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Can We Grow with our Children? The Effects of a Comprehensive Early Childhood Development Program*

Abstract

I exploit the staggered roll-out of a universal early childhood development program in Chile to assess the impact of a comprehensive approach to early childhood development on outcomes in middle childhood. Using variation across time and municipalities, I study outcomes such as school performance, cognitive development, parental stress, household relationships, and health. I use administrative data on students as well as newborns in Chile, standardized test scores of all 4th graders, and an extensive early childhood development survey. I find positive and significant effects on school performance. The effect is less pronounced for girls and the socioeconomically vulnerable population. The improvements in learning outcomes are driven by improvements in intra-household relations. Comprehensive programs are powerful tools but have several flaws.

JEL Code: I24, I28, I38, J13, J24, O15
Keywords: Education and inequality; government policy; children; human capital

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1 Introduction

The first five years of children's lives lay the foundation for their human capital accumulation and labor market outcomes during the rest of their lives (Currie and Almond 2011). Many have identified the first years of a child's life as the basis for sustainable development (Daelmans et al. 2017). At the same time, estimates by the World Health Organization (2020) find that 250 million children, or 43 % of all children from low- and middle-income countries, were unable to fulfill their full development potential in 2016. In 2017, 1 in 6 children, or 356 million children, lived in extreme poverty and 41.5 % lived in poverty (Silwal et al. 2020).[1] Children are more than twice as likely as adults to be extremely poor, and while poverty has decreased worldwide, it has decreased less for children (Silwal et al. 2020). More recently, the pandemic has increased the number of children living in multidimensional poverty by 15 % (UNICEF 2022). It is therefore more important than ever to ask how to overcome the detrimental factors to child development. Answering this question can help to counter the intergenerational transmission of poverty.

In Chile, the development gap for children under five years has fallen by 10 percentage points in only a decade. In 2006, 34 % of children under age five in Chile did not reach their development potential (Milman 2018). 10 years later, this share has decreased to 24 %. What led to this sharp decline in only 10 years? The paper at hand analyzes the impact of a nationwide, comprehensive, universal early childhood development program - called Chile Crece Contigo[2] (hereinafter ChCC) - which was introduced in 2007. I assess the impact of the program on a variety of outcomes in education, child development as well as parenting 12 years after its introduction. I take advantage of the gradual roll-out of ChCC across municipalities between July 2007 and August 2008. I apply a regression discontinuity approach matching the date of ChCC's introduction by municipality with children's dates of birth and places of residence.

[1] Extreme poverty is defined as living on less than 1.90 PPP-US-Dollars per day and poverty as living on less than 3.20 PPP-US-Dollars per day.

[2] In English: Chile Grows With You.

Analyzing the program's impact can help to generate answers on how to secure the fulfillment of children's development potentials in the most crucial period of their lives. While there is an increasing literature studying the effects of early childhood interventions, the focus is mainly on targeted programs (examples include the Perry Preschool program (Heckman et al. 2010), the Jamaika study (Gertler et al. 2014), the Abecedarian experiment (Campbell et al. 2014), or targeted programs in Colombia (Attanasio et al. 2020)). However, a recent review by Richter et al. (2017) identifies the need for more nationwide early childhood development (ECD) programs, as do Black et al. (2017)[3] and Daelmans et al. (2017)[4]. ChCC is a pioneer program of this kind. ChCC included factors that were recently identified as best practices in the literature already more than a decade ago (namely in 2007). I can therefore examine whether these best practices do indeed work as predicted by experts. This study contributes to the existing literature by exploring the question of whether positive effects on child development can only be achieved through targeted programs or also through universal programs. Moreover, Almond, Currie, and Duque (2018) identify a lack of research focusing on middle childhood within this field. My work helps to close this gap, as it analyzes the program's impact on outcomes in middle childhood.

Studying ChCC is of high policy-relevance since it has been the basis for the design of several similar programs in numerous countries.[5] It is one of the showcase models used by international organizations (Richter et al. 2017). Chile offers a relevant context for the underlying research question as it is a bench-marking country for Latin-American countries, but also for OECD countries.[6] We can consequently apply several of the findings to countries

[3]The authors state that there is an urgent need for early childhood development programs that incorporate multi-sectoral entry points for justice and equality. These include health, nutrition, security and safety, responsible care, and early learning.

[4]The researchers highlight that families who cannot provide their children with the necessary input to reach their developmental potential need support. This support should consist of materials, financial resources, knowledge, time and professional assistance, as well as protection, prevention and education. They recommend moving from small-scale civil society initiatives to nationwide programs that promote early childhood development.

[5]ChCC has inspired similar programs in Colombia, Peru, Uruguay, El Salvador and South Africa (Ministry of Health 2017).

[6]Chile joined the OECD in 2010.

like the US or countries in Europe, but also to countries with similar characteristics in Latin-America. To the best of my knowledge, this is the first paper to study the overall impact of ChCC on a variety of child development outcomes, such as schooling outcomes as well as cognitive and non-cognitive skills.

I use a variety of rich datasets to investigate the effect of ChCC on children's outcomes. To measure academic achievement, I rely on administrative data on grade point averages for the entire student population in Chile. I additionally look at standardized test scores in reading and math of all 4th graders in Chile. To measure cognitive and non-cognitive skills I use the Longitudinal Survey on Early Childhood (ELPI) containing rich information on children's development in various domains, as well as on parent-child relationships and the home environment. For the impact of early childhood education, I rely on administrative data from the Ministry of Education and Ministry of Health. Lastly, I use administrative data on newborns to measure outcomes at birth.

I find that the program has a positive effect on grade point averages and standardized math and reading test scores. The effects are more marked for boys than for girls. The impact is smaller for socioeconomically vulnerable children. The positive effect on school performance seems to be driven by important improvements in intra-household relations. Still, these improvements are limited to material goods. There is no evidence of behavioral changes of parents. Moreover, while participation in ChCC seems to effectively improve human capital outcomes in middle childhood, this paper provides evidence of important shortfalls of the program. Firstly, the program does not lead to improvements in outcomes at birth. Next, the evidence on the program's impact on cognitive and non-cognitive child development, as well as on early childhood education attendance rates, is inconclusive. This could mean that there is no clear causal effect of the program on these outcomes. Furthermore, ChCC's marginal value of public funds (MVPF) is only 1.41. This MVPF is lower than the MVPF for similar programs in the US, which could be due to inefficiencies created by the universal nature of the program. Another possible reason could be that certain subgroups benefit less from the program than others. Lastly, the effects are larger for children which are part of the second phase of the program's roll-out. This makes a case for piloting early childhood interventions.

I conduct several robustness checks to test the validity of my findings. Firstly, I consider several cutoff windows and account for different polynomial orders in my local randomization discontinuity approach. Next, I show that it is unlikely that my results are driven by a potential treatment manipulation around the cutoff. Moreover, I show that the program's impact is insignificant around a placebo cutoff. I also analyze the role of pre-treatment differences and observable student characteristics before the roll-out of ChCC as well as close to the cutoff. Additionally, I take into account potential inclusion and exclusion errors and allow for non-compliance in the participation in ChCC. My results from a fuzzy randomization discontinuity approach make clear that imperfect compliance could jeopardize the program's effectiveness. I then employ a staggered difference-in-difference estimation and an event study to further validate my findings. The findings from these two alternative empirical approaches provide evidence on that a local randomization approach is the most appropriate estimation strategy in this setting. I show that my results are not confounded by the 2008/2009 financial crisis or copper prices. The findings are also not driven by internal migration patterns.

My paper contributes to the literature studying the effects of early childhood interventions. As ChCC is a pioneer program, one of the first multisectoral, universal, nationwide ECD programs, this is the only paper to date that studies the impact of such a multifaceted program on outcomes in middle childhood. To date, there is only one other paper studying the effect of ChCC, that of Clarke, Méndez, and Sepúlveda (2020). Unlike my work, they study the effect of one specific component of *Chile Crece Contigo* on different health variables at birth.[7]

[7] They apply a staggered difference-in-differences strategy and a regression discontinuity design and find that the health components of ChCC have a positive effect on several health variables at birth, such as body weight and height at birth.

My results also contribute to a relatively large literature studying the effects of having access to social safety nets during early childhood. While one literature stream analyzes the impact on health outcomes early in life[8], a large number of papers assesses the long-term effects[9]. In general, most of the work in this field focuses on the developed world and few authors have analyzed the effect of early childhood development programs in developing countries.[10]

Moreover, my findings contribute to questions on inter-generational poverty, analyzing whether income alone is really the gateway to improvements in children's outcomes, or whether we need a more integrated approach. ChCC is a program with a strong socioeconomic development focus, trying to address cognitive, emotional as well as behavioral lags in children's development through the program's comprehensive health, education and parental approach. Parental investment, cognitive as well as emotional stimulation, and a surround health program are the entry points of ChCC to foster children's development. It therefore diverges from programs trying to lift people out of poverty through cash transfers.

My paper shows that a comprehensive approach like ChCC can effectively build human capital in the early stages of life. Still, the evidence points to weaknesses in these types of programs. While the comprehensive approach of ChCC leads to improved schooling outcomes, girls and the vulnerable population benefit to a lesser extent. This means that pre-existing gaps between different socioeconomic groups could increase under comprehensive, universal early childhood development programs. Moreover, when allowing for non-compliance, the program's impact becomes insignificant. This further stresses potential inefficiencies of universal programs when their take-up is imperfect. The relatively low MVPF for ChCC further indicates that targeted early childhood development programs might be more efficient

[8] See for example work by Almond, Hoynes, and Schanzenbach (2011), Hoynes, Page, and Stevens (2011), Amarante et al. (2016), Goodman-Bacon (2018), Ko, Howland, and Glied (2020).

[9] Example studies conducted by Chetty et al. (2011), Hoynes, Schanzenbach, and Almond (2016a), Akee, Jones, and Simeonova (2020), Bailey et al. (2020) and Bailey, Timpe, and Sun (2020) give a great entry to the topic.

[10] One paper by Amarante et al. (2016) analyzes the effect of the PANES program on birth outcomes in Uruguay.

in creating long-lasting changes in the human capital accumulation and development trajectories of countries. Targeted early childhood development programs might also be more cost-efficient. In addition, this paper shows that later roll-out groups benefit more from the program. Therefore, it is crucial for policymakers to pilot early childhood interventions. They can then use the experiences and insights gained through pilot project to maximize the effectiveness of universal, comprehensive programs.

Next, the main drivers of the observed positive impact seem to be improvements in intra-household relations. Still, the evidence shows that these improvements are limited to material goods. Participation in ChCC does not influence parents' actual behavior. Therefore, while parental workshops and in-kind transfers seem to be important mechanisms behind the program, the underlying methodology should be revised to generate real and important behavioral changes in parental care. Additionally, several of the mechanisms behind the program could be ineffective. This applies to the health as well as educational components of the program. Policymakers should revise how they can design these components more effectively and also reach those most in need. Moreover, participation in ChCC does not lead to the expected improvements in cognitive and non-cognitive child developments. This again could be related to the observed program inefficiencies.

In conclusion, the paper at hand shows that comprehensive, universal ECD programs can be an effective alternative to targeted programs. Still, it is crucial that policymakers implementing similar programs pay special attention to the inclusion of girls and the most vulnerable population. Targeted programs might be more effective and result in a higher MVPF. Future research should study the effects of ChCC on outcomes in late childhood and adulthood to assess long-term effects.

This paper is structured as follows. Section 2 describes the underlying early childhood development program Chile Grows with You. Section 3 gives an overview of the current state of literature relevant to the paper at hand. Section 4 describes the datasets at use. I then present the empirical methodology in section 5 and the results in section 6. Section 7 conducts several robustness checks. I then apply a heterogeneity analysis in section 8. Section 9 studies possible underlying mechanism, followed by a cost-benefit analysis in section 10. Section 11 concludes.

2 Program Description of Chile Crece Contigo

Chile Grows with You (ChCC – Chile Crece Contigo) is a comprehensive early childhood protection system which, alongside the social sub-programs Chile Cuida and Chile Seguridad y Oportunidad, is part of the overall social protection system of the Chilean government. The aim of the program is to accompany, protect and support all children and their families in an integrated manner. The program is defined as an integrated network, combining services of several public sector institutions.

It was introduced in 2007 with the goal of reducing the observed inequalities during the first years of a child's life in Chile. Early childhood development has been one of the priorities of Chilean politics since the 20th century, with child mortality decreasing from 370 per 1,000 births in 1900 to 7.6 per 1,000 births in 2006 (Villalobos 2011). In 2001, Chile introduced its Integrated Action Plan for early childhood and adolescence. The plan involved the creation of a public institution with the task of informing the presidency about the progress in the implementation of children's rights. The institution was established in 2003, at the same time as Chile Solidario[11].

In 2006, there were still some gaps in early childhood development.[12] This led to the founding of the National Advisory Council for the Reform of Policies for Children in 2006.

[11] Chile Solidario is the social protection system for the poor population in Chile. It offers several programs and services aimed at improving the living conditions of these people.

[12] The 2006 socioeconomic household survey (CASEN) showed that 21.9 % of children under the age of four lived in poverty, a higher share than in the overall population (13.7 %). Moreover, the National Survey on Life Quality and Health revealed some troubling results. The study found that 30 % of children below five years old did not meet internationally established development goals, and it revealed that significant developmental gaps existed between income quintiles with respect to child development. Another gap was observed in early education. Coverage of early education in general was low. Only 26.5 % of children between two and three years old attended kindergarten, while only 6 % of children under two attended pre-kindergarten. The gaps between income quintiles were marked, with four times more children from the top quintile attending early education facilities than children from the bottom quintile.

The mission of the council was to develop a social protection system for early childhood development, laying the foundation for Chile Crece Contigo, which President Bachelet announced in October 2006.

ChCC offers a variety of social services for children in their early life stages. The services offered through the program are adapted to the different needs that develop at each stage of life. It also addresses the needs of families, pregnant women, primary caregivers, and the family as a whole. The program is a universal program offered to all children and is part of the public health system (Asesorías para el Desarrollo 2012). Originally, children entered the program at their mother's first prenatal examination and left when they entered kindergarten or preschool. The program was expanded to include children from five to nine years of age in 2017.

The implementation approach of ChCC is an integrated one, recognizing that the municipality is the environment which forms and fosters the development of its children. The entry point and first contact point with the target population is the health sector, mainly through the Biopsychosocial Development Programme (PADB). The services offered through the program fall into three categories: An educational program for the Chilean citizenship and children's caregivers with the goal of raising awareness of the importance of early childhood development; services for children under the Biopsychosocial Development Programme PADB (Programa de Apoyo al Desarrollo Biopsicosocial), benefiting children from the womb to age four; special services for children belonging to the lowest 40th percentile in terms of income or non-income vulnerabilities.

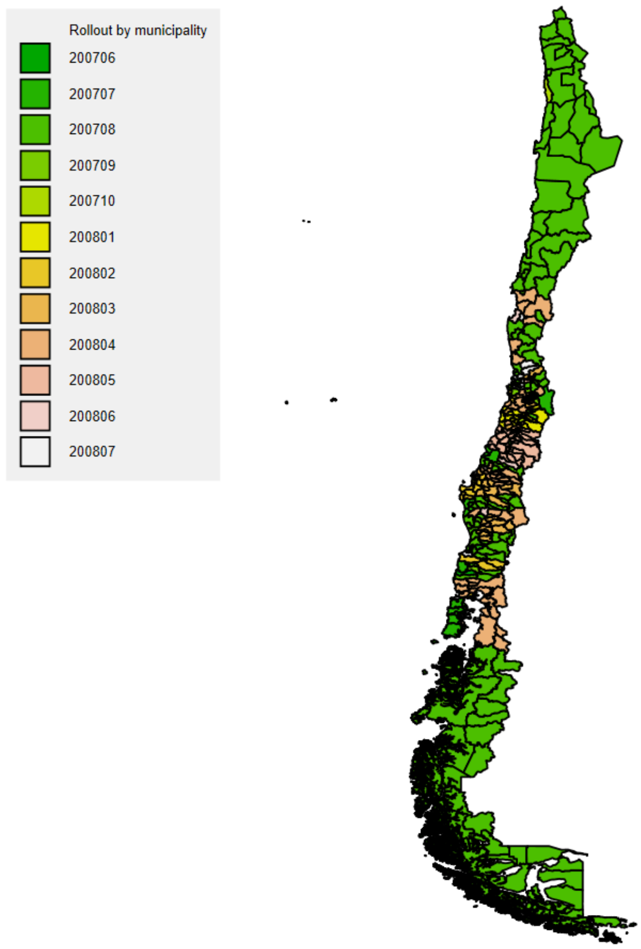
A detailed list of the services provided through ChCC can be found in Annex B. The focus program of ChCC is the PADB, through which all children enter the program.[13] The main changes that ChCC has implemented in early childhood services are the following: an increase in the time for the prenatal screening from 10-20 minutes to 40 minutes and the inclusion

[13]It is important to note that ChCC did not introduce all services listed in the Appendix, but enhanced them, developed them further, increased their scope and coverage, and improved their coordination and linkage with each other.

of psychosocial factors in risk assessment, additional to biomedical factors; a comprehensive home visit program for at-risk patients; educational workshops on pregnancy and parenting and the distribution of educational materials; a guarantee of personalized services during childbirth; the availability of local facilities to care for at-risk children or children with developmental delays; the development of a local network to address all children's needs (The World Bank 2018).

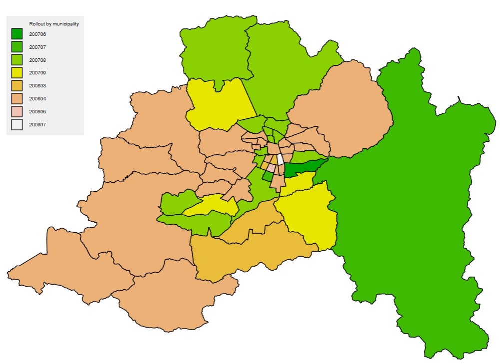
The roll-out of ChCC was gradual at the municipality level. The system was first implemented in the 159 municipalities which were best prepared for its implementation. The experience gained in the first round of roll-outs was then to be used for the implementation in the remaining municipalities in the second round. The inclusion of beneficiaries was also gradual. First, the first generation of women was included in the system. In the next year, the second generation of women and all newborns were included in the system, and so on. By 2011, the system included all pregnant women and children under four years of age. The system also introduced the different services gradually, to reflect the aging of beneficiaries. This was the case for ChCC's central program PADB as well as for its complementary instruments. In the first year, the program mainly provided services for pregnant women and newborns. Then, more activities were gradually introduced to supported children according to their age. The system immediately offered these services to the whole target population. Geographically, the roll-out of ChCC was dispersed as can be seen in Figures 1 and 2.

