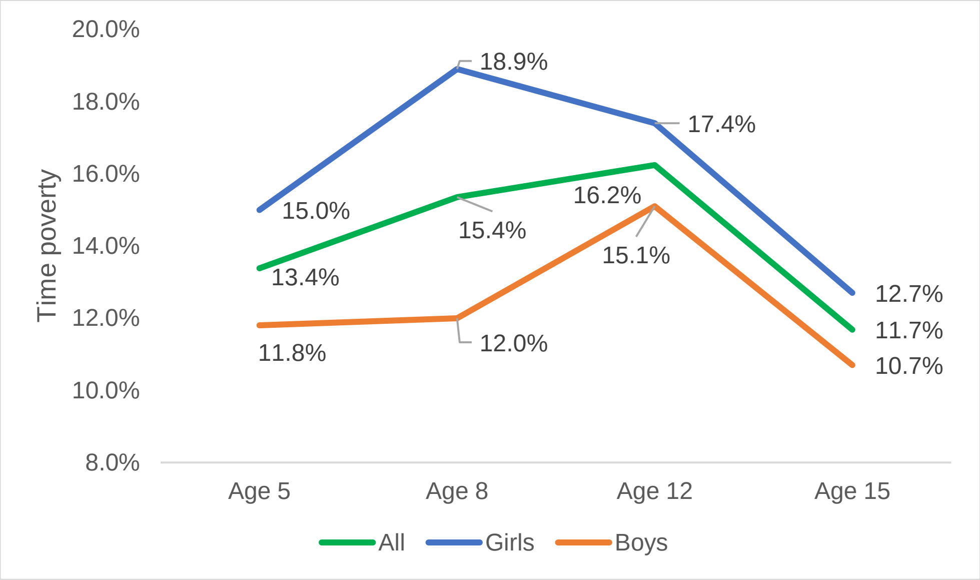
Table 3. Time use allocation by gender and age of children (in hours)

Time use category	**Age 5**			**Age 8**			**Age 12**			**Age 15**		
	Girls	Boys	Diff in minutes	Girls	Boys	Diff in minutes	Girls	Boys	Diff in minutes	Girls	Boys	Diff in minutes
Sleeping	12.3	12.3	2.6	10.0	9.9	4.8	9.6	9.7	**-6.6****	8.7	8.9	**-12.1*****
Caring for others	0.3	0.3	-1.5	0.5	0.5	3	0.9	0.8	**5.9***	0.7	0.6	3.1
Household chores	0.6	0.5	**4.9****	0.9	0.9	2.2	1.3	1.2	**7.1*****	1.5	1.2	**15.2*****
Unpaid work (at the household)	0.1	0.1	-0.42	0.2	0.3	-2.6	0.5	0.6	**-9.5*****	0.3	0.4	**-6.2***
Paid work	0	0	0	0.004	0.005	-0.06	0.1	0.1	-0.3	0.1	0.3	**-11.3*****
School	4.3	4.4	-4.9	6.2	6.2	3.5	6.2	6.1	3.2	7.1	7.0	8.6
Studying (outside school)	1.4	1.4	1.8	2.0	1.9	**7.3*****	1.9	1.9	-0.1	2.2	2.0	**12.4*****
Play and leisure	4.9	4.9	-2.9	4.1	4.4	**-17.9*****	3.7	3.6	0.2	3.4	3.6	**-9.8****
Work for the household (care +chores +unpaid work)	1.0	1.0	3	1.6	1.6	2.6	2.6	2.6	3.6	2.4	2.2	**12.2****
Education (at or outside school)	5.8	5.8	-2.7	8.3	8.1	**10.6*****	8.0	8.0	3.2	9.4	9.0	**21*****

Notes:
P-values for the differences between girls and boys calculated from a t-test for equality of means between groups
(* p<0.10, ** p<0.05, *** p<0.01)

Figure 4 presents time poverty prevalence by gender and age. Among all children, time poverty prevalence increased from 13.4% at age 5, to a maximum of 16.2% at age 12, before decreasing to a minimum of 11.7% at age 15.

Figure 4. Time poverty prevalence by gender and age



Two interesting patterns can be seen in the time poverty prevalence by gender. First, **time poverty prevalence was consistently higher among girls than boys,** although it was statistically significantly different only in early and middle childhood (ages 5 to 8), and not in adolescence. Despite the small and non-significant difference in time for play and leisure between girls and boys at the age of 5 (Table 3), there was a statistically significant gap at that age in time poverty prevalence, with girls experiencing higher rates than boys (15% versus 11.8%). The difference in the prevalence of time poverty between girls and boys was also statistically significant at age 8 (18.9% for girls versus 12% for boys).

Second, **the evolution of time poverty by gender displayed different patterns**. Girls experienced a substantial increase in time poverty between age 5 and 8 (+3.9 percentage points), followed by decreases in the next two rounds. Boys experienced a very small increase between ages 5 and 8, a large increase between age 8 and 12 (+3.1 percentage points), and then a large reduction between 12 and 15 (-4.4 percentage points).

To understand these patterns and the potential drivers of time poverty by gender, Table 4 shows changes in time allocation between rounds.

Table 4. Changes in time allocation between rounds (in minutes)

Variable	Change between ages 8-5		Change between ages 12-8		Change between ages 15-12	
	Girls	**Boys**	**Girls**	**Boys**	**Girls**	**Boys**
Sleeping	-139.7	-141.9	-23.5	-12.1	-54.4	-48.8
Caring for others	11.7	7.3	22.1	19.1	-11.8	-9.0
Household chores	17.8	20.6	23.0	18.0	9.3	1.2
Unpaid work (at the household)	7.7	9.8	14.2	21.1	-9.5	-12.8
Paid work	0.2	0.3	3.1	3.4	1.0	12.0
School	115.3	106.9	-3.2	-2.9	56.3	51.0
Studying (outside school)	33.9	28.5	-9.2	-1.8	22.5	10.0
Play and leisure	-46.9	-31.8	-26.5	-44.7	-13.4	-3.4
Work for the household (care +chores +unpaid work)	37.3	37.7	59.2	58.1	-12.1	-20.7
Education (at or outside school)	149.0	135.7	-12.2	-4.7	78.8	61.0

Notes:
Changes calculated from the values of Table 3 as the change from round t minus round t-1 for each time use variable.

Between ages 5 and 8, both girls and boys experienced a substantial decrease of approximately 2.3 hours in sleeping time. However, the decrease in sleeping time for girls (-139.7 minutes) was lower than their increase in time for education (+149 minutes). In contrast, the reduction in sleeping time for boys (-141.9 minutes) was higher than the increase in time for education (+135 minutes). These changes, along with further increases in time working for the household, resulted in a decrease of time for play and leisure, which was higher for girls than for boys (-46.9 for girls versus -31.8 for boys). **Therefore, the rise in time poverty observed between ages 5 and 8 for both girls and boys can primarily be attributed to the increase in time**

dedicated to education, with a lower but non-negligible contribution of increased work for the household (+37 minutes for both genders). This is consistent with the fact that between those ages, children transitioned from pre-primary to primary school. However, it appears that the increase in time for education had a more pronounced effect on time poverty among girls.

At the age of 12 years, girls and boys witnessed a further decrease in discretionary time. However, this decrease was smaller for girls (-29.5 minutes) compared to boys (-44.7 minutes). In both cases, this decline in discretionary time can be explained by **an increase in time worked for the household (almost one hour for both genders)**

Finally, the further decline in discretionary time between ages 12 and 15 (-13.4. minutes for girls versus -3.4 minutes for boys), can **be primarily attributed to increases in time dedicated to education for both genders** (+78,8 minutes for girls, +61 for boys).

It is important to note that the reductions in sleeping time observed across all rounds are unlikely to have a negative effect on children's cognition. The average sleeping time (Table 3) falls at all times within the recommended range for children at their respective ages (Great Ormond Street Hospital for Children, 2020).

Some of these results may look contradictory at a first glance. In all rounds children experienced a reduction in discretionary time, but also in some rounds, a reduction in time poverty. This is explained by the fact that time poverty is a relative measure of discretionary time constraints, with respect to their peers. For instance, between ages 5 and 8, girls experienced a decrease in discretionary time that was almost 15 minutes higher than boys. Therefore, the distribution of discretionary time became "more unequal", which was ultimately reflected in a larger increase in time poverty prevalence for girls. Similarly, the higher reduction in discretionary time for boys compared to girls (a decrease of 45 minutes for boys versus 26 minutes for girls) between the ages of 8 and 12 lead to a "more equal" distribution of discretionary time than before, which resulted in a reduction of time poverty for girls but an increase for boys.

This nature of time poverty as a relative measure of discretionary time deprivation, highlights the importance of controlling for the full vector of children's time allocation (excluding, of course, time for playing and leisure) in our econometric estimations.

5.5. Econometric results.

5.5.1. Fixed Effects Model

Table 5 and Figure 5 presents the results for the effect of time poverty on verbal and mathematical skills, estimated by the FE model. These results are presented for the entire sample, and for girls and boys separately. The results show that **time poverty had a positive and statistically significant effect on verbal scores for the entire sample** (0.06 sd, p-value <0.05). **This effect was driven by the subsample of girls**, with a coefficient of 0.09 sd (p-value <0.05). The effect for boys was statistically non-significant.

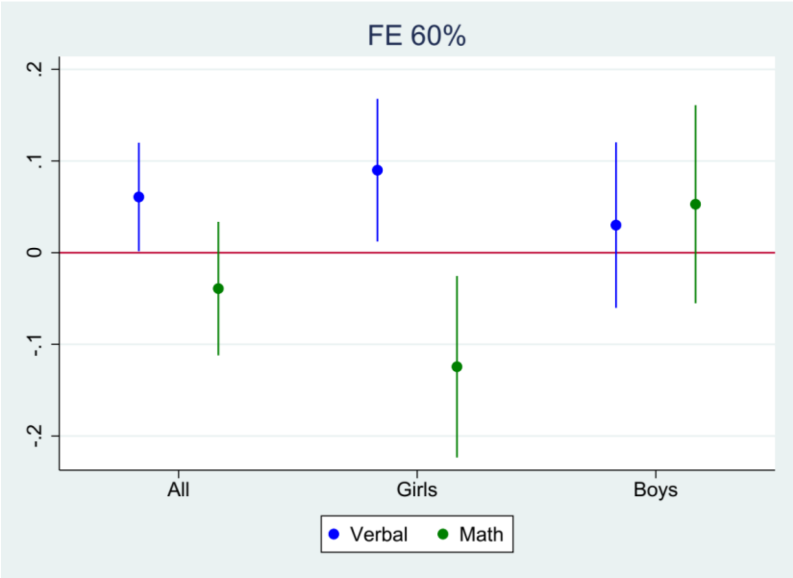
The effect for math test scores was statistically significant only among girls. Experiencing time poverty had a negative effect on test scores of -0.124 sd, significant at the 5% level.

Table 5. Effect of time poverty on test scores, FE model

Variable	**Verbal**			**Math**		
	All	**Girls**	**Boys**	**All**	**Girls**	**Boys**
Time poverty	0.0608** (0.0302)	0.0900** (0.0397)	0.0301 (0.0460)	-0.0392 (0.0372)	-0.124** (0.0505)	0.0529 (0.0551)
N	1472	726	746	1472	726	746

Notes:
FE: Fixed Effects Model
Standard errors in parentheses, clustered at the Young Lives survey cluster level
P-values : * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see sections 5.2 and 5.3)

Figure 5. Effect of time poverty on test scores, FE model



Notes:
FE: Fixed Effects Model
95% Confidence Intervals
Controlled for relevant individual and household socioeconomic characteristics (see sections 5.2 and 5.3)

5.5.2. Cumulative Value-Added Model

In this section, I present two set of results. Table 6 and Figure 6 show the effect of current time poverty at the ages of 8, 12 and 15 on child cognition. In other words, they illustrate the impact of time poverty at these specific ages on cognition at those same ages. In addition, Table 7 and Figure 7 present the effect of experiencing time poverty at the ages of 5, 8 and 12 on test scores at the age of 15.

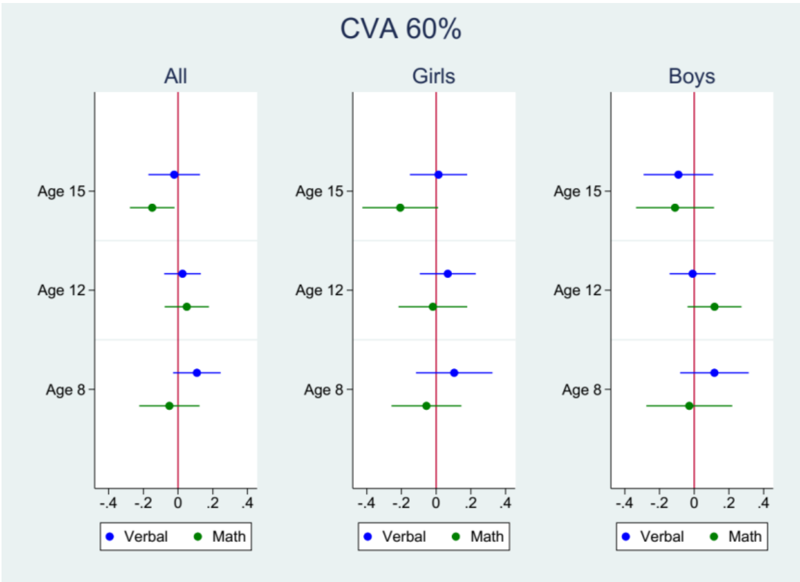
Experiencing time poverty at the age of 15 had a negative impact of -0.15 sd (p-value < 0.05) **on mathematical skills in the entire sample**. Similar to the FE case, **this result was driven by the negative effect on girls** (-0.21 sd), which was also significant but at the 10% level (Table 6 and Figure 6). The rest of the current effects of time poverty were not statistically significant.

Table 6. Effect of current time poverty on test scores, CVA model

	Verbal			Math		
	All	**Girls**	**Boys**	**All**	**Girls**	**Boys**
Current time poverty (age 15)	-0.0225 (0.0724)	0.0137 (0.0804)	-0.0911 (0.0978)	-0.149** (0.0627)	-0.207* (0.106)	-0.111 (0.110)
Current time poverty (age 12)	0.0255 (0.0515)	0.0672 (0.0787)	-0.00911 (0.0647)	0.0501 (0.0622)	-0.0189 (0.0965)	0.117 (0.0758)
Current time poverty (age 8)	0.108 (0.0665)	0.104 (0.107)	0.116 (0.0957)	-0.0501 (0.0842)	-0.0560 (0.0976)	-0.0284 (0.120)
N	1472	726	746	1468	723	745

Notes
CVA: Cumulative Value-Added Model
Standard errors in parentheses, clustered at the Young Lives survey cluster level
P-values: * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see sections 5.2 and 5.3)

Figure 6. Effect of current time poverty on test scores, CVA model



Notes:
CVA: Cumulative Value-Added Model
95% confidence intervals.
Controlled for relevant individual and household socioeconomic characteristics (see sections 5.2 and 5.3)

Considering the general pattern of non-significant results for current time poverty at ages 8 to 12, it was anticipated that time poverty would not have a delayed effect on child cognition. Indeed, most of the effects of the lags of time poverty were not statistically significant (Table 7 and Figure 7), with two exceptions. First, even though experiencing time poverty at the age of 12 had a non-significant effect on current test scores across the entire sample (Table 6), **it had a positive delayed impact on mathematical skills at the age of 15** (0.112 sd, p-value < 0.05). Moreover, **the delayed effect of time poverty at the age of 12, on boys' verbal skills at the age of 15, was positive and statistically significant** at the 10% level (0.136 sd, Table 7) although it was not significant for current verbal skills at the age of 12 (Table 6).

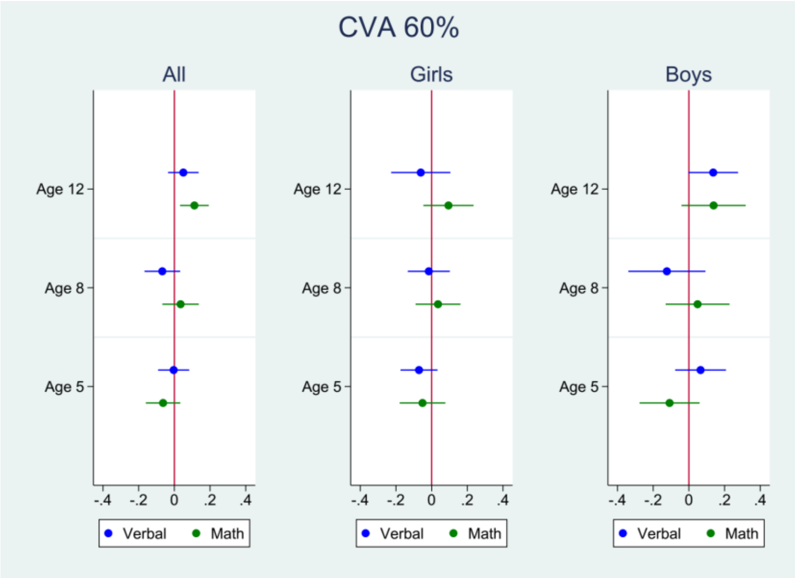
Finally, Table 7 also indicates that the lag of the test scores was consistently and significantly positive at the 1% level across all test scores and samples, with coefficients approximately around 0.6 standard deviations. This demonstrates a high level of persistence in cognitive skills, which aligns with similar findings in previous studies (Borga, 2019; Del Boca et al., 2017).

Table 7. Effect of experiencing time poverty at the ages of 5, 8 and 12 on test scores at the age of 15, CVA model

	Verbal			Math		
	All	**Girls**	**Boys**	**All**	**Girls**	**Boys**
Current time poverty (age 15)	-0.0225 (0.0724)	0.0137 (0.0804)	-0.0911 (0.0978)	-0.149** (0.0627)	-0.207* (0.106)	-0.111 (0.110)
Time poverty t-1 (age 12)	0.0503 (0.0417)	-0.0607 (0.0809)	0.136* (0.0679)	0.112** (0.0392)	0.0945 (0.0686)	0.139 (0.0877)
Time poverty t-2 (age 8)	-0.0675 (0.0487)	-0.0158 (0.0575)	-0.123 (0.105)	0.0349 (0.0497)	0.0358 (0.0613)	0.0488 (0.0872)
Time poverty t-3 (age 5)	-0.00392 (0.0425)	-0.0707 (0.0505)	0.0655 (0.0694)	-0.0632 (0.0471)	-0.0510 (0.0624)	-0.108 (0.0821)
Lag of the test	0.586*** (0.0404)	0.607*** (0.0399)	0.576*** (0.0598)	0.589*** (0.0328)	0.556*** (0.0342)	0.620*** (0.0429)
N	1472	726	746	1468	723	745

Notes
CVA: Cumulative Value-Added Model
Standard errors in parentheses, clustered at the Young Lives survey cluster level.
P-values: * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see sections 5.2 and 5.3)

Figure 7. Effect of experiencing time poverty at the ages of 5, 8 and 12 on test scores at the age of 15, CVA model



Notes:
CVA: Cumulative Value-Added Model
95% Confidence Intervals
Controlled for relevant individual and household socioeconomic characteristics (see sections 5.2 and 5.3)

5.5.3. Robustness checks

In this section, I briefly present the robustness checks carried out with the DFM and IV models, which are detailed in the Appendices 11.1 and 11.2.

It is important to note that, contrary to the CVA, IV and FE models, the DFM uses a composite measure of cognition as the outcome, as has been discussed in Section 4.5.4.1. Additionally, as discussed in that section and in Section 4.4.2, the measure of time poverty in the DFM is derived from the extended "discretionary time" definition, which, in this case, encompasses not only time for play and leisure (as in the CVA, IV, and FE models) but also time allocated to studying at home. Therefore, these two characteristics make the results from the DFM not directly comparable to the outcomes derived from the other models.

The results from the DFM revealed **a negative impact of current time poverty on the composite measure of cognition at the age of 15 for the entire sample** (-0.105 sd, p-value < 0.05). **This effect was primarily driven by its impact on boys** (-0.146 sd, p-value < 0.05), as the effect for girls was not statistically significant. Additionally, **only the lag of time poverty for girls at the age of 12 showed a negative impact on the composite measure of cognition at the age of 15**. This effect (-0.08 sd) was statistically significant at the 5% level. Furthermore, similar to the results from the CVA model, the lags of cognition, with a magnitude of around 0.7 sd, were consistently positive and statistically significant at the 1% level across all the subsamples.

Table A.1 in Appendix 11.1 details the instruments used in the IV model. The set of instruments did not meet the criteria to be considered strong instruments (an F-statistic from the first stage regression greater than or equal to 10). A similar issue was encountered in the study by Keane et al. (2022), which also utilised YL data. In their research, the authors found that the instruments were robust enough only in Ethiopia, and not in the other countries covered by the YL study, including Peru.

The weakness of the instruments, i.e., their limited ability to produce enough variation in the instrumented variables, is evident from the large standard errors and confidence intervals observed for the estimated coefficients (Tables A2-A3 and Figures A1-A2). **None of the estimated coefficients of current and lagged effects of time poverty on child cognition were statistically significant.**

5.6. Discussion and conclusions

This chapter aims to understand the effect of children's time poverty on short and long-term cognitive development. Considering that the existing literature predominantly concentrates on time poverty among adults, this chapter, to the best of my knowledge, is the first examination of the effects of child time poverty on child cognition.

I find two interesting patterns regarding children's time poverty. First, time poverty prevalence across the entire sample of children was higher at the ages of 8, when children were in the first years of primary school, and 12, when they transitioned to secondary school. Further examination of children's time allocation suggests that the primary driver for the increase in time poverty at the age of 8 was the increase in time spent at school and studying outside school, followed by a smaller but non-negligible increase in household work. However, the factor driving time poverty at the age of 12 appears to be an increase in children's engagement in household work. Finally, the driver of time poverty at the age of 15 was again an increase in time for education.

