UNIVERSITY OF SUSSEX

The contribution of *Bolsa Família* to the educational achievement of economically disadvantaged children in Brazil

Armando Amorim Simões

Thesis submitted to the University of Sussex for the degree of Doctor of Philosophy

May 2012

Declaration

I certify that the thesis I have presented for examination for the PhD degree in Education of the University of Sussex is solely my own work and has not been previously submitted to this or any other University for a degree.

_____

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SUMMARY

This study investigates effects of a conditional cash transfer programme (CCT) in Brazil – *Bolsa Família* (*BF*) – on school outcomes, particularly children's achievements on standardised tests, pass-grade rates, and dropout rates. The educational conditionality of the programme, requiring enrolment in school and minimum school attendance, figures as a major justification for public investment in *BF*. It is expected that *BF* will reduce short-term poverty and boost children's human capital, thus inducing long-term socioeconomic improvement. In order to achieve its long-term objective, *BF* should be able to improve not only enrolment and attendance rates, but also learning outcomes and grade promotion amongst beneficiary children. However, these effects, particularly learning outcomes, have not yet been reported in the literature.

The hypothesis investigated in this thesis is that length of time of participation in the programme and per capita cash amounts received by families are key variables in assessing *BF*'s effects on children's educational outcomes. As the programme improves household income, requires a high rate of school attendance, and monitors children's health and nutritional conditions, a positive effect on children's performance should be expected over time. Similarly, the amount of cash paid to families should have an impact on changes induced in the home environment that are beneficial for children's educational outcomes.

Empirically, the dissertation combines three national datasets from governmental agencies for the years 2005 and 2007. These data contain information on test scores in Portuguese Language and Mathematics for fourth grade pupils, school context, and *BF* parameters (intake, time of participation, and cash value), which are used in cross-sectional and panel analyses to test the above hypotheses.

The results show that although beneficiaries tend to attend less well-resourced schools, the influence of individual and household characteristics on test scores overshadow that of school resources, suggesting that demand-side interventions might result in gains in children's performances. The cross-sectional analysis at the school level suggests that *BF*'s contribution to school outcomes depends on the length of time of participation and the per capita cash value paid to families. In addition, these two *BF* parameters have *substitute effects*, that is, as the per capita cash increases, school performance increases; however, the contribution of time of participation to gains in school performance diminishes and *vice-versa*. As a sensitive analysis to test the direct effects of length of time of participation and per capita cash on school outcomes, a subsample was used, which includes only schools in which more than 80% of pupils are beneficiaries. Results from this subsample confirm the positive effects of time and cash on school outcomes, although only cash is statistically significant. Furthermore, a school-and-time fixed effects model is estimated using panel data for 2005 and 2007 for the same school outcomes. The results also suggest that improvements in school outcomes are expected over time as a result of exposure to the programme, although this varies across regions.

The findings support the idea that improvements in educational opportunities and outcomes for children of low-income families in Brazil require a non-educational policy measure – the reduction of the immediate income poverty – as intended by *BF*. Nevertheless, there is also an urgent need to address inequalities in standards of education supply and special attention should be given to children whose families are recipients of *BF* in promoting access to pre-school programmes. Even though educational policies are necessary, they are insufficient to promote human capital amongst the poorest families in Brazil. In this sense, CCTs do not represent an opportunity cost for educational policies. Instead, they are important allies in promoting education access and equity.

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Chapter 1. Introduction

1.1 Context and Motivation

Amongst the universal basic human rights, education has been recognised as a pathway to broaden freedoms and to empower individuals and societies; as a main vehicle to promote culture, knowledge, and social values; and as a strategic provider of benefits to other dimensions of social and human development. Education has also been captured as a key element within the machinery of capitalism to produce functioning citizens for the productive system and to promote prosperity and wealth amongst individuals, families, and society. Simultaneously and contradictorily, education can reinforce social exclusion, marginalisation, and segregation amongst individuals and social groups by means of its social and institutional organisation, including its system of provision and delivery through schools and school systems. Particularly for socioeconomically disadvantaged groups, failure in providing equality of educational opportunities can compromise children's expectations for a future free of poverty and deprivation. Failures in policy equity can occur not only through deficiencies in coverage or quality of educational services delivered, but also by the initial socioeconomic conditions of children. Initial conditions render individuals unequal in their ability to participate in public policies and to convert public services into real benefits. This makes policies focused on social disadvantage important in achieving the basic universal right to education.

The fundamental contradiction raised above derives from the fact that education can reproduce social stratification through several institutional and social mechanisms. Education is also a powerful means through which children from disadvantaged families are expected to overcome initial social inequalities and access better opportunities in life. In order to achieve the latter, social policies and education systems must act to counterbalance the initial inequalities children bring to school. Creating educational opportunities is not enough. It is essential to know how social conditions interact to produce children's educational outcomes and, thus, which policies and programmes are most effective in achieving greater educational equality.

The general focus of this study is the relationship between family income and educational outcomes, and the role that *Bolsa Família* (*BF*) – a conditional cash transfer (CCT) programme for education, nutrition, and health care in Brazil – can play in developing a more

conducive environment for children's educational outcomes, and thus to contribute to low-income children's education. In particular, this study investigates whether *BF* has any effect on children's learning outcomes, measured through achievement in standardised test scores, and whether length of time of participation in the programme and the value of per capita cash transfers to families are influential in the educational outcomes of beneficiary children.

The motivation for this research is interwoven with my work experience over the last 20 years as public policies manager in the fields of education, planning, and evaluation both at local and national levels in Brazil. At the time the first local CCTs were being implemented in Brazil (1995), I worked as planning director in the Secretariat of Education in Angra dos Reis municipality (state of Rio de Janeiro) during the second Workers' Party administration in the town. In 1998, as planning advisor in the Mayor's Cabinet, I started studying the different experiences of CCTs implemented across the country (which at that time were conditional on education only) as to their objectives, design, and strategy of implementation. In 1999, as Secretary of Education, I started the political process of creating the municipal Bolsa Escola programme in Angra dos Reis, which became a municipal law and was implemented in 2000/2001.

In 2001 I was based in Brasília. This was the year that the Federal Government launched the National Bolsa Escola programme, covering more than three thousand municipalities in its first year of implementation. Between 2001 and 2002 I worked as a coordinator in the newly created National Secretariat of the Bolsa Escola Programme in the Ministry of Education, where I framed the programme's evaluation plan and a proposal for a school attendance system that would allow control of the conditionalities attached to Bolsa Escola. In 2002 I pursued a Master's Degree at the London School of Economics in the UK, where I studied the limitations of the monetary approach to defining the operational concept of poverty used in the federal Bolsa Escola programme to identify and select beneficiaries. Back in Brazil, in 2004, while working as director of educational projects in the Ministry of Education, I maintained close cooperation with colleagues who started working in the Ministry of Social Development on the new flagship programme in Luiz Inácio Lula da Silva's government – *Bolsa Família*. In 2007 I worked in the Ministry of Planning as director of the Multi-year Plan, when the challenges of evaluating large-scale government programmes became clear to me. The plan of retreating again for a period of study, in which I could engage in the evaluation of a large-scale programme, was made possible in 2008; a natural candidate was the *BF* programme.

By the time I left for my doctoral studies at the University of Sussex (UK), *BF* had already reached full coverage and was the most popular social programme in Brazil, gaining further momentum in the second term of Lula's presidency. At the same time, the Ministry of Education had already conducted two national rounds of the new School Performance National Assessment (Prova Brasil 2005 and 2007). These new achievement indicators at the school level could be used to assess the potential contributions of *BF* to beneficiaries' learning outcomes. Despite the success of CCTs in increasing enrolment and attendance rates for children of low-income families, the efficacy of CCTs in promoting long-term poverty reduction by keeping children in school was under attack.

A paper commissioned by UNESCO triggered my interest in the subject. Entitled "Where is the 'education' in conditional cash transfers in education?" (Reimers, Silva and Trevino, 2006), the paper unleashed fierce criticism of CCTs based on the lack of evidence that these programmes could, in fact, have an impact on the learning outcomes, promotion, and completion rates of beneficiaries. Given the impoverished conditions of schools generally attended by beneficiaries, the authors argue that CCTs represent a double opportunity cost in terms of education policy. First, CCTs use proportionally high shares of the national education budget in many countries, diverting resources that could be applied to better educational opportunities for socioeconomically disadvantaged children. Second, governments move away from necessary educational reforms and justify investment in human capital by investing in cash transfers conditional on education — a policy that is both easy to implement and that is electorally attractive. In my view, the argument is valid but it misses the point that poverty has an impact on the possibility of education for disadvantaged children. This is not only or necessarily due to the lack of provision of quality schools, but is because of the impact poverty has on the capacity of children to participate in education policies and to convert educational services into real benefits. In this sense, educational policies should be complemented by social policies focused on the conditions of low-income households and their capacity to support children's education. CCTs represent an alternative means to achieve that goal.

1.2 Purpose and Rationale

Conditional cash transfer programmes were developed based on the assumption that they could contribute to poverty relief in the short term and promote human capital accumulation in the long term, thus rescuing future generations from the "poverty trap". Due to budget limitations, cash transfers have not always worked as a minimum income policy, but instead as an incentive to change families' behaviour in favour of their children's futures as

long as education, health care, and nutrition are regarded. However, if CCTs have no impact on learning outcomes, grade progression, and completion rates for beneficiary children, then their educational justification beyond a mere short-term incentive for school attendance might be compromised. The strength of political support for CCTs comes mainly from the 'educational promises' the policy makes to families with respect to their children's futures and to society as a whole in fighting structural causes of poverty in developing countries. Recognised impacts on short-term poverty alleviation, although significant, cannot sustain changes in the long term so that children can achieve a future different from that of their parents. CCT programmes are expected to interfere with family dynamics by changing behaviours towards children's time allocation for school and work, and by improving school attendance and children's nutritional and health conditions. Therefore, CCTs should have a significant impact on human capital accumulation in the long term.

Amongst the educational outcomes studied with respect to CCT programmes, learning has so far been the least contemplated, although this outcome is probably the most significant in linking present and future poverty. This research makes its contribution by investigating test score achievement, making use of new datasets available in Brazil, which include students' test scores in Mathematics and Portuguese Language as well as socioeconomic and school variables. This research also benefits from the national coverage of databases collected from government agencies allowing for a nation-wide analysis, thereby enlarging the scope of previous studies of CCT programmes in Brazil. Another positive aspect is the time period this study covers. So far, most studies focused on learning outcomes have analysed CCT programmes in their very early stages, not allowing for the accumulations this kind of policy may require in producing any significant effect on students' learning outcomes. *BF* was initiated in 2003, following its predecessor Bolsa Escola (2001). Two rounds of national examinations (2005 and 2007) are used in this research to assess potential effects on school outcomes. Differences in mean test scores, and pass-grade and dropout rates are analysed across time at the school level, accounting for differences in the length of time of exposure to the programme and to differences in cash amounts paid to families in each school.

1.3 Questions explored and hypotheses investigated

The main research question investigated in this thesis is whether *Bolsa Família* makes any positive contribution to the educational outcomes of economically disadvantaged children, particularly to the achievement in national standardised exams. In investigating that question, several other interesting issues are explored before I delve into the empirical analysis. First,

why should family income influence children's learning? What mechanisms interfere with the educational opportunities and outcomes of children of low-income families? Even if one agrees that income makes a difference, does this mean that by raising the incomes of families, children will benefit from better results at school? Is there any evidence in the literature of the income effect on educational outcomes, or of the impact of anti-poverty or welfare programmes on children's performance at school? These questions interrogate how and why income affects children's outcomes and why poverty potentially undermines educational opportunities, shedding some light on the potential contribution of anti-poverty and welfare programmes to protecting the right to education.

A second set of issues emerges by asking what it is about CCT programmes that link this type of policy to the educational opportunities of low-income children. Is there any educational rationale behind CCT programmes? Why should we expect any effect of CCTs on educational opportunities and outcomes? What has research so far revealed about the significance of these programmes to the educational opportunities and outcomes of children of poor families? Can we expect children to escape from future poverty by taking part in CCT programmes? These questions put CCT programmes in perspective with respect to the long-term objectives claimed by policy-makers that human capital accumulation is a desired and achievable goal of CCTs.

A third set of issues brings us to the Brazilian social and educational context. Why were CCTs such as Bolsa Escola and *Bolsa Família* proposed in Brazil in the first place? How has the recent evolution of access to basic education in Brazil justified these initiatives? How has *BF* evolved, what are its main characteristics, and how does it intend to tackle the intergenerational transmission of poverty? What is the theory behind the programme that can justify such a long-term objective? Is there any research-based evidence of the educational impacts of *BF*? By understanding the programme's theory and how it is designed to create a more conducive environment for children's education in the present, the critical pathways towards children's life chances in the future can be identified. Based on these pathways, hypotheses of *BF*'s potential effects on education can be tested, amongst them, the contribution to learning outcomes. This leads back to the main question raised at the outset of this section and to the core set of empirical questions investigated in this study.

In asking whether *BF* makes any positive contribution to the educational outcomes of economically disadvantaged children, I consider potential effects on achievement in test scores and in pass-grade and dropout rates. In the first part of the analysis I start by examining how beneficiaries and non-beneficiaries differ. What are the conditions of the home

environments in which children live? What kind of support do they have from parents? What expectations do they hold for the future? I also investigate the conditions of their schools and what kinds of experiences they have had in those schools. Finally, I examine how children perform on the national examination and investigate what can explain eventual differences in test score achievements between beneficiaries and non-beneficiaries.

Differences in performance between beneficiaries and non-beneficiaries are reflected in the mean test scores achieved in each school, as well as in other performance indicators such as pass-grade and dropout rates. In the second part of the analysis I look at those differences across schools vis-à-vis the *BF* factors (*BF* intake, length of time of participation, and per capita cash amounts). Do time of exposure to *BF* and per capita cash paid to families in each school positively influence school results and reduce the gap between high-*BF*-intake schools and low-*BF*-intake schools? If yes, can the positive effect on school outcomes be attributed to improvements in beneficiaries' educational outcomes? The main hypotheses I test are that *BF* effects educational outcomes depending on the length of time of exposure to the programme and on the relative cash value paid to families. These factors potentially interact with each other and moderate the effect of *BF* intake in each school, possibly revealing positive effects of *BF* on school outcomes.

Schools also change over time in terms of composition, resources, and outcomes, as well as in terms of *BF* factors. In the third part of the empirical analysis I look at changes that occurred between 2005 and 2007: did school outcomes improve between 2005 and 2007? If yes, can that improvement to some extent be associated with the level of *BF* participation in each school, independent of eventual changes in school resources and composition?

These are the questions at the heart of this thesis for which I offer answers over the forthcoming chapters.

1.4 Overview of the chapters

The remainder of this thesis comprises eight chapters. In chapter two I briefly describe the academic debate concerning the relationship between poverty and education, and how the mutual influences of socioeconomic background and school conditions on children's outcomes have been considered over the last decades. The question of whether and why family income might matter to children's outcomes is considered through the lenses of four theoretical syntheses about that relationship. This is followed by a review of some of the empirical evidence of whether anti-poverty and welfare programmes involving cash transfers

to poor families have any impact on educational outcomes. A series of randomised experiments and longitudinal studies carried out in the US over the last 40 years is the focus of the review. Then I consider how social assistance and educational policies, taken together, has been increasingly recognised as critical for reducing inequality in education and for improving the educational opportunities of economically disadvantaged children. Particularly relevant is the emergence in Latin America (LA) of a new policy initiative conflating those two dimensions – CCT programmes on education, health, and nutrition.

In chapter three I present the broad theoretical landscape of CCTs in LA and the fundamental educational rationale underpinning their long-term objectives, stated in terms of human capital accumulation. I review the literature investigating the impacts of CCTs on educational outcomes, particularly studies concerned with learning outcomes and grade progression. The conspicuous lack of evidence of impacts on learning outcomes – what I call the *missing link* – is put into perspective and is confronted with the evidence explored in chapter two.

In chapter four I briefly describe the social and educational contexts in Brazil, marking the recent progress achieved in reducing poverty and inequality and in promoting access to primary education. I also show how access to education is incomplete in Brazil, as those in the lower quintiles of income do not make it to secondary education, being the most affected by grade repetition and dropout. As a strategy to help children from low-income families to complete basic education, CCT programmes were introduced in Brazil in the 1990s, converging in the current *BF* programme, the main characteristics and educational impacts of which are discussed. The programme theory is explored, making explicit the socio-educational rationale of *BF* and why impacts on learning outcomes and grade progression should be expected as a result of participation in the programme, thereby allowing children to escape the poverty trap in the long run.

In chapter five methodological issues surrounding CCT studies are explored, in part explaining the lack of results regarding impacts on children's learning outcomes in developing countries. I also describe the set of databases collected and used in this research, the core set of research questions, and the modes of analysis undertaken in chapters six to eight.

In chapter six I use 2005 cross-sectional data at the individual level to look at the main characteristics distinguishing *BF* recipient children from their non-recipient counterparts in terms of socioeconomic and school factors. Then I analyse the achievement gap in fourth grade test scores (in Mathematics and Portuguese Language) between beneficiaries and non-

beneficiaries, and how it increases with a proxy variable to family income. I finally analyse the extent to which the characteristics of students, households, and schools explain the achievement gap between beneficiaries and non-beneficiaries. I argue that the prominence of the first two sets of variables in explaining the gap suggests that social intervention supporting children and families, such as *BF*, might be more relevant than "pure" educational policies to reducing inequality in educational outcomes.

In chapter seven I use 2007 cross-sectional data at the school level to test the central hypotheses of this thesis: that length of time of participation in *BF* and per capita cash transfer amounts are two key factors influencing learning outcomes. I investigate school-level differences in tests scores in Portuguese Language and Mathematics, as well as pass-grade and dropout rates of fourth grade students in 2007, according to three *BF* factors: level of *BF* intake, mean time of participation, and mean per capita cash amounts paid to families in each school. An interactive model is estimated using multiple regression analysis, in which the marginal effect of *BF* intake on school outcomes is found to be moderated by time and cash, both factors being significant predictors of school outcomes.

In chapter eight I take a step forward in modelling and controlling for school characteristics that might interfere with the estimated effects of participation in *BF* on school outcomes. I use two-year panel data (2005 and 2007) to estimate a school-and-time fixed effects model and to test the hypothesis of a positive change in school performance associated with the level of *BF* intake in each school. I also investigate how school resources changed between 2005 and 2007 according to *BF* intake distribution across schools.

Finally, chapter nine summarises the main findings in this thesis and indicates policy implications and issues for further investigation.

Chapter 2. Education, poverty, and inequality

2.1 Introduction

This chapter introduces the academic debate concerning the relationship amongst education, poverty, and inequality, and how the mutual influences of social and school factors have been considered over the last decades. I consider why family income matters to children's outcomes by exploring four theoretical models: (i) the archetypal economic model ('investment theory'); (ii) Mayer's heuristic income model ('investment' + 'good parenting' theory); (iii) Haveman-Wolfe's model ('economic choice theory'), and (iv) the Duncan-Murnane model ('ecological' model). These theoretical approaches address the links between household socioeconomic circumstances and children's outcomes, emphasising different sets of factors driving that relationship. I review significant empirical evidence supporting the relevance of family income and welfare programmes for improving children's educational outcomes. Finally, I consider how social and educational policies, taken together in new integrated policy initiatives, have been increasingly recognised by researchers and policy-makers as critical for reducing inequality in education and poverty in the long term.

2.2 The debate

Education in developing countries is, at different levels, segmented by social group, reflecting some degree of inequality of opportunities and outcomes. Children whose families live in poverty tend, on average, to be educationally marginalised either by total exclusion (no access), by early exclusion (no completion), or by accessing poor quality schools (Aguerrondo, 2000). Given their family backgrounds and the likelihood of attending less well-resourced schools, low-income children also tend to perform worse than their more affluent peers, frequently dropping out before graduation. In a review of the literature concerning the relationship between poverty, inequality, and education in LA, Reimers (2000b) mentions several studies showing evidence of the links between socioeconomic disadvantage and school enrolment, completion rates, school quality, and students' achievement in standardised tests. Reimers (2000b) also highlights that social conditions are so strongly associated with access, attendance, and achievement in education that a Gini coefficient for education can be created that mirrors the Gini coefficient for income. A major question derived from the influence that poverty and inequality have on education is whether schools can be held accountable for

educational outcomes without some attention being paid to what happens within children's households, neighbourhoods, communities, and even within society as a whole with respect to the distribution of economic resources, and the incidence of poverty.

The links made between social disadvantage and education are not new, for the seminal studies by Coleman (1966) and Jencks (1972) in the US had already shown that social background predicts educational outcomes, raising the controversial argument about the limitations of schools in making a difference for socially disadvantaged students. In a subsequent study in the UK, however, Rutter et al. (1979) found that secondary school characteristics could explain a significant proportion of the difference in outcomes amongst disadvantaged students in areas such as attendance and learning outcomes, as well as behaviour and delinquency rates. In the 1980s, scholars started arguing that school and teacher characteristics not only make a difference, but are quite significant for students from poor social backgrounds (Coleman and Hoffer, 1987) and are even more influential than students' socioeconomic backgrounds in developing countries (Schiefelbein and Farrel, 1982; Fuller et al., 1999). More recently, Chenoweth (2007) carefully documents successful school cases[1] in the US in which characteristics such as setting high expectations for students, data-driven instruction, wise use of school time, on-going professional development of teachers, and comprehensive leadership teams made up of principals, teachers, parents, and community members are recognised as common factors underlying "unexpected" results. If some schools can be effective for disadvantaged children, then another question can be raised: have schools and school systems been insensitive to the social context in which they operate? Have schools neglected social differences amongst children, taking a "one size fits all" approach and contributing to the educational disadvantages of poor children?

Inequality in educational outcomes amongst children and schools has stimulated debate about the interaction between children's background and school factors in producing educational outcomes. This debate is at the core of studies looking at explanations for success in so-called "effective schools" (*school effectiveness research*) and at how to improve schools (*school improvement research*). Two main assumptions underpin these studies: that social background influences but does not fully determine educational outcomes and that schools can be effective in teaching children from different social backgrounds. This trend has shifted the focus from the *societal* determination of the educational outcomes, articulated in the early studies by Coleman and Jencks, to the *(school) institutional* determination of educational

1 The cases examined are schools with high-poverty and high-minority student populations.

2 The authors do not propose any policy measure in this direction. Although they recognise that income inequality may be the

success. Intra-school factors that can influence student outcomes, regardless of social background, are still being concerned.

The early expectation that effective school factors would be common to all schools, independent of the socioeconomic settings in which they were operating, was rapidly put in doubt by scholars such as Hallinger and Murphy (1986) and Hanushek (1986). These authors agree that contextual and school factors interact to determine student performance, but go beyond that by asserting that what makes a school effective can differ from social group to social group. Therefore, any significant findings related to the effectiveness of schools for socially disadvantaged children should be seen as bound to a specific context, rather than as universal.

The perception of the existing link between poor educational attainment and poor social background also initiated a fierce debate within the sociology and philosophy of education in the first decades of the 20th century concerning the role education plays in society. One school of thought – *the critical perspective* – maintains that although education is deemed to be potentially beneficial to individuals and society, in its dominant form it is identified as an instrument for social stratification and for the reproduction of the *status quo* (Tawney, 1931; Bernstein, 1970; Illich, 1970; Bourdieu, 1974; Bowles and Gintis, 1976; Bourdieu and Passeron, 1977; Ballantine, 1998; Brint, 1998). This sociological tradition is focused on explaining the mechanisms through which the education system in capitalist societies, by offering different quality and forms of schooling to different social groups, "steers children towards the background they come from" (Collins et al., 2000, p. 135, cited in Moore, 2004). This perspective is the least reflected in policy interventions (Raffo et al., 2007), since it tends to denounce the system as mere machinery for social reproduction.

Another school of thought – *the functionalist perspective* – considers education to be a major instrument in industrialised societies to boost economic growth and to generate prosperity and well-being. As such, education should also be pursued by developing countries in order to overcome poverty and to achieve higher standards of living. Feeding into this tradition, economic studies on the value and returns of education (Schultz, 1961; 1963; Mincer, 1974; Becker, 1993; Mincer, 1993) frame *human capital theory*, which became the most influential theory informing education policy in the early 1960s. Developing nations, supported by international financial institutions, have pursued increasing investments in education and have dramatically struggled to widen access to education for all as a path to industrialisation, modernisation, development, and, consequently, to poverty reduction. The "discovery" of the private and social returns of education in economic terms easily led to the

conclusion that both families and governments should be held accountable for providing access to education to all school-age children. Education is now seen as a pre-condition for human functioning in modern society, as well as for economic and social development. It figures as one of the main policy priorities in modern democracies, and is at the top of the agenda of most developing countries and international cooperation and aid agencies.

The challenge of improving poor children's education is still on the agenda, and many governments have struggled to achieve that goal both in developed and developing countries. The answer to the policy question of how to achieve quality education for economically disadvantaged children probably lies on both sides of the supply–demand equation for the production of education in society. Both academics and policy-makers have started to look in that direction, and the idea that public policies should be integrated in tackling both social disadvantage and educational provision has started to appear in the public debate. The mutual influence of social and educational policies on educational opportunities for disadvantaged children in the UK is stated by Mortimore and Whitty (2000):

> (…) policies which tackle poverty and related aspects of disadvantage at their roots are likely to be more successful than purely educational interventions in influencing overall patterns of educational inequality. Yet if dynamic school improvement strategies can be developed as one aspect of a broader social policy, then they will have an important role to play on behalf of individual schools and their pupils. (Mortimore and Whitty, 2000, p.29)

Similarly, the Secretary of Education of the state of Massachusetts, US, Paul Reville, expresses his views on the failure of education policy in tackling educational gaps in the state system:

> (…) closing achievement gaps is not as simple as adopting a set of standards, accountability and instructional improvement strategies. While these strategies are necessary, the data on student achievement in Massachusetts, after nearly two decades of reform, makes it readily apparent that schooling solutions alone are not sufficient to achieve our aspiration of getting all students to proficiency. We have set the nation's highest standards, been tough on accountability and invested billions in building school capacity, yet we still see a very strong correlation between socioeconomic background and educational achievement and attainment. (Reville, 2011)

Duncan and Murnane (2011a) have recently argued along the same lines, that increasing economic inequality in the US over the last 30 years has augmented the school achievement gap between rich and poor children (what they refer to as the "income

achievement gap"). The focus of current educational policies on school management, they argue, misses the real cause of low achievement amongst poor students. Policies dealing with the consequences of increasing levels of poverty and inequality in the country are more promising in raising the life chances of children from low-income families. Their book explores the possible processes for linking family income and children's educational outcomes. Reardon, in the same book, (Reardon, 2011) argues that not only has the income achievement gap widened as a consequence of the increasing economic gap between rich and poor families, but that the association between family income and children's achievement has also become stronger over the years.

How does income influence children's outcomes, particularly educational outcomes? What are the possible causal links between family income and children's achievement at school? The theoretical difficulties involved in defining a framework to support the connection between income levels and educational outcomes are discussed in the next section, as is some of the empirical evidence. I also discuss why, despite some disagreements, policy-makers should consider this evidence and frame innovative, integrated, socio-educational policies to tackle socio-educational inequalities.

2.3 Does income matter for educational outcomes?

2.3.1 Theoretical models: a "mining field"

The literature studying the relationship between family income and children's outcomes is vast and shared by different disciplines using different methods of analysis and focusing on different outcomes. The disciplines sharing the bulk of the research in this field are Economics, Sociology, and Developmental Psychology. Economic theory's main interest, since the advent of the *Human Capital Theory* in the 1960s (Becker, 1993; Mincer, 1993), is the link between present and future income. It tries to understand the "intergenerational income mobility" (Blau, 1999), that is, how incomes of different generations are determined and what incentives parents have to invest productively in their children's education to enhance future earnings. **Figure 1** describes the economic framework to analyse the links of parents' economic resources, choices (quality and quantity of goods and time invested in children), and endowments (inherited abilities) with children's attainment in terms of final schooling level and future income.

Figure 1: Economic framework

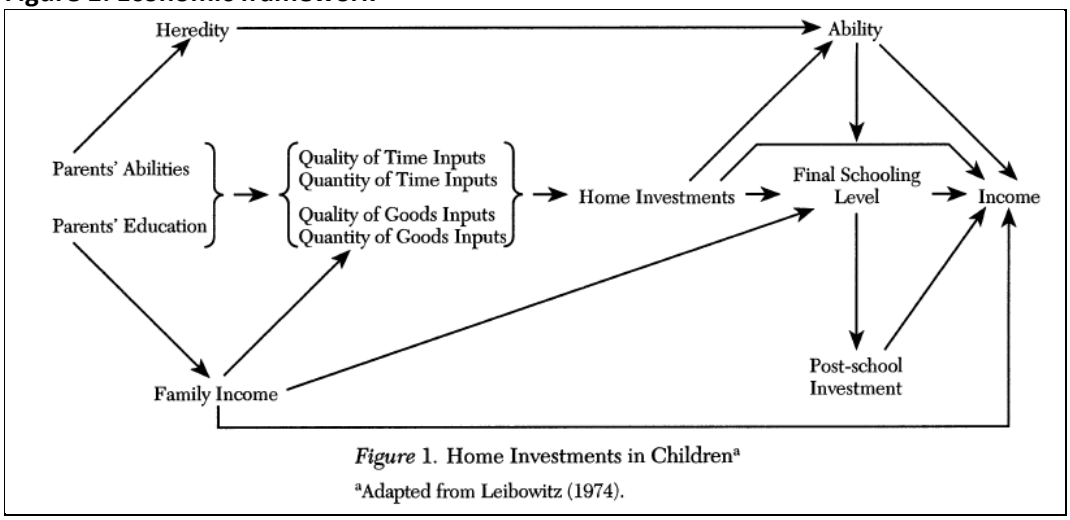


Figure 1. Home Investments in Children[a]

[a]Adapted from Leibowitz (1974).

Source: (Haveman and Wolfe, 1995, p.1833)

Sociologists and psychologists criticize this type of framework, claiming that it offers an incomplete view of factors shaping children's outcomes. It ignores broader social and psychological circumstances other than economic resources that shape families', parents', and children's lives. Sociologists and psychologists started focusing on the wider effects of income on social, familial, and individual contexts and the mutual interactions those effects may have on children and youths over time. Haveman and Wolfe (1995) combine these new theoretical perspectives into three groups: (i) the Socialisation/Role Framework, in which the influences of parents, siblings, peers, and neighbourhoods are transmitted to children's behaviours, expectations, and values shaping their cognitive, social, and psychological development; (ii) the Life-Span Development approach, which emphasises the timing of influential events over the life cycle of children and the different impacts these events may have, depending on when and in which context they occur, their duration, and the events that follow them and; (iii) the Stress-Theory and Coping Strategies, focusing on how distress and hardships experienced by parents affect children.

Although the sociological and psychological approaches expand the array of factors influencing children's outcomes beyond the sometimes narrow economic perspective, parents' income is still a factor in many of the mechanisms described. Income works as a moderating factor, aggravating or attenuating social and psychological circumstances involving families and parents due to its influence in shaping social life in modern capitalist societies. Parents' income not only partially explains the socialisation experiences of children and expectations as they develop, but also may turn already harmful episodes over a child's life cycle into much harder experiences. Income is certainly one of the main causes of parents' uncertainties and stresses.

Not surprisingly, income figures in almost all attempts to conceptualise the mechanisms explaining children's outcomes, even amongst sociologists and psychologists.

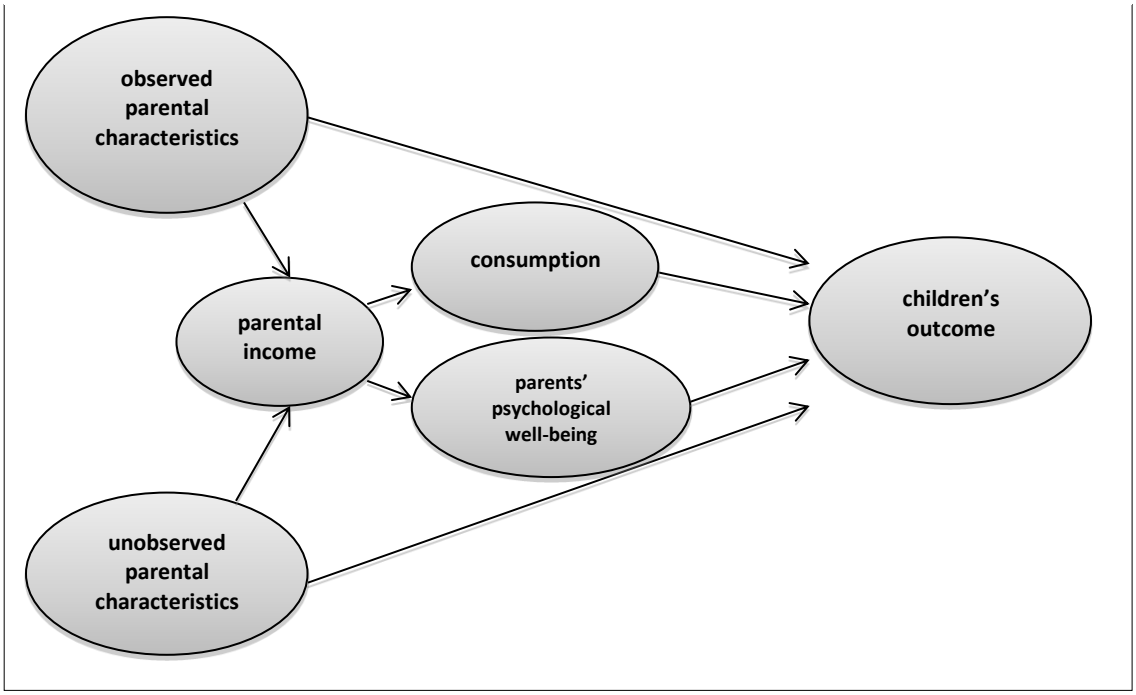
In an attempt to synthesise the research traditions mentioned above, Susan Mayer describes what she considers the two "main theoretical positions of social scientists on the importance of income" to children's outcomes (See Mayer, 1997, Ch.3). On the economic side, "investment theory", she argues, explains the link between family income and children's outcomes through the capacity of parents to invest *time* and *money* in their children. Parents and families not only pass on their genetic assets, values, and influences of the home environment to children, but also allocate economic resources towards children's development in the present. Different patterns of access to economic resources is considered to be the primary source of inequality amongst families in providing for their children's future, and is one important explanation for why poor children lag behind their peers in educational outcomes.

The second theory is referred to by Mayer (1997) as the "good-parent theory". This approach locates the mechanism through which income might be relevant to children's outcomes in the quality of the interaction between parents and children (attention, dedication, care, etc.). In one version of this theory, the "parental-stress model" (McLoyd, 1990), the shortage of income and associated labour market conditions (e.g. long-term unemployment, involuntary job loss, working conditions, low wages) may affect parents' physical and mental health, resulting in distress and depression, thus diminishing the ability of parents to provide good parenting and support to their children. In a second version of the theory, the "bad-role model", low income is associated with a lack of social prestige and self-esteem amongst parents. Persistent, long-term deprivation is translated into social norms, values, and behaviours amongst poor families and communities that not only influence parents' attitudes and expectations towards their children, but are also passed on to children, reproducing a "culture of poverty" (Lewis, 1998). As a consequence, poor families underestimate formal education and opt not to trade short-term gains for investments in children's education. A politically conservative corollary of the "culture of poverty" approach is the idea that if low investment in children's education is "culturally" determined then cash transfers to increase families' income will not result in more education for children. On the other hand, a slightly different interpretation of the "bad-role model" suggests that a psychological defence mechanism might be operating in poor parents' minds, building up a rational response to poverty in the form of low expectations and low levels of investment in their children. According to Mayer, "if parents believe their children cannot succeed in school,

not valuing education will reduce feelings of failure" (Mayer, 1997, p.51), and will probably save some money in the present. A politically liberal version of that corollary maintains that those beliefs are socially constructed and formed by unequal social structures. Therefore, reforms aimed at changing the social mechanisms (labour market segmentation, racial and gender discrimination, residential segregation, etc.) through which those values and attitudes are reproduced across generations should be implemented as part of a broad strategy to overcome inequality and to create social opportunities and new beliefs amongst poor families.

Mayer frames a model (**Figure 2**) bringing together these two theories – "investment" and "good-parenting" – to take a step forward in conceptualising the relationship between family income and child outcomes (Mayer, 1997, p.53). In this model, she tries to overcome the "conventional reduced-form model", which usually ignores that which could be producing the effect of income on children's outcomes ("blind" or "black-box" models). She also makes explicit the potentially omitted variable problem represented by unobserved parental characteristics that might create bias in estimates of income effects.

Figure 2: Mayer's heuristic income model



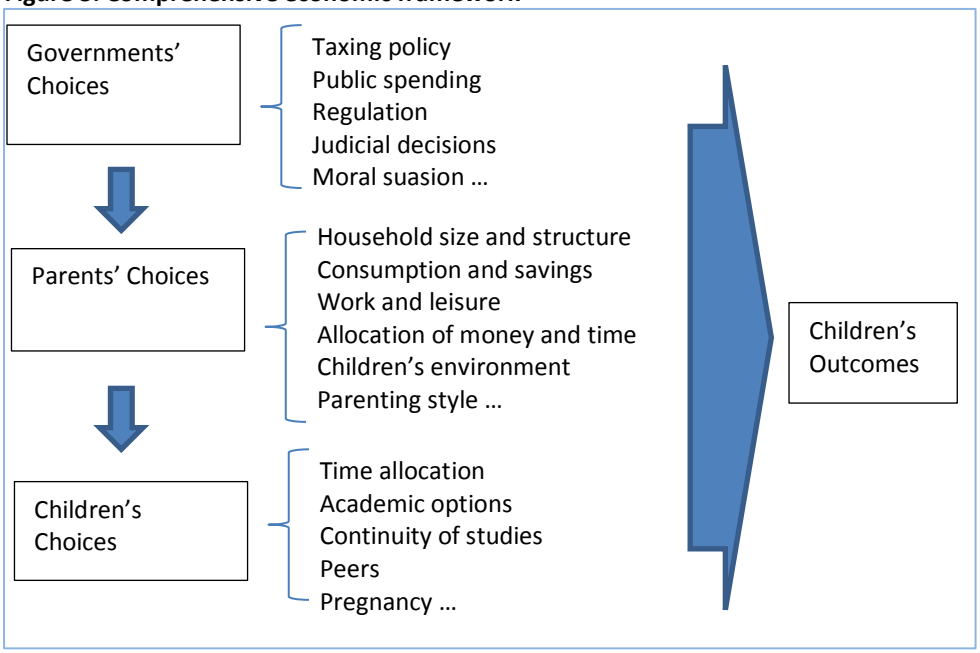
Source: Reprinted by permission of the publisher from WHAT MONEY CAN'T BUY:FAMILY INCOME AND CHILDREN'SLIFE CHANCES by Susan E. Mayer, p.53, Cambridge, Mass.: Harvard University Press, Copyright © 1997 by the President and Fellows of Harvard College.

Mayer's model is, nevertheless, a selection of the complexity of interactions between income levels and immediate results that may affect children's outcomes. Similar to Leibowitz's (1974) model (Figure 1), Mayer focuses on parental links with children that are

affected by income and on some of the possible mechanisms wherein parents' income might matter. The emphasis these models put on parental characteristics (mainly education and income) to explain children's outcomes derives from strong empirical support in the literature. However, both traditions – economic and socio-psychological – which Mayer attempts to synthesise in her heuristic model, neglect other circumstances that also explain children's outcomes.

Haveman and Wolfe (1995) argue for a "comprehensive economic framework" in which, in addition to parents' choices and circumstances, governments' and children's choices are taken into account to explain children's outcomes. By means of policy instruments (taxing, social spending, regulation, judicial decisions, moral suasion, etc.), governments shape the environment in which the choices of families and children are made. In particular, governments' choices about how much investment to make in children ("social investment") in terms of social services (health, education, nurseries, etc.), social benefits, and support for families strongly affect parents' choices and circumstances. The latter, in turn, impacts children's options, as mediated and made available by their parents, and may also affect children's choices about time allocation, continuity of studies, pregnancy outside of marriage, social relations, and so on. In **Figure 3** I provide a representation of Haveman-Wolfe's "comprehensive framework".

Figure 3: Comprehensive economic framework



Source: author's figure

Methodological limitations severely constrain feasible empirical models to test theoretical frameworks such as those delineated above. Difficulties imposed on research designs in general derive from the question being investigated, the data availability and reliability, and modelling constraints. Inasmuch as the researcher's interest moves towards understanding the processes or mechanism behind the income effects or the conditions that must be held for the effects to be observed, the methodological difficulties exponentially increase. An attempt to empirically test Haveman and Wolfe's model, for instance, requires a multiple-equation causal model, data from several sources, huge sample sizes, and previous knowledge about the potential causal links between variables included in the model on a scale that is unrealistic for the current stage of knowledge. As Haveman and Wolfe stress, "unassailable estimates of causal relationships describing the underlying process are not yet attainable" (Haveman and Wolfe, 1995, p.1839), which makes the selection of variables and the establishment of associations between variables immensely difficult.

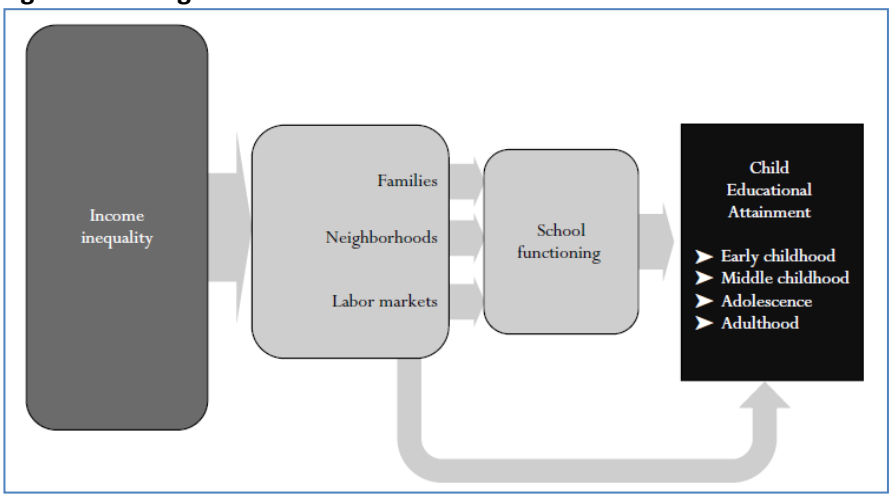
In part, those difficulties are due to the "multipurpose" nature (Mayer, 1997, p.145-46) of income. There are many pathways through which income may affect outcomes for children (Brooks-Gunn and Duncan, 1997), with parental links being just one amongst them. Income may affect children's living conditions, such as health and nutrition of family members, home material environments, parents' interaction with children, parents' physical and psychological well-being, neighbourhood characteristics, schools attended, out-of-school activities, social interactions, and so on. At the same time, children's outcomes can be measured based on several possible areas of interest. These can include children's physical and psychological health, cognitive skills, school attainment, incidence of teenage pregnancy, antisocial behaviour, future earnings, etc. The larger the spectrum of outcomes the more possible pathways leading to them in which income may play a role. Thus, it becomes more difficult to determining the underlying mechanisms operating through income that might affect outcomes.

These difficulties have not prevented scholars from developing research in this field. However, the effort follows with very little theoretical guidance for model specification. That, in part, explains the absence of a common framework guiding researchers in selecting the relevant variables and models to be tested (Haveman and Wolfe, 1995, p. 1872-73), as illustrated in this subsection. Knowledge progress in this field seems to strongly rely on testing, validating, and arriving at specific empirical hypotheses about links between income (and income-associated factors) and specific outcomes of interest. More general theoretical

frameworks based on cumulative empirical evidence are then proposed as potential candidates for a theoretical synthesis of knowledge on the subject.

A recent example of this strategy is the synthesis of five years of research in the aforementioned book by Duncan and Murnane - *Whither Opportunity? Rising Inequalities, Schools, and Children's Life Chances* (Duncan and Murnane, 2011a). These authors offer a conceptual model to explain how increasing economic inequality observed in the US over the last 30 years is widening educational gaps between rich and poor, reflecting the increasing importance of family income for children's educational attainment. They argue that income inequality affects "access to high-quality child care, schools, neighbourhoods, and other settings that help build children's skills and educational attainments" (Duncan and Murnane, 2011b, p.7). Their model offers an "ecological perspective" to explain the multiple influences of income inequality on educational outcomes. They consider including not only the immediate effects of income on families, (e.g. consumption patterns and psychological well-being), but also the potential impacts on neighbourhoods and local labour markets. The labour market is increasingly segmented in a context of increasing economic inequality and becomes a driving force determining level of income, stress, and uncertainty endured by families. High levels of income inequality also lead to residential segregation in which poor families end up living in neighbourhoods with characteristics unhelpful to child development (high crime rates; low levels of interaction with different social groups; difficulties in accessing high-quality child care, libraries, parks, etc.). **Figure 4** reproduces the Duncan and Murnane model.

Figure 4: Ecological framework



Source: (Duncan and Murnane, 2011b, p.8)

The novelty in the Duncan-Murnane model is not exactly the wider contextual approach used to explain income effects on children's outcomes, since Haveman-Wolfe's

model also encompasses macro-contextual variables. Rather, the novelty is in the status given to nation-wide income inequality in shaping families, neighbourhoods, and labour markets that, in turn, affect educational outcomes. In addition, the model is innovative in that it presents the idea that macro income inequality would also have an indirect effect on children's educational outcomes through impacts on school functioning. Income inequality impacts children's educational outcomes not only because it directly affects their families, defines their neighbourhoods, and delimits the labour markets in which their parents can find a job, but also because school functioning is affected. Schools become more economically segregated, and these schools witness higher levels of student mobility over the academic year, endure difficulties in hiring and retaining high-quality teachers, downgrade expectations and curriculum content, handle more behaviour problems in classes dominated by low achievers, and end up delivering poorer quality education. Without the support and pressure of more affluent segments of the population, schools attended by poor children tend to decline in quality. This model proposes that school settings are also shaped by the immediate social landscape, adding an important new ingredient to the long debate outlined in the first section of this chapter.

One of the fundamental questions posed in that debate is: *What can education and schools in the present do to reduce children's poverty and inequality in the future*? Duncan and Murnane's model suggests to me an inversion of that question: *What can children's poverty and inequality reduction in the present do for education and schools in the present?* Attacking income inequalities could benefit not only parents and children directly, but also the functioning of the schools children attend, which their future expectations rely on[2]. Helping families to overcome poverty and taking efforts to reduce economic inequality amongst families, according to Duncan and Murnane's model, might be very important in resetting the school functioning for the children of low-income families.

2.3.2 Raising incomes – does it make a difference for educational outcomes?

I turn now to consider evidence of the potential effects of income and income increase (through cash transfer programmes) on educational outcomes of children of low-income families. I look at whether public investments in cash transfers can be justified based on positive impacts on educational opportunities and outcomes for children of low-income

2 The authors do not propose any policy measure in this direction. Although they recognise that income inequality may be the root cause of poor schooling, reducing it directly, according to the authors, might not be the most cost-effective policy to boost school success.

families. I consider some of this evidence and how it might inform public policy initiatives in tackling educational inequalities.

Before moving forward, a conceptual issue must be clarified. What do researchers look at when analysing family income? An important distinction was brought about by Milton Friedman: the concept of "permanent income" (Friedman, 1957). The idea is that the annual earnings of individuals and families have two components: one more stable that roughly determines the pattern of consumption – "permanent income" – and another that fluctuates over the year and has an inconstant pattern – "transitory income". Individuals and families generally do not rely on the latter to define their standards of living. This has two implications for assessing income effects on children's educational outcomes (Mayer, 1997, p.63). First, it is unlikely that short-term windfalls will influence patterns of consumption, as these do not have an impact on permanent income; therefore, studies looking at effects of increases in families' incomes should consider the nature of the income rise. Second, since the usual way to operationalise the measurement of permanent income is to average income over a long period (year(s)) and assimilate the transitory income within that measure, chances are that the effects of permanent income on educational outcomes are biased downwards, since it includes the transitory component. Keeping this conceptual issue in mind, I now look at evidence of the impacts of income on educational outcomes.

Most researchers agree upon the multiple influences income exerts on people's lives and particularly about the connection it has with children's educational outcomes (Haveman and Wolfe, 1994; Brooks-Gunn and Duncan, 1997; Duncan and Brooks-Gunn, 1997; Mayer, 1997; Duncan and Murnane, 2011a). Nevertheless, social scientists continue to dispute the causal links between family income and children's outcomes (as shown by the different theoretical models discussed above), and the degree of that effect. Even more disputed is the idea that public policy interventions geared towards reducing family poverty via cash transfer mechanisms might successfully contribute to improvements in educational outcomes for children of low-income families. Methodological difficulties in this field contribute to the controversy (Blau, 1999; Blanden and Gregg, 2004; Blow et al., 2005), and more convincing results are mostly restricted to studies in which social experiments or long-term longitudinal data were available as part of the research design. The most robust set of results in this respect comes from a series of studies developed in the US beginning in the late 1960s, involving randomised social experiments and long-term longitudinal studies.

The first set of randomised experiments directly testing effects of income maintenance policies occurred in the 1960-70s in the US through Negative Income Tax (NIT) programmes.

These experiments tested the impact of direct grants on labour participation[3] of low-income families. As a side-product of these experiments, impacts on educational outcomes were also investigated in a series of studies. In the first of the NIT experiments, the New Jersey-Pennsylvania Experiment (1962 to 1972), children whose parents were enrolled in one of the NIT plans had, on average, a 25% to 90% greater chance of completing high-school, and between 1/3 and 1 1/2 more years of education than their counterparts in the control group (Mallar, 1977, cited in Salkind and Hanskins, 1982, p.174). Other studies based on NIT social experiments followed, focusing on attendance rates, academic skills, and learning outcomes.

In the Rural Experiment (1970 to 1972) in North Carolina and Iowa, Maynard (1977) tested effects on four measures of school performance: absenteeism, comportment grade, standardised achievement in test scores, and academic grade for students in grades 2 to 12. The subsample of elementary school children[4] in North Carolina showed significant effects based on all the indicators: 30% reduction in absenteeism; 6.7% increase in comportment grade; 6.2% increase in academic grade; and 19% improvement in the deviation between achievement test scores and expected grade equivalent scores. The other three samples (comprising high-school children in North Carolina and those from the state of Iowa) did not present any result. One possible explanation is that North Carolina's average income increase was significantly higher than Iowa's (US$ 800 vs. US$ 500 a year) and the authors explain that families were also more deprived in North Carolina than in Iowa[5]. Also, women in North Carolina reduced time at work per week by about four hours, whereas in Iowa, women reduced their work time by only one hour per week[6] (this may reflect the lower level of income increase in Iowa). Considered within the context of the two rural areas in which the study took place, these results suggest that income increase had an effect on absenteeism and on learning outcomes, but this depended on the level of poverty and on the value of the cash transfer. For instance, the different mean values paid in North Carolina and Iowa might explain the lack of results in the latter and, as the authors argue, as the expected payment by the programme increased, absenteeism decreased still further in North Carolina. In addition, this experiment suggests that income increase is more likely to affect younger children than older children.

3 These experiments involved cities in New Jersey-Pennsylvania; rural areas of Iowa and North Carolina; Gary, Indiana; Seattle, Washington; and Denver, Colorado.

4 Comportment grades and standardised test scores were available only for children aged two to eight. In the subsample of children aged 9 to 12 in North Carolina no significant effects were detected regarding academic grade and absenteeism.

5 This hypothesis apparently was not directly tested by the authors.

6 Time-out-of-work for mothers that could revert to children's support at home is one of the mechanisms hypothesised by the authors to explain improvements in children's educational outcomes.

Maynard and Murnane (1979), studying the effects of income on the school performance of children in grades 4 to 10, used data collected in the Gary Income Maintenance Experiment (1971 to 1974). Their results reach three important conclusions: i) there are significant effects of income increase on reading test scores for children in grades 4 to 6, but not for those in grades 7 to 10; ii) the effects become positive and are significant when test scores are measured three years or more after parents enrol in the programme; and iii) the effects are stronger for pupils whose families fall below half the poverty line than for pupils in families at half the poverty line or higher (although these pupils also observed significant positive effects). The authors attribute the "modest" results[7] to the weak impact the programme had on inducing out-of-work time for parents (only female-headed households perceived an increase of two hours per week) and to the short time for impacts on home environment and on educational outcomes to be observed. Working with low-income black families from the same experiment, Hanushek (1992) estimates an equivalent 18% standard deviation (s.d.) increase in reading and vocabulary achievement between second and sixth grades as a result of a US$ 10,000 increase in families' annual permanent incomes.

The Gary Income Maintenance Experiment was also assessed by McDonald and Stephenson (1979) as to enrolment in high-school and participation in the labour market for urban black youths aged 16 to 18. They found a significant positive effect on the probability of high-school enrolment for males (+.18) and a significant negative effect on the probability of labour force participation (-.11). Effects for females were found only at higher cash transfer values. The results support the authors' idea that "poverty among black families may be reduced directly and indirectly by a national income-maintenance programme if recipients or their children invest their subsidies in education" (McDonald and Stephenson, 1979, p. 4).

The Seattle and Denver Income Maintenance Experiments (1971,1972) had fewer data collected to assess educational outcomes (Hanushek, [no date]). However, positive and significant effects on school attendance and academic grades for children attending grades K-12 are reported in the literature (Salkind and Hanskins, 1982; Balter and Tamis-LeMonda, 2003). Interestingly, these experiments were the first to include a time frame for collected data from participant families, with 3-, 5-, and 20-year intervals. The inclusion of two other waves was a response to the concern that three years was possibly too short to allow effects to be detected. This is consistent with the idea that permanent income can only be affected

7 The "modest" results they find are 22 points difference when all children in grades four to six are considered; 53 points difference for those in the fourth grade and 26 points difference for those in the sixth grade. Without reporting the mean and standard deviation of the reading test scores it is difficult to assess the magnitude of that effect.

after a certain period of participation in the programme has lapsed, to allow families to change patterns of consumption.

In reviewing the four NIT experimental studies, Salkind and Hanskins (1982) recognise the potential capacity of NIT programmes to break the chain of intergenerational poverty. The impact on children's education and health[8] promoted by NIT programmes is, according to the authors, in part due to the fact that guaranteed income preserves the freedom of choice amongst families, relying on the assumption that parents are equally concerned about their children's well-being, regardless of socioeconomic status. As a consequence of that freedom, "more equity and more responsive outcomes" would generate gains in well-being for children. The authors also suggest that NIT is a more efficient way of spending public resources to fight poverty, since the average per capita cost of welfare programmes in the US (US$ 3,500/year), if just given to the poor, would raise a family of four above the poverty line.

The NIT experiments were not the only terrain to produce fertile research about income increase by welfare programmes and children's educational outcomes. Haveman and Wolfe's extensive study on investments in children (Haveman and Wolfe, 1994) also identifies effects of parents' economic resources on high-school graduation rates and years of schooling of children using data from the Michigan Panel Study of Income Dynamics (PSID). The family income/poverty line ratio and the number of years in poverty are found to have a significant effect on the number of years of schooling (by 0.21** and -0.06** respectively). More importantly, the number of years in poverty in which the family received the Aid to Families with Dependent Children (AFDC) benefit has a positive effect on years of education for children (0.04*). Although this last estimate is only marginally significant, it represents a two-thirds reduction in the negative impact the number of years in poverty has on years of schooling. However, those three factors are not statistically significant for high-school completion. This might be due to the wide access with no fees for high-school in the US.

The analysis of potential impacts of welfare and anti-poverty programmes on educational outcomes continued over the next decade. Gennetian and Miller (2002) analyse the effects of another welfare programme in the US – the Minnesota Family Investment Program (MFIP) – on children's outcomes, using an experimental design. According to reports from mothers, children were less likely to exhibit problem behaviours and were more likely to perform better and be more highly engaged in school. The study collected data three years after the single-mother families with two to nine year-old children had been assigned to one of

8 Those studies also show a decreased incidence of low birth-weight and increased consumption of nutritious foods amongst beneficiary children.

three groups (two different treatments of MFIP and one control defined by participation in the traditional AFDC scheme). The results show that children from MFIP families had 16% of an s.d. better performance at school and 19% of an s.d. more engagement in schools compared to AFDC families. In another study using data from the NLSY and the Earned Income Tax Credit (EITC) as a natural-experiment, Dahl and Lochner (2008) estimate a gain of 6% of an s.d. in a combined score of mathematics and reading after a US$ 1,000 annual increase in income, with larger gains for more disadvantaged children. They also conclude that the income effect is larger for younger children (ages 5 to 10 vs. 11 to 15), and that to sustain an increase in test scores the extra income must continue over a number of years. It is worth mentioning that the value of the EITC as a fraction of family income over the period analysed was, on average, 0.10, reaching a maximum proportion of 0.15 for families with two or more children in 2000; a relatively low proportion of the household income.

More recently, Duncan et al. (2011) analyse the impact of 10 welfare and anti-poverty programmes on children's achievements. The researchers use data from seven different experiments taking place in the 1990s, using income gains due to participation in welfare schemes as instruments for the effects of income on children's educational achievements, as reported by parents and teachers. They conclude that, on average, educational achievement increases by 6% of an s.d. for a US$ 1,000 increase in the annual family income when gains are sustained for between two and five years. The welfare programmes with earnings supplement components increased families' incomes on average between US$ 800 and US$ 2,200 a year, which yields an effect on school achievement of between 5% and 12% of an s.d. The authors also consider the possibility that larger increases in income might produce proportionally larger impacts on children's outcomes.

Despite the political controversy about welfare programmes in the US, the academic literature presents plenty of evidence demonstrating the negative impacts poverty has on children's development and supporting the potential positive effects of cash transfer programmes in attenuating those negative effects. It is also true that most studies on income effects use non-experimental data and, therefore, are prone to omitting variables and encountering other related problems, jeopardising internal validity. The studies reviewed, however, forward some of the most robust evidence available, since they are based on social experiments or long-term longitudinal studies.