Figure 1: Geographic roll-out of ChC



Note: The figure plots the roll-out of ChCC over time by municipalities in Chile. ChCC was rolled out at the monthly basis at the municipality level. Source: Clarke, Méndez, and Sepúlveda (2020)

Figure 2: Geographic roll-out of ChCC (metropolitan area of Santiago de Chile)



Note: The figure plots the roll-out of ChCC over time by municipalities in the metropolitan area of Santiago de Chile. ChCC was rolled out at the monthly basis at the municipality level. Source: Clarke, Méndez, and Sepúlveda (2020)

3 The Economics of Early Childhood Development

This paper contributes to three different strands of the existing literature.

First of all, it empirically manifests the results predicted by theoretical models of early childhood development, such as those developed by Heckman (2006) and then later Almond, Currie, and Duque (2018). Heckman (2006) states that early investments strongly affect the productivity of later inputs and that they are dynamic complementarities [14] rather than perfect substitutes (Cunha and Heckman 2007). Therefore, investments in early childhood are especially important. The framework developed by Almond, Currie, and Duque (2018) confirms this. The authors highlight that a reallocation of resources from later to earlier in life creates pareto improvements. My results show that the investments made in early childhood translate into positive human capital outcomes in middle childhood. They therefore confirm the theory of dynamic complementarities.

My paper also contributes to the literature showing positive effects of access to social safety nets on infant health. Almond, Hoynes, and Schanzenbach (2011) show that participation in the Food Stamp Program three months prior to pregnancy leads to increased birth weight, with the largest gains at the lowest birth weights. Hoynes, Page, and Stevens (2011) show that the implementation of WIC results in an increase in average birth weight. Amarante et al. (2016) study the effects of transfers to poor pregnant women in Uruguay, that are part of the PANES program. They find that the incidence of low birth weights decreases by 19 to 20 %. A paper by Goodman-Bacon (2018) analyzes the effect of Medicaid on infant and child mortality. The paper shows that infant and child mortality decline due to the program. Clarke, Méndez, and Sepúlveda (2020) study the neonatal health component of Chile Crece Contigo and show that it has significant positive effects on birth weight and other early human capital outcomes. Ko, Howland, and Glied (2020) examine the Supplemental Security Income (SSI) program, which includes cash transfers for poor children with

[14] Dynamic complementarities refer to the fact that early inputs in human capital affect the productivity of later inputs, a phenomenon that Cunha and Heckman (2007) call self-productivity.

disabilities. They find positive effects on a variety of health outcomes for children in the first 8 years of life.

In this context, Milligan and Stabile (2011) study the effect of an increase in child benefits, that translates into higher family income. They find significant positive effects on test scores, maternal health, and mental health, among other measures, with significant differences by gender.[15] Similarly, Akee et al. (2018) evaluate the impact of quasi-experimental unconditional household income transfers on children's emotional and behavioral health and personality traits, as well as on parental relationships. They find large positive effects.

A related large stream of literature looks at the long-term impacts of access to social safety nets during early childhood. Chetty et al. (2011) investigate the effects of the project STAR during kindergarten on earnings and find positive effects. Hoynes, Schanzenbach, and Almond (2016a) show that participation in the food stamp program leads to a reduction in the incidence of metabolic syndrome and an increase in economic self-sufficiency. Deming (2009) follow up and find positive effects on adult human capital, adult economic self-sufficiency, the quality of adult neighborhoods and an increase in life-expectancy. Bailey, Timpe, and Sun (2020) study the long-term effects of the Head Start program[16]. The program leads to a large increase in adult human capital and economic self-sufficiency. Deming (2009) finds a positive effect of 0.23 standard deviations on a summary index of young adults' outcomes. Moreover, Akee, Jones, and Simeonova (2020) study how the EITC program affects the next generation. They find significant and mostly positive effects, varying by household type and gender.

Most of the interventions outlined in the literature above focus on the effect of income or in-kind transfers on children's short or long-term outcomes. Additionally, most of them are located in developed countries like the US or Canada. My paper contributes in that it goes beyond looking at the income component of child development by studying a comprehensive,

[15] While benefits have stronger effects on educational outcomes and physical health for boys, for mental health, they are larger for girls.

[16] Head Start is a nationwide preschool program for poor children in the US, established in 1965 as part of the federal government's "War on Poverty".

integrated early childhood intervention. It also examines whether the positive effects found in the literature to date also apply to developing countries, where human capital needs are greatest.[17]

My work aligns with several papers studying the effect of policy interventions for children. Two examples are the well-known Perry Highschool Project[18] (Heckman et al. (2010) or Heckman, Pinto, and Savelyev (2013)) and the ABC/CARE program (García, Heckman, and Ziff 2018). In the same way, Attanasio et al. (2020) study the impact of a targeted early childhood intervention in Colombia and find significant gains in cognitive and socio-emotional skills among disadvantaged children. Felfe and Lalive (2018) analyze the expansion of early child care in Germany, showing strong but diverging effects on children's motor and socio-emotional skills. Most of these interventions are targeted programs and target vulnerable children. My work contributes by asking whether universal programs can have similar effects and how they differ.

While there is a number of papers investigating the effects of universal childhood interventions, none of these interventions follows the comprehensive approach of ChCC. Moreover, most of these studies analyze ECD programs in developed countries. Baker, Gruber, and Milligan (2008) analyze the introduction of universal child care in Quebec. According to their study, the provision of universal child care leads to an increase in maternal labor supply, but leaves children worse off. On the contrary, Cascio (2017) finds that attending a state-funded universal preschool in the US leads to increased test scores, particularly for the poor. Similarly, in the case of Germany, universal child care has larger treatment effects for

[17] Chile joined the OECD in 2010, only four years after the introduction of ChCC, and it is still considered by many to be a developing country.

[18] The Perry Highschool Project is a pre-school intervention targeting socioeconomically disadvantaged children. The High/Scope Perry Preschool Project started in 1962 to analyze the influence of pre-school education on children's learning outcomes. The project was created when David Weikert noticed that poor children were doing much worse in school and formed a committee to address this. As part of the project, a randomly selected group of vulnerable, ultra-poor children ages three to four were given access to pre-school as well as a weekly 90-minute home visits by a social worker, while a second group of vulnerable, ultra-poor children with similar characteristics served as a control group. Twenty-four years later, researchers compared several socioeconomic outcomes of both groups, such as criminal activities, income, and educational outcomes.

disadvantaged children (Cornelissen et al. 2018). A universal ECD program in Norway is associated with long-term improvements in educational outcomes, as well as labor market outcomes (Havnes and Mogstad 2011). Similarly, Havnes and Mogstad (2015) show that the childcare expansion in Norway results in income gains during adulthood for children from the lower and middle parts of the income distribution, but income losses for those in the upper part.

Lastly, my paper connects to the literature which analyzes how to improve children's school performance. One example is the influential paper by Duflo (2001) that studies the influence of education supply on schooling outcomes. Black et al. (2014) ask how childcare subsidies impact student performance and several papers research the interaction between initial endowments and educational outcomes[19]. A stream within this literature analyzes the effect of income increases.[20] Similar to my contribution to the social safety net literature, my work expands this literature by looking beyond a pure income channel and analyzing the effects of a more comprehensive approach, bringing together several income and non-income channels.

Finally, Almond, Currie, and Duque (2018) single-out the necessity to further study the effect of the "missing middle" years, meaning trajectory effects of early childhood and middle childhood. They identify a lack of knowledge about how early childhood, middle childhood and adulthood interact. My paper contributes to this identified gap in the literature through connecting early and middle childhood.

4 Data

In this section, I document the data I use to analyze ChCC's program impact on child outcomes in middle childhood. I mainly rely on a variety of administrative datasets provided

[19]See, for example, the work by Bharadwaj, Løken, and Neilson (2013), Bharadwaj, Eberhard, and Neilson (2018) or Almond, Mazumder, and Van Ewijk (2015).

[20]For a good introduction to the topic, see studies by Dahl and Lochner (2012), Aizer et al. (2016), Muralidharan and Prakash (2017), Barrera-Osorio, Linden, and Saavedra (2019) or Millán et al. (2020).

by different entities of the government of Chile. I additionally use a rich survey on early childhood development as well as data on standardized test scores.

Standardized test scores. The first dataset used in this paper is from the national student achievement testing system (SIMCE). The data is provided by the National Agency of Educational Quality in Chile (Agencia de Calidad de la Educación 2021) and measures educational achievements along several dimensions, such as math or reading skills. The evaluation takes place every year and evaluates all second, fourth, sixth and eighth graders in elementary school, as well as the second and third graders in secondary school. I focus here on standardized test scores in reading and math of fourth graders tested between 2015 and 2018. To enter the schooling system in Chile a child must be at least six years old on March 31 of the respective school year (Ministerio de Educación 2021b). The treated children in 2007 would therefore enter primary education in 2013 at the earliest and be in fourth grade in 2016. The treated children in 2008 would be in fourth grade in 2017. Including the 2015 and 2018 evaluation years allows me to include children born one year before to one year after the introduction of ChCC. The 2015 data includes 243,987 students and the 2018 data includes 267,769 students.

Student register. The second dataset is provided by the Ministry of Education of Chile (Ministerio de Educación 2021a).It is the Student Register, containing information on the entire student body based on administrative school registry data. The data contains information about students' municipality of residence, date of birth, grade point average, school assistance rate, whether they passed the school year, the school and class they attend, and more. It also contains information on the socioeconomic status of students, divided into priority and preferential students. Priority students are those who belong to households with a socioeconomic background that make it more difficult for them to manage the educational process. These are students who belong to Chile Solidario, students who belong to the most vulnerable 30-percentile as defined by the Social Protection Scorecard (Ficha de Protección Social - FPS); students belonging to group A of FONASA who do not have FPS (families in poverty, and receiving a family subsidy); students whose household income is below the poverty line; students whose mothers have less than four years of education; and students living in rural or poor communities. Preferential students are students who belong to the 80-percentile of the population, as defined by the social characterization score (Instrumento de caracterización social vigente del Registro Social de Hogares). The key outcome variable of interest is school performance, that is, the grade point average achieved by a student in a respective year. I use the data on grade point averages from 2015-2018 and merge the data with SIMCE data based on the electronic student ID (MRUN).

Roll-out data. I merge this data with the monthly roll-out ChCC data at the municipality level provided by Clarke, Cortés Méndez, and Vergara Sepúlveda (2018). The main explanatory variable is an indicator equal to one if a student was born after the implementation of ChCC in her respective community of residence. Table 1 gives an overview of the underlying student population by treatment group.

Table 1: Summary statistics of 4th-graders (2015-2018)

	Control group		Treatment group	
VARIABLES	N	Mean	N	Mean
Standardized math score	565,928	261.6	269,114	261.3
Standardized reading score	565,928	267.2	269,114	271.7
Rural	565,928	0.0977	269,114	0.101
Age (Years)	565,907	9.501	269,093	9.107
Grade point averages	565,928	5.808	269,114	5.886
Assistance (%)	565,928	91.06	269,114	91.49
Female	565,928	0.490	269,114	0.514
Retention	565,928	0.0108	269,114	0.00660
Vulnerable student	565,928	0.824	269,114	0.737

The information above is based on SIMCE data and the national school register from 2015-2018. The treated children are all born after the implementation of ChCC in the respective municipality of residence. Grade point averages represent the grade point average achieved by the respective child at the end of the school year. The retention rate is based on a dummy variable that takes the value of one once a child has not successfully completed the school year. Vulnerability refers to socioeconomic vulnerability based on a variety of characteristics defined by the Ministry of Education in Chile.

Precontrols. I also include information on pre-ChCC municipal characteristics published in the SINIM database (Subsecretaría de Desarrollo Regional y Administrativo 2021). I include the following pre-treatment characteristics of the municipalities: the poverty rate at the municipality level, the number of families receiving subsidies, the available budget per municipality, the share of education spending by the Ministry of Education, the type of administrative cooperation in education, the student-teacher ratio, the presence of a primary health unit, the health transfer per capita from the Ministry of Health and the share of votes in the 2004 mayoral elections.

Administrative birth data. ChCC's flagship program is the PADB program which has a strong focus on the health sector. To analyze this channel, I consider administrative data on newborns provided by the Ministry of Health (Ministerio de Salud 2021). Since 1992, the ministry provides data of all newborns on weight and height at birth, type of medical care and place of birth, gestational week, and parents' age. It provides information on the municipality of residence as well as a child's birth date. Importantly, the data for 2007 is missing. This could seriously influence the stability of my results.

Early childhood education. In addition to register data on grade point averages, the Ministry of Education also publishes information on the enrollment of children in early childhood education. The first available data is from 2011. I aggregate this data at the municipality-birth-date level and divide it by the number of children born in each cell. From this, I calculate the effect of ChCC on enrollment rates in early childhood education.

Survey data. To analyze potential channels through which ChCC affects school outcomes, I look at intermediate factors that could impact a child's performance in school. More specifically, I investigate the influence of ChCC on parental attitudes towards child-care, as well as developmental indicators such as psychomotor development, executive functioning, socio-emotional development, and anthropometric measures. To this end, I use data from the Longitudinal Survey of Early Childhood (ELPI) published by the Ministry of Social Development (Ministerio de Desarrollo Social y Familia 2021). The ELPI Survey consists of several questionnaires, addressed both to the children themselves and to their families. It also includes the use of child evaluation instruments that measure child development, as well as caretaker development and the interaction between the two. The survey questions and evaluation tools differ by age group (UNICEF 2018). Consequently, the sample size varies by variable. The survey consists of three waves from 2010, 2012, and 2017. I draw a sample of children who were observed repeatedly in the 2010, 2012, and 2017 waves. I have to exclude children who were only reported in 2010 or 2012 because no information is available for them on their place or date of birth. I weight the observations using the sample weights provided by the ELPI evaluation team. Table 2 shows some basic characteristics of the underlying sample by treatment group.

The package of evaluation instruments for children consists of a set of tests measuring the following areas of child development: psychomotor development, executive functioning, socio-emotional functioning, as well as anthropometric measures. For the purpose of my analysis, I focus on instruments that were used with children who were part of the treatment as well as control group.[21]

To measure children's general and cognitive development I consider three different measures: TEPSI (the Psychomotor Development Test), TVIP (the Peabody Picture Vocabulary Test) and TADI (test of general infant learning). I also analyze outcomes from the Batería III Woodcock-Muñoz. The TEPSI measures the psychomotor development of children and was part of the survey in 2010. The TVIP Score consists of 145 questions, and the ELPI gives an overall score generated from these questions. It is a norm-referenced measure of Spanish hearing vocabulary analyzing verbal reasoning, as well as language skills. A score below 70 is considered extremely low, and a score of more than 145 is considered extremely high. Instructors used this instrument with children in all three survey waves. The TADI score evaluates children ages three months to six years and measures four dimensions of child development: cognition, motor skills, language and socio-emotional development. This

[21]For an overview of all instruments see the ELPI User Manual (UNICEF 2018). For a detailed explanation and description of all instruments see a report published by Universidad de Chile (2015).

Table 2: Summary statistic of ELPI Sample (2010-2017)

	Control group	Treatment group
Age in months	92.77	59.43
	(42.01)	(33.17)
Male	0.489	0.488
	(0.500)	(0.500)
Vulnerable	0.425	0.398
	(0.494)	(0.489)
Indigenous	0.111	0.126
	(0.314)	(0.331)
Household members	3.621	3.942
	(2.085)	(2.014)
Share of adults with low education	0.121	0.0998
	(0.173)	(0.158)
No. of employed household members	1.658	1.696
	(0.930)	(0.951)
First survey-round	0.247	0.0742
	(0.431)	(0.262)
Second survey-round	0.278	0.185
	(0.448)	(0.389)
Third survey-round	0.475	0.740
	(0.499)	(0.438)
Observations	12404	19291

Source: ELPI 2010, 2012 and 2017. Treated children are children born after the implementation of ChCC.

evaluation instrument was part of the 2012 survey. It consists of a task given to the child, a set of questions for the primary caregiver and a professional observation of the child. The TADI score is standardized for the Chilean population. The Batería III Woodcock-Muñoz measures both the cognitive development as well as achievement of children. The 2017 survey includes this instrument and assesses three sub-categories: applied problem-solving, mathematical literacy as well as calculus.

I measure the effect of ChCC on children's executive functioning using the BDST (Backward Digit Span Task) as well as the HTKS (Head Nose Tees Shoulder Task). The BDST consists of 16 questions and measures the working memory. The ELPI reports an overall score based on these questions. The HTKS is a game for children, in which they are asked to do the opposite of what an instructor says.

I then analyze ChCC's impact on children's socio-emotional development via the CBCL1 (Child Behavior Checklist 1). The CBCL1 is a caregiver report identifying behavioral problems in children, based on the following symptoms: aggressive behavior, anxiety, attention problems, rule-breaking behavior, somatic complaints, social problems, thinking problems, and depression. The CBCL1 consists of 99 questions. The ELPI, in turn, generates an overall test score from these questions. A percentile score of less than 93 is considered normal, and a score greater than 98 is considered clinical range. A total scale score of less than 60 is considered normal, while a total scale score of greater than 83 is considered clinical range. This evaluation instrument was part of all three rounds in the ELPI survey.

For the anthropometric measures, I create a dummy variable that equals one if the interviewer observes some kind of abnormality in a child's weight, height or head circumference.

To measure the impact of ChCC on a children's immediate environment and on caregiver parenting, I use the PSI (Parental Stress Index), PSCS (perceived self-confidenc scale), CESD-10 (Center for Epidemiologic Studies Depression Scale 10) and HOME Index (Home Observation Measurement of the Environment Index). The PSI consists of 36 questions answered directly by the principal caregiver. Each question relates to a subdomain of parental stress and is scored on a five-point scale. A score of less than 80 is considered normal, while a score greater than 90 is within the clinical range. The PSCS consists of 17 items measuring the self-assessment of parenting skills. Higher scores represent greater parent confidence in their parenting skills. The CESD-10 is based on 10 items. People with higher scores are more prone to depression. I also use the HOME (Home Observation for the Measurement of the Environment) to measure household quality. The HOME Index consists of 13-43 questions. It measures several dimensions of household quality, such as the emotional interaction between the principal caregiver and the respective child, the presence of learning material, as well as maternal commitment. The interviewer assigns points for each dimension, with eight points being the maximum score. I also retrieve information on parenting practices (such as inadequate dental care) from the survey.

Table D21 in the Annex gives an overview of the evaluation instruments under consideration.