Second, the evolution of time poverty prevalence across rounds differed also by gender. Girls experienced a high increase in time poverty of 3.9 percentage points between ages 5 and 8, mainly due to an increase in time at school (due to the transition from pre-primary to primary school). Time poverty for boys remained almost constant between these rounds.

While girls time poverty declined in the next two rounds i.e., between ages 8 and 15, an increase in time poverty of 3.1 percentage points experienced by boys between ages 8 and 12. Between those ages, both girls and boys experienced a similar increase in work time (around one hour adding up all work categories, that is, household chores, unpaid work for the household and paid work). However, girls also experienced a greater reduction in sleeping time and studying outside school. This allowed girls to compensate for the decrease in discretionary time caused by increased working time, resulting in a net reduction in time poverty for them.

Finally, the increase in time for school experienced by girls (+78 minutes) and boys (+61 minutes) between ages 12 and 15, was compensated with reductions in sleeping time and some further reductions in work for the household.

These results suggest a similar pattern for children’s and adults time poverty. Previous literature has shown gender gaps in time poverty, with women experiencing higher levels of time poverty compared with men, which highlights the gendered nature of time allocation (Arora, 2015; Bardasi & Wodon, 2010; Orkoh et al., 2020).

The sources of variation in time poverty are important for the analysis of the effects of time poverty on cognitive outcomes. A rise in time poverty caused by increases in time for education will not necessarily lead to negative effects on cognitive outcomes. In this case, conditional on good school quality and parental inputs for studying at home, time poverty may lead to improvements in verbal and mathematical skills.

The effect of time poverty caused by increased working time on children’s cognition is also undetermined. On the one hand, certain types of work, especially during adolescence, may contribute to cognitive and non-cognitive skills development (Fassa et al., 2000; Mortimer, 2010). For instance, working in a family business, such as a small grocery shop, may increase children's social interactions and thus contribute to the enhancement of verbal skills. Furthermore, by engaging in tasks that require mathematical operations, children may also experience an improvement in mathematical skills.

On the other hand, if children's working time is mainly spent on activities where they are not exposed to social interactions or mathematical operations, such as manual working in the household or on a family farm, these positive effects on cognitive skills may not occur. Furthermore, as found by Keane et al. (2022), if the increase in working time crowds out time for formal education, then working time may have a negative effect on children's cognition regardless of the work activity or function.

My results show a general pattern of null effects of current and past time poverty on cognitive outcomes, measured by verbal and mathematical test scores. Most of the estimated coefficients were not statistically significant. Moreover, the results from the IV model are not reliable enough due to weak instruments. However, despite the overall lack of significant effects of current and past time poverty on cognitive outcomes, there were some notable exceptions that deserve careful consideration and interpretation.

First, among girls the FE model shows a small positive effect of time poverty on verbal skills (+0.09 sd), and a small negative effect on mathematical skills (-0.124 sd). Considering that 1) the FE model identifies within children variation in time poverty prevalence between rounds, and 2) the driver of time poverty in two of the rounds was increases in time for education, this result suggests that time poverty driven by increases in education time may indeed enhance verbal skills in this context.

However, this does not hold true for mathematical skills. In addition to the FE model, the CVA model also reveals a negative effect on mathematical skills at the age of 15 among girls that is statistically significant at the 10% level. It seems then that even if the increase in time poverty can be attributed to time spent at school, this extra time was not of enough quality to foster mathematical skills.

In light of this result, my findings regarding mathematical skills call for a closer examination of the nuances surrounding instructional time and cognitive outcomes. While, on average, international evidence suggests a positive association between increased instructional time and students' performance or cognitive outcomes, the results are heterogenous and dependent on various factors. For example, positive effects have been observed to be more significant among boys than girls in Germany (Dahmann, 2017). Additionally, these positive effects appear to be more pronounced in high income countries compared to LMICs, and are mediated by teacher quality (Wedel, 2021). High-performing students also tend to benefit more from increased instructional time, as indicated by studies conducted in Germany and Switzerland (Cattaneo et al., 2017; Huebener et al., 2017).

Finally, the gendered distribution of time poverty and its impact on measures of child cognition, as found in this study for Peru, highlight gender disparities that emerge early in life. These disparities are consistent with gender gaps documented in other aspects of children's well-being in both high and LMICs, such as parental investments or nutritional outcomes (Barcellos et al., 2014; Jayachandran & Pande, 2017; Kaushal & Muchomba, 2018).

My findings for Peru stress the need to better understand gender differences in educational experiences, as well as the impact of teacher qualifications and overall school quality on children's cognition. This understanding can facilitate the development of policy interventions targeting both the household and the school, with the objective of improving the efficiency of time invested in education. Finally, it is also crucial to develop public policies that increase awareness of gender gaps in time allocation between girls and boys, with the aim to promote more equitable human capital development and well-being outcomes from early childhood.

5.7. Limitations

My research on the impact of time poverty on child cognition has some potential limitations. First, time poverty is a measure of *relative* discretionary time deprivation. That is why I found that while the total amount of discretionary time decreased between ages 12 and 15, the prevalence of time poverty also declined. An objective measure of time poverty would involve defining the specific amount of discretionary time a child would need at different ages. To the best of my knowledge, no such measure has yet been established.

Second, even the definition of discretionary time used in this study has not yet been standardised in the literature as it depends on the level of detail in available time use data. Discretionary time is usually estimated as the residual time after subtracting a series of committed and necessary activities. The existing definitions of committed and necessary activities were initially designed for the measurement of adult time allocation and may not directly apply for children. In my case, the time use data collected by broad sweep of the YL project is not as detailed as the time use data collected in dedicated time use studies. However, I have used the category of time for playing and leisure as a proxy for discretionary time. I can safely assume that the remaining time use categories (see Table 3) can be categorised as necessary and committed activities.

Third, the collection of time use data is subject to measurement error, primarily due to recall bias. However, this is a general issue in time use research and not specific to this study. To mitigate the potential impact of recall bias, I normalised time use by considering the total time allocated within a day as 24 hours. While individuals might not recall with the amount of time devoted to the specific activities with precision, they may remember the relative ranking of time allocated to those activities. Therefore, normalising the time in such a way that it sums up to 24 hours increases the precision of time use data. Furthermore, as explained in Section 4.5.4, the DFM would control for further measurement errors in discretionary time, and its results presented overall similar patterns than those from the CVA model.

Finally, and related to the previous point, the estimation of a production function of human capital is subject to many assumptions and endogeneity issues, as has been extensively discussed in Chapter 4. Furthermore, the IV strategy proved unreliable in the context of this study, given the weakness of the available instruments. However, I have employed other state-of-the-art estimation strategies (Fixed Effects, Cumulative Valued-Added Models, and Dynamic Factor Models) used in the child development literature to address potential biases arising from endogeneity, which makes the overall conclusions from this chapter more robust.

5.8. Priorities for future work

This chapter represents, to the best of my knowledge, the first study of the short- and long-term effects of time poverty on child cognition. Consequently, I advocate for the necessity of further research on the drivers and effects of time poverty among children. This involves not only replicating this study in other contexts, but also considering the implications of time poverty for the design of time use research among children.

To this end there are at least four key aspects to consider: Firstly, there should be consistency in data collection methods for time use across the life course and ideally, across countries and settings.

Secondly, it is imperative to establish a more uniform definition of time poverty for both children and adults, accounting for transitions between different life stages. To achieve this, it will be necessary to develop a more objective conceptualisation and definition of discretionary time activities among children and adults, which also includes further methodological developments in how to measure them accurately.

Thirdly, it is essential to consider the potential impacts of time poverty on the well-being of both children and adults from the design of the data collection tools. This approach will enable the collection of key information (e.g. cognitive skills or mental health data) necessary for exploring the relationship between time poverty and various dimensions of human well-being.

Finally, and related to the previous point, more cohort and longitudinal studies should integrate the collection of time use data and well-being outcomes for both children and their parents. This expanded approach would enable the study of additional topics that were beyond the scope of this chapter due to data limitations. Examples include investigating the impact of child time poverty on labour market outcomes, studying the inter-generational transmission of time use patterns (e.g. whether parental time poverty influences children's time poverty), and assessing the effects of time poverty on parental and child health as well as cognition in the short- and long-term.

6. The short-term effect of Juntos on children's cognition and nutrition in Perú: a mediation analysis.

6.1. Introduction

Juntos is a conditional cash transfer (CCT) programme targeting poor households in the most disadvantaged municipalities in Peru. At the early phase of the programme between 2005 and 2009, Juntos transferred a bimonthly fixed amount of 100 Peruvian Soles (around 30 US Dollars or 10% of the monthly consumption of a poor household). From 2010, the transfer rose to 200 Peruvian Soles (Sanchez et al., 2020).

The conditionalities of the programme are associated with educational and health behaviours for children and pregnant women in the household: pregnant women must attend prenatal health check-ups; children under 5 years of age must attend health care centres for vaccination and growth checks, and school age children (above 5 years old) must attend 85% of school classes and must have a national ID (Díaz & Saldarriaga, 2019; Sanchez et al., 2020).To be eligible, apart from having a household income that falls below the poverty line, households must have children under 5 years old, school-age children or a pregnant woman (Díaz & Saldarriaga, 2019). Since its origin in 2005, Juntos has enrolled 736,000 families across 1,304 districts (67% of the total districts in the country, (Sanchez et al., 2020)).

Three prior studies have employed the YL dataset to assess the impact of the Juntos programme on the cognitive and nutritional status of children aged 5 to 9 years old, during the initial phase of Juntos between 2005-2009 (Andersen et al., 2015; Gaentzsch, 2020; Sanchez et al., 2020). These studies found heterogenous effects of the programme on children's nutritional outcomes and some positive effects on school outcomes.

For instance, Sanchez et al. (2020) found no discernible impact on stunting prevalence among the focal children. However, Andersen et al. (2015) reported a negative effect of the programme on stunting status among girls who had participated for more than two years and among boys who had participated for less than two years. These effects were statistically significant at the 10% level. Andersen et al. (2015) also identified a detrimental effect on girls' BMI and a positive effect on Height-for-age z-scores (HAZ) among boys. Finally, Gaentzsch's (2020) evaluation documented beneficial impacts on educational outcomes, including increased school enrolment, higher rates of primary school completion and greater progression to secondary education across the entire sample of children[7].

Despite some potential positive impacts of the programme on nutritional status and on school participation documented in these previous evaluations, the evidence on cognitive outcomes is less encouraging. Andersen et al. (2015) found no association between programme participation and grade attainment or receptive vocabulary. Similarly, Gaentzsch (2020) found no effect on vocabulary development and a negative effect on mathematical test scores among primary- and secondary- school children. Sanchez et al. (2020) initially found a positive effect on language scores, but only among the younger siblings of the index children, who were initially exposed to the programme during their first four years of life. However, this effect became non-significant when further robustness checks were carried out.

In the Peruvian case, the non-significant impact of the programme on cognitive outcomes contrasts with existing evidence which suggests that improvements in nutrition are expected to be associated with improvements in cognition among children (Alderman & Fernald, 2017; Andersen et al., 2015). Moreover, there is also evidence supporting the positive influence of parental investments, encompassing provisions such as food, clothing, medications and school uniforms, on cognitive development in Peru and other LMICs such as Ethiopia and India (Attanasio et al., 2017; Attanasio, Meghir, et al., 2020).

Therefore, in this chapter, I will study the effects of the Juntos programme on child nutrition and cognition, and attempt to better understand the extent of the relationship between these two dimensions of human capital. Specifically, I will utilise a mediation analysis framework to explore whether some of the programme's influence on children's cognition at the age of 8 can be attributed to the programme's impact on children's nutrition as an intermediary pathway.

Moreover, these three prior evaluations of the Juntos programme can be regarded as short-term evaluations of its initial expansion between 2005 and 2010, as they

[7] The evaluation by Gaentzsch (2020) studied transition to secondary school among the YL Older Cohort. As detailed in Section 4.3, this thesis only considers the Younger Cohort.

examined only one post-treatment period. This corresponds to information about programme participation in Round 3, when children were 8 years old. Additional information regarding programme participation was gathered in subsequent rounds of the YL study. This included children who joined the programme at later stages than the initial treated group, particularly during rounds when the children transitioned to secondary school (at age 12) and during their middle adolescence phase (at age 15).

Expanding the evaluation of the Juntos programme to include Rounds 4 and 5 enables the examination of the programme's longer-term effects, extending into middle adolescence. Additionally, it permits the investigation of the programme's impact on cohorts of children who started their participation at later stages and ages than those included in the initial expansion. Furthermore, given that the programme was initially deployed in the most deprived communities of the country (Sanchez et al, 2020), and that Peru experienced a rapid economic development during 2005 and 2014 (Rossini, 2015), these later cohorts may exhibit different baseline socioeconomic characteristics compared to those treated earlier. Consequently, their outcomes due to programme participation may differ. The long-term effects of the Juntos programme will be the subject of Chapter 7.

The rest of this chapter is structured as follows: first, I will introduce a methodological framework that includes a brief overview of the canonical difference-in-differences (DID) method for the 2 periods-2 groups, the causal mediation analysis framework, and their integration within this chapter. Then, I will outline the sample to be used in this chapter and provide some descriptive statistics. Following that, I will present the results, discussion and concluding remarks.

6.2. Methodological framework

6.2.1. The 2 groups-2 periods difference-in-differences method

The simplest DID design refers to two periods (before and after the intervention) with two groups (one treated and one never treated) (Wing et al., 2018). In such a case, the estimation of the Average Treatment Effect (ATE) can be done by calculating the mean outcomes of the groups before and after the introduction of the programme. Then, the difference of the difference between groups can be computed as follows:

$$ATE = E\left[Y_{i,2} - Y_{i,1} \middle| D_i = 1\right] - E[Y_{2,i} - Y_{1,i}|D_i = 0]$$

(26)

Where $D_i = 1$ represents the treatment group, $D_i = 0$ the untreated group; $Y_{i,2}$ the period after the treatment and $Y_{i,1}$ the pre-treatment period. In practice, researchers estimate the following linear model of the canonical 2x2 DID regression, which allows one to obtain standard errors for the treatment effect:

$$Y_{it} = \gamma_0 + \gamma_1 T_i + \gamma_2 Post_t + \gamma_3 T_i \times Post_t + \varepsilon_{it}$$

(27)

Where Y_{it} is the outcome of interest, T_i is equal to 1 if the individual i is treated between period 1 and 2, and 0 otherwise. The variable $Post_t$ is equal to 1 in the period following the introduction of the treatment, and 0 otherwise. The causal effect of the programme is given by γ_3, which is linked to the interaction term $T_i \times Post_t$.

One key assumption of the DID model is the *parallel trends assumption*, which states that in the absence of treatment, both groups would have undergone the same trajectory of outcome changes over time. In other words, is assumes that the difference in mean outcomes between the groups would have remained constant over time (de Chaisemartin & D'Haultfœuille, 2022).

This condition is not testable when there is only one pre-treatment period, as in our case (Round 2)[8]. This assumption may be violated when pre-treatment observable covariates differ by group, and these characteristics are linked to the dynamics of the outcome variable (Abadie, 2005). Under those circumstances, approaches such as adjusting for these observable covariates, employing a matching techniques to balance pre-treatment characteristics between groups before estimating the DID (Heckman et al., 1998; Lindner & McConnell, 2019) or utilising a semiparametric DID (Abadie, 2005), have been utilised in the applied literature. However, it is important to

[8] One common way to partially test the common trends assumption when there is more than one pre-treatment period is to plot the mean outcomes by groups and time, and visually examine whether they look parallel (Wing et al., 2018).

note that recent methodological papers have raised challenges to the combination of matching with DID, suggesting that matching on baseline covariates does not guarantee the complete elimination or reduction of bias (Lindner & McConnell, 2019; O'Neill et al., 2016).

As I will discuss in Section 6.4, children participating in Juntos had different socioeconomic characteristics than those who were never treated. This divergence in socioeconomic profiles could potentially challenge the validity of parallel trends assumption. Hence, I will control for a vector of covariates that are associated both with treatment participation and outcome evolution. This mirrors the approach adopted by Sanchez et al. (2020) in their evaluation of the Juntos programme, and aligns with the approaches employed in previous papers that integrated DID with mediation analysis (Lugo-Palacios et al., 2023; Pace et al., 2022). As I will elaborate in the next section, controlling for these characteristics is also relevant to adhere to additional assumptions made in the causal mediation analysis framework.

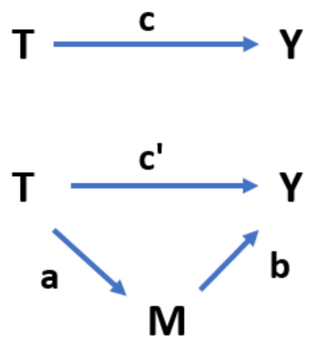
6.2.2. Mediation analysis

6.2.2.1. Baron and Kenny's mediation framework

The most common method for causal mediation is the framework proposed by Baron & Kenny (1986), B&K hereafter. Figure 1 shows this framework for the case of a single mediator (MacKinnon et al., 2007; Shrout & Bolger, 2002).

The upper part of Figure 8 shows the total effect of programme T on outcome Y. This effect can be estimated using regression analysis, where the coefficient c provides an estimate of the overall or total impact of the programme. This would be, in my case, the coefficient γ_3 in Equation 27.

Figure 8. Basic mediation model for the case of one single mediator



The bottom part of the figure shows the causal pathway from T to Y when an intermediate variable M is considered. In this case, the coefficient c' is called the "direct effect" of T on Y. It is the effect that is obtained when the mediators are entered into the model (MacKinnon et al., 2007; Shrout & Bolger, 2002). T will also have an impact on the mediator M. Coefficient a shows the change of the mediator M induced by T, that is, being exposed to the programme T will change the mediator in a units. For instance, if T is a CCT programme and M are HAZ scores, then a would be the improvement in HAZ (measured in sd) due to programme participation. Then, the impact of the mediator on Y will be measured by the coefficient b. Its interpretation is that a unit change in M will change Y in b units. If outcome Y is test scores, one sd increase in HAZ would increase test scores in b marks.

Finally, the indirect effect of T on Y is measured by the product $a \times b$ (MacKinnon et al., 2007; Shrout & Bolger, 2002). Following the example given, if children exposed to the CCT programme experience an increase of 0.5 HAZ sd (coefficient a), and each additional sd increase test scores by 5 marks (coefficient b), then the effect of the CCT programme on test scores *mediated* by its effect on HAZ would be an increase of 0.5×5 = 2.5 marks.

In practice, this model is estimated using a Linear Structural Equation Model (LSEM) in the following form (ignoring, for the moment, covariates X_i) (MacKinnon et al., 2007):

$$Y_i = \alpha + cT_i + e_i$$

(28)

$$M_i = \delta + aT_i + \mu_i$$

(29)

$$Y_i = \beta + c'T_i + bM_i + \epsilon_i$$

(30)

Equations 28 and 29 give an estimate of the effect of the treatment T_i (in my case, the Juntos programme) on outcomes Y_i and mediators M_i, respectively. It is typically claimed that both c and a must be statistically significant (MacKinnon, 2000). A statistically significant effect of c would indicate an impact of the treatment on the outcome intended to be mediated. Similarly, a statistically significant effect for a would imply that the programme also influences the mediator M_i. Consequently, the total effect c could be potentially mediated trough M.

However, as pointed out by MacKinnon (2000) and Mackinnon et al. (2007) there may be cases in which c is not significant and yet mediation exists. One possible explanation for this phenomenon could be the presence of "inconsistent" mediation, in which multiple mediators M_i have different signs, resulting in a not statistically significant total effect c.

A more general justification for weak or not statistically significant coefficients c may be attributed to distal mediation processes (Shrout & Bolger, 2002). A proximal process occurs when the impact of treatment T_i on Y_i is observed over a relatively short period of time. In such cases, Equation 28 would likely yield a strong and statistically significant coefficient c. However, in distal processes the magnitude of the effect can diminish due to three potential factors: a) a substantial portion of the effect is transmitted through mediating variables, b) it might be affected by competing causes (as elucidated earlier in "inconsistent mediation" models), or c) it could be susceptible to the influence of other random factors.

As previously discussed, the relationship between cash transfers and child cognition and nutrition is likely to be of a distal nature. Consequently, the coefficient c does not necessarily need to achieve statistical significance in my case, which allows for the non-significant effect of Juntos on verbal scores identified in two prior evaluations (Andersen et al., 2015; Sanchez et al., 2020).

6.2.2.2. Imai's et al general causal mediation framework

Despite the popularity of the B&K approach, it has faced certain criticisms. A common critique pertains to the linearity of the SEM assumed in Equations 28-30. While the model works well with continuous outcomes and mediators, deviations from this framework pose additional challenges in terms of whether the product of the coefficients $a \times b$ represents an accurate estimate of the mediation effect. That would be the case when non-continuous mediators or outcomes are used, such as in the case of binary or count data, therefore requiring the use of Probit, Logit or Poisson models (Hicks & Tingley, 2011).

Imai et al. (2010, 2011) have proposed a general framework (Imai's framework hereafter) for causal mediation analysis that allows for a non-parametric estimation of the Average Causal Mediation Effect (ACME). This means that it can be used without being restricted to a particular functional form of Equation 28-30 or relying on distributional assumptions. The algorithm proposed by the authors can be summarised in the following steps (Hicks & Tingley, 2011; Keele et al., 2015):

- Step 1: Fit models for the outcome and mediator variables. In the case of continuous mediators and outcomes, these models align with Equations 29 and 30 in the B&K framework. In other cases, alternative models such as Probit, Logit and Poisson, among others, can be fitted.
- Step 2: Drawing from the fitted mediator model (the model with the estimated values of $\hat{\delta}, \hat{a}$), two sets of predicted mediator values are calculated, both for the cases of treatment and no treatment. In the case of a continuous mediator, this involves utilising the fitted model from Equation 29 to estimate the predicted values of the mediator $\widehat{M_i}$ for each observation i when $T_i = 1$ and $T_i = 0$, that is the predicted $\widehat{M_i}(T_i = 1)$ and $\widehat{M_i}(T_i = 0)$.