Positive educational impacts of welfare programmes were found with respect to enrolment in and graduation from high-school, attendance rates, comportment and academic achievement, and years of education attained by children and youths. The strongest effects

were found for young children and those in lower grades, for those whose families were enrolled for a longer time in welfare schemes, and for those who were amongst the poorest before inclusion in the schemes. The value of cash received was also found to be influential on the level of results observed. In the same way, the length of time of exposure seems to be relevant, with most promising results being observed after three years of participation. In addition, cash transfers should be sustained over the time children from low-income families are in school, assuring grants that can be assimilated as part of families' permanent income and to have an effect on home environments for children. Two important aspects can be inferred from the experimental research described above, and are important supports for the hypothesis formulated in the forthcoming chapters. First is the relevance of time of exposure to the income transfer in enabling effects to be detected in children's educational outcomes, and second is the value of the cash transfer in per capita terms for the magnitude of that effect.

2.3.3 Is the evidence so far too "weak" to support policy innovation?

Some authors dispute the evidence discussed above, contesting the "size" of the income effect reported in the literature (Mayer, 1997; Blau, 1999). Mayer attempts to reduce the bias that she argues tends to exist in several of the "conventional studies"[9] due to the lack of control for confounding factors that potentially inflate the income effect. By pursuing five different strategies to estimate what she calls the "true effect of parental income", Mayer estimates the income effect on 12 children's outcomes, including cognitive skills, dropouts from high-school, and years of schooling. She concludes that income has an effect on children's outcomes, although with a much narrower magnitude than presumed in most of the "conventional studies". However, she also recognises that due to the multiple effects associated with income, the small impacts on several outcomes might have a substantial cumulative effect on children's lives. Similarly, Blau's paper concludes that the effect of current income on child development, as measured by scores on tests of cognitive, social, and behavioural development, is small compared to the effects of permanent income. Even the latter, he argues, is not "large enough" to make income-transfer policies a sensible solution to improving children's outcomes.

9 "Conventional models", as defined by Mayer, are those that tend to ignore the mechanisms operating through income to generate children's outcomes and that also tend to overlook potential factors associated with income, such as parents' characteristics, that might explain and therefore absorb the estimated impact of income on children's outcomes.

The studies by Blau and Mayer converge on a similar conclusion about the significant although "weak" or "small" effect of income on children's outcomes. However, the estimated small effect in the American context is probably due to the provisions of other public policies in support of poor families. By ensuring basic material needs for children, public policies help prevent children from suffering most of the hardships associated with a lack of income. The estimated income effects are, therefore, bounded by a social context in which basic needs are ensured through several direct intervention programmes, such as food stamps, Medicaid, housing, and child care subsidies. In such a context, other parental characteristics become dominant in explaining differences in children's outcomes. As Mayer puts it, "once children's basic material needs are met, characteristics of their parents become more important to how they turn out than anything additional money can buy" (Mayer, 1997, p.12). This can explain the so-called "weak" or "small" influence attributed to income in Mayer's analysis. As noted by Balter and Tamis-LeMonda (2003, Chp.18), cash income in that context does not proxy very well for material resources available to a family. However, in contexts where first-level necessities are only supplied by extra money, families can benefit from the direct effects of income, and cash transfers are probably more relevant and make a significant difference for children's well-being. In such contexts, the effectiveness of education policies, for instance, might be compromised by the severe deprivation children endure on a daily basis if no policy intervention is in place to mitigate those negative effects. Even Blau recognises this distinction:

> (…) direct intervention programs may require complementary home inputs to be effective. Thus the effect of family income on child development may be an important factor in determining the long-run success of direct interventions as well. (Blau, 1999, p.261)

Most scholars and policy-makers agree that investment in education is an important strategy in combatting long-term poverty. Yet, the conspicuous risk of compromising the effectiveness of that strategy due to the severe deprivation of many children of low-income families is also evident. Here, I reiterate the point raised at the end of the previous section, that educational policies are limited in their capacity to advance equality of opportunity in contexts of high or increasing economic inequalities.

In developed countries, the idea that education policy alone can remedy poverty in the long term has been recognised as illusionary. Esping-Andersen (2009) states that universal and free education has failed in equalising life chances in most of the developed world, mainly in those experiencing increasing levels of inequality over the last 30 years (US and UK). The standard assumption driven by human capital theory, as Esping-Andersen puts it, "was that equalizing access to all levels of the education system (…) would cancel out the effect of

parental resources on human capital acquisition with no need to alter the earnings or income distribution" (Esping-Andersen, 2009, p.122). However, education systems seem ill-equipped to create equality (they are prone to "cultural bias") and most of the cognitive and behavioural foundations for learning are now recognised as rooted in pre-school experiences and in what happens within families. The predominance of family care during a child's early years, associated with the deleterious effects of poverty on parents and the home environment, lead to a fundamental inequality of preparedness when children arrive at school, which seems to propagate over the school years. Social mechanisms affecting the opportunity structure for children are therefore linked to what Esping-Andersen calls the "three family effects": the money effect, the time investment effect, and the learning culture effect. Welfare distribution and services can break up those mechanisms of social inheritance by reducing child poverty directly and by providing high-quality child-care. Esping-Andersen concludes:

> (…) equality of opportunity requires at least some degree of equality of outcomes. The argument that 'here-and-now' equality is irrelevant and that we need only to be concerned about opportunities is clearly mistaken.(Esping-Andersen, 2009, p. 125)

Esping-Andersen advocates for a social investment strategy in developed countries that is focused not only on child-benefit-type delivery, but also on providing good quality pre-school and child-care services. Pre-school and child-care services have particularly high rates of return for the most disadvantaged children. These can simultaneously enhance children's outcomes and serve as a precondition for maternal employment (mainly in lone-motherhood situations), which, in turn, positively impact family income and reduce the necessity for distribution policies. Esping-Andersen argues that policies focused on increasing the bargain power amongst mothers, either by direct cash transfers or by supporting their employment, would diminish social differences in child investment between high- and low-income families. As consequence, direct effects on educational outcomes are also expected.

2.4 Conclusions

The debate about the relationship between education, poverty, and inequality over the last decades has challenged the conventional human capital approach and raised an important set of questions. Can schools be held accountable for educational outcomes without policy initiatives in place to address major deprivations within children's households? Can a "pure" educational policy addressing intra-school factors succeed in improving disadvantaged children's school outcomes despite what happens within the family? Can middle income countries like Brazil realistically expect to overcome poverty through human capital

investment while relying on the assumption that schools can do the job without any need for policy-makers to address income distribution issues? Persistently unequal levels of educational achievement amongst children from different socioeconomic status defy those who would answer “Yes” to the above questions. The current policy paradigm that tends to propose educational policies ignoring the important out-of-school causes of the achievement gap is in crisis and requires innovative approaches. How can research help in this task?

Over the last 40 years, several attempts to conceptualise the influence of income on children’s outcomes have taken place with no common framework being achieved so far. Nevertheless, research on this subject has been prolific and has accumulated a significant amount of evidence of how income affects educational outcomes. The several mechanisms of that influence investigated so far have found a place in at least one of the different models proposed by the authors reviewed in this chapter. More importantly, these models have at least one common element: each allows us to invert the question that for so long fuelled the conventional belief in the human capital approach. Rather than asking *what education and schools in the present can do to reduce children’s poverty and inequality in the future*, we can ask *what reductions to children’s poverty and inequality in the present can do for schools and education in the present*.

To answer that question, several studies have yielded evidence in support of the mitigating effects that welfare programmes may have on educational outcomes for the poor. The studies suggest the relevance of early intervention, the required time of exposure, the importance of the cash value, and the different impacts depending on the level of poverty. These studies show the potential benefit of welfare and anti-poverty programmes in breaking the chain of intergenerational poverty by contributing to the educational outcomes of children of low-income families. The assumption that parents are equally concerned about their children’s futures and that they are willing to support their children regardless of their socioeconomic background is hardly denied by empirical scrutiny. The freedom of choice in applying extra income in most of the cash transfer experiments is one important factor in promoting equity and in recognising the assumption of parents’ interests in supporting their children.

Despite the evidence, some scepticism regarding the size of the income effect exists in the literature. On one hand, it is suggested that cash transfer policies might not be the most efficient means to improve children’s outcomes. On the other hand, the multiple small impacts on several aspects of families and of children’s lives might yield a substantial cumulative positive effect. The “small” effect can also be attributed to the social context in which the

research reviewed was conducted. The US has in place several other public provisions capable of mitigating many of the most harmful and immediate consequences of income poverty. In such contexts, parents' characteristics might play a more significant role in defining children's well-being and achievement.

In developing countries, however, the reality of absolute poverty contests the sceptical view. Once families provide for basic needs through the economic resources they have access to, differences in income, even if small, start making a significant difference in basic provisions. Yet, if poverty means severe deprivation of the essentials of life, such as regular food, safe drinking water, sanitation facilities, shelter, clothing, and other basic needs, although a modest increase in income *per se* will not remedy the situation, it can mean the difference between a child having access to food or starving. The provision of services and investments in social infrastructure are certainly needed as well. However, as Townsend says in relation to child poverty in developing countries, "after public infrastructure investment, the most effective anti-poverty policy for children is the establishment of a child or family social security benefit" (Townsend, 2010, p.262). Townsend also articulates the direct consequences of poverty on education:

> If children are made chronically sick as a result of unsafe water supplies or inadequate sanitation or overcrowded housing conditions, they cannot go to school even if free high quality education is available. (Townsend, 2010, p.259)

The insufficiency of education policies in advancing equality of opportunities is clear once social conditions are taken into account. The social mechanisms affecting the opportunity structure for children engulfs education as well as any provision that requires active engagement on the part of the beneficiary to make those provisions effective. It is in this sense that "equality of opportunity requires at least some degree of equality of outcomes" (Esping-Andersen, 2009, p.125).

From this perspective, researchers and policy-makers alike have recognised that policy alternatives have to be sought outside of the conventional sectorial education policy to promote equity in education. By acknowledging the broader social context of education, they have looked towards integrated policies aimed at addressing inequality and severe deprivation while attempting to enhance educational opportunities for low-income children.

Chapter 3. Conditional Cash Transfer Programmes: Tackling intergenerational transmission of poverty?

3.1 Introduction

In this chapter I introduce the emergence and theoretical landscape of conditional cash transfer programmes (CCTs) in Latin America (LA) in the late 1990s as a new social policy approach to fighting poverty and inequality in the region. I bring into perspective the fundamental educational rationale underpinning the long-term objectives of CCTs, stated in terms of human capital accumulation amongst poor families as a pathway to tackle the intergenerational transmission of poverty. Then I review the literature on CCT programmes with an emphasis on their impacts on educational outcomes. Particular attention is given to research concerned with students' learning outcomes and grade progression, as these are key pathways to the completion of the basic education cycle and to human capital accumulation. So far, very few studies have focused on learning outcomes and there is no significant evidence that CCTs contribute to them. Critics of CCTs have pointed to this lack of evidence as a possible indication of the ineffectiveness of these programmes in promoting valuable education for children from low-income families. On the other hand, the lack of evidence raises questions about how methodological difficulties, data availability, and institutional constraints might explain this unexpected lack of results. The *missing link* in the chain of expected effects challenges the policy claim of the long-term human capital accumulation objective, but may also reflect methodological limitations. It is in this context that this research on CCTs is situated and justified.

3.2 The emergence of Conditional Cash Transfer Programmes in Latin America

Conditional Cash Transfer Programmes (CCTs) emerged in the late 1990s in several LA countries as a new social policy approach to fighting poverty through the promotion of human capital accumulation amongst poor families[10]. In these programmes, grant incentives are provided to poor families and are based on a set of conditions they are expected to fulfil while participating in the programme. Although the so-called 'conditionalities' can vary depending

10 The first national experiences in LA were Progresa/Oportunidades in Mexico (1997/2002), Programa de Asignación Familiar (PRAF) in Honduras (1998), Red de Protección Social in Nicaragua (2000), Bolsa Escola / *Bolsa Família* in Brazil (2001/2004), Familias en Acción in Colombia (2001), Chile Solidario in Chile (2002).

on the programme's stated objectives and design, the majority of the interventions currently in place require families to invest in their children's education, health, and nutrition as conditions to receiving the cash transfer. It is expected that by fulfilling these 'conditionalities' families will increase the probability that their children will have better opportunities to escape poverty in the future (to overcome the 'poverty trap'[11]), while simultaneously alleviating current income poverty.

Over the last decade CCTs have mushroomed in LA. According to Valencia Lomelí (2008) there are 16 countries implementing CCTs throughout the continent, covering around 70 million people (12% of the population). The successful experiences in LA quickly crossed the Atlantic, and in 2009 the World Bank reported 27 countries developing similar programmes[12]. Although programme designs vary significantly between countries, some commonalities are now recognised and researchers have been able to point out their innovative features so that it is possible to identify a family of programmes under the label of CCT. For Rawlings (2005), CCTs share three main characteristics: (i) programmes are based on cash transfers instead of traditional social assistance subsidies and in-kind transfers; (ii) programmes target poor families by means-tests; and (iii) programmes are conditional on education, health, and nutrition. Villatoro (2007) also highlights that CCTs have a common conceptual framework, which includes: (i) associating poverty alleviation in the short term with human capital accumulation in the long term; (ii) emphasis on correcting market failures by creating incentives for poor families to make optimal allocations to children; (iii) coordination of multiple institutional agents at national and local levels in the implementation process; (iv) social interventions on multiple social dimensions regarding families' well-being; (v) strengthening of women's roles within families by assigning them as the main cash recipients; and (vi) use of evaluation and monitoring systems for managerial and accountability purposes. Hanlon et al. (2010) recognise four principles underpinning CCT programmes: (i) cash transfers as necessary for the fulfilment of the basic human right to a minimum income; (ii) governments as responsible for funding a non-contributory welfare programme able to reach the most destitute; (iii) commitment to long-term provision and large-scale coverage; and (iv) recognition of the productive capacity of the poor and its importance within a broad strategy for development. Common features are also underscored by several other authors, including

11 "Poverty trap" is the vicious cross-generation cycle in which children from poor families become the new poor in the future; this is generally associated with low levels of education.

12 In addition to the 16 countries in LA, the WB reports 3 African countries (Burkina Faso, Nigeria, and Kenya), 3 countries in East Asia (Cambodia, the Philippines, and Indonesia), 3 countries in South Asia (Pakistan, India, and Bangladesh), plus Turkey and Yemen.

Simões (2006), Valencia Lomelí (2008), and Barrientos & Santibáñez (2009). How do these distinctive characteristics of CCTs help to reshape the social policy landscape in the LA region?

3.2.1 CCTs within the social policy landscape

Most of the literature on CCTs so far has dwelled on their features in terms of design, innovative characteristics, implementation strategies, and impacts, thus highlighting programme policy statements, operational aspects, and effectiveness related to social outcomes (Barrientos and Santibáñez, 2009). Besides these relevant and necessary aspects, the theoretical place CCTs occupy within the social policy landscape in developing countries has also been debated. CCTs have been recognised as a major change in the tradition of social policy in LA, but there remain different views regarding their influence and reach in terms of shaping a new paradigm for social policy in the region.

Several authors have considered CCTs as a new type of social assistance programme, being a part and representing an evolution of the safety net system in developing countries through the association of short-term poverty alleviation with long-term development goals of human capital accumulation (Morley and Coady, 2003; Rawlings, 2005; Britto, 2008; Hall, 2008). Hall (2008), for instance, stresses the strong support by the World Bank and the Inter-American Development Bank for CCTs in LA as a demonstration that these schemes "fit nicely" into the safety net approach to social policy put forward by those financial institutions. The World Bank, in particular, tends to position CCTs under the social risk management framework, as these programmes work as part of safety nets and help households to invest in their future and manage risks (Grosh et al., 2008). Barrientos and Santibáñez (2009) disagree with this characterisation of CCTs.

> It would be difficult to make a case for placing conditional cash transfer programmes squarely within a social risk management framework, as they lack explicit risk management components to deal with different adverse shocks that could affect household's current human, physical and social capital. (Barrientos and Santibáñez, 2009, p.13)

Nevertheless, income transfers conditioned on school enrolment and attendance can work as a preventive instrument to avoid undesirable household responses to several types of risks, such as the withdrawal of children from school. In this respect, for instance, it would be justifiable to include school-age children from low-income families in CCT initiatives even if they were already enrolled in school. This opposes the critique that CCTs are not cost-effective because they are focused on groups of children already enrolled in school, as articulated by

Schwartzman (2005) and Reimers et al. (2006). The inclusion of those already in the education system can be seen as safeguard against premature withdrawal from education.

Despite the common interpretation of CCTs as part of a safety net system, the latter as a model to organise and deliver social protection to vulnerable and disadvantaged people in developing countries has been criticized as 'minimalist'. As a mirror of the schemes implemented in the North, to some authors, safety nets have proven to be limited in developing country contexts (Hall and Midgley, 2004). Safety net policies were implemented and supported by the World Bank in LA through social funds as instruments intended to mitigate social impacts provoked by economic stabilisation and structural adjustment plans in the 1980s and 1990s. They have also been associated with reductions in social investments in the region during the 'lost decade'. Due to its "minimalist and selective nature (…) as well as its reliance on the voluntary sector for implementation" (Hall and Midgley, 2004, p.6), safety nets were interpreted as a form of "residualism"[13] and were criticized as insufficient and ineffective in tackling the prevailing and widespread poverty in developing countries, even in times of economic growth. Therefore, conceptualizing CCTs as an evolution or specialisation of safety nets may not do justice to their innovative design features and known results in tackling short-term mass poverty. Neither does this conceptualisation consider the wide coverage of the poor, the multidimensional approach to poverty by links to at least three fundamental underpinnings of human development (income, health, and education), or the state-implemented[14] and state-financed[15] character of CCTs.

The emergence of CCTs as a large-scale social initiative in LA has meant a shift in the emphasis of social assistance in the region from traditional (generally inefficient and ineffective) 'residualist' schemes of supporting the poor towards a human development approach to fighting poverty. This shift represents an evolution of social protection in the region towards integrated social interventions capable of addressing at least three dimensions of development: income, education, and health. According to Barrientos & Santibáñez (2009), the dominant conceptual framework sustaining CCT programmes has, as a central element, the understanding that poverty is a *multidimensional* and *intergenerational* social phenomenon. CCTs are also supported by the Capability Theory of Amartya Sen (Sen, 1999), which addresses

13 A model of social policy common in developing countries in the 1950s implying minimum state intervention; mainly supported through families, charities, churches, NGOs; addressing immediate needs of the poorest and focused on the rehabilitation of the individual through work (workfare approach).

14 Regional and local agencies have taken part in implementing CCTs, although national governments have been the main stakeholders in the decision-making process and the main funding contributors.

15 Even in countries where CCTs have received external financial support from donors, funds are based on loans and so accrue to state debts.

well-being and human development with focus on childhood. According to Barrientos & DeJong (2004) children are over-represented amongst poor individuals. Their quality of life, safety, and survival may be negatively affected by poverty and their futures may be compromised by shortcomings in nutrition, health, and education. Through the recognition of the intergenerational effects of child poverty, CCTs can be considered as part of an integrated child poverty eradication policy and as a major strategy for social development in developing countries.

Whether and how CCTs are reshaping and redefining the relationship between social policy and social development is a matter for further efforts in theorizing social policy in developing countries. Nevertheless, the educational component practically present in all CCT programmes must undoubtedly play a major role if CCTs are to be of any significance in fighting poverty in the long term and contributing to social development. This brings us closer to the central interest of this study: the effectiveness of CCT programmes in promoting the educational outcomes of children of low-income families. To understand how CCT designs frame the policy link between education and long-term poverty reduction, we must look at the basic educational rationale behind CCT programmes.

3.3 CCT's long-term objective: the educational rationale

Although CCTs are currently understood as a social assistance programme amongst social policy and social protection theorists, at the very beginning, CCT programmes were a policy experiment aimed at universalizing basic education. They were not conceived of as mechanisms to simply assist the poor (Amaral et al., 1998)[16]. Originally, CCT programmes[17] in Brazil were based on the idea that opportunity costs, alongside direct education costs, were significant obstacles faced by poor families to getting their children enrolled in and attending schools[18]. Even those who managed to enter school were likely to drop out due to economic pressures endured by their households. This problem was perceived as a matter of 'rational choice' driven by families' lack of resources. By compensating poor families for the costs

16 One of the first proponents of a conditional cash transfer programme in Brazil, Professor José Márcio Camargo, argued for a benefit centred on families with children between the ages of 5 and 16 years who were enrolled and attending a public school. This proposal came as an alternative to the minimum income programme being debated in the National Congress in 1991 (following the Bill proposed by Senator Eduardo Suplicy), which was focused on assuring individuals a minimum income. Suplicy's proposal was not linked to education or any other conditionality, and had individuals over 60 years of age as the first priority.

17 In Brazil the first CCT experiences were developed in the Federal District and Campinas (SP) in 1995 and were followed by similar initiatives in several other municipalities. These first experiences, although sometimes referred to as minimum income programmes, were invariably conditional on education and centred on families instead of individuals. The explicit link to education is branded in the very name of many of these local initiatives, which were generically referred as "Bolsa Escola" programmes (School Grant).

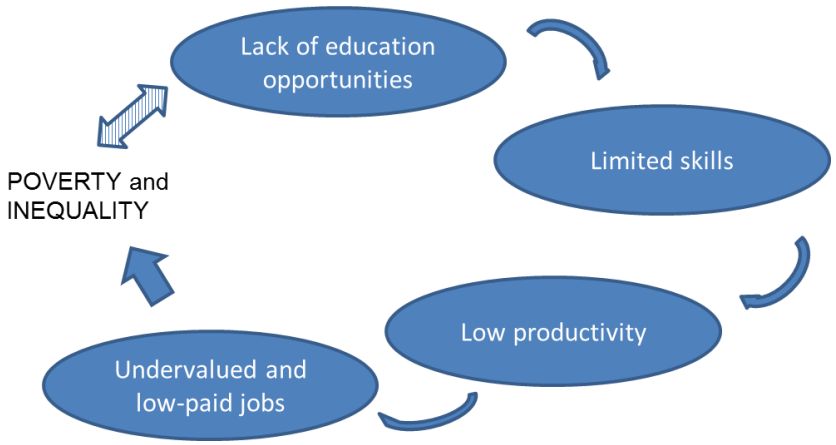
18 Even in a context of free education in public schools, as is the case in Brazil.

involved in sending their children to school, the government would counterbalance the negative effects of poverty on families' decisions with respect to children's education, and thus increase the chances that future generations would escape poverty.

The central rationale behind cash transfers conditional on education is that, once families have the right incentive to enrol children in school at the expected age, children will attend classes regularly, receive passing grades, and achieve the highest education level possible. As a result, they will be better educated and will be able to find good jobs requiring a more productive and skilled labour-force, receiving better wages. CCT programmes promote children's education in order to interrupt the intergenerational transmission of poverty.

This rationale is derived from human capital theory, formulated in late 1950s and 1960s, as mentioned in the previous chapter. According to human capital theorists, in countries with high levels of inequality and poverty a main factor explaining differences in income is the gap in access to education for different social groups[19]. Because certain segments are excluded from educational opportunities, their skills are limited, their productivity is lower, their labour is undervalued and is consequently low-paid and, often, insecure jobs are the only alternatives to unemployment (**Figure 5**). Expanding access to education for the poor is considered a pathway to sustainable economic growth, and as the most effective way to fight poverty in the long term. The formula is simply that more years of schooling (as a measure of human capital accumulation) will lead to more productivity in the economy, more economic growth, higher salaries, and less poverty.

Figure 5: Human capital rationale



19 This argument is present in one of the first propositions of CCTs in Brazil in 1993 by the economist José Márcio Camargo (Camargo, 1993).

Most of the efforts deployed by governments in developing countries since the early 1960s have relied on that rationale and attempted to expand access and create opportunities for school inclusion for all children. Nevertheless, the expansion of access very often occurred in an unequal pattern due to structural inequalities in the distribution of educational resources, by which the poorer segments generally received patchy and poor quality education services. In addition, little was known about the effects of poverty on the demand for education, leading to the belief that school provision alone (eventually with some special educational provisions to address the needs of low-income children) could assure education for all. The idea that providing educational services is sufficient to improve educational patterns amongst the poor is questioned by research investigating the effects of income poverty on children's outcomes, as discussed in the previous chapter.

The novelty of CCTs is that they add a demand-side concern to education policy, which has so far strongly focused on educational inputs. The problem not previously recognised was the effect of poverty on educational opportunities, despite the availability of education services[20]. As Bonal (2007) highlights, for many children it is impossible to take advantage of educational opportunities, even where schools and teachers are available – a fact that has often been ignored. Likewise, Simões (2006) argues that failures in policy equity can occur not only through deficiencies in coverage or quality of services delivered, but also by the beneficiaries' circumstances targeted by those policies. Initial conditions make individuals unequal in their ability to participate in public policies and to convert public services into real benefits. Public policy effectiveness is thus associated with, and can be negatively impacted by, the initial conditions of the target population (Marulanda and Guzmán, 2003). Reimers raises similar concerns, reflecting on the educational context for the poor: "Is it possible to attain equality of educational opportunity in highly unequal societies? How much of this can be accomplished with educational interventions?" (Reimers, 2000c, p.5). His book demonstrates that educational policies on their own can be beneficial to poor children by making educational opportunities more equitable. However, I argue that although necessary, in contexts of severe deprivation education policies alone are severely limited in their capacity to advance equality of educational opportunities. By considering the economic constraints affecting the demand for education amongst low-income families, as well as the negative effects poverty may have on children's well-being and choices, CCTs are an important ally in achieving universal basic education (in addition to the quest for more equitable education policies).

20 As represented by the double arrow in Figure 5.

By promoting school enrolment and attendance, CCTs are thought to lead to more education amongst poor children. Nevertheless, even when children from poor families have access to school and do not drop out, the link between schooling and learning is not straightforward. Students' school experiences vary significantly in terms of curriculum, teaching quality, learning materials, school organisation, support programmes, teachers' support and expectations of students, amongst an array of "school factors" not influenced by the CCT programme itself[21], but that are very influential on students' outcomes. Completion rates, effective learning and how this learning relates to economic opportunities available in the market have to be taken into account if schooling is to be considered a pathway out of poverty.

It is for the reasons above that the educational rationale and assumptions of this new generation of social policy have been under siege by critics such as Reimers et al. (2006). These authors suggest that CCT interventions could operate without the expected interruption of the intergenerational transmission of poverty by channelling poor children through poor and sometimes dilapidated education systems. Their rationale follows more or less like this: low quality schools and the opportunity costs involved in keeping children in school might lead parents to the rational decision to pull their children out of school. With a CCT programme in place, new incentives are created for parents to keep their children in school; yet, children may attend the same low quality school where they may not progress academically, and where they are likely to receive failing marks, repeat grades, give up in frustration after some time, and drop out. Therefore, schools would continue to contribute to the reproduction of the intergenerational poverty cycle, despite the operation of a CCT programme which consumes a good share of educational resources[22]. The argument raised by the authors is that CCT programmes have not proven to be an effective instrument to enrol children in school and to increase learning outcomes, and as such will not create a better future for beneficiaries in terms of human capital accumulation. No one can seriously disagree with that claim; however, the authors have prematurely concluded that CCTs are not contributing to learning outcomes without robust empirical evidence to support this conclusion.

The argument that CCTs represent a misuse of public resources since they have not been proven to have an effect on learning outcomes motivated my investigation of this topic. Through my research I look for evidence that CCTs can simultaneously alleviate immediate

21 Some CCT programmes, however, have a supply-side component to improve some aspect of the education services in the eligible areas, for instance, Progresa-Oportunidades (Mexico) and RPS (Nicaragua).

22 According to Morley and Coady (2003), on average around 5% of the total education budget and 15% of the primary education budget in developing countries has been diverted from education policies into CCT programmes.

poverty and contribute to children's school outcomes. I believe that even the most conducive school environment will fail in promoting poor children's education if deprivation at home is appalling and ignored by policy-makers. That being said, I do not disregard the influence of schools on educational outcomes. The idea that CCTs on their own can provide the necessary and sufficient conditions to increase school outcomes amongst poor children, regardless of intra-school factors, is naïve. Given decades of research on how schools serving poor communities can differ dramatically in their effectiveness, one cannot ignore the school's role in that respect. However, by neglecting the tremendous negative effect poverty has on children's environments and ultimately on the objective conditions for learning and development, education policies are also counterproductive. If education policies intend to be effective for all, then attention must be paid to what is preventing poor children from enrolling, attending, and learning.

How effective are CCTs in responding to the demand side of education policy? What has research found so far about the effectiveness of CCTs in contributing to children's education? Can CCTs, by functioning through education, be an effective social policy mechanism to provide a way out of the "poverty trap"? These are some of the questions researchers have addressed, and the following review of the literature shows that significant gaps remain in the knowledge on this topic.

3.4 The international research on CCT programmes

Since the first large-scale CCT programme was implemented in Mexico in 1997 (*Progresa*), a large array of studies[23] has been produced covering different features of CCT programmes, from design and conceptual framework to their main operational characteristics. In addition, the effects of CCTs on several aspects of social life such as poverty, inequality, education, nutrition, health care, child labour, gender relations, social relations, social capital, and even migration have been reported in the literature. Good reviews on CCT programmes and their effects are now available, including Rawlings and Rubio (2003), Morley and Coady (2003), Rawlings (2005), Rawlings and Rubio (2005), Villatoro (2005), Cohen and Franco (2006), Reimers et al. (2006), Lomelí (2008), Fiszbein and Schady (2009), and Hanlon et al. (2010). As these studies illustrate, there are different approaches to review research in this field. CCT programmes have been analysed by single programme/country experience (e.g. *Progresa-Oportunidades* in Mexico*, Bolsa Escola - Bolsa Família* in Brazil, *Familias en Acción* in Colombia,

23 At the time of this review there were 79 references alone by the International Food Policy Research Institute (IFPRI) on the Mexican Progresa-Oportunidades programme.

Chile Solidario in Chile, *Programa de Asignación Familiar* (PRAF) in Honduras, *Red de Protección Social* in Nicaragua); by programme characteristics and design (e.g. objectives, conceptual frameworks, conditionalities and benefits structure, costs, political contexts, implementation processes, operational designs, monitoring and evaluation systems); and by social results and impacts (e.g. on poverty, education, health, nutrition, gender relations, etc.). In my review of the literature I focus on educational effects, particularly on studies concerned with children's learning outcomes and progression in the education system.

3.4.1 What are the effects on education?

Research on CCT programmes has covered a vast array of possible educational outcomes. Amongst short-term effects, the most common are **school enrolment** (Behrman, Sengupta and Todd, 2000; Schultz, 2001; Ahmed and Ninno, 2002; Maluccio and Flores, 2004; Behrman, Sengupta and Todd, 2005; Schady and Araujo, 2006; Janvry, Dubois and Sadoulet, 2007; Oosterbeek, Ponce and Schady, 2008; Schady and Araujo, 2008; Borraz and Gonzalez, 2009; Filmer and Shady, 2009; Gitter and Barham, 2009; Glewwe and Kassouf, 2012) and **school attendance** (Schultz, 2000; Ahmed and Ninno, 2002; Duryea and Morrison, 2004; Maluccio and Flores, 2004; Bastagli, 2008; Filmer and Shady, 2009) due to the conditionalities generally stated in the programmes' theories and designs. Other education-related variables investigated include: **age at first entry** (Maluccio and Flores, 2004; Behrman, Parker and Todd, 2009), **dropout rates** (Ahmed and Ninno, 2002; Behrman, Sengupta and Todd, 2005; Baird, McIntosh and Özler, 2011; Glewwe and Kassouf, 2012), **re-entry rates** (Maluccio and Flores, 2004; Behrman, Sengupta and Todd, 2005), **promotion** and **repetition rates** (Duryea and Morrison, 2004; Maluccio and Flores, 2004; Behrman, Sengupta and Todd, 2005; Janvry, Dubois and Sadoulet, 2007; Behrman, Parker and Todd, 2009; Glewwe and Kassouf, 2012), **gender gap** (Behrman, Sengupta and Todd, 2000; 2005; Filmer and Schady, 2008; Chaudhury and Parajuli, 2010; Hasan, 2010), **transition to secondary education** (Behrman, Sengupta and Todd, 2005), **years of schooling** (Behrman, Sengupta and Todd, 2005; Behrman, Parker and Todd, 2009), **parents' attitudes towards education** (Adato and Roopnaraine, 2004), educational effects on **peers and siblings** (Ferreira, Filmer and Schady, 2009), **family expenditures on education** (Adato and Roopnaraine, 2004; Maluccio and Flores, 2004), and **students' achievement in standardised test scores** (Behrman, Sengupta and Todd, 2000; Filmer and Shady, 2009; Ponce and Bedi, 2010) and in **cognitive skills** (Paxson and Schady, 2007; Fernald, Gertler and Neufeld, 2009). My particular focus is on studies concerned with

learning outcomes and grade promotion, since these are key pathways to completion of education cycles and to more years of schooling.

So far, learning outcomes can be considered one of the most scarcely studied and challenging variables to investigate in relation to the possible contributions of CCTs to education. The few studies that have looked at this issue have found no effects of CCTs on students' academic achievement. For instance, Valencia Lomelí compares the positive impacts of CCTs on enrolment and attendance rates, stating that "results are not as promising with respect to effects on actual learning" (Valencia Lomelí, 2008, p.481) and, quoting Villatoro (2005), "learning is one of the least clear aspects of CCT programs, and most studies are unable to document positive results". This lack of evidence of learning outcomes constitutes the *missing link* in the chain of desirable causal effects of CCT programmes leading to human capital accumulation in the long term. An essential aspect linking increasing enrolment and attendance rates to grade progression and school completion is hitherto unfulfilled according to research in the field. In the absence of that evidence, either grade progression does not happen and beneficiaries will sooner or later drop out of the education system, or it happens without any meaningful learning experience that can make a real difference in children's lives. I explore the *missing link* and reflect on possible explanations for this apparent failure.

Behrman et al. (2000) developed one of the first CCT investigations looking at learning outcomes. They evaluated *Progresa's* (Mexico) short-term effects on children's achievements in test scores (Spanish and Mathematics), finding no improvement that could be associated with participation in *Progresa*[24]. Several methodological limitations can explain their result. First, the study had only one *pre/post* programme achievement test available (with tests taking place in December 1997 for the fourth grade and in December 1998 for the fifth grade). Since the first payments in *Progresa* started in May 1998, the *pre/post* programme evaluation assessed effects of just seven months of participation[25]. Second, the application of the two tests did not occur as planned by researchers at the beginning of the academic year (August), but in the middle of the academic year (December)[26]. This means that children were assessed halfway between two different grades, possibly experiencing different allocations of teachers,

24 The authors even found negative effects in some cases, which they consider might be due to compositional changes in schools.

25 The authors argue that the pre/post evaluation covered a one year interval of participation in Progresa, but in fact one should consider for evaluation purposes the start of the programme in May 1998, when benefits started to be paid to families. Besides, households interviews for the baseline took place only in October/November 1997, meaning that even if achievement tests had been applied in August of that year as planned, information on households would not have been available.

26 Initially the authors planned to run the tests in August 1997 (the month Progresa was launched and also the start of academic year in Mexico) and in August 1998 to assess differences between treatment and control groups after one year of programme participation and one year of schooling.

peers, and other school related factors that could influence results for treatment and control groups. Third, randomisation in *Progresa* occurred at the community level, but test takers were self-selected into schools. Besides, the subsamples of pupils in the control and treatment communities taking the tests were not randomly selected. Therefore, compositional effects at school and grade levels could have biased results. These methodological limitations may explain the lack of positive results.

The authors argue that effects on beneficiaries' performance are more likely to be detected if one tests future performance at school of those children currently at pre-school age. That would allow for a longer period of exposure. As stressed by the authors, their study managed to compare groups at most 1 ½ years after the target population had been exposed to *Progresa*[27]. Commenting on that study, Skoufias (2006) also states that more conclusive evidence would need a longer period of time before incentives could be translated into better scores on school exams. Skoufias also considers an additional conditionality linked to school performance to be incorporated in programme designs[28] as a possible contribution in that direction. This raises questions about whether the programme's benefit structure (which is different across CCT programmes) has a major influence on possible learning outcomes. Skoufias concludes by indicating that school enrolment and regular school attendance are necessary, though these are insufficient conditions for human capital improvement (the same argument is made by Morley and Coady, 2003; Reimers, Silva and Trevino, 2006; Valencia Lomelí, 2008).

Ponce and Bedi (2010) recognise the existence of theoretical reasons to expect that CCT programmes would have an impact on students' achievements. However, their study on the Ecuadorian *Bono de Desarollo Humano* found no significant effect on students' test scores. The authors attribute the lack of impact to the simultaneous negative effect related to crowding in schools provoked by the large impact the programme has on school enrolment [10 percentage points (p.p.)][29]. Crowding would counterbalance the expected positive effects of CCT on students' achievements. However, the authors present no evidence that increasing enrolments generated overcrowded classrooms in Ecuador. The lack of impact in Ponce and Bedi's study may be due to the absence of a time variable allowing for consideration of the potential cumulative effects mentioned in their study (attendance, nutrition, and reduction in child labour) to be translated into achievements on school exams.

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27 As argued earlier, this is not even the case if one considers the exact month in which benefits started to be paid (May 1998).

28 The new CCT programme created in New York City (Opportunity – NYC) does exactly this, as shown in Silva (2008).

29 Ponce quotes Schady and Araujo (2006) .

The "crowding" argument above was investigated by Ahmed and Arends-Kuenning (2002) in Bangladesh, where already large classes became even more crowded as a result of the Food for Education programme (FFE)[30]. Quality of education affected by increase in class size due to the impact of FFE on enrolments is the central concern in their work. The authors examine the impact of FFE on test scores in Bangla and Mathematics of fourth grade children who were not beneficiaries and who were already in school before FFE started. Assuming that positive effects of FFE on poor children would increase their enrolment and attendance rates, the authors investigate whether FFE schools, under the pressure of beneficiary student enrolment, would decline in their quality as measured by non-beneficiaries' test scores. They find a negative impact of FFE on non-beneficiaries' test scores, although not due to crowded classrooms but due to "peer effects". Insofar as the proportion of FFE students in a given school increased, the test scores of non-beneficiaries decreased. However, the authors recognise that possible unobserved household socioeconomic variables might account for that association[31]. If that is true, then it could be the case that what Ahmed and Arends-Kuenning observed was not a "peer effect" (the influence of a majority of less able beneficiaries upon the learning context for non-beneficiaries). Rather, it could be that non-beneficiaries in schools with high FFE intakes tend to be more similar to beneficiaries in terms of unobserved family characteristics relevant to test score achievement; therefore, these students would perform worse than those in schools with fewer beneficiaries.

Ahmed and Arends-Kuenning (2002) also suggest that "minimum performance standards" should be incorporated into the new Primary Education Stipend programme design to encourage school performance of both beneficiaries and non-beneficiaries, echoing Skoufia's suggestion. They also recommend investment in school resources that could clearly improve further outcomes for beneficiary and non-beneficiary children alike. Quality of schools is an issue to be tackled in every developing country, particularly in Bangladesh where class size has reached the staggering average of 62 students per teacher in non-FFE schools and 76 in FFE schools in 2000 (Ahmed and Ninno, 2002). Ahmed and Del Ninno also examine learning outcomes, comparing FFE and non-FFE schools. They observe that students in both FFE and non-FFE schools perform better if the school has better facilities, more qualified teachers, and more incentives for teachers, thus indicating that any improvement in the achievement of

30 FFE cannot be considered a typical CCT programme since it did not transfer cash, but rations of food grains. Supposedly families could, by receiving the food grains, either transfer money from food consumption to other basic needs or sell the grains to increase their income. FFE in Bangladesh started in 1993 and in 2002 was replaced by the Primary Education Stipend programme which is similar to CCTs.

31 The only family characteristic controlled for in the model is the family expenditure of non-beneficiaries. The authors report that inclusion of parents' education did not change the result in any meaningful way.

students in FFE schools should be dependent on financing those inputs. These authors recognise the complementarity between demand- and supply-side interventions in ensuring that education for the poorest is effective.

Other authors share the idea that school quality must be taken into account if governments wish to make the most of educational outcomes from CCT programmes (Morley and Coady, 2003). Without consideration of school conditions, the effects of CCTs on education might be of little significance for disadvantaged children. On the other hand, as Barrera-Osorio et al. (2008) affirm based on a series of studies on the determinants of schooling decisions amongst parents and the role played by school quality, short-term policies focused on improving school quality apparently have no effect in increasing school participation amongst the poor. Following the same reasoning, Morley and Coady state that "even when such basic quality is available, lower utilization by children from extremely poor families is still observed" (Morley and Coady, 2003, p. 36). This leads us back to the point highlighted in previous section as to whether supply-side policies alone are sufficient to improve the education of poor children. A combination of social support to families alongside school quality programmes focused on the specific group benefiting from CCTs could be the most promising policy to overcome education inequality. However, CCTs should also be seen as relevant for such results to be justified as long-term investments to tackle poverty.

A qualitative study analysing the Nicaraguan CCT programme – *Red de Protección Social* (RPS) – also concludes that participation in the programme is not clearly associated with improvements in academic performance of beneficiaries, based on parents' and teachers' perceptions[32]. The authors consider achievement to be more related to self-esteem amongst children than to school participation in RPS alone (Adato and Roopnaraine, 2004). Since there is no quantitative study analysing the effects of RPS on learning outcomes in Nicaragua, clear-cut conclusions so far are quite difficult. However, a detailed quantitative study by Maluccio and Flores (2004) showing astonishing positive results in several other educational outcomes in Nicaragua calls into question how it is possible that learning is not being affected as a result of RPS. Based on an experimental design, Maluccio and Flores (2004) report a 17.7 p.p. increase in enrolment at the start of the academic year for children aged 7 to 13 who had not yet completed primary school. They show that effects were stronger for those in extreme poverty, almost erasing the correlation between children's enrolment and per capita expenditure. Effects were stronger also for younger children (aged 7 to 9), which is consistent

32 This conclusion was reached even though teachers in three of the six municipalities studied declared that they had observed some improvement in beneficiaries' academic performance and dedication to studies.

with the increase in enrolments at the right age. Attendance rates increased for the treatment group by 23 p.p. for children aged 7 to 13, again with stronger effects for the extremely poor and younger children. Even those children who were attending school before the programme implementation became more assiduous in attending classes. The authors also assess pass-grade rates over two years of RPS operation, finding a significant overall increase of 6.5%. The figures reach 7.7% for first grade and 11.1% for fourth grade[33]. Looking at these results by the level of poverty, pass-grade rates improve more for the poor (9.0%) than for the extremely poor (5.2% at 10% significance level). Increase in transition from fourth to fifth grade by 12 p.p. is also observed. Finally, a reduction of 4.9 p.p. (at 7% significance level) in child labour is estimated for children aged 7 to 13 who had not completed the fourth grade. An estimated effect of 0.9 more years of schooling is also reported in the literature (Morley and Coady, 2003; Bradshaw and Víquez, 2008), although a continuation of RPS beyond the four years of coverage (first to fourth grade) is assumed in the estimation. How is it possible that RPS has affected outcomes ranging from enrolment and attendance to grade progression and transition without any effect on students' learning?

Unfortunately, Maluccio and Flores (2004) do not test RPS for results in learning outcomes. But Nicaragua is not the only country where significant educational outcomes as those described above have been reported in the literature as associated with CCT programmes. It is interesting that although no effect on learning outcomes has been found to be associated with participation in CCTs so far, several studies report effects on at least one educational outcome that is potentially linked to learning outcomes, for instance, grade promotion or transition rates. These studies are also suggestive of why learning outcomes are missing amongst the contributions of CCT programmes. One possible explanation is that schools are promoting beneficiaries without any concern about achievement. Another is that effects on learning have been blurred by circumstances surrounding the studies hitherto not satisfactorily sorted out by researchers.

Progresa-Oportunidades in Mexico is, by far, the most studied CCT programme to date. Although researchers have not documented effects on test scores, as mentioned earlier (see Behrman, Sengupta and Todd, 2000), several positive educational effects have been reported. Behrman et al. (2005) analyse grade progression, dropout rate, re-entry rate, and transition to secondary education. The general result points to a beneficial effect on the educational accumulation process. Participation in the programme is associated with higher enrolment rates, less grade repetition and better grade progression, lower dropout rates, and higher

33 Differences are positive but not significant for grades two and three.

school re-entry rates amongst dropouts. A significant reduction in dropouts between primary and secondary school is also observed. The authors state that the absence of spill-over effects support the conclusion that the observed impacts of the programme are due to families' responses to the incentives (demand-side effects) rather than supply–side improvements in schools[34]. The simulation proposed by Behrman et al. (2005) of long-term effects suggests a 21% increase in the number of children enrolled in junior secondary schools and 0.7 more years of schooling for participants. As in the case of Nicaragua, several educational effects potentially linked to learning outcomes are also found in Mexico.

In a 2007 study on *Progresa/Oportunidades*, Behrman et al. (2009) use a follow-up survey to assess the educational effects on children who were aged 0 to 8 at the time when they started in the programme (5 1/2 years earlier). They find a 0.9% reduction in the age of entry into primary school for girls who benefitted from *Progresa's* nutrition and health components for 1.5 years, and a 1.3% reduction for those who benefitted for 5.5 years compared to non-beneficiaries. An increase in grades of schooling completed until 2007 is also found — 8.6% for girls and 9.5% for boys aged 12 to 14 (an accumulation of 0.5 grades) after 5.5 years of benefits – but no effect for 1.5 years of participation. They also assess grade progression, finding an 11% increase for girls and 16% for boys aged 12 to 14 after 5.5 years of programme exposure, while no effects are found when measured for just 1.5 years of exposure. This study shows an important result, that different lengths of time of exposure matters for the detection of the educational effects of the programme.

Using the experimental settings of *Progresa/Oportunidades*, Fernald et al. (2009) assess, 10 years after the start of the programme, whether differences in educational outcomes can be observed due to differences in time of exposure. An 18-month difference in exposure between those families first included and those included later is the basis upon which to analyse effects on several outcomes, including the verbal and cognitive development of children aged 8 to 10 in 2007. The authors find no effect due to differences in time of exposure; instead, they find differences in verbal and cognitive scores associated with the amount of cash transferred to households during the time of participation in the programme. They also consider 18 months difference in exposure too short a time to enable effects on cognition and language to be detected. Attanasio et al. (2010) criticize their approach, arguing that higher cumulative cash amounts are very likely to reflect those families whose children

34 Different from most CCT programmes, *Progresa* has a supply-side component aimed at improving school resources in beneficiary areas.

made it to secondary school[35] over the years due to factors other than the cash transfer itself, for instance, higher ability and better development, which could create a reverse causality and invalidate the conclusions of the authors.

In another CCT programme in Cambodia – the CESSP[36] Scholarship Program[37] (CSP) – Filmer and Shady (2009) find significant effects on school enrolment and attendance. Beneficiary children are 25 p.p. more likely to enrol than non-beneficiaries, spend more hours in school, their parents can afford more educational expenditures, and they also tend to progress on time and attain more schooling. However, after 18 months, no differences are observed between beneficiaries and non-beneficiaries in mathematics and vocabulary tests. The authors also look at schools with low intakes of beneficiaries to assess if overcrowding could explain the lack of results in test scores. Beneficiaries do not fare any better in these more advantaged schools. Filmer and Shady argue that children staying in school due to CSP are drawn mainly from the bottom of the cognitive ability distribution, and that school quality might also be an important variable defining test score results. In addition, complementary programmes focused on learning for low-achievers should be in place as well. The authors recognise that 18 months is perhaps too short a period to allow the effects of CSP to be perceived in measurements of learning. They recommend future research on medium and long-term impacts of demand-side incentive programmes. In another paper (Filmer and Schady, 2011), the authors analyse whether differences in the value of cash produces effects of different magnitudes on school attendance. They conclude that for the two cash values paid in Cambodia (US$ 45 and US$ 60 per year), no difference is observed on the effect. Children whose families receive US$ 45, on average, have a 25 p.p. higher attendance rate than those not enrolled in the programme. However, attendance rates of children in families receiving US$ 60 do not differ from those in families receiving US$ 45. This study draws attention to another possible factor explaining educational effects so far not fully explored in the literature – the cash value paid to families.

3.4.2 What can explain the lack of evidence of effects on learning outcomes?

One could justify the scanty evidence of impacts on learning outcomes reported above by recalling that very few CCT programmes explicitly incorporate objectives related to

35 In *Progresa-Oportunidades* cash transfers increase when children graduate to secondary school.

36 Cambodia Education Sector Support Project.

37 Cambodia programme is conditional on school enrolment, regular attendance, and satisfactory grade progress and pay a small cash for three years of secondary school.

children's cognitive achievements[38]. Therefore, poor results in this respect would not compromise programme success, which is mainly related to enrolling and keeping children in school over a prescribed number of years. However, two points should be considered. First, as Reimers et al. (2006) emphasise, cash transfers conditional on education have in general two levels of objectives: (i) common general objectives including human capital accumulation, alleviation of poverty, and provision of a safety net and; (ii) education-specific objectives encompassing enrolment, attendance, and attainment increases. The first general objective and the last education-specific objective seem very unlikely to be reachable without a clear contribution of CCTs to students' learning achievements. Second, most of the authors studying CCT effects on learning outcomes (Behrman, Sengupta and Todd, 2000; Ponce and Bedi, 2010) argue that regular school attendance, together with improvements in children's well-being promoted by CCTs, should also improve the chances of children achieving better scores. Once children are better nourished and healthier, family consumption increases, and child labour tends to diminish for many CCT beneficiaries, their learning conditions at home and learning outcomes should also be expected to improve. Nevertheless, the theoretical expected effects on achievement have not yet been observed in studies published to date.

Reimers et al. (2006) seem to be the most severe critics of CCT programmes[39]. They argue that any gain that children might receive in the long term by participating in a CCT programme is likely to derive from the benefit of improved capacities and skills promoted by good education and real opportunities to take part in a labour market that values such capacities and skills. These capacities and skills do not naturally evolve from mere enrolment in school, or even from regular attendance. Much depends on the kind of learning experience schools are able to provide in pursuit of good education for children and, I would add, the kind of support children receive at home. The fact that several CCT programmes are funded by a share of education budgets raises the question of whether governments would be diverting public resources from relevant education policies to a new kind of social assistance without clear evidence of its educational benefits in the long run. In sum, what has been considered as the main long-term effect of CCT programmes – allowing poor children to escape the poverty trap by accumulation of human capital – is still not clear, despite the fact that it comprises the main political justification for public investments in most of the existing cases. Whether or not

38 Some CCT programmes such as *Programa de Asignación Familiar* (PRAF) in Honduras and New York Opportunities in the US, explicitly incorporate students' achievements objectives in their design. Also, the Cambodia Scholarship Programme is conditional on on-time grade progression in addition to enrolment and attendance.

39 Some criticism can also be found in the public debate by eminent scholars such as Gary Becker in "Bribe 'Third World' parents to keep their kids in school", Business Week, Nov 22, 1999; and Alan Krueger in "Putting Development Dollars to Use South of the Border", The New York Times, May 2, 2002.

CCTs can, in fact, influence human capital accumulation in the long term is yet to be tested, but an important element of these future effects is its immediate impact on learning outcomes while children are in school.

Why have learning outcomes not yet been detected? Is it, as argued by Ponce and Bedi (2010), because of the overcrowding effect on schools? Is it because beneficiaries are going to poor quality schools in the first place (Reimers, Silva and Trevino, 2006)? Perhaps, as argued by Adato and Roopnaraine (2004), low self-esteem amongst beneficiaries is playing a role, reinforced by teachers, peers, and even parents. Schools might also be failing to teach beneficiaries disproportionally more than they fail in teaching non-beneficiaries due to inadequate pedagogy and curricula. Relevant household issues might still not be resolved through cash transfers (e.g. parenting quality, home environment, intra-household relationships, etc.) (Fiszbein and Schady, 2009). The structure of the benefit might be inadequate to create the necessary incentives to promote improvements in learning outcomes or a longer time of exposure may be necessary for outcomes to be detected, as suggested by Skoufias (2006). Perhaps several of these factors can be put together to explain the *missing link* in the chain of the expected results of CCTs. Although these are plausible explanations, the reasons for the lack of evidence may well be related to methodological issues.

Several methodological aspects surrounding CCT studies can be raised to explain the *missing link*. First, very few studies have focused on learning outcomes, and generally rely on small samples that are unable to detect small effects. Second, relevant data on learning outcomes in many developing countries is inexistent, unavailable, or unreliable. Third, the complex nature of learning measures defies researchers who attempt to isolate effects on that variable. Finally, the lack of experimental conditions in most CCT studies requires the use of more complex statistical methods and that many assumptions hold. These aspects must be taken into account so that methodological limitations are not confounded with programme limitations.

Despite methodological difficulties[40], the studies reviewed shed some light on possible new approaches to be considered in research. First, the studies investigating CCTs' effects on students' achievement have thus far considered relatively short periods of time to measure learning outcomes. It might be the case that learning outcomes are not sensitive to CCT on such a short time scale, and longer intervals would be necessary to gauge learning effects. In those studies, there seems to be a 'trap of 18 months' confining attempts to measure effects

40 In chapter five I discuss in more detail the methodological context of research on CCTs and the challenges posed to assess these programmes in developing countries.

on learning outcomes. Such a short period generally relates to institutional constraints shaping many of the research designs. Timeframes defined by the governments commissioning research and programme implementation processes are the main obstacles to the consideration of longer intervals. The latter involves programmes achieving full coverage over short periods of time, thus erasing any possible experimental control group to be followed up with over time. Second, the value of cash paid to families might be an important variable to consider, along with time of participation, to allow for different weights cash transfers have on families' consumption patterns. CCT cash values as a proportion of families' incomes varies enormously across country experiences, ranging from 2% to 3% in Cambodia to 29% in Nicaragua (Filmer and Schady, 2011). Inasmuch as cash transfers are expected to counterbalance direct and indirect costs of education and to alleviate short-term poverty, educational effects will probably depend on the weight transfers have on families' income. These are two hypotheses so far not fully explored in the literature, and are areas that I explore in this research.

3.5 Towards the research justification

As mentioned in the previous section, research on CCTs has found evidence of the operational efficacy of these programmes as well as positive effects on several educational and other social outcomes (See Morley and Coady, 2003; Rawlings and Rubio, 2003; Rawlings and Rubio, 2005; Villatoro, 2005; Fiszbein and Schady, 2009). However, research has not yet produced any significant evidence as to any positive impact of CCTs on children's learning outcomes. This result conspicuously contrasts with evidence of the impacts of welfare and anti-poverty programmes in the US on learning outcomes discussed in the previous chapter. Positive educational effects were found in several social experiments over the last 40 years in the US, covering attendance rates, comportment grades, achievements on test scores, academic grades, high-school graduation, and years of schooling. Similar to some studies reviewed in this chapter, studies in the US found stronger educational effects for young children and those in lower grades, and also for those who were amongst the poorest. More importantly, both reviews show that effects seem to impact more those children whose families are enrolled for a longer time in the welfare schemes. The lack of evidence as to the impact on learning outcomes might be due to the time of participation allowed in CCT studies as compared to American income transfer programmes. While best-controlled evaluations of CCTs have been constrained within less than two years of programme exposure, the studies in the US invariably report results of learning outcomes for a time of participation that is more

than two years (generally three or more years). Besides, in the US studies, the value of cash was found to influence the level of results observed. The cash value, so far, has not been fully considered in evaluations of CCTs when assessing learning outcomes[41]. Perhaps some educational outcomes are more sensible than others with respect to the level of cash. For instance, the fact that income transfers in American welfare programmes reported in Duncan et al. (2011) range from values around US$ 800 to US$ 2,200 a year, while *Progresa-Oportunidades* in Mexico works with values ranging around US$ 24 and US$ 239 per year (Valencia Lomelí, 2008), might explain the different findings for effects on learning outcomes in the two countries.

Since school learning is a key variable leading to long-term social outcomes with respect to human and social development such as poverty reduction between generations, the lack of results can signal that CCTs are failing in delivering the promise of human capital accumulation amongst the poorest. This is a widely claimed objective underpinning the justification for social investments in CCT programmes. Failure to demonstrate results in this respect might not lead to the abolition of such programmes, given the significant results in reducing poverty and improving other aspects of families' lives. However, it might strengthen the criticism of CCTs as being mere hand-outs, with potentially undesirable side effects such as welfare dependency and the propensity to keep adults out of work with no long-term positive outcomes. In the end, such critiques might lead to a retreating of political support, which has strongly relied on the long-term objective of this kind of programme.

Given the methodological limitations encountered by researchers in their attempts to assess CCTs' educational outcomes, we cannot form a conclusive position as to the long-term effectiveness of CCTs. The question of whether CCTs have any impact on beneficiaries' learning outcomes is still open to research. However, conditions in place are quite limiting when researchers come to assess the potential social outcomes of CCT programmes in developing countries. These limitations can partially explain the apparent contradiction between the number of results reported in chapter two as to learning outcomes and the absence of results in the case of CCTs in developing countries. The only certainty until now is that those reports that indicate that CCTs have no effect on students' learning outcomes cannot provide definitive conclusions, since there are many methodological limitations regarding research designs and data availability for studies on the ground in developing countries. Thus, the key question is still unanswered: *Do children whose families receive conditional cash transfers*

41 Two exceptions are Fernald et al. (2009) and Filmer and Schady (2011) reviewed in the previous section. The first found positive effects on verbal and cognitive development but has been criticised as to its internal validity. The second did not focus on learning outcomes but on attendance rates.

benefit with respect to their educational achievements? This research explores this gap in the literature by investigating the case of *Bolsa Família* in Brazil.