5 Identification Strategy

In this section, I describe the identification strategy I use to empirically investigate the effect of participation in ChCC on human capital accumulation in middle childhood. Simply regressing an indicator variable, which is equal to one if a child is part of ChCC, and zero otherwise, on child outcomes in middle childhood might lead to biased estimators. Children from earlier birth-cohorts might significantly differ from children in later birth-cohorts. This is problematic especially if they differ on unobservable dimensions, which also affect the outcome variables of interest. To give an example, children of the pre-treatment group might be subject to different education policies than children of the treatment group. These policies might significantly affect schooling outcomes, but are unobservable in the data at hand. Therefore, a simple ordinary least square regression might mistake the effects generated from changes in education policies for changes generated through the implementation of

ChCC.

To address these endogeneity concerns, I exploit the fact that there is a random cutoff for the participation in ChCC, that is, the date of birth of a child, and apply a regression discontinuity design (RDD). The intuition behind RDDs is that students are very similar around the cutoff. Therefore, the potential existence of unobservable confounding factors is less likely. Comparing outcomes of students located closely below the cutoff to outcomes of students located closely above the cutoff delivers the local treatment effect of ChCC. Hence, I estimate the following equation:

$$Y_i = \alpha + \beta ChCC_i + \gamma_1(X_i - c) + ChCC_i\gamma_2(X_i - c) + \varepsilon_i \tag{1}$$

, where:

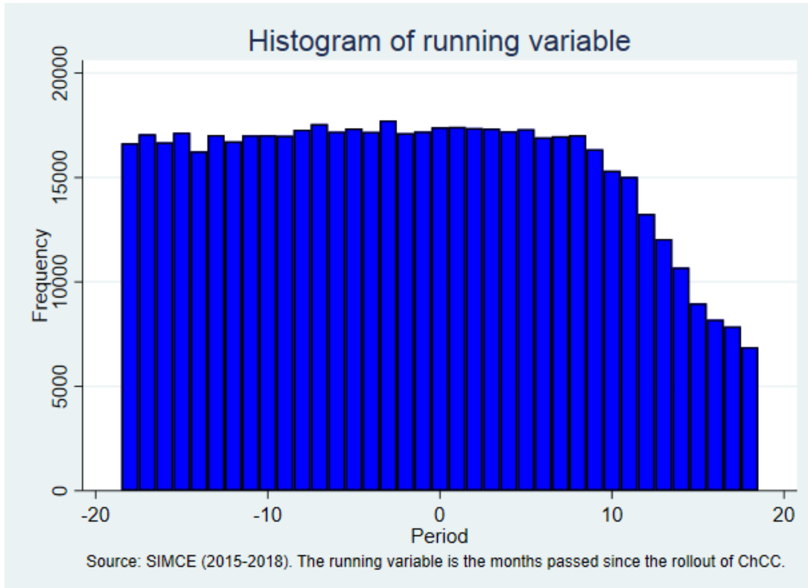
$$ChCC_i = \begin{cases} 1, & X_i \geq c \\ 0, & X_i < c \end{cases} . \tag{2}$$

The receipt of the ChCC is determined by the threshold c (being born after the implementation of ChCC) of the discrete variable X_i, the date of birth of the respective child. As the roll-out of ChCC was staggered, I first normalize the threshold. I do so through setting the roll-out date to zero and then calculating the difference between each child's date of birth and the roll-out date. The running variable is then equal to the number of months ChCC was in place in a respective municipality. X_i depends on the bandwidth b of data used. The bandwidth is equal to the number of periods under consideration before and after ChCC's implementation took place. X_i is therefore as follows:

$$c - b \leq X_i \leq c + b \tag{3}$$

The RDD was originally designed for settings with continuous running variables. Importantly, it is only possible to apply the RDD to settings with discrete running variables when

Figure 3: Histogram of the Running Variable (months since roll-out)



the number of mass points is large (Cattaneo, Idrobo, and Titiunik 2019). If the number of mass points is small, a local randomization approach might be more appropriate than a RDD. Figure 3 shows a histogram of the running variable. I restrict the sample to all children born 18 months before to 18 months after ChCC's roll-out, a setup under which there are sufficient observations in each of the cells of the underlying dataset. This results in 37 mass points, a relatively small number.

Consequently, I decide to implement a local randomization approach instead of a continuity-based method. Differently from settings with continuous running variables, I do not have to choose a window around the treatment cutoff, as it is easy to identify the minimum window in settings with discrete variables. In my case, the minimum window consists of all children born exactly one month after to one month before the roll-out of ChCC in their respective municipality.

The underlying assumption of the local randomization approach is that the assignment of each child to the treatment was random and that there was no manipulation into treatment. To test this assumption I conduct a falsification test. I find that there are 17,203 observations in the month before ChCC's roll-out, and 17,410 observations during the roll-out. In the month after ChCC's roll-out there are 17,426 observations. This suggests that there is no manipulation or non-random selection into the treatment. The ratio of observations close to the cutoff is nearly 1. This is consistent with the assumption that treatment assignment is random and close to a probability of 0.5. Additionally, in this specific setup manipulation might be of low concern, as it might be difficult to time child birth to the monthly level. A pregnancy takes 10 months and it is unlikely that the roll-out date of ChCC played a role in the monthly timing of pregnancies. I conduct a binomial test to confirm this empirically, and the p-value of a binomial test is close to 1. This confirms that treatment manipulation is of low concern in this setup.

A potential threat to my identification strategy is the non-randomness of the ChCC roll-out. If the timing of the ChCC implementation is correlated with underlying factors which also affect the outcome variables under consideration, the observed effect could be biased. The same problem arises from the simultaneous roll-out of potential alternative government programs, which also impact school achievement. The roll-out of ChCC has to be random, and no other policies that affect my outcome of interest should be implemented at the same time as ChCC. To investigate if the timing of the ChCC implementation is driven by pre-treatment characteristics, I conduct a logistic regression at the municipality level. The dependent variable is a dummy variable set to one for municipalities who were part of the early roll-out group and the explanatory variables are a number of pre-treatment municipality characteristics.

Table 3 shows that including relevant fixed effects and clustering standard errors decreases the number of variables which significantly influence the probability to form part of the early roll-out group. In Column 2 only the average age of students in the sample is a significant predictor of membership in the early adoption group. This is not surprising as the date of birth determines program participation. In Column 1, I do not control for survey-year and regional fixed effects and do not cluster standard errors. Under this model specification, several pre-treatment characteristics influence the probability to form part of the early roll-out group: the municipal poverty rate, the number of family subsidies paid in the municipality, having an administrative cooperation, the student-teacher ratio, the health transfer per capita, as well as the votes for the communist party or the independent party. The marginal effect in Column 1 is largest for the votes received by the communist party. As Lee and Lemieux (2010) point out correctly, it is not necessary to include fixed effects for identification in RDDs. Therefore, the results in Column 2 validate the implementation of a local randomization approach in this setting.

Additionally, the local randomization approach relies on the assumption that individuals close to the cutoff are similar on observable and unobservable characteristics. While I cannot analyze the similarity of unobservable student characteristics around the cutoff, this is possible for observable covariates. I employ finite-sample methods to determine the cutoff window under which the assumption of randomized treatment assignment is most plausible. I follow Cattaneo, Titiunik, and Vazquez-Bare (2016) and implement a window-selection procedure based on balance tests. I find that the optimal window is equal to four periods around the cutoff. This means that the optimal cutoff window consists of the two birth cohorts previous to ChCC's implementation and the first three birth cohorts participating in ChCC. For the underlying figures and details behind the optimal window length selection see the Annex.

I test if treated and control groups at the cutoff are on average similar in terms of observable characteristics. I can observe three covariates in the data at use, namely students' gender, socioeconomic vulnerability and the degree of urbanization of the school they attend.

Table 4 shows the mean values of the three observable student characteristics in the minimum window around the cutoff. It also shows the resulting p-value of a t-test, which investigates the equality of means in the minimum cutoff window in Column 3. I cannot reject the null hypothesis of no significant difference in the means in the minimum cutoff window. This applies to all of the three observable covariates.

The evidence presented speaks for the identification assumptions of the local randomization approach to be likely fulfilled in this setting. Most importantly, there seems to be

Table 3: Determinants of early roll-out

	Robust	Clustered
Female share	-0.0351	-0.0923
	(0.0684)	(0.120)
Rural share	-0.0500	0.393
	(0.0644)	(0.384)
Age in years	0.335***	0.983***
	(0.0277)	(0.0568)
Vulnerable student (share)	0.175*	0.259
	(0.0876)	(0.252)
Municipal poverty rate (2006)	-0.00976***	0.0407
	(0.00255)	(0.0346)
No. of family subsidies paid in the mun. (2006)	-0.0000324***	-0.0000549
	(0.00000829)	(0.0000461)
Available budget per municipality (2006)	0.00229***	-0.000891
	(0.000215)	(0.00198)
Share of educational spending coming from MINEDUC (2006)	0.0156***	-0.0196
	(0.00181)	(0.0162)
Administrative cooperation	0.702***	0.508
	(0.0543)	(0.491)
Teacher student ratio - municipality (2006)	-0.0568***	-0.0204
	(0.00594)	(0.0460)
Without health service	0.985***	0.0630
	(0.0744)	(0.782)
Health transfer per capita - MINSAL (2006, in Mio. Pesos)	0.000253***	0.000186
	(0.0000245)	(0.000171)
Communist Party	1.247***	0.756
	(0.220)	(1.678)
Cristian Democratic Party	-0.0347	0.0704
	(0.0472)	(0.288)
Independent	-0.134**	-0.319
	(0.0502)	(0.460)
For Democracy Party	0.0576	0.0149
	(0.0684)	(0.616)
Radical Sociodemocratic Party	0.167	-0.130
	(0.115)	(0.967)
National Renovation Party	-0.0883	0.177
	(0.0654)	(0.517)
Constant	-3.714***	-4.654**
	(0.338)	(1.653)
Survey-year fixed effect	No	Yes
Regional fixed effect	No	Yes
Clustered standard errors	No	Yes

Marginal effects; Standard errors in parentheses

The outcome variable is an indicator equal to one for all municipalities in the early rollout group.

Source: SINIM, SERVEL and Clarke et al..

(d) for discrete change of dummy variable from 0 to 1

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Baseline municipality characteristics (2 periods around cutoff)

	Control mean	Treatment mean	T-test p
Female	0.50	0.50	0.50
Vulnerable student	0.74	0.74	0.20
Rural	0.10	0.10	0.37
Standardized math score	262.36	263.24	0.36
Standardized reading score	270.35	272.41	0.00
Grade point averages	5.86	5.89	0.05
Observations	34324	52206	86530

Source: SIMCE (2015-2018), MINEDUC (2015-2018).

no manipulation into treatment around the cutoff. Moreover, randomness in the program's roll-out is plausible. Lastly, there is no discontinuity of observable student characteristics around the threshold.

6 Impacts on Schooling Outcomes

After establishing the plausibility of the underlying identification assumptions, I analyze the local randomization discontinuity (RD) effect of participation in ChCC on the main outcome variables of interest, namely the standardized math and reading score as well as grade point averages. If the program successfully increases the accumulation of human capital in middle childhood, I would expect to see positive and significant effects of program participation on schooling outcomes.

Before analyzing the program's impact on schooling outcomes, I further investigate the role of potential confounding factors around the cutoff. To do this, I estimate the local RD effect on the three observable student characteristics. The last three columns in Table 5 confirm that there is indeed no discontinuity in any of the observable covariates in the optimal cutoff window. The resulting p-values from a local RD estimation on the three observable covariates in the optimal cutoff window are larger than the most commonly used significance levels. This shows that the assumption of similarity between observed covariates is plausible in this cutoff window.

Table 5 implies that participation in ChCC leads to improved schooling outcomes in the optimal window length. Column 2 shows that the program increases standardized math scores by 0.883 points, standardized reading scores by 2.059 points and grade point averages by 0.03 points. The p-values in Column 3 are zero or very close to zero. Consequently, the reported point estimates are significant at the 1% significance level. Compared to the mean values in the optimal window, this corresponds to an increase of 0.337 % in standardized math scores, 0.762 % in standardized reading scores, and 0.512 % in grade point averages. Figure 4 to 6 show the related local randomization design plots for schooling outcomes in the optimal window.

My results illustrate that the program successfully improves schooling outcomes in middle childhood. The results are positive and significant across the three educational variables investigated in this paper. This shows that universal, comprehensive ECD programs like ChCC can indeed successfully foster a country's human capital accumulation. Still, it is important to assess if these improvements outnumber the costs. To evaluate the cost-efficiency of the program, I conduct a cost-benefit analysis later in this paper.

My findings could be driven by the chosen window length or my assumptions about the underlying functional form. Hence, in the following, I conduct several robustness checks to validate my findings.

Table 5: Local RD effect of ChCC on schooling outcomes in the optimal window around cutoff

0	Variable	RD Estimate	P-Value	N (left)	N (right)
1	Standardized math score	0.882594	0.008	34324	52206
2	Standardized reading score	2.058952	0	34324	52206
3	Grade point averages	0.029693	0	34324	52206
4	Gender	0.000075	0.983	34324	52206
5	Vulnerability	-0.000302	0.921	34324	52206-
6	Rural	-0.002747	0.185	34324	52206

Note: The table shows the local RD effect of ChCC in the four months around the cutoff. This means that the estimation considers all students born two months before and after the roll-out of ChCC as well as those born during its roll-out. The first column shows the point estimates of participating in ChCC on the three schooling outcomes and observable covariates. Column 2 presents the related p-values. Column 3 and 4 show the number of observations on each side of the cutoff. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Figure 4: RD plot (standardized math scores)

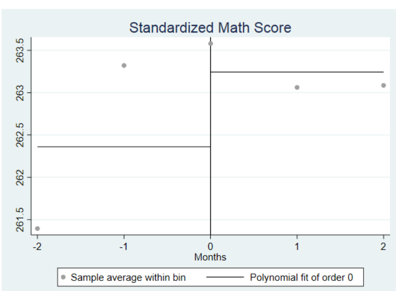


Figure 5: RD plot (standardized reading scores)

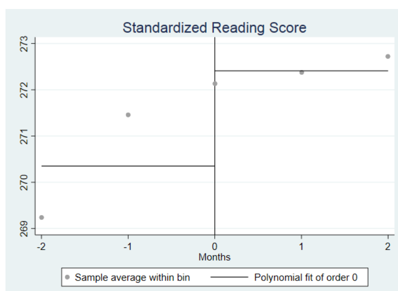
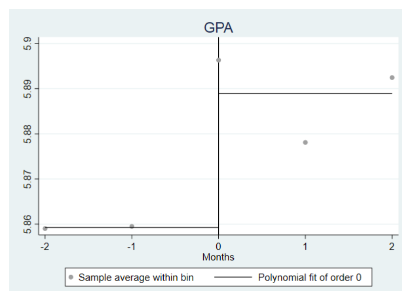


Figure 6: RD plot (grade point averages)



Note: The figures above show the local randomization design plots for schooling outcomes. The left panel shows the plot for standardized math scores, the middle panel the one for standardized reading scores, and the right panel the one for grade point averages. I restrict the periods shown to the optimal window length, namely four periods. This means that the figures show the average values of schooling outcomes for all children born two months previous to the roll-out of ChCC to two months after its roll-out. The black horizontal line features the threshold of the local RD approach, namely zero. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

7 Robustness Checks

In the following, I conduct several robustness checks to validate my findings. Firstly, I analyze the local RD effect in alternative window lengths. I then employ a parametric estimation of the local RD approach. Next, I conduct a number of falsification tests to show that the window length does not drive my results. I also look at a placebo cutoff.

In addition, I analyze the implications when allowing for non-compliance in the program take-up. Afterwards, I employ two alternative estimation strategies, namely a staggered difference-in-difference approach and an event study. Lastly, I show that my results are not confounded by migration patterns nor by the financial crisis and copper prices.

7.1 Alternative Cutoff Windows

To analyze if my results are driven by the number of windows around the cutoff, I repeat my analysis using two alternative cutoff windows. The local randomization approach relies on the assumption that individuals close to the cutoff are similar on observable and unobservable characteristics. While I can analyze the similarity of observable student characteristics around the cutoff, this is not possible for unobservable covariates. Similarity between unobserved characteristics is most plausible very close to the cutoff point. For this reason, I start by only considering the minimum window around the cutoff, namely all children born one month before until one month after the program's implementation.

Table 6 shows the mean values of the three observable student characteristics in the minimum window around the cutoff. It also shows the resulting p-value of a t-test, which investigates the equality of means in the minimum cutoff window in Column 3. I cannot reject the null hypothesis of equality in the means in the minimum cutoff window. This applies to all of the three observable covariates. Consequently, systematic differences in observable covariates close to the threshold are unlikely.

Table 6: Baseline municipality characteristics (1 period around cutoff)

	Control mean	Treatment mean	T-test p
Female	0.50	0.51	0.90
Vulnerable student	0.74	0.74	0.10
Rural	0.10	0.10	0.61
Observations	17203	34836	52039

Source: SIMCE (2015-2018), MINEDUC (2015-2018).

To further validate the similarity of covariates close to the cutoff, I estimate the local RD effect of ChCC on the predetermined covariates. The last three columns of Table 7 show the resulting coefficients in Column 1 and p-values in Column 2. I cannot reject the null hypothesis that the treatment has no effect on the observed control variables in the minimum cutoff window.

After establishing the plausibility of the underlying identification assumptions, I employ my empirical strategy to the three main outcome variables of interest. I find no significant effects in the case of standardized math scores, but in the case of standardized reading scores and grade point averages in the minimum cutoff window (see Table 7). The point estimate on reading scores is only significant at the 10% significance level. The difference-in-means between the control and treatment group in the smallest window around the cutoff is 0.797 on reading scores, and 0.028 on grade point averages. When compared to the mean value of these two outcome variables in the smallest possible window around the cutoff, the program's average impact on reading scores and grade point averages is less than 1 %. It increases grade point averages by 0.476 %, on average, and reading scores by 0.293 %.

Table 7: Local RD effect of ChCC on schooling outcomes in the minimum cutoff window

		RD Estimate	P-Value	N (left)	N (right)
1	Standardized math score	-0.000117	1	17203	34836
2	Standardized reading score	0.796722	0.107	17203	34836
3	Grade point averages	0.027713	0.003	17203	34836
4	Gender	0.002774	0.552	17203	34836
5	Vulnerability	-0.003582	0.379	17203	34836
6	Rural	-0.002901	0.298	17203	34836

Note: The table shows the local RD effect of ChCC in the minimum cutoff window. This means that the estimation considers all students born one month before and after the roll-out of ChCC as well as those born during the roll-out. The first column shows the point estimates of participating in ChCC on the three schooling outcomes and observable covariates. Column 2 presents the related p-values. Column 3 and 4 show the number of observations on each side of the cutoff. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

While the assumption on a similarity of unobserved characteristics is most plausible in smaller cutoff windows, there are downsides to restricting the sample to few windows. I might loose important information on the variation or trends in the data when relying on a window length of less than five. For this reason, I validate my findings considering a larger cutoff window. I randomly choose a cutoff window of twenty months for my robustness check. This means that I consider all students born ten months before and after the rollout of ChCC.