- Step 3: Using the fitted outcome model (Equation 30 with the estimated coefficients $\hat{\beta}, \widehat{c'}, \hat{b}$) and the predicted values $\widehat{M_i}$ from the previous step, we calculate the imputed potential outcomes *under treatment* $T_i = 1$. This means to impute the potential outcome $\widehat{Y_i}\left(\widehat{M_i}(T_i = 1), T_i = 1\right)$ and the counterfactual outcome $\widehat{Y_i}\left(\widehat{M_i}(T_i = 0), T_i = 1\right)$
- Step 4: The ACME is then estimated as the average of the differences $\widehat{Y_i}\left(\widehat{M_i}(T_i = 1), T_i = 1\right) - \widehat{Y_i}\left(\widehat{M_i}(T_i = 0), T_i = 1\right)$

This process is repeated *N* times to account for the uncertainty introduced in the model due to the use of predicted variables in Steps 3 and 4. This repetition helps obtain final estimates of the ACME along with their associated standard errors.

6.2.2.3. Sequential ignorability assumption

Imai et al. (2011) and Imai & Yamamoto (2013) have demonstrated that causal mediation, whether using their algorithm or the B&K framework, relies on the assumption of sequential ignorability. This assumption is expressed by the following two equations:

$$\{Y_i(t, m), M_i(t')\} \perp\!\!\!\perp T_i | X_i = x \tag{31}$$

$$Y_i(t', m) \perp\!\!\!\perp M_i(t) | T_i = t, X_i = x \tag{32}$$

Where, X_i is a vector of covariates, and $0 < \Pr(T_i = t | X_i = x)$, $0 < \Pr\,(M_i = m | T_i = t, X_i = x)$, for $t = 0{,}1$ and all x and m in the support of X_i and M_i (that is, for any value of x, t, t' and m).

The sequential ignorability assumption states that, conditional on observed covariates, treatment assignment is considered ignorable, meaning that it is assumed to be statistically independent of outcomes and mediators (Equation 31). Then, Equation (32) states that the observed mediator is ignorable when considering treatment assignment and observed covariates. In simpler terms, it suggests that there are no unmeasured covariates that may confound the relationship between the outcome and the mediator (Pace et al., 2022).

It is worth noting that, in order to comply with the sequential ignorability assumption, the vector of covariates X_i must be incorporated into the models for both the outcome and the mediator, regardless of whether one is utilising Imai's or B&K framework. Furthermore, Imai et al. (2011) have demonstrated that under this assumption, the product $a \times b$ in the B&K framework provides an asymptotically consistent estimate of the ACME, provided that the linearity of the SEM is maintained.

Imai et al. (2010) examine the plausibility of the two conditions of the sequential ignorability assumption. In randomised trials, the first condition is usually fulfilled. In observational studies, it is common practice to collect relevant pre-treatment characteristics to enhance the reliability of the condition after adjusting for those factors. It is worth noticing that this aspect is already considered in the DID strategy, where I also control for pre-treatment characteristics that might affect treatment participation.

However, according to Imai et al. (2010), satisfying the second condition is more challenging. There could be unobservable variables that confound the relationship between the outcome and the mediator. In my specific case, as mentioned earlier, the relationship between children's nutrition and cognition might be endogenous. In such cases, employing instruments can enhance the credibility of the second part of the sequential ignorability assumption. In this scenario, Imai et al. (2011) have demonstrated that with a set of instruments Z_i and a vector of X_i observed covariates, the outcome and mediator models can then be written by:

$$M_i = \delta + aT_i + \lambda Z_i + \varphi X_i + \mu_i \tag{33}$$

$$Y_i = \beta + c'T_i + bM_i + \epsilon_i \tag{34}$$

Thus, the mediator is a function of the covariates, the treatment, and the instruments. Similar to the case without instruments, Imai et al. (2011) demonstrate that, under linearity, the product of the coefficients $a \times b$ provides a valid estimate of the ACME.

6.2.3. Final functional form.

Building on the previous evaluation of the Juntos programme conducted by Andersen et al. (2015), Gaentzsch (2020) and Sanchez et al. (2020) using a DID framework, as well as the studies by Anselmi et al. (2017), Lugo-Palacios (2023) and Pace et al. (2022) that integrated a linear SEM with DID, the latter also in the context of a cash transfer, I will employ the following outcome and mediator equations:

$$Y_{it} = \gamma_0 + \gamma_1 T_i + \gamma_2 Post_t + \gamma_3 T_i \times Post_t + \gamma_4 X_{i,t} + \varepsilon_{it} \tag{35}$$

$$M_{it} = \alpha_0 + \alpha_1 T_i + \alpha_2 Post_t + \alpha_3 T_i \times Post_t + \alpha_4 X_{i,t} + \mu_{it} \tag{36}$$

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 Post_t + \beta_3 T_i \times Post_t + \beta_4 M_{i,t} + \beta_5 X_{i,t} + \epsilon_{it} \tag{37}$$

Where the variables and coefficients have been defined before. Therefore, under the sequential ignorability assumption, the effect of the Juntos programme on the mediators (nutritional outcomes) will be given by α_3, while the direct effect of Juntos on the outcomes (cognitive outcomes) will be provided by β_3. In the B&K framework, the product $\alpha_3 \times \beta_4$ will provide an estimate of the ACME. The standard errors will be clustered at the YL's cluster level and computed by 500 bootstrapping replications.

Following Lugo-Palacios (2023) and Pace et al. (2022), robustness checks will be carried out using the Imai et al framework. In that case, Equations 36 and 37 will be used in the initial steps of the algorithm outlined in Section 6.2.2.2, to derive their estimate of the ACME. In addition, the estimate of the ACME and standard errors are computed using 500 simulations of this algorithm.

As mentioned earlier, the second part of the sequential ignorability assumption could be violated if there exists endogeneity between nutrition and cognition. This might occur, for example, if parental investments in food expenditures or medicines respond to children's cognition, either to offset or enhance their initial cognitive status. However, as previously discussed, evidence from Peru suggests that parental investments do not respond to children's cognition and health (Attanasio et al., 2017). Hence, concerns about endogeneity arising from reverse causality are relatively low in this context.

Nonetheless, as and additional robustness check, I will estimate the following augmented Equation 38 with a set of instruments $Z_{i,t}$. To do so, I will adopt the approach outlined by Imai et al. (2011) and Pace et al. (2022) to estimate the following first-stage equation for the mediators:

$$M_{it} = \alpha_0 + \alpha_1 T_i + \alpha_2 Post_t + \alpha_3 T_i \times Post_t + \alpha_4 X_{i,t} + \alpha_5 Z_{i,t} + \varepsilon_{it} \tag{38}$$

Where $Z_{i,t}$ represents a set of instruments. I will employ the same instruments utilised for Chapter 1 i.e. variables of indexes of prices and wages at the level of the community, and whether the household has experienced a series of economic, environmental and family decomposition shocks. These instruments are detailed in Table A.1 in appendix 11.1.

6.3. Data

I use data from Round 2 (age 5) and Round 3 (age 8) of the YL dataset for Peru, as previously described in Section 4.3.

The primary outcomes for this analysis will be verbal and mathematical skills, while the mediators considered will include stunting status, BMI z-scores and HAZ scores. As covariates, I consider the following socioeconomic characteristics of the children and their household: gender, language, age and ethnicity of the child, a dummy variable indicating whether the child was the first born child of the family; age in months, gender and education of the household’s head, number of other children in the household (aged less than 18 years old), number of other adults in the household (aged 18+), and a dichotomous indicator for area of residence (1=urban, 0= rural).

For the case of the verbal and mathematical test scores, I also control for whether the test was applied in the mother tongue of the child. As *pre-treatment* covariates, I include children's time use (sleeping, work for the household, education, play and leisure[9]) and total expenditure per capita. These variables are included solely as pre-treatment covariates since they are likely to be affected by programme participation, thereby avoiding issues related to "bad controls" (Zeldow & Hatfield, 2021).

6.4. Descriptive statistics

Comprehensive descriptive statistics for the overall sample have been provided in Section 5.4. In this subsection, I present the descriptive statistics separately for the treated and never-treated groups.

At the age of 5, children participating in Juntos scored lower math and verbal skills than never treated children. Regarding nutritional outcomes, there was no statistically significant difference in BMI-for-age z-scores between the two groups. However, the prevalence of stunting was notably higher among children in the Juntos programme (61%) compared to those who were never treated (25.4%). Additionally, HAZ scores were lower for children in Juntos (-2.3) in contrast to their never-treated peers (-1.3).

It is also important to highlight that for both treated and never-treated children, the BMI is greater than zero, indicating higher weight, while the HAZ is lower than zero, indicating lower height, both compared to the WHO reference levels. However, in the case of BMI, it still falls within the normal threshold (between -2 and 1). Conversely, for HAZ among children in Juntos, the average HAZ lower than -2 is considered stunting status, which explains the higher prevalence of stunting among these children.

In terms of socioeconomic characteristics, fewer children in Juntos reported Spanish as their mother tongue than never-treated children (42.2% versus 95.2%). Moreover, fewer treated children were the first-born children in their household, treated children lived in households with a larger number of other children but a lower number of other adults, and more frequently resided in rural areas.

Additionally, treated children belonged to poorer households, as measured by the total household expenditure per capita. In most households, the household head was female (more than 90% in both groups). No statistically significant differences between

[9] Time working outside the household was not included given that in Round 2 all households reported zero paid child labour.

the groups were found in terms of the age of the household head and children's gender.

Finally, I also document different patterns of time use. Children in Juntos treated households slept more (40 minutes), contributed more to household chores (60 minutes), but spent less time on education (-69 minutes) and less time on recreational activities (-32 minutes). It is important to note that none of the households reported that children engaged in paid work outside the household at the age of 5.

Table 8. Descriptive statistics for treated and never treated samples, Round 2 (age 5)

Variables	In Juntos (N=262)	Never treated (N=1272)	Difference
Verbal scores (in standard deviations)	-0.723	0.154	-0.878***
Mathematical scores (in standard deviations)	-0.326	0.102	-0.428***
Stunting status (in percentage, 1=yes, 0 otherwise)	0.611	0.254	0.357***
BMI-for-age z-scores	0.655	0.632	0.024
Height-for-age z-scores	-2.264	-1.334	-0.929***
Child language (in percentage, 1 = Spanish, 0 = other)	0.427	0.952	-0.524***
Child sex (in percentage, 1 = girls, 0 = boys)	0.515	0.497	0.018
Birth order (in percentage, 1 = being first born children in the household)	0.26	0.408	-0.148***
Household head's age	37.782	38.411	-0.629
Household head's sex (in percentage, 1 = male, 0 = female)	0.077	0.095	-0.018
Number of other children (aged < 18)	2.683	1.679	1.004***
Number of other adults (18+)	2.432	2.663	-0.232***
Area of residence (urban =1, 0 = rural)	0.168	0.812	-0.644***
Total expenditure per capita (in 100's of soles)	0.805	1.778	-0.973***
Sleeping (in hours)	12.85	12.184	0.666***
Housework (in hours)	1.861	0.858	1.004***
Paid work (in hours)	0	0	0
Education (in hours, time at school+ studying at home)	4.87	6.014	-1.143***
Play and leisure (in hours)	4.418	4.945	-0.526***

Notes:
P-values for the differences between groups calculated from a t-test for equality of means (* p<0.10, ** p<0.05, *** p<0.01)

6.5. Regression results

6.5.1. Zero order conditions

In this subsection, I present the results for the effects of the Juntos programme on outcomes and mediators. The regressions were estimated using Equations 35 and 36 in Section 6.2.3, and the results are presented in Table 9 below.

The results show that the **programme had a statistically significant negative effect on verbal scores for the entire sample and for girls**. The effect for boys was also negative, smaller in magnitude and not statistically significant. **A negative and statistically significant result was found for mathematical test scores for the entire sample, and separately for girls and boys.**

Regarding the effect of the programme on the mediators, **no significant impact on stunting status or HAZ scores was observed**. The only statistically significant effect of Juntos on the mediators was observed in the case of BMI. **The programme had a negative impact on BMI for the entire sample, and separately for girls and boys**. Among boys, the effect was slightly more pronounced, with a decrease of -0.36 sd compared to -0.23 sd among girls.

It is important to emphasize that, as outlined in Section 6.2.2.1, the total effect of the programme on cognition does not necessarily have to be statistically significant for the existence of mediation (MacKinnon et al., 2007; Shrout & Bolger, 2002). Nevertheless, the relationship between the programme and the mediators still needs to attain statistical significance. If the programme has no discernible impact on the mediator, it is improbable that the mediator plays a causal role in the pathway from Juntos' participation to child cognition. Therefore, the only plausible mediator in this case is BMI. Hence, in the next subsection, I explore the relationship between BMI and child cognition.

Table 9. Effect of the Juntos programme on outcomes and mediators

Outcome	All	Girls	Boys
Verbal scores	-0.25*** (-0.40, -0.11)	-0.40*** -0.59, -0.19)	-0.11 (-0.31, 0.09)
Mathematical scores	-0.48*** (-0.66, -0.30)	-0.55*** (-0.80, -0.29)	-0.40*** (-0.66, -0.15)
Stunting status	-0.09 (-0.22,0.04)	-0.07 (-0.20, 0.05)	-0.11 (-0.27,0.05)
BMI-for-age z-score (BMI)	-0.29*** (-0.46, -0.13)	-0.23** (-0.46, -0.01)	-0.36*** (-0.60, -0.11)
Height-for-age z-scores (HAZ)	0.07 (-0.07, 0.22)	-0.05 (-0.19,0.10)	0.20 (-0.05,0.46)
N	1534	767	767

Notes:
95% confidence intervals in parenthesis
P values: * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see Section 6.3)

6.5.2. The effect of BMI of child cognition.

Table 10 presents the effects of BMI on child cognition. This corresponds to coefficient β_4 in Equation 37. The coefficients are relatively small but statistically significant for the entire sample and for girls. For example, a one standard deviation increase in BMI among girls was associated with a 0.12 standard deviation increase in girls' verbal scores.

Table 10. Effect of BMI on child cognition

Outcome	All	Girls	Boys
Verbal	0.07*** (0.04, 0.09)	0.12*** (0.09, 0.16)	0.01 (-0.03, 0.05)
Math	0.06*** (0.02, 0.09)	0.08*** (0.04, 0.12)	0.03 (-0.01, 0.07)
N	1534	767	767

Notes:
95% confidence intervals in parenthesis
P-values: * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see Section 6.3)

Considering the lack of an effect of BMI on test scores in boys, I anticipate a null mediated effect for boys. In contrast, I expect a positive and statistically significant effect among girls. This will be tested in the following section.

6.5.3. Mediation analysis results

Table 11 displays the results of the mediation analysis using BMI z-scores as a mediator. The outcomes are presented separately for mathematical and verbal scores, and categorised by gender. In the "total" column, the total effect of the Juntos programme on the outcome is displayed. In the B&K approach, these results are the same as those obtained in Table 9. The direct effect is the one estimated when the mediator variable is included, denoted as β_3 and estimated using Equation 37.

Table 11 shows that **the direct effects are slightly lower than the total effect, confirming that some of the total effect was mediated through the intermediate variable BMI.**

All the intermediate effects are negative, which was expected considering the negative effect of the Juntos programme on BMI (as shown in Table 9) and the positive impact of BMI on verbal and mathematical skills (as shown in Table 10). In other words, higher BMI was associated with a positive effect on child cognition. **Therefore, given that Juntos had a negative effect on BMI, then the mediated effect of the programme through BMI was also negative.**

As expected, the mediated effects were significant only for the entire sample and among the subsample of girls. Due to the non-statistical significance of BMI on verbal or math skills among boys, all the effects among them were also not statistically significant. However, for girls, the effects were only significant at the 10% level and were of low magnitude (less than -0.03 sd). The proportions mediated, that is, the ratio between the total and the mediated effects, for the entire sample and girls were around 8% for verbal skills and 3% for mathematical skills.

Table 11. Results of the mediation analysis, B&K framework

Outcome	All	Girls	Boys
Verbal			
Mediated effect	-0.02*** (-0.03, -0.01)	-0.03* (-0.06, .00047)	-0.0033 (-0.02, .01183)
Direct effect	-0.23*** (-0.38, -0.09)	-0.36*** (-0.56, -0.16)	-0.11 (-0.31, .09805)
Total effect	-0.25*** (-0.4, -0.11)	-0.40*** (-0.59, -0.19)	-0.11 (-0.31, 0.09)
Proportion mediated (mediated / total)	7.6	7.6	3.0
Math			
Mediated effect	-0.02** (-0.03, -.0036)	-0.02* (-0.04, .0024)	-0.01 (-0.03, .0049)
Direct effect	-0.47*** (-0.65, -0.28)	-0.53*** (-0.78, -0.27)	-0.39*** (-0.65, -0.14)
Total effect	-0.48*** (-0.66, -0.3)	-0.55*** (-0.8, -0.29)	-0.4*** (-0.66, -0.15)
Proportion mediated (mediated / total)	3.4	3.6	2.8
N	1534	767	767

Notes:
B&K: Baron & Kenny's (1986) causal mediation framework
95% confidence intervals in parenthesis
P-values: * $p<0.10$, ** $p<0.05$, *** $p<0.01$
Controlled for relevant individual and household socioeconomic characteristics (see Section 6.3)

6.5.4. Robustness checks

Table 12 presents the results estimated using the Imai's and Imai + IV approach. These models were estimated using Equations 36 and 37 as the mediator and outcome models in the Imai's approach and Equations 38 and 37 in the Imai + IV approach. As previously discussed, I have included economic and environmental shocks, an index of prices, and an index of wages as instruments (detailed in Appendix 11.1). The first notable point is that the instruments used were not strong enough, similar to what was found in Chapter 5. None of the F-statistics from the first-stage regression were greater than 10. This indicates that the IV results are not sufficiently reliable.

However, the results from these two methods are, in most of the cases, virtually identical to those obtained in Table 11 using the B&K approach in terms of magnitude, direction of the effects, significance, and proportion mediated. The main difference is that in the Imai's and Imai + IV approaches the mediated effects for mathematical test scores were not statistically significant, while in the B&K approach they were significant at the 10%. It is worth to notice that the similar results in terms of coefficients between approaches was expected because both my outcome and mediator variables were continuous (Hicks & Tingley, 2011).

Table 12. Results of the mediation analysis, Imai's et al. and IV frameworks

	All		Girls		Boys	
	Imai	Imai + IV	Imai	Imai + IV	Imai	Imai + IV
Verbal						
Mediated	-0.02*** (-0.04, - 0.01)	-0.02** (-0.04,-0.01)	-0.03* (-0.07, .00318)	-0.03* (-0.07 ,0.0003)	-0.0035 (-0.02, 0.01)	-0.004 (-0.02 ,0.01)
Direct	-0.24*** (-0.39, - 0.08)	-0.24** (-0.39,-0.08)	-0.36*** (-0.57, -0.16)	-0.36*** (-0.57 ,-0.16)	-0.11 (-0.32, 0.1)	-0.11 (-0.32 ,0.1)
Total	-0.26*** (-0.41, - 0.1)	-0.26*** (-0.41,-0.11)	-0.39 *** (-0.59, -0.18)	-0.4*** (-0.59,-0.19)	-0.12 (-0.32, 0.1)	-0.12 (-0.33 ,0.1)
Proportion mediated (mediated / total)	7.7	8.1	7.6	8	2	2.4
Math						
Mediated	-0.02** (-0.03 -.0041)	-0.02** (-0.03,-0.005)	-0.02 (-0.05, .002)	-0.02 (-0.05 ,0.0002)	-0.01 (-0.03, .0045)	-0.01 (-0.04 ,0.005)
Direct	-0.47*** (-0.64 -0.28)	-0.47*** (-0.64,-0.28)	-0.53*** (-0.78, -0.28)	-0.53*** (-0.78,-0.28)	-0.4 *** (-0.64, -0.15)	-0.4** (-0.64,-0.15)
Total	-0.48*** (-0.66 -0.3)	-0.48*** (-0.66,-0.31)	-0.55*** (-0.79, -0.3)	-0.55*** (-0.78,-0.3)	-0.41 *** (-0.66, -0.16)	-0.41** (-0.67,-0.16)
Proportion mediated (mediated / total)	3.5	3.6	3.6	3.8	2.7	3
F-statistic for the first stage regression	NA	8.5	NA	3.8	NA	4.4
N	1534		767		767	

Notes:
Imai: Imai et al (2011) causal mediation framework. IV: instrumental variables. 95% confidence intervals in parenthesis. P-values: * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see Section 6.3). The first stage F-statistics is common to both verbal and mathematical results (see Equation 38)

6.6. Discussion and conclusions

In this chapter, I conducted a mediation analysis to examine the short-term effects of the Juntos programme on children's cognition. I consider child nutrition as an intermediate pathway to cognitive outcomes, as it is likely to be influenced by the programme and the links between nutrition and cognition are well documented (Alderman & Fernald, 2017). I combined various evaluation techniques, including DID, causal mediation analysis (both the Baron and Kenny's (1986) and Imai's et al. (2010, 2011) frameworks) and instrumental variables. To the best of my knowledge, this represents the first formal study of the intermediate factors that explain the effects of the Juntos programme on child cognition in Peru.