Chapter 4. The Social and Educational Landscape in Brazil and *Bolsa Família*

4.1 Introduction

In this chapter I explore the social and the educational contexts in Brazil as two interwoven realities mirroring each other's inequalities. On one hand, Brazil has made remarkable progress over the last 20 years in expanding access to primary education across all social groups. On the other hand, many students do not complete primary school and never make it to secondary school. Those failing to do so are more likely to come from low-income families. Policy-makers have not paid attention to the fact that provision of educational opportunities for poor children cannot lead to positive long-term outcomes unless child poverty is tackled simultaneously. Social policies have only recently awakened to the necessity of integrating efforts to support poor families in supporting their children's development and education, one of the main characteristics of *Bolsa Família (BF)* programme. The programme's short-term impacts on poverty and income inequality are well recognised in the literature and are described in section 4.2.1. Education advances in terms of access to primary education, as discussed in section 4.2.2, although remarkable, have not been equally successful across social groups with respect to completion rates and progression to secondary school. *BF*'s main characteristics are introduced in section 4.3.1 and its educational effects as reported in the literature are presented in section 4.3.2. Although implicit in the programme's theory (section 4.3.3), the expected educational effects of *BF* that would allow children to escape the "poverty trap" in the long run, learning outcomes, and grade progression have not yet found support in the literature. This gap is the focus of this research.

4.2 Brazil's social and educational context

4.2.1 Poverty and inequality: Recent progress

With a population of nearly 190.7 million (84.4% urban and 15.6% rural), Brazil is a middle-income country (GNP per capita of US$ 11,185)[42], though it has high levels of absolute poverty (21.4%) and extreme poverty[43] (7.3%). Brazil's income inequality is one of the highest

42 Source: http://seriesestatisticas.ibge.gov.br/series.aspx?vcodigo=SCN55&sv=41&t=produto-interno-bruto-per-capita (Accessed on 07/02/2012). Reference 2010. Exchange rate:1.7 (BR/USD)

43 Poverty and extreme poverty measured as the share of population living in households with less than one-half and one-quarter of the minimum wage per capita in 2009. Source: IpeaData (http://www.ipea.gov.br/default.jsp)

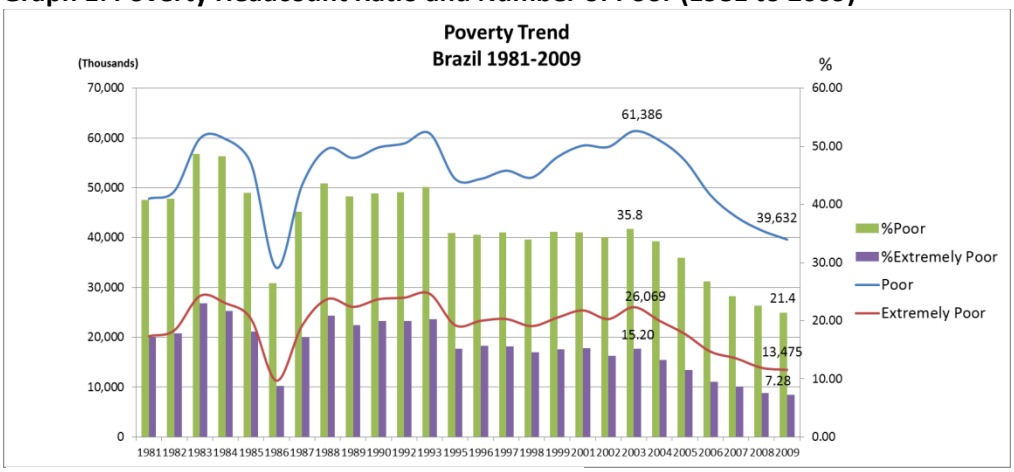
in LA and in the world. For 2009, the Gini index was 0.543, with the 10% richest appropriating some 43% of the national income and as much as 17 times, on average, the income of the 40% poorest[44]. Inequality in income distribution is considered one of the main reasons why Brazil remains a country with a significant share of the population living in poverty, despite the fact that the country achieved the position as the sixth largest economy in the world in 2011 and figures as a middle-income country amongst developing nations. Reduction of inequality has been considered a key factor in combating poverty, alongside sustainable economic growth.

Although the figures for poverty and inequality are high, they represent a significant improvement given historical trends. In fact, the persistent levels of poverty and inequality only recently started to decline, as shown in **Graph 1** and **Graph 2**. Recent studies have documented progress towards lowering the levels of poverty and inequality in Brazil, mainly after 2001 when CCTs gained national status (Ferreira, Leite and Litchfield, 2006; Soares et al., 2006; Oliveira et al., 2007; Osório et al., 2007; Haddad, 2008). Poverty and extreme poverty significantly decreased, with poverty rates dropping from 35.8% in 2003 to 21.4% in 2009, and extreme poverty falling from 15.2% to 7.3% in the same period. The Gini coefficient for individual income distribution started to fall from the steady historical levels of 0.600, achieving 0.543 in 2009, the lowest level in the series. Between 2002 and 2009 the Gini coefficient dropped at an average rate of 1.16% per year. The share of income appropriated by the top 10% relative to the bottom 40% dropped from 30 in 1989 to 16.7 in 2009, also the lowest in the series (Graph 2). These are unprecedented results, even when compared to historical experiences of developed countries in reducing inequality (Soares, 2008). This is a major achievement, considering the long history of stable poverty and inequality in the country over the last two decades of the 20th century[45].

44 Source: IpeaData at http://www.ipea.gov.br/default.jsp

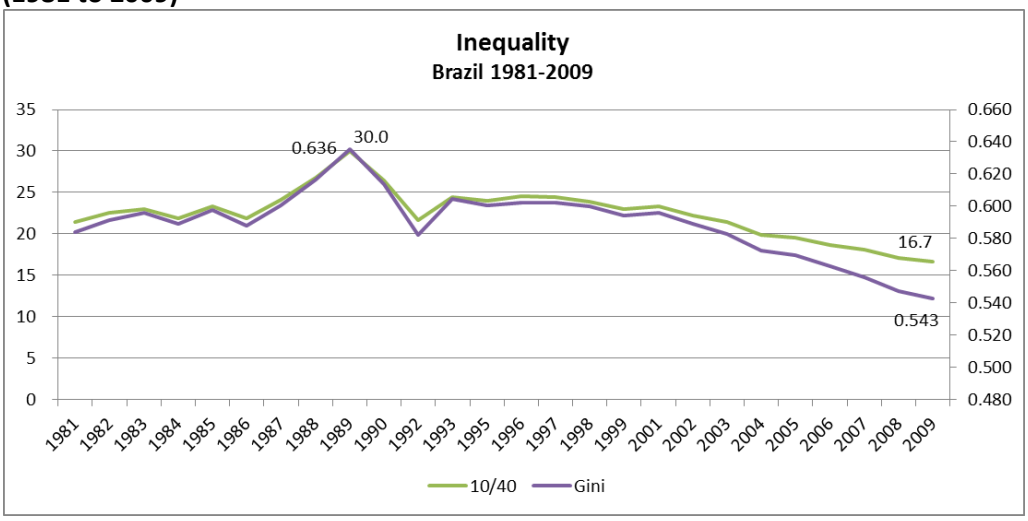
45 In respect to the long trajectory of stable high level of poverty, see, for instance, Barros et al. (2001)

Graph 1: Poverty Headcount Ratio and Number of Poor (1981 to 2009)



Data Source: IpeaData

Graph 2: Gini coefficient for individuals' income distribution and 10/40 income ratio (1981 to 2009)



Data Source: IpeaData

Amongst the main factors associated with the observed reduction in poverty and inequality, *BF* stands out in several studies that attempt to disaggregate the potential contributors to that reduction. For instance, Veras et al. (2006) estimate that, with 80% success in targeting the poor population, *BF* contributes to 21% of the observed reduction in the Gini coefficient between 1995 and 2004. Barros et al. (2006) conclude that social protection networks in Brazil are responsible for one-third of the reduction in income inequality in that same period. Ferreira et al. (2006) find that 71% of the reduction in inequality observed since 1993 occurred between 2001 and 2004, the period in which targeted CCT programmes were disseminated in Brazil. Hoffman (2006) also concludes that 28% of the reduction in inequality between 1997 and 2004 can be explained by governmental cash transfers like *Bolsa Escola-Bolsa Família*. Looking at the period of implementation of these two

schemes (2002 to 2004), the author finds a staggering 87% contribution of cash transfers to reducing inequality in Brazil's Northeast region, and an 86% contribution to national poverty reduction in that period. Results like these, in their own right, could justify *BF* in Brazil, given its effectiveness in changing the country's long history of persistent patterns of poverty and inequality. However, due to the connections between poverty, inequality, and education opportunities, as discussed in the previous chapters, more is expected from *BF* in contributing to long-term poverty reduction by immediately improving education amongst the poorest. Can *BF*, by reducing poverty and inequality, also be an effective policy with respect to human capital accumulation amongst the poor?

The answer to that question relies on the hypothesis that educational opportunities and outcomes can directly improve as a result of reductions in poverty and inequality. Reimers (2000a) highlights that income inequality is strongly associated with educational inequality in LA. Brazil stands out as the country in the region with the highest level of income inequality, and it also has the highest rates of educational inequality. However, the direction of the causal link derived from that kind of association is generally based on the assumption that schools are unresponsive and that educational processes are inadequate, resulting in the failing school experiences of children from low-income families and low levels of educational attainment. Restricted future choices with respect to jobs and reduced likelihood of escaping poverty in adulthood are the expected long-term outcomes. The main rationale followed by this line of thought is that inequality of educational opportunities explains the persistent patterns of poverty and income inequality in LA.

The arrow of causality pointing from unequal educational opportunity to poverty and inequality has dominated the academic debate in Brazil over many decades. The role of education in reproducing or reducing social inequality has been discussed in the economic literature about Brazil since the early 1970s. Pioneering studies by Fishlow (1972) and Langoni (1973) concluded that a major determinant of income inequality was unequal access to education. More recently, Barros et al. (2002) have shown that schooling heterogeneity in Brazil's labour force explains not only the huge gap in wage earnings, but also the significant inequality in the country compared to developed nations. However, those studies do not offer any explanation for existing inequalities in education. An explanation was attempted by Souza (1979), who pointed to regional disparities in educational expenditures and parents' education as the main variables determining education inequality in Brazil. Ferreira (2000) goes further, arguing that educational heterogeneity not only causes income inequality, but provokes inequality in political power. This, in turn, reproduces and reinforces inequality in educational

opportunities. A reverse causality between poverty and educational opportunity was proposed based on the association between economic and political power. Barros et al. (2001) investigate potential determinants of low educational attainment in Brazil (based on years of schooling) amongst children and youths aged 11 to 25, looking at four factors: the availability and quality of educational services, labour market attractiveness (time opportunity cost), household resources (financial and non-financial), and community resources. Amongst those factors, household resources are found to be the most relevant, with household per capita income and parents' education (mainly the mother's) being the most influential amongst them[46]. Parents' education is not only related to the household's permanent income, thus determining long-term trends in children's schooling, but also to lower costs in providing education to children[47]. Family income and parents' education are thus recognised as two of the most influential extra-school factors affecting children's educational outcomes in Brazil. The reverse causality was made evident by these last studies, suggesting that policies focused on the demand-side could be as important as those focused on supply-side provisions. The evidence for that causal link and the mechanisms behind it remain to be investigated further by scholars.

The extensive literature reviewed in chapters two and three suggest that positive effects of anti-poverty programmes on the educational opportunities of disadvantaged children can be expected. The fact that a disproportionate number of children from low-income families drop out before graduating from basic education, or are systematically retained in the same grade over years before finally abandoning school, cannot be fully attributed to educational policies and schools. By ignoring the influence of poverty on the educational opportunity of children, progress in poverty reduction in the long run will hardly be achieved.

4.2.2 Education access in Brazil: Successes and failures

School participation in Brazil has achieved major progress over the last 20 years, achieving enrolment rates comparable to many developed countries for compulsory education[48]. In 2009, the participation rate for those aged 7 to 14 reached 98.1% (**Graph 3**). It

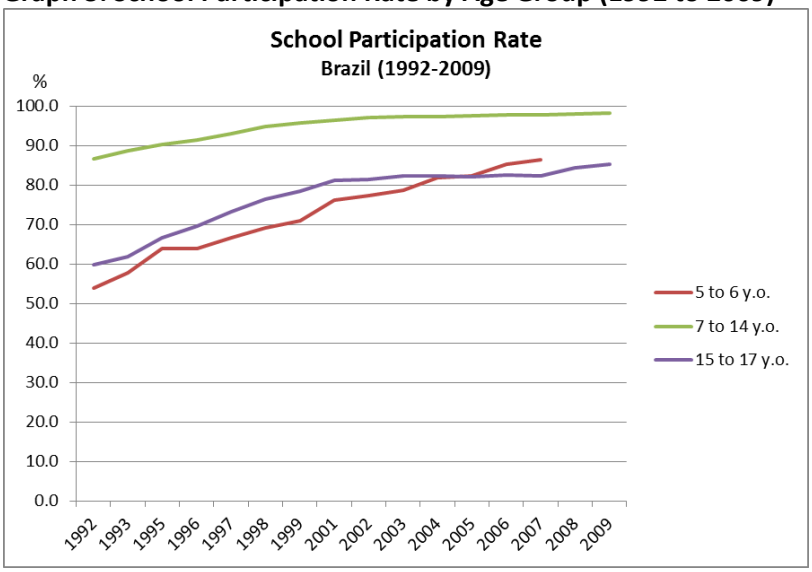
46 The authors estimate that an additional year of parents' schooling entails 0.3 years more of children's schooling. They also find that parents' schooling is even more influential than family per capita income (one year of a parent's schooling being as relevant as a R$ 340 increase in family income).

47 Lower costs here means that children's learning is facilitated by parental support at home, availability of books, access to cultural events, social interactions that reinforce the value of education and potentially raises the demand for schooling.

48 Until 1971 compulsory education in Brazil comprised only 4 years (primary school). In 1971 compulsory education was extended to 8 years and formed what was then designated "1o grau" ('first degree') and all children aged 7 to 14 were supposed

is interesting to note that school participation rates for children aged five to six observed a steep increase over that 20-year period, outstripping the rate for youths (ages 15 to 17) in 2006, with the latter group starting to catch up in 2008 after six years of stagnation around 82%. The graph also shows a clear participation gap between those aged 7 to 14 and those aged 15 to 17, indicating that many students do not move on to the secondary level.

Graph 3: School Participation Rate by Age Group (1992 to 2009)



Data Source: IBGE – Séries Estatísticas

Most of the recent progress in expanding access to compulsory education occurred in the 1990s when the participation rate (for 7 to 14 year olds) rose from 87% (1992) to 96% (1999). As shown in **Table 1**, progress was driven by the inclusion of children from the poorest income groups. An increase of 16 p.p. for the group living below the poverty line between 1992 and 1999 and a declining difference for upper income groups show that disadvantaged children have increased school participation proportionally more during the 1990s. By the end of that decade disadvantaged children had achieved enrolment rates closer to those in the upper income groups. The enrolment gap between those living below the poverty line and the upper income group fell from 22 p.p. in 1992 to 6.5 p.p. in 1999, a 70% reduction in seven years.

to attend school. In 1996 the new National Law of Education reorganised basic education in three stages: Infant Education (0 to 6 years old), Fundamental Education (7 to 14 years old), and Secondary Education (15 to 17 years old), but only Fundamental Education was compulsory. In 2005 an extra year was introduced by new legislation (Law No 11.114), mandating that parents enrol children aged 6 in Fundamental Education, thereby extending compulsory education to 9 years and redefining infant education for those young children aged 0 to 5 (Constitutional Amendment No 53). In 2009, another Constitutional Amendment (No 59) made education compulsory for all children and youths aged 4 to 17, giving education systems until 2016 to adapt and re-organise themselves to comply.

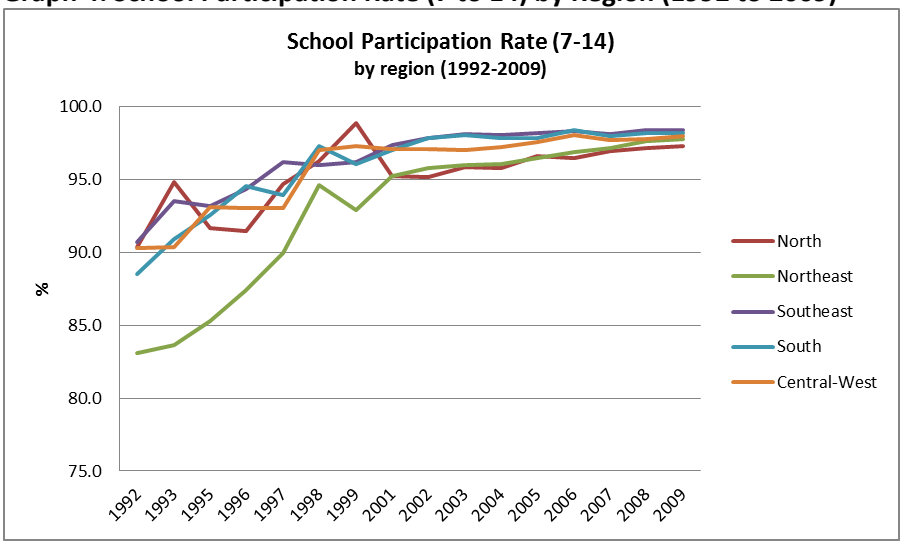
Table 1: School participation rate for children aged 7 to 14 by household per capita income as fraction of minimum wage – 1992/1999 [49].

	Groups by monthly household income per capita (m.w.)											
	Total[(1)]		Up to ½ m.w.		1/2 to 1		1 to 2		2 to 3		> 3	
	1992	**1999**	**1992**	**1999**	**1992**	**1999**	**1992**	**1999**	**1992**	**1999**	**1992**	**1999**
School participation rates for children aged 7 to 14	86,6	95,7	76,8	92,8	90,0	96,0	95,5	97,7	97,4	98,6	98,6	99,3
Difference (p.p.)	9,1		16		6		2,2		1,2		0,7	

Data Source: PNAD 1992/1999
(1) Including households which head has no income or no declared income.

Despite regional disparities in Brazil, compulsory school participation also achieved high rates in the less developed regions of the North (97%) and Northeast (98%) by 2009 — the same level achieved for the more developed regions of the Central-South (**Graph 4**).

Graph 4: School Participation Rate (7 to 14) by Region (1992 to 2009)



Data Source: IBGE – Séries Estatísticas

The net enrolment rate for fundamental education[50] in 2008 was as high as 95%, and was relatively equal for all income ranges[51] (94% for the first quintile and 96% for the fifth quintile) as well as for gender, race, locality (rural/urban) and region (Governo do Brasil, 2010). This relative success in providing access to fundamental education is reflected in the literacy rate, which by 2008 was as high as 97.8% for the population aged 15 to 24, and in the increasing mean years of schooling for males and females alike (**Graph 5**). However, the latter

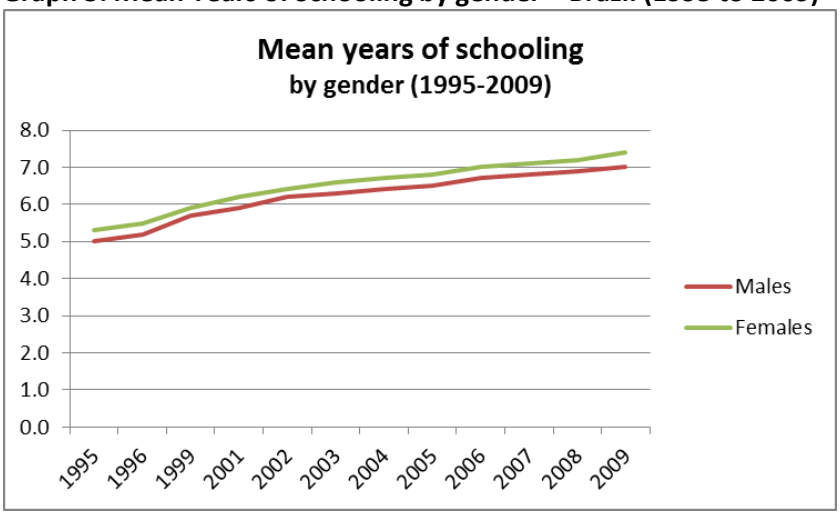
49 Source: Simões (2003)

50 Fundamental education is the compulsory education cycle, here meaning school grades one to eight.

51 All quintiles of income have had net enrolment rates above 91% since 2003.

indicator implies that if one randomly draws a Brazilian from the population, the highest probability is that she/he will not have completed eight years of compulsory education.

Graph 5: Mean Years of Schooling by gender – Brazil (1995 to 2009)

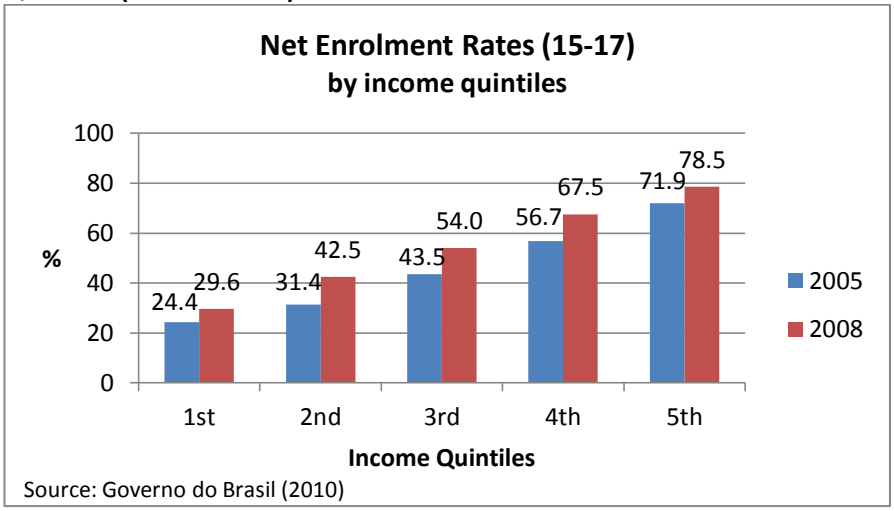


Data Source: IBGE – Séries Estatísticas

Although net enrolment rates are high in fundamental education, completion rates are low. For instance, the completion rates floated around 88% for the fourth grade and 54% for the eighth grade between 1996 and 2005, with no clear progress over the period of increase in the participation rate. Net enrolment rates at the secondary level for those aged 15 to 17 are still very low and remain unequal. For instance, in 2008 the net enrolment rate in secondary school was 50.4% and the gap between the Southeast and the Northeast was as large as 25.5 p.p.[52]. Also, a gender gap is observed in favour of females (56.8% versus 44.4% for males) as well as a racial gap in favour of Whites (61% versus 42.2% for Afro-descendants). The rural population is also at a disadvantage, with a net enrolment rate of 33.3% versus 54.3% for the urban population. Finally, differences by family income are telling, as displayed in **Graph 6**. Although some improvement can be observed, the net enrolment gap in 2008 still reached a staggering 49 p.p. between the bottom 20% and the top 20% of the income distribution (Governo do Brasil, 2010).

52 In the northeast, the rate reached 36.4% and in the southeast it reached 61.9% that year.

Graph 6: Net Enrolment Rates in Secondary Education by Income Quintiles (2005 to 2008)

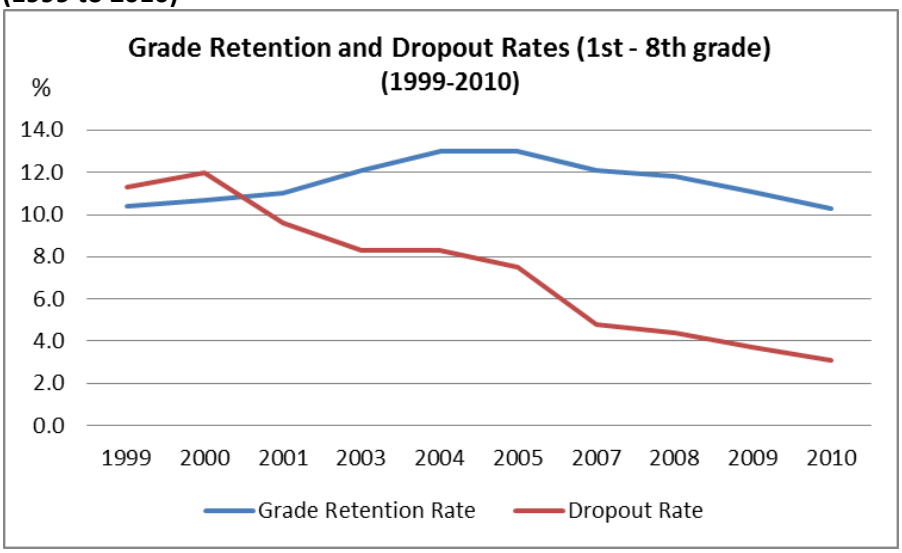


Source: Governo do Brasil (2010)

Low completion rates in compulsory education and gaps in access to secondary education indicate that some children are systematically repeating grades and dropping out before graduating from compulsory education; therefore, they are not continuing to the secondary level. In the north and northeast regions, for instance, half of those concluding the fourth grade drop out before concluding the compulsory fundamental school (Governo do Brasil, 2007). This suggests that the poorest groups of children and youths are still the most affected by dropout and repetition.

National trends regarding grade retention and dropout rates in fundamental education are shown in **Graph 7**. Increasing retention rates between 1999 and 2005 may reflect the massive influx of disadvantaged children into the education system in the 1990s, as previously mentioned. The dropout rate has continuously decreased since 2001, which might reflect the incentives created by Brazil's first national CCT programme, implemented in 2001, requiring a minimum attendance rate. Despite the increasing retention rates over the first years of CCT programmes, a reduction has been observed since 2005. This might indicate a positive effect of CCT programmes, although disentangling the potential factors behind such improvements is not simple. In the forthcoming chapters I investigate the hypothesis that *BF* in Brazil plays a role in that improvement.

Graph 7: Grade Retention and Dropout Rates in Fundamental Education (1999 to 2010)



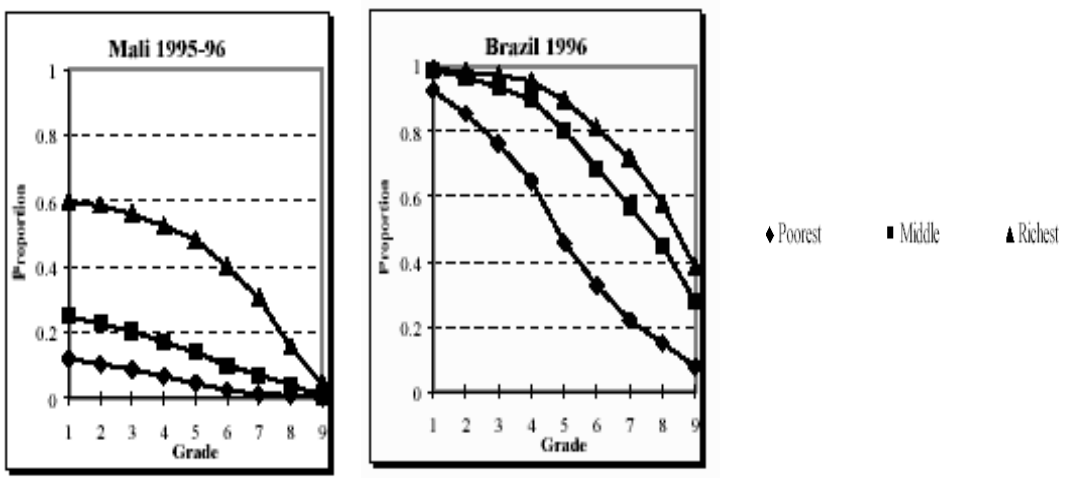
Data Source: IBGE – Séries Estatísticas

Despite the positive trends shown in Graph 7, disaggregation of those indicators by children's family income is not possible[53]. From an international perspective, however, Filmer and Pritchett (1998) demonstrate the existence of what they call the 'wealth gap', that is, differences in enrolment and educational attainment between the richest 20%, the 40% middle-income group, and the poorest 40% in terms of a proxy for income within several developing countries[54]. Using comparative analysis, they show that low primary school attainment by the poor is driven by two distinct patterns of enrolment and dropout: (i) poor children never enrol in school (Mali provides one example of this trend); (ii) enrolment is almost universal but dropout remains a key problem (Brazil is an example of this pattern, as shown in **Figure 6**).

53 The National School Census does not provide socioeconomic information for students; therefore, disaggregation of educational indicators (school inputs and outcomes) by household income is not possible.

54 The authors use data on household assets to create a proxy variable to overcome the lack of data on income or expenditure in the household surveys they use in the study.

Figure 6: Attainment profiles for ages 15 to 19, by economic group: Mali and Brazil



Source: Filmer and Pritchett (1998b)

Due to the 'wealth gap', increasing schooling amongst the poor is vital to the achievement of universal basic education. Filmer and Pritchett (1998) argue that rather than the availability of school facilities, it is access to quality schooling and households' demand for education that are the key constraining factors involved in the achievement of universal basic education in countries like Brazil. Although much progress has been achieved over the past 20 years in terms of the enrolment rates in primary school, even amongst low income groups, disadvantaged children are still more likely to repeat grades, progress slowly, and, often, abandon school before completion. Poverty and inequality are closely related to the quality of educational opportunities and educational attainment in Brazil. If the Millennium Development Goal of achieving primary education for all is to be achieved by 2015, the focus on equity has to be brought to the core of the national education agenda. CCT programmes emerged in Brazil as a strategy to help children from low-income families to enrol, attend, and complete basic education. *BF* is the programme currently in operation in Brazil and is the focus of the next section.

4.3 *Bolsa Família* programme

4.3.1 History and characteristics

Brazil has witnessed a remarkable process of social protection reform over the last 24 years, following the 'lost decade' of the 1980s and a long tradition of social protection, which was based on '*residualistic*' social assistance (mainly through subsidised short-reach

philanthropic action) for the severely poor and on a contributory social insurance system for formal sector workers. Triggered by the National Congress' approval of the new Constitution (1988) and the *Organic Act of Social Assistance* (*LOAS*) (1993)[55], the reform was marked by increasing decentralisation and participation in decision making[56], expansion towards a universal and publicly-funded system, and integration of cash transfer programmes conditional on education, health, and nutrition into social assistance schemes (Vaitsman, Andrade and Farias, 2009). In the early 2000s a three-pronged social protection system started to emerge, encompassing renewed social assistance, food and nutritional security, and cash transfer programmes. This new paradigm marks a departure from the '*residualistic*' approach to poverty alleviation; it represents a move towards a developmental approach to social and productive inclusion of the poor as a social right. Cash transfer programmes have played an important role in this reform, culminating in the current *Bolsa Família* programme.

The first national CCTs in Brazil were created in the wake of a new wave of social protection programmes. The *LOAS* regulated article 203 (V) of the Constitution, providing a universal benefit of one minimum wage for all citizens over 65 years of age and disabled persons living below the extreme poverty line (one-quarter of the minimum wage) – the *Continuous Cash Benefit* (*BPC*). *BPC* was implemented in 1996, covering 346 thousand beneficiaries, and was extended over the last decade, reaching 3.8 million people in 2011 (R$ 24.6 billion / US$ 12.3 billion)[57]. Amongst the disabled, two-thirds are children and youths below 24 years of age. Along with the *Rural Pension* – a non-contributory pension paid to rural workers whose livelihoods are based on family economic activity – *BPC* is responsible for reducing the incidence of poverty amongst the elderly from around 65% to less than 10% (Barros and Carvalho, 2003).

In addition to *BPC*, two other cash transfer programmes created in the late 1990s are worth mentioning due to their connections to child protection and alleged conditionality on education: the *Child Labour Eradication Programme* (PETI) and *Youth Agent*. PETI was created in 1996, combining monthly cash transfers to families (R$ 25/child in rural areas and R$ 40/child in urban areas) with the requirement of school enrolment and attendance at extended school activities for participant children below the age of 16 engaged in the worst

55 In addition, in 1995 the First Social Assistance National Conference was held in Brasilia, introducing the theme of social assistance as a basic citizenship right and discussing two main topics: (i) decentralised and participatory social assistance systems; and (ii) financing and public-private partnerships.

56 For instance, several National Social Assistance Conferences were held as of 1995 and a significant increase in the resources transferred to local governments to offer social assistance services occurred after the approval of the National Social Assistance Policy in 2004.

57 See MDS (2011).

forms of child labour[58]. Starting as a pilot project, it reached one million children in 2005 (Ipea, 2007) when the process of incorporating PETI into *Bolsa Família*[59] began. The *Youth Agent* programme was created in 1999, focusing on youths aged 15 to 17 whose families were living below the poverty line (half of one minimum wage). It was designed to create opportunities for community engagement and capacity building in socially relevant activities, such as those related to health, culture, tourism, environment, sports, and citizenship. A monthly grant of R$ 65 was paid over one year of participation in the programme when the beneficiaries attended six months of training programmes and were engaged the following six months in field activities. A major objective was to strengthen the youths' bonds with their communities and to create incentives for their continuation in the school system. The programme covered 112 thousand youths per year by the time it was replaced by a new program, *Projovem* (2007), which aimed to achieve the participation of 1 million youths by 2010.

In 2001 the Federal Government launched two new CCT programmes supporting children aged 0 to 6 – *Bolsa Alimentação* (Food Grant) – and children 6 to 15 – *Bolsa Escola* (School Grant). The *Bolsa Alimentação* programme was aimed at reducing nutritional deficiencies in families living below the poverty line and was conditioned on children's immunization and monitoring of their development, and parents' participation in prenatal care and educational activities related to health care and nutrition. The programme transferred a monthly cash allowance of R$ 15 per child in the household, up to a limit of three children. The programme benefitted 1.7 million families in 2003 when it was merged with *Bolsa Família*. The National *Bolsa Escola* Programme was inspired by the first local experience of cash transfer programmes conditional on education, developed in 1995 in the Federal District[60], and by the minimum income guarantee programme developed in the city of Campinas (SP). *Bolsa Escola* started payments in June 2001 and quickly reached all municipalities in the country. By June 2003 there were 5 million families and 8.3 million children participating in the *Bolsa Escola* programme. The main objective of *Bolsa Escola* was to "promote education of children living in low-income families by increasing their school attendance" (Governo do Brasil, 2001). The programme guaranteed cash grants to families living below the administrative poverty line (R$

58 Initially, PETI covered children working in tea plantations and charcoal ovens in the state of Mato Grosso do Sul and then was extended to sugar cane, sisal and citric fruit plantations, and gold-digging fields in several other states.

59 Portaria No 666 (28/12/2005) and Instrução Operacional Conjunta SENAR/SNAS No 1 (14/03/2006).

60 The conditionality on education was first proposed by Professor Cristovam Buarque in the late 1980s, who would become governor of the Federal District in 1995. He implemented the first *Bolsa Escola* programme, which was followed by implementation by several other local and state governments. Between 1995 and 1999 seven states in Brazil created similar programmes (Amapá, Mato Grosso do Sul, Alagoas, Minas Gerais, Rio de Janeiro, Goiás and Acre) as did dozens of municipalities throughout the country.

90 per capita), provided that they enrolled and kept children aged 6 to 15 in school. The grants ranged from R$15 to R$45[61] per month, depending on the number of children registered in the Program (R$15 / child to a maximum of three). A minimum attendance of 85% was required for families to receive the grants. If a child attended less than 85% of school days in a month, 15R$ would be deducted from the family's cash amount until the child recovered the minimum 85% attendance required. As the largest cash transfer programme in operation in 2003, *Bolsa Escola* beneficiaries were the first to be transferred to the newly created *Bolsa Família* programme in October 2003.

The 2003-2010 period was marked by a strong commitment from Brazil's new government to fight hunger and to reduce poverty and inequality. A new cash transfer programme was created under the umbrella of the *Fome Zero* (Zero Hunger Plan), focused on families living below the extreme poverty line – the *Cartão Alimentação* (Food Card) programme. The programme was introduced and articulated with other initiatives to assure food security amongst poor families, and included nutritional education, family agriculture support, food distribution, food stocks, community green gardens, community kitchens, popular restaurants, and school meals. The large expansions of the previous cash transfer programmes and the new umbrella plan posed coordination challenges for the new government. Thus, the new Ministry of Social Development and Fight Against Hunger was created in 2004 and the diffuse array of social programmes were brought under its coordination, including CCTs, food security actions, and social assistance programmes previously run by different ministries. The process of integrating the cash transfer programmes gave rise to *Bolsa Família (BF)*.

In October 2003, under a new government, a Provisional Act[62] initiated *Bolsa Família.* The Act became a Federal Law, passed in the National Congress in January 2004[63]. It integrated four existing cash transfer programmes, all created between 2001 and 2003: *Bolsa Escola* (School Grant), *Bolsa Alimentação* (Food Grant), *Cartão Alimentação* (Food Card), and *Auxílio Gás* (Gas Subsidy)[64]. In 2006, PETI was also merged into *BF*. *BF* was initially targeted at an estimated 11.1 million families living in poverty or extreme poverty in Brazil. It was created as part of *Fome Zero* (Zero Hunger), a flagship policy in Luiz Inácio Lula da Silva's government

61 Approximately US$ 7.50 to US$ 22.50.

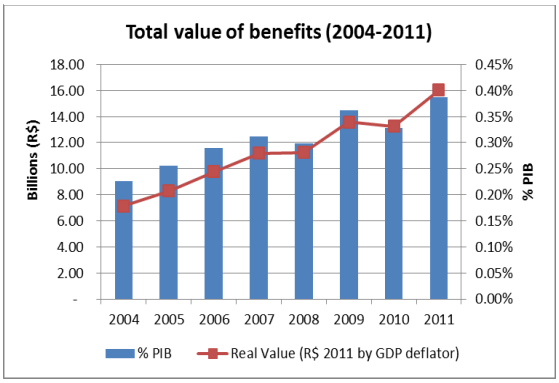
62 Provisional Act Nº 132, Oct 20, 2003 (link: http://www.planalto.gov.br/ccivil_03/mpv/Antigas_2003/132.htm)

63 Law Nº 10.836, Jan 09, 2004 (link: http://www.planalto.gov.br/ccivil_03/_Ato2004-2006/2004/Lei/L10.836.htm)

64 A good summary of the integration process that generated *Bolsa Família* can be found in Hall (2006)

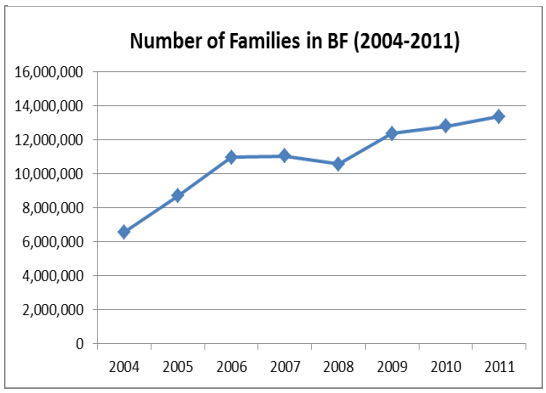
(2003 to 2010) that continues under Dilma Roussef's government (2011 to 2014)[65]. The main *BF* objectives are: (i) to strengthen access to public services, particularly to education, health care, and social assistance; (ii) to fight hunger and to promote nutritional and food security; (iii) to enhance the ability of families to overcome vulnerability and poverty; (iv) to fight poverty; and (v) to promote integration, complementarity, and synergy of social policies[66]. The *BF* programme budget was R$ 16 billion in 2011 (US$ 9.4 billion), representing 0.39% of the GDP, and there are now 13.3 million families participating in the programme[67]. The size of the initial target population was increased, not because the number of poor has increased, but because the threshold to be included in *BF* has changed twice since its creation; from a monthly per capita income of R$ 100 (US$ 35[68]) in 2004 to R$ 120 (US$ 57[69]) in 2006[70], and finally to R$ 140 (US$ 64[71]) in 2009[72]. The threshold to be considered in extreme poverty is half of the previous figures. The programme evolution in terms of total cash transferred to families and the number of participant families is displayed in **Graph 8** and **Graph 9.**

Graph 8: Evolution of *BF* cash transfers



Data Source: IpeaData

Graph 9: Evolution of *BF* participation



Data Source: IpeaData

Bolsa Família cash transfers to participant families vary according to the family's level of income and to the number of eligible family members, which include children aged 0 to 15, youths aged 16 to 17, and expectant or breast-feeding mothers. Cash transfers have two

65 Under Roussef's government *Fome Zero* was assimilated into a new political brand *Brasil Sem Miséria* (Brazil without Extreme Poverty) and its policies related to food security and nutrition integrated into the new National Plan for Food Security and Nutrition 2012-2015. The *Bolsa Família* programme, however, has survived the transition as one of the main social policies integrating the *Brasil Sem Miséria* Plan.

66 Decree No.5209/2004.

67 See http://www.mds.gov.br/adesao/mib/matrizviewbr.asp?

68 Exchange rate US$ 1= R$ 2.9 (Apr 2004)

69 Exchange rate US$ 1=R$ 2.1 (Apr 2006)

70 Presidential Decree No.5749 / 2006

71 Exchange rate US$ 1=R$ 2.2 (Apr 2009)

72 Presidential Decree No.6917 / 2009.

components: one basic flat rate paid to families living in extreme poverty (an administrative cut-off point of R$ 70 per capita income), independent of family composition, and an additional variable component, which depends on family composition. Families living with a monthly per capita income of less than R$ 140 are eligible for the variable component. **Table 2** describes the current components of the cash transfer[73] according to the families' level of per capita income and to the number of eligible family members. Different family compositions lead to different combinations of benefits, meaning that the total monthly transfer to families living in extreme poverty varies from R$ 70 (US$ 41.20[74]) to R$ 306 (US$ 179.90) and for families living in poverty from R$ 32 (US$ 18.80) to R$ 236 (US$ 138.70).

Table 2: *Bolsa Família* programme: Criteria and benefits

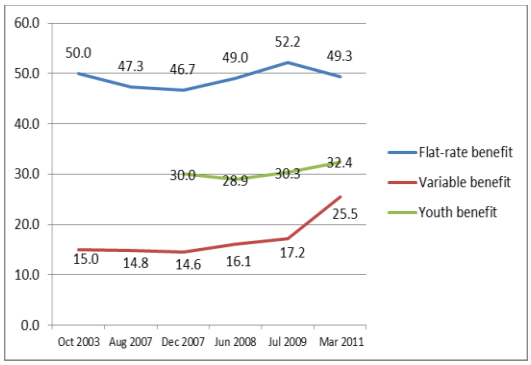
Criteria		Expectant or breast-feeding mothers / Children aged 0 to 15	Cash Transfer	Youths aged 16 to 17	Cash Transfer
Family Situation	**Household per capita Income**				
Families in poverty: receive variable benefit	From R$ 70 to R$140 (US$ 41.20 to US$ 82.40)	1 individual	R$ 32 (US$ 18.80)	1 youth	R$ 38 (US$ 22.35)
		2 individuals	R$ 64 (US$ 37.60)	2 or more youths	R$ 76 (US$ 44.70)
		3 individuals	R$ 96 (US$ 56.40)		
		4 individuals	R$ 128 (US$ 75.20)		
		5 or more individuals	R$ 160 (US$ 94)		
Families in extreme poverty: receive variable + fixed benefit	Up to R$ 70 (US$41.20)	Even if no children/youths and no expectant/breast feeding mother in the family	R$ 70 (US$ 41.20)		

Since the creation of *BF*, several alterations of the benefit values occurred. **Graph 10** shows the change in the real values of the benefit components and **Graph 11** shows the accumulated real increase by component since the creation of the programme. It can be seen that for the flat-rate component, the increases basically maintained the benefit's real value established at the start of the programme (R$ 50). In the case of the variable component for children aged 0 to 15 (and mothers), there has been an increase in real terms of about 70% since the start of *BF*. A lower real increase rate is observed for the youth component introduced in 2007 (8%) and the maximum cash transfer has increased by 168%.

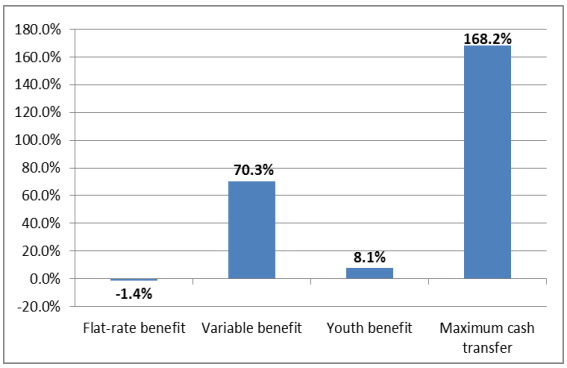
73 Current benefit values according to Decree No. 7447/2011.

74 Exchange rate US$ 1=R$ 1.7 (May 2012)

Graph 10: Evolution of benefit real values (R$) adjusted by the consumer price index (IPCA) 2003 to 2011



Graph 11: Accumulated real rate of variation by benefit component (2003 to 2011)



In *BF*, cash transfers are conditional upon several family responses towards the education, nutrition, and health care of mothers, children, and youths[75]. Families are required to ensure that children below seven years of age are vaccinated and make frequent visits to health care centres so their growth, nutrition, and development can be monitored. Pregnant women between 14 and 44 years of age are required to attend pre- and post-natal care visits as well as take part in educative activities on nutrition and breast-feeding. The programme also requires a minimum attendance rate of 85% of school days for children aged 6 to 15 and 75% for youths aged 16 and 17. For children who are at risk of or have just been withdrawn from child labour, a minimum attendance of 85% in extended school day activities are required. Families are instructed to inform school administrators whenever their child cannot attend class and must justify the absence. In 2009, an additional provision was created stating that the occurrence of child labour in the family shall potentially result in suspension or even cancelation of the benefit[76]. These conditions are seen as strategies to promote access to basic educational, health, and social assistance services. The monitoring of conditionalities focuses on encouraging vulnerable families to promote their children's basic social rights and to highlight public sector responsibilities to provide social services.

Bolsa Família has also been articulated with complementary programmes to promote families' autonomy and to help them to achieve self-reliance and decrease dependency on cash transfers. Coordinated actions with municipal officials, state governments, and several ministries in the Federal Government include adult and youth education (literacy and professional development), micro-credit programmes, family agriculture financing, and civil

75 Source: http://www.mds.gov.br/bolsafamilia/o_programa_bolsa_familia/condicionalidades/o-que-sao-condicionalidades.

76 Decree No. 7013/2009.

registration, amongst others[77]. These initiatives demonstrate that, instead of viewing *BF* as a self-sufficient programme to combat poverty, the Brazilian government recognises that poverty and social exclusion have multiple causes and that integrated policies are necessary to cope with the complex and interconnected chain of causes. Nevertheless, education remains paramount in the *BF* strategy to fight long-term poverty. Has the programme been at all effective in promoting education for children from low-income families?

4.3.2 The educational impacts of *Bolsa Família*: A brief review

Today, *BF* is by any measure the largest CCT programme in operation in developing countries. Nevertheless, it remains one of the least studied with respect to its impacts on the social development of children and families until very recently. Amongst the few papers that can be found on *BF*'s effects, many have focused on economic effects related to poverty, income inequality, and consumption (Soares et al., 2006; Osório et al., 2007; Resende and Oliveira, 2007; Tavares, 2008; Lignani et al., 2011), and more recently on adults' participation in the labour market (Tavares, 2008; Foguel and Barros, 2010). Studies of the impacts of *BF* on child development, health, and nutritional status of children can also be found in the literature (Morris et al., 2004; Castiñeira, Nunes and Rungo, 2009; Reis, 2010; Lima, Rabito and Dias, 2011; Paes-Sousa, Santos and Miazak, 2011; Piperata et al., 2011). As to effects on education and child labour, although there are very few studies to date, these were, in fact, the primarily areas of interest at the time of the first CCT experiences, when education was the heart of the programmes.

The first evaluation conducted on CCT programmes in Brazil was carried out by Waiselfisz et al. (1998) on the pioneer *Bolsa Escola*, implemented in 1995 in the Federal District. Based on that evaluation, the World Bank pointed out preliminary evidence of *Bolsa Escola*'s impacts on poverty and education. The first comprehensive report on a CCT programme by the Bank was released in 2001, the year the Federal *Bolsa Escola* programme was launched as a national programme in Brazil. The World Bank summarises the main findings:

> First, school attendance is higher and dropout rates are lower for beneficiaries. More beneficiary children enter school at the right age compared to their non-beneficiary counterparts. Second, as a consequence of good targeting, the program reduces the income gap between beneficiaries and non-beneficiaries, implying short-term poverty reduction. However, it has been difficult to ascertain the effects on child labor. This is

77 A full list of Federal programmes being articulated with *Bolsa Família* is described by Campos (2007)

> partly because in Brasilia, where the evaluation was undertaken, there is little child labor. (World Bank, 2001, p. ii)

Effects on child labour have been evaluated beyond the initial boundaries of the first *Bolsa Escola* experience in the Federal District. Bourguignon et al. (2003), working with simulation models of the demand for schooling, developed an *ex-ante* evaluation of the national *Bolsa Escola* programme. They tested families' behavioural responses regarding child labour and schooling (time-allocation decisions) against different cash transfer levels and income cut-off points (the means test of household income). Applying the model to *Bolsa Escola* characteristics, they conclude that 40% of non-enrolled children aged 6 to 15 would enrol in response to the programme, and this figure would be even higher for poor children (those targeted by the programme), reaching a 60% response rate amongst the non-enrolled. However, effects on child labour and current poverty were not detected. Another study involving an *ex-post* evaluation (Cardoso and Souza, 2004) analyses differences in child labour and school attendance rates between 1992 and 2001, covering *pre/post* periods of early municipal and state experiences of CCT programmes in Brazil. They also found that income transfer programs did not have significant effects on child labour, but had a positive and significant impact on school attendance. The authors conclude that the cash transfer levels were sufficient to induce school enrolment but not to curb child participation in the labour force.

One aspect regarding the previous analyses of CCTs in Brazil is that they did not investigate experiences with national coverage, but were confined to either local or regional experiences. The first large-scale national experience – *Bolsa Escola* – lasted only three years before being integrated within the new *BF* programme in 2004 and very little can be found about its impacts. Janvry et al. (2007) have arguably developed the first systematic impact evaluation of the Federal *Bolsa Escola*[78]. In their study of local government characteristics and *Bolsa Escola* efficiency, they attempt to isolate those variables that better correlate with the programme's expected outcomes. Using data covering 1999 to 2003, the authors reported that *Bolsa Escola* strongly influenced dropout rate reduction (by 7.8 p. p.)[79], although grade failure increased by 0.8 p.p. More students were completing the academic year while a small share of them received failing grades. However, the net effect was a decline in grade retention by 6.2 p.p. Nevertheless, the authors identified that 21% of *Bolsa Escola* beneficiaries were either dropping out or failing. They recognised the need for supply-side improvements so that

78 A study on Bolsa Escola was developed in 2005 using data from the National Household Survey by Schwartzman (2005)

79 This effect was found to be larger for students on night shifts who are, in general, older than 15 years old and have experienced dropouts or grade failures before.

schools could help to retain more beneficiaries and help them to succeed. Janvry et al. also identified implementation practices and local features associated with different levels of impact on those educational variables.

Despite the studies mentioned, authors such as Janvry et al. (2007) and Hall (2008) point out the lack of systematic impact evaluations on national CCT programmes in Brazil since the first national programme was adopted (*Bolsa Escola*). In particular, studies about *BF*'s effects on students' educational outcomes are quite rare. An exception is a recent paper by Glewwe and Kassouf (2012) analysing a long-term panel data covering the pre- and post-implementation periods of *Bolsa Escola-Bolsa Família*. Data from the National School Census between 1998 and 2005 shows that the availability of the programme had an impact at the school level by augmenting the enrolment rate, diminishing the dropout rate, and increasing the promotion rate. The latter is associated with better school performance, though not assessed in terms of national standards.

Commissioned by the Ministry of Social Development in 2005, CEDEPLAR[80] developed a preliminary impact evaluation of *BF* on several programmes' expected results, using health, education, employment, and expenditures as indicators between 2004 and 2005 (Oliveira et al., 2007). The educational variables studied were school attendance, dropout, progression, and time allocation between labour and school. The study finds that attendance rates tend to be higher for beneficiaries in comparison to those not taking part in similar programmes, especially for girls in the Northeast region. Also, positive impacts on dropout rates are reported for beneficiaries. However, in relation to school progression, the study points out that progression rates amongst beneficiaries are lower than the rates observed for non-beneficiaries. This result contrasts with the findings of Glewwe and Kassouf (2012) previously mentioned. Nevertheless, the authors point out that lower school progression might occur in the short term due to the fact that beneficiary drop-out rates are lower, leading to grade repetition. The researchers believe that subsequent measures could result in different evidence[81]. In fact, they use just a one-year interval in the analysis, while Glewwe and Kassouf (2012) use a seven-year panel in their study.

Bolsa Família's positive effects on school attendance are also estimated by Cacciamali et al. (2010) using data from the 2004 National Household Sample Survey. The analysis of the joint effects of *Bolsa Família* on the probability of attending school and of working is notable.

80 *Centro de Desenvolvimento e Planejamento Regional.*

81 A follow-up study (Brauw et al., 2012) shows impact on progression rates for children aged 6 to 17 of 7 p.p. (not statistically significant for boys and a significant 10 p.p. for girls).

Cacciamali et al. report a national reduction by 2.5% in the probability of idleness (neither working nor studying) amongst children aged 7 to 15 (a 3.5% reduction in the Northeast and a 4% reduction in rural areas). The estimates predict that beneficiaries have a lower probability of being only at work in three regions (the North, Northeast and Central-West). In the same three regions, beneficiaries are more likely to be only engaged in studying, although in the Southeast and Northeast the probability of both working and studying also increases. Nationally, *BF* increases the chances that a child will be only studying by 1.4% (1.6% in Northeast, 2.3% in the Central-West, and 3.5% in the North), although the chances that a child will be both working and studying increase by the same figure (1.4%).

Studies so far have indicated that CCT programmes in Brazil have a clear positive effect on increasing school attendance and reducing dropout rates, yet these are the immediate results expected from the educational conditionalities attached to the programme. Effects on child labour, however, are mixed and seem to depend on regional and household characteristics, as well as on the type of work children are engaged in. Less clear is the effect on grade promotion, which apparently varies depending on the timescale within which the analysis is conducted. Finally, we observe a complete absence of investigations on potential effects on learning outcomes, completion rates, or progression to secondary education. The focus of this research is on learning outcomes, one *missing link* of CCT effects on education both in Brazil and in international experiences.

4.3.3 Programme theory

As discussed in chapter three, there are several commonalities, including a common educational rationale, underpinning most of CCT programmes. Despite this, each CCT programme differs in how specific goals and objectives are framed in the policy discourse, as well as in design and implementation strategies. The specifics of CCTs include targeting structure, frequency and value of cash transfers, payment system, conditionality architecture, complementary programmes, agency organisation, and so forth (Fiszbein and Schady, 2009). Such differences are rooted in how CCTs are conceptualised by policy-makers, and in how they conceive the logic of the programme (what it intends to achieve and how it is expected to operate). The specific “manner in which a programme relates to the social benefits it is expected to produce and the strategy and tactics the program has adopted to achieve its goals and objectives” (Rossi, Lipsey and Freeman, 2004, p.64) is called the “programme theory”. As consequence, each CCT programme must be evaluated against its own context, design and programme theory (Weiss, 1972; Clarke and Dawson, 1999; Rossi, Lipsey and Freeman, 2004).

Evaluation should be based on individual programme rationales; it should account for the direction of change as well as policy-makers' views on expected results and how the programme will achieve them. Behind every programme theory there is an explanation about the social problem the programme intends to tackle and the way to achieve programme goals. The problem is that the programme theory is very rarely made clear, or even considered, in the policy process, making the task of evaluation even harder.

Bolsa Família is not different in that respect. The theory behind *BF* has not been explicitly described in any of the policy documents or studies available. Even the main objectives of the programme are framed, not in the Federal Law that created it, but in the Presidential Decree, making it easier to change and adjust objectives 'on the fly'[82]. The purpose of *BF*, as framed in Law No 10836, is to unify procedures for the management and execution of the federal cash transfer programmes created between 2001 and 2003. It clearly does not refer to any social objective or how to achieve it. Presidential Decree No 5209, which regulates the operational aspects of the programme, states the five programme objectives mentioned in section 4.3.1[83]. Those objectives are quite broad and certainly overlap with the objectives of several other programmes being implemented by the Federal Government in Brazil. Nevertheless, it is part of the evaluation process to reconstruct the logic and rationale of public programmes in order to assess design, implementation, and/or outcomes.

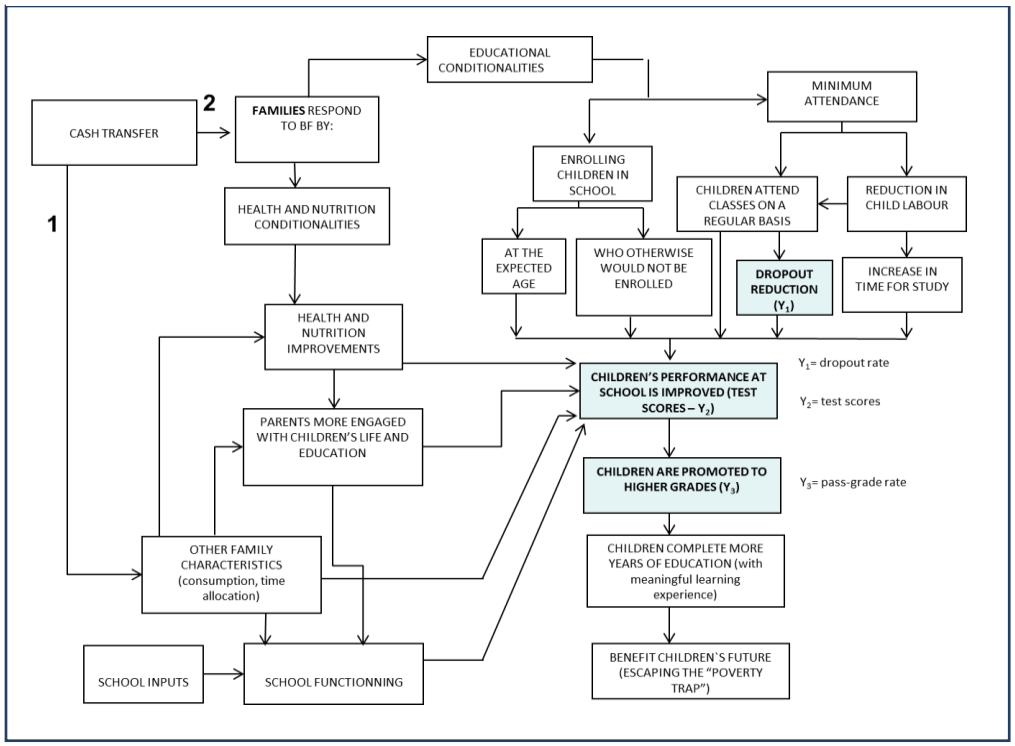
As far as an evaluation of outcomes is concerned, clarifying the programme theory helps in understanding the expected functioning of the programme, what it is expected to deliver, and how the delivery is expected to affect social outcomes. The programme theory helps to identify short-, middle-, and long-term objectives, and to determine how the programme's effectiveness can be assessed. It also helps to locate critical paths through which changes should be detectable, so that the intended outcomes can be made possible. The modelling of the programme theory results in a map of inputs, outputs and outcomes making explicit the chain of events that hypothetically will lead to the achievement of the programme's long-term objectives. That map ultimately describes the mechanisms the programme is based on, how it is expected to work, and its rationale.