Table 9 shows that students systematically differ from each other on observable characteristics when considering the larger cutoff window. The p-values are zero in the case of gender and socioeconomic vulnerability. Consequently, it is not possible to reject the null hypotheses of treatment effects on observable covariates in the larger cutoff window. This is confirmed by a t-test on baseline characteristics. Table 8 shows that the related p-values on gender and socioeconomic vulnerability are zero. The systematic differences in individual controls is an important caveat and might confound my results in the larger cutoff window. The possibility of significant unobservable confounding factors might be more plausible under this model specification.

Turning attention to results reported on the three schooling outcomes, the local RD approach in the larger window confirms my findings from the optimal cutoff window. Table 8 provides evidence that the participation in ChCC significantly improves schooling outcomes. Column 1 shows that the program leads to increases in standardized math scores of 0.347 points, in standardized reading scores of 2.987 points, and in grade point averages of 0.03 points. When compared to the local RD estimators in the optimal window length, the point estimates are similar in terms of magnitude in the case of grade point averages, but smaller in the case of standardized test scores. Especially the coefficient on standardized math scores more than halves when compared to the baseline estimator. Furthermore, Column 2 shows that the p-value on standardized math scores increases in the larger window. The point estimate associated with standardized math scores is only significant at the 2.5% significance level. In contrast, the p-value on standardized reading scores and grade point averages remain at zero and are therefore highly significant.

In summary, the majority of my findings hold when choosing alternative window lengths.

The program seems to successfully improve schooling outcomes in middle childhood. While the local RD effect on standardized math scores is insignificant in the minimum window around the threshold, the coefficient is significant and positive in the twenty periods around the cutoff. This means that the point estimate on mathematical schooling outcomes is more sensitive to empirical specifications. In the case of standardized reading scores and grade point averages, my results are stable in the three window lengths investigated. Figure 4 to 6 show the related local randomization design plots for schooling outcomes in a window length of twenty periods.

Table 8: Baseline municipality characteristics (20 periods around cutoff)

	Control mean	Treatment mean	T-test p
Female	0.50	0.51	0.00
Vulnerable student	0.73	0.74	0.00
Rural	0.10	0.10	0.60
Observations	172695	186707	359402

Source: SIMCE (2015-2018), MINEDUC (2015-2018).

Table 9: Local RD effect of ChCC on schooling outcomes in the 20 periods around the cutoff

		RD Estimate	P-Value	N (left)	N (right)
1	Standardized math score	0.356881	0.025	172695	186707
2	Standardized reading score	2.986731	0	172695	186707
3	Grade point averages	0.030194	0	172695	186707
4	Gender	0.008352	0	172695	186707
5	Vulnerability	0.009613	0	172695	186707
6	Rural	0.000215	0.829	172695	186707

Note: The table shows the local RD effect of ChCC in the twenty months around the threshold. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. The first column shows the point estimates of participating in ChCC on the three schooling outcomes and observable covariates. Column 2 presents the related p-values. Column 3 and 4 show the number of observations on each side of the cutoff. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Figure 7: RD plot (standardized math scores)

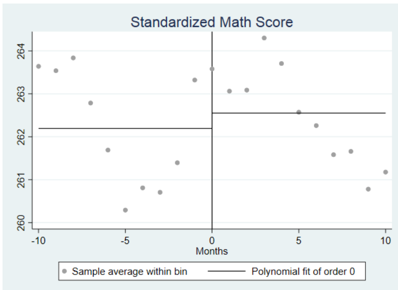


Figure 8: RD plot (standardized reading scores)

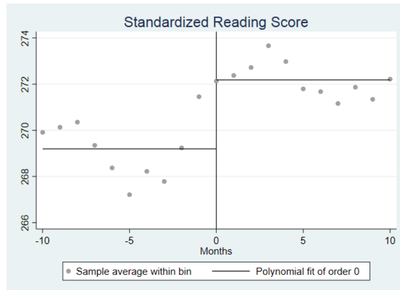
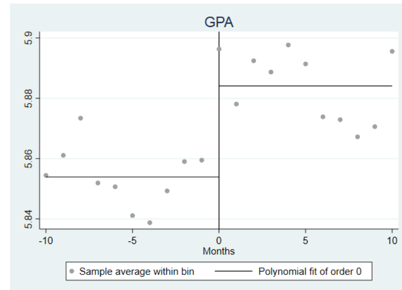


Figure 9: RD plot (grade point averages)



Note: The figures above show the local randomization design plots for schooling outcomes. The left panel shows the plot for standardized math scores, the middle panel the one for standardized reading scores, and the right panel the one for grade point averages. I restrict the periods shown to a window length of twenty. This means that the figures show the average values of schooling outcomes for all children born ten months previous to the roll-out of ChCC to ten months after its roll-out. The black horizontal line features the threshold of the local RD approach, namely zero. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

7.2 Parametric Estimation

My estimation strategy relies on the assumption that the relationship between participation in ChCC and schooling outcomes in middle childhood has a regression slope equal to zero. But if the true relationship is linear or even non-linear, the local randomization approach could mistake linear and non-linear relationships for discontinuities. To account for this caveat, I employ a parametric regression specification of the local randomization approach. In detail, I estimate the following regression:

$$Y_i = \alpha + \beta ChCC_i + \gamma_1 c_i + \gamma_2 c_i^2 + \varepsilon_i \tag{4}$$

, where:

$$ChCC_i = \begin{cases} 1, & c_i \geq c \\ 0, & c_i < c \end{cases} . \tag{5}$$

I consider the randomly chosen number of cutoff windows, namely ten treatment and ten control periods, and include the three observable student characteristics.[22] I start by

[22]The optimal window length is the minimum window in case of the polynomial estimation of order one,

estimating a polynomial fit of order 1. Table 10 presents the results. Based on the p-values in Column 2, all three coefficients are significant and positive at the 1% significance level. When comparing the point estimates in Column 1 to the point estimates from the baseline specification, the local RD estimator is larger. This could mean that the non-parametric estimation underestimates the true impact of the program. The other way around, it could also indicate that accounting for a polynomial order of one overestimates the effect of participating in ChCC.

Table 10: Local RD effect of ChCC on schooling outcomes in the 20 periods around the cutoff with a polynomial fit of order 1

		RD Estimate	P-Value	N (left)	N (right)
1	Standardized math score	3.157387	0	172695	186707
2	Standardized reading score	3.886528	0	172695	186707
3	Grade point averages	0.042569	0	172695	186707

Note: The table shows the local RD effect of ChCC in the twenty months around the cutoff window. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. The results are based on a parametric regression specification of the local randomization approach of degree one. Column 1 reports the point estimates, Column 2 the p-values, and Column 3 and 4 the number of observations on each side of the threshold. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

I next estimate a quadratic version of the local randomization approach. Table 11 shows that accounting for a polynomial order of degree two increases the p-values. The p-values reported in Column 2 are larger than the ones from the baseline specification, or the ones from the linear polynomial estimation. Especially the coefficient on standardized math scores loses significance. Column 2 shows that the point estimate on mathematical schooling outcomes is insignificant at the 10% significance level. This confirms previous findings which indicated that the results on standardized math scores are more sensitive to the underlying empirical assumptions. In the case of standardized reading scores, the program's impact is only significant at the 10% significance level. By contrast, the point estimate on grade point

and four periods in case of the polynomial estimation of order two. It might be difficult to estimate the true underlying slope of the functional relationship from less than five data points. I therefore opt for the larger window for the parametric estimation.

averages remains highly significant. The p-value reported in Column 2 is close to zero.

While the magnitude of the coefficient on grade point averages is similar to the baseline specification, the coefficient on standardized reading scores falls by almost a half. This stands in contrast to findings from the polynomial estimation of order one. It is important to emphasize that the magnitude of the program's impact on standardized test scores varies with the empirical specifications investigated. The true magnitude might lie somewhere in the middle of the different point estimates reported in this paper.

Figure 10 and 11 plot the discontinuity for standardized test scores and Figure 12 the one for grade point averages. From the figures one can conclude that the quadratic specification might be the most appropriate approximation for the true underlying functional form of the relationship between participation in ChCC and schooling outcomes. Still, as previously shown, the similarity of observable covariates does not hold in the larger cutoff window. Therefore, the results should be taken with caution, as they might be confounded by unobservable covariates.

Table 11: Local RD effect of ChCC on schooling outcomes in the 20 periods around the cutoff with a polynomial fit of order 2

		RD Estimate	P-Value	N (left)	N (right)
1	Standardized math score	0.343557	0.499	172695	186707
2	Standardized reading score	0.94229	0.096	172695	186707
3	Grade point averages	0.031662	0.003	172695	186707

Note: The table shows the local RD effect of ChCC in the twenty months around the cutoff window. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. The results are based on a parametric regression specification of the local randomization approach of degree two. Column 1 reports the point estimates, Column 2 the p-values, and Column 3 and 4 the number of observations on each side of the threshold. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Overall, the results from the parametric estimation confirm the main findings from the baseline local randomization approach. Participation in ChCC leads to significant improvements in schooling outcomes in middle childhood. Still, the exact magnitude of the program's impact on standardized test scores is sensitive to the parametric assumptions behind the em-

Figure 10: RD plot (stand. math scores - p=2)

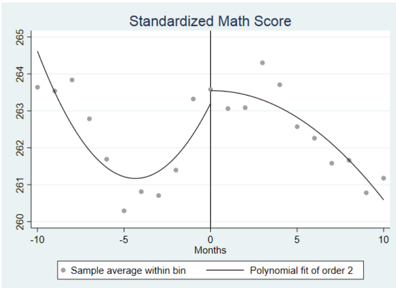


Figure 11: RD plot (stand. reading scores - p=2)

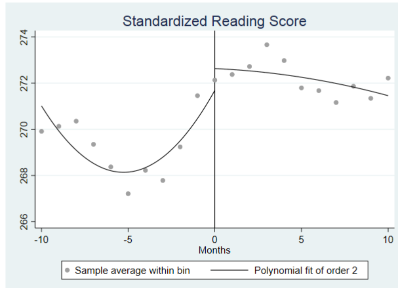
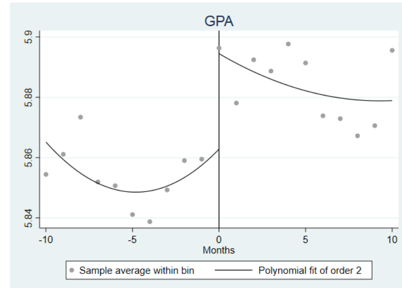


Figure 12: RD plot (grade point averages - p=2)



Note: The figures above show the local randomization design plots for schooling outcomes assuming a polynomial fit of order two. I consider a cutoff window of ten periods before and ten periods after the actual cutoff. The cutoff is equal to zero and represented by the black horizontal line. The left panel shows the plot for standardized math scores, the middle panel the one for standardized reading scores, and the right panel the one for grade point averages. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

pirical estimation. Additionally, the findings from the parametric estimation confirm that the coefficient on standardized math scores is less stable across model specifications. This could mean that the program's impact on this specific schooling outcome is less straightforward.

7.3 Falsification Tests

I conduct several falsification tests in order to verify the robustness of my findings. I start by analyzing whether the chosen window around the cutoff drives the empirical results. When considering different nested windows, namely up to twenty months around the cutoff, the ratio of observations around the cutoff remains balanced (see Figure 3). Consequently, the probability of treatment assignment remains around 0.5 and it is unlikely that the window size drives my results.

To further validate my findings, I choose a placebo cutoff and consecutively conduct a randomization-based analysis using the randomly chosen number twenty as the window length. If my results are robust, the impact of ChCC should be insignificant around the placebo cutoff. And, in fact, when randomly choosing the date six months prior to the actual roll-out of ChCC as the artificial cutoff, the effect of participation in ChCC is insignificant for all three outcome variables (see the Annex for the detailed results). These findings only hold when assuming a polynomial order of one or two. On the contrary, when abstracting from polynomial estimations, the local RD coefficients are significant around the placebo cutoff. This hints towards the importance of including polynomial orders when approximating the true underlying empirical function of the relationship investigated in this paper. This insight is also in line with the empirical intuition drawn from the graphical representation of this relationship (see Figures 10 to 12). Nevertheless, when estimating the placebo effect in the optimal window, which is the minimum window in this case, shows that the local RD effect is insignificant for all three model specifications.

7.4 Inclusion and Exclusion Errors

Often, policy programs are implemented over a longer period of time and include an implementation period in which not all target groups are enrolled in the program. Moreover, most social protection programs have inclusion and exclusion errors (Kidd 2017). This is also the case with ChCC. An evaluation conducted by Asesorías para el Desarrollo (2012) shows that although the program ChCC is universal by design, and targets the entire population of children born after the date of its implementation, it took time to reach the target population. Treatment effects on the treated (ATETs) calculate the average of the individual effects of the program on all potential beneficiaries who all have a different likelihoods of being part of the program. Participation in ChCC might not be 100 %, even in the later birth-cohorts (born in 2012, for example) and to a lesser extent in early birth-cohorts (born immediately after the 2007/2008 roll-out). Participation rates vary depending on the services offered by ChCC. Parent workshop participation rates were 61 % in 2016, coverage rates for programs for women in prenatal and birth stages were 67 % in 2017, and coverage rates for the Abbreviated Psychosocial Assessment (EPsA) for pregnant women were 97 % from 2014-2017 (Milman 2018).

To take into account potential inclusion and exclusion errors around the roll-out of ChCC, I apply a fuzzy local randomization approach. I do not observe the actual treatment status of children. Hence, I construct the treatment variable based on several assumptions. In a first step, I assign each child a random number between 0 and 1 drawn from a uniform distribution. I assume that the actual probability of participating in ChCC is 80 %. I randomly assign each child a dummy variable in a way that results in 80 % of children being treated and 20 % not being treated. I then use this dummy variable as a proxy for the actual endogenous treatment variable and run a fuzzy local randomization approach.

In the optimal window length, I find no significant effects of ChCC on the three main outcome variables investigated in this paper. This finding also holds when employing a parametric regression specification of the local randomization approach, or when considering the randomly chosen number twenty as the window length (see the Annex for the detailed results).

This could mean that the program's take-up rate significantly influences its effectiveness. If those children who start off better in order to build valuable human capital are also more likely to comply with the program, ChCC might suffer from inefficiencies. To investigate this further, I employ a heterogeneity analysis later in this paper. In conclusion, non-compliance might seriously jeopardize the program's impact and cost-efficiency.

7.5 Early versus Late Roll-out Group

As detailed earlier in this paper, ChCC was rolled out in two phases. The early roll-out group consisted of all municipalities best prepared for its implementation. The experiences gained in the first round of roll-outs was then used for the implementation in the remaining municipalities. To investigate if the two-phased roll-out confounds my findings, I analyze the program's impact for the early and late roll-out group separately. I start by estimating the local RD effect in the optimal window, abstracting from a parametric estimation. I then analyze the program's impact in a window length of twenty, consecutively increasing the parametric degree from zero to two.

Table 12 suggests that the program's impact is driven by the late roll-out group. Based on the p-values reported in Column 1, the coefficients on all three schooling outcomes are insignificant at the 1% significance level for the early roll-out group. This is not the case for the late roll-out group. The p-values reported in Column 2 are zero in all three cases. Moreover, the point coefficients reported in Column 2 are larger than the ones reported in Column 1. When repeating the analysis in the larger window, and when employing a parametric estimation, the differences in the p-values are not as persistent (see the Annex for the detailed results). Still, while most of the p-values are significant for both the early and late roll-out group across the other three model specifications, the local RD estimators are larger for the late roll-out group.

These results imply that policymakers and implementing partners in the second phase of the program's roll-out benefited from important insights and experiences gained in the first phase of the roll-out. This, on the other hand, speaks for the importance of pilot projects when implementing early childhood interventions. Pilot projects give policymakers and their implementing partners the opportunity to correct for flaws in these type of interventions, and to make program's more effective.

Table 12: Local RD effect of ChCC on schooling outcomes in the optimal window - Early versus late roll-out group

		Early	Late
0	Panel 1: Standardized math scores		
1	RD Estimate	-1.011975	4.354651
2	P-Value	0.035	0
3	Panel 2: Standardized reading scores		
4	RD Estimate	-0.527493	4.354651
5	P-Value	0.326	0
6	Panel 3: Grade point averages		
7	RD Estimate	-0.003441	0.059106
8	P-Value	0.736	0
9	No. of observations (left)	16116	18208
10	No. of observations (right)	24685	27521

Note: The table shows the local RD effect of ChCC in the optimal window around the cutoff. This means that the estimation considers all students born two months before and after the roll-out of ChCC as well as those born during the roll-out. The early roll-out group consists of all municipalities, which implemented ChCC before 2008. The late roll-out group consists of those municipalities, which implemented ChCC during the second phase of its rollout. Panel 1 shows the results for standardized math scores, Panel 2 the ones for standardized reading scores, and Panel 3 the ones for grade point averages. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

7.6 Staggered Difference-in-Difference Estimation

To validate my findings I employ an alternative empirical estimation strategy. I take advantage of the staggered implementation of ChCC across municipalities and years, and apply a quasi-experimental research design. The municipality of residence, as well as the date of birth jointly determine a child's exposure to ChCC. I compare children with exposure to ChCC within their municipality to those without exposure to ChCC in the same municipality and across municipalities.

A comparison between cohorts within the same municipality could be driven by changes over time that determine differences between younger and older cohorts. However, comparing younger and older cohorts to an alternative municipality without ChCC exposure controls for these overall time effects. At the same time, simply comparing a cohort from a municipality with early exposure to ChCC to the same cohort from a municipality with late exposure could lead to biased estimated due to systematic differences between both municipalities. The inclusion of younger and older cohorts controls for potential time-invariant confounding factors between municipalities.

To test the effect of ChCC I apply a staggered difference-in-difference design (DiD) as in Hoynes, Schanzenbach, and Almond (2016b). I estimate the following regression equation:

$$y_{imb} = \alpha + \beta ChCC_{mb} + \eta_m + \lambda_b + \theta_s \times b + \gamma X_i + \delta M_{pre} \times b + \varepsilon_{imb} \tag{6}$$

, where i stands for student, m for municipality, b for a birth-cohort effect and s for the region of residence. y_{imb} is the outcome variable of interest, and β the main effect of interest, namely the effect of being exposed to ChCC. η_m is a municipality fixed effect, λ_b is a birth-cohort fixed effect, and θ_s are state-specific linear birth cohort trends. The state-specific linear time trend accounts for potentially confounding time-varying state policies. I include regional fixed-effects interacted with a birth-cohort trend to control for regional effects that changed across birth cohorts. I cluster standard errors at the regional level.