In a first step, I studied the effect of the programme on cognitive outcomes. I found a negative effect of Juntos on both verbal and math test scores. The results on math test scores were statistically significant for both girls and boys, while the effects on verbal scores were significant only among the subsample of girls.

My findings in terms of cognitive outcomes contrast with three previous evaluations that also reported negative but statistically insignificant effects of the programme on verbal scores (Andersen et al., 2015; Gaentzsch, 2020; Sanchez et al., 2020). However, I found similar results as Gaentzsch’s (2020) evaluation that identified negative and statistically significant effects on math test scores.

Therefore, the main distinction between my evaluation and previous works, lies in the statistical significance of these total effects on cognitive outcomes (and, as previously explained, the examination of the intermediate factors that may explain these effects on cognitive outcomes). To enhance the reliability of the statistical significance of my results, I employed two key methods. First, I clustered standard errors at the YL cluster level, and second, I employed bootstrapping techniques with 500 replications. These measures were implemented to better account for potential sources of variation and uncertainty in the evaluation. It is noteworthy that the estimated coefficients exhibit consistency across all three models employed (B&K, Imai’s and Imai + IV), as presented in Tables 11-12.

In terms of nutritional outcomes, I identified statistically significant effects of Juntos only on BMI-for-age z-scores. No significant impact was observed on stunting status and HAZ scores. The three previous evaluations of Juntos have reported inconsistent findings regarding the programme's effects on nutrition, as discussed in the introduction to this chapter. Nutritional effects appear to vary based on children's age, the statistical methodology employed, and the duration of exposure to the programme (for instance, for the index children or for their younger siblings).

Specifically for BMI, my results were negative and statistically significant for the entire sample, and among boys and girls, with higher effects for boys than for girls. In contrast, the evaluation of the Juntos programme by Andersen et al. (2015) found statistically significant negative effects among the entire sample and girls, but the effect was not statistically significant for boys.

The higher reduction of BMI among boys in my evaluation can be attributed to the presence of gender disparities in this indicator at the baseline (Round 2, age 5). Specifically, girls who later enrolled in Juntos exhibited an average BMI of 0.47 standard deviations, with a range from -2.05 to 1.98. Moreover, 25% of them were categorised as overweight or obese (more than 1 sd of the BMI-z-scores). In contrast, boys who later joined the Juntos programme displayed an average BMI almost double that of girls (0.85), with a range from -1 to 3.9. Furthermore, 37% of them fell into the category of overweight or obese. Consequently, it is expected that if the programme were to reduce BMI among all children, the effect would be more pronounced among boys due to their initially higher BMI levels.

Since the programme’s effect on nutritional variables was only significant in BMI, I conducted a mediation analysis to determine if BMI served as a potential pathway through which Juntos influences child cognition.

Therefore, the second step of the mediation analysis was to test the relationship between BMI and child cognition in our sample. In Table 10, I show that the effect of BMI on verbal and math test scores was positive and statistically significant at the 1% level across the entire sample, driven by the effect among the subsample of girls. Once again, given that most of the girls had a normal BMI at the baseline (0.5 sd) and only 25% of them were considered overweight or obese (more than 1 sd of BMI z-scores), marginal increases in BMI, without crossing into the overweight or obese category, were expected to have a positive effect on their cognition. This result is indeed consistent with previous literature that has found a positive correlation between normal weight and child cognition (Hjorth et al., 2016; Patraca-Camacho et al., 2022), with obesity or overweight being negatively correlated with cognitive outcomes (Galvan et al., 2014; Hansen et al., 2022; Li et al., 2008)

The third step of my mediation analysis involved combining the previous two results. Thus, given that BMI had a positive effect on test scores, which was statistically significant only among girls, and considering that Juntos had a negative impact on BMI, I expected that part of the negative effect of the programme on girl's cognition was mediated by its effect on nutrition. This was confirmed by the negative sign of the coefficients for the mediated effects, as shown in Tables 11 and 12.

Concerning the impact of Juntos on verbal skills, BMI served as a mediator for approximately 7.6% to 8% of the total effect of the programme among the entire sample and among girls. This mediated effect was statistically significant at the 1% level among the entire sample but only at the 10% level for girls. These results were robust to alternative model specifications. In terms of the effects of the programme on mathematical skills, BMI mediated approximately 3% of the effects among both the entire sample and girls. These results were statistically significant at the 5% level for the entire sample, and at the 10% level for girls in the B&K framework. However, they were not statistically significant in the robustness checks carried out using the Imai's and Imai + IV frameworks.

The effects of the programme exclusively through the BMI channel can be explained by the responsiveness of BMI to short-term changes in nutritional inputs. Compared to the other variables initially considered as potential mediators i.e. stunting status or HAZ, BMI is more susceptible to short-term fluctuations and thus may be more responsive to short-term interventions, habits or changes in nutrition inputs (Caballero, 2004; Gonzalez-Suarez et al., 2009; Siegrist et al., 2013). Therefore, a reduction in BMI for a sample of girls with already normal weight could be due to short-term reductions in nutrition inputs, which, in the Peruvian context, seemed to be enough to have a negative impact on their cognition.

Comprehending the negative effects of the programme on girls' cognitive development through its influence on nutrition holds significance for policymaking. This responsiveness of BMI to short-term interventions makes these negative effects potentially easily reversible; for instance, short-term nutritional interventions accompanying the Juntos programme may have a positive effect on both nutrition and cognition.

Furthermore, any CCT programme aimed at enhancing children's health and cognitive abilities should be carefully designed to avoid potential adverse effects on these dimensions, particularly those that might exacerbate gender disparities. Recent literature has highlighted specific areas and strategies for the designing of more gender- and child-sensitive CCT programmes. These include enhancements in targeting and delivery mechanisms, a reconsideration of the conditions associated with these programmes, and their complementation with awareness campaigns to enhance overall outcomes (Esser et al., 2019). Implementing these strategies in the Juntos programme may contribute to the amelioration of nutritional and cognitive outcomes, and a reduction in gender disparities among children in Peru.

In Chapter 7, I will examine the long-term effects of the Juntos programme on nutritional and cognitive outcomes. Therefore, in that chapter, I will offer additional policy implications drawing from the comparison between the short- and long-term effects of this CCT programme on child development. Lastly, in Chapter 8, I will offer additional insights for further research on the effects of CCT programmes within the framework of the overarching conclusions of this thesis.

6.7. Limitations

This chapter has some limitations worthy of discussion. First, I assume that the measures of cognition and nutritional status are measured without error. In contrast to the previous chapter, applying the Dynamic Factor Latent model as a robustness check to correct for measurement errors was not feasible here. This limitation arises from the requirement of lags for model identification, and the fact that measures of cognition were collected from Round 2 of the YL surveys. Consequently, error-corrected variables could only have been obtained from Round 3 and would have excluded the pre-treatment round. However, it is important to note that the assessments of the Juntos programme previously discussed in this chapter, as well as much of the research conducted using YL surveys, have relied on the raw versions of these variables, which confirm their reliability as appropriate measures for children's cognition and nutrition.

Another limitation of this study pertains to the reliance on the parallel trends assumption, which is common in most DID models. In this case, there is only one pre-treatment period. Therefore, I cannot formally test this assumption. Additionally, imbalances between groups in baseline characteristics during Round 2, which could impact the evolution of outcomes, pose a challenge. To address this, a conditional parallel trends methodology was employed by controlling for a set of covariates, as is usually done in the context of DID evaluations.

Furthermore, as shown in Table 11 by the low F-statistics from the first stage regression in the Imai + IV framework, the instruments available for this study may not be robust enough to produce highly reliable results. However, as previously discussed, research by Attanasio et al. (2017) for Ethiopia and Peru has shown that child cognitive skills and child health were not significant determinants of parental investments at any age. Moreover, while instrument strength remains a limitation, the magnitude of the effects obtained using the IV model were largely consistent with those from the B&K and Imai's frameworks.

Lastly, it is important to acknowledge that the proportion of the total effect mediated by BMI, which does not exceed 8% for the entire sample and among girls, suggests the presence of other factors that may mediate the programme's impact on cognition. Some of these variables, which are likely to be influenced by the programme and may influence cognition, include parental investments and children's time allocation (such as time spent in school). Some leads for future research in terms of these variables are discussed in the general conclusions of this thesis in Chapter 8.

7. The long-term effects of Juntos on children’s nutrition and cognition in Perú.

In the previous chapter, I used mediation analysis to examine the short-term effects of the Juntos programme on child cognition. This analysis focused on children included in Rounds 2 and 3 of the YL study, encompassing children aged between 5 and 8 years old.

In this chapter, I investigate the long-term effects of the Juntos programme on children between the ages of 5 and 15 (Rounds 2 to 5 of the YL study). Evaluating these effects introduces additional complexities in establishing the programme's causal impact. To begin with, the conventional 2x2 DID approach cannot be applied because I study children who remained in the programme for more than one period. Additionally, the Juntos programme had a staggered implementation, meaning that children did not all enrol at the same age or round. Consequently, there may be heterogeneity in the programme's effects across different cohorts and rounds. In the following subsection, I will discuss the methodologies available to address these challenges.

7.1. Methodology: the Two-Ways Fixed-Effects model.

When there are multiple time periods and the treatment adoption is staggered, researchers typically employ a Two-Ways Fixed-Effects (TWFE) specification of the following form (Callaway & Sant’Anna, 2021; Wing et al., 2018):

$$Y_{it} = \alpha_i + \beta_t + \gamma_1 D_{i,t} + \varepsilon_{it} \tag{39}$$

Where α_i is an individual fixed effect, β_t is a time fixed effect and $D_{i,t}$ is a dichotomous indicator equal to 1 if the individual is treated at time t, and 0 otherwise. The causal effect, under the common trend assumptions is given by γ_1.

As noted by de Chaisemartin & D’Haultfoeuille (2022) and Roth et al. (2023) , the TWFE specification has been applied in more complex research designs involving multiple groups, multiple treatment periods, staggered adoption of treatment, switching of treatment status (some individuals switching off the treatment), and non-binary treatments.

Goodman-Bacon (2021) demonstrated that the ATE estimated by the γ_1 coefficient from a TWFE model estimated with multiple periods and treatment groups, represents a weighted average of all potential 2x2 DID ATEs. These weights are dependent on group sizes and the variance in treatment levels (Cunningham, 2021). Some of these 2x2 DID estimations may include "bad comparisons", which can occur when units that have already received treatment (e.g., in an earlier period) are mistakenly considered part of the control group. Similar findings have been reported by Chaisemartin & D'Haultfoeuille (2022) and Imai & Kim (2020) and they demonstrate that some of these weights may be negative. Consequently, it is even possible that the coefficient γ_1 may be negative while all the individual ATEs are positive.

However, according to Chaisemartin & D'Haultfoeuille (2022), the TWFE approach could yield an unbiased ATE estimator only if two conditions are met: 1) the parallel trends assumption holds, and 2) the treatment effects remain constant across different groups of individuals (cohorts) and over time.

As discussed in the previous chapter, a common method to enhance the reliability of the parallel trends assumption is to condition on pre-treatment covariates, which was the strategy employed in that chapter and can also be applied to the TWFE model. The second condition, which requires that treatment effects remain constant between groups of individuals (cohorts) and over time, is unlikely to hold in most empirical applications, including the case I am studying.

The Juntos programme was progressively rolled out in Peru, starting with the most deprived municipalities in 2005 (Andersen et al., 2015). The Young Lives (YL) data reveals that we have three different cohorts of children who were treated at different points in time: those treated between Rounds 2 and 3 (ages 5 and 8), referred to as Cohort 3 hereafter; those treated between Rounds 3 and 4 (ages 8 and 11, Cohort 4 hereafter); and those treated between Rounds 4 and 5 (ages 11 and 15, Cohort 5 hereafter). It is important to note that, by definition, the short-term evaluation carried out in the previous chapter were done using only Cohort 3 at the ages between 5 and 8 (one pre-treatment and one post-treatment period).

As a result, children in different cohorts began treatment at different ages, ranging from middle childhood to adolescence. This means that they entered the programme at different stages of their development. Previous literature demonstrates that the age at which children receive monetary and time investments can significantly impact their future development (Attanasio, Meghir, et al., 2020). Therefore, it is plausible that children who were treated earlier may be more influenced by the programme, not only because of the longer duration of exposure but also because they received treatment at a younger age.

Moreover, Peru experienced rapid economic development during the period 2005-2014 (Rossini, 2015). This economic growth, combined with the initial rollout of the programme in the poorest municipalities (Sanchez et al., 2020), suggests that the first cohort (Cohort 3) may have had observable and unobservable characteristics that differed from the cohorts treated later. Therefore, it is likely that the treatment effect is not homogenous between cohorts treated at different rounds and ages, and it may also vary within the same cohorts at different rounds and ages. In addition to these potential pre-treatment differences *between treated cohorts*, there were also observable pre-treatment characteristics between the *treated* and *never-treated* samples (see Table 14).

In summary, the long-term evaluation of the Juntos programme proposed in this chapter faces two challenges: 1) differences in pre-treatment characteristics that could invalidate the parallel trends assumption, and 2) heterogeneous treatment effects by cohort of children and over time, which may bias the effects obtained through a TWFE regression. In the following section, I describe an alternative method to calculate ATEs that are robust to these potential issues.

7.2. Alternative DID methods for staggered treatment adoption.

Over the last five years, there has been a surge in the development of alternative estimators inspired by the DID method. The review conducted by Chaisemartin & D'Haultfoeuille (2022) identified a total of six different papers published after 2020, proposing various alternative estimators.

Among these estimators, the outcome regression approach proposed by Callaway & Sant'Anna (2021) (referred to as CS hereafter) is particularly relevant for my evaluation of the Juntos programme. The CS estimator is designed for cases involving multiple time periods, staggered adoption of a policy, and heterogeneous treatment effects by cohort and time. It is based on the outcome regression approach introduced by Heckman et al. (1998) to address issues related to unbalanced pre-treatment characteristics. The CS estimator can identify an ATE even when differences in observed pre-treatment characteristics lead to non-parallel trends between treatment and non-treatment groups. This makes the CS estimator well-suited to tackle the two challenges I have outlined for this long-term evaluation of the Juntos programme.

The outcome regression estimator by Callaway and Sant'Anna (2021) relies on the conditioning of parallel trends on a vector of pre-treatment covariates X_i. In general, the ATEs under an Outcome Regression (OR) approach can be expressed as follows: (Roth et al., 2023):

$$ATE = E\left[Y_{i,2} - Y_{i,1} \middle| D_i = 1\right] - E[E[Y_{i,2} - Y_{i,1}|D_i = 0, X_i]|D_i = 1] \tag{40}$$

An estimator for this ATE can be obtained by computing the conditional expectation of the outcome among untreated units and then calculating the average of these predictions using the empirical distribution of X_i among the treated units:

$$\widehat{ATE} = \frac{1}{N} \sum_{i:D_i=1} \{\left(Y_{i,2} - Y_{i,1}\right) - \hat{E}\left[Y_{i,2} - Y_{i,1} \middle| D_i = 0, X_i\right]\} \tag{41}$$

Specifically, the term $\left(Y_{i,2} - Y_{i,1}\right)$ represents the difference between the post and pre-treatment periods for the treated units (note that the summation is limited to all i's for which $D_i = 1$). The term $\hat{E}[Y_{i,2} - Y_{i,1}|D_i = 0, X_i]$ represents the estimated conditional function fitted to the control units ($D_i = 0$), but evaluated using the covariates X_i for the treated units ($D_i = 1$). The key assumption here is that the outcome model $\hat{E}[Y_{i,2} - Y_{i,1}|D_i = 0, X_i]$ is correctly specified. This conditional function can be estimated using a linear model or other semi or non-parametric models (Roth et al., 2023).

It is important to note that in the context of a canonical DID, where all individuals are treated at the same time, the comparison of outcomes typically involves the pre-treatment period and the post-treatment period. In the case of the short-term evaluation presented in the previous Chapter, these periods correspond to Rounds 2 and 3, respectively.

The CS estimator, on the other hand, uses the most recent untreated period as the baseline for each cohort. In other words, it compares the outcomes in the *round before each cohort was treated for the first time* to the subsequent rounds when they were treated. For example, for Cohort 3 in Round 3, the pre-treatment period is Round 2, so the model compares outcomes from Round 3 to Round 2. For children from Cohort 4 in Round 5, the baseline period is Round 3, and the model compares outcomes from Round 3 to Round 5.

Another distinction of the CS approach is that all the covariates X_i used are pre-treatment covariates. In contrast, the 2x2 DID and the TWFE models can include post-treatment covariates, but these variables must not be influenced by the programme to maintain the validity of the models (avoiding “bad controls”).

The CS will be the used as the main estimation approach for this chapter. This choice is based on its advantages over the TWFE to deal with differences in pre-treatment characteristics that could invalidate the parallel trends assumption, and with heterogeneous treatment effects by cohort of children and over time (Callaway & Sant’Anna, 2021; de Chaisemartin & D’Haultfœuille, 2022; Roth et al., 2023). In Section 7.5.2, I will present the results from the TWFE just for comparison purposes.

It is worth noting that the CS estimator is a relatively new approach, and to the best of my knowledge, there has not been a well-established theoretical basis for integrating it with a causal mediation framework. One key limitation is that this approach assumes that all variables included in the model are considered as pre-treatment covariates. This poses a challenge for a mediation framework because the mediator should also be affected by the programme, requiring it to be measured as a post-treatment covariate.

In contrast, both the 2x2 DID and the TWFE models are linear and have previously been integrated with the Linear Structural Equation Modelling framework by Baron and Kenny (1986) as well as with the general causal framework by Imai et al. (2011) in prior studies (Anselmi et al., 2017; Lugo-Palacios et al., 2023; Pace et al., 2022). As such, given the limitations of the TWFE model in the context of the long-term evaluation of the Juntos programme in presence of heterogenous effects by cohort and over time, conducting a causal mediation analysis for its effects will not be attempted in this chapter and this will be the subject of future research.

7.3. Data

I use Rounds 2 (age 5) to Round 5 (age 15) of the YL dataset for Peru. For the estimation of the outcome model in the CS estimator, several pre-treatment socioeconomic characteristics of the children and their households. These characteristics comprise the child's gender, language, age and ethnicity, as well as a binary indicator denoting whether the child is the first-born in the family. Additionally, the model incorporates the age in months, gender and education level of the household's head, along with the number of other children in the household aged below 18 and other adults (aged 18 and older). A dichotomous variable indicating the area of residence (1 for urban, 0 for rural) and the total expenditure per capita of the household, used as a proxy for household income, are also considered in the analysis.

I also account for children's time allocation, including the number of hours spent on education (both at school and studying at home), the number of hours dedicated to work (comprising market work and tasks within or for the household), and the number of hours allocated for leisure and play. Additionally, I incorporate variables to control for whether the verbal and math test scores were administered in the children's mother tongue.

Just for comparison purposes, I also run a TWFE model computed using Equation 39. It is important to observe that the TWFE incorporates all time-invariant covariates, both observed and unobserved, within the individuals' fixed effect term α_i. Consequently, the model already accounts for all pre-treatment characteristics (time use and expenditures), along with child-specific attributes like gender, ethnicity, and language.

In contrast to the CS model, in the TWFE model I employ additional controls for time-variant covariates that might influence outcome changes but remain unaffected by the treatment. These variables include the age of the children, the age, gender and education level of the household's head (household heads can change between rounds, along with their characteristics), the number of other children in the household (aged less than 18 years old), the number of other adults (aged 18+) in the household, and a binary indicator for area of residence (1=urban, 0=rural). Additionally, for verbal and mathematical test scores, I control for whether the test was administered in the child's mother tongue.

7.4. Descriptive statistics

Table 13 displays the sample sizes for both the treated and never treated groups after the database has been cleaned. Round 2 is first initial period during which data on cognitive outcomes (verbal and mathematics test scores) were collected. During this round, none of the children were receiving treatment. For my analysis, I included children living in households that remained in the programme until Round 5. A total of 106 children were considered switchers, meaning they lived in households that entered the programme and subsequently left it before Round 5. These switchers are not included in this study, as the CS estimator assumes that individuals remain in treatment once they were enrolled.

Table 13. Number of children treated by round

Cohort-Round/ average age (in years)	Treated sample	Change treated (Cohorts)	Never treated
Round 2 (age 5)	0	NA	967
Round 3 (age 8)	157	157	967
Round 4 (age 12)	246	89	967
Round 5 (age 15)	317	71	967

Cohort 3 consists of 157 children, while Cohorts 4 and 5 include 89 and 71 new children respectively, who became part of the programme between those rounds. Table 14 presents the descriptive statistics for the treated and never-treated children at the baseline. This table is similar to Table 8 in the previous chapter, although it features different sample sizes in the treated and untreated groups due to the long-term nature of the evaluation in this chapter. Nevertheless, the general patterns closely resemble those presented in the previous chapter for the short-term evaluation.

At the age of 5, children who later participated in Juntos (starting in any round) scored lower in math and verbal skills compared to never-treated children. However, there was no statistically significant difference in BMI-for-age z-scores between the two groups. The prevalence of stunting status was higher among children in Juntos (56.8%) than among never-treated children (20%). Additionally, HAZ scores were lower for children in Juntos (-2.16) compared to never-treated children (-1.2).