82 The absence of clear statements about objectives is a very common problem in public policies and programmes; formulations are always changing. This makes it difficult for evaluators to be clear about what they should look at when evaluating results.

83 The lack of clarity in respect to programme objective was not any better in the case of the Federal Bolsa Escola programme, in which neither the Federal Law that created the programme (Law No 10219/2001) nor the Presidential Decree regulating it (Decree No 4313/2002) mentioned the programme objective. The Bolsa Escola objective is found in an operational manual (Bolsa Escola: manual de procedimentos, Brasília: Secretaria do Programa Nacional de *Bolsa Escola* / MEC) in which it is said that Bolsa Escola is aimed at "promoting education of children living within low-income families by increasing their school attendance."

A basic socio-educational rationale can be inferred from the context, history, and design of *BF*, as well as from the knowledge produced so far regarding its effects on education. **Figure 7** describes my understanding of the rationale of *BF*, with respect to educational outcomes and how the programme is expected to contribute to helping children to escape the "poverty trap".

Figure 7: Socio-educational rationale of *Bolsa Família*



The core delivery of *BF* is the cash transferred to families on a monthly basis, which is expected to trigger two chains of events (1 and 2 in Figure 7). Through chain 1, cash transfers are expected to have a direct impact on household patterns of consumption and investment by changing household capacities, behaviours, and choices. These changes are expected to improve children's environments, support, and learning conditions at home, ultimately impacting their school performance. First, cash transfers contribute to smooth consumption and compensate for the opportunity costs of having children attend school rather than working. Second, direct costs of education such as uniforms, shoes, transport, and school materials can be partially covered by the benefit, allowing families to send children to school without feeling demeaned or excluded[84]. Third, families can spend more on food in terms of quantity or quality, thereby diversifying food consumption and improving the nutritional and

84 An interesting aspect related to families' perceptions of BF cash transfer relates to what can be bought with the cash benefit. In a recent evaluation commissioned by the Ministry of Social Development (Brauw et al., 2012), almost all surveyed beneficiaries (89%) asserted that BF cash transfers must be used to buy school materials for children (a non-existent requirement in BF).

health status of children (central to children's learning conditions). Fourth, families may also be able to afford medicines for treatable diseases, reducing school absenteeism due to sickness. Fifth, cash transfers can help households to manage risks by reducing the incidence of negative coping strategies (e.g. cutting back on food or schooling; selling out assets; consuming seed grains and livestock) in the case of unexpected shocks (e.g. job loss, diseases, droughts, floods) that can affect children's education. Sixth, cash transfers can have an effect on credit available to families, since the mere possession of the *BF* electronic card works as a voucher in the local markets, allowing for either consumption or small investments in productive assets. Therefore, cash transfers can increase families' economic capacities, reduce the risk of child labour to supplement household income, and of parents borrowing against their children's future earnings so they can support their families in the present.

Many of these effects are expected because mothers are given preference as recipients of cash transfers, as they are recognised as being more reliable in allocating resources to benefit children. As the main recipients of *BF*, women are empowered as decision makers in the household. When women are empowered as decision makers, decisions about what and when to spend money, whether to use contraceptives, or whether children go to school or to work tend to be taken in the best interests of the whole family, particularly children.

Once cash transfers increase the economic capacity of parents and reduce vulnerability to risks, they can attenuate psychological distresses and hardship, and improve parental mental health and well-being. In turn, the quality of interaction between parents and children is expected to improve. This can contribute to parents' engagement with children's lives and in their education, and should be reflected in parents' support and supervision of children's school activities and homework. Finally, poverty reduction can have a positive effect on parents' self-esteem and their readiness to participate in school meetings, and can cause them to reassess the value of education for their children as opposed to work. As a result, opportunities for parents to engage in school settings increase, as do their chances of selecting better schools for their children. School functioning can also be expected to improve as parents' participation and choices increase. All these factors should contribute to better performance of children at school.

Chain 2 (Figure 7) pertains to the conditionalities with which families must comply in order to receive the benefit. The health and nutrition of mothers and young children is directly affected by *BF* conditionalities, requiring mothers to regularly visit health care centres and take part in information sessions and courses, as well as to follow up on children's immunisations

and early development. Prenatal consultations, as required by *BF*, can contribute to expectant mothers' health and reduce risks during pregnancy. Health and nutritional conditionalities can contribute to improvements in children's weight at birth, and can encourage the practice of breastfeeding and strong parental control over children's immunisations. By improving pre-school care, *BF* can improve children's early development and readiness for school, thereby increasing the chances that children from low-income families will have advantages when they start at school.

With respect to education conditionalities, late enrolment is expected to decrease as families have an extra incentive to enrol their children at the right age. A good start, both in terms of readiness and age, contributes to better performance at school. Nevertheless, in communities where a high number of children might be found out of school, increasing enrolment of overage, less ready, or less able children might result in classes filled with more disadvantaged students and induce a decline in school performance. The net effect of enrolments encouraged by *BF* on school performance will depend on the weight of each of those effects in local settings. In addition to enrolment, *BF* educational conditionalities require a minimum attendance rate of 85% of school days. Minimum attendance can contribute to more regular participation in classes and reduce dropouts over the academic year. At the same time, by requiring higher rates of school attendance, *BF* can reduce the occurrence of child labour or the number of hours children spend in paid work, as well as in home chores such as doing the laundry, cleaning, or taking care of younger siblings. As a consequence, time for study and homework tend to increase, as do children's chances of succeeding in school. The potential effect of *BF* on child labour probably varies by age group and between boys and girls, as well as with the ratio represented by the cash transfer over the household income in per capita terms. Participation in *BF* is also expected to delay the age of entrance into the labour market for children and adolescents when labour market participation cannot be avoided at all. These are potentially positive effects of *BF* on beneficiaries' outcomes, which are expected to feed into learning outcomes as well.

Some key assumptions must hold for the model just described to have empirical value. First the cash value paid to families has to reach a per capita value well-suited to per capita needs and local market conditions (level of employment, average wage, economic opportunities etc.). Second, beneficiaries have to correctly identify and understand the health/nutritional and educational conditionalities as required by *BF*. Third, beneficiaries must not have problems in complying with the conditions to receive the benefit. For instance, if health care units are not available or are located far from where beneficiaries live, or if

available health care units do not offer the required services, health conditionalities will not be effective. In the same way, if schools are unavailable or lack the necessary resources to work properly (e.g. schools are not within a reasonable distance, have high levels of teachers' absenteeism, or have dilapidated infrastructure), conditionalities may fail to achieve the intended objectives. Fourth, parents are expected to be concerned about their children's future and willing to support their children to the best extent that their living circumstances allow. Finally, education must have economic returns in the market where beneficiaries live to make it beneficial for parents to invest and for children to have real opportunities to escape the poverty trap in the future.

The programme model just described provides an analytical framework in which outcome variables can be identified and selected for testing according to the availability of data. By selecting critical pathways in that framework, we can test whether *BF* is conducive for children's education in the present and for their life chances in the future. In this research, the central element is to test what I have called so far the *missing link* in the expected chain of CCT effects: the learning outcomes of beneficiary children. I also look at dropout and pass-grade rates. In Figure 7 these three educational outcomes are shaded in light blue (Y_1, Y_2, and Y_3). They are the pathways through which completion rates and years of schooling can increase, generating long-lasting benefits for children's futures and increasing their chances of escaping the poverty trap.

Chapter 5. Methodology, Data and Methods

5.1 Introduction

In this chapter some of the methodological issues encountered in research on the impacts of CCT programmes are discussed, which, in part, explain the lack of results regarding CCT effects on children's learning outcomes (section 5.2). I also describe the databases collected from different government agencies in Brazil that feed into the analysis that follows in the next chapters (section 5.3). Particularly relevant is the effort made in this study to link information on school resources and outcomes with school composition (student and household characteristics) and the level of participation in *BF* in each school. The resulting datasets allow the investigation of school conditions and outcomes *vis-à-vis* the level of *BF* participation in each school. In addition, information on cash paid to families and the length of time they have been in the programme, collected from *BF* administrative records, allows the testing of the two hypotheses investigated in this thesis: that length of time of participation and cash value are relevant variables inasmuch as learning outcomes are concerned. Finally, I present the research questions to be explored in the analytical chapters and the methods I apply to perform the analysis (section 5.4).

5.2 Methodological issues in CCT studies

Most of the studies on CCT programmes reviewed in this research[85] are found in the economics literature. Economists have had prominence in investigating the impacts of CCTs, probably driven by the interest of governments and international organisations in the cost-effectiveness of such programmes and their potential capacity to combat poverty and income inequality. As such, research strategies and methods used in CCT studies are those mostly employed by economists. These are mainly quantitative strategies, either social experiments with different levels of randomisation, or quasi-experiments in which different methods to circumvent selection bias are attempted. By extension, considerable use is made of econometric models in attempting to identify causal links between CCTs and social outcomes, and to estimate the significance and strength of eventual effects of CCTs on social outcomes. What are the main assumptions behind those strategies and methods used to study CCT

85 See Chapter 3.

programmes? What are the main methodological challenges to be overcome in studying the educational effects of CCTs in developing countries?

If one takes the two broad classical distinctions for research strategies – quantitative and qualitative approaches – the vast majority of studies carried out on CCT programmes lie in the quantitative cluster[86] with strong influence from the economics research tradition. As defined by Bryman, a research strategy is "a general orientation to the conduct of social research" (Bryman, 2004, p.19). By 'general orientation' Bryman refers to the basic stances researchers adopt in terms of the relationship between theory and research, the nature of the social world, and how valid knowledge about the social world is produce. Quantitative strategies mainly address the social world by testing theories and hypotheses within a deductive approach, taking the social as an objective, measurable, and knowable reality and making use of quantitative methods derived from the natural sciences.

In terms of what Creswell (2002) calls "paradigms of research", CCT studies seem to share positions from two sets of philosophical assumptions: *postpositivism* and *pragmatism*. Many of the CCT studies investigate social effects or outcomes and try to associate them with causes, sharing the *postpositivist* position of the notion of *determinism*. In fact, once CCT is public policy, the notion of determinism here is better described as a probabilistic process. This means that causes are seen as intentional interventions promoted by governments aiming at some stated goal, and the effects are measured in terms of the likelihood of that goal being achieved within a certain period of time. Studies on CCTs generally select hypotheses for testing and thus try to capture social phenomena within a limited set of variables. Therefore, one can argue that they reduce the social reality into either a descriptive or predictive model, simulating a controlled experiment in which observation and measurement are the means to reach an objective reality. All these assumptions have roots in *postpositivism*, as described by Creswell (2002).

On the other hand, CCT studies share *pragmatic* assumptions as well. They are mainly concerned with the production of usable knowledge — knowledge that can solve practical issues in policy design and implementation. For instance, what is the best design and implementation strategy for cash transfer programmes to succeed as instruments to reduce short- and long-term poverty? From a pragmatic perspective, different methods can be adopted as long as they can contribute to an understanding of the problem under scrutiny and to guiding policy. The best research result is considered the one that can offer possible courses

86 Research on CCT programmes is not limited to quantitative approaches. In fact, one can also find qualitative studies and mixed-methods approaches such as in Adato (2008).

of action within some degree of confidence. In a pragmatic sense, CCT studies also share many of the characteristics described by Hakim (2000) for policy research.

Policy research in general, and evaluation research in particular, are distinctive in their characteristics (Hakim, 2000). Policy research pursues "knowledge for action" in order to orient policy-makers towards better designs and implementation processes for public interventions. Generally, policy research is developed under the pressure of time constraints dictated by political agendas and social demands. It focuses on "actionable factors" — those upon which policy-makers can act and are also willing to act. Researchers working in policy analysis generally have a specific intended audience made up of the main stakeholders (policy-makers, donor agencies, pressure groups, NGOs, governments, international organisations, etc.). Policy research tends to follow multi-disciplinary and multi-dimensional approaches, usually achieved by bringing together different specialists from different fields. Researchers also seek national or regional representativeness for their findings; otherwise results may be considered irrelevant from the perspective of policy-makers. They also tend to work with "macro-level causal processes" and to estimate the size and strength of social factors under analysis to provide policy recommendations. Most of the CCT studies can actually be understood as policy research endeavours and share those characteristics.

In terms of research design, defined as "a framework for the collection and analysis of data" (Bryman, 2004, p. 27), many of the CCT programmes have been assessed using quasi-experimental designs. Although some CCT programmes allowed an experimental setting for research to be carried out[87], quasi-experimental settings are the most likely scenarios for analysing CCTs in developing countries. Quasi-experiments, although comparing treated and untreated groups, lack the randomisation process of assigning benefits to potential participants in the programme. Instead, quasi-experiments try to identify or construct comparable groups of participants and non-participants through the use of several research designs, such as regression-discontinuity design, natural-experiments, matched constructed control groups, statistically equated constructed controls, and instrumental variables (Meyer, 1995; Rossi, Lipsey and Freeman, 2004; Morgan and Winship, 2007; Murnane and Willett, 2011). These methods have been largely used in evaluation research and improve causal inference in social studies in which randomisation is not possible.

87 For instance, in Progresa (Mexico), randomly selected rural communities were included as targeted areas in the first phase of the programme. After the rural communities were randomly included in or excluded from Progresa, households were then registered in the programme based on socioeconomic profiles through means-test interviews that were supposed to take place in their homes. Other programmes allowing experimental designs are Red de Protección Social in Nicaragua and PRAF in Honduras.

The reasons why quasi-experiments are more common than 'true' experiments in impact evaluation research are sometimes related to political and ethical issues during programme implementation. Public policy interventions, even if targeted to certain social groups or geographic areas, are rarely implemented by randomly choosing beneficiaries. Selection processes are generally based on administrative criteria and beneficiaries are either self-selected (e.g. unemployment insurance, training courses, clinical assistance, conditional cash transfers) or induced by governmental or administrative decisions (e.g. schools adopting a new textbook, curriculum, or pedagogic model; sanitation infrastructure benefiting entire communities; fiscal incentives based on tax cuts geared to specific production sectors). The exclusion of potential participants from benefits to allow for a randomised experiment is politically unsustainable from both government and community perspectives. It is also ethically questionable, as is the case of intentionally excluding children from immunisation or school feeding programmes, or excluding their mothers from health care assistance, in order to establish a "control group" for a social experiment. This explains why, in many cases, the only route open for researchers studying public programmes such as CCTs is through quasi-experimental approaches.

CCT programmes have generally been phased in over periods ranging from months to years due to operational and budgetary constraints. This means that, even if unintentionally, CCTs do not achieve total coverage of the intended target population in the first stages of the implementation process. This makes possible the use of research designs based on quasi-experiments and constructed-control groups (Rossi, Lipsey and Freeman, 2004) formed by uncovered (although eligible) targeted individuals. However, several of the alternative methods based on quasi-experiments are quite complex in terms of controlling design effects on the estimated net outcomes of an intervention, and researchers assume many hypotheses while adopting specific research designs. Therefore, research design in quasi-experimental contexts can be quite influential on reaching significant, reliable, and valid research results.

The difficulties with research design could explain why, so far, researchers have found no significant contribution of CCTs to students' learning outcomes. For instance, the isolation of learning effects is difficult in CCT studies due to what is called in the literature the "programme placement effect" (Pitt, Rosenzweig and Gibbons, 1993) or "selectivity". This means that the potential positive effects of CCTs can be obscured by the very implementation of the programme and, therefore, become difficult to measure. For example, when CCTs are targeted at poor children and have the immediate effect of bringing these children to school and promoting regular attendance, other factors, for example, previous learning experiences

and/or family background may mean that these poor children tend to perform poorly in comparison to their non-beneficiary peers. As such, the average school test score is potentially affected by the proportion of these selective enrolments and disentangling this effect is a major challenge for research designs intended to assess beneficiaries' progress in learning outcomes.

Methodological difficulties such as the "selectivity" problem mentioned above should be taken into account when assessing research on the effects of CCTs. There are several reasons why CCT studies face design constraints. First, CCT programmes rarely have a comprehensive evaluation plan from the outset, and programme designers usually do not consider evaluation as an important factor to be accounted for, as is common with public policies in developing countries[88]. Thus, researchable variables based on programme objectives and baselines are, in general, not defined from the beginning. This can seriously compromise 'evaluability' and can limit the aspects that can be assessed after programme implementation starts. Second, education assessment systems that periodically record students' achievements on standardised tests are also very recent in many developing countries. As a result, the lack of data makes the construction of baselines upon which to assess any significant impact of those programmes on students' learning outcomes quite difficult. Third, most researchers relying on administrative records and performing secondary analyses on CCTs may be at risk of using poor quality information, analysing inaccurate data, and assessing only 'treated' official data. Finally, the 'noisy' nature of the effects on learning measures[89], associated with a lack of experimental conditions, defy researchers, pushing them towards more complex statistical methods and assumptions. The general context surrounding CCT programmes both constrains and imposes conditions that potentially have an impact on evaluation results.

In view of those limitations, research based on administrative records should be "back to front", as pointed out by Hakim (2000). That is, instead of framing the research design at the outset, researchers first have to identify and collect data to be able to decide upon the adequate research design. Data usually imposes limitations and reshapes the research question, and also influences the research design. Additionally, administrative records do not provide 'ready-to-use' data, and failures in managing and selecting valid information can weaken results or even compromise the entire research programme. In the following sections I

88 *Progresa* in Mexico is an internationally recognised exception.

89 Effects on learning measures are very difficult to measure due to the complexity and multitude of intervening factors. It is considered by quantitative researchers a 'noisy' measure, difficult to isolate 'true' effects.

briefly describe the sources of data used in this research and how the available data influenced and reshaped the research question and design to investigate the educational effects of the *BF* programme in Brazil.

5.3 The sources of secondary data

This research makes use of secondary data drawn from large public databases held by two government agencies – the *Ministry of Social Development* (MDS) and the *National Institute for Educational Studies and Research Anísio Teixeira* (INEP). My personal networks within the relevant government agencies facilitated the process of contacting government officials, identifying and selecting databases, negotiating the release of data, and following through with the process of retrieving data. The lack of a clear protocol to orient government officials in how to proceed when assisting researchers and others interested in making use of public data created an atmosphere of uncertainty and anxiety amongst staff members[90]. At times, data was released based on personal confidence without any formality. Other times the process was paralysed due to the lack of clarity on how to proceed in terms of what kind of data could be disclosed, what risks were involved, and how officials could be assured of the good use of the public information. The collection of the databases during the spring term of 2008/2009 took longer than expected, in part due to this environment of uncertainty. In the end, the datasets were released with two agreements signed in order to guard against sharing the data beyond those involved in this research project.

5.3.1 INEP Datasets

Prova Brasil National Exam (2005 and 2007)

INEP[91] is a research institute linked to the Ministry of Education, and is in charge of the national statistics and educational assessment systems for all levels of education. It has undertaken an annual School Census since late 1980s, as well as a national basic education evaluation system (SAEB[92]) since early 1990s based on national samples of fourth and eighth grade students of Fundamental Education and third year students of Secondary Education. SAEB is only representative at the state level and cannot be used to make inferences at the municipal or school levels. In 2005 a national standardised exam assessing the performance of

90 This situation has probably changed after the 'Access to Public Information Law' (Law 12527) was sanctioned by the President in November 2011.

91 http://portal.inep.gov.br/

92 Basic Education Evaluation System.

all public urban schools at grades 4 and 8 was implemented, and is now administered every two years. All students attending assessed schools in those grades take the same exam (*Prova Brasil*) in two subjects: Mathematics and Portuguese Language. Since 2005, *Prova Brasil* has allowed comparisons between schools and municipalities, and the examination is now in its fourth edition (*Prova Brasil 2011*).

The first round of *Prova Brasil* was carried out in 5,398 municipalities, in November 2005, encompassing all 27 states in Brazil. It evaluated 3,306,378 pupils in the fourth and eighth grades of Fundamental Education. Pupils were distributed across 122,463 classes in 40,920 urban public schools. Only schools with at least 30 pupils registered in the evaluated grades took part in the exam. Tests of Portuguese Language (with a focus on reading) and Mathematics (with a focus on problem solving) were applied based on state curricula and on the recommendations of the National Curricular Guidelines. In November 2007, a second national round of *Prova Brasil* took place, this time expanded to urban public schools with at least 20 pupils enrolled in the assessed grades.

In addition to testing students, *Prova Brasil* has a student questionnaire that covers information on students and their families, including social and economic factors. The main variables available in the student questionnaire are: gender, race, age, house facilities, family composition, parents' education, students' habits, students' expectations, parental support, students' school history, teachers' support, and students' participation in the *BF* Programme[93].

For this research I collected the 2005 and 2007 test scores in Mathematics and Portuguese Language for fourth grade students, as well as the background information of children and family socioeconomic status. These data are central to the analysis in the forthcoming chapters. As discussed in the previous chapter (subsection 4.2.2), progression beyond the fourth grade in Brazil remains a challenge for many children, mainly those from low-income families. *Bolsa Família* could contribute to overcoming that barrier if it can contribute to learning outcomes — a necessary pathway to upper levels of education. For the first time ever, *Prova Brasil* has created the opportunity for researchers to investigate school learning outcomes (as measured by standardised test scores) and its connections to school environment and family background (including *BF* participation) on a national scale.

93 This last variable was removed from the questionnaire for 2007.

The National School Census (2005 and 2007)

The annual National School Census collects several variables on school characteristics including: infrastructure (facilities, equipment, pedagogical resources, ICT); human resources (number of staff and teachers, and teachers' education); enrolments by grade, gender, race, age, and school shift; school performance (grade failure, pass-grade rates, and dropout rates). The School Census covers all basic education schools, both private and public, and all levels of basic education (infant education, fundamental education, and secondary education). The census was radically changed in 2007, when INEP launched an online module to collect individual students', teachers', and head teachers' records (besides school resources data) directly from schools. In order to use data for the years 2005 and 2007, I had to analyse the structure of both censuses and identify the relevant variables to be included in the analysis. Data from the School Census was used to identify the number of fourth grade students by school and to create new variables describing school resources, which are amongst the main variables used in the forthcoming analytical chapters.

5.3.2 MDS Datasets

The *Bolsa Família* programme runs three different information systems from which relevant variables were collected for this research: the Unified Record for Social Programmes, the School Attendance System[94], and the Benefit Payment System. The Unified Record for Social Programmes ('*CadUnico*') is a national instrument to identify and characterise the socioeconomic conditions of low-income families. It is also used to select households for social programmes and to integrate them[95]. All families below the threshold of half a minimum wage per capita must be registered in the Unified Record in order to gain access to national social programmes[96]. This massive database is also available for state and municipal governments who wish to use socioeconomic variables to identify social demands and to select households for local or state level social programmes. Information for the Unified Record is collected at the local level by municipalities (preferably through household interviews) and is fed into the national information system managed by a federal bank ('*Caixa Econômica Federal*'). The main variables recorded include household characteristics including family composition; civil identification of each family member and their education qualifications and labour market situation; and household income and expenditures. A Social Identification Number (NIS) is

94 This system is jointly managed by the Ministry of Education and the Ministry of Social Development (MDS).

95 See http://www.mds.gov.br/bolsafamilia/cadastro_unico/o-que-e-1/

96 Families below 3 minimum wages of monthly income were also registered in the Unified Record.

issued to every individual registered in the Unified Record, allowing the tracking of individual records in social programmes. The municipality can modify information on the Unified Record at any time, and is responsible for reviewing and confirming the information for each registered household at least once every two years.

As a CCT programme, *BF* oversees the compliance of the education conditionality with support from the Ministry of Education. The School Attendance System registers every two months whether beneficiaries' attendance rates are reaching the minimum level of 85% of the school days for those aged 6 to 15[97]. Attendance records also register the reasons given if it is the case that a child did not comply with the minimum attendance as expected. The variables available include student NIS, student name, birth date, household code, school code, grade attended, attendance rate, reason for low attendance, and parents' NIS. Finally, the Benefit Payment System is a monthly record of the benefits paid to families included in the Unified Record as *BF* beneficiaries, calculated based on compliance with the conditionalities. It registers the amounts received by families each month, as well as the month and year the household started to receive the cash transfer. These variables allowed me to create the two variables used in the analysis to test my hypothesis: time of participation in the programme and per capita cash transfer.

5.3.3 The resulting datasets for analysis of *Bolsa Família*

By linking data from the five different sources and using them to construct new variables, three different datasets are created, which are described and analysed in the next three chapters. Some general aspects make the resulting datasets unique for the purpose of investigating *BF* in relation to educational inputs and outcomes. First, learning outcomes, as measured by standardised test scores at the school level, are used for the first time in a study about *BF* on a national scale. Second, by using participation in *BF* as a proxy for poverty incidence in each school, it is possible to analyse distributional aspects of educational resources by social group according to socioeconomic status. Third, two variables of potential value in analysing the effects of *BF* on education – time of participation and per capita cash transfer – are available to investigate their mutual role in conditioning *BF* contributions to educational outcomes. These variables have not received much attention and have not been considered in tandem in CCT studies to date. Finally, two years' worth of data from the national exams (2005 and 2007) allow for the construction of a panel of schools in which *BF*

97 Since 2008 this has also been in place for youths aged 16 and 17, who should have a minimum of 75% attendance once registered in *Bolsa Família*.

intake, its association with school resources, and its influence on school outcomes can be analysed over time.

5.4 Research question and methods of analysis

As mentioned in section 5.2, several methodological issues have defied attempts to empirically test causal relationship between participation in CCT programmes and educational outcomes, particularly learning outcomes. Empirical studies in this regard demand a research design capable of producing a research response to a proposed question using available data and accounting for modelling constraints. In the case of this research, the initial research question asks *whether Bolsa Família makes any positive contribution to the educational outcomes of economically disadvantaged children, particularly to achievement in national standardised exams*. As such, this research question poses a challenge for the research design inasmuch as the available data is not experimentally generated but retrieved from the administrative records related to the operation of the *BF* programme, the National School Census, and the national exam (*Prova Brasil*). Given the data available, the strategy followed in this research cannot produce a straightforward answer to that question, since the selectivity into the programme is not modelled at the individual level in a way that could satisfactorily identify equivalent groups of students participating and not participating in *BF* and their respective educational outcomes. Nevertheless, the strategy followed allows for a strong indication of the answer to that question by analysing what happens to the marginal effect[98] of the level of participation in *BF* on school outcomes. Positive changes in the marginal effect of *BF* intake on school outcomes would reflect improvements in beneficiaries' results, and would thereby indicate a positive contribution of *BF* to children's educational outcomes.

The analysis developed in the next three chapters addresses specific research questions leading to the core issue of whether or not *BF* contributes to beneficiaries' learning outcomes, also considering effects on pass-grade and dropout rates. In the first part of the analysis (chapter six) I investigate the achievement gaps in test scores between beneficiaries and non-beneficiaries, and I explore some of the socioeconomic and school factors explaining those gaps. The following questions are addressed in chapter six:

1) *How do beneficiaries and non-beneficiaries differ with respect to individual, household, and school characteristics?*

98 The marginal effect is the change observed in school outcomes due to a small increase in the proportion of beneficiaries in the school (e.g. one percentage point).

2) *How do beneficiaries and non-beneficiaries differ in terms of test score achievement on standardised tests in Portuguese Language and Mathematics?*

3) *To what extent can socioeconomic and school factors explain the achievement gap, if any, between beneficiaries and non-beneficiaries on the national exam?*

In order to answer these questions I use a national random sample of fourth grade students who took part in *Prova Brasil* (2005) and school data from the National School Census for the same year. The first round of the national exam was also the only round in which pupils were asked whether they were registered in *BF*, along with several other socioeconomic characteristics. Therefore, it is possible to link *BF* participants to their results in test scores and to their individual and family characteristics, as well as to compare them with non-beneficiaries in terms of test score results and socioeconomic and school characteristics. I use this cross-sectional data at the individual level first to describe both beneficiaries and non-beneficiaries (who they are, how they live, and what type of school they attend). Then, in a multivariate regression model I estimate the existence of achievement gaps between beneficiaries and non-beneficiaries and analyse which sets of factors are more influential in explaining eventual gaps. Student, household, and schools' characteristics are then compared in terms of the proportion of the differences in learning outcomes between beneficiaries and non-beneficiaries that they can explain. If student and household characteristics play a major role in explaining those differences, then it might be the case that disadvantaged children would benefit from participation in *BF*. Because *BF* is expected to mitigate some of the social disadvantages caused by severe deprivation while requiring high attendance rates, it is likely to contribute to improving beneficiaries' test scores and to reducing the eventual achievement gap between participants and non-participants.

In the second part of the analysis (chapter seven) I investigate school level outcomes in terms of tests scores in Portuguese Language and Mathematics, pass-grade rates and dropout rates of fourth grade students in 2007, according to the level of participation in *BF* in each school. The main hypothesis investigated in this thesis is then addressed. Do time of exposure to *BF* and per capita cash paid to families in each school positively influence school results and reduce the achievement gap between high-*BF*-intake schools and low-*BF*-intake schools? If yes, can the positive effect on school outcomes be attributed to improvements in beneficiaries' educational outcomes? The specific questions orienting the analysis in chapter seven are:

1) *How do schools differ in terms of composition, resources, and outcomes by level of Bolsa Família intake?*

2) *How does a school perform relative to other schools with similar composition and resources given its specific characteristics of Bolsa Família participation (intake, length of time of participation, and per capita cash)?*

3) *Do length of time of participation in Bolsa Família and per capita cash transfer have any positive effects on school outcomes, taking into account the level of Bolsa Família intake?*

To answer these questions I use cross-sectional data for the fourth grade students' results in *Prova Brasil* 2007 (including socioeconomic variables), aggregated at the school level, and data on school characteristics from the National School Census for the same year. In addition, data from the *BF* programme (including the date of the first benefit for each family; the value of per capita cash transfer; and the identification of the number of beneficiaries in each school) are used to generate three aggregated *BF* indicators at the school level: the mean time of participation in *BF*; the mean per capita cash paid to families; and the proportion of beneficiaries in each school (*BF* intake).

I analyse 2007 data at the school level, because unlike the data from *Prova Brasil* 2005, in the 2007 data there is no way to link a student's test score results to participation in *BF*[99]. Although it is possible to know how many beneficiaries exist in each school (thus, *BF* intake can be calculated) it is not possible to know how beneficiaries perform relative to non-beneficiaries in each school. Nevertheless, schools can be compared based on *Bolsa Família* parameters: *BF* intake, mean time of participation in *BF*, and mean per capita cash transferred to families in each school. Using these variables at the school level I investigate the hypothesis that length of time of participation and amount of cash paid to families are relevant variables in assessing the effects of *BF* on children's educational outcomes.

The strategy followed in this second part of the analysis is essential to understand how this research intends to estimate *BF* effects on test scores and, therefore, to offer an answer to the main research question raised in the opening of this section. The idea behind the strategy is relatively simple: it is expected that schools with high *BF* intakes, on average, perform worse than schools with low proportions of beneficiaries. This expected educational gap would simply mirror structural inequalities[100]. However, the idea that CCTs can improve long-term human capital accumulation amongst children living in low-income families raises the hypothesis of a reduction in the educational gap over time. The gap can reduce as a result of

99 In 2007 the question in the socioeconomic questionnaire about participation in *Bolsa Família* was removed.

100 It is worth reminding the reader that in 2007, *Bolsa Família* had already achieved full coverage of the target population of poor families as estimated by the Brazilian government (11.2 million families). Therefore, the level of *BF* intake by school is expected to reflect the proportion of poor families in each school, and differences in test scores between schools with different levels of *BF* intake are expected to reflect achievement gaps associated with family socioeconomic background.

the cash transfer and/or the conditionalities attached to it. Therefore, in schools where beneficiary families have a longer mean time of exposure to the programme (controlling for the level of *BF* intake and other relevant factors), beneficiary children are expected to perform better than in schools with a shorter mean time of participation. In a similar vein, if per capita cash does make a difference in improving children's outcomes, then in schools where, on average, beneficiary families receive a higher per capita cash beneficiary, children are expected to demonstrate more improvements relative to those schools in which the mean per capita cash transfer is lower. These improvements for beneficiary children would be detectable in the schools' mean outcomes, as associated with *BF* factors.

To test these hypotheses, an interactive model comprising three *BF* factors (*BF intake*, *Time*, and *Cash*) is proposed and estimated by ordinary least squares (OLS) to capture the potential effects of time of participation and per capita cash on school outcomes. First, I estimate how these two *BF* factors moderate the marginal effect of *BF* intake on school outcomes. Then I estimate how those two factors affect school outcomes directly by looking at schools of the last quintile of *BF* intake (80% to 100%) in which *BF* intake loses its relevance as an explanatory variable for school outcomes. More details about this estimation strategy are given in chapter seven.

In the third part of the analysis (chapter eight) I move to a more robust model to analyse the potential contribution of *BF* to school outcomes. A *two-way fixed effect model*[101] is estimated using school level panel data for 2005 and 2007 to test the hypothesis of a positive change in school performance for fourth grade beneficiaries between those years. In this model, although the two variables analysed in chapter seven (time of participation and per capita cash) are not available for 2005, the time dimension and its influence on the marginal effect of *BF* intake are captured by the structure of the panel data. The panel data allows the measurement of the *Bolsa Família intake effect* over time on school performance indicators (mean test score, pass-grade rates, and dropout rates) and to address the following questions in chapter eight:

1) *How did school composition, resources, and outcomes change between 2005 and 2007, including composition in terms of BF intake?*

2) *How did the marginal effect of BF intake on school outcomes change between 2005 and 2007? Is there any marginal gain for school outcomes associated with BF intake over time?*

101 The two-way model is, in fact, a school-and-time fixed effects model in which both time invariant differences and temporal trends invariant across schools are discounted from the estimated effect of *BF* intake over time.

In the following three analytical chapters, the datasets, methods, and models briefly referenced above are discussed in more detail.

Chapter 6. Analysing the achievement gap in *Prova Brasil* 2005

6.1 Introduction

Even though *Bolsa Família* is expected to contribute to the learning outcomes of beneficiary children (as argued in chapter four), socioeconomic disadvantage remains potentially linked to poor performance at school in comparative terms. Therefore, it is expected that school achievement gaps between beneficiaries and non-beneficiaries are to be found in cross-sectional analysis, reflecting the structural differences between social groups within Brazilian society. The analysis of that income-based achievement gap can be developed using *BF* status as a proxy variable to identify children from low-income families. Children like *BF* beneficiaries whose families suffer from several types of deprivations tend to have diminished educational opportunities and poor achievement outcomes, even when their non-poor peers are succeeding under the same school conditions. Knowing how *BF* beneficiaries perform in standardised test scores compared to other children and which factors explain eventual achievement gaps can suggest whether demand-side policies such as *BF* will have an impact on the learning outcomes of beneficiary children.

In this chapter I investigate gaps in test scores between beneficiaries and non-beneficiaries of *BF*, as well as socioeconomic and school factors that explain those gaps. I use a national random sample of fourth grade students who took part in *Prova Brasil* 2005 to analyse: (i) differences between beneficiaries and non-beneficiaries as to individual, household, and school characteristics; (ii) differences in test scores obtained by beneficiaries and non-beneficiaries in Portuguese Language and Mathematics and; (iii) whether socioeconomic and school factors can explain achievement gaps, if any, between beneficiaries and non-beneficiaries on the national exam.

6.2 Considerations on data, effect size, statistical power, and sample size

6.2.1 Data

In 2005 Brazil implemented a national standardised test in Mathematics and Portuguese Language (*Prova Brasil*) for all urban public schools of Fundamental Education with 30 or more pupils in the assessed grades (fourth and eighth). Students sitting for the *Prova Brasil* exam also answered a socioeconomic questionnaire, allowing the identification of some

of their individual and household characteristics, including whether or not their families were taking part in the *BF* programme in 2005. In addition to these data, I also calculated school indicators based on variables retrieved from the National School Census 2005 to characterise the school context in which those students were situated at the time of the exam. This allows, to some extent, an appraisal of the education quality offered to the two groups of concern, beneficiaries and non-beneficiaries of *BF*.

The 2005 dataset of students' test scores is very large, comprising 1,975,635 students in grade four who sat for the exam that year. Before randomly selecting a sample of students for this analysis, several steps were taken to ensure that the random sample was complete for all variables in all cases. First, students who did not answer the socioeconomic questionnaire were excluded from the dataset[102], reducing the number of observations by 74,826 students (3.8%). The resulting dataset with test score and socioeconomic records had 1,900,809 observations. However, not all students in this dataset answered all items in the socioeconomic questionnaire, including the question about their participation in *BF*. Following the analysis of incidence and pattern of missing data performed on the dataset, the following was observed: (i) there was a very low incidence of missing values across the 44 variables of the socioeconomic questionnaire and the incidences were rather equally distributed, ranging from a minimum of 1.9% (Q17) to maximum of 5.6% (Q3); (ii) the patterns of missing values across cases were also rather equally distributed, with each pattern occurring in less than 1% of cases and; (iii) a large proportion of cases had no missing values (67%). Therefore, all observations with missing values in any of the questionnaire variables were excluded to produce a "complete case" dataset (Hair et al., 2010, p. 51). A total of 547,481 cases were excluded, resulting in a dataset of 1,353,328 observations and representing 68.5% of the original dataset. **Table 3** summarises the steps leading to the number of observations described.

102 The analysis of missing data showed that cases without questionnaire were not statistically distinguishable from the group of beneficiaries with questionnaire in terms of test scores. As the remaining dataset still holds 540,185 cases of beneficiaries with questionnaire (28.4% of the observations) the excluded cases will not generate bias in any random sample drawn from this dataset.

Table 3: Number of Valid Observations for 2005 *Prova Brasil* Exam

4th GRADE		
Dataset	**Number of Observations**	**%**
4th grade students with exam results in Portuguese and Mathematics	1,975,635	100%
4th grade students with socioeconomic questionnaire	1,900,809	96.2%
4th grade students with full information in all socioeconomic variables	1,353,328	68.5%

With over one million cases in the remaining dataset, most trivial correlations or differences between variables in the datasets tend to be statistically significant, even if not sufficiently large to be considered of any practical significance. Another problem is the amount of computing time required to process analyses using such a large dataset; this leads to the problem of specifying a sensible sample size to avoid unnecessary computational burden and to strike a balance between Type I and Type II errors. As argued by Vaus (2002, p. 180-181), by using power analysis one can calculate a sample size that is large enough to detect meaningful differences or effects (avoiding Type II errors[103]), but not so large that the risk of detecting trivial effects or committing Type I errors[104] increases.

Statistical power, defined as "the probability of correctly rejecting the null hypothesis when it should be rejected" (Hair et al., 2010, p.9), is affected by three other factors: sample size, effect size (considered as of any practical significance to be detected), and the level of statistical significance set to reject any considered null hypothesis. As stated by Cohen (1977) the four variables are related so that "when any three of them are fixed, the fourth is completely determined" (Cohen, 1977, p. 14). According to Vaus (2002, p. 181), the statistical power should be set between 0.8 and 0.9 to avoid Type I error[105]. The level of statistical significance is generally set either at 5% or 1% (95% and 99% level of confidence respectively), depending on how stringent one is in rejecting any considered null hypothesis. I discuss the effect size below, taking into account the practical significance associated with any difference in test score based on the scale used in the *Prova Brasil* national exam[106]. The sample size is derived from the three other parameters.

103 Failing to reject the null hypothesis when it is in fact false.

104 Rejecting the null hypothesis when it is in fact true

105 If it is set above 0.9 the chances of committing Type I error increases.

106 Test scores is the main variable of interest and, therefore, the effect size will be discussed in relation to this variable.

6.2.2 The *Prova Brasil* proficiency scale and how to gauge differences in test scores ("effect size")

Fourth grade test scores in *Prova Brasil* are expressed using a proficiency scale that ranges from 125 to 325 points in Portuguese Language and from 125 to 350 points in Mathematics. These represent the score intervals at which meaningful cognitive skills can be identified and assessed amongst fourth and eighth grade examinees based on *item response theory*[107]. A performance level is associated with each interval of the scale, based on a set of cognitive skills characterising the expected learning achievements of students at that level. Although the cognitive skills are different for each discipline and for each level of the scale, results are comparable across grades and across time. Students who demonstrate skills at one level also have the abilities represented in the lower levels of the scale. The Ministry of Education does not set minimum performance levels to be achieved at the end of each grade by discipline. The educational system is highly decentralised in Brazil, with the 27 states and 5,564 municipalities having autonomy in the organisation of the curriculum across grades and the learning objectives for each grade. Hence, the national exam proficiency scale and test score results are understood to be references based on which schools and school systems can assess their educational objectives and, eventually, establish achievement goals for each grade[108].

In order to attribute practical significance to differences in test scores three alternative measures are considered for each discipline: (i) an equivalent in amount of time of schooling necessary to achieve a certain level of cognitive skills; (ii) an equivalent in percentage of the interval between two adjacent cognitive levels (25 points); and (iii) an equivalent in standard deviations observed in national exam results.

Using the scale range associated with meaningful cognitive skills for each discipline, differences in mean test scores can be assessed considering an *ideal* mean number of points per year necessary to achieve the top ranks of the scale (as shown in **Table 4)**. On average, the expected gains in cognitive skills for each year of schooling are equivalent to 25 points in Portuguese Language and 28 points in Mathematics. This means that after four years of regular schooling, a fourth grade student is expected to achieve 225 points in Portuguese

107 The same scale is used in the exam for the eighth grade, for which the top ranges of the scale are possibly achieved. However, as this research looks only at the effects on test scores at the fourth grade level, in general the highest scores are not expected to go beyond 300 points.

108 For instance, the State of São Paulo uses the same proficiency scale for the SARESP, the state level exam, in which students are classified into four different achievement levels (below basic, basic, adequate, and advanced) according to their scores.

Language[109] and 237.5 points in Mathematics. By the end of the eighth grade, the student would have achieved the upper bounds of the Mathematics and Portuguese scales[110]. By considering the proficiency scale levels in such an ideal mode, fractions of one scale level can be associated with months of schooling. For instance, one month of schooling equals 2.8 points in Portuguese Language and 3.1 points in Mathematics[111]. These figures can then be taken as references for significant differences in assessing achievement gaps or potential effects of *BF* on test scores, the expected **effect size**.

Table 4: Proficiency scale limits and average points per month to achieve the upper limit of the scale

Discipline	Lower bound	Upper bound	Range	Years of schooling	Average points per year	Average points per month
Mathematics	125	350	225	8	28	**3.1**
Portuguese	125	325	200	8	25	**2.8**

An alternative criterion is to take the interval between two adjacent cognitive levels and calculate differences in mean test scores as percentages of one cognitive level (as described in **Table 5**). Associating the measured differences in test scores to percentages of cognitive skills achieved by a student over one academic year creates practical meaning. For instance, in Portuguese Language, an effect size of 2.5 points is associated with a 10% difference in acquired cognitive skills, 5 points with 20%, 6.2 points with 25%, and so on.

Table 5: Points in test scores as percentages of one cognitive level in the proficiency scale

Discipline	(%)					
	10%	**20%**	**25%**	**50%**	**75%**	**100%**
Mathematics	**2.8**	5.6	7	14	21	28
Portuguese	**2.5**	5	6.2	12.5	18.7	25

109 I consider here the progress achieved in four years in relation to the lower bound of the scale (125 points), for which meaningful learning was consistently measured.

110 This is a linear and, therefore, rather simplistic way of interpreting progress through the different cognitive levels of the proficiency scale. Nevertheless, the objective here is to offer a way of interpreting differences in test scores for which the approximation suffices.

111 I consider nine months of school attendance per year based on the mandatory minimum of 200 days per year in any grade of basic education.

A third possibility is to look at the distribution of test scores and to assess the practical significance of observable differences as fractions of one s.d. **Table 6** shows the main descriptive statistics for test scores in Mathematics and Portuguese Language based on all valid observations in the dataset for the fourth grade in 2005.

Table 6: Test Score in Mathematics and Portuguese – 4th grade 2005 – all valid observations

Discipline	N	mean	s.d.	p10	p25	p50	p75	p90
Mathematics	1,353,328	185.3	39.4	137.1	158.3	182.2	210.3	238.3
Portuguese	1,353,328	178.3	41.3	125.9	148.7	176.0	204.9	233.2

The test score distribution approximates to a normal distribution[112]. Sixty-eight percent of students are found within one s.d. from the mean, approximately 95% lie within two s.d., and 99.7% within three s.d. from the mean. One s.d. is approximately 40 points measured on the proficiency scale for Mathematics and Portuguese Language. Differences in test scores can be associated to fractions of one s.d., as shown in **Table 7**[113]. In this case, differences as fractions of one s.d. can locate students at different points of the distribution. For instance, a difference of 16 points in test scores between two groups of students represents 0.4 of one s.d. This difference places the two groups at points of the distribution as far as the median and the 65th percentile (182 and 198 points respectively[114]) in Mathematics.

Table 7: Points in test scores as fractions of one standard deviation in observable result for 2005

s.d.	0.1	0.2	0.3	0.4	0.5	…	1.0
Points	**4**	8	12	16	20	…	40

The three ways of interpreting points in the proficiency scale can be helpful in gauging differences in test scores in terms of time of schooling, share of cognitive skills, or fractions of an s.d. They also give us references to judge what a practically significant difference is when assessing test scores using the proficiency scale, and I use them to interpret results in this thesis. Any of the equivalent scales above can be used to calculate a reasonable sample size to be used in the forthcoming analysis.

112 Skewness is 0.4 for both disciplines and kurtosis is 3.1 in Mathematics and 3.0 in Portuguese Language.

113 Once the standard deviation is very close for Portuguese and Maths I use an approximation of 40 points to calculate the fractions in the table.

114 The 65th percentile of the distribution is 197.6 points on the scale.

6.2.3 Sample

The three different ways of attributing practical significance to differences in test scores discussed above can give us an idea of the effect size one can expect to be meaningful when assessing differences in means or coefficients in multiple regression analysis. For instance, a test score value of 2.5 points represents 10% of one cognitive level in Portuguese Language and can be used as a parameter to perform the statistical power analysis to calculate the sample size necessary to achieve a balance between Type I and II errors. The null hypothesis of zero test score difference between groups or zero effect on test scores of a given variable can be tested against an alternative hypothesis of differences as small as 2.5 points by randomly selecting *at least* 9,360 observations, as shown in **Table 8**[115]. This is the minimum sample size required to detect an effect size that is equivalent to approximately 10% of one cognitive level (or one month of schooling, or yet .06 of one s.d.) according to the conventional use of α=.05 (statistical significance) and (1-β) = 0.8 (statistical power).

Table 8: Sample size and effect sizes (power=0.8 and alpha=.05)

Equivalence	Effect Size	Sample Size
differing means	2.5	9360
regression slopes	2.5	9180

Note: two-sided test

A final remark on setting a sensible sample size is regarding the relative importance given to the two possible errors in making statistical inferences. For example, by fixing power at 0.8 and α=.05, we also set how we value the two possible errors. The probability ratio between Type II error and Type I error is given by:

$$\Pr(Type\ II\ error)/\Pr(Type\ \ I\ error) = \frac{\beta}{\alpha} = \frac{0.2}{0.05} = 4/1$$

According to Cohen (1977, p.5), the ratio β/α provides the relative importance given to each type of error by the researcher. In the case above, I implicitly consider the possibility of committing Type I error to be four times more serious than making the Type II error, that is, I consider the error of rejecting a true null hypothesis to be four times more important to avoid than the error of failing to reject the null when it is actually false. This balance between Type I and II errors pushes towards the necessity of stronger evidence to assert differences in test scores between groups or significant effects on test scores.

115 The power analysis is performed using the software PS Power and Sample Size Calculations - Version 3.0, January 2009.

Based on the previous discussion, a 1% sample of the dataset containing no missing data (13,533 observations) is more than enough to gauge differences in test scores as small as 2.5 points. Nevertheless, the analysis of the answers to the socioeconomic questionnaire revealed that 17.2% of the students in the sample, although answering all questions, did not know whether their families were taking part in *BF* (**Table 9**)[116]. The analysis of the three groups from **Table 9** showed that those in doubt about their *BF* status had different mean test scores from both beneficiaries and non-beneficiaries[117]. These cases are treated as missing data and are excluded from the sample, which retains a large enough number of cases to perform the analysis. The resulting dataset is then formed by 11,204 observations with all information needed to perform the analysis in this chapter, including students' participation in *BF*[118].

Table 9: Frequency distribution by *Bolsa Família* participation

Are you in *Bolsa Família*?	**Freq.**	**(%)**	**Cumulative Freq. (%)**
Yes	3,788	27.99	27.99
No	7,417	54.81	82.8
Don't know	2,328	17.2	100
Total	13,533	100	

6.3 How do beneficiaries and non-beneficiaries compare in terms of individual, household, and school characteristics?

Looking at the sample of fourth grade students sitting for the national exam in Mathematics and Portuguese Language in 2005, one-third were beneficiaries of *BF* (33.8%), as described in **Table 10**. These are all urban children, since the *Prova Brasil* exam in 2005 was applied only in urban schools with 30 or more pupils enrolled in the assessed grades. In the following sections I describe how beneficiaries and non-beneficiaries differ in terms of individual, household, and school characteristics.

116 This is considered a different case of missing information, since respondents actually replied to the question but, in responding it, generated an indefinite position as to their participation in *Bolsa Família*.

117 The "don't know" group scores 11.6 points higher than beneficiaries and 8.1 points lower than non-beneficiaries. Therefore, this is an intermediate group in terms of school performance. These might be those students either eligible for *Bolsa Família* who had not been included in the programme by 2005 or those in the neighbourhood of the cut-off point of eligibility.

118 One student is also excluded from the sample because his/her school was not found in the National Census.

Table 10: *Bolsa Família* participation (2005)

Are you in *Bolsa Família?*	Freq.	%
No	7,416	66.2
Yes	3,788	33.8
Total	11,204	100.00

6.3.1 Student Characteristics

As shown in **Table A - 1** there are remarkable differences between beneficiary and non-beneficiary students in terms of demographic characteristics. The proportions of beneficiary students in the North and especially in the Northeast, the poorest regions in Brazil, are significantly higher than in the Centre-South regions. White students are significantly less represented amongst beneficiaries compared to non-beneficiaries, whereas Afro-descendant minorities, mixed-race groups, as well as indigenous groups and Asians are significantly more represented amongst beneficiaries. Finally, looking at the distribution of students by age in the two groups, those fourth graders at the right age-for-grade in 2005 (10 years old) appear in a significantly higher proportion in the non-beneficiary group, whereas students aged 11 years and older are proportionally more represented amongst beneficiaries. The proportion of boys in the beneficiary group is slightly higher than that of girls.

The second set of characteristics displayed in **Table A - 2** also shows significant differences between beneficiaries and non-beneficiaries as to their school history. While 40% of *BF* students have experienced at least one grade repetition over their years at school, the proportion for non-beneficiaries is much lower (24%). The same is seen in the proportion of those who have dropped out in past years. Roughly 10% of beneficiaries have abandoned school in previous years, whereas this figure is lower amongst non-beneficiaries (around 7%). Both retention and dropout are contributors to the overage indicator, which is 4.6 p.p. higher for beneficiaries in the sample. Also, non-beneficiaries attended pre-school education in higher proportions (79%) than beneficiaries (71%)[119]. Another important aspect distinguishing students from both groups is the impressions of students regarding the attention they receive in the classroom. Apparently, a much higher proportion of beneficiaries perceive themselves to have been neglected in the classroom (22%) than do non-beneficiaries (14%). Also, there is relatively less school mobility (ability to change schools) for beneficiaries compared to non-beneficiaries (3 p.p.).

119 Although the proportion figures for those who declared to have attended pre-school are statistically equivalent, when taken together with the proportions of kindergarten attendance, the differences is 7 percentage points lower for beneficiaries.

The last set of variables in **Table A - 3** accounts for students' habits, reporting on characteristics of students' time allocation while not in school and their expectations as to continuing in school. As the time spent watching television increases on the scale, the difference in the proportions between the two groups increases in favour of non-beneficiaries. This reveals that beneficiaries tend to spend fewer hours than non-beneficiaries watching television. However, this result does not necessarily mean that beneficiaries are spending their spare hours engaged in educational activities. In fact, as the differences in the next variable show, beneficiaries spend more hours engaged in domestic work. In addition, the proportion of beneficiaries in paid work is around 7 p.p. higher than that of non-beneficiaries. This could also explain why beneficiaries are at a disadvantage when it comes to the share of students who declare that they complete their homework[120]. The last variable in the table expresses students' expectations in relation to study and work after finishing compulsory education. The majority of students in both groups expect to continue in school (≈ 60%) after completing their fundamental education, either in combination with work or not. Nevertheless, many students (≈30% in both groups) still did not know at that point (fourth grade) whether they would be studying, working, or doing both in the future. Curiously, beneficiaries are slightly ahead in the group declaring that they would be only studying in the future. This might express the influence of *BF* on families and on children's aspirations of continuing in school.

6.3.2 Household Characteristics

As expected, the material environment in beneficiary households is poorer when compared to non-beneficiaries. As **Table A - 4** shows, almost 10% of *BF* households do not have running water and the proportion of those without power supply is almost double that of their non-beneficiary counterparts (4% to 2%). In terms of durable goods, two patterns are observed. Basic goods such as televisions, radios, and refrigerators are common even amongst *BF* households (around 90% of respondents declare that they have these goods at home). Differences between beneficiaries and non-beneficiaries range from 3 to 7 p.p. when comparing those families without such goods. For goods such as VCRs, freezers, washing machines, vacuum cleaners, and cars, the proportional difference between *BF* and non-*BF* households for those not having such goods ranges from 7.5 to 16 p.p. With respect to housing infrastructure, almost 4% of *BF* households do not have a bathroom and 1.5% of *BF* families live in houses with no bedroom. These proportions are twice those of non-*BF* households.

120 The trends in the percentages are basically equal for Portuguese Language and Mathematics.

In terms of learning environment, **Table A - 5** shows that non-*BF* households are at an advantage compared to *BF* households. Approximately 60% of *BF*-households have less than 20 books; 13% do not even have a dictionary. Surprisingly, differences with non-*BF* households are not substantial (4 and 3 p.p. respectively). More surprising is the fact that there is no statistically significant difference between the two groups when asked if they have a peaceful place to study at home. When it comes to computer and Internet access, the differences are larger. Eighty-seven percent of *BF* households do not have a computer (against 75% of non-*BF* households) and the proportion of non-*BF* households with Internet is double that of beneficiary households.

Although *BF* households have a slightly higher proportion of female-headed families, the two groups do not differ significantly in terms of parenting structure, as shown in **Table A - 6**. However, *BF* family sizes tend to be significantly larger, and the differences between the two groups increase with the number of family members. *BF* beneficiaries are clearly at a disadvantage where parental education is concerned. As described in **Table A - 7**, the *BF* group has lower a proportion of mothers and fathers with secondary education or higher, whereas the non-*BF* group has a lower proportion of parents with four years of education or less when compared to the *BF* group. An interesting statistic is the 25% of students declaring that they do not know their mothers' level of education, and the 36% stating that they do not know their fathers' level of education. These proportions are statistically equivalent for beneficiaries and non-beneficiaries. Beneficiaries also appear to be at a slight disadvantage (around 1 to 2 p.p.) in relation to parental encouragement for students to pursue activities such as doing homework, reading, going to school, and studying. *BF* parents are also less engaged in attending school meetings.

The material disadvantages of *BF*-households are expected, given that the programme is designed to target the poorest families. Nevertheless, the disadvantages of *BF* students at home go beyond their material environments, and include family structure, parents' characteristics, and level of parental encouragement. The question of whether the material and learning environment is more influential on children's outcomes than family structure and parents' characteristics is analysed later in this chapter.

6.3.3 School Characteristics

School infrastructure is assessed in terms of school size, facilities, and available equipment. School facilities are measured by an index ranging from zero to one, computed

using binary variables for 34 items included in the National School Census in 2005[121]. School libraries, computer rooms, and laboratories are also examined separately from the general facilities index, because they represent resources directly linked to pedagogical purposes. A second index for school equipment, ranging from zero to one, measures the availability of 19 items included in the School Census[122]. Two variables related to school organisation measuring student-teacher ratio and class size are compared for the two groups. The former is a proxy for the intensity of teacher interaction with students. For instance, in many primary schools in Brazil the four basic areas of study[123] for grades 1 to 4 are divided between two (sometimes amongst four) teachers. Also, some school projects include special teachers for library, computer, physical education, and arts activities, decreasing that ratio and enabling students to have more options for teachers to support them. These two variables are measured for grades 1 to 4 altogether, since they are not disaggregated by grade in the available data. The student-teacher ratio is correlated with class size[124], although the two indicators measure different aspects of school organisation. Finally, teachers' characteristics are described in terms of teachers' education and in relation to class management by discriminating between those who assign homework to students on a regular basis and those who regularly correct the homework.