To control for the potential non-random roll-out of ChCC and possible confounding factors that influenced this decision, as well as the outcome variable of interest, I control for pre-treatment municipality characteristics and interact them with a birth-cohort trend ($M_{pre} \times b$). As a robustness check, I further control for individual-level characteristics (X_{imb}).

The results in Table 13 show the coefficients for standardized test scores and the grade point average when applying a staggered difference-in-difference strategy. The effect of being exposed to ChCC is insignificant for all educational outcomes under consideration. When restricting the sample to all children born 18 months before and after ChCC's implementation, the coefficients go in the same direction and remain insignificant. This also holds when restricting the sample to 13 or 10 months around the roll-out. Consequently, my findings from the local randomization approach do not hold when exploiting the staggered nature of ChCC's rollout.

There could be several reasons for that. First of all, while systematic differences between early and late adopters do not seem to play a significant role, there might be unobservable variables confounding my results from the staggered difference-in-difference estimation. The main distinction between the staggered DiD design and the local randomization approach is that the staggered DiD design takes into account the chronological roll-out of ChCC across municipalities, while the RD estimator abstracts from the spatial dimension. It is therefore likely that the observed differences stem from the chronological spatial sequence of the program's implementation. The staggered DiD estimator might be confounded by systematic, unobservable trends hiding the true effect of ChCC.

Moreover, the insignificant DiD estimators might speak for students differing significantly from each other when going farther away from the cutoff. This, on the other hand, might lead to biased staggered DiD estimators. In fact, when plotting event study graphs of my main outcome variable of interest, there is evidence of pre-treatment trends for all three variables (see Figure 13, 14 and 15). This could be evidence of unobservable confounding factors, which I cannot account for through the introduction of fixed effects and observable control variables. Moreover, several researchers have pointed out important empirical shortfalls in the estimation of two-way fixed effect estimators. Firstly, the staggered DiD estimators are not guaranteed to have a policy-relevant interpretation (Borusyak and Jaravel (2017), De Chaisemartin and d'Haultfoeuille (2020), and Goodman-Bacon (2021)). Secondly, it is not possible to rigorously interpret event study coefficients as dynamic treatment effects (Sun and Abraham 2021). On these grounds, the local randomization approach might be the most appropriate identification strategy in this particular set-up.

Table 13: The effect of ChCC on educational outcomes

	(1) Math	(2) Math	(3) Math	(4) Reading	(5) Reading	(6) Reading	(7) GPA	(8) GPA	(9) GPA
ChCC Indicator	0.529	0.389	0.431	0.537	0.667	0.652	-0.00173	0.000479	0.0000500
	(0.442)	(0.669)	(0.659)	(0.387)	(0.489)	(0.472)	(0.00593)	(0.00583)	(0.00528)
Constant	261.3***	261.3***	264.5***	268.5***	268.3***	265.6***	5.834***	5.832***	5.801***
	(0.142)	(0.216)	(0.406)	(0.125)	(0.158)	(0.426)	(0.00191)	(0.00188)	(0.00885)
Municipality fixed-effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date of birth fixed-effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Precontrols	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Individual controls	No	No	Yes	No	No	Yes	No	No	Yes
Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.0657	0.0681	0.0747	0.0471	0.0496	0.0594	0.0257	0.0296	0.0352
N	834848	810051	810043	834848	810043	810043	834848	810043	810043

Standard errors in parentheses

Source: SIMCE 2015-2018, MINEDUC 2015-2018, SINIM 2006, Census 2002, and Clarke et al..

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The table shows the staggered DiD estimators on the three schooling outcomes investigated in this paper. I control for municipality as well as birth of date fixed effects. I include a regional time trend. I include the following pre-treatment municipality characteristics: the poverty rate at the municipality level, the number of families receiving subsidies, the available budget per municipality, the share of education spending by the Ministry of Education, the type of administrative cooperation in education, the student-teacher ratio, having or not having a primary health unit, the health transfer per capita from the Ministry of Health and the share of votes in the 2004 mayoral elections. I control for the following individual characteristics: gender, the socioeconomic status of a student, and urbanity. Standard errors are in parentheses and clustered at the regional level.

7.7 Event Study

To further investigate the diverging results from the staggered DiD estimation as well as the local randomization approach, I employ an event-study design. My main explanatory variable is the number of months that ChCC had been in place in a certain municipality when a child was born. Let's assume, for example, that a child was born in August 2008. If ChCC was introduced in her respective municipality in August 2007, the main explanatory variable has a value of 12. If the child was born in August 2006, the main explanatory variable has a value of -12. The regression estimation for the event study is as follows:

$$y_{mb} = \alpha + \beta \sum_{m=-13}^{13} I_m + \eta_m + \lambda_b + \theta_s \times b + \delta M_{pre} \times b + \gamma X_i + \varepsilon_{imb} \tag{7}$$

,where m stands for the municipality, and b for the birth-cohort. One cell in the sample represents a combination of a specific municipality and birth-cohort. y_{mb} is the outcome of interest (as, for example, the average municipality-level standardized test score for a certain birth-cohort) and β the main effect of interest. η_m is a municipality fixed effect, λ_b a birth-cohort fixed effect, and $\theta_s \times b$ a state-specific linear time of birth trend. Standard errors are clustered at the regional level. I omit period -1. Additionally, I interact some pre-treatment municipality characteristics with a time of birth trend ($M_{pre} \times b$) and control for individual time-varying controls (X_i).

Schmidheiny and Siegloch (2019) recommend a binning approach in which the number of pre-periods included in the event study is equal to the first year of data for the dependent variable (in my case, July of 2007) minus the effect window (which in my case is 13 periods). Figure 13 shows the results for standardized math scores, Figure 14 for standardized reading scores and Figure 15 for GPAs.[23]

Figure 13 shows that there is no pre-trend for standardized math scores and that math scores increase consistently after the introduction of ChCC. The same is true for standard-

[23] I take advantage of the command *eventdd* provided by Clarke and Schythe (2020).

Figure 13: Event study for stand. math score

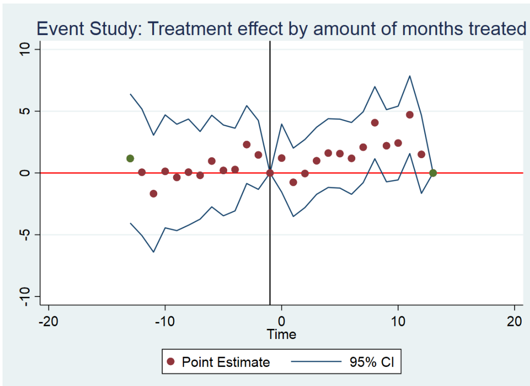


Figure 14: Event study for stand. reading score

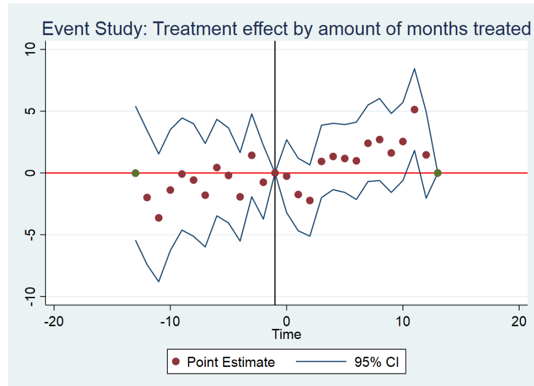
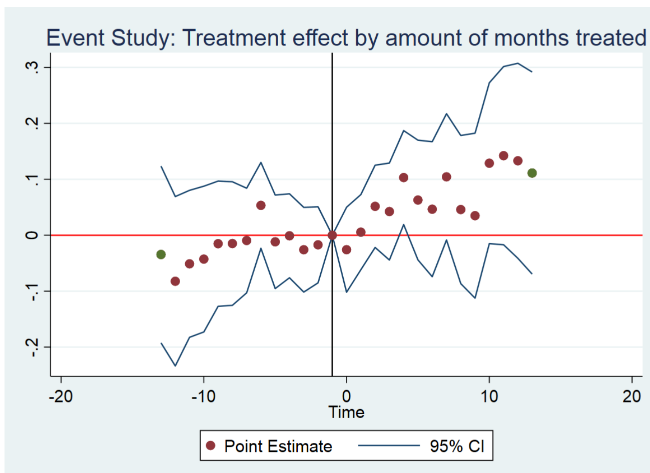


Figure 15: Event study for grade point averages



Note: The plots above shows results for an event study on standardized math and reading scores as well as grade point averages. I aggregate data at the date of birth municipality level and run a regression using the average standardized test score and grade point averages for each cell as the outcome variable. The main explanatory variable is the number of treatment periods relative to the ChCC roll-out period. I additionally control for municipality fixed effects as well as date of birth fixed effects. I include an interaction term between the birth cohort and the region of residence, as well as pre-treatment controls. I include the following pre-treatment municipality characteristics: the poverty rate at the municipality level, the number of families receiving subsidies, the available budget per municipality, the share of education spending from the Ministry of Education, the type of administrative cooperation in education, the student-teacher ratio, having or not having a primary health unit, the health transfer per capita from the Ministry of Health, and the share of votes in the 2004 mayoral elections. I control for the following individual characteristics: the share of female students, the share of vulnerable students, and the share of students from rural areas. The omitted event time is period -1, represented by the vertical black line. Standard errors are clustered at the regional level. Source: SIMCE and MINEDUC (2014-2018).

ized reading scores (Figure 14), although there might be a small periodic trend in the pre-treatment period. Still, standardized reading scores increase after the introduction of ChCC. Figure 15 makes clear that there is evidence of a slight but negligible pre-trend in the case of grade point averages. Grade point averages increase steadily in the post-treatment period. Overall, my event study confirms the results from the local randomization approach. It shows that participation in ChCC has a positive effect on all three variables.

I conduct a joint significance test of the 13 lags included in the event study. I can reject the null hypothesis that all coefficients are zero at the commonly used levels of significance.

7.8 Migration

I do not have information on the children's municipality of birth, only on the children's municipality of residence. Therefore, my treatment and control group could be confounded by internal migration patterns. To test this, I analyze these patterns using data from the latest 2017 micro-census. The data shows that 15.8 % of the population are internal migrants (Instituto Nacional de Estadísticas 2020). Internal migrants are defined as all people who changed their residence between 2012 and 2017 by moving between regions or within one region but between municipalities. Importantly, households with children are less likely to migrate internally, and the share is lowest among the youngest and oldest population (less than 2 %). Internal migration patterns only begin to take hold for children over age 15. As these groups are not included my sample, I conclude that internal migration patterns of less than 2 % for my target group should not be a significant confounding factor in the definition of my treatment and control group.

7.9 The Financial Crisis and Copper Prices

I account for economic fluctuations by including a time dummy in my regression specification. Since the implementation of ChCC took place just before the onset of the financial crisis in 2008, I analyze the potential effect of the financial crisis as a confounding factor. The financial crisis would be a problem for my estimation strategy if it had a systematically different effect on the schooling and developmental outcomes of children in municipalities introducing ChCC earlier than those municipalities introducing ChCC at a later stage. This channel could arise from a transitory effect of the financial crisis on income and poverty and then on schooling outcomes.

The implementation of ChCC was completed in August 2008. Like most emerging economies, the financial crisis hit Chile later than the developed countries. This is why Chile did not enter a severe recession until late 2008 (Cortés 2016). Real GDP growth (year-to-year) began to fall in third quarter and fourth quarter of 2008 (3.5% and 0.9 % respectively), and quarterly growth was negative by the first quarter of 2009 (-2.6%) (OECD 2021). I therefore conclude that the financial crisis is no threat to my identification strategy.

With respect to copper prices, the same reasoning applies. If municipalities introducing ChCC earlier are municipalities which depend heavily on the Chilean copper industry, and if these industries are then hit hard by a negative development of copper prices, the development of copper prices might be a confounding factor. As the copper price did not start to fall sharply until September 2008, I can rule it out as a confounding factor.[24]

8 Impact on Schooling Outcomes by Subgroups

Next, I analyze the effects of the program on educational outcomes by subgroups. I divide the treatment group by gender and socioeconomic vulnerability. I then estimate the local RD effect of the program's impact in the optimal window, namely in the four periods around the cutoff.

Table 14 shows that the program's impact significantly differs across subgroups. Comparing the point coefficients in Column 1 and 2 of Panel 1 makes clear that the program has larger effects on boys' standardized math scores than on girls'. In fact, the local RD

[24]For the detailed development of copper prices see `https://tradingeconomics.com/commodity/copper`, and for a graphical overview see Figure C1 in the Appendix.

estimate is only significant at the 10% significance level when restricting the sample to girls only. According to Column 2, participation in ChCC increases standardized maths scores by 0.996 points for boys but only by 0.771 points for girls. When compared to the average in the optimal window, this is a relative increase of 0.376 % for boys compared to 0.295 % for girls. Turning attention to Panel 2 and 3, the same patterns become apparent for the program's impact on the other two schooling outcomes investigated in this paper. While in the case of standardized reading scores and grade point averages all RD estimators are significant at the 1% significance level, participation in ChCC results in smaller effects on both schooling outcomes for girls than for boys. Column 2 shows that the program increases standardized reading scores by 2.315 points for boys, but only by 1.805 points for girls. The same is true for grade point averages (see Column 2 and 3 in Panel 3).

The different impact is even larger when analyzing it by socioeconomic vulnerability. For socioeconomically vulnerable children the program's impact on standardized math scores is insignificant. The p-value presented in Column 3 is above 0.1. Column 3 and 4 in Panel 1 present the respective point estimates. While the program leads to an increase in standardized math scores of 1.639 for non-vulnerable children, this same effect is only 0.615 for vulnerable children. The effect is therefore nearly three times larger for the socioeconomically privileged group. The program's impact on standardized reading scores is 1.8 times larger for the socioeconomically privileged group (3.107 points versus 1.689 points). It is 2.8 times larger in the case of grade point averages (0.056 points versus 0.020 points).

These findings hold when accounting for a larger cutoff window, as well as for polynomial orders of degree one and two by subgroups (for the detailed results see the Annex).

The heterogeneous impact of ChCC by subgroups could mean that the program fails to address important human capital gaps between different groups and benefits those who are already more privileged most. This hints towards important shortcomings in the inclusiveness of universal, comprehensive early childhood interventions. Less privileged groups might be less likely to comply with the program, or the inclusion error might be larger for

Table 14: Local RD effect of ChCC in the optimal window around the cutoff on schooling outcomes by groups

	Subgroup	Boys	Girls	Vulnerability	Non-vulnerability
0	Panel 1: Standardized math scores				
1	RD Estimate	0.996001	0.771383	0.615283	1.63933
2	P-Value	0.037	0.094	0.116	0.008
3	Panel 2: Standardized reading scores				
4	RD Estimate	2.315476	1.80484	1.688649	3.107319
5	P-Value	0	0	0	0
6	Panel 3: Grade point averages				
7	RD Estimate	0.034327	0.025105	0.020464	0.055809
8	P-Value	0	0	0	0
9	N (left)	17039	17285	25375	8949
10	N (right)	25912	26294	38579	13627

Note: The table shows the local RD effect of ChCC in the optimal window, namely four months around the threshold. This means that the estimation considers all students born two months before and after the roll-out of ChCC as well as those born during the roll-out. Panel 1 shows the results for standardized math scores, Panel 2 for standardized reading scores, and Panel 3 for grade point averages. I first report the local RD estimator, and then the p-value. For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016). Source: SIMCE (2015-2018) and MINEDUC (2015-2018).

these groups. Another possible explanation is that the quality and quantity of the services offered to these groups might be worse. The heterogeneous impact of participation in ChCC could also be evidence of the program falling short on addressing important drivers behind human capital gaps, such as gender stereotypes. The results presented cause doubt on the effectiveness of universal, comprehensive programs like ChCC in closing human capital gaps between socioeconomic groups. While the program's impact is overall positive for all groups investigated, the heterogeneous impact by vulnerability and gender is worrisome. Targeted programs might be more suited to address the needs of vulnerable children.

9 Potential Drivers of Improved Schooling Outcomes

ChCC is a comprehensive program addressing several different aspects of early childhood development. To shed some light on the mechanisms that are behind the positive outcomes on education in middle childhood, I analyze the program's impact on intermediate outcomes.

I apply the same local randomization approach described earlier in the paper. I report the local RD estimator in the minimum cutoff window as well as the larger cutoff window.[25] In case of the larger window, I employ a non-parametric estimation as well as parametric estimations of degree one and two. In what follows, I report local RD estimators and p-values for these four model specifications.

9.1 Outcomes at Birth

One important component of ChCC aimed at the enhancement of birth-giving conditions in Chile. Hence, the first group of intermediate outcomes I consider are birth outcomes. I rely on administrative data provided by the Ministry of Health. I analyze the impact of ChCC on six different outcomes at birth: if a birth took place without medical attention, if it took place in a hospital, if it was premature, if the baby was of low-weight, the baby's height, as well as the mother's age at birth.

I start by investigating the effect of participation in ChCC in the minimum window around the threshold. Table 15 shows that the program does not significantly improve any of the birth outcomes investigated in this paper. Column 2 reports only one p-value close to zero, namely the one on hospitalized births. According to the point estimate reported in Column 1, participation in ChCC increases the probability of hospitalized childbirth by 0.008 percentage points. When increasing the window length around the threshold to twenty, none of the local RD estimators is significant. All p-values besides one are larger than 0.1. While the point estimate on the probability of childbirth without medical attention is significant around the 5% significance level when accounting for a parametric order of one, this significance does not hold under the other three model specification.

Table 15 also indicates that the local RD estimators are sensitive to the underlying model specifications. Not only do the p-values vary widely across the four model specifications presented, but also do the point estimates change signs. Concentrating on the results from

[25] I also implement window selection procedures based on balance tests to find the optimal window length. None of the windows passes the covariate test.

the parametric model specification of degree two in Column 7, the coefficients go into the expected direction but are insignificant. Overall, from the presented evidence, it is unlikely that ChCC successfully alters birth outcomes.

Table 15: Local RD effect of ChCC on birth outcomes

	Specification	Window/2	1	Window/2	10	Polyn. order	1	Polyn. order	2
0	Variable	RD Estimate	P-Value	RD Estimate	P-Value	RD Estimate	P-Value	RD Estimate	P-Value
1	Without medical attention	-0.000788	0.248	0.000356	0.638	0.001204	0.054	0.000754	0.735
2	Hospitalized	0.008338	0.001	0.004036	0.356	-0.002647	0.465	0.000676	0.925
3	Premature	0.00152	0.656	0.010996	0.181	0.008704	0.19	-0.000877	0.959
4	Low-weight	-0.000441	0.628	0.003554	0.888	0.003913	0.514	-0.000029	0.998
5	Height	0.017305	0.668	-0.035287	0.672	-0.069576	0.343	-0.127738	0.482
6	Age (mother)	0.13684	0.423	0.193301	0.177	-0.149338	0.47	0.549954	0.25

Note: Preterm birth is defined as all births before the 37th gestational week. Low-weight means a birth weight of less than 2,500 grams. Height is in cm and maternal age is in years. The table shows different local RD effects. The first column refers to a local randomization approach considering the minimum window around the cutoff. The next three columns consider twenty windows around the cutoff, subsequently increasing the polynomial order from zero to two. For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016). Importantly, the data for 2007 is missing. Given that 2007 is part of the period during which ChCC was rolled out, this could bias the results shown in this table. Source: MINSAL 1992-2018 and Clarke et al..