Table 14. Descriptive stats for children treated and never treated samples, Round 2 (age 5)

Variable	In Juntos (N=317)	Never treated (N=967)	Difference
Verbal scores	-0.674	0.324	-0.999***
Math scores	-0.298	0.188	-0.486***
Stunting status (1=yes, 0 otherwise)	0.568	0.201	0.367***
BMI-for-age z-scores	0.653	0.632	0.021
Height-for-age z-scores	-2.164	-1.207	-0.957***
Child language (1=Spanish, 0=other)	0.622	0.977	-0.356***
Child sex (1= female, 0=male)	0.508	0.486	0.022
Birth order (1=being first born children in the Household)	0.268	0.417	-0.148***
Household head's age	38.167	38.123	0.044
Household head's sex (1=male, 0=female)	0.044	0.093	-0.049***
Number of other children (aged < 18)	2.637	1.539	1.099***
Number of other adults (18+)	2.542	2.66	-0.117
Area of residence (urban =1, 0 = rural)	0.312	0.889	-0.576***
Total expenditure per capita (in 100's of soles)	10.312	10.023	0.289***

Notes:
P-values for the differences between groups calculated from a t-test for equality of means (* p<0.10, ** p<0.05, *** p<0.01)

In terms of socioeconomic characteristics, fewer treated children reported Spanish as their mother tongue compared to never-treated children (62.2% versus 97.5%). Additionally, fewer treated children were the first-born children in their households, and they tended to live in households with a larger number of other children. Children who later participated in the programme more frequently lived in rural areas and in households with a slightly higher total expenditure per capita, compared to the never-treated group. In most households for both groups, the household head was female, but the proportion of female household heads was slightly higher among beneficiaries (95.6% versus 90.7%). However, there were no statistically significant differences between the two groups in terms of the age of the household head, children's gender, and the number of adults in the household.

7.5. Results

7.5.1. Results from the CS estimator

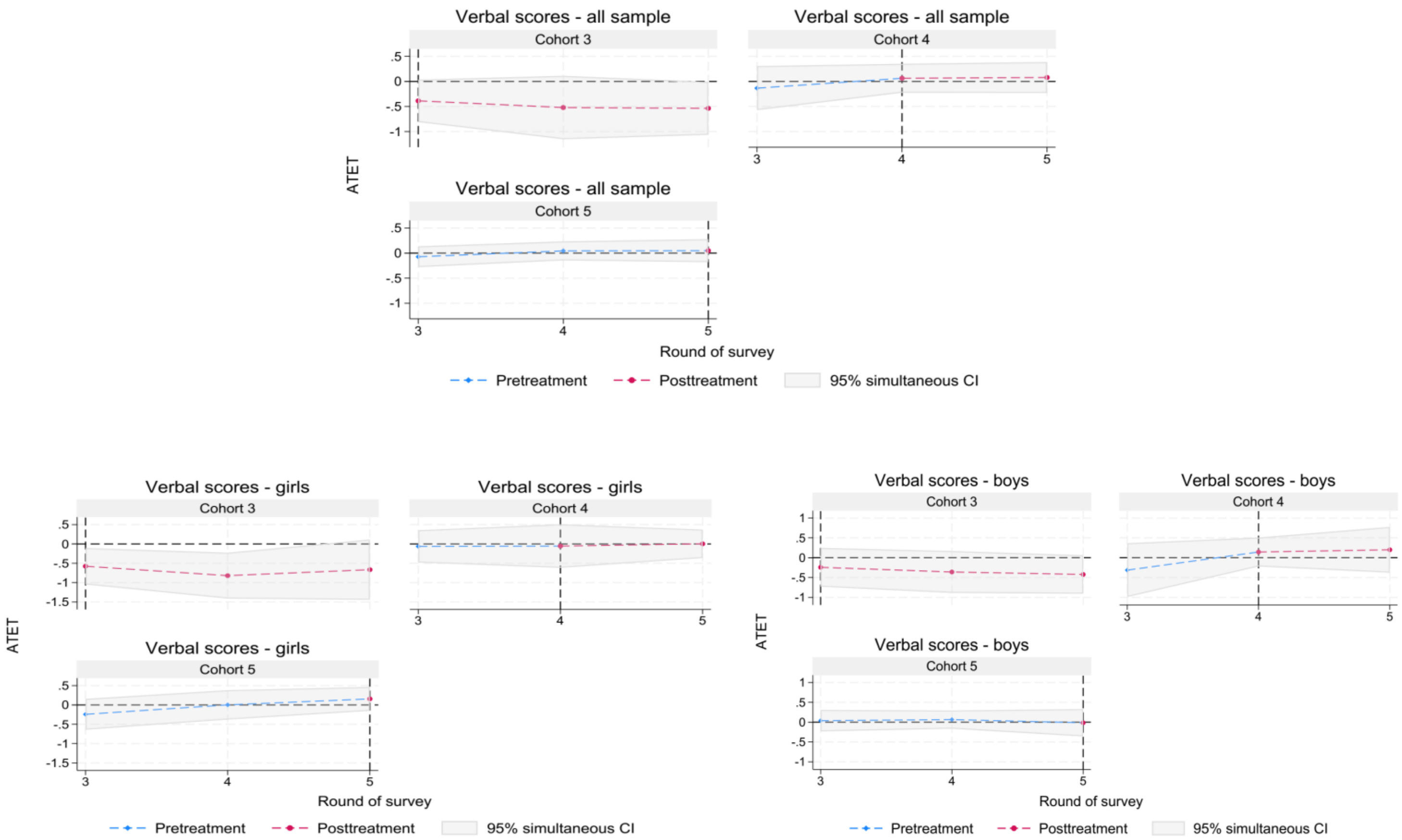
Due to the large number of coefficients generated by the CS estimator, the results are visualised through graphs. Each figure depicts the evolution of outcomes by cohorts and rounds. For Cohorts 4 and 5, the coefficients estimated prior to the programme's introduction are utilised to assess anticipation effects of programme participation, providing insight into the credibility of the parallel trends assumption. The graphs include 95% simultaneous confidence intervals, as recommended by Callaway and Sant'Anna (2021). These intervals are clustered at the YL cluster level and calculated using 999 bootstrap replications. Tables A6 – A10 in the appendix present the estimated coefficients.

The analysis based on the CS estimator reveals a consistent pattern across various outcomes, with the majority of effects not reaching statistical significance at the 5% level. However, the few significant effects observed were primarily concentrated among the first cohort of treated children (Cohort 3), and among girls.

Figure 9 illustrates the results for verbal scores**.** None of the effects for the entire sample reached statistical significance for any of the cohort-years. The gender-specific results, on the other hand, indicate that the programme might have a detrimental impact on the verbal skills of girls within the first treated cohort, particularly at the ages of 8 (-0.521 sd) and 12 (-0.82 sd). These effects were statistically significant at the 5% level.

It is important to note that, although not statistically significant, most of the coefficients for Cohort 3 were negative, while most of the coefficients for the other cohorts were positive.

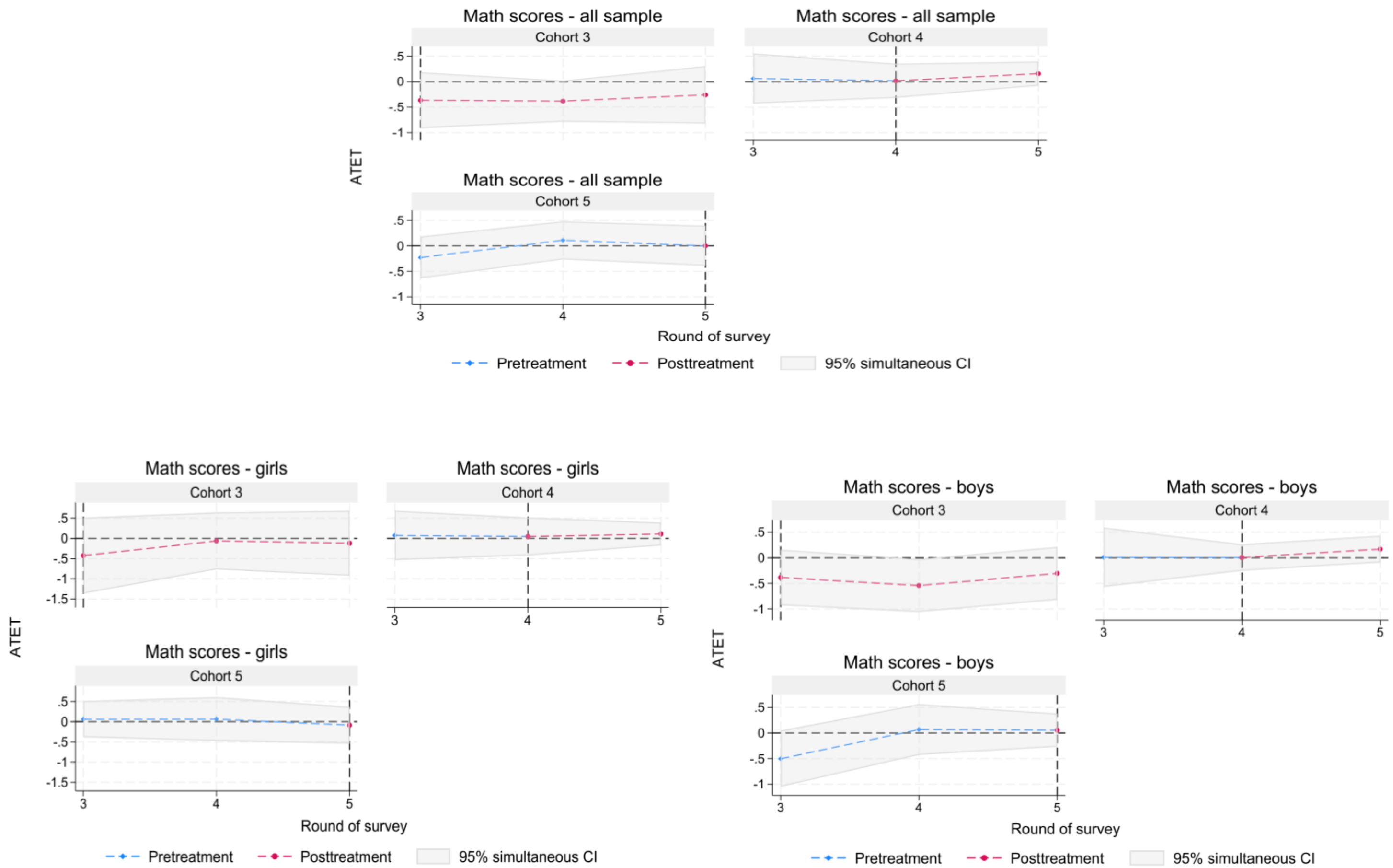
Figure 9. The effect of the Juntos programme on verbal scores



Notes: Effects estimated using Callaway and Sant'Anna's estimator (Callaway & Sant'Anna 2021). 95% simultaneous confidence intervals in parenthesis. Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

In the case of math test scores (Figure 10), the graphs exhibit a similar pattern to that of verbal scores: most of the coefficients were not statistically significant. The only statistically significant effect observed was a negative impact of -0.541 sd among boys from Cohort 3 in Round 4. Once more, similar to the verbal scores, the majority of the coefficients for Cohort 3 were negative, though not statistically significant, while the other cohorts displayed more positive coefficients.

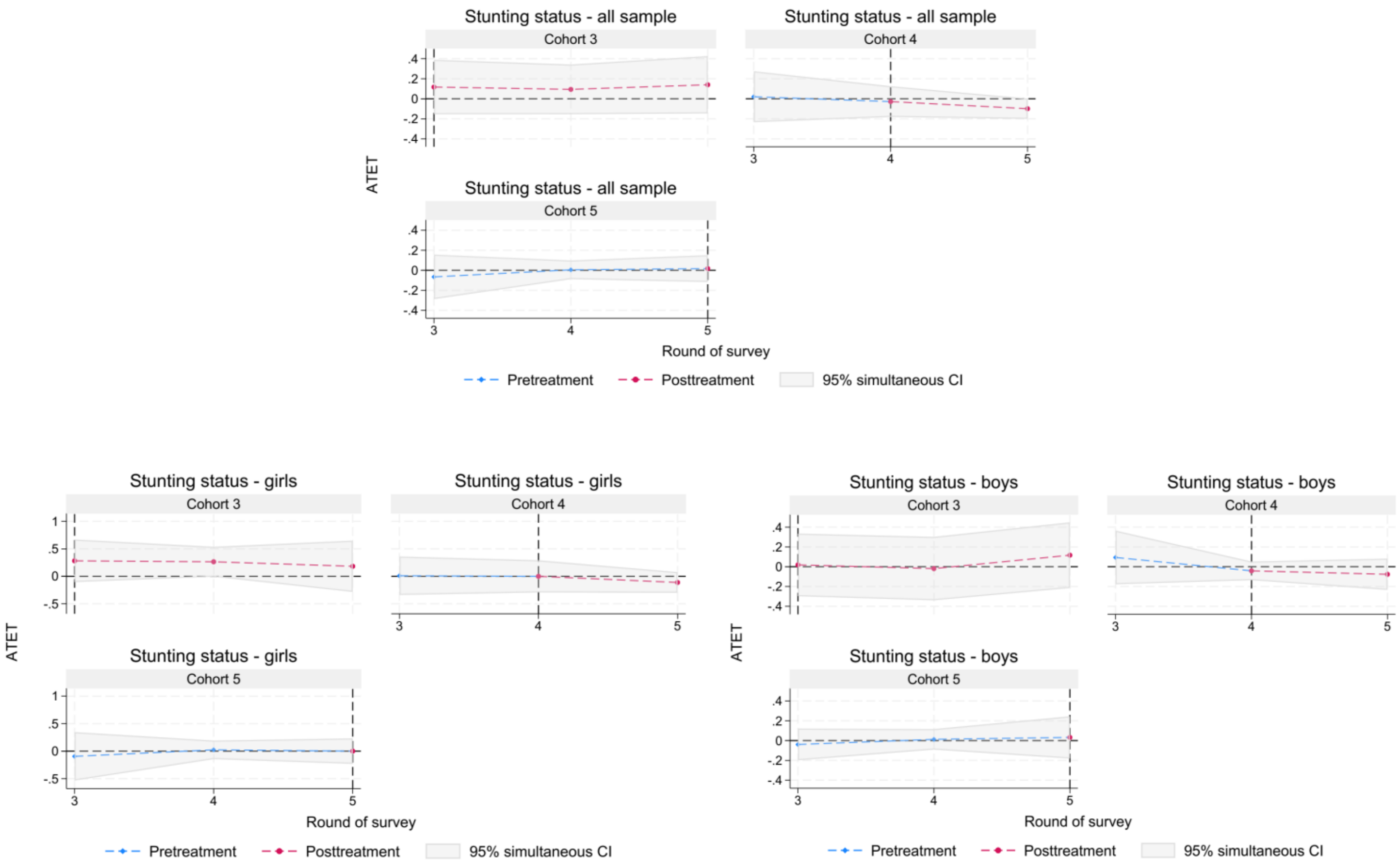
Figure 10. The effect of the Juntos programme on math test scores



Notes: Effects estimated using Callaway and Sant'Anna's estimator (Callaway & Sant'Anna 2021). 95% simultaneous confidence intervals in parenthesis. Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

Figure 11 presents the results for stunting status. None of the coefficients were statistically significant. However, the coefficients were of a larger positive magnitude for girls from the third Cohort, compared to the other cohorts and rounds.

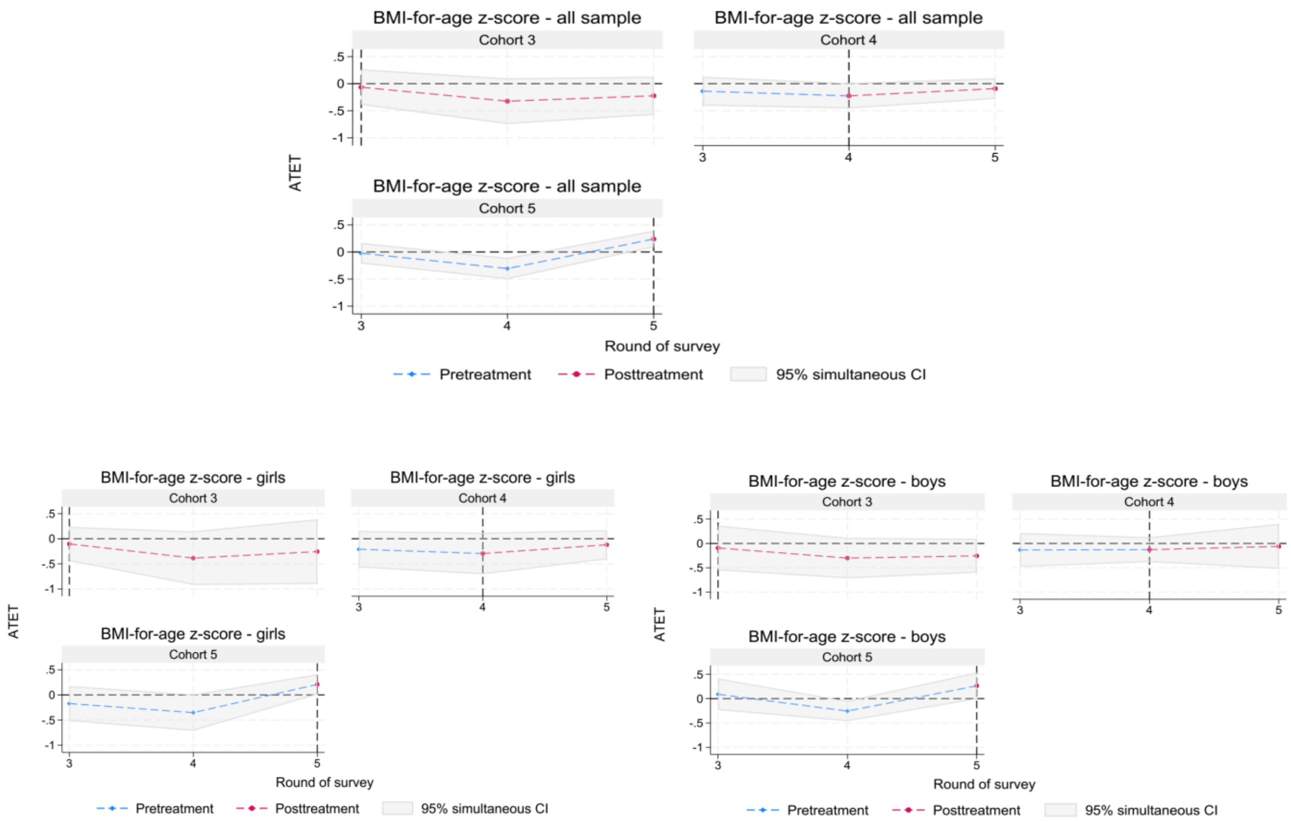
Figure 11. The effect of the Juntos programme on stunting status



Notes: Effects estimated using Callaway and Sant'Anna's estimator (Callaway & Sant'Anna 2021). 95% simultaneous confidence intervals in parenthesis. Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

The results for BMI were not statistically significant for most of the cohorts-years (Figure 12). However, all the coefficients for Cohorts 3 and 4 had a negative sign. The only positive coefficients were obtained for Cohort 5 in Round 5 among all subsamples. However, for all children (the entire sample) and boys, the effect in Round 4 (a period before the introduction of the treatment) was statistically significant, which suggests that the parallel trends assumption was not met. Therefore, only the effect for girls is considered significant in this case, indicating a positive impact of the programme of 0.21 sd on girls' BMI at the age of 15.

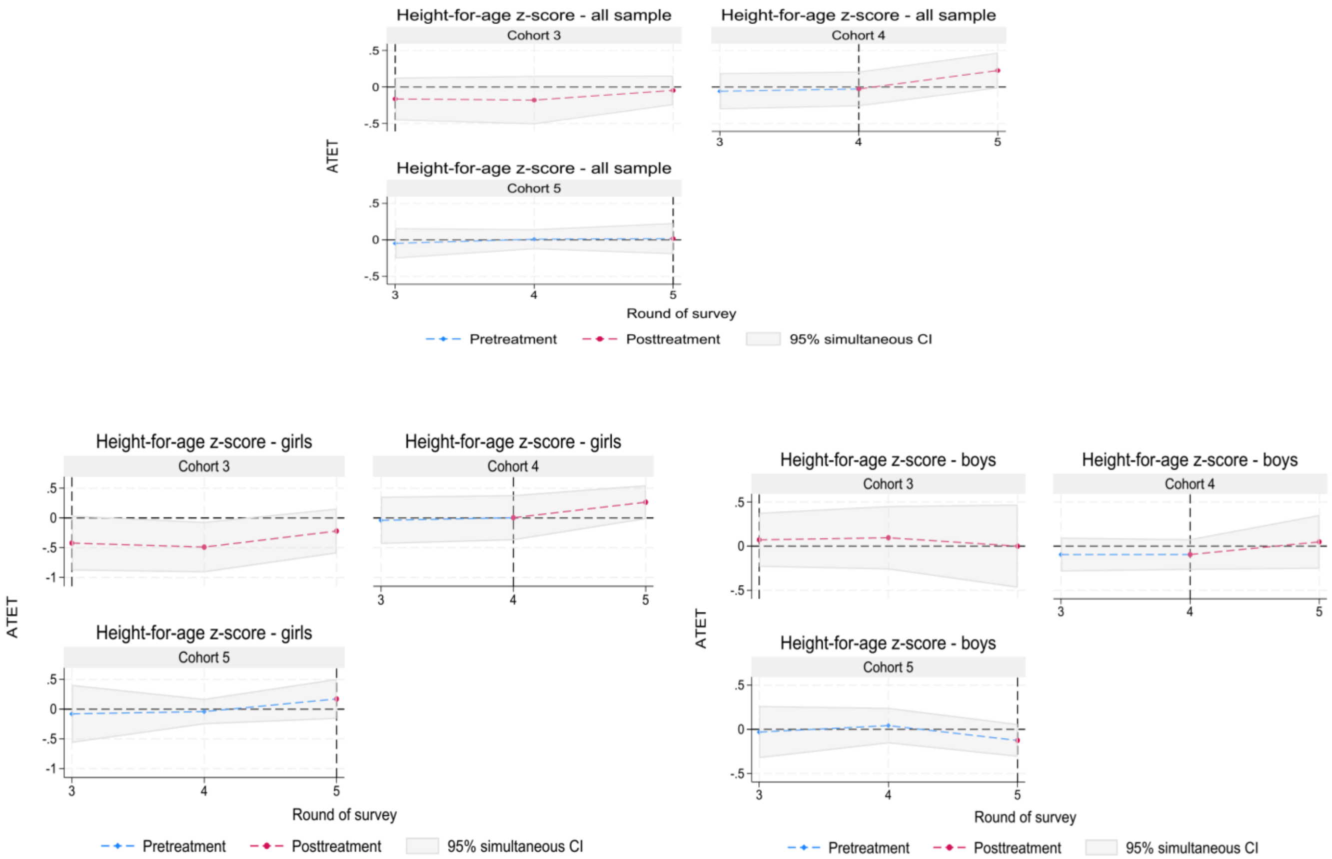
Figure 12. The effect of the Juntos programme on BMI-for-age z-score



Notes: Effects estimated using Callaway and Sant'Anna's estimator (Callaway & Sant'Anna 2021). 95% simultaneous confidence intervals in parenthesis. Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

Finally, Figure 13 presents the results for HAZ scores. Most of the results were again not statistically significant. The only significant result (-0.5 sd) was observed among girls from Cohort 3 in Round 4.