School resources differ significantly for beneficiaries and non-beneficiaries. As displayed in **Table A - 8**, beneficiaries tend to attend smaller schools on average, which could explain why they also attend schools with fewer resources when it comes to school facilities. Beneficiaries are found in higher proportions in schools without libraries or reading rooms, computer rooms, or laboratories. Also, for the two calculated indexes (facilities and equipment), differences, however small, are in favour of non-beneficiaries. Student-teacher ratio and class size do not differ significantly between the two groups and have lower statistical significance (**Table A - 9**). Although beneficiaries tend to attend schools with a slightly higher student-teacher ratio, the available data reveals a trend in favour of *BF* students in relation to class size. However, the difference in teachers' education is substantial between the two groups. There is a 7 p.p. difference in the proportion of teachers with higher education, with beneficiaries attending schools where teachers tend to be less qualified (as

121 The facilities include administrative rooms, resource rooms, sport areas, infant education spaces, sanitation facilities, alimentation areas, special education resource rooms, laboratories, and meeting rooms. The index is calculated by dividing the number of facilities in the school by the total number of facilities assessed in the National School Census.

122 The category includes devices such as audio-video equipment, photocopy machines and printers, overhead and PowerPoint projectors.

123 Mathematics, Portuguese Language, Social Studies, and Sciences.

124 The correlation coefficient is 0.54.

shown in **Table A - 10**). Also, beneficiaries are more represented in schools where teachers do not tend to correct the homework for both disciplines (around 3 p.p. difference). However, no significant differences are found between beneficiaries and non-beneficiaries regarding teachers who do not assign homework. School characteristics do not favour the already disadvantaged *BF* group. On the contrary, instead of compensating for family background by supplying an improved school environment, education systems seem to reinforce inequalities between the two groups, as beneficiaries attend schools that are less well-resourced than those attended by non-beneficiaries.

At this point is important to highlight that all of the differences presented between beneficiaries and non-beneficiaries may, in fact, be downgraded, since by October 2005 not all eligible families were registered in the programme[125] (MDS, 2005). It is likely that there are children amongst the non-beneficiaries for which the observed characteristics are likely to be very similar to those of beneficiaries. If this is true, the differences between the two groups are underestimated. In addition, as mentioned, *Prova Brasil* is only applied in urban schools; 23% of *BF* beneficiaries are enrolled in rural schools[126] that are not surveyed, probably further reducing the estimated differences between the two groups.

In sum, looking at how the two groups are distributed in relation to individual, household, and school characteristics it can be seen that, on average, *BF* students are not only socially disadvantaged (as expected for those eligible for the programme). They are also in a situation of educational disadvantage, entrenched in their personal school histories and in the schools they attend, which are, in many respects, less well-resourced than the schools attended by non-beneficiaries. As a result, the educational gap extends to school outcomes, measured by performance in test scores, as shown in the next section.

6.4 The achievement gap between beneficiaries and non-beneficiaries

I now look at the characteristics of the test score distribution, how test scores differ between beneficiaries and non-beneficiaries, and how the mean test score varies according to specific students' socioeconomic and school characteristics available in the dataset. The characteristics described in the previous section distinguish students and their performance on the exam. These characteristics also partially explain the existing achievement gap between beneficiaries and non-beneficiaries, and this section will also identify which of those amongst

125 According to the Ministry of Social Development, in October 2005 there were 8 million families already registered in *Bolsa Família* out of an estimated 11.2 million eligible families.

126 This figure is based on available data from 2007 for all beneficiary children.

them are the most relevant. Inasmuch as *BF* can influence some of these factors, it can potentially contribute to reducing the achievement gap.

6.4.1 Test scores distribution and association with income

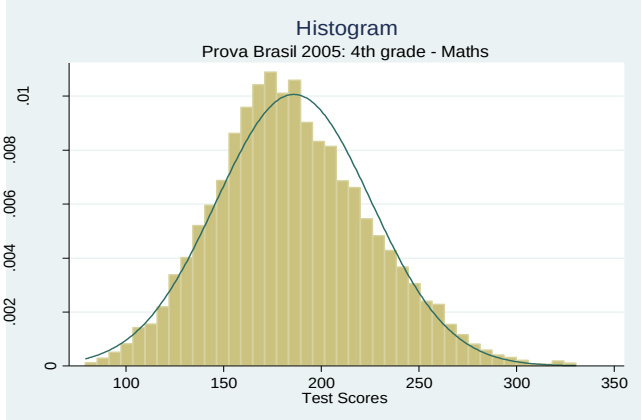
Table 11 shows the main statistics for the fourth grade test score distribution considering the sample taken from the *2005 Prova Brasil* national dataset.

Table 11: Test Score in Mathematics and Portuguese – 4th Grade/ 2005

Variable	N	mean	s.d.	p10	p25	p50	p75	p90	skewness	kurtosis
Mathematics	11,204	186.1	39.9	136.6	158.4	183.2	212.3	240.0	0.3	3.0
Portuguese	11,204	179.0	42.1	125.5	148.2	176.9	206.2	234.9	0.3	2.9

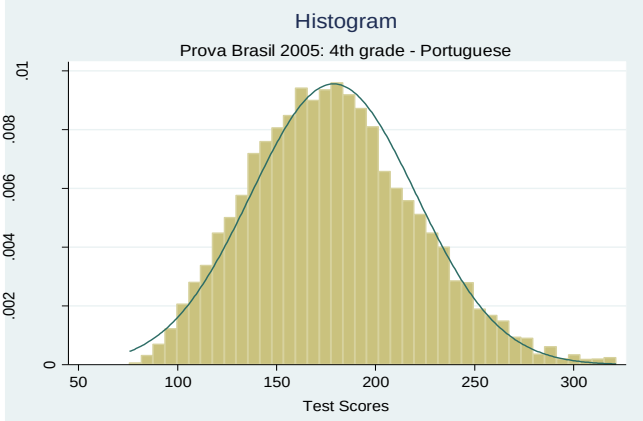
Based on the values for skewness and kurtosis[127] as well as on the shape of the distributions depicted in **Graph 10** and **Graph 11**, it can be concluded that the test score distribution for both disciplines assumes, with good approximation, the symmetrical characteristic of a *normal distribution*. Based on this approximation and the properties of the normal distribution, inferences can be made about the population parameters using estimated parameters from the sample.

Graph 10: Test Score Distribution – Mathematics 4th grade



127 Skewness and kurtosis statistics for a normal distribution are 0 and 3 respectively.

Graph 11: Test Score Distribution – Portuguese 4th grade

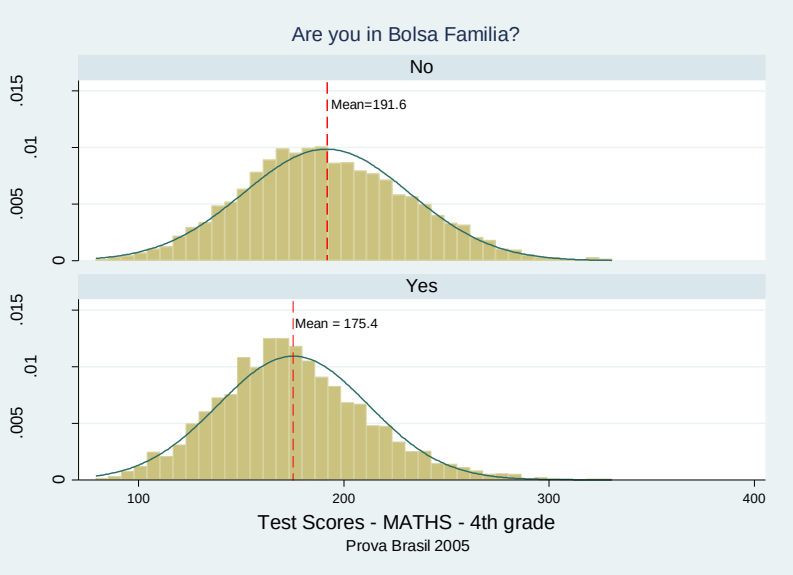


Fourth grade test scores in Portuguese Language and Mathematics for beneficiaries and non-beneficiaries are reported in **Table 12**. On average, *BF* beneficiaries perform less well than non-beneficiaries and below the national mean in both disciplines. The dispersion of results amongst beneficiaries is slightly smaller than for non-beneficiaries. The distributions for both disciplines by group are shown in **Graph 12** and **Graph 13**.

Table 12: Average 4th grade test scores in Mathematics and Portuguese by participation in *Bolsa Família*

4th Grade Test Scores	Are you in *Bolsa Família*?									
	No					Yes				
	N	Mean	s.d.	Min.	Max.	N	Mean	s.d.	Min.	Max.
Mathematics	7416	191.6	40.5	79.1	330.6	3788	175.4	36.5	84.10	330.41
Portuguese	7416	185.7	42.7	75.7	321.0	3788	166.0	37.6	81.0	321.2

Graph 12: Mathematics test score distribution by *BF* participation



Graph 13: Portuguese test score distribution by *BF* participation

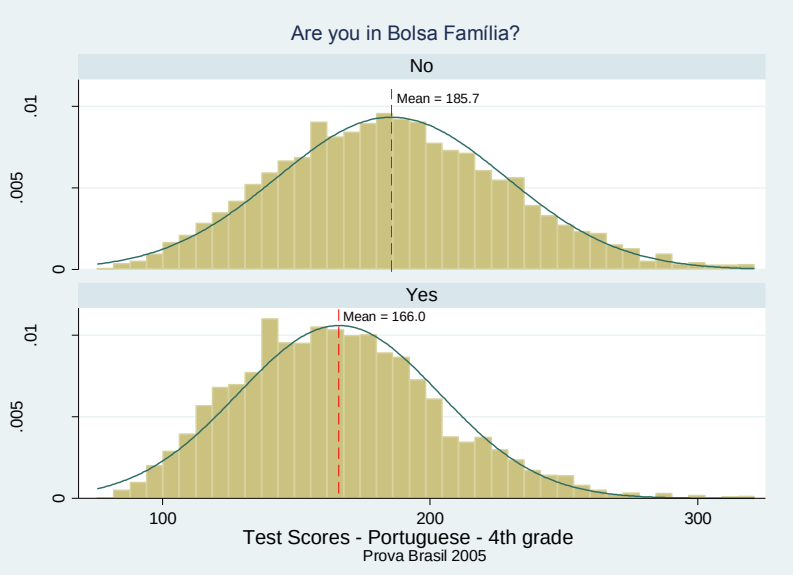


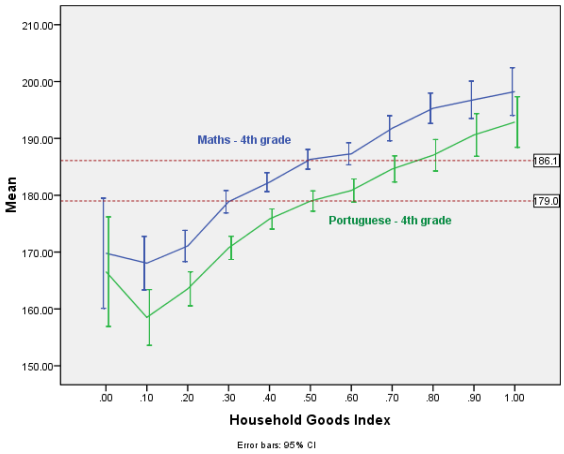
Table A - 11 and **Table A - 12** report the statistical tests for equality of mean test scores in Mathematics and Portuguese for the two sub-groups in the sample (beneficiaries and non-beneficiaries of *BF*). The respective differences **Δ=16.2** and **Δ=19.7** between the two groups are statistically significant and are equivalent to 65% and 77% of one cognitive level in the *Prova Brasil* proficiency scale. This means that, on average, beneficiaries are lagging behind by approximately two-thirds of one cognitive level in Mathematics and by three-quarters in Portuguese. Those differences correspond to approximately 0.4 and 0.5 s.d. from the national mean. The differences can also be translated into five and seven months of schooling respectively. Thus, we can see that although the two groups do not differ substantially in terms of the observed variance in each discipline, they do differ as to the mean achievement in test scores in both subjects, both in statistical terms and in practical significance. Why should this be the case? What can explain the gap?

Beneficiaries perform less well than non-beneficiaries on average. They come from the poorest households in the sample according to *BF* eligibility rules and they tend also to be disadvantaged in many respects that can negatively influence their performance at school. These disadvantages include the material conditions and learning environment at home, and family structure or parents' characteristics (including level of education, support, and encouragement for children's education). As stated by Jencks in his classic book, *Inequality*, "the way a family brings up its children is obviously influenced by its economic position" (Jencks, 1972, p.78). The association between a family's income level and children's school outcomes has long been reported in the literature (as discussed in chapter two), despite the controversial debate on the extent of such influence and whether it is a causal relationship.

Using *Prova Brasil* 2005 data, that association can also be found in the case of Brazilian children.

Because the socioeconomic variables available in *Prova Brasil* 2005 do not include household income, I use a proxy to estimate the mean household income and then look at the association with the mean test score. The proxy variable measures the number of durable goods in the household (household goods index)[128]. In examining the test score mean values by that proxy, a clear association between the mean test scores in both disciplines and the durable goods index can be observed, as shown in **Graph 14.** The dotted lines represent the mean test score value for the entire sample of fourth grade students in Mathematics and Portuguese Language. Across the range of values for durable goods, the mean test score in Mathematics is higher than in Portuguese. Assuming that the index for durable goods indirectly indicates the expected socioeconomic status in terms of the mean household per capita income, it can be argued that, on average, the higher the household income per capita the higher the child's school outcomes as measured by test scores. That, in principle, could explain the differences in test scores between beneficiaries and non-beneficiaries. To see whether that is the case, I look at the same association shown in Graph 14 for beneficiaries and non-beneficiaries when analysed separately.

Graph 14: Mean test scores by household goods index

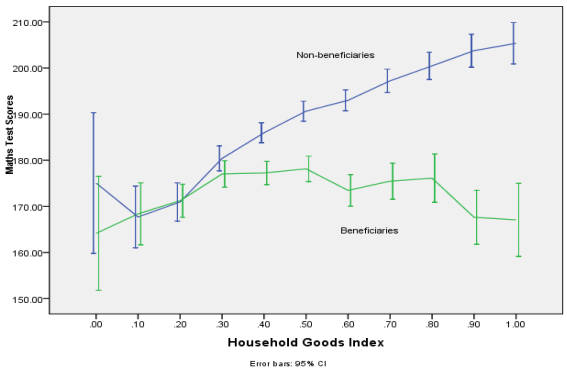


As shown in **Graph 15** and **Graph 16,** the mean test score for beneficiaries follows a different pattern. While the mean test score in both disciplines for non-beneficiaries continuously increases with the proxy for the mean household income per capita, the mean test scores for *BF* children do not present the same trend over the goods index. The test score

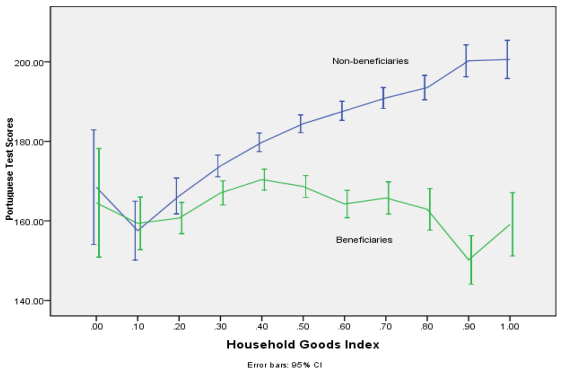
128 The validity of this variable as a proxy for the mean household income per capita is tested using 2005 data from the National Household Sample Survey (PNAD) for a subset of the same durable goods surveyed in the Prova Brasil socioeconomic questionnaire in 2005 (radios, televisions, washing machines, refrigerators, freezers, and computers). The A- Graph 1 in the appendix presents this association. This proxy is used here only to illustrate the association between family income and students' achievement in test scores. This is a very inefficient estimator of the household per capita income, as can be seen by the size of the standard deviation and the loss of accuracy as we move upwards in the goods index scale presented in Table A - 13. This means that a very poor family can also have a high goods index, although the association with the mean household income per capita is valid on average for the entire rank of goods index.

gap between the two groups does not exist for the first range of the goods index values (up to 0.3 in Mathematics and 0.2 in Portuguese). That could be explained by the fact that non-beneficiaries at those associated levels of income probably have family situations that are more similar to those eligible to take part in *BF* than to their non-beneficiary counterparts, although they were not yet registered in 2005[129]. The two groups then detach from each other and the gap in terms of mean test scores increases across the levels of the goods index. Why does this happen? Why does the beneficiary group's mean not follow a clear increasing pattern (tending to decrease across the upper ranks of the goods index)?

Graph 15: Mean test scores (Maths) by household goods index and participation in *Bolsa Família*



Graph 16: Mean test scores (Port.) by household goods index and participation in *Bolsa Família*



An evident explanation is that the durable goods index, as a proxy for household per capita income, does not account for the variance in this variable, and it also loses accuracy as we move upwards on the scale as made clear by the increasing s.d. presented in **Table A - 13**. This means that a very poor family can also have a high goods index in the sample. Although the association between mean test scores and mean household income per capita (as measured by the goods index) is valid, on average, for the entire rank of goods index in the population, the goods index is an inaccurate proxy for the average income for the beneficiary group. This group is located at the lower extreme of the income distribution in each level of the goods index. In fact, since the threshold for a family to take part in the programme in 2005 was R$ 100[130] (approximately US$ 45) per capita, we can assume that for the beneficiary group the income level is actually constrained throughout the ranks of the goods index, while that "ceiling" does not apply for non-beneficiaries. In that sense, the trend lines in Graph 15 and

129 As explained earlier, by 2005 the number of poor families registered in the programme was around 8 million out of 11.2 million estimated poor families.

130 This threshold was established by the Presidential Decree No 5.209, 17 September 2004, for families with school-age children. For families in extreme poverty without children, the threshold was 50 *reais* per capita (approximately US$ 23 in 2005).

Graph 16 are, in fact, representing the achievement gap in test scores between the two groups at different levels of non-beneficiaries' income, since the mean household income per capita for the non-beneficiary group increases while the beneficiaries' mean household income is roughly within the same band. Therefore, on average, the achievement gap between beneficiaries and non-beneficiaries increases with the increasing gap in the household per capita income.

Nevertheless, there are important factors other than income intervening in children's outcomes that also characterise the condition of deprivation, and that might contribute to the gap between those groups across income levels. For instance, as previously described in this chapter, beneficiaries attend schools where teachers tend to be less qualified. If teachers' pre-service education is positively correlated with students' test scores, then the negative difference in test scores for beneficiaries could be explained, at least partially, by the fact that they are attending schools with less qualified teachers. It might be the case that the difference in test scores between the two groups is due also to the fact that beneficiaries tend to be in the labour force in higher proportions than non-beneficiaries. This would make the time allocated to school activities, including homework and study at home, more restricted for beneficiaries. Parents' characteristics could also have an influence in producing that difference, since beneficiaries' parents tend to be less educated on average than the parents of non-beneficiaries. If parents with more education tend to be more effective in supporting children in school activities at home, the difference in test scores could be due to the education gap between the parents of the two groups. Controlling for variables such as these will give us a better understanding of at least some of the factors other than income explaining that difference.

6.4.2 How do test scores vary according to *Bolsa Família* participation taking into account specific characteristics of students, families, and schools?

One way to condition out possible influences on the mean test score difference between beneficiaries and non-beneficiaries is to estimate the coefficient associated with the indicator of programme participation while controlling for other possible factors in a regression function, as described below. Using multiple regression analysis, this section explores how test scores vary according to different factors related to students' social background and school environment, particularly family participation in *BF*. Different models are created by introducing additional explanatory variables in an attempt to assess their influence on the observed gap between beneficiaries and non-beneficiaries. By adding sets of

variables in the successive models, I examine how their inclusion interferes with the size and direction of the estimated coefficients already in the model, especially the parameter **α** associated with the variable indicating *BF* participation. The general model to be estimated is represented below, whose variables are described in the following paragraphs.

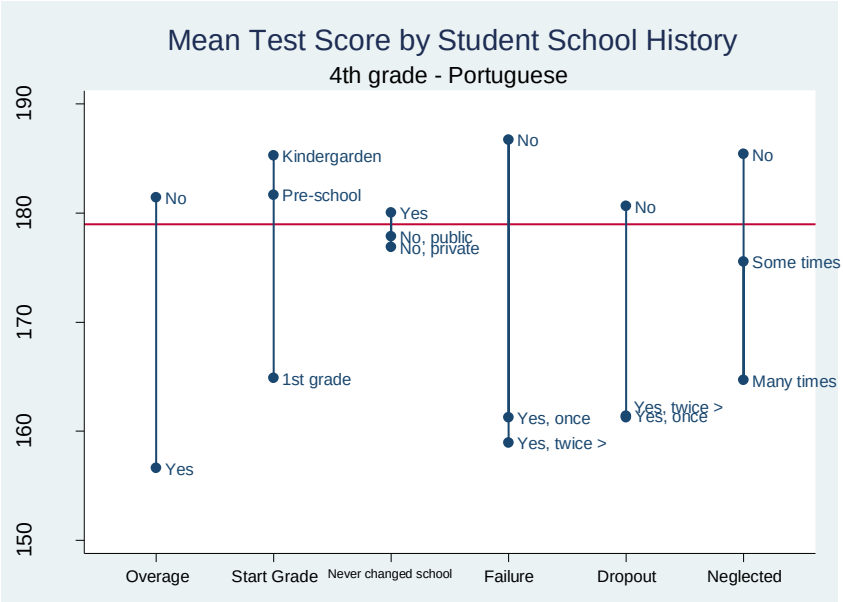
Model 1

$$y_i = \beta_0 + \sum_{k=1}^{n} \beta_k X_{ki} + \alpha . BF_i + u_i$$

In **Model 1** the variable $\mathbf{y_i}$ represents the test score for the student **i**; $\mathbf{X_{ki}}$ represents the observed characteristic **k** for the student **i**; $\boldsymbol{BF_i}$ is a dummy variable expressing whether the student's family takes part in *BF*; and $\mathbf{u_i}$ is the error term expressing all other factors affecting test scores that are not included in the model. The factors (X_{ki}), to be introduced in the model, are those describing student, household, and school characteristics.

Since all available variables in the 2005 *Prova Brasil* socioeconomic questionnaire are categorical, in some cases containing several levels, I reduced the number of categories whenever convenient for the sake of simplification and parsimony. This is done by collapsing variable levels based on the substantial similarity of the categories involved and also on the differences observed in test scores between the categories. For instance, taking the students' school history variables and plotting the mean test score in Portuguese Language for each category against the overall mean (**Graph 17**) we observe that all the variables in the set can be collapsed into binary values.

Graph 17: Mean Test Score by Student School History



Some of the categories are clustered on one side of the red line, which represents the mean test score considering all students in the sample (179 points). For instance, the overage variable is already a binary variable[131], but the entrance grade variable can also be collapsed into two categories: those students entering at any pre-school[132] level and those starting in the first grade. School swap can also be collapsed into those who have changed schools and those who did not change schools before fourth grade. Failure at any grade during the first four years of schooling can be reduced to two categories: those who failed and those who did not fail; the same is applied to dropouts. Finally, the impression of being neglected in the classroom can be also converted into a binary variable by collapsing those declaring to have felt neglected many times or sometimes, so forming a common category of students who experienced isolation in school, against those declaring not to have had such an experience. The same logic is applied to all the other categorical variables used in the regression analysis (except region)[133]. The mean test score in Portuguese Language, plotted by the remaining categorical variables, is displayed in **A- Graph 2** to **A- Graph 7** in the appendix; and the resulting sets of variables used in the regression analysis are described in **Table 13**[134].

131 It is constructed by considering students who are two years or more above the recommended age (10 years old) for the fourth grade as overage students.

132 At the time the questionnaire was created, pre-school in Brazil was formally understood to be for children ages of 4 to 6 years old (a non-compulsory level of education) and was informally grouped into "maternal" education (for 4 year-olds), "jardim de infância" (for 5 year-olds), and "pré-escola" (sometimes meaning any of the previous and/or for 6 year-olds). As the questionnaire uses three categories, I translate them here as follows: "jardim de infância / maternal" is designated as "kindergarten"; "pré-escola" is designated as "pre-school", and "primeira série" is designated as the first grade. The first two categories are collapsed into "pre-school" in line with the official definition.

133 In the case of the variables indicating durable goods, they are used to create a new variable (durable goods index) ranging from zero to one.

134 Table A - 14 to Table A - 16 in the appendix show the statistical difference between beneficiaries and non-beneficiaries with respect to these variables.

Table 13: Description of variables used to estimate Model 1

VARIABLES		DESCRIPTION
Demographic Characteristics	***Bolsa Família***	Dummy variable equal to 1 if the pupil is *BF* beneficiary.
	Male	Dummy variable equal to 1 if the pupil is male.
	Non-white	Dummy variable equal to 1 if the pupil is non-white.
School History	Overage	Dummy variable equal to 1 if the pupil is two years or more above the recommended age for the 4th grade (10 years-old)
	Pre-school	Dummy variable equal to 1 if the pupil started school before the 1st grade.
	Same school	Dummy variable equal to 1 if the pupil has not changed school since the 1st grade.
	Fail	Dummy variable equal to 1 if the pupil has failed any grade in past years.
	Dropout	Dummy variable equal to 1 if the pupil has dropped out any time in past years without returning until the end of the academic year.
	Neglected	Dummy variable equal to 1 if the pupil declares feeling neglected in the classroom.
Habits and Expectations	Watch-TV	Dummy variable equal to 1 if the pupil declares watching TV at home.
	Domestic work (2h or more)	Dummy variable equal to 1 if the pupil declares working at home for two or more hours a day.
	Child Labour	Dummy variable equal to 1 if the pupil declares being engaged in any form of work outside home.
	Do Homework Port.	Dummy variable equal to 1 if the pupil declares doing Portuguese Language homework at least once in a while.
	Do Homework Maths	Dummy variable equal to 1 if the pupil declares doing Mathematics homework at least once in a while.
	Intend to continue studies	Dummy variable equal to 1 if the pupil declares intention to continue studies after concluding compulsory education.
Material Environment	Electricity	Dummy variable equal to 1 if the pupil declares living in a house with power supply.
	Tap Water	Dummy variable equal to 1 if the pupil declares living in a house supplied with piped water.
	Bedroom	Dummy variable equal to 1 if the pupil declares living in a house with at least one bedroom.
	Bathroom	Dummy variable equal to 1 if the pupil declares living in a house with at least one bathroom.
	Dictionary	Dummy variable equal to 1 if the pupil declares living in a house with at least one dictionary.
	Peaceful place to study	Dummy variable equal to 1 if the pupil declares having a peaceful place to study at home.

VARIABLES		DESCRIPTION
	Books ≥ 20	Dummy variable equal to 1 if the pupil declares living in a house where there are 20 or more books.
	Computer	Dummy variable equal to 1 if the pupil declares living in a house with a computer (with or without internet).
	Goods (0-10)	Number of durable goods at pupil's home measured amongst 10 items surveyed in the socioeconomic questionnaire.
Family Structure	Family size (≥ 7)	Dummy variable equal to 1 if the pupil declares living in a house with 7 or more people.
	Both parents	Dummy variable equal to 1 if the pupil declares living with both parents.
Parents' Characteristics	Mother Educ. (post-primary)	Dummy variable equal to 1 if the pupil declares the mother has completed secondary school or College.
	Father Educ. (post-primary)	Dummy variable equal to 1 if the pupil declares the father has completed secondary school or College
	Parents in school meetings	Dummy variable equal to 1 if the pupil declares parents often attend school meetings.
	Parents encouragement (0-4)	Index number ranging from 1 to 4 measuring parents incentives to pupil's education. The four binary components are: incentive to study, incentive to do homework, incentive to read and incentive to go to school.
School Infra-Structure	Facilities (0.0 - 1.0)	Index number ranging from 0 to 1 measuring the availability of school facilities amongst 34 items measured by the National School Census in the school attended by the pupil.
	Equipment (0.0 – 1.0)	Index number ranging from 0 to 1 measuring the availability of school equipment amongst 19 items measured by the National School Census in the school attended by the pupil.
Teachers	Student/teacher ratio	Number of pupils enrolled in grades 1 to 4 divided by the number of teachers working in grades 1 to 4 in the school attended by the pupil.
	Prop. teachers HE	Proportion of teachers with higher education working in grades 1 to 4 in the school attended by the pupil.
	Teacher correct homework (Port.)	Dummy variable equal to 1 if the pupil declares teacher often corrects Portuguese Language homework.
Region	North	Dummy variable equal to 1 if the pupil's school is located in the North region.
	Northeast	Dummy variable equal to 1 if the pupil's school is located in the Northeast region
	Central-West	Dummy variable equal to 1 if the pupil's school is located in the Central-West region
	South	Dummy variable equal to 1 if the pupil's school is located in the South region

(1) Student Characteristics and Test Scores

The regression analysis is carried out using test scores in Portuguese Language as the dependent variable. **Table 14** reports the regression coefficients and robust standard errors for the three models encompassing students' characteristics and how the coefficient for *BF* participation changes across these models.

Table 14: Regression of Portuguese Test Score on STUDENT CHARACTERISTICS – 4th grade

	VARIABLES	(1) Portuguese	(2) Portuguese	(3) Portuguese	(4) Portuguese
	Bolsa Família	-19.7***	-18.5***	-14.1***	-13.0***
		(0.787)	(0.784)	(0.761)	(0.752)
	Male		-7.8***	-5.7***	-4.8***
Demographic Characteristics			(0.768)	(0.736)	(0.738)
	Non-white		-9.5***	-6.3***	-6.0***
			(0.809)	(0.771)	(0.757)
	Overage			-7.4***	-4.7***
				(1.266)	(1.261)
	Pre-school			11.9***	10.8***
				(0.831)	(0.823)
Student's School History	Same school			-0.5	-0.3
				(0.745)	(0.733)
	Fail			-17.3***	-16.2***
				(0.827)	(0.816)
	Drop			-7.1***	-4.7***
				(1.335)	(1.321)
	Neglected			-10.4***	-9.1***
				(0.733)	(0.724)
	Watch TV				6.2***
					(0.990)
	Domestic Work (2h or more)				-9.1***
					(0.965)
	Child labour				-13.3***
					(1.003)
Student's Habits and Expectations	Do Homework Port.				10.0***
					(1.415)
	Do Homework Maths				3.4*
					(1.813)
	Intend to continue studies				3.6***
					(0.735)
	Constant	185.7***	195.1***	193.1***	174.8***
		(0.496)	(0.778)	(1.153)	(2.380)
	Observations	11,204	11,204	11,204	11,204
	R-squared	0.049	0.070	0.162	0.191

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Student characteristics alone explain 19% of the variance in test scores and reduce the gap between beneficiaries and non-beneficiaries by around 34% (from -19.7 to -13.0 points). Several factors stand out as contributors to students' performance in test scores. Male and non-white students are at a disadvantage in the Portuguese exam, regardless of their status as

beneficiaries or non-beneficiaries. Other factors related to students' school histories, habits, and expectations reveal potential influences on their performance.

School history has strong explanatory power with respect to test scores, since its inclusion more than doubles the explained variance, as seen in the R^2 reported in model (3). Three factors stand out: (i) early childhood education, as measured by pre-school participation; (ii) the experience of failing; and (iii) the experience of being neglected in the classroom. First, pre-school attendance holds a significant and positive coefficient in the regression model on test scores achievement, estimating approximately 0.2 to 0.3 s.d. increase on average. In the literature, early childhood education is strongly supported as a major contributor to students' future performance at higher grades in their school life (Leuven and Oosterbeek, 2007; Esping-Andersen, 2009, p. 132-7). Pre-school not only prepares children for primary school, but, in general, also means better nutritional care in childhood for the poorest children. It can also bridge the transition between home and school for the youngest, giving them more self-confidence and precocious stimulus for school activities. Amongst *BF* beneficiaries, those who attended pre-school are less likely to have dropped out or failed during the first four years of primary school[135].

Second, by failing a grade during the early school years a child can see his/her results in Portuguese at the fourth grade reduced by approximately 0.4 s.d. according to model (4). By driving a child to repeat a grade the school system is probably inflicting a deleterious effect on her/his self-esteem, causing more harm than benefit in the school years to come. There are no learning gains in repeating a grade; on the contrary, on average repeaters perform less well following this experience. It is also worth noting that when the variable for grade failure is included in the model, the overage coefficient loses statistical significance after all controls are considered (see **Table A - 17**). This is not surprising, since failing a grade is an obvious cause of being overage[136] in subsequent grades. 'Overage' is a cumulative or stock variable recording students' previous school histories. Either by failing and consequently repeating a grade, or by dropping out and returning to the same grade in the following year, the pupil enters the overage group and increases the school age-grade distortion.

135 Amongst beneficiaries, 36% of those who attended pre-school failed a grade over their first four years of primary education, against 49% of those who did not attend pre-school. For non-beneficiaries, the figures are 21% and 38% respectively. For both groups, early childhood education is positively associated with fewer chances of failing a grade.

136 The overage variable indicates those pupils two years or more above the recommended age for the fourth grade (10 years-old). Overage has been reported in the literature as a relevant factor associated with students' school achievement. In general, students who experienced multiple repetitions and/or late entrance to school end up composing a group ahead of the recommend age for grade. They also perform worse in school, on average.

Third, being ignored in the classroom has a significant negative effect on test scores, as estimated in the model, and is a subjective element concerning students' perceptions of teachers' attitudes towards them[137]. The feeling of being neglected or dispensed insufficient attention in the classroom can negatively affect students' self-esteem and can also contribute to their isolation and loss of interest in school matters. On average, the lack of attention and support from teachers felt by students is roughly associated with a 0.2 s.d. decline on test scores.

The second set of individual characteristics analysed is students' habits and expectations about their future schooling. Three major factors stand out: (i) home chores; (ii) child labour; and (iii) homework. First, a significant negative effect on students' performance is captured by the variable indicating home chores done by children for two hours or more on a daily basis. By allocating time to domestic work, children lose around 0.2 s.d. on average in their performance. Second, and in the same direction, being involved in any form of child labour[138] negatively impacts students' performances, and is the second largest effect amongst all the variables (0.3 s.d.). These two variables have independent effects on test scores. Third, students' habits of doing homework (in this case, Portuguese Language homework) have a significant positive effect on test scores (0.2 s.d.). Whether the student does mathematics homework has no effect on Portuguese test scores, but having expectations of carrying on studies after completing fundamental education is positively correlated with test scores.

(2) Household Characteristics and Test Scores

Household factors, when taken alone, explain 14% of the variance in test scores and also explain some of the differences in test scores between beneficiaries and non-beneficiaries, as shown in **Table 15** (a reduction of 26% from -19.7 to -14.6).

137 The dummy variable captures those students stating different levels of experience in feeling ignored in the classroom, ranging from "many times" to "once in a while", against not having or rarely having such a feeling.

138 Child labour here refers to any form of outside home work in which children allocate time.

Table 15: Regression of Portuguese Test Score on HOUSEHOLD CHARACTERISTICS – 4th grade

	VARIABLES	(1) Port_4th	(2) Port_4th	(3) Port_4th	(4) Port_4th
	Bolsa Família	-19.7***	-16.7***	-15.8***	-14.6***
		(0.787)	(0.792)	(0.791)	(0.785)
Material Environment	Electricity		17.4***	16.2***	13.2***
			(2.233)	(2.220)	(2.196)
	Tap Water		11.3***	10.6***	9.0***
			(1.319)	(1.304)	(1.293)
	Bedroom		12.9***	12.3***	10.7***
			(3.846)	(3.748)	(3.648)
	Bathroom		11.7***	11.4***	10.4***
			(2.191)	(2.177)	(2.118)
	Dictionary		9.5***	9.4***	7.2***
			(1.214)	(1.202)	(1.189)
	Peaceful place to study		5.4***	4.5***	1.9*
			(1.066)	(1.062)	(1.057)
	Books ≥ 20		-3.4***	-2.6***	-3.8***
			(0.855)	(0.852)	(0.838)
	Computer		3.7***	3.7***	1.3
			(1.188)	(1.177)	(1.174)
	Goods (0 - 10)		1.7***	1.4***	0.7***
			(0.245)	(0.244)	(0.243)
Family Structure	Family size (≥ 7)			-10.5***	-9.5***
				(0.863)	(0.856)
	Both parents			4.4***	3.8***
				(0.809)	(0.801)
Parents' Characteristics	Mother Educ. (post-primary)				8.4***
					(1.058)
	Father Educ. (post-primary)				3.2***
					(1.048)
	Parents in school meetings				8.0***
					(0.756)
	Parents encouragement (0-4)				5.8***
					(0.491)
	Constant	185.7***	111.7***	115.7***	101.7***
		(0.496)	(4.628)	(4.565)	(4.602)
	Observations	11,204	11,204	11,204	11,204
	R-squared	0.049	0.093	0.106	0.137

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Factors related to the household's material environment, although explaining only a minor part of the variance in test scores, are very good discriminators of the poorest families. Households with no bathroom or no bedroom, or as yet deprived of power and water supplies are probably amongst the poorest in the country. Children from families living in such environments are at a significant disadvantage, as can be seen by the associated coefficients shown in Table 15**.** Looking at material factors related to what I call the "learning environment at home" (dictionaries, books, computers, and a peaceful place to study) the dummy variable for dictionary is the only one to keep its significance, even when all other controls are included

in the regression (**Table A - 17**)[139]. The amount of durable goods does not hold any practical significance for test scores in the context of the other household characteristics.

Family structure (size and having both parents) is also influential on test scores. Larger families in deprived contexts generally face more hardships, but can also provide extra hands to work as breadwinners. Large families in the examined sample are associated with the presence of child labour[140] and can suggest less time allocated per child when education support at home is considered. This is reflected in the negative coefficient associated with families with seven or more members living in the same household (a reduction of 0.2 s.d.). Results also show an advantage for children living with both mother and father, as opposed to those living with a lone parent or with no parents at all. This can reflect the household's income, but also more support from parents and a more stable environment for children. Looking at the disaggregated data, lone parenting has a negative association with children's performance (with lone fathers as the worst).

Parents' characteristics, particularly the mother's education, are positively associated with children's performances on test scores. This can reflect two different circumstances. Mothers can be more present in children's day-to-day life, even when at work or raising children on their own; they can also play the main role as caregivers in the family, while fathers are the main breadwinners. It is worth noting that children whose parents hold a secondary school degree are at more of an advantage in relation to children whose parents have less than four years of education, than children whose parents hold a degree in higher education. Other results can help to understand this trend. A higher proportion of parents with secondary education attend school meetings (66%)[141] and they also have a higher mean in the index for parents' encouragement for children's education (3.78). A possible explanation is that parents' who achieved secondary education are keener to make an extra effort so that their children can achieve higher levels of education. Parents' encouragement for children's education and attendance at school meetings are also significantly correlated with test scores. Nevertheless, the relationship between parents' attendance in school meetings and test scores could actually be working in the opposite direction. Parents whose children perform better at school end up feeling proud of them and they are willing to go to school to hear compliments from teachers, whereas parents whose children are performing poorly or are known by their

139 In this case, having a computer becomes statistically significant.

140 Cross-tabulation between child labour and family size shows that the larger the family size, the higher the proportion of children engaged in child labour.

141 The percentage for higher education holders is 64%, for fundamental education (eighth grade) holders it is 59%, and for those with fourth grade or less it is 57%.

bad behaviour at school may feel embarrassed or be unwilling to attend school meetings. Both factors remain significant, even after controlling for all variables in the full model (**Table A - 17**) representing a 0.1 s.d. increment for test scores.

(3) School Characteristics and Test Scores

The regression of Portuguese Language test scores on school factors is reported in **Table 16**. The output shows that school factors not only explain the smallest share of the variance in test scores so far (around 10%), but is also the set of factors that has the smallest effect in reducing the observed gap between beneficiaries and non-beneficiaries in test scores (from -19.7 to -16.8, around a 15% reduction).

Table 16: Regression of Portuguese Test Score on SCHOOL CHARACTERISTICS – 4th grade

	VARIABLES	(1) Portuguese	(2) Portuguese	(3) Portuguese	(4) Portuguese	Beta for Model (4)
	Bolsa Família	-19.7***	-17.3***	-17.3***	-16.8***	-0.189
		(0.787)	(0.784)	(0.783)	(0.778)	
Infrastructure	Facilities (0.0 - 1.0)		29.1***	27.2***	21.9***	0.056
			(4.100)	(4.110)	(4.144)	
	Equipment (0.0 – 1.0)		36.7***	35.2***	30.2***	0.107
			(2.980)	(2.982)	(3.050)	
Teachers	Student/teacher ratio			-0.3***	-0.3***	-0.052
				(0.046)	(0.046)	
	Prop. teachers HE				8.2***	0.064
					(1.236)	
	Teacher correct homework (Port.)				10.2***	0.102
					(0.862)	
	Constant	185.7***	158.6***	169.0***	158.5***	
		(0.496)	(1.371)	(2.002)	(2.097)	
	Observations	11,204	11,204	11,204	11,204	
	R-squared	0.049	0.081	0.085	0.099	

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In interpreting the coefficients for the index numbers (facilities and equipment) it should be noted that as the maximum value is 1.0 (meaning that 100% of the surveyed facilities / equipment exists in the school) the coefficient actually represents a "full" expected effect; that is, the expected increase in the student test score if a school has 100% of the surveyed facilities (equipment) as compared to one with no facilities (equipment) at all. For example, the coefficient for facilities (21.9) reported in model (4) says that if a school has, for instance, 50% of the facilities surveyed in the National School Census, on average, and holding all other factors constant, a student's test score in Portuguese is expected to be around 11

points higher in comparison to what it would be if the school had no facilities at all. In order to facilitate the comparison between the coefficients I report the standardised beta coefficients in the last column of Table 16 for model (4).

School infrastructure has a positive association with test scores, both in terms of facilities and in equipment available, the latter presenting an estimated effect that doubles that for facilities (see standardised betas). Student-teacher ratio has a negative effect; an increment of 10 students per teacher is expected to reduce a student's test score by three points in the exam scale, though this is less than one-tenth of an s.d. The effect size of student-teacher ratio compares to that of school facilities, but in the opposite direction. Teachers' education is positively associated with students' results, but has a smaller effect than the practice of correcting homework in the class on a regular basis. A gain of around 10 points, or 0.25 s.d., is estimated for a student whose teacher usually assigns and corrects the homework. This factor compares to the effect associated with a fully-equipped school. Part of these results does not hold when the "full model" is analysed, as shown in **Table A - 17**.

6.4.3 What explains the achievement gap between beneficiaries and non-beneficiaries?

As summarized in **Table 17**[142] the test score gap in Portuguese Language between beneficiaries and non-beneficiaries, taken at *face value,* is around 20 points in the proficiency scale (model (1)). This corresponds to 80% of one cognitive level in that scale, which is also equivalent to approximately seven months of schooling or 0.5 s.d. in the observable results for the 2005 national exam. This *prima facie* value is reduced inasmuch as additional factors are considered, while estimating the performance gap between the two groups as shown in Table 17. Students' characteristics reduce the gap by 34%, amongst which the subset of variables standing for students' school history is by far the most significant set of factors explaining that gap, since it reduces the gap by around 24% after taking into account demographic characteristics (gender and race). An additional 8% reduction is observed due to factors describing students' habits (time allocation when not at school) and expectations. Household characteristics contributed to a reduction of the gap by 15.4%; the major contributors are the household material environment and parents' characteristics. School characteristics and geographic region have rather weak explanatory power when compared to the first two sets of characteristics, reducing the gap by 4.5% and 2.9% respectively. Curiously, adding teachers'

142 Details can be seen in Table A - 17.

attributes does not change the gap any further. The three teacher-related variables do not explain any portion of the differences in test scores captured by the *BF* indicator variable.

Table 17: Test score gap between beneficiaries and non-beneficiaries – Portuguese 4th grade

		Students' Characteristics			Household Characteristics			School Characteristics		Region
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CONTROL ADDED	No control	Demog.	School History	Habits Expect.	Material Environ.	Family Structure	Parents Charact.	Infra-structure	Teachers	Region
Bolsa Família	-19.7***	-18.5***	-14.1***	-13.0***	-12.0***	-11.6***	-11.0***	-10.5***	-10.5***	-10.2***
	(0.787)	(0.784)	(0.761)	(0.752)	(0.757)	(0.757)	(0.754)	(0.750)	(0.748)	(0.745)
Δ% Gap	-	-6.1%	-23.8%	-7.8%	-7.7%	-3.3%	-5.2%	-4.5%	0.0%	-2.9%
	-	-	-	-34.0%	-	-	-15.4%	-	-4.5%	-2.9%
Observ.	11,204	11,204	11,204	11,204	11,204	11,204	11,204	11,204	11,204	11,204
R^2	0.049	0.070	0.162	0.191	0.206	0.210	0.224	0.232	0.235	0.244

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Despite attempts to "absorb" the differences in test scores by controlling for observable attributes usually applied in production-function studies, the performance gap still holds, although reduced by around 50% (from 20 to 10 points). The limitation of the available variables in capturing all relevant factors affecting test scores can explain the remaining achievement gap. The explanatory econometric models attempted in this chapter explain, at best, no more than 24% of the observed variance in test scores. If non-observable variables are also correlated with participation in *BF*, then the remaining gap could be reduced even further by their inclusion in the model. Child poverty is probably only partially characterised by the available socioeconomic data from *Prova Brasil* 2005[143] and the omitted variables are captured in the *BF* dummy variable. This means that participation in the programme still conceals characteristics other than those explored in this chapter that explain why *BF* recipient children tend to perform worse than their counterparts.

6.5 Conclusions

In this chapter I have investigated *BF* beneficiaries' achievement in test scores as compared to non-beneficiaries. First, I looked at differences between beneficiaries and non-beneficiaries regarding individual, household, and school characteristics. Second, I considered whether or not significant differences in test scores exist between beneficiaries and non-beneficiaries. Third, I investigated whether differences regarding the observed characteristics can, to some extent, explain the test sore gap between the two groups.

143 Since household income is not available for students in Prova Brasil, poverty cannot be properly dimensioned and characterised, even in the very limited sense of monetary poverty.

Beneficiaries are disproportionally concentrated in the Northeast region of Brazil, are more represented amongst boys and non-whites, and tend to be overage in a higher proportion than non-beneficiaries. Beneficiaries are more often those with poor school records in terms of pre-school participation, grade failure, and dropouts, and also tend to be amongst those who declare that they have been neglected in the classroom. Household conditions also tend to put beneficiaries at a disadvantage. They not only live in more crowded and materially poorer households, but they are also more likely to be engaged in domestic or paid work, compromising the time dedicated to study at home. Their parents are often less educated and less supportive of school-related activities. Beneficiaries also tend to attend less well-resourced schools, either in terms of infrastructure or human resources. Despite all these disadvantages, beneficiaries seem to share with non-beneficiaries an optimistic perspective on their future. When asked about continuing studies beyond fundamental education, they are no different from non-beneficiaries. A majority of both groups aims at achieving secondary education. However, beneficiaries are lagging behind in the national exam compared to their non-beneficiary peers — a gap that increases with income inequality.

The analysis of the three blocks of variables, namely student, household, and school characteristics, reveals how influential those factors are in explaining the achievement gap between beneficiaries and non-beneficiaries. Student characteristics have significant explanatory power with respect to how students perform on the national exam, explaining 19% of the variance in test scores. Particularly relevant are variables reporting children's past experiences in school, especially regarding pre-school attendance, failing any grade, and experiencing a lack of attention or isolation in the classroom. These factors alone can represent an estimated difference of 28 points in the test scores for Portuguese Language, a difference equivalent to 0.7 s.d. (equivalent to one cognitive level or one year of schooling). Habits at home in terms of how children have their time allocated when not at school also have an influence on their test scores. Harmful effects are associated with time allocated to any form of child labour, either at home or outside the home, while doing homework is clearly beneficial. As to students' expectations about their future after finishing the compulsory cycle of fundamental education, a positive association with test scores is observed for those who express the intention of either being only a student or combining study and work in the future.

Household characteristics taken in isolation from other variables explain 14% of the variance in test scores, but add only 3.3 p.p. in the explained variance after students' individual characteristics are controlled for. Household material poverty, family structure, and parents' education and commitment to their children's education are also associated with achievement

in test scores. Although a household's material conditions cannot fully describe poverty, the estimated differences in test scores observed suggest that severe material deprivation has a deleterious effect on students' performances[144]. Living in a large family reduces test scores (around 0.1 s.d.), while living with both parents can compensate for half of that reduction. Mothers' education has a strong influence on test scores (0.15 s.d.) while fathers' education is only marginally significant, with only one-quarter of the mothers' influence. Both parents' encouragement and participation in school meetings are positively associated with test scores (0.1 s.d.).

School factors are the least influential amongst the investigated variables. The proportion of teachers with higher education is not relevant, nor is the number of facilities and the student-teacher ratio. Only school equipment and teachers' practices of assigning and correcting homework are significant, although only the latter holds practical significance. Finally, regional differences still hold and explain a significant 1% of the variance in test scores.

Inasmuch as individual and household social disadvantages are more relevant than school factors in explaining the achievement gap between beneficiaries and non-beneficiaries, one might expect policies focused on mitigating some of the social disadvantages (such as *BF*) to be more promising in reducing the achievement gap. In this case, demand-side interventions linking social assistance with education would be more successful in improving school results for children found in the conditions such as those described for *BF* beneficiaries than "pure" educational policies focused on school inputs and processes.

144 For instance, children living in homes without power supply, piped water, or sanitation facilities such as bathrooms.

Chapter 7. Looking for evidence of *Bolsa Família* effects on school outcomes

7.1 Introduction

The long-term poverty reduction objective of *Bolsa Família* (*BF*) relies on its capacity to deliver substantive social and educational benefits to children in the present. That is, effects should be seen during the time when children are expected to be in school, attending classes on a regular basis, learning, and progressing towards upper levels of the educational system. Only by influencing children's educational outcomes can *BF* expect to contribute to children's future social and economic opportunities, thereby reducing the risk of future poverty. As discussed in chapter three, most of the research produced thus far about the impacts of CCTs on educational outcomes has focused on school enrolment and attendance rates, and has gathered consistent evidence of positive and significant effects on participants' schooling insofar as access and attendance is concerned (Fiszbein and Schady, 2009). However, the lack of evidence of the contribution of CCTs to beneficiaries' school performance, progress, and completion of basic education has raised some scepticism regarding the value of these initiatives in promoting long-term objectives of human capital accumulation. If that is true, then CCTs will fail poor children in their promise to bring them out of the intergenerational poverty cycle.

Despite the achievement gap between beneficiaries and non-beneficiaries discussed in chapter six, *BF*'s socio-educational rationale (see chapter four) predicts that by taking part in the programme children will improve their educational outcomes. These improvements will not only be in terms of enrolment at the proper age and in higher attendance rates, as reported in the literature, but also in terms of learning outcomes, promotion rates, and completion of basic education. The **first** hypothesis analysed in this chapter is that ***time of participation*** is a key variable in assessing whether *BF* has any significant contribution to beneficiaries' educational outcomes. This hypothesis is underpinned by four ideas: (i) permanent income[145] defines families' patterns of consumption and well-being; (ii) permanent income can be affected by assured cash transfers *over time* and produce changes in the household's pattern of consumption[146]; (iii) it takes time for improvements in consumption

145 Permanent income is usually measured as income averaged over a period of time. In this sense, cash transfers would accrue to permanent income after a time lag.

146 Patterns of consumption can also improve slowly or proportionally less than the amount of cash transferred due to possible contra-effects of the programme on families' income, for instance, if losses of income occur due to reduction in child labour as a

patterns to have an impact on the home environment and to benefit children[147]; and (iv) the conditionalities on education, health, and nutrition do not reach families before a time lag of some months, since operational aspects of the programme implementation impose some delays between the start of receiving benefits and the family being supervised (even for the more straightforward required responses such as school enrolment and attendance). These ideas suggest that the length of time of participation should be considered in any model proposed to capture *BF*'s effects on children's educational outcomes.

The **second** hypothesis is that the ***amount of cash*** transferred to families in per capita terms matters inasmuch as *BF* is concerned with short-term poverty alleviation and with reducing families' credit constraints that could prevent investments in children's education. This second hypothesis can be sustained by three basic economic ideas: (i) direct and indirect costs of education must be compensated if poor families are expected to value of keeping their children in school against their other immediate needs; (ii) opportunity costs, represented by the forgone earnings once children go to school, are relative to local labour market characteristics, being higher in more affluent regions, states, and towns; and (iii) the potential effect of cash transfers in changing household decisions and children's environments at home will depend on the share of household per capita income it represents. Therefore, *BF* effects may differ based on the relative importance cash transfers have in families' budgets, and the amount of cash paid to families should be considered in any model proposed to capture *BF*'s effects on children's educational outcomes.

In this chapter I look for evidence of the effects of *BF* on school outcomes by testing the hypotheses that *time of participation* and *per capita cash transfer value* are important factors in assessing those effects. I investigate school level outcomes in terms of tests scores in Portuguese Language and Mathematics, and pass-grade and dropout rates of fourth grade students in 2007. Can differences in those indicators across schools be associated with the level of *BF* participation, considering the mean time of participation in the programme and the cash value transferred to families? First, I briefly describe the school level cross-sectional national data gathered to carry out the analysis, which includes schools that took part in the national exam (*Prova Brasil*) in 2007 (section 7.2). I describe the main characteristics of these schools in terms of composition and resources, contrasting this with the distribution of *BF* beneficiaries (section 7.3). A discussion follows on how those schools compare with respect to

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consequence of the educational conditionalities. In addition, consumption can also be substituted for debt payments or small investments in productive assets (MDS, 2007).

147 Including their nutritional, health, and general well-being that can contribute to a better disposition to study and learn.

their *BF* participation and school outcomes, and how *BF* factors are correlated with such outcomes (sections 7.4 and 7.5). An interactive model is proposed to capture potential effects of *BF* factors on school outcomes using regression analysis (section 7.6). The resulting analysis is reported in the following subsections with a focus on a subsample of schools in the fifth quintile of the distribution of *BF* intake concluding the section. Finally, conclusions and limitations are discussed in sections 7.7 and 7.8.

7.2 Data and Sample

A cross-sectional national dataset at the school level was constructed for the analysis in this chapter. Three different sources of administrative records from the Federal Government in Brazil were used: (1) the *Prova Brasil* 2007 dataset, which contains socioeconomic variables from fourth grade pupils and their families as well as the results of test scores for Portuguese Language and Mathematics[148]; (2) the *Bolsa Família* dataset containing information on the main variables to be used in the analysis - number of beneficiaries by school in 2007[149], cash transfer, length of time of participation in *BF*, and household per capita income and; (3) the National School Census 2007 dataset, from which variables for several school characteristics are retrieved. All these datasets contain information at the individual level. However, it was not possible to link an individual's participation in *BF* to their test score results. The only possible link amongst those three sources of data was through the school code. Therefore, all variables describing student, household, and school characteristics were aggregated at the school level and represent mean values of the indicators at that level.

In 2008, the *National Institute for Pedagogical Studies and Research (INEP)* released the results of the *Prova Brasil* national exam, administered in 2007 by urban public schools with more than 20 pupils in the fourth grade. Official results were announced for a total of 37,262 schools (in 5,483 municipalities) by INEP. This number represents 34.2% of the public schools in Brazil with fourth grade pupils, and the number of pupils registered in the national exam in those schools represents 67% of fourth grade pupils in public schools (**Table 18**). The smaller number of public schools with published results derives from the scope of the national exam (only urban schools with more than 20 pupils enrolled in the assessed grade) as well as

148 Although the available dataset contains individual results on the national exam for each pupil, the variables used in the analysis as the school mean achievement in test scores in both subjects were not calculated by the author. Instead, the official result by school released by the National Institute for Pedagogical Studies and Research (INEP) in 2008 was used, which already corrected the mean results by school according to the sample size in each school and the necessary parameters to achieve a significant result at the school level.

149 One of the *BF* databases is the School Attendance Record, which keeps the attendance rate for each beneficiary student and also the school code that allows for identification of the student's location.

the set of rules required to validate the school results as statistically representative[150]. Also, participation in the national exam is by agreement with municipal and state education systems and although the vast majority takes part in it, some may not. Finally, for security[151] or climate[152] reasons, schools might not open on the day of the exam or students might not be able to reach the school site. The *Prova Brasil* dataset with 37,262 schools is therefore used as the primary dataset into which the other variables are merged.

Table 18: Number of 4th grade schools and pupils in 2007

School Description	Number of schools	Percentage over the total number of schools	Percentage over the total number of public schools	Number of 4th grade pupils (1)	Percentage over the total number of pupils	Percentage over the total number of pupils in public schools
Schools with results in *Prova Brasil* 2007	37,262	30.2%	34.2%	2,303,707	60.0%	66.6%
Urban Public Schools with 4th grade pupils	39,922	32.4%	36.6%	2,747,421	71.6%	79.4%
Public Schools with 4th grade pupils	109,103	88.4%	100%	3,459,028	90.2%	100%
Schools with 4th grade pupils (Public and Private)	123,382	100%		3,836,615	100%	

(1) The first figure is based on the *Prova Brasil* dataset and represents the number of pupils registered in the national exam in 2007 in schools for which results were officially released. The other figures are the enrolments according to the National School Census 2007.

The first dataset merged was the National School Census 2007. Technically, all 37,262 schools should be found in the National School Census dataset but, in fact, 326 schools did not match as fourth grade schools, corresponding to 0.9% of the schools with results for the national exam. That difference is due to schools' staff miscoding the grades of students when filling out the electronic form online[153] to respond the annual school census. Somehow in these schools all fourth grade students were coded as enrolled in a different grade[154]. In these cases

150 For instance, the number of pupils sitting for the exam must be at least 10, otherwise the school result is considered not statistically representative and is not released.

151 For instance, it is possible that in urban conflict zones in big cities schools are unable to open due to armed disputes between gangs.