9.2 Early Childhood Education

ChCC also increased the supply of early childhood education (ECE) provided by the public sector. I therefore investigate whether the program increases the share of children attending early childhood education facilities. I again rely on administrative data on children attending these establishments provided by the Ministry of Education. I calculate the attendance rate in each respective municipality-birth cell by counting the number of children attending ECE and then dividing it by the number of births in this same cell.

Table 16 shows that the program does not lead to a higher attendance rate in early childhood education facilities, neither for boys nor for girls. The p-values in the minimum window around the threshold are large and the point estimates reported in Column 1 therefore insignificant. While the local RD estimates in Column 3 and 5 are significant at the 1% significance level, this does not hold for the parametric model specification of degree two in Column 7. Similar to my findings on birth outcomes, the point estimates and p-values on ECE are highly sensitive to the different model specifications.

Consequently, the evidence on ChCC's impact on early childhood education is inconclusive. Taking the model specification, which seems to best approximate the true underlying functional form of the relationship investigated in this paper, as the main result, the coefficients go into the expected direction but are insignificant (see Table 16 Column 7 and 8). Again, another important component seems to fail in achieving real change.

Table 16: Local RD effect of ChCC on early childhood education (ECE)

	Specification	Window/2	1	Window/2	10	Polyn. order	1	Polyn. order	2
0	Variable	RD Estimate	P-Value	RD Estimate	P-Value	RD Estimate	P-Value	RD Estimate	P-Value
1	ECE	-0.003454	0.871	-0.189182	0	-0.076807	0	0.025563	0.334
2	Boys	-0.020453	0.506	-0.188207	0	-0.097728	0	0.022678	0.61
3	Girls	0.014876	0.61	-0.190228	0	-0.054947	0.021	0.030319	0.41

Note: ECE is the share of children attending early childhood education facilities. To calculate this share I aggregate the number of children attending early childhood education facilities in 2011 by date of birth and municipality. I then divide this number by the number of children born in this same date of birth municipality cell. Importantly, the data on births is missing for the year 2007. Given that 2007 is part of the period during which ChCC was rolled out, this could bias the results shown in this table. The table shows different local RD effects. The first column refers to a local randomization approach considering the minimum window around the cutoff. The next three columns consider twenty windows around the cutoff, subsequently increasing the polynomial order from zero to two. For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016). Source: MINEDUC 2011, MINSAL 1992-2018 and Clarke et al..

9.3 Cognitive and Non-cognitive Development

Cunha, Heckman, and Schennach (2010) analyze the interaction between cognitive and non-cognitive skills and their importance for learning outcomes. They find that students with higher cognitive and non-cognitive skills in the early years of life are more successful in learning these skills later in life. These skills then affect a variety of outcomes, as, for example, test scores, schooling, and wages. Additionally, several papers have stressed the importance of the home environment in shaping children's development (see Almond, Currie, and Duque (2018) or Currie and Almond (2011)). This is why I investigate the program's impact on cognitive and non-cognitive child development. For a detailed description of the variables at use see the data section.

Table 17 shows inconclusive results of the program's impact on cognitive child development. While some of the p-values reported in the table are below 0.1, the p-values vary widely for the different model specification. Column 2 shows that none of the local RD estimators is significant in the minimum cutoff window. According to Column 8, the program significantly decreases the TEPSI score when estimating the parametric model specification of degree two. All other point estimates are insignificant under this model specification. The program also significantly decreases the TEPSI Score reported in Column 5. Still, given that the sign of this coefficient changes across model specifications, and given that the p-value increases considerably in the non-parametric estimation, the program's impact on this evaluation instrument is inconclusive. The same is true for the other two point estimators which are significant in Column 3: the TVIP and TADI Score. While the p-values are below 0.1 in Column 4, they increase significantly when estimating the other three model specifications.

I next analyze the impact of the program on children's non-cognitive development. Table 18 shows that participation in ChCC has no significant effects on the indicators investigated in this paper. While the local RD coefficient on the existence of an abnormal head circumference is significant under the parametric model specification of degree two in Column 7, this does not apply to the rest of the model specifications. The p-value in Column 8 is close to zero and the local RD estimator significant at the 1% level. ChCC's impact on this specific variable might be positive as the program led to significant improvements in the screening for risk factors for children's development. Therefore, the coefficient might reflect a more rigorous screening and not an actual increase in abnormal child developments. While some of the other point estimators are significant in some of the model specifications, the p-values increase under the parametric model specification of degree two, and none of the other coefficients is significant at the commonly used significance levels (see Column 8).

In conclusion, unlike the findings on schooling outcomes, the coefficients on cognitive and non-cognitive child development seem to be unstable and significantly affected by underlying empirical assumptions. This could be due to the fact that the results shown in Table 17 and 18 rely on survey data and not administrative data. Moreover, the fact that I cannot observe children's birth of date in the first and second survey wave might lead to an important loss of information affecting the stability of the local RD estimators investigated with this data.

Table 17: Local RD effect of ChCC on cognitive child development

	Specification	Window/2	1	Window/2	10	Polyn. order	1	Polyn. order	2
0	Variable	RD Estimate	P-Value	RD Estimate	P-Value	RD Estimate	P-Value	RD Estimate	P-Value
1	TEPSI	-0.39371	0.686	0.30287	0.461	-1.890565	0.013	-3.205454	0.005
2	TVIP	1.473552	0.22	-1.924468	0	0.971032	0.251	0.850546	0.528
3	TADI	1.081999	0.199	-0.874395	0.002	0.817985	0.165	1.224317	0.204
4	Applied problem-solving	0.111839	0.891	-0.059239	0.841	-0.379972	0.529	-0.11468	0.904
5	Mathematical literacy	0.083988	0.908	0.101503	0.766	-0.373332	0.544	0.579665	0.562
6	Calculation	-0.4043	0.632	-0.363169	0.22	-0.878042	0.149	-0.174868	0.858

Note: The table shows different local RD effects on cognitive child development. The first column refers to a local randomization approach considering the minimum window around the cutoff. The next three columns consider twenty windows around the cutoff, subsequently increasing the polynomial order from zero to two. For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016). Source: ELPI 2010-2017, SINIM 2006, Census 2002, and Clarke et al..

Table 18: Local RD effect of ChCC on non-cognitive child development

	Specification	Window/2	1	Window/2	10	Polyn. order	1	Polyn. order	2
0	Variable	RD Estimate	P-Value	RD Estimate	P-Value	RD Estimate	P-Value	RD Estimate	P-Value
1	BDS	0.809455	0.157	-0.541879	0.019	0.427865	0.362	0.568647	0.434
2	HTKS	0.630429	0.503	0.13194	0.697	1.324687	0.055	1.18822	0.278
3	CBCL1	-0.604417	0.377	0.115074	0.653	-0.102116	0.834	-1.111738	0.155
4	Abnormal weight	0.010277	0.581	0.031982	0	0.006694	0.621	-0.005716	0.793
5	Abnormal height (ECD)	-0.026326	0.327	0.005547	0.527	-0.01542	0.431	-0.042569	0.174
6	Abnormal head circ.	0.031357	0.175	0.007411	0.398	0.024179	0.164	0.076885	0.005

Note: The table shows different local RD effects on non-cognitive child development. The first column refers to a local randomization approach considering the minimum window around the cutoff. The next three columns consider twenty windows around the cutoff, subsequently increasing the polynomial order from zero to two. For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016). Source: ELPI 2010-2017, SINIM 2006, Census 2002, and Clarke et al..

9.4 Intra-household Relations and Parenting

ChCC was not only directed at the children themselves, but also at their caregivers. I therefore also analyze the impact of ChCC on a variety of parental outcomes. I focus on the program's impact on the official evaluation instruments measuring intra-household relations included in the ELPI survey. These are the parental stress index (PSI), the self-assessment of parenting skills (PSCS), the presence of depressive symptoms (CESD), and the home environment index (HOME). For a detailed explanation of these evaluation instruments see the data section. I also consider a number of additional variables measuring parenting skills: the degree of gender-neutral parenting, the space at home available for children's toys, the amount of learning equipment and books at home, if the mother reads out to her child, if parents share a meal together with their child, and evidence of inappropriate dental care.

Table 19 shows that there is no strong evidence of the program altering any of the indices measuring parental outcomes. None of the p-values on the parenting evaluation instruments in Column 2 and 8 are below 0.1. Therefore, none of the point coefficient reported in Column 1 and 7 are significant in the minimum window nor in the parametric model specification of degree two. Still, there is slight evidence of the program decreasing the presence of depressive symptoms. Column 6 shows that the point estimate on CESD is significant at the 7.8% significance level. It is therefore possible that the program positively affected the mood scale of caretakers. Moreover, there is some evidence suggesting positive impacts of participation in ChCC on the home environment. The p-value in Column 4 is close to zero. Therefore, the local RD estimator on the HOME index in Column 3 is significant around the 1% significance level. The coefficients on these two parenting outcomes do not switch signs across model specifications and are therefore more stable than the intermediate outcomes on child development reported previously. Still, given that the p-values vary widely across model specifications, the results remain inconclusive.

Turning the attention to the additional variables measuring parenting skills, Table 19 suggests that the program influences important dimensions of parental care. Several of the p-values on the space available for toys, the amount of learning equipment and the number of books are below 0.1. Moreover, the point estimator on the latter do not switch signs across model estimations and are therefore stable. This is not the case for the local RD estimator on the space available for toys. Additionally, when concentrating on the parametric model specification of degree two, only one of the p-values in Column 8 is below 0.1, namely the one on the number of books available. Hence, while there is some suggestive evidence on the program improving parental outcomes, the results are not stable across model specifications.

Nevertheless, from the overall evidence presented in Table 19, it is possible to state that participation in ChCC leads to significant improvements in parental outcomes. Importantly, these outcomes seem to be limited to material goods. There is no evidence of any changes in parental behavior. None of the coefficients on reading, or sharing meals, are significant at the commonly used significance levels. My findings speak for the importance of parental workshops and parental care for children's human capital accumulation.

Table 19: Local RD effect of ChCC on parental outcomes

	Specification	Window/2	1	Window/2	10	Polyn. order	1	Polyn. order	2
0	Variable	RD Estimate	P-Value	RD Estimate	P-Value	RD Estimate	P-Value	RD Estimate	P-Value
1	PSI (Int.)	-1.557468	0.61	-0.662928	0.528	-1.986408	0.352	-1.567741	0.615
2	PSCS	0.452106	0.615	-0.066013	0.831	0.852322	0.176	0.099286	0.317
3	CESD	-0.454792	0.317	-0.089587	0.581	-0.582611	0.078	-0.941102	0.926
4	HOME	0.020851	0.926	0.195127	0.011	0.027934	0.859	-0.04734	0.851
5	Gender-neutral parenting	-0.002373	0.854	-0.008512	0.066	-0.003074	0.744	0.012153	0.416
6	Space for toys	0.025218	0.041	0.006698	0.084	-0.009626	0.235	0.018451	0.172
7	Learning equipment	0.044075	0.005	0.069597	0	0.039363	0.001	0.014682	0.424
8	Books	0.042976	0.001	0.025653	0	0.02771	0.001	0.025851	0.065
9	Reading (mother)	0.011775	0.715	-0.007013	0.534	0.037146	0.106	-0.016815	0.648
10	Sharing meals	0.005365	0.814	-0.008192	0.286	-0.004671	0.768	-0.005731	0.822
11	Lacking dental care	0.024345	0.152	0.001118	0.852	0.035658	0.003	0.019578	0.315

Note: The table shows different local RD effects on parental outcomes. The first column refers to a local randomization approach considering the minimum window around the cutoff. The next three columns consider twenty windows around the cutoff, subsequently increasing the polynomial order from zero to two. For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016). Source: ELPI 2010-2017, SINIM 2006, Census 2002, and Clarke et al..

9.5 Summary

To sum up, ChCC has significant effects on mechanisms within households. Still, these mechanisms are limited to in-kind transfers and there is overall limited evidence of observable changes in parents' behavior. While ChCC leads to a significant increase in the number of books and learning equipment in households as well as the space made for children's toys, the time spent together with children does not increase. Participation in ChCC does not increase the probability that mothers read out to their children nor the probability of eating a meal together. It does also not influence the degree to which parents treat female and male children equally.

Additionally, I do not find any evidence of significant effects on birth outcomes. The results on the program's impact on attendance rates in early childhood education facilities are inconclusive. Lastly, while some of the local RD estimators on cognitive and non-cognitive child outcomes are significant, these results are not stable across the different model specifications under consideration in this paper. Overall, my results cause doubt that the program influenced these channels. My findings could mean that the program is unsuccessful in improving these intermediate outcomes. The inconclusive evidence could also be driven by important shortfalls in the survey data at use.

10 Cost-Benefit Analysis

To compare the costs and benefits of ChCC to alternative programs, I use a framework developed by Hendren (2016) and Hendren and Sprung-Keyser (2020) to calculate the marginal value of public funds (MVPF). Benefits are captured by beneficiaries' willingness to pay and costs capture initial program spending and fiscal externalities. The MVPF is the ratio of both.

To calculate beneficiaries' willingness to pay, I estimate the average lifetime earnings in Chile based on the 2017 socioeconomic household survey (CASEN). I first calculate the mean income of all individuals included in the survey by age. I restrict the working population to all individuals between the ages of 18 and 64. I then assume a discount rate of 3 % and calculate the average present value of lifetime earnings (PVLE) in Chile. I estimate that the PVLE in Chile is 220,312.4 US-Dollar.[26]

Based on data provided by the government of Chile, the estimated average unit cost of ChCC is 23,647.2 US-Dollar. Distributing these 23,647.2 US-Dollar over the life of an average person in Chile yields a present value of the annual average unit costs of 12,864.5 US-Dollar. The paper by French et al. (2015) shows that a 1 % increase in GPAs leads to an average increase in income of around 12 to 14 %. The average GPA in my sample is 5.8 (see Table 1). Based on the different model specifications investigated in this paper, the average impact of ChCC on GPAs is approximately 0.03. This corresponds to an increase of 0.5 %. The equivalent increase in income would therefore be approximately 7.5 %.

A 7.5 % increase in lifetime earning leads to a difference in the present value of lifetime earnings between the pre- and post-program world of 16,523 US-Dollar per participant and an additional present value of tax revenues of 1,156.6 US-Dollar per participant. The MVPF is then 1.41 per participant.

The MVPF is lower than the MVPF of the Food Stamp Program in the US of 56.25 (Bailey et al. 2020) or the Perry Preschool Project's MVPF of 43.61 (Hendren and Sprung-

[26]A detailed overview of the methodology can be found in the Annex.

Keyser 2020). This could be due to the fact that girls and the socioeconomically vulnerable do not benefit equally from the program and that several of the intended channels seem to be unaffected by the program. The evidence shown in this paper might speak for a more targeted approach, as these might be more effective than universal, comprehensive programs such as ChCC.

11 Discussion and Conclusion

In this paper I study the effects of a comprehensive early childhood development program that combines components of health, education and parental care on medium-term outcomes for children. In particular, I study the effect of the program Chile Crece Contigo on educational achievement as well as cognitive and non-cognitive skills. I additionally look at birth outcomes, parental care, as well as attendance rates in early childhood education facilities.

I find that the program has a positive effect on grade point averages as well as standardized math and reading test scores. The effects are more marked for boys than for girls. The effect is smaller for the socioeconomically vulnerable population. The positive impact on school performance seems to be driven by improvements in intra-household relations. Still, this effect is limited to material goods. There is no evidence of the program leading to behavioral changes of parents. The program does also not improve outcomes at birth. Lastly, the evidence on the program's impact on cognitive and non-cognitive skills, as well as early childhood attendance rates is inconclusive. This might be due to the program not having a clear effect on these outcomes, or due to shortfalls in the data at use. When allowing for non-compliance, the local RD coefficients turn insignificant. This means that imperfect program take-up influences its effectiveness. Moreover, the program's impact is larger for municipalities implementing the program during the second phase of its roll-out. This makes a case for piloting early childhood interventions.

My findings are robust to several model specifications. I employ different polynomial estimations of the local RD approach applied in this paper. I show that there is no manipulation around the cutoff, and that there are no significant effects around a placebo cutoff. Moreover, I consider several cutoff windows and investigate the role of pre-treatment characteristics and observable student characteristics in the results shown. Lastly, I compare my findings to a staggered difference-in-difference estimation as well as an event study, and show that the local RD approach is indeed the most appropriate empirical strategy in this setting.

The findings contribute to a large literature analyzing early childhood development. The paper at hand shows that income as a channel is not the only possible measure to increase human capital and that a comprehensive approach to early childhood development can lead to improvements in child development across several dimensions. It also shows that, in addition to targeted programs for poor children, universal interventions also have positive effects on child development. This paper also fills the gap of the "missing middle years", showing positive effects of investments in early childhood on outcomes observed during middle childhood.

Policymakers should use the insights of this paper to design more integrated and comprehensive approaches to early childhood development and develop strategies to decrease disparities in human capital. They should pay special attention to the gender dimension of such programs so that boys and girls benefit equally. Additionally, mechanisms need to be designed to have a greater effect on the most vulnerable populations. To offset the per capita costs of the program, a 5.8 % increase in wages is required. The relatively low MVPF of 1.41 could indicate that more targeted programs with a higher MVPF are more effective in reducing human capital gaps.

Further research should analyze if the trajectory effect carries over from early to middle childhood into adulthood, and study the impact of ChCC on long-term outcomes, like tertiary education, wages and health in the long run.

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Tables

List of Tables

Figures

List of Figures

Appendix A - Program Roll-out

The table below details the inclusion of beneficiaries into the program during the early years of ChCC.