Figure 13. The effect of Juntos programme on Height-for-age z-score



Notes: Effects estimated using Callaway and Sant'Anna's estimator (Callaway & Sant'Anna 2021). 95% simultaneous confidence intervals in parenthesis. Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

7.5.2. Potential bias from the TWFE model

As discussed in Section 7.1, to illustrate the potential bias associated with the use of the TWFE model in the context of the long-term evaluation of the Juntos programme, I briefly discuss in this section the results obtained through that methodological approach. The full set of estimated coefficients are presented in Table A11 in Appendix 11.4.

The outcomes from the TWFE model indicate the absence of statistically significant effects of time poverty on test scores. In contrast, the CS estimator show statistically significant effects among children from the first treated cohort.

Moreover, the TWFE suggest negative effects on stunting status across the entire sample and for both genders, while the CS model shows non-significant effects for all cohorts and rounds. Furthermore, negative effects on BMI were observed in the overall sample and among girls in the TWFE model, while the only significant effect in the CS estimator for BMI was a positive impact among girls at the age of 15.

Finally, the TWFE model indicates positive effects on HAZ scores in the overall sample and among girls. In contrast, the CS estimator unveils a negative effect for girls at the age of 12.

Hence, the disparity between these results in the TWFE model and the CS estimator underlines the significance of selecting the appropriate estimator to obtain unbiased Average Treatment Effects of the Juntos programme in the long-term, as highlighted by the recent methodological DID literature (Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2022; Roth et al., 2023)

7.6. Discussion and conclusions

In this chapter, I conducted a long-term impact evaluation of the Juntos programme, spanning from the pre-treatment period at age 5 to adolescence at age 15. This research contributes to the existing literature by extending the findings of previous evaluations, which focused on the short-term impact of the programme from age 5 to age 8 (Andersen et al., 2015; Gaentzsch, 2020; Sanchez et al., 2020).

To the best of my knowledge, this study is not only the first long-term evaluation of the Juntos programme but also the first one that uses the new impact evaluation methodologies designed to improve the estimation of ATEs in the presence of staggered adoption of an intervention. These methodologies have demonstrated their ability to generate more accurate estimates than the traditional TWFE, also enabling the identification of heterogenous treatment effects within treatment cohorts and specific round-age groups (Callaway & Sant'Anna, 2021; Roth et al., 2023)

The results from the CS estimator generally demonstrate a pattern of non-significant effects of Juntos on cognitive and nutritional outcomes. However, among the effects that were statistically significant, the majority were concentrated among girls from the first cohort. Notably, negative effects on verbal skills (at ages 8 and 12) and HAZ scores (at age 12) were observed for girls from the first treated cohort. Additionally, a negative impact on boys' mathematical skills from the first treated cohort at the age of 12 was also identified. Finally, the programme had a positive effect on BMI for girls from the last treated cohort at the age of 15.

The prevalence of negative effects primarily among children from the first cohort can be explained by the fact that these children commenced their programme participation between the ages of 5 and 8, and resided in the most deprived municipalities of Peru at the baseline (Sanchez et al., 2020).

These results, coupled with those from the short-term mediation analysis conducted in the previous chapter, underscore the importance of carefully designing Conditional Cash Transfer (CCT) programmes to mitigate potential adverse effects among children, particularly among girls and those who belong to the most vulnerable populations.

In the next paragraphs, I discuss the results derived from the long-term evaluation and draw comparisons with those obtained from the short-term evaluation conducted in the preceding chapter. Four overarching conclusions can be derived from the analyses in these two chapters:

- The selection of the evaluation method affects the results of the analysis in a non-trivial manner. For example, the findings from the short-term evaluation utilising a 2x2 DID model and those from the CS estimator in the long-term exhibit certain similarities. However, had I employed the TWFE model as the primary empirical methodology, the long-term results would have differed more substantially from those from the short-term evaluation.
- Regarding nutritional outcomes, the findings follow a general pattern of non-significant effects in the long-term assessment, with only a few exceptions discussed earlier. In the short-term evaluation, the programme had a negative impact solely on BMI.
- The evidence in the short- and long-term suggests that the Juntos programme does not yield a positive impact on children's cognition, and in some cases, it may even have a negative effect. In the short-term, the effect of Juntos on BMI mediates some of the total programme's effects on cognition.
- There is evidence of heterogeneous gender effects within the Juntos programme, with the majority of statistically significant negative effects of the programme on cognition and nutrition being observed among girls. This result holds both in the short- and long- term.

I elaborate on these observations further below.

7.6.1. Short- versus long-term and choice of method

In the short term, the findings derived from the 2x2 DID method suggest that the programme might have an adverse impact on cognitive abilities, particularly among girls. Notably, the programme's effects on nutrition were significant solely in relation to BMI i.e., in the short term, the programme reduced BMI both among girls and boys. This negative effect on BMI mediated part of the adverse effects of the programme on girl's cognition.

The long-term effects estimated through the CS estimator presented some similarities with the outcomes derived from the 2x2 DID analysis in the short-term. Both approaches suggest that the programme had a negative impact on girls' verbal skills, particularly among those in the first treated cohort. Furthermore, the majority of the effects on nutrition did not achieve statistical significance, with the few exceptions previously discussed, i.e., a negative effect on girl's HAZ scores at the age of 12 for the first treated cohort and a positive effect on girl's BMI at the age of 15 for the last treated cohort.

In contrast, the results from the TWFE approach, showed no significant effects of the Juntos programme on cognitive skills. However, they did indicate certain impacts of Juntos on nutritional outcomes, including a reduction in stunting prevalence, a decrease in BMI, and an increase in HAZ scores. This highlights the importance of selecting the appropriate methodology and underscores the advantages that the new estimators offer over the traditional TWFE model within the context of staggered DID models.

7.6.2. Cohort, age and exposure

My long-term results also differed by cohort and the age at which children were exposed to the programme. The signs of the coefficients derived from the CS approach, suggest that the Juntos programme could potentially have modest but heterogeneous effects based on the cohort of children, the age at which they initiated treatment, and the duration of their participation. However, it is important to note that most of these coefficients failed to reach statistical significance. Consequently, these findings should be interpreted with caution.

The first cohort, which received treatment at younger ages (between 5 to 8 years), exhibited the highest number of statistically significant effects. Moreover, even the coefficients that did not reach statistical significance displayed magnitudes and directions that differed from those of other cohorts. For instance, the coefficients for the effect of Juntos on verbal scores consistently showed negative values in the first treated cohort, whereas they were mostly positive for the other two cohorts.

As previously discussed, the timing of when children receive these interventions is crucial. Prior research has also indicated that investments in children may have more substantial impacts on their cognition and health when provided at earlier ages (Attanasio, Meghir, et al., 2020), which could explain why most of the significant results were found among the first treated cohort at the ages of 8 and 11. This is important not only because the inputs received may have varying levels of effectiveness at different ages but also because, given the evidence of the persistence of lagged inputs, they can continue to influence future development (Attanasio et al., 2022).

Additionally, the early treatment of the first cohort intersects with the programme's initial implementation in the country's most impoverished municipalities. Moreover, Peru also experienced a rapid economic growth during the period 2005-2014 (Rossini, 2015). Consequently, children from subsequent cohorts were not only older but also, presumably, slightly more economically advantaged compared to the first cohort of children who received treatment, which could have prevented the programme to have an impact on them. In conclusion, these patterns in the results supports the findings from previous research that the age of enrolment and the duration of exposure can indeed shape the potential outcomes of a CCT programme (Molina Millan et al., 2020; Sanchez et al., 2020).

7.6.3. Heterogenous results by gender

My results also exhibited heterogenous effects based on the gender of the children. This conclusion is more robust since it consistently holds across most of the estimations. For instance, in the short-term evaluation, the negative coefficients for the effect of Juntos on verbal and math scores were consistently slightly larger for girls than for boys (and for boys, the results on math scores were not significant). Regarding BMI, the coefficient was slightly larger for boys than for girls (-0.36 sd vs. -0.23 sd). However, this difference can be partially explained by the fact that boys had a higher mean BMI at baseline.

The negative effects on verbal scores in the long-term evaluation were observed exclusively among girls from the first treated cohort (Cohort 3), specifically when they were 9 and 12 years old. The effect at the age of 15 was no longer statistically significant. Additionally, a negative effect on math test scores was found among boys from the first treated cohort, but only when they were 12 years old (Round 4). A negative and statistically significant effect for HAZ z-scores was also observed for girls from Cohort 3 at the age of 11. Furthermore, gender preferences have been evident since Round 2 (age 5), as evidenced by the disparities in BMI between girls and boys, even before the implementation of Juntos.

Parents' gender preferences can significantly impact how families allocate both financial and non-financial resources to their children, including any additional resources generated through unearned income (Almond et al., 2018; Attanasio et al., 2022; Duflo, 2003). As discussed in my literature review, these preferences may also influence how children's paid and unpaid labour patterns respond to cash transfer programmes (de Hoop & Rosati, 2014). In some cases, this may result in increased labour participation among girls and a higher likelihood of girls dropping out of school, as observed in Pakistan by Awawori Churchill et al. (2021). As I will discuss in the next Chapter, I did not observe any effects of the programme on the time spent on education by gender. However, it appears that the programme did have an impact on parental investments, but this effect was exclusive among boys.

7.6.4. Effects on cognitive skills and nutrition

The remaining question is why the programme may have a statistically significant negative impact on cognitive skills, as suggested by the short-term evaluation and by some of the long-term effects obtained by the CS estimator, and whether this impact can be attributed to the programme's effect on nutrition. As discussed before, a negative effect on verbal scores among index children was also found in the three previous evaluations, but they were not statistically significant (Andersen et al, 2015; Gaentzsch, 2020; Sanchez et al, 2020). Among them, only Andersen et al. (2015) studied the effects by gender, and the negative coefficient for girls was larger than for boys (-0.22 sd versus -0.025 sd). In terms of math scores, I found a negative significant effect, similar to that found by Gaentzsch (2020) in their evaluation of Juntos.

The negative effects that Juntos had on cognition are unusual but not surprising. Most of the available evidence have found non-significant effects of CCTs on measures of child cognition (Bastagli et al., 2016). However, one study evaluating the *Familias en Accion* programme in Colombia, found a statistically insignificant effect on math test scores but a small negative effect (-0.05 sd) on Spanish test scores, which was significant at the 10% level. The disaggregated effects by gender were statistically significant for boys and girls depending on specific model specifications (Baez & Camacho, 2011).

A potential explanation for the statistically significant negative effect of Juntos on test scores may be linked to the recurring negative associations of the programme with nutritional scores. In the long-term evaluation, the coefficients for the effect of Juntos on nutritional scores were usually negative, particularly among the first treated cohort. However, with the exception of HAZ z-scores in Round 4 (-0.5 sd) for girls from the first treated cohort, and of BMI in Round 5 (0.21 sd) for girls from the last treated cohort, these coefficients were not statistically significant.

In the short-term evaluation, I formally tested whether the programme's effect on BMI serves as an intermediate pathway that explains the negative effects on verbal and mathematical skills. The results of the mediation analysis indicated that BMI mediated approximately 7.5% of the total effect on verbal skills and 3.5% of the total effect on math skills. These effects were typically statistically significant for the entire sample and among girls (with the exception of those obtained in the robustness checks for girls in math scores), but were not significant for boys.

The general pattern of non-significant effects on nutrition in the long-term evaluation and the proportions mediated by BMI in the short-term evaluation suggest the presence of other factors that may mediate the programme's impact on cognition. In Chapter 8, I will provide some directions for future research in this area.

7.7. Limitations and avenues for future research

The primary limitations of this chapter remain similar to those from the previous chapter. Firstly, I assume that all measures of cognition and nutritional outcomes are measured without error, as I am unable to employ the Dynamic Factor Latent model.

Secondly, the robustness of my results relies on the validity of the parallel trends assumption. I condition on a set of pre-intervention covariates, as done in most of the applied literature and recommended by Callaway and Sant'Anna (2021). The CS estimator provides graphical evidence for Cohorts 4 and 5, indicating the plausibility of the parallel trends assumption in the presence of anticipation effects. In most cases, I can dismiss anticipatory effects, with some exceptions previously documented and discussed. Unfortunately, graphical evidence cannot be obtained for Cohort 3 due to it having only one pre-treatment period.

Thirdly, my analysis excluded individuals who were switchers, meaning those who entered the programme and left it before Round 5. This was done because the CS estimator is designed to study always compliers. Therefore, the treatment group was composed of “always compliers”, and the control group consisted of those who were “never treated”. Switchers may have different characteristics than always compliers, so my results can only be generalised to always compliers. The investigation of the programme's effect on switchers is not within the scope of this thesis and is left for future work.

Finally, as mentioned earlier, a mediation analysis was not conducted for this long-term assessment of the Juntos programme. Examples of mediation analyses that merge the traditional DID approaches and IV methodologies can be found in the applied literature, as detailed in the previous chapter. However, to the best of my knowledge, the theoretical framework for the combination of the CS estimator, or any of the other alternative staggered DID estimators, with mediation analysis has yet to be established. Understanding the potential mechanisms through which the Juntos programme influences cognitive development and nutritional outcomes is essential for gaining deeper insight into the programme's long-term effects on children in Peru. This remains a priority for future research. Additional avenues for future research are explored in the upcoming chapter, within the context of the overall conclusions drawn from this thesis.

8. General conclusions from this thesis

8.1. Summary of results

Early and middle childhood are widely regarded as crucial periods for the accumulation of human capital. Childhood experiences during this phase have been linked to lasting effects on cognition, health and labour market outcomes in adolescence and adulthood (Almond et al., 2018; Duncan et al., 2010).

The primary objective of this thesis is to explore how the circumstances of children in Peru, during their early and middle childhood years, shape their human capital accumulation. Specifically, I investigate the impact of children's time allocation and the receipt of a CCT programme on child cognition and nutrition, considering both short- and long-term effects.

In Chapter 5, I investigated the potential influence of children's time allocation on child cognition. Employing a human capital framework and drawing from the existing research on time poverty among adults (Giurge et al., 2020; Kalenkoski & Hamrick, 2013), I explored whether facing severe limitations on discretionary time, referred to as time poverty, has an impact on child cognition. It is important to reiterate that the existing research on time poverty primarily concentrates on adults. To the best of my knowledge, this represents the first study investigating the potential effects of time poverty in childhood, on short- and long-term child cognition.

In Chapter 5, I documented gender disparities in time allocation and the prevalence of time poverty that manifested from early to middle childhood. The incidence of time poverty was consistently higher among girls than among boys. For instance, at the age of 8, 18.9% of girls experienced time poverty, while the rate among boys stood at 12%.

The results of the econometric estimations show heterogenous effects of time poverty on children’s cognition. In the Fixed Effects model, time poverty had a positive impact on girls' verbal test scores and a negative effect on girl’s mathematical test scores. Furthermore, the Cumulative Value-Added model suggests a potential negative effect of time poverty on girls' mathematical skills. Additionally, the Cumulative Value-Added model also suggests a potential positive delayed effect of experiencing time poverty at the age of 12 on boys' verbal scores at the age of 15, and a delayed positive impact of experiencing time poverty on mathematical test scores among all children (the entire sample of boys and girls).

In Chapter 6, I then explored whether nutritional outcomes acted as intermediate factors that mediated or mitigated the short-term effects of the Juntos programme on child cognition in Peru. Three previous short-term evaluations of Juntos found that the programme had no significant overall impact on vocabulary skills (Andersen et al., 2015; Gaentzsch, 2020; Sanchez et al., 2020), a negative effect on mathematical skills (Gaentzsch, 2020), and some mixed effects on children's nutrition (Andersen et al., 2015; Sanchez et al., 2020).

To better understand the effects of the Juntos programme, I conducted a causal mediation analysis to investigate the role of nutrition as an intermediate mechanism through which the programme may influence child cognition. To the best of my knowledge, this represents the first attempt to employ a formal causal mediation framework to understand the influence of Juntos on cognition through the study of intermediate variables.

The findings in Chapter 6 indicate that Juntos had a negative impact on girls' verbal test scores, as well as on both girls' and boys' mathematical test scores. Also, the programme had a negative impact on BMI within our sample, with no observable effects on stunting status or Height-for-Age z-scores (HAZ). Hence, BMI was the only plausible mediator. A positive relationship between BMI and girls' cognitive skills was also identified. Integrating these findings, my mediation analysis using the Baron & Kenny's (1986) mediation approach revealed that the impact of the programme on BMI mediated approximately 7.5% of an overall negative effect of the programme on girls' verbal skills (significant at the 10% level, robust to alternative modelling strategies), and potentially around 3.5% of a total negative effect on girl's mathematical skills (significant at the 10% level, but no significant in robustness checks). BMI did not serve as a mediator for the programme's impact on boys' test scores.

Finally, in Chapter 7, I assessed the programme's long-term effects on both cognitive and nutritional outcomes. In doing so, I extended the earlier short-term evaluations to examine the programme's impact from childhood to adolescence, spanning ages 5 to 15. This analysis included two additional cohorts of children who received treatment after the initial expansion of Juntos, a dimension not included in the previous short-term evaluations.

Moreover, I utilised a novel estimator proposed by Callaway and Sant'Anna (2021). This approach is designed to yield improved estimates of the programme's effects in the context of staggered adoption, where different cohorts of children received treatment at various points in time. Therefore, to the best of my knowledge, this is not only the first study of the long-term effects of the Juntos programme on child cognition and nutrition, but also the first one that incorporates the utilisation of new methodological advancements in impact evaluation that have been published in recent years.

The results from Chapter 7 revealed a general pattern where the majority of the estimated Average Treatment Effects by Cohort and Round-Age were not statistically significant. However, the effects that did reach statistical significance were typically negative and predominantly observed among girls from the initial treated cohort (children who began programme participation between the ages of 5 and 8). For instance, within the group of children from the first treated cohort, Juntos had a negative effect on girls' verbal test scores at the ages of 8 and 12, as well as on Height-for-Age z-scores at the age of 12. In addition, a negative effect on boys' mathematical test scores at the age of 12 was also observed. The only positive and statistically significant effect of Juntos was an increase in BMI among girls from the last treated cohort, who began programme participation between the ages of 12 and 15, at the age of 15.

As mentioned earlier, the initial treatment cohort comprised children who initiated programme participation between the ages of 5 and 8, coinciding with the transition from pre-primary to primary school. Moreover, as a result of the initial programme rollout in the most deprived regions of the country, prior to Peru's rapid economic development between 2005 and 2014 (Rossini, 2015), children in this cohort were presumed to be in more impoverished circumstances compared to later cohorts. Hence, it can be concluded that the programme exhibited heterogeneous effects depending on gender, the duration of exposure (short-term versus long-term), the child's age, initial deprivation, and the age at which children-initiated programme participation.

8.2. Discussion and policy implications

The primary overarching finding of my thesis is the identification of gender disparities that manifested early in childhood. These disparities were observed in a diverse set of variables such as child time allocation, cognitive outcomes and nutritional status. Additionally, the impact of time poverty and the CCT programme on child cognition and nutrition also exhibited gender-specific effects, typically resulting in more adverse consequences for girls compared to boys.

As previously discussed, gender disparities that originate in early childhood have been observed in various settings, impacting parental investments and child outcomes. However, the findings have shown variation depending on the specific context under examination. While there is evidence of gender-based parental investments in the UK, United States, and Canada that tend to benefit girls (Baker & Milligan, 2016; Chuan et al., 2022) other studies conducted in India and Uganda have indicated a greater allocation of parental resources to boys, with negative implications for girls' nutritional and educational outcomes (Barcellos et al., 2014; Björkman-Nyqvist, 2013; Jayachandran & Pande, 2017).

Based on this body of evidence, it appears that detrimental gender gaps for girls are more likely to be prevalent in LMICs where child labour, both at home and in the market, and beliefs about differing returns to education by gender, may still play a significant role in perpetuating gender inequities. This could potentially account for the gender gaps observed in Peru.

Social protection programmes are often considered crucial tools for reducing gender disparities and advancing the empowerment of girls and women (Schüring & Loewe, 2021). However, the available body of evidence highlights that many of these programmes, including cash transfers, may not fully accomplish this goal, and in certain instances, may have unintended adverse consequences (Camilletti, 2021). This appears to be the case with the Juntos programme, as demonstrated in Chapters 6 and 7, where it had some detrimental effects on children's cognition and nutritional outcomes, particularly among girls and among children from the first treated cohort.