152 In regions subject to floods or droughts, students can be prevented from attending school on the day of the exam, and/or the school may not be open.

153 In 2007 a new electronic system was introduced nationally to collect school data at the individual level, requiring schools to fill out an electronic form that replaced the old paper and pencil system.

154 For instance, the school CAIC Albert Sabin (school code 53009649) located in the Federal District (Brasília) coded all its 853 primary education pupils (first to fourth grade) as if they were all enrolled in the first grade.

it was not possible to identify the total number of fourth grade students for those schools and I had to exclude those schools from the dataset[155]. The sample was reduced to 36,936 schools.

Another inconsistency was found in the resulting dataset. In 1,038 schools the number of fourth grade pupils enrolled according to the national census was lower than the number of pupils sitting for the exam in 2007. Some schools had also misinformed the National Census by counting fewer pupils in the fourth grade than were, in fact, enrolled. In addition, after bringing in variables from the *BF* dataset, another set of schools was found to have more beneficiaries than enrolled pupils in the fourth grade (811). This process also found 242 schools out of the 36,936 for which no *BF* beneficiaries existed (these schools were set to zero proportion *BF* intake schools). As such, a diagnostic of measurement error in the number of fourth grade students was performed[156] and, in the end, all 1,738 schools with inconsistencies between the number of fourth grade pupils and the number of beneficiaries or the number of pupils sitting for the national exam were withdrawn from the sample, amounting to a dataset of 35,198 schools. I also excluded 18 federal schools from the sample to restrict the sample to schools managed by either municipalities or states. Finally, eight schools with missing values for schools outcomes were also excluded, leading to a final sample of 35,172 schools (**Table 19**).

Table 19: Number of schools in the sample

School Sample	Number of schools	Percentage over the total
Schools with results in *Prova Brasil* 2007.	37,262	100%
After exclusion of 326 schools without records for 4th grade pupils in the National Census.	36,936	99.1%
After exclusion of 1,738 with inconsistencies in the number of 4th grade pupils.	35,198	94.5%
After exclusion of federal schools (18) and schools with missing values for school outcomes (8).	35,172	94.4%

155 Not knowing the total number of fourth grade pupils in a school prevents the calculation of the proportion of *Bolsa Família* beneficiaries in the school (a key factor to be used in the analysis).

156 I test if differences in the proportion of schools by region and state are statistically significant if schools with inconsistencies are excluded from the sample. No statistically significant differences between the remaining consistent cases and the original cases are found across regions and states.

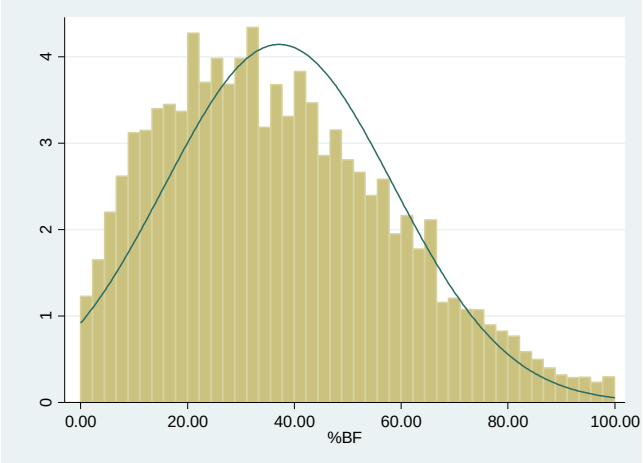
7.3 Where do *Bolsa Família* beneficiary children study?

In the reduced sample of 35,172 schools (as described in the previous section) a total of 2,621,427 students were enrolled in 2007, 878,650 of which were beneficiaries of *BF* (34%) and 2,211,048 (84%) of which took part in the national exam (*Prova Brasil 2007*).

Table A - 18 shows figures for schools by region as well as for fourth grade pupils, students sitting for the exam, and beneficiaries in those schools. The majority of schools and pupils are located in the Northeast and Southeast regions — the most populated regions in Brazil and also the poorest and wealthiest regions respectively in terms of GDP per capita[157]. The schools in the Northeast have the highest concentration of beneficiaries, followed by those in the Southeast. These regions hold some 71% of school children whose families participate in *BF*.

Most of the analysis developed in this section takes into account how schools differ when considered in terms of their main characteristics, including resources, composition, and performance. It also considers how those characteristics vary according to the intake of *BF* beneficiaries in each school. By taking the proportion of beneficiaries by school, I create a categorical variable to stratify schools per quintile of *BF* intake and, in what follows, I describe a range of student, household, and school characteristics according to the level of *BF* intake in schools.

Graph 18: Distribution of schools by percentage of *BF* intake



As presented in **Table A - 19,** a large proportion of schools (58%) has less than 40% of pupils taking part in the programme, and a small percentage (3.7%) has 80% or more pupils as beneficiaries[158]. **Graph 18** describes a positive skewed distribution in which 50% of schools has no more than 34.8% of *BF* intake and 90% has less than 66.7% (**Table 20**). The majority of beneficiaries (65%) attend schools with between 20% and 60% of *BF* intake and only 6.4% of beneficiaries are enrolled in schools with 80% or more *BF* intake. These figures suggest that there is a balance in terms of

157 According to the Brazilian Institute for Geography and Statistics (IBGE), in 2008 the Northeast GDP per capita was R$ 7,487.55 while for the Southeast it was R$ 21,182.68 (some 2.8 times higher).

158 There are 222 schools (0.63%) with no beneficiaries and 87 schools (0.25%) with 100% of pupils as beneficiaries of *BF*.

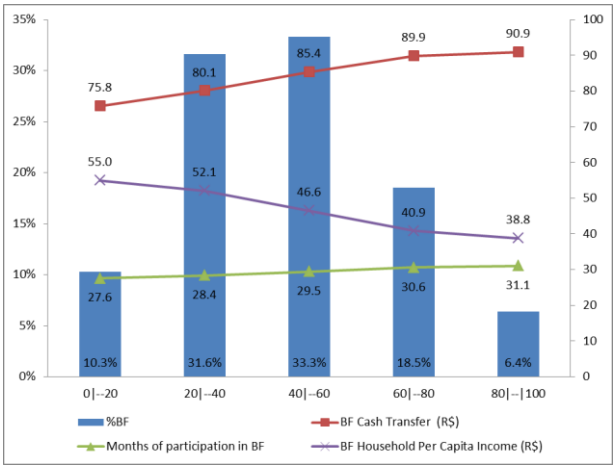
beneficiaries' distribution in schools according to the level of *BF* intake. There is no significant concentration of beneficiaries in schools with high *BF* intakes, which would, for instance, be characterised by a negative skewness reflecting a more economically segregated school composition for beneficiaries. Inasmuch as school composition might have a significant impact on students' performance, any indication of a negative skewed distribution would connote an additional disadvantage for beneficiary pupils. A less diversified school composition would create fewer opportunities for beneficiaries to integrate with more advantaged classmates, to benefit from eventually higher expectations amongst teachers and parents, and to participate in a more challenging academic environment[159].

Table 20: Percentage of *BF* intake in schools (quantiles)

	Percentiles						
	5	10	25	50	75	90	95
%*BF*	6.7	10.6	20.3	34.8	51.6	66.7	76.5

Graph 19 displays *Bolsa Família* characteristics by quintiles of beneficiaries in schools. The average cash value transferred to households increases as the proportion of *BF* rises in the school, as well as the mean time of participation in the programme (see also **Table A - 20**). Schools with high *BF* intakes are very likely to be located in poorer communities, a fact reflected in the household per capita income and in the higher mean cash paid to households (probably due to the basic flat-rate component of *BF* cash transfers[160]). If these schools are in the poorest areas, then they are also expected to have a longer mean time of participation, as observed, indicating that municipalities have prioritised the poorest families in the programme registration process. In fact, as described in **Table A - 21**, not only is the mean household per

Graph 19: *BF* indicators by groups of schools according to *BF* intake (%)

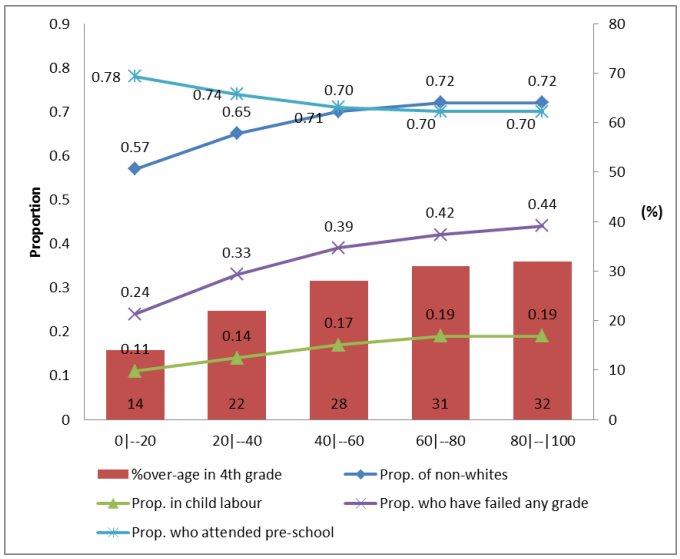


159 As described in the previous chapter, beneficiary pupils tend to perform less well than non-beneficiaries on average, which suggests that schools with higher concentrations of beneficiaries also have lower levels of performance and, as a consequence, are less academically challenged.

160 The *Bolsa Família* cash transfer has two components, one basic flat-rate paid to families living below the administrative cut-off per capita income that characterises extreme poverty (70 reais per capita) and an additional variable component paid to families living in poverty (below R$140 per capita) with children between 6 and 15 years old.

capita income roughly 30% lower in schools in the fifth quintile of *BF* intake compared to those in the first quintile, but also the mean household size increases from 4.7 to 5.0 members, indicating another reason why the mean cash transfer paid to households is higher in the fifth quintile. Poor families with more children receive more money due to the variable component of the *BF* benefit, which is based on the number of children in the family.

Graph 20: Students' characteristics by *BF* intake (%)

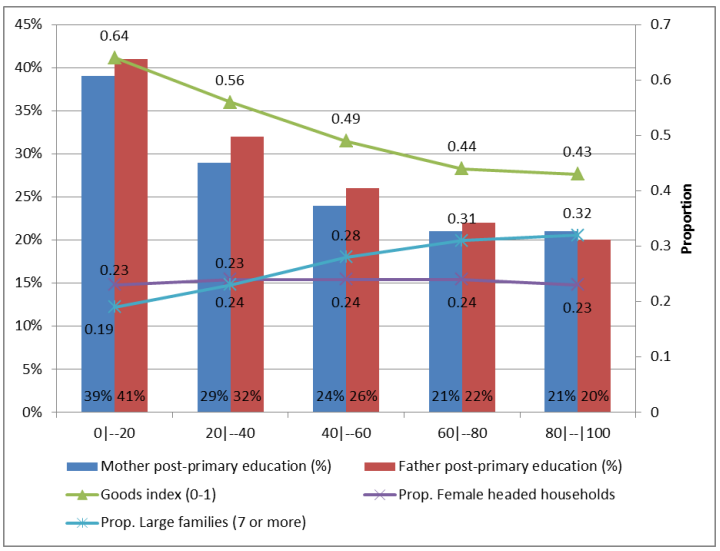


School composition regarding students' characteristics by quintiles of beneficiaries in schools is shown in **Graph 20**[161]. Participation by gender is balanced between quintiles with equivalent proportions of girls and boys in all levels of *BF* intake (see **Table A - 22**). However, non-whites appear in higher proportions in schools at the upper quintiles, with a difference of 15 p.p. Schools with more than 80% *BF* intake also have higher proportions of overage pupils (32%) compared to schools with less than 20% *BF* intake (14%), in part because they also outnumber the latter in terms of the proportion of pupils who have failed a grade (0.44 to 0.24). Pre-school attendance amongst schools with the lowest proportions of beneficiaries is higher (0.78) than in those schools with the highest proportions of beneficiaries (0.70). A pupil attending a school in the fifth quintile of *BF* intake is 1.7 times more likely to be found in child labour than those attending schools in the first quintile. In sum, the level of participation in *BF* is not the only factor in school composition that distinguishes schools from each other. Pupils' previous experience at school, including educational opportunities in early years, and incidence of child labour are main factors associated with school outcomes that also distinguish schools with different proportions of beneficiaries.

161 See also Table A - 22.

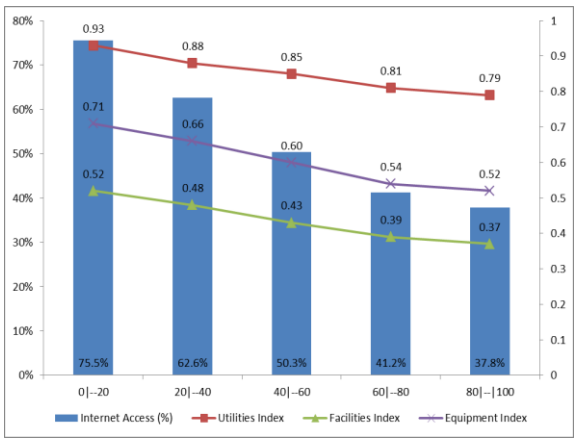
School composition in terms of household characteristics is shown in **Graph 21**[162]**.** The proportion of parents with post-primary education tends to diminish as the proportion of *BF* intake increases (0.39 to 0.21 for mothers and 0.41 to 0.20 for fathers). The same trend is found for the durable goods index[163] (varying from 0.64 to 0.43). On the other hand, the proportion of students with large families (seven or more people) increases from 0.19 in the first quintile to 0.32 in the fifth quintile. As to the female-headed household indicator, there is no significant variation across groups of schools based on level of *BF* intake; all groups have an average value of around 0.24 for the proportion of pupils living in female-headed households.

Graph 21: Households' characteristics by *BF* intake (%)

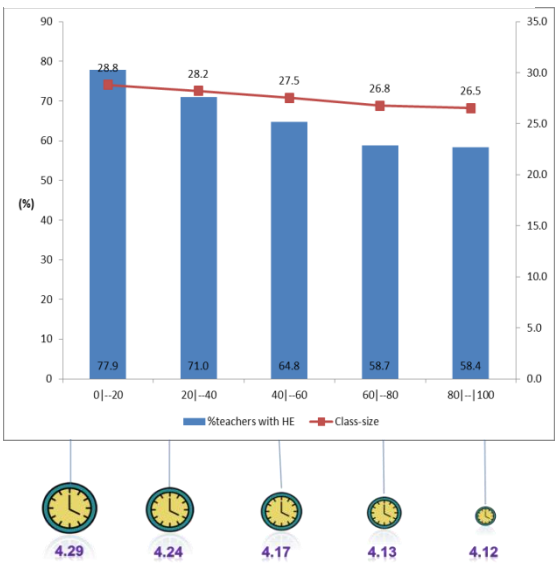


Schools with high *BF* intake rates are those assisting the children of the poorest families and are also the least resourced in the system (**Graph 22, Graph 23,** and **Graph 24**[164]).

Graph 22: School Infrastructure



Graph 23: Teachers with higher education (%), class size and school day (min.)

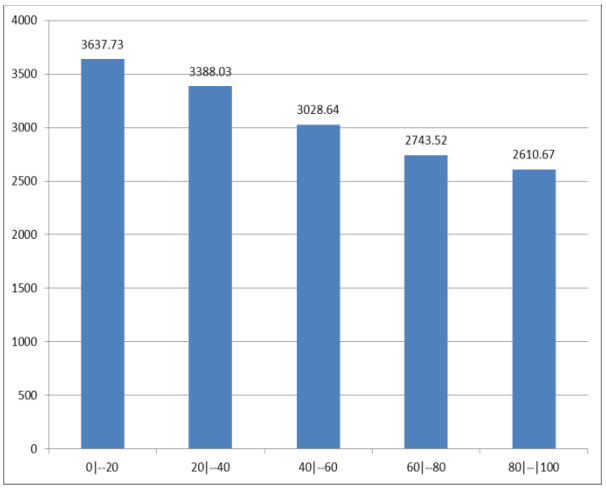


162 See also Table A - 23.

163 This index is calculated in the same way as described in chapter six.

164 See also Table A - 24 and Table A - 25.

Graph 24: Mean expenditure per pupil 2008/2009 (R$)



Apart from the class size indicator that actually tends to decrease as we move up the *BF* intake scale, all the other indicators decline. Comparing the two extremes of the distribution, one observes a school day on average 6% shorter in schools of the fifth quintile compared to those in the first quintile (Graph 23). This means that pupils in schools of the last quintile have, on average, school-hours reduced by 17 minutes each day, amounting to 14 school days over the academic year[165]. In addition, only 58% of teachers in the fifth quintile have higher education, compared to 78% in the first quintile. School infrastructure is also remarkably different between the two quintiles, as expressed in the indexes measuring school facilities, utilities, and equipment (Graph 22)[166]. For instance, on average, 75% of schools in the first quintile have access to Internet, while only 38% have access in the fifth quintile. Even the availability of school food differs by 2 p.p. against those in the fifth quintile, however small this difference may be considered (Table A - 25). The mean annual per pupil expenditure has been widely used as a single indicator to summarise school inputs (Graph 24). Here, the observed differences are even more astonishing, given the last 14 years of effort by the Federal Government to equalise education financing[167]. On average, per capita expenditure is 28% lower in the fifth quintile of schools compared to the first quintile – a per capita mean difference of R$ 1,027 (around US$ 570).

In the previous chapter I demonstrated that beneficiaries, on average, tend to study in less-resourced schools than non-beneficiaries. This section reveals that as the proportion of beneficiaries increases in the school, the quality in terms of the measurable resources made available decays significantly, characterising a regressive education system in which those who need more get less from the system in terms of school resources.

165 According to the Law 9394/96 education systems shall provide a minimum of 200 school days and 800 school-hours per year, meaning a typical school-day should have a minimum of 4 hours of pedagogical activities.

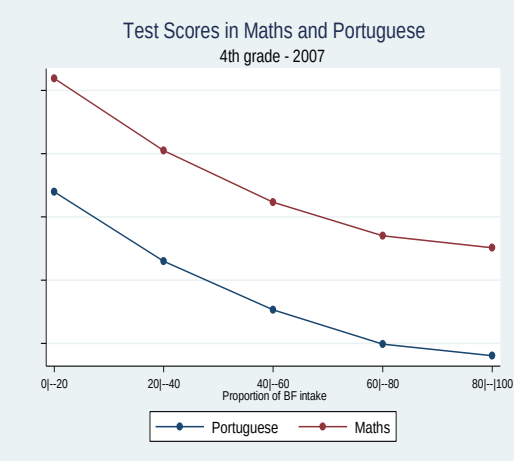
166 The indices vary between 0 and 1.

167 A major comprehensive review of the education funding system looking at equity issues took place in 1996, which created a national minimum per capita expenditure to be applied in every education system in the country offering fundamental education (first to eighth grades). In 2006, a new version of the funding policy was proposed to include pre-school and secondary school, also establishing a minimum per capita expenditure for those levels.

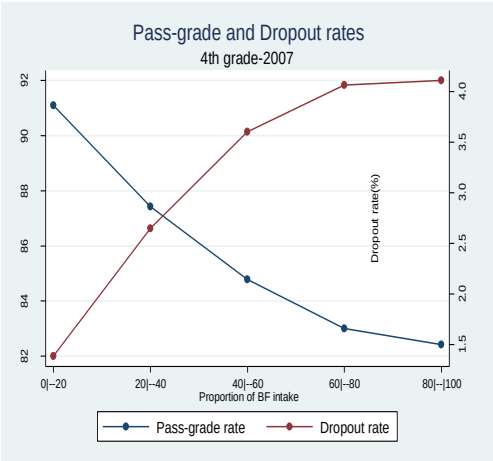
7.4 How do schools compare as to *Bolsa Família* participation and observed outcomes?

Given the scenario described in the previous section, it is expected that a relationship will be found between school outcomes and proportion of beneficiaries in schools. **Graph 25** and **Graph 26** (see also **Table A - 26)** confirm the expected trend for the mean test scores in Mathematics and Portuguese Language on the 2007 national exam and for school performance indicators (pass-grade and dropout rates) in the same year. Results in Mathematics are consistently higher than in Portuguese Language across groups (Graph 25). The gap between the first and fifth quintiles reaches some 27 points for Mathematics and 26 points in Portuguese Language; these figures roughly represent one cognitive level difference in the proficiency scale[168]. In Graph 26 a drop in pass-grade rates of around 9 p.p. between the extreme quintiles of schools is observed. The difference in dropout rates between the two groups reaches 2.7 p.p., that is, a rate three times higher for schools with more than 80% beneficiaries in relation to those with less than 20% *BF* intake.

Graph 25: School achievement in test scores by *BF* intake (%)



Graph 26: School performance indicators by *BF* intake (%)



School outcomes would not be significantly different, given the previously described characteristics of schools with high *BF* intakes. Social disadvantage and school inequality add to the reproduction of a vicious cycle in which poor children access poor education and end up failing in the school system and in accessing better paid jobs in the future. However, as mentioned earlier, *BF* beneficiaries are not concentrated in schools situated in the last two quintiles of *BF* intake. In fact, only 25% of beneficiaries attend schools in which more than 60% of their students take part in the programme, and these schools do not differ significantly in

168 In Chapter six I discuss three different ways of interpreting differences in test score achievements on the national exam.

terms of composition, resources, and results[169], and are the least resourced in the system. There is a distribution of beneficiaries across quintiles of schools by *BF* intake that could attenuate the negative effects of school composition and school resources for a large proportion of children participating in the programme. However, one must not forget that one-quarter of beneficiaries study under the worst conditions available in the system.

7.5 *Bolsa Família* factors and school outcomes

Participation of children in *BF* differs across schools, not only in terms of the share of students taking part in the programme, but also in relation to the length of time of participation, the per capita cash transferred to families, and their level of poverty measured by household per capita income. Therefore, I consider the correlation coefficients between these *BF* factors and school outcomes as a first step in the analysis of the possible influence of *BF* on school results.

Looking at the correlation coefficients between *BF* intake and school outcomes in **Table 21**, a substantial negative correlation is observed for mean test scores and a moderate correlation for pass-grade and dropout rates. A high proportion of *BF* intake is associated with low performance at the school level, as measured by the mean test scores and mean pass-grade and dropout rates. The mean household per capita income of beneficiary families[170] is positively correlated with school outcomes[171]. This result shows that differences in the level of poverty amongst beneficiary families might influence school results.

169 See Graph 20 to Graph 26.

170 The indicator household per capita income in the data is measured only for *BF* households, therefore not reflecting the average per capita income of all families whose children attend school, only the beneficiary families.

171 In the case of dropout a negative correlation coefficient implies a reduction in dropout rates with increases in household per capita income representing an improvement in performance.

Table 21: Pearson's correlation coefficient between BF factors and school outcomes

	***Bolsa Família* variables at the school level**				**School outcomes**			
	% BF intake	*BF* household per capita income	Time of participation	Per capita Cash Transfer	Pass-grade rate	Dropout rate	Mathematics	Portuguese
% BF intake	1.00							
BF household per capita income	-0.33*	1.00						
Time of participation	0.16*	-0.23*	1.00					
Per capita Cash Transfer	0.19*	-0.71*	0.05	1.00				
Pass-grade rate	-0.24*	0.30*	-0.10*	-0.17*	1.00			
Dropout rate	0.18*	-0.26*	0.05*	0.16*	-0.58*	1.00		
Mathematics	-0.45*	0.45*	-0.14*	-0.29*	0.36*	-0.30*	1.00	
Portuguese	-0.47*	0.45*	-0.15*	-0.28*	0.38*	-0.32*	0.93*	1.00

Note: Pairwise calculation with correlation coefficients statistically significant at 0.01% level indicated by *.

Table 21 also allows us to interpret another two key correlations between *BF* factors and school outcomes. **Time of participation** and **per capita cash transfer** are negatively correlated with all four indicators of school outcomes, that is, as the mean number of months of participation in *BF* increases, the observed outcomes, on average, worsen. The same results are seen for increasing values of per capita cash. The negative correlation with school outcomes holds, even when analysed at different quintiles of *BF* intake as **Table A - 27** describes for the case of test scores in Portuguese Language and time of participation in *BF*.

The last result seems to deny, from the outset, the hypotheses I test in this chapter; that time of participation in *BF* and the cash value paid to families will be found as key variables explaining potential positive effects of *BF* on children's school outcomes. The problem, however, is that face value correlation coefficients between two variables taken in isolation can be misleading, since it does not allow for other possible interactions from factors that can simultaneously affect both variables, producing a correlation coefficient that is not only due to the examined relationship, but carries external influences as well. For instance, because household per capita income is negatively correlated with time of participation[172] (-0.23), it might be that this correlation would bias the observed (negative) correlation between time of participation and school outcomes. As per capita income decreases, school outcomes also decrease, therefore, a longer time of participation masks low income as the driving force behind poor school outcomes. The same happens with per capita cash, with a much stronger

172 An expected result considering that the higher the average per capita income for beneficiary families in a given school, the less likely those families were to have been prioritised in the registration process to take part in the programme, resulting in a shorter time of participation on average.

coefficient (-0.71), showing that the poorer the family the higher the per capita cash paid to them[173]. A first useful way to control for multiple influences on the correlation coefficient between two variables is to use partial and semipartial correlation coefficients.

As an example**, Table 22** reports the partial and semipartial correlation coefficients between the test scores in Portuguese Language and each of the *BF* factors examined. The partial correlation coefficient expresses the correlation of each of the *BF* factors with test scores, holding the others constant. The semipartial correlation discounts the mutual influences of each *BF* factor on each other, while assessing their correlation with test scores. In the case of time of participation in *BF*, the partial correlation coefficient is still negative but is much closer to zero and below the conventional significance level. As to the coefficient for per capita cash transfer, it becomes significantly positive. This means that the simultaneous correlation of per capita income with test scores, time of participation in *BF*, and per capita cash transfer were blurring the correlations with test scores of these two *BF* factors. It is evident that even by employing a simple and rather crude approach, the initial negative correlation of time of participation and cash with school outcomes dramatically changes. A more sophisticated way of dealing with such confounding effects is to make use of multiple regression analysis.

Table 22: Partial and semi-partial correlation coefficients between Portuguese Test Scores and *BF* factors

Variable	Partial Corr.	Semipartial Corr.	Significance Value
% *BF* intake	-0.37	-0.33	0.00
BF household per capita income	0.29	0.25	0.00
Time of participation	-0.01	-0.01	0.11
Per capita cash transfer	0.05	0.04	0.00

In order to explore further the relevance of time of participation and cash value on school outcomes, in the next section I estimate several multiple regression models using cross-sectional data, in which I explore potential effects and interactions between time of participation and per capita cash on school outcomes and discuss potential causes of bias influencing that relationship. Controls will be created in an attempt to purge those sources of bias.

173 In fact, families below the extreme poverty line receive a fixed R$ 50 in additional to the variant component due to the number of children in the family, potentially making the per capita cash higher for the poorest families.

7.6 The model and analysis

Using the cross-sectional data at the school level described earlier, I estimate a model using multiple regression analysis in which I examine the marginal effect[174] of the proportion of beneficiaries in *BF* – moderated by *time of participation* and *per capita cash value* – on school outcomes. The hypothesis to be tested is that although the proportion of beneficiary students in schools is expected to be negatively correlated with school outcomes, due to what has been called the "programme placement effect", this trend would have diminishing marginal effects across schools depending on the average time of participation in the programme and also on the mean cash value paid to families. If confirmed, these trends will signal a positive contribution of *BF* on school outcomes, going beyond the broadly explored and disseminated positive effects on enrolment and attendance.

7.6.1 From the hypotheses to the econometric model

As the proportion of beneficiaries is thought to affect school outcomes, a simple econometric model to describe and estimate that effect can be expressed as follows:

$$Y_i = \alpha_0 + \alpha_1 PropBF_i + \epsilon_i \qquad \text{(I)}$$

where $\boldsymbol{Y_i}$ is the school outcome of interest for school **i**[175], $\boldsymbol{PropBF_i}$ is the proportion of beneficiaries in a school **i**, and $\boldsymbol{\varepsilon_i}$ is the error term capturing the variation in the school outcome not accounted for the variable *PropBF.* The coefficient $\boldsymbol{\alpha_0}$ in this model is held constant across schools and represents the average or expected value of the school outcome Y when *PropBF* is zero. The coefficient $\boldsymbol{\alpha_1}$ is also held constant and gives the marginal variation on Y due to a unit increase in *PropBF*, that is, the "effect" of the proportion of beneficiaries on school outcomes. In such a model, $\boldsymbol{\alpha_0}$ and $\boldsymbol{\alpha_1}$ are the parameters to be estimated.

In order to test the hypotheses of *time* and *cash* as two key factors moderating the effect of *PropBF* on school outcomes ($\boldsymbol{\alpha_1}$), these variables must be introduced in model (I). This is done in two steps.

First, the parameter $\boldsymbol{\alpha_1}$ is allowed to vary across schools by making it dependent on the mean time of participation in each school. I make the effect $\boldsymbol{\alpha_1}$ a linear function of the variable

174 "Marginal effects" is used here since it is normally referenced in the econometrics literature. The term "effect", however, does not necessarily mean causal effect. The "marginal effect" or simply "effect" of a variable X on Y is, in fact, the partial derivative of Y in respect to X; that is, the marginal variation of Y associated with an infinitesimal change in X.

175 The subscript "i" indicating the unit of analysis (school) will be omitted to simplify the notation.

Time, which can be thought of here as the *moderator* or *intervening* variable, responsible for changing the effect of *PropBF* on *Y*, as described in equation (II).

$$\alpha_1 = \delta_1 + \delta_2 Time + \epsilon_1 \qquad \text{(II)}$$

The coefficient $\boldsymbol{\delta_2}$ gauges how the effect of *PropBF* on school outcomes changes by a unit increase in the mean time of participation in *BF*. The coefficient $\boldsymbol{\delta_1}$ gives the mean effect of *PropBF* when time of participation is zero[176]. The error term ε_1 is necessary to allow a non-deterministic dependency of $\boldsymbol{\alpha_1}$ on *Time*.

Likewise the coefficient $\boldsymbol{\alpha_0}$ can also be expressed as a function of *Time* in order to allow the intercept in expression (I) to vary across schools to reflect differences on the mean school outcome derived from mean time of participation when the proportion of beneficiaries is zero[177].

$$\alpha_0 = \omega_1 + \omega_2 Time + \epsilon_2 \qquad \text{(III)}$$

Now substituting (II) and (III) into (I):

$$Y = (\omega_1 + \omega_2 Time + \epsilon_2) + (\delta_1 + \delta_2 Time + \epsilon_1) PropBF + \epsilon =$$

$$= \omega_1 + \omega_2 Time + \delta_1 PropBF + \delta_2 PropBF \times Time + (\epsilon + \epsilon_2 + \epsilon_1 \times PropBF) \qquad \text{(IV)}$$

Expression (IV) shows that making the effect of *PropBF* on Y conditional on *Time* generates a model with an interactive term (*PropBF x Time*) and an error term ($\epsilon + \epsilon_2 + \epsilon_1 \times PropBF$) that will not have a constant variance across schools, since it depends on the variable *PropBF*. As argued by Kam and Franzese (Kam and Franzese, 2007, pp. 128-129) the heteroskedasticity (non-constant variance) can be easily addressed in the regression by using the White's procedure to generate a consistent variance-covariance matrix and, therefore, to correct the coefficient-estimates' standard errors in the estimated model[178].

The second step is to integrate the variable *Cash* in the model. The hypothesis here is that time of participation changes the effect of *PropBF* on school outcomes differently from school to school depending on the level of cash paid to families in each school. This

176 Although unlikely in the data, it is technically possible that schools with beneficiaries have zero time of participation, since this variable is measured as months of participation and beneficiaries with less than 30 days in the programme would allow such a case to exist (there is only one school in the sample that fits this case).

177 It might be argued that allowing the intercept to vary with Time would make no practical sense, since it represents the mean value for school outcome (e.g. test score) when the proportion of beneficiaries is zero. I will argue for the maintenance of a variant intercept (α0) later in this chapter.

178 This is done in STATA using the option "robust" for the regression command.

dependency can be expressed by making the coefficients $\boldsymbol{\delta_1}$, $\boldsymbol{\delta_2}$,$\boldsymbol{\omega_1}$ and $\boldsymbol{\omega_2}$ in (II) and (III) functions of *Cash*:

$$\alpha_1 = \delta_1 + \delta_2 Time + \epsilon_1 \qquad \alpha_0 = \omega_1 + \omega_2 Time + \epsilon_2$$

$$\begin{cases} \delta_1 = \beta_1 + \beta_2 Cash + \epsilon_3 & \text{(V)} \\ \delta_2 = \beta_3 + \beta_4 Cash + \epsilon_4 & \text{(VI)} \end{cases} \qquad \begin{cases} \omega_1 = \beta_0 + \beta_5 Cash + \epsilon_5 & \text{(VII)} \\ \omega_2 = \beta_6 + \beta_7 Cash + \epsilon_6 & \text{(VIII)} \end{cases}$$

Now substituting (V) to (VIII) into (IV):

$$Y = (\beta_0 + \beta_5 Cash + \epsilon_5) + (\beta_6 + \beta_7 Cash + \epsilon_6).Time + (\beta_1 + \beta_2 Cash + \epsilon_3).PropBF + (\beta_3 + \beta_4 Cash + \epsilon_4).PropBF.Time + (\epsilon + \epsilon_2 + \epsilon_1.PropBF)$$

$$= [\beta_0 + \beta_5 Cash + \beta_6 Time + \beta_7 Time.Cash] + [\beta_1 + \beta_2.Cash + \beta_3.Time + \beta_4.Time.Cash]PropBF + u \qquad \text{(IX)}$$

where, $u = \epsilon + \epsilon_2 + \epsilon_5 + \epsilon_1.PropBF + \epsilon_3.PropBF + \epsilon_4 PropBF.Time + \epsilon_6.Time$ (X)

The first term in brackets in expression (IX) is the conditional intercept of *Y* on *PropBF* ($\boldsymbol{\alpha_0}$) and the second term is the conditional slope of Y on Prop*BF* ($\boldsymbol{\alpha_1}$). The error term ***u*** also varies across schools and will require correction for heteroskedasticity, as mentioned earlier.

Therefore, the school-outcome model to be tested assumes the following functional form:

Model 2

$$\mathbf{Y} = \boldsymbol{\beta_0} + \boldsymbol{\beta_1 PropBF} + \boldsymbol{\beta_2.PropBF.Cash} + \boldsymbol{\beta_3.PropBF.Time} + \boldsymbol{\beta_4.PropBF.Time.Cash} + \boldsymbol{\beta_5 Cash} + \boldsymbol{\beta_6 Time} + \boldsymbol{\beta_7 Time.Cash} + \sum_k \boldsymbol{\alpha_k X_k} + \sum_j \boldsymbol{\gamma_j D_j} + \boldsymbol{u}$$

In **Model 2**, **Y** represents school outcomes measured as the mean values of test scores in Mathematics and Portuguese Language, pass-grade, and dropout rates in school **i**. The variable ***PropBF*** represents the proportion of *Bolsa Família* students in school **i**. The variables ***Time*** and ***Cash*** measure respectively the mean number of months of participation in *Bolsa Família* and the mean value of per capita cash paid to beneficiary families whose children attend school **i**. The variables $\mathbf{X_K}$ represent the observable school factors such as school composition and resources that are potentially correlated either to the observed level of participation in *Bolsa Família* and/or to school outcomes. The variable $\mathbf{D_j}$ is a dummy for

region. Finally, **u** represents the disturbance term capturing the variance in school outcomes not explained by the variables included in the model.

Model 2 brings the three main factors associated with the *BF* programme at the school level as explanatory variables of school outcomes: the share of pupils taking part in the programme, the mean time of participation, and the mean cash transfer paid to families. It is an interactive model since the key *BF* factors being analysed (*PropBF*, *Time,* and *Cash*) enter into the model as product terms, meaning that these variables can be thought of as moderators of each other. For instance, *Time* and *Cash* can be regarded as moderators of the variable *PropBF* or, conversely, being moderated by *PropBF* and by one another, since the two factors interact with each other. It is a symmetric model in which one variable (e.g. *Time*) modifies, and in fact conditions, the effects of the other variables, and vice-versa.

In interactive models the estimated parameters cannot be directly interpreted as effects (Kam and Franzese, 2007, pp.19-20). This means that coefficients $\boldsymbol{\beta_1}$ to $\boldsymbol{\beta_7}$ in Model 2, taken in isolation, cannot tell us the estimated effects on the outcome **Y**. Each variable involved in interaction terms has multiple effects, depending on the levels of the other variables with which it interacts. For this reason, the estimated effects of each of the variables of interest (*PropBF*, *Time,* and *Cash*) must be assessed using the conditional effects expressions as described below.

Equation 1

$$\frac{\partial Y}{\partial PropBF} = \boldsymbol{\beta_1} + \boldsymbol{\beta_2 Cash} + \boldsymbol{\beta_3 Time} + \boldsymbol{\beta_4 Time.Cash}$$

Equation 2

$$\frac{\partial Y}{\partial Cash} = \beta_5 + \beta_7 Time + \beta_2 PropBF + \beta_4 PropBF.Time$$

Equation 3

$$\frac{\partial Y}{\partial Time} = \beta_6 + \beta_7 Cash + \beta_3 PropBF + \beta_4 PropBF.Cash$$

These are simply the partial derivatives expressing the marginal effects of *PropBF*, *Time,* and *Cash* on school outcomes. Taking, for instance, Equation 1 above, the effect of *PropBF* on school outcomes **Y** is not given by the coefficient $\boldsymbol{\beta_1}$ as the mean estimated effect for all schools. The role played by the other variables (*Time* and *Cash*) in the proposed model is to moderate the effect the proportion of beneficiaries (*PropBF*) has on school outcomes, making it change according to those two characteristics in each particular school. Equation 1 tells us that although the "programme placement effect" potentially has a negative effect on school outcomes (it brings more disadvantaged children to school), the programme

mechanisms in operation through the cash transfer (reduction of immediate poverty) and the conditionalities on health, nutrition, and education might potentially be positively contributing to the improvement of school outcomes over time. This will empirically be supported if the combination of the estimated $\boldsymbol{\beta_1}$, $\boldsymbol{\beta_2}$, $\boldsymbol{\beta_3}$, and $\boldsymbol{\beta_4}$ yields a positive and statistically significant marginal effect of *PropBF* at some values of *Time* and *Cash*, or if a negative marginal effect of *PropBF* can be proven to diminish across different values of *Cash* and *Time*.

Because interactive models with all the constitutive terms are mathematically symmetrical, it is also possible to analyse the marginal effects of *Time* and *Cash* (as in Equation 2 and Equation 3) as being moderated by *PropBF* in each school and by one another. Both directions of analysis are mathematically possible, although it seems more intuitive to think of *PropBF* as the focus variable, whose influence on test scores is modified according to *Time* and *Cash*. In fact, the reasoning I developed to construct Model 2 implicitly assumes this direction of events: children in school first need to be registered in the programme (*PropBF*≠0); families then start receiving a monthly per capita cash transfer (*Cash*≠0); and they receive the transfer for a certain period of time (*Time*≠0)[179]. Besides, Equations 2 and 3 suggest that marginal effects of *Time* and *Cash* might be estimated (in the case β_5 or β_6 were statistically different from zero), even if the proportion of beneficiaries was zero. This sounds counterintuitive, since no effect of *Cash* or *Time* should be expected when there are no beneficiaries in the school.

The above-mentioned difficulty with Equations 2 and 3 derives from the first term in brackets of expression (IX). That term results from making the intercept in expression (I) conditional on *Time* and *Cash*. It is worth noting that Equation 1 does not change if that term is dropped from the model. Dropping that term from expression (IX) only changes Equations 2 and 3, which lose the first two terms, $(\beta_5 + \beta_7 Time)$ and $(\beta_6 + \beta_7 Cash)$ respectively, the parts predicting an implausible result. This might suggest the exclusion of that term from expression (IX) to eliminate the implausibility and eventually work with Equations 2 and 3 as well. However, allowing the intercept $\boldsymbol{\alpha_0}$ in expression (I) to vary as function of *Time* and *Cash* is statistically necessary. As I argue later in this chapter, it ensures that all constitutive terms[180] are included in the model and thus circumvents bias in the estimated coefficients entering Equation 1. Therefore, I consider *PropBF* as the variable whose effect on school outcomes is moderated by the other two varying characteristics of *BF* in each school (*Cash* and *Time*), making Equation 1 central to the analysis.

179 I followed a path in developing the model that brought Time and then Cash into the model, but the order in which these two variables enter into the model is irrelevant once Prop*BF* is fixed as the focus variable.

180 By "constitutive terms" I mean (following Kam and Franzese (2007)) all variables composing the interaction terms in the model.

7.6.2 Selecting controls for families' and schools' characteristics

A set of covariates is also included in Model 2 ($\sum_k \alpha_k X_{ki}$) representing variables to be controlled for in estimating the model so to reduce the potential influence of omitted variables. Omitted variables are variables not included in the model that can seriously bias the estimation of any of the coefficients β_k due to simultaneous correlations with the dependent variable and one or more of the independent variables already in the model. In Section 7.3 I described many school factors related to school composition and resources that might be simultaneously correlated with school outcomes and the level of participation in *BF*[181], particularly with the variables being investigated (*Time* and *Cash*). If a particular **observable** school characteristic affecting school outcomes such as geographic location is correlated with *BF* factors and is not included in the model, the risk of bias in estimating β_1, β_2, β_3, and β_4 (parameters of interest to estimate the marginal effect of *PropBF*) increases considerably. For example, the Northeast region is the least developed region in Brazil and its school outcomes, on average, tend to be lower than for schools in the other regions. It is very likely that municipalities in the Northeast were amongst the first to be mobilised and to have families included in the programme due to its poverty levels. Consequently, the tendency is that schools in that region have larger proportions of beneficiaries and also have longer mean times of participation in the programme. These schools also tend to score lower in school outcomes[182]. Therefore, the coefficients associated with marginal effects of *BF* participation on school outcomes would be biased if regions were omitted from the model. Geographic location is a relevant variable to be included in the model, as it potentially influences school outcomes and also is correlated with the variables of interest[183].

It might also be the case that **unobservable** variables influencing the level of participation in the programme in each school would also be correlated with school outcomes. For instance, the level of information amongst families in relation to their rights and the possibility of registering for the programme could be driving the different proportion of *BF* families in each school. Those differences in information probably reflect different levels of

181 In section 7.5 I discussed how correlation coefficients between *BF* factors and school outcomes could be affected by the influence of other variables in a way that could even change the sign of the correlation.

182 For example, schools in the Northeast have an average of 52% of students participating in *BF*, mean time of participation of 31 months, and schools received 158 points in Portuguese test scores. The Southeast has 30% participation in *BF*, 28 months' mean time of participation, and 179 points in Portuguese test scores.

183 I also consider estimating separate models for each region, which allows the parameters of interest related to *BF* participation and the coefficients on all the other variables included in the model to differ across regions.

knowledge and skills amongst parents in *BF* targeted families, that is, human capital gaps[184]. Different levels of human capital amongst parents are also correlated with differences in the quality of support children can have at home and, thus, with children's school outcomes. As a result, schools with high proportions of *BF* students may reflect those families with high levels of human capital amongst the poor. The estimated effects of participation in *BF* on school outcomes in this case would be biased upwards. A proxy variable to the level of information should be included in the model to control for those differences in human capital amongst parents. The proportion of parents holding post-primary education degrees at the school level is available in the dataset and can be used as a proxy for human capital in this case.

The same reasoning can be extended to other variables that potentially influence school outcomes and can also be correlated with participation in *BF* and/or with *Time* and *Cash*. How can these variables be identified? Although an exhaustive identification of such variables is not possible, I select those from the socioeconomic questionnaire of *Prova Brasil* and from the National School Census that fall into that criterion to try to reduce the potential bias in the estimates. First I identify the socioeconomic factors associated with participation in *BF* and examine the partial correlation coefficients of these factors with school outcomes. Then I look at a set of school characteristics and their partial correlation with *BF* participation and school outcomes. The relevant controls to be included in Model 2 are those variables that are correlated with participation in the programme and that are also correlated with any of the school outcomes.

To identify the socioeconomic variables associated with participation in *BF*, I estimate a *logit model* to test a set of socioeconomic characteristics of students and their families as predictors of the probability of participation in *BF*[185]. From the 25 socioeconomic variables tested, 14 were found to be statistically significant as predictors of participation in *BF*, from which 13 are available in the 2007 dataset to be aggregated at the school level. **Table A - 28** reports the partial correlation coefficients of the socioeconomic and school characteristics with school outcomes[186] and *BF* factors, and indicates those to be introduced as controls in Model 2.

184 Gary Becker refers to search for information about incomes as one of the forms of human capital amongst others, such as schooling, on-the-job-training, medical care, and migration (see Becker, 1993,p.11).

185 The logit model is estimated using the dataset of Prova Brasil 2005 from the previous chapter, since only in 2005 was participation in *Bolsa Família* identified in the socioeconomic questionnaire.

186 Only 11 socioeconomic variables are reported in the table. Two variables (piped water at home and books at home) were not included, as they are related to material conditions at home already captured by household per capita income and the durable goods index.

All socioeconomic variables selected as predictors of participation in *BF* and school factors correlated with *BF* have statistically significant partial correlation coefficients with at least one of the school outcomes investigated (see Table A - 28). Initially, I considered all those variables as controls in Model 2. However, there is a caveat: by including as controls those variables correlated both with school outcomes and participation in *BF*, and which are also expected to change as result of the policy being implemented, we risk neutralising the expected effect of *BF* in the regression. This engenders what is called "over controlling" (Wooldridge, 2009, p.203). In Table A - 28, three socioeconomic factors fall into this category: the proportion of child labour, the overage rate, and the durable goods index. For instance, *BF* cash transfers and conditionality on minimum school attendance rate create conditions to reduce the propensity of child labour amongst beneficiary families. Since child labour negatively affects school outcomes, one way in which *BF* could be effective in improving students' school achievements would be to reduce the propensity of child labour amongst poor families. By including child labour as a control in Model 2, and considering the *ceteris paribus* framework for interpreting regression results, we would attempt to measure *BF* effects on school outcomes holding the proportion of child labour constant, that is, freezing one of the variables that could be the one changing as a result of the programme and causing some improvements to school outcomes. The same thinking can be extended to the overage rate and the durable goods index variables. Finally, a control for the proportion of pupils sitting for the exam in each school is also included in the model. This control is important, because it is possible that schools with high *BF* intakes have lower rates of participation in the exam[187]. In the last column of Table A - 28 I indicate the variables included as controls in estimating Model 2 and the full description of the variables entering Model 2 is presented in **Table 23.**

187 This can happen either if beneficiary students do to not turn up on the exam day, or if schools tend to be selective by excluding some of the beneficiaries from taking part in the exam. In fact, the correlation coefficient is -0.1 (ρ=0.000) showing that there is a small but significantly negative correlation between *BF* intake and the proportion of students sitting for the exam.

Table 23: Description of variables used to estimate Model 2

	VARIABLES	DESCRIPTION
Bola Família Factors	Prop*BF*	Proportion of 4th grade *BF* beneficiaries in the school.
	Cash	Mean per capita cash paid to *BF* beneficiary families of 4th grade pupils in the school.
	Time	Mean number of months of participation in *BF* of 4th grade pupils' families in the school.
School Composition (students' and households' characteristics)	*BF* households' per capita income	Mean household per capita income of *BF* beneficiary families of 4th grade pupils in the school.
	Prop. of Boys	Proportion of boys amongst the 4th grade pupils in the school who sat for the national exam in 2007.
	Prop. of Non-whites	Proportion of non-whites amongst the 4th grade pupils in the school who sat for the national exam in 2007.
	Prop. Pre-school	Proportion of pupils who attended pre-school amongst the 4th grade pupils who sat for the national exam in 2007.
	Prop. Failed	Proportion of pupils who have failed any grade in past years amongst the 4th grade pupils who sat for the national exam in 2007.
	Prop. in Large Families (7 or more)	Proportion of pupils who live in families with 7 or more members amongst the 4th grade pupils who sat for the national exam in 2007.
	Prop. Fem. Headed Household	Proportion of pupils who live in female headed households amongst the 4th grade pupils who sat for the national exam in 2007.
	Prop Mothers with post-primary education	Proportion of pupils whose mothers have completed secondary school or College amongst the 4th grade pupils who sat for the national exam in 2007.
	Prop Fathers with post-primary education	Proportion of pupils whose mothers have completed secondary school or College amongst the 4th grade pupils who sat for the national exam in 2007.
	Prop. Students sitting for the exam	Proportion of 4th grade pupils in the school who sat for the national exam in 2007.
School Resources	Class size	Number of enrolments in classes with 4th grade pupils divided by the number of classes with 4th grade pupils.
	School day (hrs./day)	Mean number of hours in a school day for 4th grade pupils.
	Prop. Teachers with HE	Proportion of teachers working in 4th grade classes who have higher education.
	Utilities index	Index number ranging from 0 to 1 measuring whether the school has access to: piped water, power supply, sewage, and garbage collection.
	Facilities index	Index number ranging from 0 to 1 measuring the availability of school facilities amongst 13 items measured by the National School Census.

VARIABLES		DESCRIPTION
	Equipment index	Index number ranging from 0 to 1 measuring the availability of school facilities amongst 9 items measured by the National School Census.
	Expenditure per pupil 2008/2009	Average expenditure per pupil in fundamental education calculated in the school system to which the school is administratively subordinated using available data for 2008 and 2009.
Regions	1.North	Dummy variable equal to 1 if the school is located in the North region.
	2.Northeast	Dummy variable equal to 1 if the school is located in the Northeast region.
	3.Central-West	Dummy variable equal to 1 if the school is located in the Central-West region.
	5.South	Dummy variable equal to 1 if the school is located in the South region.
Test Scores	A2007Port	School mean test score result in 2007 for Portuguese Language.
	A2007Math	School mean test score result in 2007 for Mathematics.

7.6.3 Results (I): Testing the model

Table 24 reports the results of the regression coefficients and robust standard errors of test scores in Portuguese Language on *BF* factors at the school level once the controls described earlier are applied. Column (1) reproduces the first simple model described in expression (I), added with the control variables. Because it has no interaction term, a straightforward interpretation of the coefficient β_1 is possible. As expected, β_1 is significant and negative reflecting the "programme placement effect" resulting in the association between schools' high proportions of beneficiaries with low achievements on test scores. The model predicts a reduction of one-tenth of a point on the mean test score in Portuguese Language by increasing one percentage point in the proportion of beneficiaries in schools. Column (2) includes in the model the variables of interest, *Time* and *Cash*, as interactive terms with the proportion of beneficiaries (*PropBF*). These are the terms in the model whose coefficients enter the expression for the marginal effect of *PropBF*. As mentioned earlier, this model assumes that the intercept α_0 in expression (I) does not depend on *Time* and *Cash*, and implies the exclusion of the first two terms in Equation 2 and Equation 3, although not affecting Equation 1. Column (3) includes all the constitutive terms of the interactions with *PropBF*

entering in Model 2 to test whether the exclusion of these terms from the model affects the coefficients of interest β_1 to β_4.

Table 24: Regression of test scores (Portuguese Language) on *Bolsa Família* factors controlling for schools' socioeconomic composition and resources.

	(1)	(2)	(3)
VARIABLES	Portuguese	Portuguese	Portuguese
Prop*BF* (β_1)	-0.109***	-0.216***	-0.385***
	(0.004)	(0.062)	(0.089)
Prop*BF* x Cash (β_2)		0.006*	0.012**
		(0.003)	(0.005)
Prop*BF* *x Time* (β_3)		0.005**	0.008***
		(0.002)	(0.003)
Prop*BF* x Time x Cash (β_4)		-0.000**	-0.000*
		(0.000)	(0.000)
Cash (β_5)			-0.172
			(0.137)
***Time* (β_6)**			-0.073
			(0.080)
Time x Cash (β_7)			-0.003
			(0.005)
Controls	Yes	Yes	Yes
Constant **(β_0)**	161.109***	161.193***	169.115***
	(1.320)	(1.337)	(2.836)
Observations	33,857	33,805	33,805
Adjusted R-squared	0.523	0.522	0.523

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The coefficients in column (3) show that neither *Time* nor *Cash* (or *Time* x *Cash*) have statistically significant coefficients (shaded area) if not interacting with the proportion of beneficiaries in the model. At first glance, this can suggest that those terms can be excluded from the model without causing any problem. In fact, as argued earlier, their exclusion would yield a more intuitive expression for the marginal effects of *Time* and *Cash* by eliminating the first two terms in Equation 2 and Equation 3, which suggest that even though *PropBF*=0 marginal effects of *Time* and *Cash* would still be expected. However, there are several caveats to note. First, these variables are jointly significant, meaning that the null hypothesis of all three coefficients being simultaneously zero can be rejected with a high level of statistical significance[188]. Second, as argued by several authors (Friedrich, 1982; Brambor, Clark and Golder, 2006; Kam and Franzese, 2007) once researchers use an interactive model, all constitutive terms should be included, otherwise the risk of incurring in inferential errors dramatically increases. This recommendation is supported even though the coefficients of

188 The F-test yields F=22.56 (p=0.0000)

some (or all) constitutive terms are not statistically distinguishable from zero (as is the case in column (3)). The reason is that the other parameters of interest (β_1 to β_4) will be estimated with bias if the constitutive terms are dropped from the model. As can be seen in Table 24 (column (2)), all coefficients on the remaining terms change either in size or significance if *Time*, *Cash*, and *Time x Cash* are not included in the model. The bias is due to the fact that the excluded terms are correlated with those remaining in the model[189] and are also correlated with test scores[190]. Finally, as argued earlier, the exclusion of those terms implicitly assumes that the intercept of **Y** on *PropBF* ($\boldsymbol{\alpha_0}$ in expression (I)) is not conditional on *Cash* and *Time*, thereby imposing a fixed intercept into the model. The consequence is that the slope ($\boldsymbol{\alpha_1}$) for different values of *Time* and *Cash* will have to change to allow crossing at the same intercept ($\boldsymbol{\alpha_0}$), even though these changes of slopes are not the best fit to the empirical data. Given the results in Table 24 and based on these arguments, I adopt Model 2 as described in column (3) to estimate the coefficients on *BF* factors for all school outcomes.

Table 25[191] reports coefficients and robust standard errors for different school outcomes. Although the non-significance of the coefficients in columns (3) and (4) may raise some doubt about the relevance of *BF* factors for the last two outcomes in Table 25, the test for joint significance of those factors interacting with *PropBF* reject the null hypothesis of β_1 to β_4 equal to zero. Due to the interactive model, the relevance of the remaining constitutive terms, as argued earlier, rely on the role those terms have, if absent, in creating biased estimates of β_1 to β_4. The estimated coefficients presented in Table 25 will be used in the following section to assess the marginal effects of *BF* factors on school outcomes.

189 This should be obvious, since the remaining variables are interactions with those being dropped from the model.

190 Note that their coefficients in column (3) are not exactly zero and their magnitudes are comparable to the other coefficients.

191 Table A - 29 reports the coefficients and robust standard errors for all variables included in Model 2.

Table 25: Regression of school outcomes on *Bolsa Família* factors controlling for school composition and resources.

VARIABLES	(1) Portuguese	(2) Maths	(3) Pass-grade	(4) Dropout
Prop*BF* (β_1)	-0.385***	-0.390***	-0.071	0.001
	(0.089)	(0.101)	(0.067)	(0.028)
Prop*BF* x Cash (β_2)	0.012**	0.011**	0.004	-0.000
	(0.005)	(0.006)	(0.004)	(0.002)
Prop*BF* x *Time* (β_3)	0.008***	0.007**	0.001	-0.001
	(0.003)	(0.003)	(0.002)	(0.001)
Prop*BF* x Time x Cash (β_4)	-0.000*	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Cash (β_5)	-0.172	-0.290*	0.142*	-0.020
	(0.137)	(0.159)	(0.083)	(0.035)
***Time* (β_6)**	-0.073	-0.093	0.049	0.006
	(0.080)	(0.092)	(0.047)	(0.019)
Time x Cash (β_7)	-0.003	-0.003	-0.004	-0.000
	(0.005)	(0.005)	(0.003)	(0.001)
Controls	Yes	Yes	Yes	Yes
Constant	169.115***	187.238***	62.618***	16.689***
	(2.836)	(3.244)	(1.996)	(0.895)
Observations	33,805	33,805	33,805	33,805
Adjusted R-squared	0.523	0.491	0.320	0.294
Test for joint significance of coefficients on terms including ***PropBF***				
Ho: β_1= β_2= β_3= β_4=0				
F-test	172.5	139.6	5.735	22.27
Prob > F	0	0	0	0

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

7.6.4 Results (II): Marginal effects of *BF* intake on school outcomes

How can the results in Table 25 be interpreted? What do they indicate about the relevance of *BF* factors on school outcomes? As *BF* factors enter into the model in an interactive form, we cannot have a straightforward interpretation of the meaning of the coefficients $\boldsymbol{\beta_k}$ in respect to school outcomes. Considering the *ceteris paribus* nature of the regression analysis framework, it does not make any sense to ask for the effect of a unit of change, for instance, in *PropBF*, while holding *PropBF x Time* and the other terms interacting with *PropBF* constant. In this case, the slope of the relationship between school outcomes and the proportion of beneficiaries (*PropBF*) in the school depends on the values of *Time* and *Cash* as referenced in Equation 1 reproduced below.

$$\frac{\partial Y}{\partial PropBF} = \boldsymbol{\beta_1} + \boldsymbol{\beta_2 Cash} + \boldsymbol{\beta_3 Time} + \boldsymbol{\beta_4 Time.Cash}$$

The point estimation of the marginal effect will vary according to the estimated coefficients β_1 to β_4 and in respect to the values of the moderator variables *Time* and *Cash.* Every point estimation will have a confidence interval and significance level, both of which

require the estimated variance of the marginal effect of *PropBF* on school outcome ($\widehat{\boldsymbol{\sigma}}^{\boldsymbol{2}}_{\left(\frac{\partial \hat{y}}{\partial \boldsymbol{PropBF}}\right)}$) in order to be calculated. This will also depend on the values of *Time* and *Cash* and is given by the expression below[192]:

Equation 4

$$\hat{\sigma}^2_{\left(\frac{\partial \hat{y}}{\partial PropBF}\right)} = \widehat{Var(\beta_1)} + Cash^2.\widehat{Var(\beta_2)} + Time^2.\widehat{Var(\beta_3)} + Cash^2.Time^2.\widehat{Var(\beta_4)}$$
$$+ 2.Cash.\widehat{Cov(\beta_1,\beta_2)} + 2.Time.\widehat{Cov(\beta_1,\beta_3)} + 2.Cash.Time.\widehat{Cov(\beta_1,\beta_4)}$$
$$+ 2.Cash.Time.\widehat{Cov(\beta_2,\beta_3)} + 2.Time.Cash^2.\widehat{Cov(\beta_2,\beta_4)}$$
$$+ 2.Cash.Time^2.\widehat{Cov(\beta_3,\beta_4)}$$

Therefore, in order to estimate the marginal effect given by Equation 1 for a particular outcome (**Y**), a pair of values for *Time* and *Cash* must be chosen. A reasonable initial approach is to estimate $\boldsymbol{\partial Y/\partial PropBF}$ at "typical" values of the moderator variables in the sample of schools. **Table 26** shows the mean values and s.d. for school outcomes and *BF* factors nationally and by region.