Figure A1: Beneficiaries of ChCC (roll-out)

Coverage	2007	2008	2009
Municipalities	159	345 (all)	345 (all)
Pregnant women	47,683	202,729	205,935
Births	40,119	160,643	171,373
Children under 1		168,823	173,733
Children aged 1 to 2		174,286	176,854
Children aged 2 to 4			324,338

Source: Adapted from The World Bank (2018)

Appendix B - Detailed Program Description

Summary of the Program

The social subsystem ChCC is a decentralized program that operates locally through municipal networks (called Red Comunal). Children and their mothers start to form part of the social subsystem ChCC during the first prenatal control check-up. From that moment onwards, children are part of the program, with special services offered to them and their families. The services offered start during gestation. They consist of regular health check-ups, parental education programs, the hand-over of materials for stimulation, as well as the assessment of risk factors and the development of personalized health plans. Additionally, pregnant women who are part of the vulnerable population, have access to a family subsidy, and home visits. The program also includes a personalized birth-giving process, which is facilitated through a number of actionable items.

ChCC offers a variety of services and benefits to children under five and to their parents. These services range from the handover of didactic materials on how to stimulate children to the introduction of educational group workshops, personalized hospitalization, the development of individual health plans and special services offered to children with disabilities or development lags. It also gives children who are part of the 40 % most vulnerable population free access to early childhood education.

In the following, I will describe the different components of ChCC in more detail.

Pregnancy and Childbirth

ChCC offers special services to pregnant women and significantly improved the birth-giving experience. First of all, it increased the time of prenatal checkups from 20 to 40 minutes.[27] The program also introduced psycho-social risk factors into the risk screening of pregnant women. ChCC introduced the development of a personalized health plan and personalized home visits. These plans are applied to all women who suffer from any kind of risk factor.[28]

Another entry point of ChCC is the guarantee of equal access to information about pregnancy and birth-giving. Families receive a so-called Gestation Guide during their first prenatal check-up. [29] Moreover, ChCC provides the possibility to participate in prenatal workshops targeted at pregnant women and their partners. The workshops consist of six sessions and provide information about birth-giving and child-care, and provide cognitive and emotional support. Also, ChCC introduced the transfer of educational materials about pregnancy and birth-giving to expectant parents. Additionally, ChCC personalized the birth-giving process and introduced a campaign with the goal to raise awareness about the importance of being accompanied while giving birth. It introduced a variety of actionable items aiming at the facilitation of birth-giving. Additionally, ChCC introduced an additional education session with information about the child-bed. In 2008, a nutritional component was developed, called Purita Mamá.

Newborns

In 2009, ChCC introduced a component specifically addressing the needs of newborns, called PARN (Programa de apoyo al recien nacido). The program consists of in-kind transfers of materials that are useful for the care-taking of a newborn (as oils, creams, a towel, clothes, and a blanket, among others). It also includes educational materials for parents with information on how to take care of newborns.

[27] The so called EPsA (Psycho-social evaluation) is conducted during the first pregnancy control to detect any risk factors. Risk factors are, for example, depression or gender violence. The time of the control was increased from 20 minutes to 40 minutes. The EPsA is re-applied during the third gestation trimester.

[28] These women get access to personalized social services through the municipality network ChCC.

[29] The Gestation Guide contains information about the pregnancy, birth-giving, labor rights, and other practical advice.

Health

In 2007, the government of Chile introduced evaluation tools that aim to detect risk factors for the development of children under four.[30] Similarly, ChCC introduced the evaluation of psycho-motor deficits.[31] As part of ChCC the attention of children in hospitals was revisited, and a concept introduced that tried to minimize the stress experienced by children hospitalized during early childhood. This involved, among others, the introduction of a special technical orientation of medical staff.

Parental Education

ChCC offers several other group education programs targeted at caregivers, addressing topics, such as a child's socio-psychological stimulation, educational child-rearing guidelines, and more. It also introduced a variety of workshops targeting the most vulnerable population of Chile. Moreover, ChCC diffuses information as well as materials on child-care for free. These are available through the web portal of ChCC [32], a telephone line through which it is possible to clarify doubts, a radio program, a TV program, educational booklets, TV campaigns, and a manual of best practices. The goal of these components of ChCC is to create easy access to experts and create informational materials about early child development. In 2008, a special musical program was introduced directed at children between zero and five years old.

In 2009, the program Nadie es perfecto (Nobody is perfect) was introduced. Nadie es perfecto is a workshop series, which consists of six to eight sessions directed at all caretakers. The program was inspired by a similar program in place in Canada. It also involved in-kind transfers directed at the cognitive stimulation of children between zero and five years old.

Early Childhood Education

Another set of actions forming part of ChCC are the ones addressing early childhood education. These policies aim to achieve equal access to early childhood education through increasing its coverage and quality. The goal was to create 70,000 new places in nurseries and 43,000 new places in kindergardens between 2006 and 2010 through the INTEGRA foundation as well as JUNJI. Moreover, there was an increase in opening hours. Early childhood education facilities also increasingly open during holidays. They also offer parental education, and a special educational program for rural children.

[30] The risk assessment includes the detection of neurological problems and maternal depression.

[31] The evaluation is conducted through the EEDP (Scale of Psycho-motor Development Evaluation), as well as the TEPSI (Test of Psycho-motor Development).

[32] www.crececontigo.cl

The number of early childhood education facilities with increased opening hours increased from 484 in 2006 to 655 in 2009. Moreover, the number of facilities opening exclusively during the summer holidays augmented from 82 in 2006 to 102 in 2009. Also, ChCC introduced a mobile kindergarden, which reached 187 children in 2009. From 2005 to 2010 JUNJI increased the number of daycare centers by 505 % (from 539 to 3,259) and of kindergartens by 92 % (from 46,990 spots to 85,690 spots). JUNJI also introduced a new educational program into its facilities.

Special Services for the Most Vulnerable Children

ChCC comprises special services and benefits offered to children forming part of the vulnerable population in Chile. Health officials develop a personalized action plan addressing deficits and risks detected in the thorough evaluation plan. These health action plans consist of a set of psycho-social actionable items targeting both children and their families.[33] In addition to that ChCC grants special social protection services to families characterized by some form of vulnerability.[34] These special services are the inclusion of pregnant women living in vulnerability into the the Subsidio Único Familiar (Unified family subsidy). The program also offers them prioritized access to social services offered by the public sector.

Services Targeting the General Citizenship

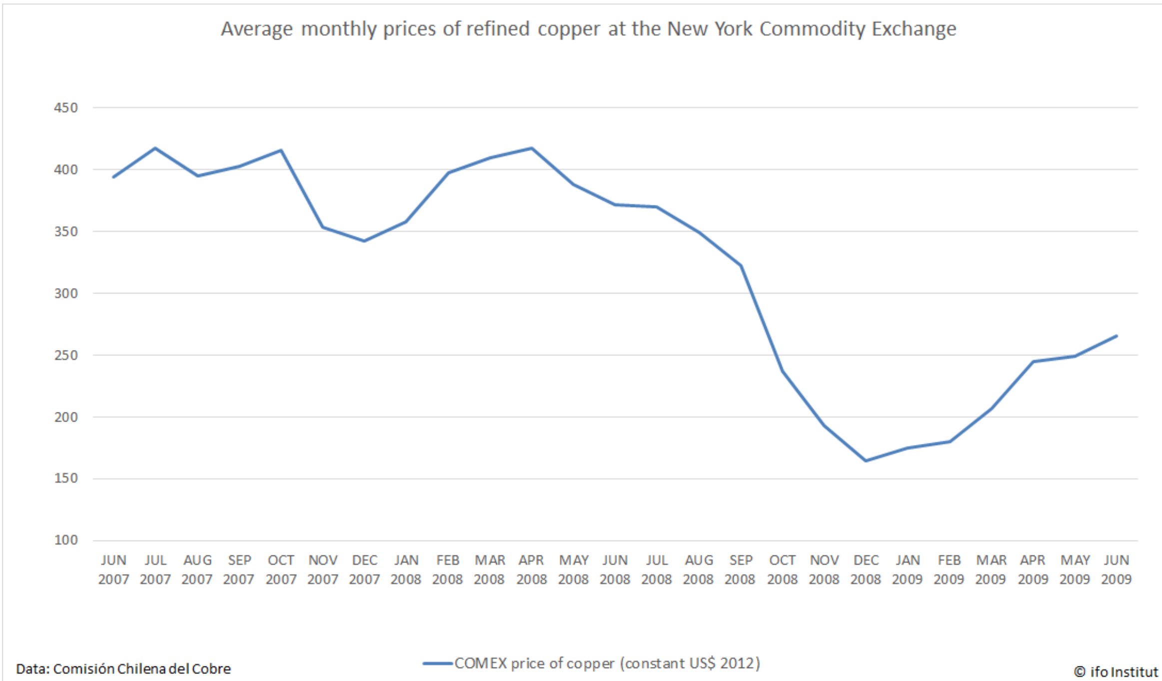
One of ChCC's main goals was to raise the general awareness about the importance of investments in early childhood development. It therefore introduced a website, a free hotline and a radio program targeting the overall Chilean population.

[33]These actionable items consist of home visits, group educational programs, local stimulation centers, playrooms, among others.

[34]From 2007 to 2009 these were directed to the 40 % most vulnerable, in 2010 to the 50 %, and in 2011 to the 60 % most vulnerable.

Appendix C - Copper Prices

Figure C1: Evolution of monthly copper prices (2007-2009)



Note: The figure plots the evolution of copper prices on a monthly basis for the period between June 2007 to June 2009.

Appendix D - Additional Tables

Placebo Cutoff

The tables present the local RD estimators around the placebo cutoff. I take the sixth month prior to the actual roll-out of ChCC as the placebo cutoff and estimate the impact of the program in the twenty windows around the actual date on which the program was implemented. I start by estimating a non-parametric specification of the local randomization approach and then add polynomial orders of one and two. The results show that the local RD estimator is significant when abstracting from polynomial orders and insignificant when introducing polynomial orders of one or two. This indicates that the true underlying relationship between the participation in ChCC and the outcome variables of interest might be best approximated by a functional form that considers polynomial orders of one or two.

I repeat the analysis using empirical methods to choose the optimal window length. In this case, the optimal window length is the minimum window. The tables below show the

Table D1: Local RD effect of ChCC at placebo cutoff (p=0)

0	Variable	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Standardized math score	-1.31922	0	68891	290511	Yes
2	Standardized reading score	1.007589	0	68891	290511	Yes
3	Grade point averages	0.011596	0.005	68891	290511	Yes

Note: The table shows the local RD effect of ChCC using the date six months prior to the actual roll-out of ChCC as a placebo cutoff. The cutoff window under consideration is twenty. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a polynomial order of zero. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D2: Local RD effect of ChCC at placebo cutoff (p=1)

0	Variable	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Standardized math score	-1.011088	0.034	68891	290511	Yes
2	Standardized reading score	-0.831683	0.112	68891	290511	Yes
3	Grade point averages	-0.0094	0.334	68891	290511	Yes

Note: The table shows the local RD effect of ChCC using the date six months prior to the actual roll-out of ChCC as a placebo cutoff. The cutoff window under consideration is twenty. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a polynomial order of one. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D3: Local RD effect of ChCC at placebo cutoff (p=2)

		RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Standardized math score	-1.531196	0.141	68891	290511	Yes
2	Standardized reading score	-1.24044	0.277	68891	290511	Yes
3	Grade point averages	0.008892	0.677	68891	290511	Yes

Note: The table shows the local RD effect of ChCC using the date six months prior to the actual roll-out of ChCC as a placebo cutoff. The cutoff window under consideration is twenty. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a polynomial order of two. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

results of the placebo regression in the minimum window around the placebo cutoff. Similarly to the results in the larger window, the coefficients are significant when abstracting from polynomial orders (but not in the case of grade point averages). When estimating parametric versions of the local randomization approach, the placebo coefficients are insignificant in the optimal window.

Table D4: Local RD effect of ChCC at placebo cutoff (p=0)

0	Variable	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Standardized math score	-1.797613	0	17562	34554	Yes
2	Standardized reading score	-1.560719	0.001	17562	34554	Yes
3	Grade point averages	-0.006066	0.503	17562	34554	Yes

Note: The table shows the local RD effect of ChCC using the date six months prior to the actual roll-out of ChCC as a placebo cutoff. The cutoff window under consideration is the minimum window. This means that the estimation considers all students born one month before and after the roll-out of ChCC as well as those born during the roll-out. I assume a polynomial order of zero. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D5: Local RD effect of ChCC at placebo cutoff (p=1)

0	NaN	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Standardized math score	0.302142	0.735	17562	34554	Yes
2	Standardized reading score	0.180881	0.644	17562	34554	Yes
3	Grade point averages	0.008286	0.854	17562	34554	Yes

Note: The table shows the local RD effect of ChCC using the date six months prior to the actual roll-out of ChCC as a placebo cutoff. The cutoff window under consideration is the minimum window. This means that the estimation considers all students born one month before and after the roll-out of ChCC as well as those born during the roll-out. I assume a polynomial order of one. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D6: Local RD effect of ChCC at placebo cutoff (p=2)

0	Variable	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Standardized math score	0.302142	0.644	17562	34554	Yes
2	Standardized reading score	0.180881	0.854	17562	34554	Yes
3	Grade point averages	0.008286	0.735	17562	34554	Yes

Note: The table shows the local RD effect of ChCC using the date six months prior to the actual roll-out of ChCC as a placebo cutoff. The cutoff window under consideration is the minimum window. This means that the estimation considers all students born one month before and after the roll-out of ChCC as well as those born during the roll-out. I assume a polynomial order of two. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Fuzzy Regression Discontinuity Design

There is evidence showing that, while ChCC was intended as a universal program, the compliance rate is not equal to one. I assume that the actual participation rate in ChCC is 80 percent, and randomly assign 80 percent of the children in my sample a treatment status. The remaining 20 percent are non-compliers. I assume a cutoff window of twenty and sequentially increase the polynomial order of the local RD approach from zero to two. The tables below show that the fuzzy RD estimator is significant when abstracting from any polynomial orders, but becomes insignificant when adding a polynomial order of one or two. Given that the model specification with two polynomial orders is most likely the most appropriate model specification, the evidence hints towards important program inefficiencies when allowing for non-compliance.

Table D7: Local RD effect of ChCC using a fuzzy design (p=0)

0		RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Standardized math score	-0.033662	0	17203	34836	Yes
2	Standardized reading score	228.4618	0	17203	34836	Yes
3	Grade point averages	7.946796	0	17203	34836	Yes

Note: The table shows the local RD effect of ChCC using a fuzzy RD design in order to account for imperfect take-up of the program. I assume that 20 percent of students are non-compliers. The cutoff window under consideration is 10. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a polynomial order of zero. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

I repeat this analysis in the optimal number of windows. In case of the baseline local RD approach, I find insignificant effects (see Table D10). When assuming a polynomial order of degree one or two, the window-selection procedure identifies the minimum window as the optimal window length. It is not possible to estimate the programs impact in the minimum

Table D8: Local RD effect of ChCC using a fuzzy design (p=1)

0	Variable	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Standardized math score	-1624.751034	0.473	172695	186707	Yes
2	Standardized reading score	-1999.957546	0.473	172695	186707	Yes
3	Grade point averages	-21.905442	0.475	172695	186707	Yes

Note: The table shows the local RD effect of ChCC using a fuzzy RD design in order to account for imperfect take-up of the program. I assume that 20 percent of students are non-compliers. The cutoff window under consideration is 10. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a polynomial order of one. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D9: Local RD effect of ChCC using a fuzzy design (p=2)

0	Variable	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Standardized math score	152.94412	0.678	172695	186707	Yes
2	Standardized reading score	419.487799	0.616	172695	186707	Yes
3	Grade point averages	14.095192	0.605	172695	186707	Yes

Note: The table shows the local RD effect of ChCC using a fuzzy RD design in order to account for imperfect take-up of the program. I assume that 20 percent of students are non-compliers. The cutoff window under consideration is 10. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a polynomial order of two. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

window and assuming polynomial orders as the equation is not identified. Therefore, I only show the results for the baseline model specification in the optimal cutoff window.

Table D10: Local RD effect of ChCC using a fuzzy design and optimal number of windows (p=0)

0	Variable	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Standardized math score	-0.033662	1	17203	34836	Yes
2	Standardized reading score	228.4618	0.419	17203	34836	Yes
3	Grade point averages	7.946796	0.374	17203	34836	Yes

Note: The table shows the local RD effect of ChCC using a fuzzy RD design in order to account for imperfect take-up of the program. I assume that 20 percent of students are non-compliers. The cutoff window is the optimal number of windows, in this case the minimum window. This means that the estimation considers all students born one month before and after the roll-out of ChCC as well as those born during the roll-out. I assume a polynomial order of zero. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Early versus Late Roll-out Group

The tables below present the results of the local RD estimation in the larger window, namely a window length of twenty periods. I subsequently increasing the parametric order from zero to two.

Table D11: Local RD effect of ChCC in the twenty periods around the cutoff on schooling outcomes - Early versus late roll-out group (p=0)

		Early	Late
0	Panel 1: Standardized Math Scores		
1	RD Estimate	0.246616	3.204760
2	P-Value	0.29	0
3	Panel 2: Standardized reading scores		
4	RD Estimate	2.929805	3.204760
5	P-Value	0	0
6	Panel 3: Grade point averages		
7	RD Estimate	0.022088	0.038562
8	P-Value	0	0
9	No. of observations (left)	79459	93236
10	No. of observations (right)	89194	97513

Note: The table shows the local RD effect of ChCC in the twenty months around the cutoff window for the late and early roll-out group. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D12: Local RD effect of ChCC in the twenty periods around the cutoff on schooling outcomes - Early versus late roll-out group (p=1)

		Early	Late
0	Panel 1: Standardized Math Scores		
1	RD Estimate	0.717719	7.220307
2	P-Value	0.126	0
3	Panel 2: Standardized reading scores		
4	RD Estimate	0.167004	7.220307
5	P-Value	0.749	0
6	Panel 3: Grade point averages		
7	RD Estimate	0.005663	0.075630
8	P-Value	0.566	0
9	No. of observations (left)	79459	93236
10	No. of observations (right)	89194	97513

Note: The table shows the local RD effect of ChCC in the twenty months around the cutoff window for the late and early roll-out group, assuming a parametric order of degree one. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D13: Local RD effect of ChCC in the twenty periods around the cutoff on schooling outcomes - Early versus late roll-out group (p=2)

		Early	Late
0	Panel 1: Standardized Math Scores		
1	RD Estimate	-4.151552	6.092534
2	P-Value	0	0
3	Panel 2: Standardized reading scores		
4	RD Estimate	-5.124929	6.092534
5	P-Value	0	0
6	Panel 3: Grade point averages		
7	RD Estimate	-0.030289	0.084468
8	P-Value	0.054000	0
9	No. of observations (left)	79459	93236
10	No. of observations (right)	89194	97513

Note: The table shows the local RD effect of ChCC in the twenty months around the cutoff window for the late and early roll-out group, assuming a parametric order of degree two. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Parametric Estimation of Heterogeneity Analysis

The below table shows the local randomization approach of estimating the impact of ChCC on schooling outcomes by subgroups in the twenty periods around the cutoff. The findings confirm the results in the optimal window around the cutoff.