Hence, the implementation of gender- and child-sensitive strategies for the Juntos programme, such as sensitisation campaigns or improvements in targeting and delivery mechanisms (Esser et al., 2019), could potentially contribute to the enhancement of nutritional and cognitive outcomes, along with a reduction in gender disparities among children in Peru.

Another overarching theme in this dissertation is the role of the quality of education and parental inputs in determining cognitive gains. As documented in Chapter 5, children experienced an increase in the time allocated to education between the ages of 5 and 8, and again between the ages of 12 and 15. This increase in educational time was identified as the primary driver of time poverty during these age intervals. As discussed in that chapter, the expansion of educational time may explain the positive effect of time poverty on verbal skills. However, despite the increased time dedicated to education, a negative effect on mathematical skills was observed.

Moreover, previous evidence has shown that Juntos had a positive effect in enrolment status among treated children (Gaentzsch, 2020), which is in line with the programme's conditionality related to school attendance. However, enrolment was already high before the introduction of the programme (more than 90%), so the effect of the programme on this indicator was small. Nevertheless, the programme did not yield positive effects on cognitive outcomes. Together, the results from Chapters 5, 6 and 7, suggest that increased school enrolment and increases in time dedicated to education were insufficient to generate a substantial impact on overall child cognition.

In line with this, a potential explanation for the non-significant or negative effects of time spent in school on child cognition often discussed in the context of CCTs, is a crowding-out effect at schools. These programmes typically target children from the poorest areas of the country, where the quality and supply of education might already be low. By increasing school enrolment and/or attendance without expanding the capacity of schools, a CCT may lead to increased classroom congestion and induce negative peer effects (Baez & Camacho, 2011). However, as discussed before, school enrolment was already high in Peru before the start of the programme. Nevertheless, it might have influenced attendance, making the crowding out effect a potential pathway for the negative effects on cognition previously documented.

To informally explore this potential mechanism, I conducted additional estimations of the short-term effect of Juntos on education time (time spent at school and studying at home). These results are presented in Table A.12 in Appendix 11.5. However, I did not find a statistically significant effect on the time spent in education.

Nevertheless, even if the programme has had a positive relationship with time spent on education, as discussed by Bastagli et al. (2016), factors such as parental human capital and the quality of teaching delivery, may co-mediate or influence the effect of CCTs on child cognition. For instance, in the specific case of Peru, the evaluation of the One Laptop per Child programme showed that improving children's access to computers did not increase mathematical or vocabulary test scores (Cristia et al., 2017). Furthermore, there is international evidence showing that increasing enrolment rates or time spent in school is not sufficient to improve children's outcomes if they are not complemented by improvements in the quality of teaching and school resources (Ford, 2021; Nega, 2012; Radinger & Boeskens, 2021).

Another potential pathway for the effect of the Juntos programme on cognition is through parental investments. Table A.12 in Appendix 11.5 also presents the effects of Juntos on parental investments. Previous studies have highlighted the importance of parental investments for school-aged children (Attanasio, Meghir, et al., 2020; Todd & Wolpin, 2007), including a study conducted in Ethiopia and Peru (Attanasio et al., 2017). My results suggest that Juntos may increase parental investments, but this effect was statistically significant only for the entire sample ($p < 0.05$) and among boys ($p < 0.1$). If parental investments were associated with cognition through channels other than child nutrition, then the Juntos programme may have had an independent effect on cognition. This could potentially counteract the negative effects of the programme on cognitive outcomes, but only among boys.

These results highlight the importance for policymakers to consider supplementing CCT programmes with policies designed to enhance the quality of education delivery, build the human capital of parents and address potential subconscious gender biases. Such measures would ensure that children receive better inputs and investments from both the education system and their parents, regardless of their gender.

To this end, since 2015 Peru has implemented the “Complete School Day” programme, which has as its objective not only increasing time at school, but also improving the quality of teaching delivery and school resources. Until 2021, this programme had been applied in 2,000 secondary schools and there is some evidence that it can be a useful tool to improve mathematical and reading skills of adolescents (Ford, 2021).

8.3. Strengths and limitations

This thesis has several notable strengths. Firstly, the Young Lives study stands out as a unique dataset that gathers a comprehensive range of variables at the individual, household and community levels, which can be challenging to locate in comparable datasets. This rich information, encompassing child cognitive scores, nutritional status and time allocation, as well as household resources and expenditures, household-level shocks, and community-level data on prices of goods and wages, has been used in this thesis to test my hypotheses employing a diverse range of modelling approaches.

In line with this, I have conducted a comprehensive review of the main econometric strategies employed for the estimation of production functions of human capital and for conducting programme impact evaluations. These models have been thoroughly examined in the previous chapters, with careful consideration given to addressing concerns such as endogeneity in the production of human capital, for instance due to reverse causality between child cognition and time allocation or parental investments. Additionally, the appropriateness of the DID models in both short and long-run contexts has been also considered when choosing my preferred estimation strategies.

Furthermore, I have also presented a range of alternative robustness checks to compare with my primary estimation strategies, all of which have been discussed in their respective chapters. Therefore, the chosen modelling strategies, along with the alternative robustness checks, provide a comprehensive framework for assessing the effects of time poverty and the Juntos programme on human capital development, while addressing potential sources of bias and endogeneity.

Nevertheless, this thesis has some limitations, most of which have been addressed previously in each chapter but are summarised here. Firstly, the examination of time poverty is constrained by the fact that time poverty is a relative measure of discretionary time deprivation. Consequently, depending on the determinants of time poverty, one might even expect it to have a positive relationship with child cognition. This underlies my proposed explanation for the positive effect of time poverty on girls' verbal skills, suggesting that time poverty resulting from increased time allocated to school activities may have a positive impact on verbal test scores.

Secondly, there is a lack of a precise definition for discretionary time. The existing definitions of committed and necessary activities are primarily focused on adults' time allocation that may not directly translate to children. In this thesis, my principal definition of discretionary time was the time that children had available for playing and leisure. Therefore, there is a need for a more objective and universally applicable definition of discretionary time, particularly when considering children.

Thirdly, in both Chapters 5 and 6, I have employed Instrumental Variables to address potential endogeneity due to reverse causality between child cognition and parental investments, as well as between child cognition and children's nutrition. I used a series of instruments related to prices of goods, wages and shocks experienced by households. The selection of these instruments was based on similar studies, such as the study conducted by Keane et al. (2022) using YL data.

However, as in their study, the instruments for Peru did not exhibit sufficient strength, making the IV results less reliable. Nevertheless, as previously discussed, evidence from Peru indicates that parental investments do not significantly respond to children's cognition and health (Attanasio et al., 2017). Therefore, concerns regarding endogeneity stemming from reverse causality are relatively low in this context. Despite this, I also employed alternative estimation strategies, such as Fixed Effects, Cumulative Value-Added Models and Dynamic Factor Models, which further control for endogeneity.

Another limitation of this work is that the evaluation of the Juntos programme in the short term comprised only one pre-treatment period. As a result, I could not formally test the parallel trends assumption. Imbalances in pre-treatment covariates between children in Juntos and those never treated were also observed. However, a conditional parallel trends methodology was employed, incorporating control for a set of covariates, as is common practice in the context of DID evaluations.

Concerns about the parallel trends assumption are also applicable to Cohort 3 in the long-term evaluation, as it too had only one pre-treatment period. Once again, pre-treatment covariates were employed to enhance the validity of this assumption. For the other cohorts with more than two pre-treatment periods, graphical analysis was used to evaluate the parallel trends. In most instances, the assumption appeared to be satisfied, and the few instances where this was not the case were discussed in Chapter 7.

A final limitation for the long-term evaluation using the CS model is the existence of switchers i.e. children who commenced programme participation but did not remain in the programme until the age of 15 (the last age-round with information about programme participation). These children were not included in my analysis, as the CS model assumes that once an observation is treated, it remains in the programme. Switchers may possess different characteristics than always compliers, so my results can only be generalised to always compliers. The analysis of switchers is a topic for future research.

8.4. Directions for future research

In this thesis, I have identified several priorities for future research. As discussed in Chapter 5, there is a need for further research on the potential effects of time poverty on children's cognition and health. This not only implies replicating this study in different settings but also addressing some of the limitations I have previously outlined to enhance the reliability of research on children's time poverty, as well as allowing the exploration of new topics related to child time poverty. This should begin with standardising the definition of time poverty among children and adults, which also requires a more objective conceptualisation and definition of discretionary time activities for both children and adults.

Furthermore, future cohort and longitudinal studies should incorporate the collection of time use data from early childhood. Moreover, it is essential to ensure that time use data is collected in a consistent way to enable comparisons of trends and analyses over time and across various contexts. This would allow the evaluation of how different policy interventions may impact children’s time use. Furthermore, this would also facilitate the exploration of topics beyond the scope of this thesis due to data limitations. Examples include investigating the impact of child time poverty on labour market outcomes, studying the intergenerational transmission of time use patterns (e.g. whether parental time poverty influences children's time poverty), and assessing the effect of this intergenerational transmission of time use on parental and child health, as well as on cognition, in the short- and long-term.

In Chapter 6, I identified a negative effect of the Juntos programme on cognition that was partly mediated by the effects of the programme on BMI. Future research should investigate the mechanisms through which Juntos influences children's nutrition. This exploration should not only consider its effect on BMI but also its non-significant effects on stunting status and HAZ scores. Gaining a deeper understanding of how Juntos affects nutrition could inform programme improvements aimed at enhancing both children's nutrition and cognition.

Additionally, my results from the short-term evaluation of Juntos on child cognition, indicated that the proportion of the total effect on cognition mediated by BMI did not exceed 8% for verbal scores and 3.5% for math scores, for the entire sample and among girls. This implies that there may be other factors that could mediate the programme's impact on cognition. As previously discussed, some of these factors influencing the non-positive effects of the programme on child cognition may be linked to its impact on school attendance and parental investments. Therefore, a comprehensive understanding of how the programme affects children's time use and parental investments, and whether these factors mediate its effects on cognition and nutrition, could improve the effectiveness of conditional cash transfer initiatives.

Lastly, there is a need for methodological advancements in integrating mediation analysis with the new difference-in-differences estimators designed for staggered adoption of interventions with multiple post-treatment periods. This would improve our ability to analyse the intermediary mechanisms through which programmes like Juntos influence child nutrition and cognition also over the long-term. Further work in this area could enhance our understanding of the effects of social programmes, build the human capital of fast-growing populations in Low- and Middle-Income Countries and ultimately improve the futures of many young people.

9. Declarations

This study, being a secondary data analysis, did not require ethical approval. The Young Lives Dataset is available at the UK Data Service (https://ukdataservice.ac.uk/) and the Young Lives website (https://www.younglives.org.uk/data).

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I acknowledge the use of ChatGPT 3.5 (Open AI, https://chat.openai.com) and Grammarly (https://grammarly.com) as tools for grammar correction. In the case of Chat GPT, the prompt used is the following:

"For the next task, you will act as a corrector of grammar and clarity. This is research for a PhD thesis. You will help me to improve clarity. Use academic but clear language. When making corrections and suggestions, keep the text short and do not try to extend it. Use British English"

10. References

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11. Appendix

11.1. Instrumental Variables results

In the IV model estimation, five potential variables were considered included: time poverty, parental investments, and three time use categories (sleeping, work, education). Therefore, at least five instruments are needed to identify the model[10]. Drawing on previous literature, the following instruments were tested (see Table A.1 for more details):

- Prices of a common basket of 21 goods recovered in all rounds, which includes investments goods (notebooks, shoes, children's clothes), medicines, food and fuel. As in Keane et al. (2022) all the relevant prices are included, without restricting them to only the prices of investment goods. This choice allows for a more accurate representation of the households' budget constraint. Given the multiplicity of prices, I employed Principal Component Analysis (PCA) to construct two price indices (taken as the first and second principal components). These indices are then used as instruments in the analysis. The use of prices as instruments relies on the assumption that the effect of prices on children's cognition, conditioned on household resources and the current stock of child cognition, operates through their influence on time use and parental investments.
- Wages for agricultural and non-agricultural activities. Similar to the approach with prices, PCA is used to reduce the dimensions of the number of wages, resulting in the creation of two indices based on the first two principal components. Wages were also employed as instruments in the work by Keane et al. (2022). Similarly, the assumption is that the effect of wages on child cognition will be through their effects on parental investments or children's time use, conditioned on household resources and the current stock of chid cognition. For instance, higher wages may lead to increased maternal labour supply and household income, which could result in increased parental investments. Furthermore, it may also increase children's engagement in household chores, which could potentially increase time poverty.

[10] Note that, in order to reduce the demand on the number of instruments, I have merged the categories of work at the household and working outside the household as one single category called "work".

- Three variables for shocks and adverse conditions, divided into the following categories: **Economic shocks,** if the household has experienced at least one of the following: increase in agricultural input prices, decrease in agricultural output prices, death of livestock or loss of job or source of income. **Environmental shocks**, if the household has experienced at least one of the following: drought, flooding, pest on crops or crop failure; and finally, **family shocks**, if they have experienced at least one of the following: death of father or mother, illness of father or mother or the birth of a new family member. Families facing these shocks may employ child labour as a coping mechanism, potentially affecting children's school attendance (Alam, 2015; Bandara et al., 2015; Beegle et al., 2006) and potentially increasing time poverty. Additionally, the decline in household income may lead to reduced parental investments in children.

Therefore, I have seven instruments (two indices of prices, two indices of wages and three variables for shocks) for five endogenous variables. The model estimated was the following:

First stage regression: $$T_{i,t,m} = \pi_n' X_{i,t,n} + \phi_n' Z_{i,t,j} + \varepsilon_{i,t} \quad (42)$$

Second stage regression: $$Y_{i,t} = \rho Y_{i,t-1} + \beta_n' X_{i,t,n} + \psi_m' \widehat{T_{i,t,m}} + \epsilon_{i,t} \quad (43)$$

Where $Z_{i,t,j}$ is the vector of the $j = 7$ instruments, $T_{i,t,m}$ is vector of the $m = 5$ endogenous regressor, and $X_{i,t,n}$ is the vector of n exogenous controls (see Section 5.2).

Following Keane et al. (2022) , I included a lagged test in the second stage regression. Therefore, my approach is a combination of a CVA with an IV model.

Table A1. Instruments for the IV approach

Category	Variables
Prices of goods	Notebooks, shoes, shirts, trousers, skirts, rehydration salt, paracetamol, amoxicillin, mebendazole, potatoes, rice, spaghetti, coffee, milk, sugar, cooking oil, salt, cigarettes, detergent and fuel (kerosene or gas)
Wages	Salaries for agricultural and non-agricultural jobs
Economic shocks	Experiencing at least one of the following: increase in agricultural input prices, decrease in agricultural output prices, death of livestock or loss of job or source of income
Environmental shocks	Experiencing at least one of the following: drought, flooding, pest on crops or crop failure
Familiar shocks	Experiencing at least one of the following: death of father or mother, illness of father or mother and the birth of a new family member

11.1.1. Relevance of the instruments

To test for weak instruments in the presence of multiple endogenous variables, I followed the approach suggested by Sanderson & Windmeijer (2016). Instead of relying on the F-statistic from the first stage regressions, as done in the case of one endogenous regressor, I employ the Cragg-Donald F-statistic. I also use the Kleibergen-Paap F statistic, which is considered more robust when standard errors are clustered (Baum et al., 2007).

Both F-statistics should be compared to the Stock-Yogo critical values (Stock & Yogo, 2005). However, these critical values are not available for the case of five endogenous regressors and five instruments. Therefore, as in Keane et al. (2022), I employ the common rule of thumb of an F-statistics equal to or greater than 10 as an indicator of an instruments' robustness.

Tables A.2-A3 presents the Cragg-Donald and the Kleibergen-Paap F statistics. Both statistics were lower than 10 in all cases, suggesting that my instruments are weak. A similar result was found by Keane et al (2022). Their instruments for Peru, Vietnam and India were weak. As discussed by the authors, weak instruments for children's time were commonly observed in previous literature on the impact of child work on children's human capital.

11.1.2. Estimated coefficients

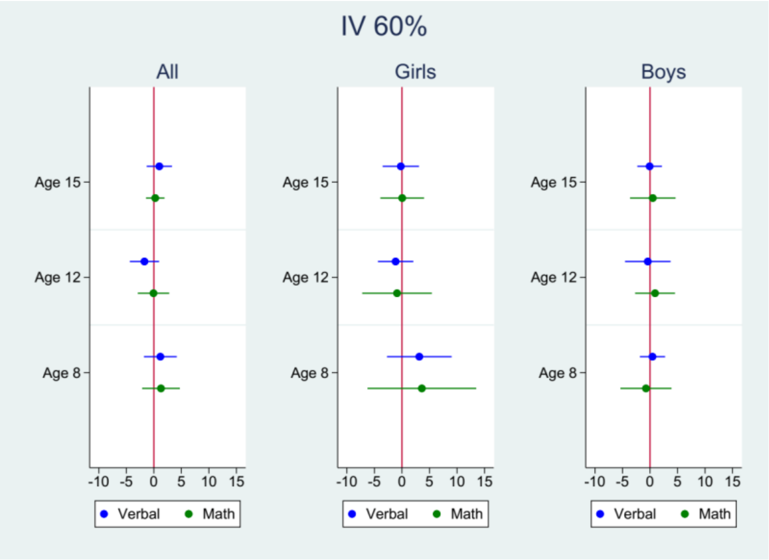
In this section, I show the estimated coefficients for the current effects of time poverty on test scores at all ages (Table A2 and Figure A1) and the lagged effects of time poverty on test scores at the age of 15 (Table A3 and Figure A2), respectively. The weakness of the instruments, i.e., their limited ability to produce enough variation in the instrumented variables, is evident from the large standard errors observed in the estimated coefficients. None of the estimations of current and lagged effects were statistically significant. Furthermore, the estimated coefficients were usually larger in absolute value than those from the CVA model (for instance, the effect of time poverty on verbal scores at the age of 15 for all the sample was -0.0225 in the CVA model versus 0.991 in the IV model)

Table A2. Effect of current time poverty on test scores, IV model

Age	Outcomes	Verbal			Math		
		All	Girls	Boys	All	Girls	Boys
Age 15	Current time poverty (age 15)	0.991 (1.173)	-0.205 (1.684)	-0.0655 (1.142)	0.247 (0.860)	0.0482 (2.025)	0.506 (2.110)
	Weak identification (K-P F-stat)	0.342	0.358	0.113	0.869	0.436	0.086
	Weak Identification (Craig Donald F-stat)	0.373	0.187	0.0890	0.518	0.248	0.071
Age 12	Current time poverty (age 12)	-1.712 (1.356)	-1.149 (1.641)	-0.400 (2.113)	-0.0706 (1.458)	-0.890 (3.229)	0.918 (1.849)
	Weak identification (K-P F-stat)	0.427	0.542	0.355	0.546	0.483	0.270
	Weak Identification (Craig Donald F-stat)	0.359	0.277	0.230	0.372	0.279	0.200
Age 8	Current time poverty (age 8)	1.178 (1.523)	3.158 (2.997)	0.452 (1.169)	1.290 (1.748)	3.620 (5.038)	-0.731 (2.368)
	Weak identification (K-P F-stat)	0.574	0.285	0.210	0.529	0.108	0.112
	Weak Identification (Craig Donald F-stat)	0.601	0.262	0.223	0.495	0.152	0.191
	N	1472	726	746	1468	723	745

Notes:
IV: Instrumental variables model
K-P F-stat: Kleibergen-Paap F-statistic for the first stage regression
Craig Donal F-stat: Craig Donald F-statistic for the first stage regression
Standard errors in parentheses, clustered at the Young Lives survey cluster level.
P-values : * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see Section 5.2)

Figure A1. Effect of current time poverty on test scores, IV model.