Table 26: Mean and standard deviation for school outcomes and *Bolsa Família* factors

Region	*Portuguese*	*Maths*	*Pass-grade rate*	*Dropout rate*	*PropBF*	*Time*	*Cash*
North	162.8	177.7	85.1	4.0	41.2	27.6	18.2
	12.8	13.1	12.4	5.5	19.4	6.2	2.6
Northeast	157.9	174.2	80.3	5.9	52.1	30.9	18.7
	13.8	14.4	13.8	7.5	20.0	5.8	2.3
Central-West	174.1	190.5	90.5	2.5	32.1	25.9	16.6
	14.7	15.9	9.2	4.8	18.5	6.0	3.0
Southeast	179.0	197.3	89.4	1.3	30.1	27.8	16.5
	17.0	18.8	10.3	3.2	18.3	6.3	3.0
South	181.2	200.1	91.6	0.6	27.7	29.6	15.9
	15.1	17.0	8.6	1.8	17.8	7.1	3.4
Brazil	171.5	188.9	86.9	2.8	37.1	28.8	17.2
	18.2	19.9	12.1	5.4	21.4	6.5	3.1

Using Model 2[193] to estimate the coefficients entering Equation 1 and Equation 4, the marginal effect of *PropBF* on school outcomes can be estimated at the mean values of *Time* and *Cash* at the national level and by geographic region as reported in **Table 27**.

192 The STATA command "lincom" calculates the point estimate, the standard error, and the degrees of freedom, which then can be used to calculate the t-statistic and the level of significance.

193 When estimating by region, the covariate "region" is dropped from the model. When regressing pass-grade and dropout rates test scores are included as covariates in the model.

Table 27: Marginal effects of *PropBF* on school outcomes by region estimated at the mean value of *Cash* and *Time*.

School outcome	Brazil	Regions				
		North	Northeast	Central-West	Southeast	South
Portuguese	-0.110***	-0.075***	-0.069***	-0.122***	-0.099***	-0.173***
Maths	-0.112***	-0.073***	-0.069***	-0.126***	-0.093***	-0.186***
Dropout rate	-0.016***	-	-0.024***	-	-	-
Pass-grade rate	0.011***	-	0.014**	-	-	-

*** p<0.01, ** p<0.05, * p<0.1, - Not significant

Table 27 shows a "positive" marginal effect of *PropBF* on dropout and pass-grade rates[194] nationally and for the Northeast region. These are "positive" marginal effects in the sense that, on average, in schools where beneficiary families have been participating in *BF* for a period of about 2 1/2 years and receiving an average of R$ 17.2 per capita, the proportion of beneficiaries in the school is correlated with more students being promoted and fewer students abandoning school over the academic year. In other words, a higher proportion of beneficiaries in those schools is not deteriorating those indicators, but improving them.

However, Table 27 also indicates that *BF* intake has a consistent negative marginal effect, nationally and across regions, regarding school performance on the national exam for both Portuguese Language and Mathematics, when estimated at the mean values of *Time* and *Cash*[195]. This result both contrasts and seems to contradict the national positive marginal effects of *BF* intake on pass-grade rates. It suggests that schools with proportionally larger numbers of beneficiaries (after controlling for test scores) are not retaining proportionally more students, although they hold poorer results on average for the national exam. If schools with higher proportions of *BF* intake perform worse on the national exam, better pass-grade rates would not be expected, at least where promotion is based on learning outcomes as measured by the national exam[196].

The result here says something different and allows us to raise some hypotheses. It might be the case that poor social background expressed by participation in *BF* influences the decision on grade-retention in the opposite direction than would be expected based on test score results. Families' socioeconomic conditions might play a role in smoothing schools'

194 Here, "positive" means that an increase in the proportion of beneficiaries is associated with a reduction in dropouts (negative sign) and an increase in pass-grade rates (positive sign).

195 It is also clear that these effects are not different between the two disciplines. This is an expected result, since test scores in Portuguese and Mathematics are strongly correlated (see Table 21, Pearson's coefficient=0.93)

196 In fact, Table 21 shows positive correlation coefficients between pass-grade rates and mean test scores in both disciplines.

internal criteria for not promoting students. As a result, the pass-grade rate would be "inflated" by a factor proportional to the share of students from disadvantaged socioeconomic backgrounds. In this case, higher pass-grade rates associated with *BF* intake would not reflect improvements in learning, but attenuation of the retention criteria due to socioeconomic disadvantage. On the other hand, it is more likely that the reduction in the dropout rate due to *BF* intake, as expressed by the negative marginal effect of *PropBF* (-0.016), is the factor contributing to the increase in the pass-grade rate. In fact, since pass-grade, dropout, and grade failure are complementary rates (their sum equals one), we can say that the marginal reduction in the dropout rate must be followed by marginal increases in one or both of the other two indicators. Because the marginal effect on pass-grade is smaller than the marginal effect on dropout rate in absolute terms, one can conclude that both grade-failure and pass-grade rates are increasing as a result of *BF* intake. It is a positive result in the sense that not only are more pupils staying in school until the end of the academic year, but also in that more students are being promoted to the next grade, even considering that the grade-failure rate would be increasing as well.

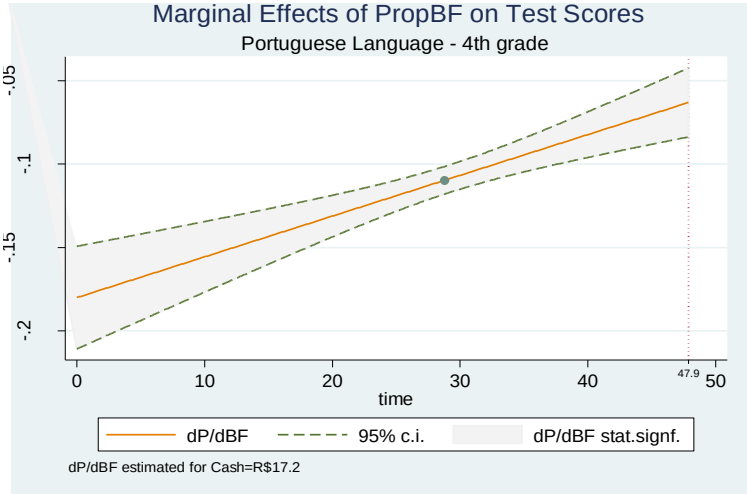
What about the test scores? The marginal effects of *PropBF* on test scores in Table 27 are all negative, both nationally and across regions. Although the results presented in Table 27 provide some evidence of the partial effects on school outcomes associated with *BF* intake, those are marginal effects estimated at just one point (mean values of *Time* and *Cash*) amongst several possibilities. Equation 1 tells us that changes in the *Cash* value, for instance, will produce different marginal effects with different statistical significances. In this sense, the resulting marginal negative effect of *BF* intake on test scores nationally and across regions, reported in Table 27, cannot be taken as a general result, only as the estimates calculated at the mean values of *Cash* and *Time*. As stressed by Kam and Franzese (2007), it is misleading to talk about "generally positive" or "generally negative" effects of variables involving linear interactions based only on point estimates. In order to provide a more complete picture of the marginal effects when estimating an interactive model, those authors strongly recommend the use of graphs, in which the estimated conditional effects and confidence intervals are plotted against a range of meaningful values of the conditioning variables.

Graph 27 shows the marginal effects of *BF* intake on Portuguese Language test scores at the national level plotted against time of participation in *BF*, holding fixed the cash transfer at its mean value (R$ 17.2)[197]. The dashed lines (hourglass shape) represent the 95% confidence interval of the estimated partial effect ($\partial Port./\partial PropBF$), which is plotted on the

197 Since the marginal effect depends on two variables, a two-dimensional graphical representation requires fixing one of them.

solid line. The shaded area shows the region of values of *Time* for which the marginal effect is statistically different from zero. The dot on the line marks the point estimate first reported in Table 27 (-0.110), which lies in the statistically significant region; it is, in fact, one estimate amongst various[198]. What does Graph 27 tell us?

Graph 27: Marginal Effects of *BF* intake on Portuguese test scores by time of participation in *BF*.



The graphic representation of the marginal effects allows us to see aspects that cannot be observed by just one point estimate, even if calculated at meaningful values of the relevant variables such as the mean. The estimated national marginal effect of *BF* intake on Portuguese Language test scores is actually statistically negative along the range of values for *Time* observed in the sample of schools, however with *diminishing values*. The longer the families receiving R$ 17.2 per capita are in the programme on average, the lower the negative effect perceived on school test scores due to the proportion of beneficiary children in the school. This suggests that schools in which families have benefited from *BF* for a longer period of time will, on average, have fourth grade beneficiary children performing *better* on the national exam, since their presence in the school degrades the mean test scores by a lower marginal rate. Graph 27 tells us that time of participation has an *attenuation effect* on the negative effect *BF* intake has on school test scores. If a causal relationship exists between the share of beneficiaries in a school and its poor performance on the national exam, this relationship appears to weaken with the time of participation in *Bolsa Família*. What about the per capita

198 Although several other estimates exist, the point estimate at the mean values of Time and Cash is the most accurate estimate amongst all possible estimates, as can be seen by the narrowing of the shaded area around that point.

cash? Does it also have a moderation effect on the negative association *BF* intake has on test scores at the school level?

By estimating the marginal effect line described in Graph 27 for different values of per capita cash, we can observe how *Cash* influences the *BF* intake marginal effect on Portuguese test scores at the national level. **Figure 8** shows the marginal effect lines for different percentiles of *Cash*[199]. The point estimate (-0.110) is also plotted as a reference point[200]. The scale on the vertical axis is fixed between -0.3 and +0.1 to allow the observation of changes on the slope.

Figure 8: Marginal Effects of *BF* intake on Portuguese test scores by time of participation at different percentiles of per capita *Cash*.

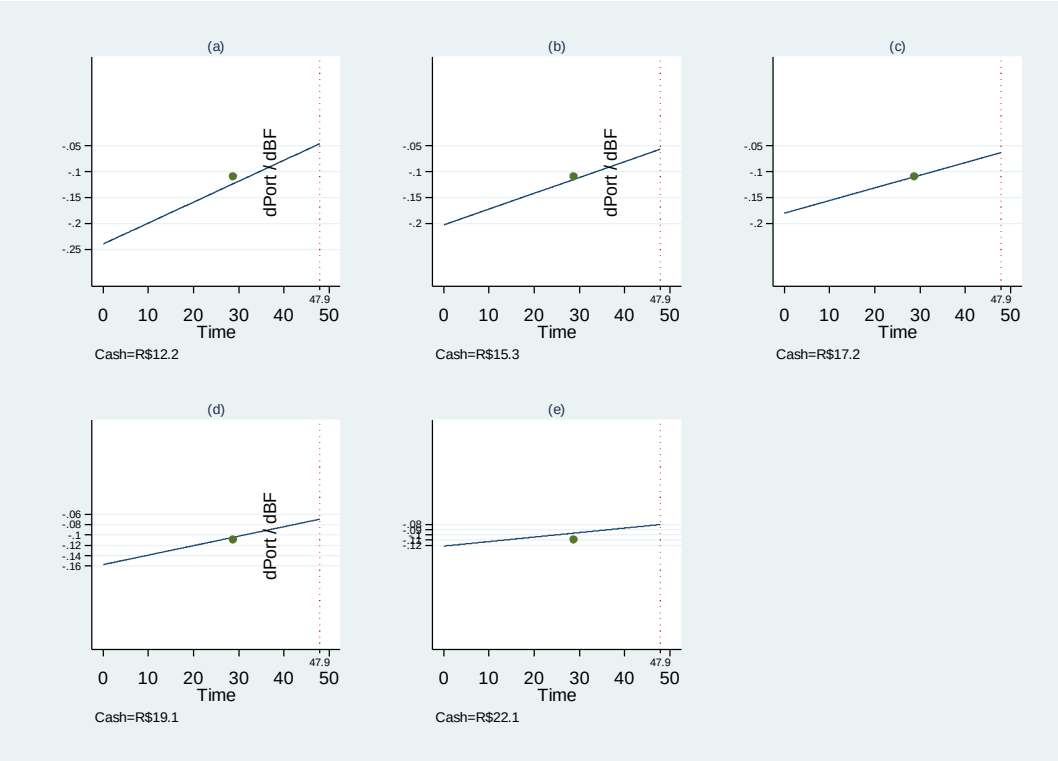


Figure 8 shows the same trend of diminishing negative marginal effects along time of participation for different values of *Cash*. It also indicates that for higher values of *Cash*, the marginal effect line shifts up at the same time as its slope decreases. The shift upwards for higher values of per capita cash suggests that the marginal effect of *BF* intake not only decreases with time of participation, but also with increases in the value of the benefit[201]. For

199 The following percentiles are estimated: 5th, 25th, 50th, 75th, and 95th.

200 Graph 27 is replicated in Figure 8(c). Notice that the mean for cash coincides with the median.

201 Notice that the point estimate (-0.110) is below the marginal effect line for higher values of per capita cash and above for lower cash values.

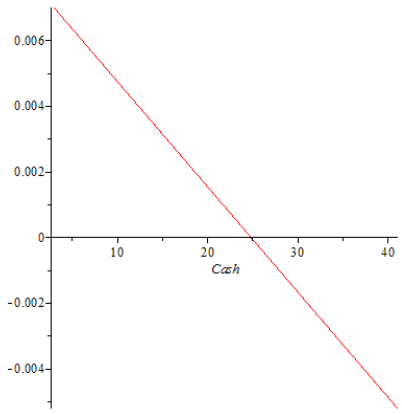
instance, schools with an average of 12 months of participation in *BF* with families receiving, on average, R$ 15.3 per capita (25th percentile) have an estimated marginal effect due to the proportion of beneficiaries equal to -0.166. Those schools where families receive R$ 19 per capita (75th percentile) have an estimated marginal effect of -0.136. However, the slope of the marginal effect line also diminishes as per capita cash increases. Thus, the rate at which the marginal effect declines with time of participation is lower for higher values of cash. In other words, *Time* loses its attenuation power over the marginal effect of *BF* intake as the benefit value increases. This variation in the slope is given by the partial derivative of **Equation 1** in relation to *Time* as expressed below[202]:

Equation 5

$$\frac{\partial^2 Y}{\partial PropBF\ \partial Time} = \beta_3 + \beta_4 . Cash$$

A graphic representation of the decline on the slope of the marginal effect line, based on the estimated coefficients β_3 and β_4 and cash values ranging within the available data, is displayed in **Graph 28**. The higher the cash value, the lower the variation rate of the *BF* marginal effect with time of participation. For values of *Cash* over R$ 24.80 per capita, increases in time of participation will not have any positive influence in reducing the marginal effect of *BF* intake. This result suggests the existence of a threshold for the positive moderation effect *Time* exerts on the marginal effect of *PropBF*. This threshold lies on the 99th percentile, meaning that less than 1% of the schools in the sample has such high mean values for per capita cash transfer. Therefore, a large majority of schools in 2007 were still able to accumulate gains from time of participation in *BF*.

Graph 28: Variation of the slope with cash value

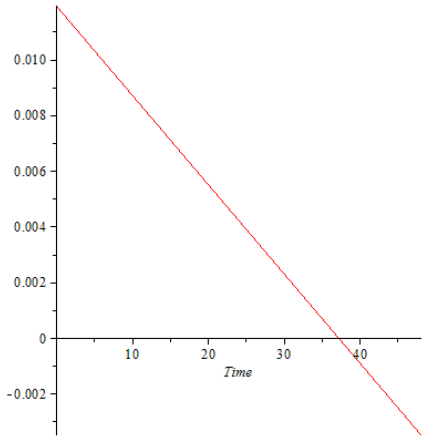


The inverse can also be shown. A threshold is also predicted by the model that limits the moderation effect *Cash* has on the marginal effect of *PropBF*. For instance, instead of plotting the marginal effect of *BF* intake against time of participation, **Figure A- 1** plots it against per capita cash. Each graph in Figure A- 1 shows the estimated marginal effect lines for different percentiles of *Time*. It can also be seen that the model predicts decreasing negative marginal effects over increasing values of *Cash,* although with diminishing slopes over the variable *Time*.

202 It is in fact the second derivative of Y on Prop*BF* and Time.

At the 95th percentile of time of participation, the slope is negative, an indication that the possible gains on the mean test scores due to increases in the value of the benefit have reached (and outstripped) a threshold now defined by the variable *Time*. **Equation 6** below gives the variation rate of the marginal effect of *BF* intake (slope) with *Cash*, which depends on *Time*. **Graph 29** shows a positive slope that diminishes with time of participation, where crossing zero when *Time* is equal to 37.2. Beyond this threshold, increases in per capita cash will not have any positive influence in reducing the marginal effect of *BF* intake. This value lies on the 90th percentile of the distribution of schools by *Time* so that there is only 10% of the sample with higher values for that variable. This suggests that 90% of the schools in 2007 were still improving their average test scores had families received more per capita cash over the previous months or years of participation.

Graph 29: Variation of the slope with time of participation



Equation 6

$$\frac{\partial^2 Y}{\partial PropBF\ \partial Cash} = \beta_2 + \beta_4.Time$$

To understand the meaning of such thresholds, we need to look again at Equation 5 and Equation 6. The negative estimated β_4 in Equation 5 indicates that as the value of the per capita cash transfer increases, the marginal gains for test scores due to time of participation in *BF* is attenuated. Likewise, the negative coefficient β_4 in Equation 6 implies that as time of participation increases, the marginal gains for test scores due to the per capita value of the benefit diminishes[203]. These are *compensating effects* and can be interpreted in terms of the relative importance of these two *BF* factors for improvements in test scores. For instance, time of participation in *BF*, as a generator of eventual gains for the schools' mean test scores, is increasingly of minor relevance as the value of per capita cash transfer increases. In other words, by paying more to families in per capita terms, the time lag necessary for a certain effect on test scores as a result of participation in the programme will be shorter. On the other hand, for low per capita cash, time of participation becomes more relevant for any improvement to be observed in test scores.

203 These relationships can also be inferred from Equation 2 and Equation 3 by rewriting those equations as:

$$\partial Y / \partial Cash = (\beta_5 + \beta_7.Time) + (\beta_2 + \beta_4.Time).PropBF$$
$$\partial Y / \partial Time = (\beta_6 + \beta_7.Cash) + (\beta_3 + \beta_4.Cash).PropBF$$

Because β4 is negative, the slope measuring the variation of the marginal effect of Cash (Time) with *BF* intake decreases with increases in Time (Cash).

Therefore, the negative β_4 suggests a *substitute* effect between these two *BF* factors, meaning that policy-makers, by allowing a more generous stipend to families, will probably observe improvements in test scores in a shorter period of time; conversely, by paying lower levels of benefits, policy-makers will wait longer for the expected results.

So far we have seen that conclusions about marginal effects based only on point estimates such as those described in Table 27 are misleading when interactions occur in the model being tested. The conclusion that the proportion of beneficiaries has a constant "generally negative" effect on test scores, based on the point estimates presented in Table 27, is not correct. It assumes an invariant marginal effect regardless of the two programme factors most likely to affect children's performance at school: the per capita cash value paid to families and the duration of the programme for those children. By examining the estimated marginal effect of *BF* intake along a series of values of *Cash* and *Time*, our conclusion completely changes. In fact, the results suggest that test scores are changing (improving), as consequence of either the value of the benefit paid to families or the time spent in the programme. In order to see more clearly the magnitude of these improvements, I will now look at the predicted test scores in Portuguese Language based on the estimated model at different values of *Time* and *Cash*.

7.6.5 Results (III): Predicted Test Scores and *Bolsa Família* factors

By analysing differences in predicted test scores across different values of *BF* intake, per capita cash, and time of participation using the estimated Model 2, it is possible to infer how influential *BF* factors are with respect to school achievement in test scores under different combinations of *BF* factors. For instance, Figure 8(a) shows how the negative marginal effect of *BF* intake decreases with time of participation while per capita cash is held at the fifth percentile (R$ 12.2). The decreasing negative marginal effect is expected to translate into diminishing gaps between schools with low and high *BF* intakes as time of participation increases. This gap reduction is illustrated in **Table 28**, which shows the predicted values of Portuguese Language test scores for different values of *BF* intake and *Time*, holding cash fixed at R$ 12.20[204] and the remaining covariates at their mean values. Reading down the first column one sees that schools in which beneficiary children have been enrolled for one year in *BF*, the estimated gap between the two extremes of the *BF* intake rank[205] is -19.1 points in the proficiency scale, while in schools where the mean time of participation reaches four years

Table 28: Predicted Test Scores (Portuguese) by *BF* intake and Time of Participation in *BF* (Cash=R$12.20)

BF intake	Time of Participation (months)				
(%)	12	24	36	48	Δ
0	181.7	180.4	179.1	177.8	-3.9
10	179.8	179	178.1	177.3	-2.5
20	177.9	177.5	177.2	176.8	-1.1
30	176	176.1	176.3	176.4	0.4
40	174.1	174.7	175.3	175.9	1.8
50	172.2	173.3	174.4	175.5	3.3
60	170.2	171.8	173.4	175	4.8
70	168.3	170.4	172.5	174.6	6.3
80	166.4	169	171.5	174.1	7.7
90	164.5	167.6	170.6	173.7	9.2
100	162.6	166.1	169.7	173.2	10.6
Δ	-19.1	-14.3	-9.4	-4.6	

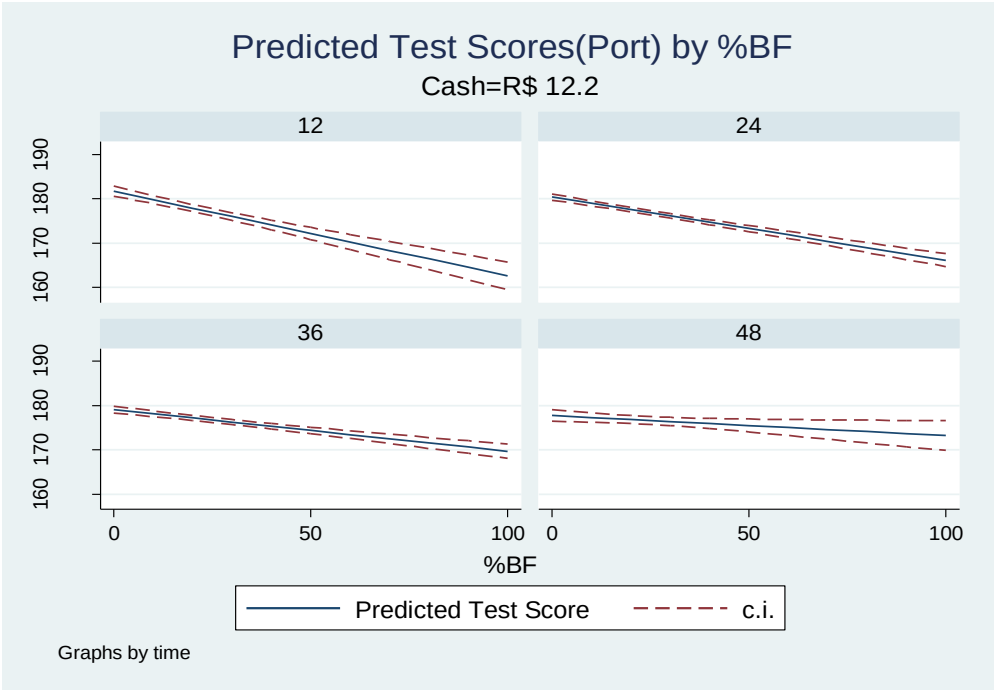
204 The predicted values in Table 28 are representative of the Southeast region (the reference category in Model 2). The indicated gaps will be the same for any region although the predicted test scores will vary. This is because the dummy variable Dj for region enters the model as an additive variable only, not interacting with the term moderating the slope (α1) that characterises the variation of test scores with *BF* intake. The dummy for region shifts the fitted model up and down, depending on the value of its coefficient **γj**.

205 Although one could argue that the group of schools with 0% *BF* intake and 12 months of participation is a nonsense that group can be thought of as representing those schools with a very low proportion of *BF* students (e.g. < 0.1%).

that gap is just -4.6 points. This represents a 14.5-point reduction in the gap (76%) equivalent to 0.85 s.d.[206]. By reading across each of the rows in Table 28, it is also noticed that in schools where beneficiaries represent more than 30% of the students, a positive and increasing difference in test scores is estimated between those with, on average, one and four years of *BF* participation. The largest difference is estimated in schools in which all children are beneficiaries in *BF*. The 10.6-point positive gap represents a gain of 0.62 s.d. in the mean test score for those schools with four years in *BF* in relation to those with just one year.

The predicted values shown in Table 28 are plotted in **Figure 9** with 95% confidence intervals for the estimates included. This shows how the slope measuring the negative effect of *BF* intake on test scores diminishes as time of participation increases. As *Time* increases, the fitted line reduces its inclination indicating the attenuation effect of time of participation on the negative relationship observed between *BF* intake and test scores. As consequence, a reduction in the gap between schools at different levels of *BF* intake across different values of *Time* is also detected.

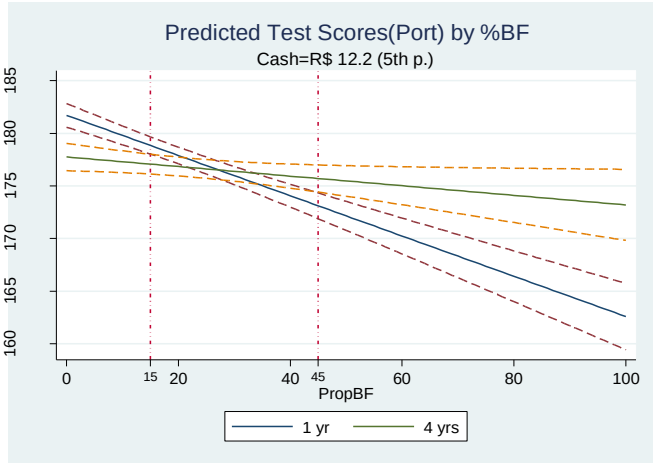
Figure 9: Predicted Test Scores (Portuguese) for different periods of participation in *BF*.



206 See Table 26 for standard deviations of school outcomes. I use the values for the Southeast region since the estimates are calculated for that region.

The reduction in the gap is better evaluated by overlapping the confidence intervals as illustrated in **Graph 30**, in which the fitted lines for two different values of *Time* (12 and 48 months) are plotted together with their confidence intervals. It shows that not all differences presented in each row of Table 28 are statistically significant. In fact, only values of *BF* intake above 45% have estimated positive gaps that are statistically significant different from zero[207]. As the proportion of beneficiaries increases the relevance of time of participation becomes more and more discernible, since *BF* students tend to dominate the mean school test score. For schools with very low proportions of beneficiaries, whatever happens to this group will not affect the school mean in any meaningful way. In the context presented in Graph 30[208] only schools with more than 45% of students taking part in *BF* are likely to notice differences in the mean test score over time.

Graph 30: Predicted Test Scores (Portuguese) for one and four years of participation in *BF*.



So far I have explored the potential effect of *BF* on school outcomes based on an interactive model in which three factors associated with participation in the programme were considered: proportion of beneficiaries in school, mean time of participation, and mean per capita cash paid to families. The model tested (Model 2) considered all schools in the sample, i.e. schools in which those three factors are shown to be variable and influential on school outcomes. Two of those factors (*Time* and *Cash*) were found to be moderators of the marginal effect of *BF* intake in a way that suggests a positive effect of *BF* on school outcomes. In the next subsection I analyse a subsample in which schools lie in the same quintile of *BF* intake

207 A statistically significant difference is also observed for values below 15%. However, it is very unlikely that in schools with less than 15% *BF* intake any factor involving those participants will be of major relevance in determining school achievement in test scores.

208 Schools with a mean cash of R$ 12.20 and household and school resources around the mean value.

(the fifth quintile). For this subsample it is shown that one of the *BF* factors (*BF* intake) is not relevant to school outcomes, since all schools are very similar in respect to that factor. The direct effects of *Time* and *Cash* on school outcomes are then estimated using a simplified version of Model 2.

7.6.6 Results (IV): Focusing on the 5th quintile of *BF* intake.

I now analyse a subsample of schools in which one of the *BF* factors – the proportion of beneficiaries – can be considered constant across all schools in terms of its influence on school outcomes. I take only schools from the fifth quintile of *BF* intake, that is, schools in which more than 80% of pupils are beneficiaries in *BF*. By doing this, the interaction model discussed in the previous subsections is simplified, as is the analysis of the marginal effects of *Time* and *Cash*, since they will no longer depend on a third variables (on *PropBF*). The analysis of marginal effects of *Time* and *Cash* on school outcomes for this subsample is also relevant inasmuch as this is the less well-resourced group of schools and also the one in which pupils' families come from the most disadvantaged socioeconomic backgrounds (as discussed in section 7.3). Any significant positive marginal effect of *BF* factors on school outcomes will be estimated in a far more adverse school context than in the previous section.

I start by testing the assumption that *PropBF* is not relevant for school outcomes in schools of the fifth quintile. Using Model 2, I test whether the coefficients on terms involving the variable *PropBF* are jointly significant. As reported in **Table 29**, the null hypothesis stating $\beta_1=\beta_2=\beta_3=\beta_4=0$ is not rejected for all school outcomes. It is expected that in schools where more than 80% of the students are beneficiaries, outcomes are not significantly influenced by changes in the proportion of beneficiaries. Although not all students in these schools are necessarily enrolled in the programme, a large majority of beneficiaries tend to determine the mean school outcome. Therefore, the terms including *PropBF* can be excluded from the model.

Table 29: Regression of school outcomes on *BF* factors controlling for school composition and resources (5th quintile of *BF* intake)

VARIABLES	(1) Portuguese	(2) Mathematics	(3) Pass-grade	(4) Dropout
Prop*BF* (β_1)	-0.808	0.093	3.306	-0.874
	(2.403)	(2.774)	(2.478)	(1.011)
Prop*BF* x Cash (β_2)	0.052	0.005	-0.184	0.030
	(0.129)	(0.150)	(0.134)	(0.056)
Prop*BF* x *Time* (β_3)	0.028	0.005	-0.094	0.025
	(0.081)	(0.094)	(0.083)	(0.033)
Prop*BF* x Time x Cash (β_4)	-0.002	-0.000	0.005	-0.001
	(0.004)	(0.005)	(0.005)	(0.002)
Cash (β_5)	-2.415	0.469	16.777	-3.105
	(11.495)	(13.283)	(11.881)	(5.016)
***Time* (β_6)**	-1.328	0.045	8.272	-2.486
	(7.184)	(8.288)	(7.333)	(2.943)
Time x Cash (β_7)	0.077	0.000	-0.457	0.080
	(0.388)	(0.450)	(0.398)	(0.165)
Controls	Yes	Yes	Yes	Yes
Constant	188.216	147.056	-245.311	105.225
	(214.904)	(246.734)	(219.735)	(90.664)
Observations	1,277	1,277	1,277	1,277
Adjusted R-squared	0.297	0.256	0.195	0.156
Test for joint significance of coefficients on terms including ***PropBF***				
Ho: $\beta_1 = \beta_2 = \beta_3 = \beta_4 = 0$				
F-test	**0.781**	**0.982**	**0.749**	**1.895**
Prob > F	**0.538**	**0.416**	**0.559**	**0.109**

The model to be estimated then turns into a simplified version of Model 2 that includes only two interacting *BF* factors – *Cash* and *Time*:

Model 3

$$Y = \beta_0 + \beta_1 Cash + \beta_2 Time + \beta_3 Time.Cash + \sum_k \alpha_k X_k + \sum_j \gamma_j D_j + u$$

The set of covariates is the same as in Model 2[209] (see Table 23) and the intercept is also allowed to vary according to region by the inclusion of the dummy variable $\mathbf{D_j}$. **Table 30** reports coefficients and robust standard errors on *BF* factors for different school outcomes, as well as the F-statistic to assess the joint significance of those factors[210]. The results show that the null hypothesis stating that *BF* factors are jointly insignificant as predictors of test scores in Portuguese Language and pass-grade rates can be rejected (at the 5% level). In the case of test scores in Mathematics, we cannot confidently reject the null hypothesis. It can be inferred that *BF* factors are, in this case, irrelevant at the national level for test scores in Mathematics in schools pertaining to the fifth quintile of *BF* intake. However, the null hypothesis is rejected for

209 As in Model 2 when regressing pass-grade and dropout rates test scores are included as regressor. Also, the dummy for region is excluded when running the model for subsamples by region.

210 See Table A - 30 in the appendix for the full set of variables.

the Northeast (at the 10% level) and South regions (at the 0.1% level) if the test for Mathematics is run by region (see **Table A - 31**).

In the case of dropout rate, at first glance the temptation is also to conclude for the joint insignificance of *BF* factors at the national level, since the F-statistic also yields a rho value above the conventional levels of statistical significance. However, here some caution is needed. The fact that β_2 is marginally significant (ρ=0.098) cannot be ignored[211]. As stressed by Wooldridge (Wooldridge, 2009, p.149) it is statistically possible that a group of insignificant variables is clustered with a significant variable, yielding a jointly insignificant result at the conventional levels. Logically, if the hypothesis of β_2=0 is rejected, even with marginal confidence, an inconsistency is engendered if one does not likewise reject the null Ho: β1=β2=β3=0, although the F-statistic may suggest the opposite. This conflict between the t-test for an individual variable and the F-test for a group of variables cannot be resolved without some critical judgement on the part of the researcher. I will, at this point, reject the null (Ho) for dropout rates and move on to estimate the marginal effects of *Time* and *Cash*. If, in fact, *BF* factors are not relevant for dropout rates at the national level, this will become evident in the confidence intervals plotted with the estimated marginal effects of *Time* and *Cash* in the forthcoming paragraphs. Moreover, the test run by region shows that the null hypothesis can be rejected for the Northeast and Central-West regions at the 10% level (see **Table A - 32**).

Table 30: Regression of school outcomes on *Cash* and *Time* (5th quintile of *BF* intake)

VARIABLES	(1) Portuguese	(2) Mathematics	(3) Pass-grade	(4) Dropout
Cash (β_1)	2.258***	1.024	0.445	-0.531
	(0.826)	(0.948)	(0.902)	(0.342)
Time (β_2)	1.168**	0.485	-0.038	-0.329*
	(0.503)	(0.575)	(0.558)	(0.199)
Time x Cash (β_3)	-0.068**	-0.034	0.004	0.015
	(0.027)	(0.031)	(0.030)	(0.011)
Controls	Yes	Yes	Yes	Yes
Constant	115.438***	153.806***	48.463**	28.554***
	(17.502)	(19.907)	(19.018)	(7.221)
F- statistic for the joint significance of *Bolsa Família* factors (Ho:$\beta_1=\beta_2=\beta_3=0$)	**F(3,1252) = 3.45**	**F(3,1252) = 1.50**	**F(3,1250) = 2.86**	**F(3,1250) = 1.75**
	ρ = 0.0161	**ρ = 0.2136**	**ρ = 0.0359**	**ρ = 0.1543**
Observations	1,277	1,277	1,277	1,277
Adjusted R-squared	0.297	0.256	0.196	0.155

*** p<0.01, ** p<0.05, * p<0.1

211 Also β1 is not only sizable but is estimated with ρ=0.120.

As argued earlier, given the interactive nature of the model being tested, the coefficients in Table 30 do not coincide with the effects of the variables involved in the interaction (*Time* and *Cash*). The meaningful effects are, in fact, the *marginal effects* of *Time* and *Cash*, which depend upon two coefficients and on the values of the other variable[212].

The estimated marginal effects of *Time* and *Cash* on school outcomes (Y) are given by the expressions:

Equation 7

$$\frac{\partial \hat{Y}}{\partial Cash} = \widehat{\beta_1} + \widehat{\beta_3}.Time$$

Equation 8

$$\frac{\partial \hat{Y}}{\partial Time} = \widehat{\beta_2} + \widehat{\beta_3}.Cash$$

The variances of the marginal effects are estimated by the expressions below, which are used to calculate the standard errors of the estimated marginal effects[213]:

Equation 9

$$\hat{\sigma}^2_{\left(\frac{\partial \hat{Y}}{\partial Cash}\right)} = \widehat{Var(\beta_1)} + Time^2.\widehat{Var(\beta_3)} + 2.Time.\widehat{Cov(\beta_1, \beta_3)}$$

Equation 10

$$\hat{\sigma}^2_{\left(\frac{\partial \hat{Y}}{\partial Time}\right)} = \widehat{Var(\beta_2)} + Cash^2.\widehat{Var(\beta_3)} + 2.Cash.\widehat{Cov(\beta_2, \beta_3)}$$

Once the coefficients β_1, β_2, and β_3 are estimated for each school outcome, a set of general hypotheses can be tested as to whether or not *BF* factors have any influence on school outcomes. Three general hypotheses are tested for each school outcome[214] at the national level in relation to each *BF* factor (*Time* and *Cash*) for the schools on the fifth quintile of *BF* intake: (i) Do *BF* factors affect the school outcome? ; (ii) Do *BF* factors improve the school outcome? ; (iii) Do the effects of *BF* factors on the school outcomes depend on each other? Table **31** describes the hypothesis to be tested for each school outcome and *BF* factor, the statistical test used, the result, and the conclusion.

212 The same happens to the variance of the marginal effects, upon which the statistical significance of the estimated marginal effects depend.

213 Again, the STATA command "lincom" does the job of generating the estimates for the marginal effect as well as the standard error and degrees of freedom so to allow the statistical significance to be calculated.

214 All school outcomes are tested except for the test scores in Mathematics, since we failed to reject the null hypothesis ($\beta1=\beta2=\beta3$) at the national level in that case.

Table 31: Hypothesis tests for effects on school outcomes

School outcome (Y)	*Bolsa Família* factor	Hypothesis to be tested	Ho (Null hypothesis):	Test	Result	Conclusion
Test Score in PORTUGUESE	**Cash**	Does *Cash* affect test score?	$\beta_1 = \beta_3 = 0$	F-test	F=3.878 Prob. > F = 0.0209	Cash **affects** test score.
		Does test score **increase** with Cash?	$\beta_1 + \beta_3.\text{Time} \leq 0$	Multiple t-tests (for each value of *Time*)[215]	Multiple results to be assessed by plotting the marginal effects of *Cash* across different values of *Time*.	(Discussed in the text)
		Does the effect of *Cash* on test score depend on *Time*?	$\beta_3 = 0$	t-test	t=-2.52 Prob. > |t| = 0.012	The effect of *Cash* on test score **depends** on *Time*.
	Time	Does *Time* affect test score?	$\beta_2 = \beta_3 = 0$	F-test	F=4.073 Prob. > F = 0.0173	*Time* **affects** test score
		Does test score **increase** with *Time*?	$\beta_2 + \beta_3.\text{Cash} \leq 0$	Multiple t-tests (for each value of *Cash*)[216]	Multiple results to be assessed by plotting the marginal effects of *Time* across different values of *Cash*.	(Discussed in the text)
		Does the effect of *Time* on test score depend on *Cash*?	$\beta_3 = 0$	t-test	t=-2.52 Prob. > |t| = 0.012	The effect of *Time* on test score **depends** on *Cash*.
PASS-GRADE RATE	**Cash**	Does *Cash* affect pass-grade rates?	$\beta_1 = \beta_3 = 0$	F-test	F=4.286 Prob. > F = 0.0140	Cash **affects** pass-grade rates.
		Does pass-grade rate **improve** with *Cash*?	$\beta_1 + \beta_3.\text{Time} \leq 0$	Multiple t-tests (for each value of *Time*)[217]	Multiple results to be assessed by plotting the marginal effects of *Cash* across different values of *Time*.	(Discussed in the text)
		Does the effect of *Cash* on pass-grade rate depend on *Time*?	$\beta_3 = 0$	t-test	t=-0.14 Prob. > |t| = 0.892	The effect of *Cash* on pass-grade rate **does not depend** on *Time*.
	Time	Does *Time* affect pass-grade rates?	$\beta_2 = \beta_3 = 0$	F-test	F=0.161 Prob. > F = 0.851	Time **does not affect** pass-grade rates
		Does pass-grade **improve** with *Time*?	$\beta_2 + \beta_3.\text{Cash} \leq 0$	Given the previous result no test is needed.	N/A	Since *Time* does not affect pass-grade rate the latter cannot improve either.
		Does the effect of *Time* on pass-grade rate depend on *Cash*?	$\beta_3 = 0$	t-test	t=-0.14 Prob. > |t| = 0.892	The effect of *Time* on pass-grade rate **does not depend** on *Cash*[218].

215 As stressed by Kam and Franzese (2007), in the context of linear-interactive models, hypotheses tests for the sign of the dependency tend to be ambiguous since the estimated value (e.g. β1+ β3.Time) might be positive, negative, or null depending on the values of the variable interacting (e.g. Time). For this reason, the assessment of how test scores vary with Cash will be assessed using graphs for marginal effects, as developed in the previous subsection.

216 See note 218.

217 See note 218.

218 This test is also unnecessary given that the first test had already failed to reject the null hypothesis leading to the conclusion that Time has no effect on pass-grade rate. Since Time has no effect on pass-grade rate this effect does not exist and therefore cannot depend on anything else.

School outcome (Y)	*Bolsa Família* factor	Hypothesis to be tested	Ho (Null hypothesis):	Test	Result	Conclusion
DROPOUT RATE	**Cash**	Does *Cash* affect dropout rates?	$\beta_1 = \beta_3 = 0$	F-test	F=1.33 Prob. > F = 0.2649	*Cash* **does not affect** dropout rates.
		Does dropout rate **decrease** with *Cash*?	$\beta_1 + \beta_3.Time \geq 0$	Given the previous result no test is needed.	N/A	Since *Cash* does not affect dropout rates the latter cannot decrease either.
		Does the effect of *Cash* on dropout rate depend on *Time*?	$\beta_3 = 0$	Given the previous result no test is needed.	N/A	Since *Cash* does not affect dropout rates the latter cannot depend on *Time*.
	Time	Does *Time* affect dropout rates?	$\beta_2 = \beta_3 = 0$	F-test	F=2.47 Prob. > F = 0.0852	*Time* **affects** dropout rates
		Does dropout **decrease** with *Time*?	$\beta_2 + \beta_3.Cash \geq 0$	Multiple t-tests (for each value of *Cash*)[219]	Multiple results to be assessed by plotting the marginal effects of *Time* across different values of *Cash*.	(Discussed in the text)
		Does the effect of *Time* on dropout rate depend on *Cash*?	$\beta_3 = 0$	t-test	t=- 1.38 Prob. > |t| = 0.168	The effect of *Time* on dropout rate **does not depend** on *Cash*.

For **Portuguese Language** we can confidently reject the null hypothesis of no effect of *Time* and *Cash* on test scores and also conclude that the effects of *Time* on test scores depend on the level of *Cash* and *vice-versa*. As to whether those effects are associated with improvements on test scores can only be assessed at particular values of each *BF* factor upon which the marginal effect depends (to be assessed later in this section). **Pass-grade rate** seems to be affected by *Cash*, but not by the mean *time* of participation in *BF*, and the effect of *Cash* does not depend on the value of *Time* either. In other words, time of participation turns out to be irrelevant to pass-grade rates and also does not change the effect that *Cash* has on that outcome. The sign of that effect can be assessed later using graphical analysis. Finally, **dropout rate** is not affected by the value of cash, but we can marginally reject the hypothesis of no influence of *Time* (at the 10% level). The effect of *Time* does not depend on the value of cash and its direction is better assessed using graphical analysis later in this subsection[220].

The hypotheses tested do not give us any idea about the size of the effects of *Time* and *Cash* on school outcomes when they happen to be considered statistically different from zero. One way of looking at the size of the marginal effects estimates given by Equation 7 and Equation 8 nationally and regionally is by estimating those effects at meaningful values of the

219 See note 218.

220 I do not develop the same set of hypotheses step-by-step as I did for the national level by region. The results by region are reported for the point estimates, in the same way I presented in section 7.6.4, in Table 33.

variables upon which they depend. For instance, taking the mean values of *Cash* and *Time* nationally and by region for schools in the fifth quintile of *BF* intake (**Table 32**), the estimated marginal effects of *Time* and *Cash* and level of significance are reported in **Table 33**[221].

Table 32: Mean and standard deviation of school outcomes and *Bolsa Família* factors in schools of the fifth quintile of *BF* intake

Region	*Portuguese*	*Mathematics*	*Pass-grade rate*	*Dropout rate*	*Time*	*Cash*
North	159.1	175.2	83.0	4.3	27.6	17.8
	12.1	11.9	14.2	6.0	5.6	2.1
Northeast	154.7	171.4	80.9	4.9	31.8	18.6
	13.3	14.6	13.2	6.8	5.4	2.0
Central-West	168.3	184.3	91.0	2.1	25.7	17.6
	13.4	14.8	9.3	3.5	5.7	2.7
Southeast	169.2	188.9	86.2	1.7	30.8	16.7
	15.1	18.1	14.4	2.8	5.8	1.9
South	164.9	182.9	86.5	0.9	31.9	17.5
	12.4	15.3	13.9	3.1	5.9	1.9
Brazil	158.0	175.0	82.4	4.1	31.1	18.2
	14.5	16.3	13.6	6.2	5.8	2.2

Table 33: Marginal effects of *Time* and *Cash* on school outcomes by region, estimated at the mean values of *cash* and *time* for schools in the fifth quintile of *BF* intake

School outcome	*Bolsa Família factor*	Brazil	Regions				
			North	Northeast	Central-West	Southeast	South
Portuguese	*TimeBF*	-	-	-	-	-	-
	CashpcBF	-	-	-	-	-	-
Maths	*TimeBF*	-	-	-0.190**	-	-	-
	CashpcBF	-	-	-	-	-	-3.010***
Dropout rate	*TimeBF*	-0.061*	-	-	-	-	-
	CashpcBF	-	-	-	-0.908**	-	-
Pass-grade rate	*TimeBF*	-	-	-	-	-	-
	CashpcBF	0.570***	-	0.420*	-	2.209***	-

*** p<0.01, ** p<0.05, * p<0.1, - Not significant

Although the point estimates in Table 33 are insufficient to assess the whole of the marginal effects associated with *BF* factors[222], they allow us to gauge the magnitude of those effects and see how they differ across regions for particular values of *Time* and *Cash*. For instance, at the national level, the marginal effect of *Time* on **dropout rates** (-0.061) suggests that, for schools in which the mean value of cash paid to families is R$ 18.20, each additional 10 months of participation in the programme is associated with a reduction of 0.6 p.p. in the dropout rate. As I did not reject the hypothesis of $\beta_3=0$ in Equation 8 for time of participation

221 The shaded areas mark the "positive" marginal effects, that is, those effects associated with improvements in school outcomes.

222 As argued in subsection 7.6.4 they are only one amongst several estimates that can be more properly assessed by graphical analysis.

(see **Table 31**), I conclude that the marginal effect of *Time* on dropout does not depend on the level of cash paid to families for that group of schools. This does not mean that the estimated effect of time will occur for every value of cash[223], but that when the marginal effect of *Time* is different from zero this will not differ across different levels of cash, and one can assume that the estimate at the mean value of cash (0.061) is the national mean effect of *Time* on dropout rates for schools with a large majority of pupils registered in *BF*. Likewise, the national marginal effect of *Cash* on **pass-grade** rates does not depend on *Time*. Therefore, the estimated marginal effect (0.570) at the mean value of time (31 months) represents the national average effect of a unit increase of per capita cash on pass-grade rates for children attending the most disadvantaged schools, independent of how long they have been in the programme. One *real* (R$) increase in the per capita cash value is associated with a 0.6 p.p. increase in pass-grade rates. This estimate gives us a significant and high associated effect of *Cash* on pass-grade rates for those schools in which that indicator is the lowest observed amongst schools (see Graph 26).

Across regions we can also observe significant marginal effects of *Cash* on pass-grades and dropout rates. In the Central-West**,** a reduction of 0.9 p.p. in **dropouts** is estimated for an increase of *one real* (R$) in the mean per capita cash paid to families in schools of the region with a mean time of participation of 26 months. In the Northeast, a marginal effect on **pass-grade rates** of a 0.4 p.p. increase for each additional *real* (R$) on the mean value of per capita cash paid in schools of the region is estimated at a 10% significance level. In the Southeast, the marginal effect of *Cash* is estimated with high level of significance and is also the highest, suggesting an increase of 2.2 p.p. in pass-grade rates for a one *real* (R$) increase in mean per capita cash[224]. Again, these are only point estimates at the mean values of *Cash* and *Time* and cannot translate the whole picture of how those effects vary according to changes in those variables.

In the same way, the predominant absence of significant marginal effects of *Cash* and *Time* for test scores nationally and across regions in Table 33 cannot be taken as a general result. Not only do both *Cash* and *Time* affect test scores in Portuguese Language at the national level (see **Table 31**), but those variables also affect each other's effects on test scores. It is possible that the effects of *BF* factors are also detectable for test scores in Mathematics if

223 I will show later the interval of cash values for which the marginal effect of time is statistically different from zero.

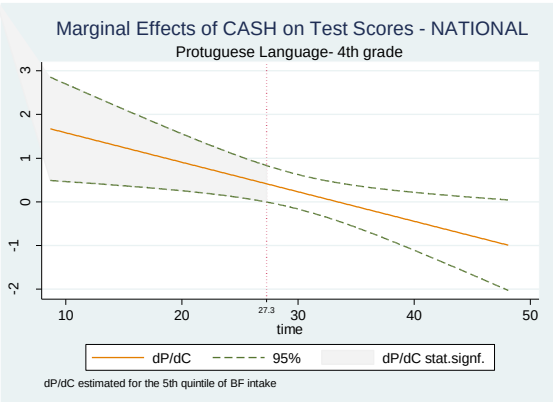
224 One can raise the question why there is a higher effect in the Southeast compared to the Northeast. Perhaps better school conditions in the Southeast play a role in the final results of beneficiaries.

the analysis is drawn from the national to the regional[225]. As discussed earlier, by looking at single point estimates alone, one can draw incorrect conclusions about the relevance of *BF* factors for school outcomes. Therefore, in the following paragraphs I present the marginal effect lines of each *BF* factor at the national level for different school outcomes.

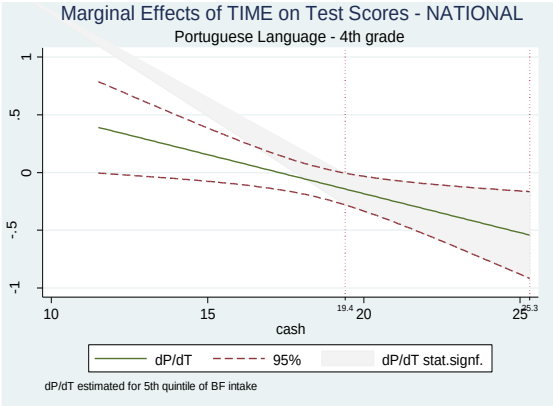
7.6.6.1 Test Scores

The full description of the marginal effects reported in Table 33 requires graphical representation of Equation 7 and Equation 8, including the confidence intervals at each estimated point. This means that for each cell in Table 33 a graph is needed[226] to show the variation of the marginal effect. The interval of values for which the marginal effects are estimated is also limited to the real range of the values of *Time* and *Cash* in the sample of schools in the fifth quintile (**Table A - 33** and **Table A - 34**). **Graph 31** and **Graph 32** show the marginal effect lines of *Cash* and *Time* on test scores in Portuguese Language at the national level.

Graph 31: Marginal Effects of CASH on Portuguese Test Scores



Graph 32: Marginal Effects of TIME on Portuguese Test Scores



225 Remember that results in Table 31 are valid only for the national level and that, in the case of Mathematics, the test by region revealed joint significance of β1, β2, and β3 for the Northeast and South regions (Table A - 31), which explain the two significant, although negative, marginal effects estimated for Mathematics in Table 33 in those regions.

226 I display here only the marginal effect lines for the national level.

Based on the range of significant estimates in Graph 31, it can be argued that in schools of the fifth quintile of *BF* intake the value of per capita cash paid to poor families not only affects test scores in Portuguese Language, but it does so in a positive direction[227]. The significant marginal effects vary between 1.67 when time of participation is around 9 months (the minimum value) to 0.42 when time is around 27 months (25th percentile), a four-fold drop across the range of values for time of participation. This shows a diminishing marginal effect of *Cash* with *Time,* the rate of which is given by the estimated coefficient β3 in Table 30 (-0.068). The slope in Graph 31 describes that trend. The question of whether test scores in Portuguese Language increase with cash (raised in Table 31) can now be answered by analysing the interval in which the marginal effect line in Graph 31 is significant and positive, which is the case for values of *Time* below 27 months. As expressed in Equation 7, the partial effect of *Cash* depends on the initial value of *Time* at which it is estimated. The longer families have been in the programme, on average, the lower the expected gain in test scores (Portuguese) due to a one *real* increase in the per capita cash for those families. This result suggests that potential positive effects on test scores due to increases in the per capita cash transfer to families are constrained by a timeframe within which that policy is more likely to generate educational benefits. The maximum gain from cash transfer increments on test scores would materialise if it occurred at early stages of participation in *BF*. Even in less well-resourced schools, such as those with more than 80% of children enrolled in *BF*, the analysis so far suggests that children would benefit educationally from higher values of cash transferred to their families through higher test scores on the national exam, provided that any increase in cash occurs before they have been enrolled in *BF*[228], on average, for 27 months.

Graph 32 shows a decreasing marginal effect of *Time* along values of per capita cash. Although the estimates are positive for the initial range of cash values, they are not statistically distinguishable from zero (at a 5% level of significance) for values lower than R$19.40, and becomes negative above that threshold. This threshold corresponds to the 72nd percentile in the subsample of schools analysed here. Therefore, for a large majority (72%) of schools, the marginal gain from time of participation seems to be irrelevant and has a negative contribution

227 This result shows us again how misleading point estimates, such as those presented in Table 33, can be if taken as general conclusions about the effects of interactive variables.

228 A clear limitation of Model 3 is to not account for the possibility that such marginal gains from increases in per capita cash may not be the same for every level of household per capita income or for every cash value transferred to families. It is likely that the marginal effect of cash diminishes with both household per capita income and current value of cash transfer. This non-linear change of the marginal effect of cash would reflect the idea that one extra real (or dollar) would benefit poor children more than it would benefit less poor children.

for those above the mean value of R$ 19.40 per capita. How do the two marginal effects (of *Cash* and *Time*) combine to change results in schools of the fifth quintile of *BF* intake?

The reasoning behind marginal effects can be counterintuitive at times, so it is worth exploring an example to clarify their implications for test scores[229]. Consider two schools, A and B, from our group of high *BF* intake schools. In 2007, the year of the national exam, these two schools have equivalent school composition and school resources in terms of the variables described in **Table A - 30** (these variables are fixed at their mean values). They are also located in the same region in Brazil (Southeast). Assume that both schools have families with an average of 24 months of participation in *BF* by the time they take part in the national exam. During the previous two years before the exam, however, the two schools experienced different courses of benefits paid to families. In school A, families were paid an average of R$ 13.60 per capita/month[230] over their entire period in *BF*. In school B, families had an increase in the benefit after 12 months of enrolment in the programme, increasing the average per capita cash to the 95th percentile of the distribution (R$ 19.70). How would this affect their outcomes?

Differences in test scores in Portuguese Language obtained by schools in 2007 are predicted according to finite changes in *Time* and *Cash* by the following expressions:

Equation 11

$$\Delta Y = (\beta_2 + \beta_3 . Cash) . \Delta Time$$

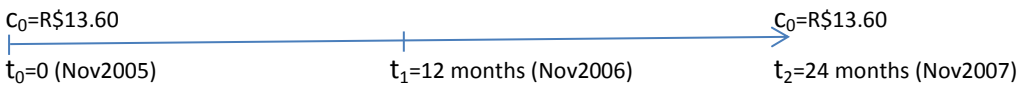
Equation 12

$$\Delta Y = (\beta_1 + \beta_3 . Time) . \Delta Cash$$

In 2007, School A has two years of participation over which families received an average of R$ 13.60, as depicted below.

Ex. 1

School A:



By plugging the estimated coefficients (β_2 and β_3), the per capita cash (c_0), and the period of participation in *BF* over which the families received the cash transfer (t_2-t_0) into

229 References to test scores in this subsection refer to achievements in Portuguese Language unless otherwise specified.

230 This is equivalent to the fifth percentile of the per capita cash distribution in the Southeast (see Table A - 33).

Equation 11, one can estimate the expected difference in test scores in 2007[231] due to the combined effects of time of participation and cash transfer.