Table D14: Local RD effect of ChCC in the twenty periods around the cutoff on schooling outcomes by groups

	Group	Boys	Girls	Vulnerability	Non-vulnerability
0	Panel 1: Standardized math scores				
1	RD Estimate	1.054608	-0.27404	-0.023176	1.43459
2	P-Value	0	0.218	0.902	0
3	Panel 2: Standardized reading scores				
4	RD Estimate	3.596665	2.237178	2.437405	4.54527
5	P-Value	0	0	0	0
6	Panel 3: Grade point averages				
7	RD Estimate	0.04	0.02	0.03	0.04
8	P-Value	0	0	0	0
9	N (left)	86562	86133	125975	46720
10	N (right)	92026	94681	137991	48716

Note: The table shows the local RD effect of ChCC in the twenty months around the cutoff window. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

The below tables show the parametric estimation of participation in ChCC in the larger window, using twenty periods around the cutoff. The results confirm the findings from the non-parametric estimation.

Table D15: Local RD effect of ChCC in 10 periods around cutoff window on standardized math scores by groups (p=1)

0	NaN	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Boys	3.087073	0	86562	92026	Yes
2	Girls	3.241779	0	86133	94681	Yes
3	Vulnerability	2.898913	0	125975	137991	Yes
4	Non-vulnerability	3.915286	0	46720	48716	Yes

Note: The table shows the local RD effect of ChCC in the ten months around the cutoff window. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a local polynomial order of degree one. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D16: Local RD effect of ChCC in 10 periods around cutoff window on standardized reading scores by groups (p=1)

0	NaN	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Boys	3.927107	0	86562	92026	Yes
2	Girls	3.797787	0	86133	94681	Yes
3	Vulnerability	3.498093	0	125975	137991	Yes
4	Non-vulnerability	5.023019	0	46720	48716	Yes

Note: The table shows the local RD effect of ChCC in the ten months around the cutoff window. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a local polynomial order of degree one. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D17: Local RD effect of ChCC in 10 periods around cutoff window on grade point averages by groups (p=1)

0	NaN	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Boys	0.04153	0	86562	92026	Yes
2	Girls	0.042805	0	86133	94681	Yes
3	Vulnerability	0.038518	0	125975	137991	Yes
4	Non-vulnerability	0.05418	0	46720	48716	Yes

Note: The table shows the local RD effect of ChCC in the ten months around the cutoff window. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a local polynomial order of degree one. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D18: Local RD effect of ChCC in 10 periods around cutoff window on standardized math scores by groups (p=2)

0	NaN	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Boys	0.260998	0.722	86562	92026	Yes
2	Girls	0.432892	0.539	86133	94681	Yes
3	Vulnerability	0.262611	0.662	125975	137991	Yes
4	Non-vulnerability	0.566026	0.513	46720	48716	Yes

Note: The table shows the local RD effect of ChCC in the ten months around the cutoff window. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a local polynomial order of degree two. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D19: Local RD effect of ChCC in 10 periods around cutoff window on standardized reading scores by groups (p=2)

0	NaN	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Boys	1.388329	0.096	86562	92026	Yes
2	Girls	0.497463	0.513	86133	94681	Yes
3	Vulnerability	0.424863	0.522	125975	137991	Yes
4	Non-vulnerability	2.385988	0.028	46720	48716	Yes

Note: The table shows the local RD effect of ChCC in the ten months around the cutoff window. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a local polynomial order of degree two. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Table D20: Local RD effect of ChCC in 10 periods around cutoff window on grade point averages by groups (p=2)

0	NaN	RD Estimate	P-Value	N (left)	N (right)	Covariates
1	Boys	0.041352	0.006	86562	92026	Yes
2	Girls	0.02198	0.138	86133	94681	Yes
3	Vulnerability	0.017704	0.157	125975	137991	Yes
4	Non-vulnerability	0.069801	0.001	46720	48716	Yes

Note: The table shows the local RD effect of ChCC in the ten months around the cutoff window. This means that the estimation considers all students born ten months before and after the roll-out of ChCC as well as those born during the roll-out. I assume a local polynomial order of degree two. Source: SIMCE (2015-2018) and MINEDUC (2015-2018). For details on the estimation procedure see Cattaneo, Titiunik, and Vazquez-Bare (2016).

Summary Statistics of ELPI Outcomes

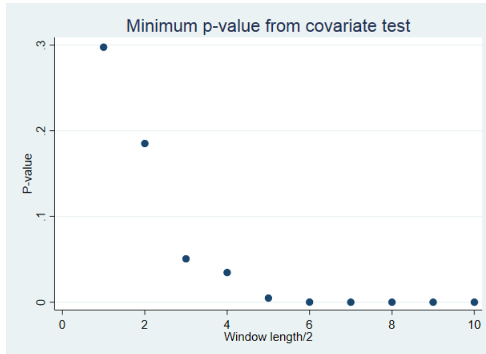
Table D21: Summary statistic of ELPI Sample (2010-2017)

TEPSI	54.01
	(12.28)
TVIP	103.1
	(16.87)
TADI	51.75
	(9.007)
Applied problem-solving	50.32
	(10.00)
Mathematical literacy	50.24
	(9.991)
Calculation	50.14
	(9.959)
BDS	45.70
	(8.017)
HTKS	49.55
	(10.44)
CBCL1	56.84
	(11.38)
Abnormal weight	0.161
	(0.368)
Abnormal height (ECD)	0.251
	(0.434)
Abnormal head circ.	0.193
	(0.395)
(PSI (Int.))	43.07
	(34.64)
PSCS	66.48
	(10.12)
CESD	7.139
	(5.413)
HOME	11.19
	(4.553)
Observations	35941

mean coefficients; sd in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure F1: Optimal window selection

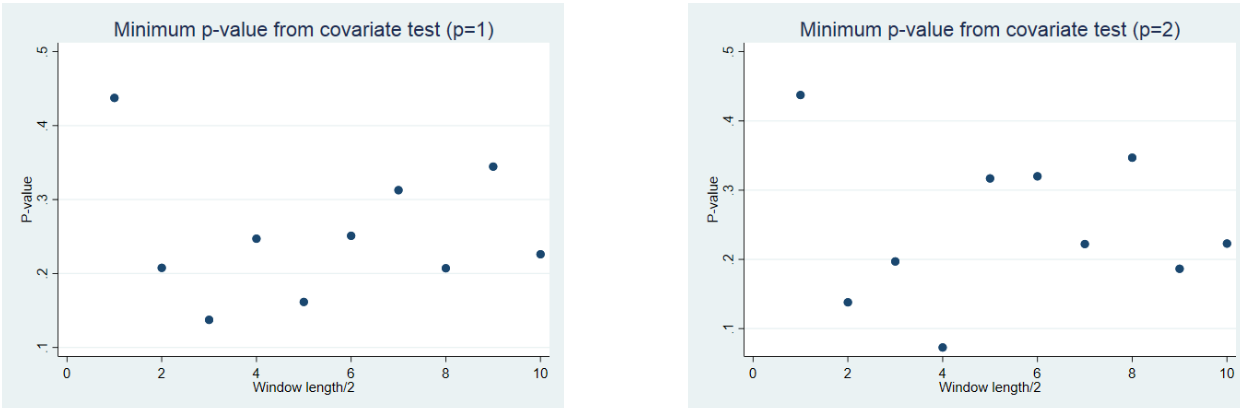


Note: The graph plots the optimal window selection procedure for the local randomization approach. For a detailed overview of the methodology see Cattaneo, Titiunik, and Vazquez-Bare (2016). I include the following three variables for the covariance balance tests: a child's gender, a dummy variable for socioeconomic vulnerability as well as if the child assists a rural or urban school. The covariate balance test uses a large-sample approximation. The optimal window is four. This means that the optimal local randomization approach considers the two birth cohorts before and after ChCC's rollout.

Appendix F - Additional Figures

Optimal Window Selection

Figure F2: Optimal window selection under a polynomial order of 1 and 2



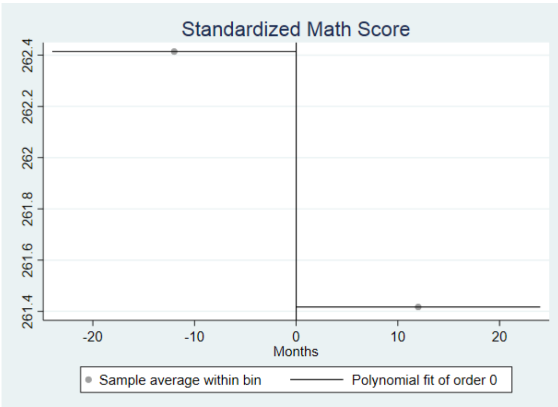
Note: The graph shows the optimal window selection for the local randomization approach. For a detailed overview of the methodology see Cattaneo, Titiunik, and Vazquez-Bare (2016). I include the following three variables for the covariance balance tests: a child's gender, a dummy variable for socioeconomic vulnerability as well as if the child assists a rural or urban school. The covariate balance test uses a large-sample approximation. The left panel shows the results under an estimation assuming a polynomial order of one. The optimal window is four. This means that the optimal local randomization approach considers the two birth cohorts before and after ChCC's roll-out. The right panel shows the results for an estimation assuming a polynomial order of two. The optimal window is the minimum window. This means that the optimal local randomization approach should consider the birth cohort before and after ChCC's roll-out.

Regression Discontinuity Plots

The figures below show the RD plots of a local randomization approach considering four windows around the cutoff. The cutoff is zero and refers to the date on which ChCC was rolled out.

The graphs below show the RD plots for a local randomization approach considering twenty windows and a polynomial order of one degree.

Figure F3: RD plots for standardized test score in the optimal window



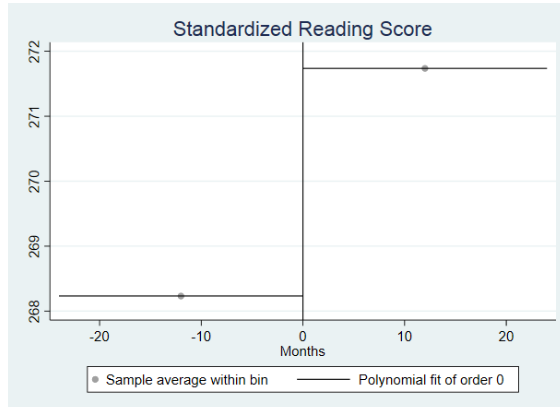


Figure F4: RD plot for grade point averages in the optimal cutoff window

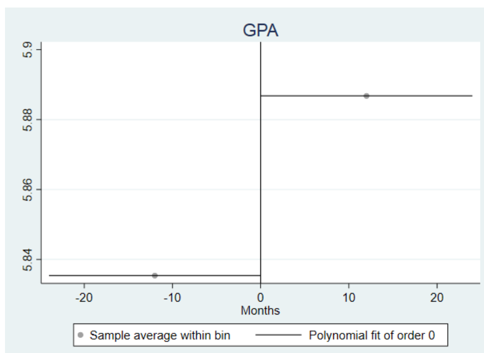
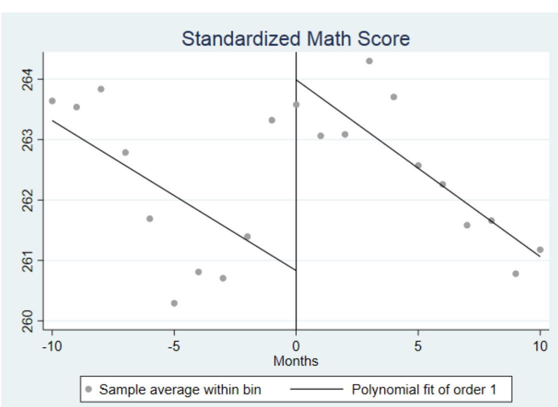


Figure F5: RD plots for standardized test score of polynomial order 1



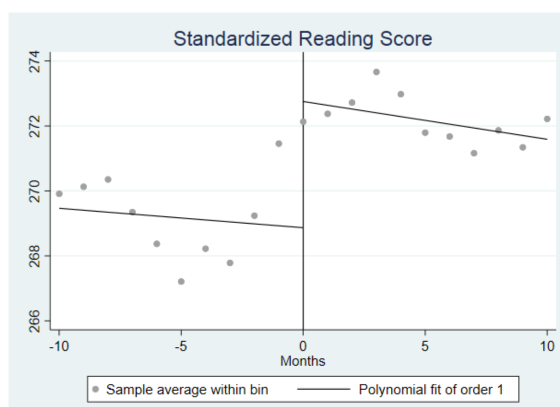
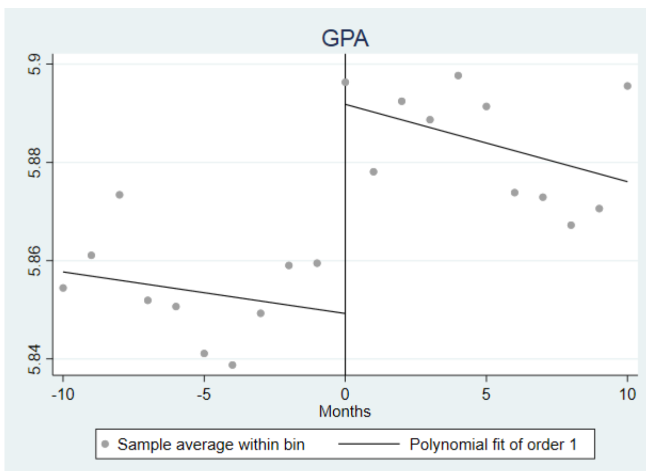


Figure F6: RD plot for grade point averages of polynomial order 1



Appendix G - Detailed Cost-Benefit Analysis

To calculate the program's costs per participant I take advantage of data provided by the government of Chile. I have data on the program's costs per component per year as well as on the number of units benefiting from each component. Depending on the program component these units are children, pregnant women, municipalities or newborns. I restrict my period to the years 2007 to 2017, as the program's target group was expanded in 2017. The total costs of the program for all units amount to 236,472.2 US-Dollar for the period 2007 to 2017. I then calculate the average unit cost per year and convert these values to US-Dollars using data on exchange rates published by the OECD for each year. I then sum up the costs for each respective year from 2007 to 2017. Next, I divide the sum by the number of years. This gives me the average unit cost per year for the period 2007 to 2017. The average unit cost per year is 23,647.2 US-Dollar.

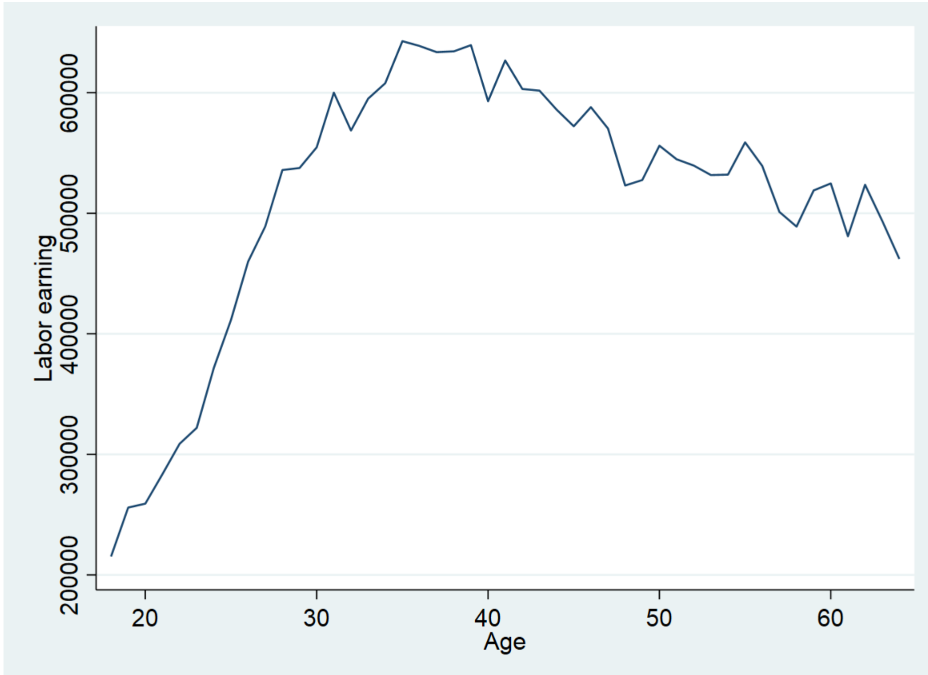
To calculate the marginal willingness to pay for ChCC by program participant I calculate the present value of lifetime earnings in Chile. I take advantage of data published by the Ministry of Social Protection, namely the socioeconomic survey (CASEN) from 2017.

I first calculate the mean labor income of all individuals between 18 and 65 years old in 2017. The results are shown in figure G1. I assume that this distribution is representative for the average lifetime earning distribution in Chile. I then calculate the Present Value of this earning stream in Excel. I assume a discount rate of 3 %. I then convert the values to 2017-US-Dollar, taking the average exchange rate for 2017 from data on exchange rates published by the OECD. This results in an average present value of lifetime earnings of 220,312.4 US-Dollar.

Next, I equally distribute the average per year program unit cost of 23,647.2 US-Dollar across a typical lifetime of an individual. I then calculate the present value of this cost stream, which is 12,864.5 US-Dollar. This is 6 % of the average present value of lifetime earning in Chile. Consequently, participants would need to increase their earnings by 6 % over the course of the life to meet the program costs of ChCC.

Work by French et al. (2015) shows that a 1 % increase in the GPA leads to an average increase of around 12 to 14 % in earnings. The average GPA in my sample is 5.8 (see table 1). Based on the different model specifications investigated in this paper, the average impact of ChCC on grade point averages is approximately 0.3. This is an increase of 0.5 % over the mean grade point average. The equivalent increase in income would therefore be approximately 7.5 %. From this information I create the post-program average income flow, adding 7.5 % to the average income per age year. I then take the net present value of this post-program income flow. A 7.5 % increase in the lifetime earning leads to a difference in the

Figure G1: Average lifetime earnings in Chile (2017 in CL)



Source: Ministerio de Desarrollo Social y Familia (2017)

present value of lifetime earnings between the pre- and post-program world of 16,523.4 US-Dollar per participant and an additional present value of tax revenues of 1,156.6 US-Dollar per participant. The average income tax in Chile was 7.0 % in 2019[35].

The marginal value of public funds is equal to the ratio of participants' marginal willingness to pay for the program and the initial program costs (costs minus fiscal externalities). I therefore divide the difference in the pre- and post-program NPLE by the difference between the costs per participant and the fiscal revenue generated through the program. The MVPF is then 1.41 per participant.

[35] `https://www.oecd.org/tax/tax-policy/taxing-wages-chile.pdf`