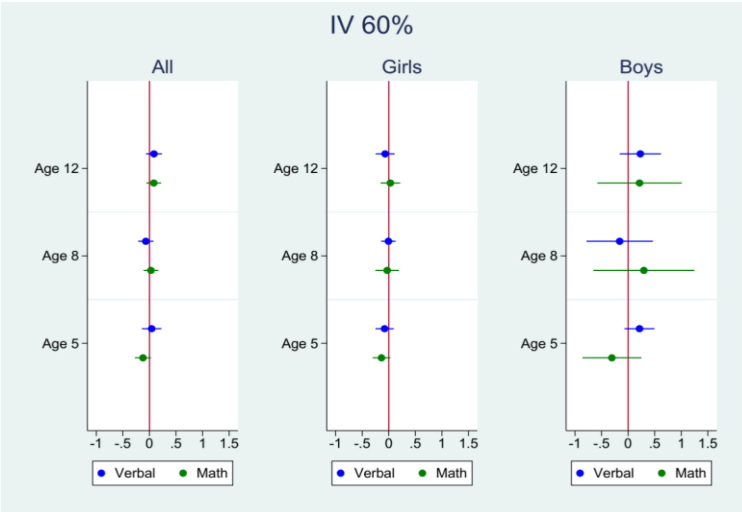
Notes:
IV: Instrumental variables model
95% confidence intervals
P-values: * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see Section 5.2)

Table A3. Effect of experiencing time poverty at the ages of 5, 8 and 12 on test scores at the age of 15, IV model

Outcome	Verbal			Math		
	All	Girls	Boys	All	Girls	Boys
Current time poverty (age 15)	0.991 (1.17)	-0.205 (1.68)	-0.065 (1.14)	0.247 (0.86)	0.0482 (2.03)	0.506 (2.11)
Time poverty t-1 (age 12)	0.0838 (0.08)	-0.0699 (0.09)	0.229 (0.20)	0.0807 (0.07)	0.0294 (0.09)	0.214 (0.41)
Time poverty t-2 (age 8)	-0.0692 (0.07)	-0.00685 (0.07)	-0.159 (0.32)	0.0274 (0.07)	-0.0309 (0.11)	0.295 (0.49)
Time poverty t-3 (age 5)	0.041 (0.09)	-0.0797 (0.09)	0.213 (0.14)	-0.122 (0.08)	-0.138 (0.09)	-0.306 (0.28)
N	1472	726	746	1468	723	745
Weak identification (K-P F-stat)	0.342	0.358	0.113	0.869	0.436	0.086
Weak Identification (Craig Donald F-stat)	0.373	0.187	0.089	0.518	0.248	0.0707

Notes:
IV: Instrumental variables model
K-P F-stat: Kleibergen-Paap F statistic for the first-stage regression
Craig Donal F-stat: Craig Donald F-statistic for the first stage regression
Standard errors in parentheses, clustered at the Young Lives survey cluster level
P-values : * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see Section 5.2)

Figure A2. Effect of experiencing time poverty at the ages of 5, 8 and 12 on test scores at the age of 15, IV model



Notes:
IV: Instrumental variables model
95% confidence intervals
P-values: * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see Section 5.2)

11.2. Dynamic Factor Models

Further robustness checks were carried out employing the DFM approach. In this approach, I assume that cognition, parental investments and time poverty are latent. The model was estimated following a four-step approach (Attanasio, Meghir, et al., 2020; Heckman et al., 2013):

1. In the first step, I used proxies for cognition, time poverty and parental investments, to set up a measurement system and recover the "factor loadings for each measure".
2. In the second step, I used these factor loadings to obtain the error-corrected time poverty, parental investments and children's cognition.
3. In the third step, I used the error-corrected parental investments to estimate the investment equation (Equation 22 in Section 4.3.4.3).
4. In the fourth step, I estimated the following production function of human capital:

$$\widehat{\theta_{\iota,t}} = \beta_{0t} + \beta_{1t}\widehat{\theta_{\iota,t-1}} + \gamma_{1t}\widehat{\lambda_{\iota,t}} + \gamma_{2t}\widehat{\lambda_{\iota,t-a}} + \varphi_{1t}\widehat{\delta_{\iota,t}} + \varphi_{2t}\widehat{\delta_{\iota,t-a}} + \pi_{n,t}\mathrm{X}_{i,t,n} + \partial_{i,t}\widehat{\varsigma_{\iota,t}} + \kappa_{i,t} \tag{44}$$

Where $\widehat{\theta_{\iota,t}}$ is the error-corrected measure of cognition, $\widehat{\lambda_{\iota,t}}$ are the error-corrected parental investments, $\widehat{\delta_{\iota,t}}$ is the error corrected time poverty, $\mathrm{X}_{i,t,n}$ is the vector of n exogenous variables and $\widehat{\varsigma_{\iota,t}}$ is the error term from the investment equation, used to correct for endogeneity of parental investments in a control function approach. $\kappa_{i,t}$ is a well-behaved error term.

It is important to note that in Step 3 I used the error-corrected versions of cognition and parental investments and in Step 4 I used the error-corrected versions of cognition, time poverty, parental investments and the estimated error term from the investment equation. Therefore, I computed standard errors using clustered bootstrapping to account for the inclusion of estimated variables.

Finally, as discussed in Section 4.5.4.2, due to the utilisation of lags for the proxies for cognition, discretionary time (to estimate time poverty) and parental investments in Equations 15-17, the identification of the error-corrected variables is possible only from ages 8, 12, and 15. Furthermore, given the need for a first lag of the error-corrected cognition measure in Equation 23, the model was estimated solely for ages 12 and 15.

Finally, as also previously discussed, in the case of the DFM I have only a single measure of cognition, composed by the two proxies used in the measurement system (verbal and math test scores).

Table A4 and Figure A3 show the effect of time poverty on current child cognition at the ages of 12 and 15.

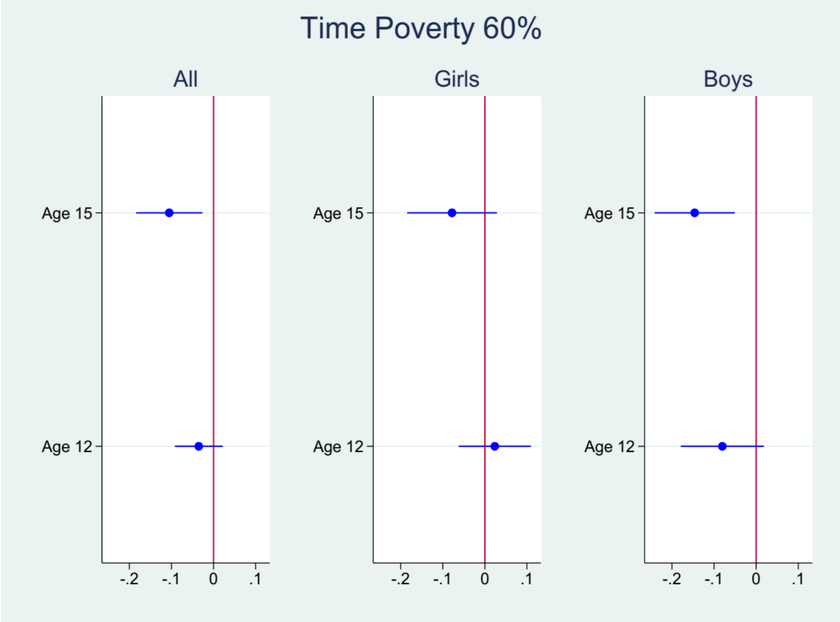
The results show a negative effect of time poverty on chid cognition for the entire sample of children (-0.105 sd, p-value < 0.05), which was driven by the effect among boys (-0.146, p-value < 0.05). All the other coefficients were not statistically significant.

Table A4. Effect of current time poverty on chid cognition, DFM model

Outcome	**Cognition**		
	All	**Girls**	**Boys**
Current time poverty (age 15)	-0.105** (0.0402)	-0.0780 (0.0545)	-0.146** (0.0485)
Current time poverty (age 12)	-0.0352 (0.0291)	0.0236 (0.0438)	-0.0804 (0.0503)
N	1454	713	741

Notes:
DFM: Dynamic Factor Model
Standard errors in parentheses, clustered at the Young Lives survey cluster level.
P-values : * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see Section 5.2)

Figure A3. Effect of current time poverty on child cognition, DFM



Notes:
DFM: Dynamic Factor Model
95% confidence intervals
Controlled for relevant individual and household socioeconomic characteristics (see Section 5.2)

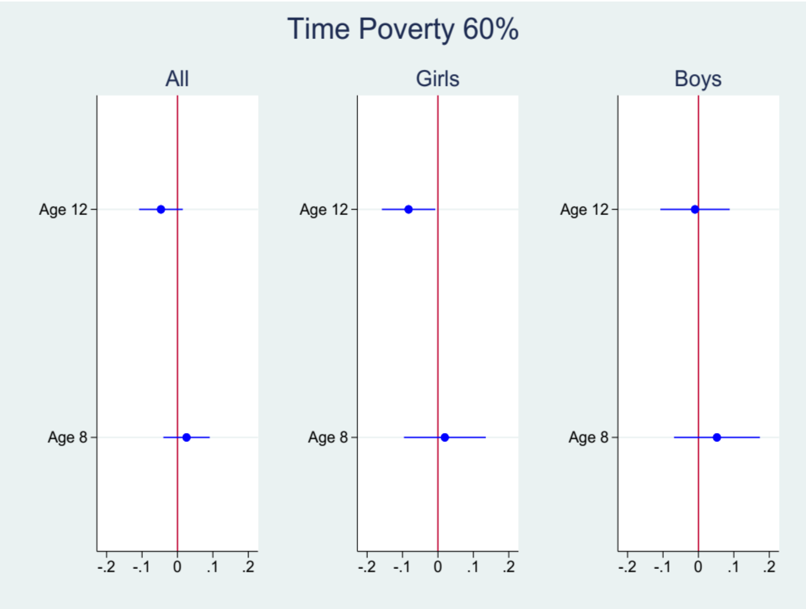
Table A5 and Figure A4 shows the effect of experiencing time poverty at the ages of 8 and 12 on cognition at the age of 15. Time poverty at the age of 12 had a negative effect on cognition at the age 15 among the subsample of girls (-0.08 sd, p-value < 0.05). This result may seem counterintuitive, as the current effect of time poverty at the age of 12 was non-significant but positive (Table A4). However, it appears to have a delayed negative effect three years later. As discussed previously, similar statistically significant delayed effects for time poverty at the age of 12 were also observed in the CVA model (Table 7). However, in that case, the effects three years later were positive for both mathematical skills across the entire sample and for verbal scores among boys.

Table A5. Effect of experiencing time poverty at the ages of 8 and 12 on child cognition at the age of 15, DFM

Variable	All	Girls	Boys
Current time poverty (age 15)	-0.105** (0.0402)	-0.0780 (0.0545)	-0.146** (0.0485)
Time poverty t-1 (age 12)	-0.0466 (0.0315)	-0.0829** (0.0385)	-0.00965 (0.0499)
Time poverty t-2 (age 8)	0.0257 (0.0334)	0.0196 (0.0589)	0.0522 (0.0619)
Lag of cognition	0.705*** (0.0228)	0.709*** (0.0330)	0.710*** (0.0432)
N	1454	713	741

Notes:
DFM: Dynamic Factor Model
Standard errors in parentheses, clustered at the Young Lives survey cluster level
P-values : * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see Section 5.2)

Figure A4. Effect of experiencing time poverty at the ages of 8 and 12 on child cognition at the age of 15, DFM



Notes:
DFM: Dynamic Factor Model
95% confidence intervals
Controlled for relevant individual and household socioeconomic characteristics (see Section 5.2)

11.3. Results from the Callaway and Sant'Anna estimator

Table A6. The effect of Juntos on verbal test scores by cohort-round, CS estimator

Cohort	Round (Age)	ATET	Std. Err.	95% Lower CI	95% Upper CI
All children					
3	3 (8)	-0.39	0.16	-0.81	0.04
3	4 (12)	-0.52	0.24	-1.15	0.11
3	5 (15)	-0.54	0.20	-1.06	-0.01
4	3 (8)	-0.13	0.17	-0.57	0.31
4	4 (12)	0.06	0.11	-0.23	0.35
4	5 (15)	0.08	0.12	-0.23	0.39
5	3 (8)	-0.07	0.08	-0.28	0.14
5	4 (12)	0.04	0.07	-0.15	0.23
5	5 (15)	0.05	0.09	-0.18	0.28
Girls					
3	3 (8)	-0.58	0.17	-1.05	-0.10
3	4 (12)	-0.82	0.21	-1.41	-0.23
3	5 (15)	-0.66	0.28	-1.44	0.11
4	3 (8)	-0.06	0.15	-0.48	0.35
4	4 (12)	-0.05	0.20	-0.62	0.51
4	5 (15)	0.004	0.13	-0.36	0.37
5	3 (8)	-0.24	0.14	-0.64	0.16
5	4 (12)	0.00	0.14	-0.38	0.38
5	5 (15)	0.16	0.11	-0.15	0.47
Boys					
3	3 (8)	-0.24	0.18	-0.73	0.25
3	4 (12)	-0.36	0.20	-0.89	0.17
3	5 (15)	-0.42	0.18	-0.91	0.06
4	3 (8)	-0.31	0.25	-0.99	0.36
4	4 (12)	0.14	0.14	-0.22	0.51
4	5 (15)	0.20	0.22	-0.38	0.78
5	3 (8)	0.04	0.10	-0.23	0.31
5	4 (12)	0.06	0.09	-0.16	0.29
5	5 (15)	-0.02	0.13	-0.36	0.33

Notes: CS: Callaway and Sant'Anna's estimator (Callaway & Sant'Anna, 2021). Standard errors in parenthesis, clustered at the Young Lives survey cluster level. 95% simultaneous confidence intervals. Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

Table A7. The effect of Juntos on math test scores by cohort-round, CS estimator

Cohort	Round (Age)	ATET	Std. Err.	95% Lower CI	95% Upper CI
All children					
3	3 (8)	-0.37	0.21	-0.91	0.18
3	4 (12)	-0.38	0.15	-0.79	0.02
3	5 (15)	-0.26	0.21	-0.82	0.30
4	3 (8)	0.06	0.19	-0.43	0.55
4	4 (12)	0.02	0.13	-0.32	0.35
4	5 (15)	0.16	0.09	-0.08	0.39
5	3 (8)	-0.23	0.16	-0.64	0.18
5	4 (12)	0.11	0.14	-0.27	0.48
5	5 (15)	-0.001	0.15	-0.39	0.39
Girls					
3	3 (8)	-0.43	0.36	-1.36	0.51
3	4 (12)	-0.06	0.27	-0.77	0.64
3	5 (15)	-0.12	0.31	-0.93	0.68
4	3 (8)	0.07	0.24	-0.54	0.69
4	4 (12)	0.05	0.18	-0.42	0.52
4	5 (15)	0.11	0.11	-0.18	0.39
5	3 (8)	0.06	0.17	-0.39	0.51
5	4 (12)	0.06	0.21	-0.48	0.61
5	5 (15)	-0.09	0.18	-0.54	0.37
Boys					
3	3 (8)	-0.38	0.21	-0.93	0.16
3	4 (12)	-0.54	0.20	-1.06	-0.02
3	5 (15)	-0.30	0.20	-0.82	0.21
4	3 (8)	0.01	0.23	-0.57	0.59
4	4 (12)	0.01	0.10	-0.25	0.26
4	5 (15)	0.17	0.10	-0.09	0.43
5	3 (8)	-0.50	0.21	-1.05	0.05
5	4 (12)	0.07	0.19	-0.43	0.56
5	5 (15)	0.05	0.13	-0.27	0.38

Notes: CS: Callaway and Sant'Anna's estimator (Callaway & Sant'Anna, 2021). Standard errors in parenthesis, clustered at the Young Lives survey cluster level. 95% simultaneous confidence intervals. Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

Table A8. The effect of Juntos on stunting status by cohort-round, CS estimator

Cohort	Round (Age)	ATET	Std. Err.	95% Lower CI	95% Upper CI
All children					
3	3 (8)	0.12	0.11	-0.15	0.39
3	4 (12)	0.09	0.10	-0.15	0.34
3	5 (15)	0.14	0.11	-0.15	0.43
4	3 (8)	0.02	0.10	-0.23	0.27
4	4 (12)	-0.03	0.06	-0.18	0.13
4	5 (15)	-0.10	0.04	-0.20	0.00
5	3 (8)	-0.07	0.09	-0.29	0.16
5	4 (12)	0.005	0.04	-0.09	0.10
5	5 (15)	0.02	0.05	-0.12	0.15
Girls					
3	3 (8)	0.28	0.15	-0.10	0.67
3	4 (12)	0.26	0.11	-0.01	0.54
3	5 (15)	0.18	0.19	-0.28	0.65
4	3 (8)	0.01	0.14	-0.34	0.36
4	4 (12)	0.00	0.12	-0.29	0.29
4	5 (15)	-0.11	0.07	-0.30	0.08
5	3 (8)	-0.09	0.18	-0.53	0.35
5	4 (12)	0.02	0.07	-0.14	0.19
5	5 (15)	0.001	0.09	-0.23	0.23
Boys					
3	3 (8)	0.02	0.12	-0.30	0.33
3	4 (12)	-0.02	0.12	-0.34	0.30
3	5 (15)	0.12	0.13	-0.22	0.45
4	3 (8)	0.09	0.10	-0.18	0.36
4	4 (12)	-0.04	0.04	-0.14	0.05
4	5 (15)	-0.08	0.06	-0.23	0.08
5	3 (8)	-0.04	0.06	-0.20	0.12
5	4 (12)	0.01	0.04	-0.09	0.12
5	5 (15)	0.03	0.08	-0.18	0.25

Notes: CS: Callaway and Sant'Anna's estimator (Callaway & Sant'Anna, 2021). Standard errors in parenthesis, clustered at the Young Lives survey cluster level. 95% simultaneous confidence intervals. Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

Table A9. The effect of Juntos on BMI-for-age z-scores by cohort-round, CS estimator

Cohort	Round (Age)	ATET	Std. Err.	95% Lower CI	95% Upper CI
All children					
3	3 (8)	-0.06	0.13	-0.39	0.26
3	4 (12)	-0.32	0.16	-0.75	0.10
3	5 (15)	-0.22	0.14	-0.58	0.13
4	3 (8)	-0.14	0.10	-0.40	0.13
4	4 (12)	-0.22	0.09	-0.46	0.01
4	5 (15)	-0.09	0.07	-0.28	0.10
5	3 (8)	-0.02	0.07	-0.21	0.17
5	4 (12)	-0.31	0.08	-0.50	-0.11
5	5 (15)	0.24	0.06	0.09	0.39
Girls					
3	3 (8)	-0.10	0.13	-0.44	0.24
3	4 (12)	-0.39	0.20	-0.92	0.15
3	5 (15)	-0.26	0.24	-0.90	0.39
4	3 (8)	-0.21	0.14	-0.57	0.15
4	4 (12)	-0.29	0.16	-0.71	0.12
4	5 (15)	-0.12	0.11	-0.41	0.17
5	3 (8)	-0.17	0.13	-0.52	0.18
5	4 (12)	-0.35	0.14	-0.71	0.01
5	5 (15)	0.21	0.08	0.01	0.41
Boys					
3	3 (8)	-0.09	0.17	-0.55	0.37
3	4 (12)	-0.30	0.16	-0.72	0.12
3	5 (15)	-0.25	0.13	-0.60	0.09
4	3 (8)	-0.13	0.13	-0.48	0.22
4	4 (12)	-0.13	0.10	-0.38	0.13
4	5 (15)	-0.06	0.17	-0.52	0.40
5	3 (8)	0.09	0.12	-0.23	0.41
5	4 (12)	-0.25	0.08	-0.46	-0.05
5	5 (15)	0.26	0.10	-0.01	0.54

Notes: CS: Callaway and Sant'Anna's estimator (Callaway & Sant'Anna, 2021). Standard errors in parenthesis, clustered at the Young Lives survey cluster level. 95% simultaneous confidence intervals. Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

Table A10. The effect of Juntos on Height-for-age z-scores by cohort-round, CS estimator

Cohort	Round (Age)	ATET	Std. Err.	95% Lower CI	95% Upper CI
All children					
3	3 (8)	-0.16	0.11	-0.46	0.13
3	4 (12)	-0.18	0.12	-0.51	0.15
3	5 (15)	-0.05	0.08	-0.25	0.15
4	3 (8)	-0.06	0.09	-0.31	0.19
4	4 (12)	-0.03	0.09	-0.26	0.21
4	5 (15)	0.23	0.09	-0.02	0.47
5	3 (8)	-0.05	0.08	-0.26	0.16
5	4 (12)	0.01	0.05	-0.13	0.15
5	5 (15)	0.02	0.08	-0.20	0.23
Girls					
3	3 (8)	-0.42	0.19	-0.88	0.04
3	4 (12)	-0.49	0.17	-0.91	-0.07
3	5 (15)	-0.22	0.15	-0.60	0.16
4	3 (8)	-0.04	0.16	-0.44	0.36
4	4 (12)	0.00	0.15	-0.38	0.38
4	5 (15)	0.26	0.11	-0.02	0.55
5	3 (8)	-0.08	0.20	-0.57	0.41
5	4 (12)	-0.04	0.09	-0.26	0.17
5	5 (15)	0.17	0.14	-0.17	0.51
Boys					
3	3 (8)	0.07	0.12	-0.23	0.38
3	4 (12)	0.10	0.14	-0.26	0.45
3	5 (15)	0.00	0.18	-0.47	0.47
4	3 (8)	-0.10	0.07	-0.29	0.10
4	4 (12)	-0.09	0.07	-0.27	0.08
4	5 (15)	0.05	0.12	-0.26	0.35
5	3 (8)	-0.03	0.11	-0.33	0.26
5	4 (12)	0.04	0.08	-0.16	0.24
5	5 (15)	-0.13	0.07	-0.31	0.06

Notes: CS: Callaway and Sant'Anna's estimator (Callaway & Sant'Anna, 2021). Standard errors in parenthesis, clustered at the Young Lives survey cluster level. 95% simultaneous confidence intervals. Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

11.4. Results from the Two-Ways Fixed-Effects model

Table A 11. The effect of time poverty on cognitive and nutritional outcomes, TWFE model

Outcome	All	Girls	Boys
Verbal	-0.109 (0.12)	-0.139 (0.15)	-0.0739 (0.11)
Math	-0.153 (0.09)	-0.202 (0.12)	-0.107 (0.12)
Stunting status	-0.0757** (0.03)	-0.0792** (0.04)	-0.0714* (0.04)
BMI-for-age z-score	-0.145** (0.06)	-0.203*** (0.07)	-0.0973 (0.09)
Height-for-age z-score	0.0945** (0.04)	0.118*** (0.04)	0.0771 (0.07)
N	1284	631	653

Notes:
TWFE: Two-Ways Fixed Effects.
Standard errors in parenthesis, clustered at the Young Lives survey cluster level.
Controlled for relevant individual and household socioeconomic characteristics (see Section 7.3).

11.5. Effects of Juntos on education time and parental investments

Table A 12. The effect of Juntos on education time and parental investments, 2x2 DID model

Outcome	All	Girls	Boys
Education time	0.211 (-0.648,1.069)	0.429 (-0.491,1.348)	-0.00446 (-0.960,0.951)
Parental investments	0.385** (0.00304,0.767)	0.303 (-0.389,0.995)	0.547* (-0.0441,1.137)
N	1534	767	767

Notes:
Education time: time at school and time studying at home
Parental investments: household expenditures (in 100's of soles) in children's clothes, books and stationery and school uniforms
2x2 DID: Two-periods two-groups difference-in-differences model.
95% confidence intervals in parenthesis.
P-values: * p<0.10, ** p<0.05, *** p<0.01
Controlled for relevant individual and household socioeconomic characteristics (see Section 6.3).