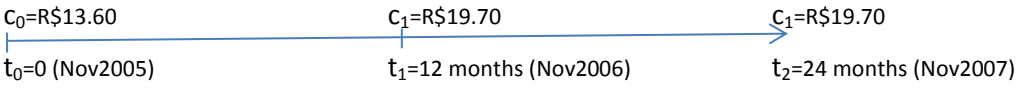
$$\Delta Y = (\beta_2 + \beta_3.c_0).(t_2 - t_0) = (1.168 - .068 \times 13.60).24 = 5.84$$

School A is expected to have a mean test score in Portuguese Language that is 5.84 points higher than a school from the same group that has just started in *BF* in 2007, with families receiving the same cash (c_0)[232].

Families in School B had an increase in their benefit, raising the average per capita cash at the school level to R$ 19.70 after 12 months in *BF*, as represented below.

Ex. 2

School B:



We now need both equations to estimate the gain in test scores, since both *Time* and *Cash* change for school B. The increase in cash after 12 months splits the period into two parts and three contributions to test scores have to be estimated as follows:

Between t_0 and t_1: $\Delta Y_1 = (\beta_2 + \beta_3.c_0).(t_1 - t_0) = (1.168 - .068 \times 13.60).12 = 2.92$

At t_1: $\Delta Y_2 = (\beta_1 + \beta_3.t_1).(c_1 - c_0) = (2.258 - .068 \times 12) \times 6.10 = 8.80$

Between t_1 and t_2: $\Delta Y_3 = (\beta_2 + \beta_3.c_1).(t_2 - t_1) = (1.168 - .068 \times 19.70).12 = -2.06$

The resulting gain in test scores for school B is given by adding up the partial contributions:

$$\Delta Y = \Delta Y_1 + \Delta Y_2 + \Delta Y_3 = 2.92 + 8.80 - 2.06 = 9.66$$

School B is expected to have a mean test score in Portuguese that is 9.66 points higher than a school from the same group that has just started in *BF*, receiving, on average, R$ 13.60 per capita per family. Notice that the estimated gain in test scores due to the increase in per capita cash in Nov 2006 will have a "discount" over the next 12 months by an amount proportional to the second term in the programme, as shown by the negative value of ΔY_3. Nevertheless, the model still predicts a gain in test scores for school B that is 3.82 points higher than school A, suggesting that the mid-term variation in *Cash* contributes positively to test scores in school B. If the raise of cash for school B in November 2006 had achieved a value

231 Notice that because there was no change in the value of cash over the period we do not need equation 12, since there are no predicted gains due to variations in the per capita cash to be estimated.

232 In this example I am not considering the statistical significance of the expected difference in test scores, which will be assessed later in this subsection.

lower than the threshold of R$ 19.40, the estimated ΔY_3 would not have been statistically different from zero[233], meaning that the "discount" would not have applied to the gains in test scores due to the increase in cash[234]. On the other hand, if the change in the value of cash had occurred after school B had completed an average of 27 months of participation in *BF*, it would have a null contribution to gains in test scores in 2007, since that value of *Time* delimits the zone of statistically positive marginal effects of *Cash* (see Graph 31). This example illustrates how *BF* factors, through marginal effects, are associated with expected differences in test scores, also explaining how those factors interfere with each other by attenuating one another's effect.

Table 34 reports a set of finite estimated differences in test scores and level of statistical significance for changes in time of participation, evaluated at different levels of per capita cash[235]. The values of cash range from the minimum to the maximum figures found in the national sample of the fifth quintile group. As anticipated in Graph 32, for values of per capita cash higher than R$ 19.40 [for which the marginal effect of time is statistically significant (and negative)] the estimated differences in test scores are also found to be statistically significant. It can be seen from Table 34, for instance, that the difference in test scores ΔY_1 (=2.92) estimated in Ex. 2 (for cash=13.60 and Δtime=12) lies somewhere in the third row, between the minimum value and p5, and is not statistically significant[236]. Likewise, in the same example (Ex. 2), the estimated difference ΔY_3 (=-2.06) for cash=R$ 19.70 and Δtime=12 is close to the estimated value in the third row and fifth column of Table 34 (=-1.9) and is statistically significant.

Table 35 reports the estimated differences in test scores for increases in per capita cash by fractions of one s.d. (of the national sample) at different levels of *Time*, ranging from 0 to 48 months[237], and level of statistical significance. Taking, for instance, the estimated difference ΔY_2 (=8.80) from Ex. 2, it can be located in Table 35 somewhere in column five (time=12) between the two last rows, indicating a significance level of 1% for that estimate.

233 It is because the marginal effect of time is not statistically different from zero below that threshold, as shown in Graph 32.

234 Notice also that the estimated ΔY1 is also not statistically significant because the cash value for which it is estimated (R$ 13.60) has a positive (around 0.25) but not statistically significant marginal effect of time. Taking into account the level of statistical significance (5%) the more likely estimate for ΔY would be 6.74 (ΔY2 - |ΔY3|).

235 The estimates in Table 34 are calculated using Equation 11 in a Stata algorithm.

236 In fact, the estimated difference ΔY1 using the algorithm in Stata gives us a more accurate value of 3.03 and significant at the 10% level.

237 As shown in Table A - 34 the range of values for time of participation within the fifth quintile group varies between 9 and 48 months. However, I keep the out of range values of 0 and 6 months to see how changes in per capita cash would influence test scores when schools have less time in *BF*.

For values of *Time* above 27 months, the estimated differences in test scores due to increases in values of cash are no longer statistically significant (as predicted in Graph 31).

Table 34: Differences in test scores (Port) by per capita cash and variation in TIME (NATIONAL)

	CASH						
Δtime	**min**	**p5**	**p25**	**p50**	**p75**	**p95**	**max**
	11.50	**14.90**	**16.80**	**18.20**	**19.60**	**21.90**	**25.40**
6	2.3	1.0	0.2	-0.4	-0.9	-1.9	-3.3
	10%	-	-	-	5%	1%	1%
12	4.7	1.9	0.4	-0.7	-1.9	-3.7	-6.6
	10%	-	-	-	5%	1%	1%
18	7.0	2.9	0.6	-1.1	-2.8	-5.6	-9.9
	10%	-	-	-	5%	1%	1%
24	9.4	3.9	0.8	-1.5	-3.8	-7.5	-13.2
	10%	-	-	-	5%	1%	1%
30	11.7	4.8	1.0	-1.9	-4.7	-9.4	-16.5
	10%	-	-	-	5%	1%	1%
36	14.1	5.8	1.2	-2.2	-5.6	-11.2	-19.7
	10%	-	-	-	5%	1%	1%
42	16.4	6.8	1.4	-2.6	-6.6	-13.1	-23.0
	10%	-	-	-	5%	1%	1%
48	18.8	7.7	1.6	-3.0	-7.5	-15.0	-26.3
	10%	-	-	-	5%	1%	1%

Positive differences in test scores by increases in time of participation shown in Table 34 are only marginally significant and for the very lowest values of per capita cash observed in the sample[238]. One might conclude that time in *BF* is not important at all for children in this group of schools, but that is not the case (as we saw in Table 33, time of participation in *BF* is the factor contributing to reductions in dropout rates). The estimated null gain refers to how extra time in the programme would affect test scores, and this effect seems not to be relevant in the face of the influence of the cash transfer itself, unless cash values are at the top of the distribution. For high values of cash, time of participation plays a "discount" function, as shown in Ex. 2, attenuating potential gains in test scores due to increases in cash.

238 Even at extreme values the estimate cannot be taken with much confidence, since very few schools are situated in the neighbourhood of those values.

Table 35: Differences in test scores (Port) by time of participation and variation in CASH (NATIONAL)

		TIME								
Δcash/s.d.	**Δcash**	**0.00**	**6.00**	**12.00**	**18.00**	**24.00**	**30.00**	**36.00**	**42.00**	**48.00**
0.25	0.54	1.2	1.0	0.8	0.6	0.3	0.1	-0.1	-0.3	-0.5
		1%	1%	1%	1%	5%	-	-	-	10%
0.50	1.08	2.4	2.0	1.6	1.1	0.7	0.2	-0.2	-0.6	-1.1
		1%	1%	1%	1%	5%	-	-	-	10%
0.75	1.62	3.7	3.0	2.3	1.7	1.0	0.4	-0.3	-0.9	-1.6
		1%	1%	1%	1%	5%	-	-	-	10%
1.00	2.16	4.9	4.0	3.1	2.2	1.4	0.5	-0.4	-1.3	-2.1
		1%	1%	1%	1%	5%	-	-	-	10%
1.25	2.70	6.1	5.0	3.9	2.8	1.7	0.6	-0.5	-1.6	-2.7
		1%	1%	1%	1%	5%	-	-	-	10%
1.50	3.24	7.3	6.0	4.7	3.4	2.1	0.7	-0.6	-1.9	-3.2
		1%	1%	1%	1%	5%	-	-	-	10%
1.75	3.78	8.5	7.0	5.5	3.9	2.4	0.9	-0.7	-2.2	-3.7
		1%	1%	1%	1%	5%	-	-	-	10%
2.00	4.32	9.8	8.0	6.3	4.5	2.7	1.0	-0.8	-2.5	-4.3
		1%	1%	1%	1%	5%	-	-	-	10%
2.25	4.86	11.0	9.0	7.0	5.1	3.1	1.1	-0.9	-2.8	-4.8
		1%	1%	1%	1%	5%	-	-	-	10%
2.50	5.40	12.2	10.0	7.8	5.6	3.4	1.2	-0.9	-3.1	-5.3
		1%	1%	1%	1%	5%	-	-	-	10%
2.75	5.94	13.4	11.0	8.6	6.2	3.8	1.4	-1.0	-3.4	-5.9
		1%	1%	1%	1%	5%	-	-	-	10%
3.00	6.48	14.6	12.0	9.4	6.7	4.1	1.5	-1.1	-3.8	-6.4
		1%	1%	1%	1%	5%	-	-	-	10%

By reading through the estimates in Table 35, one observes that differences in test scores increase with increasing variations of per capita cash, but decrease given a particular Δcash with longer mean time of participation, becoming non-significant beyond an estimated threshold of 27 months for all values of Δcash. This trend suggests that potential gains in the mean test score, as a contribution of increases in cash transfer, are maximised for the shortest time of participation in the programme. In schools in which families have more than two years on average in *BF*, children have benefitted and accrued gains over that period and apparently will not improve further in their achievements in test scores by new increments in the level of the benefit. Considering the national s.d. in test scores for the group of schools analysed (14.5 points), the estimates in Table 35 show that for one s.d. increase, on average, in per capita cash (R$ 2.16) at the start of participation in *BF*, an expected gain of 0.34 s.d. (4.9 points) in test scores is estimated. This means, for instance, a move for a school from the median to the 62nd percentile or from the 39th percentile to the median. However, this estimated gain is 3.5 times lower if the school has a mean time of 24 months' participation in *BF*, achieving only 0.10 s.d. improvement in the mean test score.

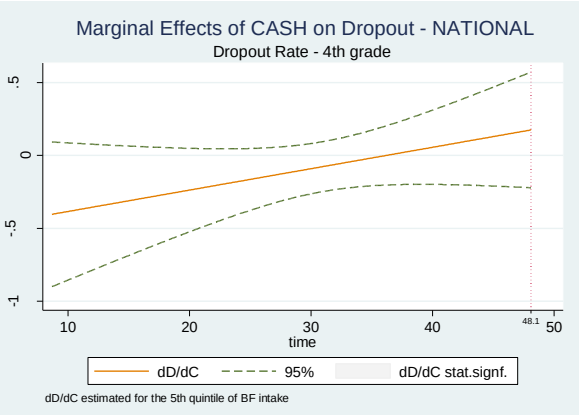
The estimates above suggest that any educational gain in terms of learning outcomes for the poorest group of schools in the sample derives mainly from the initial incentives dispensed by *BF*, materialised in the per capita cash value transferred to families. Higher values

of cash or increments in benefits delivered, on average, before two years of participation in *BF* is achieved, is likely to positively change expected results in test scores. Given the estimated timeframe of two years, any expected contribution of *BF* to children's achievement in test scores seems to depend on the initial level and eventual increases in cash value taking place over that initial period.

7.6.6.2 Dropout and Pass-grade rates

I now briefly turn to dropout and pass-grade rates to see how these outcomes are affected by *BF* factors, and how these effects vary across different values of *Cash* and *Time*. While testing the hypotheses in Table 31 (subsection 7.6.6) we saw that per capita cash appears as an influential factor in pass-grade rates, but time of participation did not. Also, we saw that the effect of cash does not depend on the variable *Time*. On the other hand, time of participation turned out to be an influential factor on dropout rates, whereas per capita cash did not, and that effect does not depend on cash either. I also estimated both national marginal effects for schools of the fifth quintile of *BF* intake (see Table 33), showing that for the mean values of cash and time (R$ 18.20 and 31 months), the expected marginal effect of *Time* on dropout was -.061 and the marginal effect of *Cash* on pass-grade rate was 0.570. A more complete picture of these marginal effects is shown in the graphs below (**Graph 33** and **Graph 34**), which plot the estimated marginal effects across the fifth quintile sample's range of values of *Time* and *Cash* as well as the 95% confidence interval, with shaded areas showing the region of statistical significance at the 5% level.

Graph 33: Marginal effect of CASH on DROPOUT rate

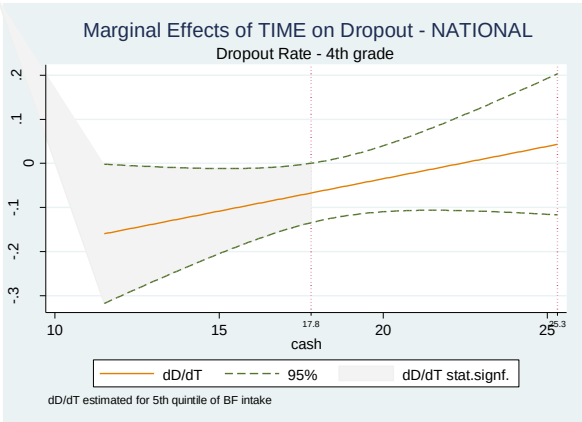


The marginal effect line for dropout rates in Graph 33 confirms that per capita cash is not significant across the range of values for Time, reflecting that amongst schools in the fifth

quintile of *BF* intake, differences in the mean cash value paid to families do not matter with respect to the school dropout rate. This does not imply, however, that cash itself is not relevant to keeping children in school and avoiding higher dropout rates[239]. This means that if there is a per capita cash threshold for which schools below or above that threshold would perceive significant differences in dropout rates, that threshold is not part of the range of values for per capita cash in the sample; therefore, such an effect cannot be detected. As the lowest mean per capita cash amount received in that group of schools is R$ 11.50 (see **Table A - 33**), one can say that above that cash level no reduction in dropout rates is expected as a result of increases in per capita cash in schools with more than 80% of students in *BF*.

According to the marginal effects estimates plotted in Graph 34**,** time of participation has an effect on dropout rates for values of per capita cash below R$ 17.80, with its marginal effect increasing (in absolute terms) as cash value decreases. Nevertheless, although the marginal line has an estimated positive slope, we cannot reject the null hypothesis (β_3=0 as shown in **Table 31**). In fact, one can see that a horizontal line, or even a line with a negative slope, could easily fit the confidence interval (dashed lines), replacing the one plotted in the graph. As such, although we can say with 95% confidence that *Time* has a negative marginal effect on dropout rates for values of cash below R$ 17.80, we cannot say that this marginal effect is statistically different across values of cash. The two extreme significant marginal effects are presented in **Table 36** with the confidence interval. We see that estimates here are very inaccurate, and any value ranging between -0.318 and a practical zero effect could be considered. The best guess, however, is in the value of cash found within the significant region

Graph 34: Marginal effect of TIME on DROPOUT rate



239 Because the analysis carried on here does not include comparisons with schools where *BF* is not operating, it is not possible to detect effects of cash transfers on dropout rates. It is only possible to detect whether different levels of cash would matter for those schools with a majority of students enrolled in *BF*.

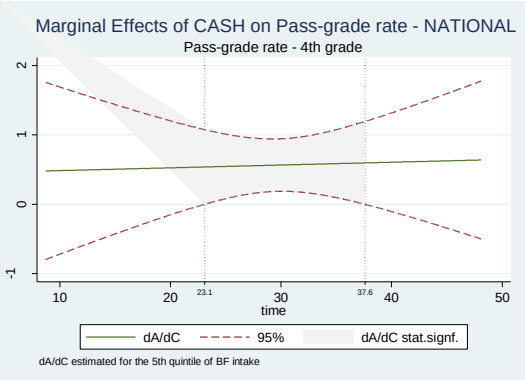
that is the closest to the mean (R$ 18.20), where the estimate is more accurate. This estimate is -.069 for cash equal to R$ 17.70, which is quite close to the estimate at the mean value (see Table 33). Accordingly, it can be said that for schools with per capita cash below R$ 17.80, one additional year in *BF* is associated with some 0.8 p.p. reduction in dropout rates. However, if families are, on average, receiving amounts above that threshold, time of participation does not seem to retain its relevance.

Table 36: Marginal effect of *Time* – confidence intervals

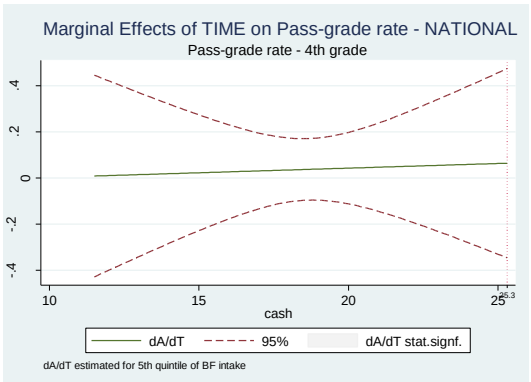
Per capita cash	Marginal effect of time	95% c.i.
R$ 11.50	-0.160	[-0.318, -.0017]
R$ 17.70	-.069	[-0.136, -.0007]

According to the marginal effect line plotted in **Graph 35** per capita cash has a positive effect on pass-grade rates, but statistical significance occurs only for schools where participation in *BF* is approximately between two and three years. The estimated effect at the mean value of time (31 months), as mentioned, is 0.570 and does not vary statistically across values of *Time*. This result estimates an increase by 0.6 p.p. in pass-grade rates for an R$1 increase in per capita cash. Time of participation has no effect on pass-grade rate, as shown in the test conducted in Table 31 (see **Graph 36**).

Graph 35: Marginal effect of CASH on PASS-GRADE rate



Graph 36: Marginal effect of TIME on PASS-GRADE rate



7.7 Conclusions

In this chapter I have shown evidence of positive contributions of *BF* to school outcomes by examining variations in per capita cash and time of participation in the programme. However, a downside of the educational context surrounding that policy is also revealed, since educational resources clearly deteriorate as the level of *BF* intake increases in schools[240]. This latter suggests the existence of what I have referred as a *regressive education system,* in which the more disadvantaged the socioeconomic context of families and children, the less public funding provided to schools. As a consequence, poorer children attend less well-resourced schools. School outcomes tend to deteriorate for schools with high *BF* intakes, and an estimated gap of one cognitive level on the national exam proficiency scale is found between the two extreme quintiles. In the same vein, dropout rates are three times higher and pass-grade rates are 9 p.p. lower for the fifth quintile of *BF* intake schools, compared to schools in the first quintile. At the same time, the correlation coefficients between school outcomes and *BF* household per capita incomes suggest that the level of poverty amongst *BF* families can make a difference for the aggregated result of the school, supporting the hypothesis that *BF* would have a positive effect if cash transfers were sufficient to counterbalance some of the negative effects of poverty level on school outcomes.

Based on the linear-interactive model tested, it was demonstrated how *BF* factors interact in predicting changes in school outcomes. The negative effect *BF* intake has on test scores, for instance, is reduced according to increases either in time of participation or in per capita cash paid to families. However, these two *BF* factors were found to have *substitute effects*. As cash value increases, the attenuation effect of time of participation on the negative influence of *BF* intake on test scores is reduced. On the other hand, in schools with high mean times of participation in *BF*, the attenuation effect of cash on the negative effect of *BF* intake on test scores also diminishes. Thresholds were also estimated, limiting those attenuation effects for *Cash* and *Time*. For values of per capita cash over R$ 24.80, increases in time of participation no longer have any expected positive influence in reducing the marginal negative effect of *BF* intake on test scores. In turn, for time of participation in *BF* beyond 37 months (around three years), increases in per capita cash ceases to have a diminishing effect on the negative marginal effect *BF* intake has on test scores. These thresholds were still out of reach for the large majority of schools in the sample. As consequence, most schools were still in the

240 The analysis of the distribution of beneficiaries in the school systems also showed that the majority of *BF* children attend schools with less that 60% *BF* intake, representing a relative advantage given that, as the proportion of beneficiaries increases, school conditions worsen in terms of resources.

process of improving their average test scores, either by families receiving higher per capita cash amounts or by accumulating more months in the programme.

The analysis of predicted test scores in Portuguese Language across a range of values for *BF* intake and *Time* [holding *Cash* fixed at the fifth quintile (R$ 12.20)] showed that positive and statistically significant differences are expected in test scores for schools with more than 45% beneficiaries when time of participation varies from one to four years in the programme. For instance, a positive gain in test scores of 10.6 points (0.62 s.d.) is estimated for schools in which all children take part in *BF*, when time of participation increases by three years. Likewise, a 14.5-point (0.85 s.d.) reduction is estimated in the achievement gap between schools in the two extremes of the *BF* intake distribution, when time of participation increases from one to four years. The analysis suggests that the maximum gain from time of participation occurs at the lowest levels of per capita cash and the maximum gain from per capita cash occurs at the lowest values of time in *BF*. This shows that the *BF time-effect* is a substitute for the *BF cash-effect*, although both *BF* factors positively contribute to mean school test scores.

The analysis focusing on the fifth quintile of *BF* intake reveals that both *Cash* and *Time* have significant marginal effects on school test scores in Portuguese Language but not in Mathematics at the national level. Pass-grade rates are improved by the amount of per capita cash paid to families, and dropout rates are reduced as long as time in the programme increases. In schools with a mean time of participation of 31 months, the model predicts a 0.6 percentage point increase in pass-grade rates for an R$1 increase in per capita cash. Likewise, an estimated 0.6 percentage point reduction in dropout rates is expected in schools with an average of R$18.20 per capita cash for each additional ten months that families remain in *BF*. Regionally, cash value appears to be more influential than time of participation with respect to pass-grade and dropout rates. “Positive” and significant marginal effects of cash are detected for the Northeast, Central-West, and Southeast regions.

As for test scores in Portuguese Language, for schools of the fifth quintile, the significant marginal effects of cash are estimated, ranging from 1.67 for 9 months in *BF* to 0.42 when time of participation reaches 27 months. Beyond 27 months, no significant effect of cash is predicted. As pointed out earlier, the maximum gain on test scores from cash increments would materialise if cash amount increases were to occur at the early stages of participation in *BF*. For instance, by increasing per capita cash by one s.d. (R$ 2.16) at the start of the programme, test scores are expected to improve by 0.34 s.d. (4.9 points). However, the gain will be 3.5 times lower if the school has a mean time in *BF* of 24 months, achieving only 0.10

s.d. improvement in the mean test score (1.4 points). In schools with more than two years on average in *BF*, children have benefitted and accrued gains over that period and will not improve further in their achievements in test scores through new increases in the level of the benefit. Time seems to be irrelevant in this group of schools for test scores and, in fact, has a diminishing effect over the gains yielded by increases in per capita cash for values above R$ 19.40. The overall conclusion concerning learning outcomes, as measured by the national exam, is that any educational gain for the poorest group in the sample derives mainly from the initial incentives dispensed to families. Higher values of cash or increments in benefits delivered before two years of participation in *BF* is achieved are likely to positively change the expected results in test scores. After that, the investment seems to be ineffective inasmuch as learning outcomes are concerned.

Can the positive effects of *BF* factors (*Time* and *Cash*) on school outcomes, as estimated in this chapter, be attributed to improvements in beneficiaries' performance at school? Moreover, can those improvements be seen as a direct effect of *BF* on beneficiaries' educational outcomes? The analysis in this chapter, in fact, does not estimate the *impact* of *BF* on beneficiaries' educational outcomes, since the estimated effects refer to school results measured based on the whole group of students (beneficiaries and non-beneficiaries). Nevertheless, this fact does not invalidate the conclusion that the estimated improvement in school results is ultimately due to the betterment of beneficiaries' performance at school as a consequence of participation in *BF*. Although I do not estimate the impact of *BF* on beneficiaries' outcomes, I estimate the improvement in school outcomes based on parameters of *BF* (*Time* and *Cash*) and attribute that improvement to a better performance by the group of students in schools to whom those parameters apply – the *BF* beneficiaries. The available data do not allow the estimation of the *size* of the improvement for beneficiaries, but it allows for inference about that improvement. This conclusion can be reached by reasoning about and excluding alternative explanations.

The first alternative explanation for the results presented in the empirical analysis is that the non-beneficiaries are the ones with improving results, but this should be systematic and associated with the two *BF* factors tested in the models (*Time* and *Cash*). This alternative explanation seems very unlikely, since the schools with higher values of *Time* and *Cash* are also the schools with the highest incidence of poor students and, as a consequence, these are schools where non-beneficiaries tend to be very similar to beneficiaries, thereby having a tendency to achieve worse instead of better results. Moreover, the analysis using the subsample of schools in the 5th quintile of *BF* intake demonstrated effects of *Time* and *Cash* on

school outcomes that could only be associated with beneficiaries' performance given the fact that, on average, in those schools 88% of students are beneficiaries and thereby determine the school results.

The second alternative explanation is that beneficiaries are improving relative to non-beneficiaries, but due to reasons other than participation in *BF*. This explanation requires that whatever other reason accounts for beneficiaries' improvement should also be correlated with the two *BF* parameters investigated (*Time* and *Cash*) and could not include any of the factors for which the models tested in this chapter control for. It points to alternative policies focused on the same group of *BF* participants and schools that are able to affect the same school outcomes examined. In addition to being correlated with the two *BF* parameters analysed (*Time* and *Cash*), such a hypothetical policy would need to have national coverage to systematically affect all schools across the country, and would need to operate such that its outcomes could be confused with the outcomes of *BF*. A policy with such characteristics, to the best of my knowledge, did not exist in 2007.

7.8 Limitations

In this chapter a cross-sectional multivariate model using an interactive hypothesis was estimated to gauge potential educational contributions at the school level derived from poor families' participation in *BF*. Despite the effort and rigour in applying those techniques, some caution in interpreting the stretch of the findings must be kept in mind. In dealing with policy evaluation, several problems can challenge the results. In the case of *BF* some issues have to be considered. In this section I comment on them.

First, the differences in school outcomes are estimated considering the entire group of pupils in each school, not only the beneficiary group, even when the sample is restricted to schools with more than 80% beneficiaries. This means that I have estimated what the literature of programme evaluation calls the "average treatment effect"; that is, I have estimated what is expected to happen, for instance, with the schools' mean in test scores, not with the beneficiaries' mean test scores in schools. Because the available data did not allow me to link test scores (or information on who passed or dropped out) with the pupils' status of participation in *BF*, the analysis in this chapter is restricted to the mean effect on the entire group of pupils. It can be concluded that the estimated effects would be even higher if evaluated only for the beneficiaries, in which case we would have the "treatment on the treated effect".

Second, specification issues can be raised in relation to the models tested in this chapter. The specified models do not allow for variations in the marginal effects of *BF* factors across different school and household characteristics as defined by the covariates. The models only allow for variations across *BF* factors. This gives the marginal effects an invariant status across different contexts that might not be true[241]. That said, the results are not invalidated, though caution is required in their interpretation. The results represent national (or regional) *average effects*, and as such are subject to variations once we move to more specific contexts. In addition, the estimated interactive model does not consider diminishing returns of per capita cash with household per capita income or with the very cash value paid to families. It is a well-established result in the economics literature that a one currency unit increase in per capita terms for a poor family will have a higher effect than the same unit transferred to a less poor family. This suggests that a non-linear model is generally a better fit than linear models when income is amongst the explanatory variables. A common form of dealing with the diminishing returns hypothesis, as stressed by Mayer (1997, p.66), is to estimate the effect of income using the natural logarithm of income as the independent variable. Therefore, a different specification for the models estimated in this chapter might include the logarithm form of the variable *Cash* (Ln (*Cash*)) and its interaction with household per capita income (Ln (*Cash*) x RFPC) in addition to the other terms. Likewise, it might also be considered that the variable time of participation (*Time*) would have a diminishing effect with household per capita income and with time in the programme itself. This hypothesis would lead to the inclusion of a log form for *Time* (Ln (*Time*)) and its interaction with household per capita income (Ln (*Time*) x RFPC) in the model. Despite all of this, the estimated model in this chapter showed a diminishing effect of per capita cash and time of participation through the interaction term (*Cash* x *Time*), which introduces a non-linear component into the model. The interaction term reflects the per capita gain over a period of participation in the programme and, as such, translates the increase in families' permanent income in per capita terms. The higher the increase in the per capita permanent income, the lower the effect of an additional *real* in per capita cash transfers on school outcomes. Therefore, the linear-interactive model[242] does capture diminishing effects of *Time* and *Cash* by using interaction terms in the specification.

241 For instance, by estimating the model by region, we see that some differences emerge as to the potential effects of *BF* factors. It might be the case that in allowing coefficients to vary across other household and school characteristics, we would see different pictures emerging.

242 The term "linear" here means linear in the parameters estimated, not in the explanatory variables; therefore, there is no contradiction with what is said above, that the interaction term (Cash x Time) introduces a "non-linear" component in the model.

Third, measurement errors in explanatory variables can create bias in the estimates and lead to incorrect conclusions. For instance, because *BF* was created by merging previously existing programmes and by migrating beneficiaries from the *Bolsa Escola* programme to *BF* over the implementation process, school composition in 2007 in terms of time of participation in *BF* may be inaccurate in some cases. For instance, 448 schools, representing 1.3% of the sample (35,172), had families completing, on average, more than 48 months in *BF* by January 2008[243]. In these schools, many families were migrated from *Bolsa Escola* to *Bolsa Família* between October and December 2003, while *BF* was operating under a Provisional Government Act[244]. Because the recorded date in *BF* was reset to zero when those families were migrated, their time in *BF* does not include the period in which they were participating in *Bolsa Escola*. The "lost" time can reach up to two years and three months, since the first payments under the previous programme began in June 2001. Thus, the variable *Time* for those schools has a measurement error that can cause some bias in the estimated coefficients. As *Bolsa Escola* was also a CCT programme for education, time spent in that programme might have also changed the "initial" conditions in *BF* for those families in ways that cannot be controlled for by looking only at the available variables in 2007. In other words, schools where families had taken part in *Bolsa Escola* might eventually have better results compared to those schools in which families started only in *BF*. The potential educational "gain" from the previous time in *Bolsa Escola* would be absorbed as an "initial condition," as if no intervention had taken place and, therefore, biased coefficients would lead to biased marginal effects of *BF*. Nevertheless, the number of schools affected is proportionally low (1.3% of the sample), and just one out of 1,295 schools from the fifth quintile group is included in that group. Therefore, this problem is essentially eliminated for the last group of schools, as is the risk of bias.

Finally, cross-sectional analysis can fail the assumption of equivalent groups when comparing schools by holding constant a set of variables, no matter how significant they might be as predictors of the outcome of interest. If the schools compared differ in some relevant characteristic that can affect the outcome of interest, and which is also correlated with the explanatory variables of interest (*BF* factors), then the estimated coefficients would be biased. The analysis developed in this chapter was based on an attempt to model the effects of *BF* factors deemed to influence school outcomes by controlling other determinants (household and school characteristics) that are simultaneously correlated with school outcomes and with the *BF* factors investigated. By using 2007 cross-sectional data, an implicit assumption is made

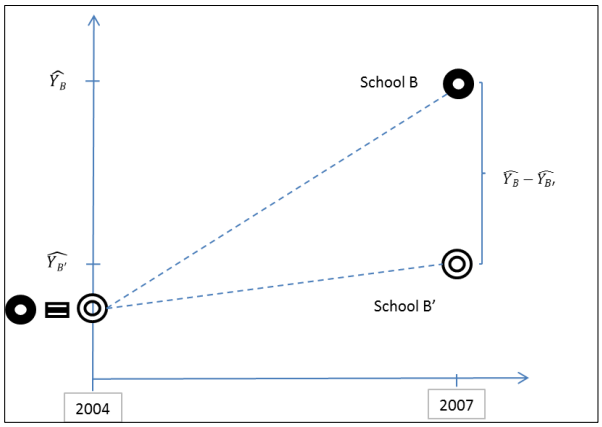
243 Mean time of participation varies between 48.1 and 51 months in the subsample.

244 The Bill creating *Bolsa Família* was passed in the National Congress in January 2004 (Law 10.836, January 9, 2004).

that after controlling for those variables schools are equalised in all possible characteristics that could explain different outcomes amongst schools with differences in *BF* parameters. Therefore, differences in school outcomes would be associated with (and explained by) variations in those parameters.

For instance, suppose that two schools, B and B', are considered equal in all measured aspects, including *BF* intake, but differ in the other two *BF* parameters (*Time* and *Cash*). As represented in **Figure 10**, this assumption is equivalent to comparing the two schools in 2007 as if they had come from the same initial conditions in 2004, when *BF* was created, and had followed different trajectories between 2004 and 2007, depending only on the two characteristics associated with *BF* (the average time families had been in *BF* since 2004 and the average per capita cash received during that period). As the schools were considered equivalent in all possible characteristics explaining test scores that were also correlated with *BF* factors, they would have the same average test score in 2004 had the national exam taken place that year. Under this assumption, the estimated difference in the mean test scores between the two schools in 2007 is thus taken as the "true" effect of *BF* factors on test scores (Y).

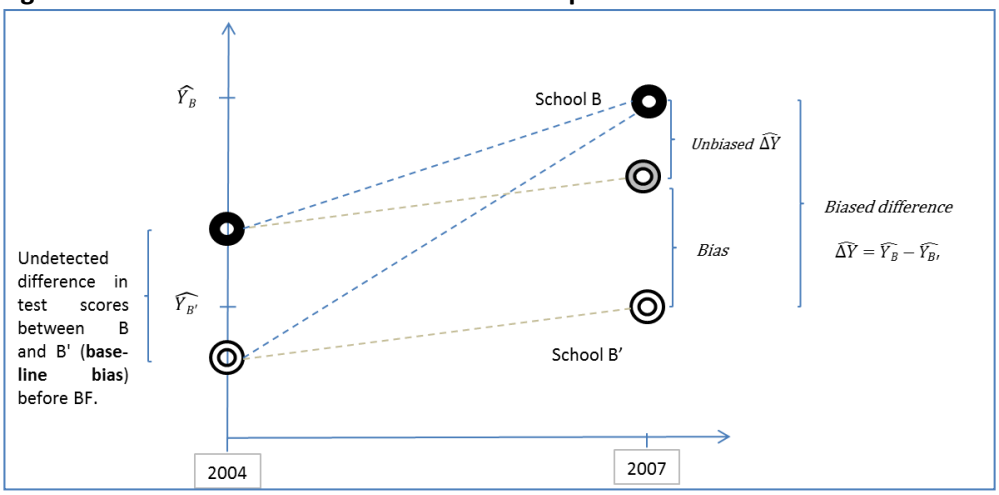
Figure 10: Assumption behind cross-sectional comparison.



However, that assumption might be wrong if, for instance, some unknown, unsuspected, or unobservable characteristic of schools or households highly correlated with test scores and *BF* factors was, in fact, creating a difference in test scores in 2004 that remained undetected. In this case, as illustrated in **Figure 11,** school B' is no longer a good counterfactual model for school B; that is, school B' cannot represent the state of school B in 2004. As a consequence, the estimated difference in test scores ($\widehat{\Delta Y}$) in 2007 based on the previous assumption is biased. In order to correct for that bias it would be necessary to know

the difference in test scores in 2004 between school B and B' (the "baseline bias"), and to discount that difference from the estimated difference in 2007 through a procedure known as *difference-in-differences* or *double-difference* estimation.

Figure 11: Potential bias in cross-sectional comparison.



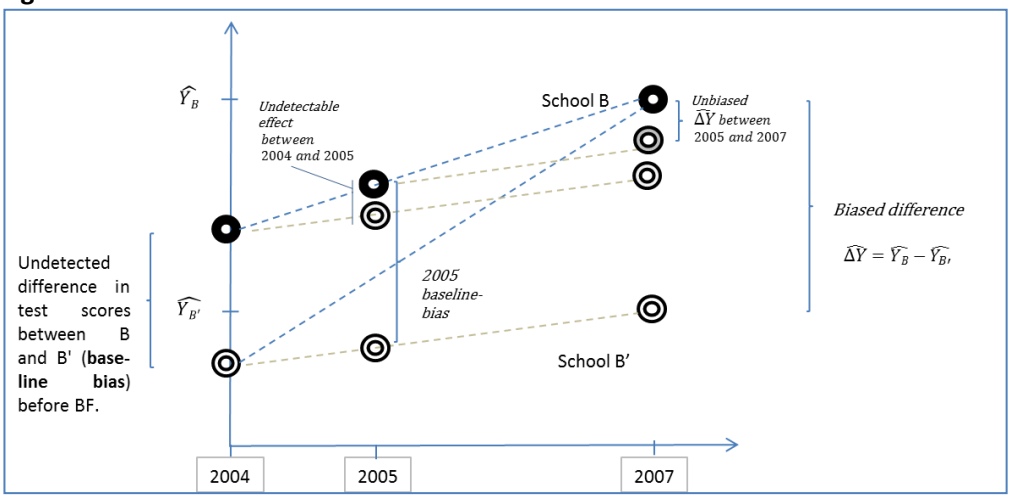
The problem in implementing that approach with *BF* is that there is no baseline, either for 2004 or for any year before that. There was no national exam in place that could provide measures of learning outcomes significant at the school level before 2005, when *BF* was already in place for two years. Therefore, I cannot, for instance, match schools' performances in test scores and school composition (student and household characteristics)[245] prior to the implementation of CCTs in Brazil to look at differences achieved in test scores over time according to variations in *BF* factors. Characteristics of individuals, households, and schools prior to the implementation of targeted CCT programmes in Brazil can be a source of bias and cannot be fully captured by regression analysis using cross-section data, which basically assess differences at one point in time controlling for a set of observable characteristics. Even controlling for many of the factors that could affect both school outcomes and participation in the programme in terms of the mean values of time and cash and the proportion of beneficiaries in schools, the possibility that other unobservable variables would be operating and interfering with observed outcomes before the implementation of CCT programmes cannot be disregarded.

An option is to look for an alternative post-programme baseline and then try to apply the double difference estimation relative to that baseline. For instance, using *Prova Brasil* 2005 and 2007, it is possible to create a two-period panel data to analyse differences in test scores

245 These variables come from the socioeconomic questionnaire applied in Prova Brasil as of 2005.

associated with the level of *BF* participation, although the 2005 to 2007 interval does not cover the before-after programme period. As described in **Figure 12**, a baseline for school outcomes in 2005 will allow us to proceed with a difference-in-differences technique and to estimate ΔY due to *BF* intake in 2007, but it will be an effect relative to 2005[246].

Figure 12: Double difference estimation



It must be noted that the baseline bias in 2005 might carry an undetected effect of *BF* factors between 2004 and 2005 and, therefore, the "true" *BF* effect in 2007 would be discounted as a result of the unknown level of baseline bias before *BF* had been implemented in 2004. In addition, the cohort of fourth grade students in the two national exams is different, which might raise questions about any assumption of invariant school composition between the two years. Nevertheless, the two years of panel data will allow us to yield a more robust analysis of the potential effects of *BF* on school outcomes than allowed by the cross-sectional analysis developed in this chapter. This is the approach proposed in the next chapter.

246 This approach was used in the research commissioned by the Ministry of Social Development in 2005, which assessed several *Bolsa Família* impacts by creating a post programme base-line to be used as a reference in a panel studies ((MDS, 2007))

Chapter 8. Panel data analysis (2005-2007) of *Bolsa Família* contribution to school outcomes

8.1 Introduction

In the previous chapter I explored differences in school characteristics and outcomes showing that schools with high *BF* intake, on average, perform worse than schools with low proportions of *BF* beneficiaries. That educational gap mirrors the structural inequalities between schools with high and low numbers of poor children. However, the idea that CCTs can increase long-term human capital accumulation amongst children of poor families raises the hypothesis that this gap must be reducing over time. Using cross-sectional analysis, it was shown that the length of time of participation and the value of cash transferred to families are significant factors explaining improvements in school outcomes. However, the cross-sectional analysis holds an implicit assumption: after controlling for the set of characteristics included in the model, schools are equalised in all possible aspects that could explain different outcomes between schools with different *BF* parameters, such as time of participation and cash value.

In this chapter, a step forward is made in modelling and controlling school characteristics that might interfere with the estimated *BF* effects on school outcomes. A two-year panel (2005-2007) is used in a *school-and-time fixed effects model* to estimate the marginal effects of *BF* intake over time. The idea behind the proposed model is rather simple: if schools are equivalent in the relevant characteristics that might explain different outcomes between 2005 and 2007, then differences observed in the marginal effect of *BF* intake on those outcomes are the result of participation in *BF*. That is, those differences can be attributed to the programme's effects on school outcomes.

The caveat is the fact that the two interactive key factors associated with *BF* participation at the school level used in the previous chapter (mean time of participation and per capita cash value for each family) are not available for 2005. Therefore, effects associated with different combinations of those *BF* factors cannot be estimated in the panel. However, the relevance of time as a key variable in assessing *BF* effects on school outcomes can be investigated, since the panel analysis relies on comparisons of the same schools across time. By including an interaction term with the variable measuring *BF* intake and a dummy for year, the model proposed in this chapter can capture changes of the *BF*-intake effect on school performance over time. Those changes, I argue, are another way of providing evidence of *BF*'s contribution to school outcomes.

In the next section, I briefly describe the data structure and the problems found in gathering relevant variables in a sample of schools to form the panel (section 8.2). Then I describe how school composition, resources, and outcomes changed between 2005 and 2007, as well as their composition in terms of *BF* intake (section 8.3). This is followed by a presentation of the *school-and-time fixed effects model* (section 8.4) and estimated results (section 8.5). Finally, conclusions and limitations are discussed in section 8.6.

8.2 Panel Data of Schools

The data structure used in this chapter follows what has been defined as a *panel data structure*. The distinctive character of *panel* (or *longitudinal*) data is its construction, which involves at least two observations of the same units of analysis on the same set of variables at different points in time (White, Payne and Lakey, 2000; Wooldridge, 2009). It differs from the *cross-sectional* data structure used in the previous chapters, since the latter comprises measures of a set of variables for different individuals or units of observation at one point in time[247]. Several authors emphasise that even when observations are collected at different points in time, the data structure can still depart from a panel type (Finkel, 1995; White, Payne and Lakey, 2000; Menard, 2002). This is the case in the *repeated-cross-sectional* data structure, in which the same variables are measured at different points in time over possibly different units of observation, although the sampling procedure to select the units each time the data collection takes place can ensure comparability over time[248]. This is also the case with *time-series* data and *aggregate time-series* (or *pooled cross-sectional* data). In the former, data is collected for one or several variables for the same unit at several points in time (e.g. expenditure per pupil for a particular country in several years), while in the latter structure one simply aggregates data on a set of common variables for units (not necessarily the same) at different points in time (e.g. mean test scores by school over several years). The data structures encompassing time variation across observations, including those without any necessity of having the same units of observation, are generally referred to as *longitudinal data.* Some authors will distinguish these data structures from the *panel data* type (Finkel, 1995; Menard, 2002), regarding the latter as a special structure of longitudinal data in which

247 In fact, the synchronic aspect of cross-sectional data in many practical situations is approximated in that the collected data for different sets of variables may take place at different moments, although the time link can still be established across units for the same year. This is the case, for instance, for the data collected in 2007 by the National School Census (31st of March), the Prova Brasil dataset (November) and the *Bolsa Família* (October/November).

248 This is the case, for instance, of the National Household Sample Survey (PNAD) that takes place in Brazil every year. The national sample each year keeps its national and regional representativeness and allows comparisons of demographic and socioeconomic variables across time, although the families in the sample can actually be different from one survey to the other.

there is no room for different units of observation. White, Payne and Lakey (2000) also distinguish another special case of longitudinal data: *cohort data*. The authors define cohort data as a sample of "units that pass through a defining event at about the same time, and are then followed over time" (White, Payne and Lakey, 2000, p.9). Therefore, cohort data can be thought of as a panel data with a common link between units characterised by a particular occurrence at the time data is collected. As described next, the sample of schools used in this chapter can be characterised as a panel of the last type – that is, a cohort of schools whose common link is participation in the first national exam in 2005.

The *panel of schools* with fourth grade pupils for 2005 and 2007 is derived from the administrative records of the standardised national tests in Portuguese Language and Mathematics (*Prova Brasil*). The panel also includes the socioeconomic data of the students and their families collected on the day of the exam. In addition to school outcomes and school composition data retrieved from *Prova Brasil* datasets, information on school resources for those years, derived from the National School Census, is also included in the panel data. Finally, the number of beneficiaries in each school is introduced from two different sources. For 2005, beneficiaries by school are identified based on one item in the socioeconomic questionnaire applied during the national exam in that year, which asks exam-takers whether they were beneficiaries of *BF*. For 2007, beneficiaries by school are identified based on the records of *BF* for school attendance, which provides the number of beneficiaries in each school in November 2007 (the same month of the application of *Prova Brasil* 2007).

The original databases required massive data management to allow merging and organisation in a panel format. Particularly relevant is the management of missing values for the number of beneficiaries by school in 2005, given that many pupils did not reply to the specific question on participation in *BF*. Although the large size of the *Prova Brasil* 2005 dataset (around 1.9 million pupils) allowed the *listwise* deletion performed in chapter six without compromising a random sample for that analysis, the same process could not be used here. The process of aggregating data by school to organise the panel required a missing value analysis at the school level. Given its extension, the analysis and discussion of the procedures adopted to work out a solution for missing values is included in the appendix (A) of this chapter.

As described in the previous chapter, the number of schools with official results on the national exam falls short of the total number of schools with fourth grade pupils. This is partially due to the scope of the exam, which covers only public urban schools with a minimum of 20 pupils enrolled in the assessed grade in 2007 (30 in 2005); partially due to the rules

required to yield statistically valid samples at the school level; and partially due to the voluntary nature of the exam for subnational governments. Besides, for each year, some schools were excluded from the sample before being merged into the panel due either to a high proportion of missing values in key variables for the analysis or due to inconsistencies in measurements detected by cross-examination using different sources of data. The consolidated set of schools for each year was then united in a balanced panel, with 23,747 schools as described in **Table 37**.

Table 37: Number of schools in the panel data

Year	Schools with 4th grade (Regular Fundamental Education)	**Public** Schools with 4th grade (Regular Fundamental Education)	**Urban Public** Schools with 4th grade (Regular Fundamental Education)	Urban Public Schools with official results in *Prova Brasil*	Urban Public Schools after consolidation[1, 2]	Urban Public Schools in the panel data 2005/2007
2005	134,694	118,292	40,361	27,951	26,239	23,747
2007	123,382	109,103	39,922	37,262	35,172	

1) For 2005: exclusion of 1,596 schools with 50% or more missing values on the variable identifying participation in *Bolsa Família* (Q44); exclusion of 72 schools from state of *Rio Grande do Norte* (RN); exclusion of 35 schools with missing data for school outcomes; exclusion of 9 federal schools.

2) For 2007: exclusion of 2,064 schools with measurement error in the number of fourth graders based on the National School Census checked by cross-examination with data from *Bolsa Família* programme and from *Prova Brasil*; exclusion of 18 federal schools and 8 schools with missing data for school outcomes.

Some real and potential limitations of the sample of schools used in the panel data analysis are worth mentioning. Although it is a large sample of schools, the existing restrictions in the application of *Prova Brasil* in 2005 and 2007 limit the scope of schools to the urban units with more than a certain number of pupils enrolled in the examined grades[249]. This significantly reduces the number of schools assessed by *Prova Brasil*, as many schools are small with few pupils enrolled in the fourth grade. Nevertheless, the number of pupils taking part in the exam is large, since those schools not included in the exam tend to be multi-grade schools located in rural areas, generally with one or two classrooms with less than 20 pupils, and only one teacher who is also in charge of administrative tasks. Because the sample does not cover rural areas or very small urban schools, it is not representative of groups of rural children, multi-grade classrooms, multi-grade schools, and schools with only one teacher. These exclusions bring up an issue raised by White, Payne and Lakey (2000, p.6), that "nationally representative samples are not necessarily representative of sub-groups" in society. Bearing in mind these limitations, the forthcoming analysis is expected to shed some light on how school outcomes changed between 2005 and 2007, and on whether *BF* played a role in such changes.

249 At least 30 pupils in 2005 and 20 pupils in 2007 were expected to attend the fourth grade of fundamental education for a school to take part in Prova Brasil.

8.3 School outcomes, school composition, and school resources (2005-2007)

The set of variables grouped in the panel, their mean values, and s.d. by year is described in **Table 38**.

Table 38: School outcomes, school composition and school resources 2005-2007

VARIABLES	2005		2007	
School Outcomes	**Mean**	**s.d.**	**Mean**	**s.d**
Mean Test Score PORT	172.1	17.3	172.3	17.8
Mean Test Score MATHS	179.7	17.5	189.8	19.7
Pass-grade rate	84.8	12.6	87.4	11.4
Dropout rate	4.3	7.1	2.8	5.1
School Composition				
Students' Characteristics				
Prop. of boys	0.51	0.08	0.50	0.09
Prop. of non-whites	0.65	0.17	0.66	0.17
Prop. over-age	0.23	0.16	0.23	0.16
Prop. who have failed any grade	0.33	0.15	0.34	0.16
Prop. who attended pre-school	0.74	0.14	0.73	0.14
Prop. in child labour	0.16	0.08	0.15	0.08
Households' Characteristics				
goodsindx (durable goods indice)	0.52	0.11	0.54	0.12
Prop. whose mother has post-primary education	0.29	0.15	0.30	0.16
Prop. whose father has post-primary education	0.30	0.16	0.32	0.17
Prop. living in female headed households	0.20	0.08	0.24	0.11
Prop. living in large families (7 or more)	0.25	0.12	0.24	0.12
Prop. in *Bolsa Familia*	0.35	0.15	0.38	0.21
Prop.students in *Prova Brasil*	0.82	0.14	0.84	0.13
School Resources				
Facilities index	0.44	0.17	0.47	0.17
Equipment Index	0.60	0.23	0.63	0.21
Utilities index	0.86	0.17	0.87	0.16
Computer room available	0.28	-	0.40	-
Laboratory for Sciences available	0.11	-	0.11	-
Library/Reding room available	0.63	-	0.66	-
Class Size (1st-4th)	28.4	5.3	26.5	4.9
Prop. of teachers with higher education (1st-4th)	0.54	0.34	0.66	0.30
Number of Schools	23,747		23,747	

By looking at the mean values of the variables in Table 38 one can see that all school outcomes improved between 2005 and 2007, except for test scores in Portuguese. On the other hand, school composition essentially remained the same in the sample of schools, although a small increase is observed in the durable goods index, the proportion of fathers with post-primary education, and the proportion of children living in female-headed households. The average proportion of beneficiaries in *BF* increased as expected[250], as did the proportion of pupils taking part in *Prova Brasil*. School resources clearly improved between those years, including the proportion of teachers with higher education and school infrastructure (notably IT facilities), not to mention the average class size, which also

250 In 2007, the estimated number of poor families (11.2 million) was covered by the programme.

diminished by around two pupils per classroom. In the next subsections I look at the changes that occurred between 2005 and 2007 that are conditional on the proportion of *BF* beneficiaries by school.

8.3.1 School outcomes across years

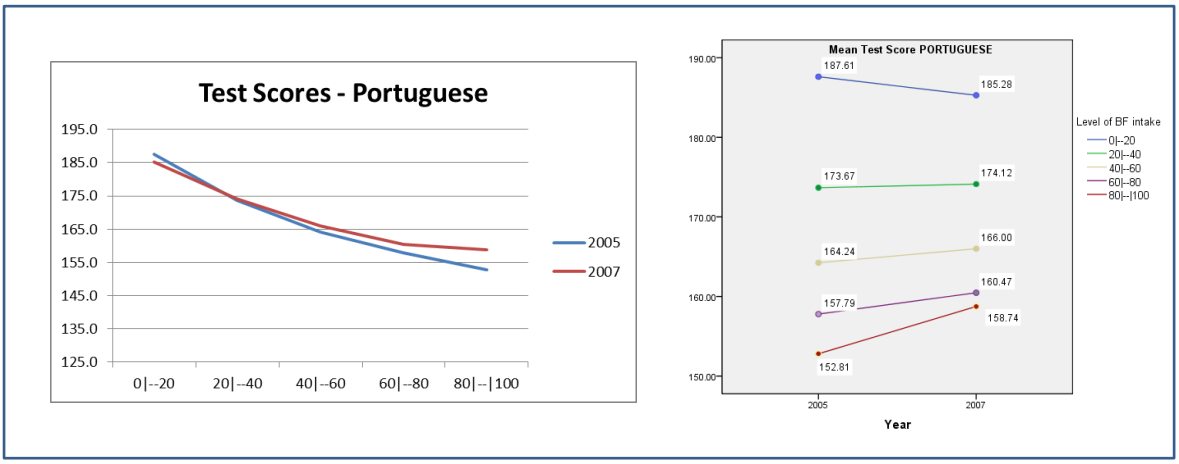
Using 23,747 schools in the two years' worth of panel data, **Table 39** reports mean values of school outcomes by level of *BF* intake for the fourth grade in 2005 and 2007, the mean differences between the two years and the gap between the first and fifth quintiles. In Portuguese Language, although no significant overall difference is observed between 2005 and 2007, the mean test score increased for all groups except for the first quintile of *BF* intake. There is also a positive increasing difference as we move from the second to the fifth quintile, the latter improving 12 times more than the former. In Mathematics, an overall improvement is observed in the mean test score of around 10 points in the proficiency scale, as well as a similar pattern of increasing gains across the quintiles, although at a more moderate rate. Increasing improvements across groups of schools according to *BF* intake can also be noticed for the pass-grade and dropout rates between the two years. An increase of 7 p.p. in pass-grade rates and a reduction of 3 p.p. in dropout rates are observed for the fifth quintile, these figures being seven times higher than observed for the first quintile. These figures show that schools with higher proportions of beneficiaries saw greater improvements between those years compared to those with lower proportions of *BF* participation — a trend illustrated in **Graphs 39 to 42**.

Table 39: School outcomes by level of *BF* intake across years – 4th grade

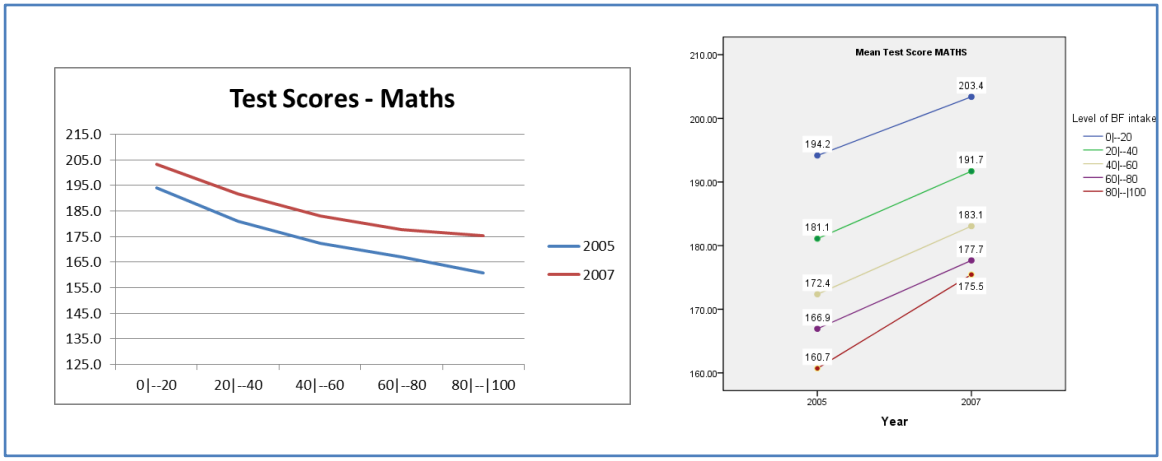
Level of BF intake	Observations		Test Score PORT		Δ	Signif.(1)	Test Score MATHS		Δ	Signif.(1)
	2005	2007	2005 Mean	2007 Mean			2005 Mean	2007 Mean		
0|--20	4,044	5,442	187.6	185.3	-2.3	**	194.2	203.4	9.2	**
20|--40	10,729	8,274	173.7	174.1	.5	*	181.1	191.7	10.6	**
40|--60	7,600	6,288	164.2	166.0	1.8	**	172.4	183.1	10.7	**
60|--80	1,320	2,879	157.8	160.5	2.7	**	166.9	177.7	10.7	**
80|--|100	54	864	152.8	158.7	5.9	**	160.7	175.5	14.7	**
Total	23,747	23,747	172.1	172.3	.2	-	179.7	189.8	10.1	**
Inter-quantile gap (1st-5th)			34.8	26.5	-8.3	**	33.4	27.9	-5.5	**
Level of BF intake	Observations		Pass-grade rate		Δ	Signif.(1)	Dropout rate		Δ	Signif.(1)
	2005	2007	2005 Mean	2007 Mean			2005 Mean	2007 Mean		
0|--20	4,044	5,442	90.7	91.5	.9	**	1.7	1.3	-.4	**
20|--40	10,729	8,274	85.8	88.2	2.4	**	3.7	2.5	-1.3	**
40|--60	7,600	6,288	81.5	85.2	3.7	**	6.0	3.6	-2.4	**
60|--80	1,320	2,879	78.6	83.6	5.0	**	7.4	4.1	-3.3	**
80|--|100	54	864	75.6	82.6	7.1	**	7.3	4.3	-3.0	**
Total	23,747	23,747	84.8	87.4	2.6	**	4.3	2.8	-1.6	**
Inter-quantile gap (1st-5th)			15.1	8.9	-6.2	**	-5.6	-3.0	2.6	**

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

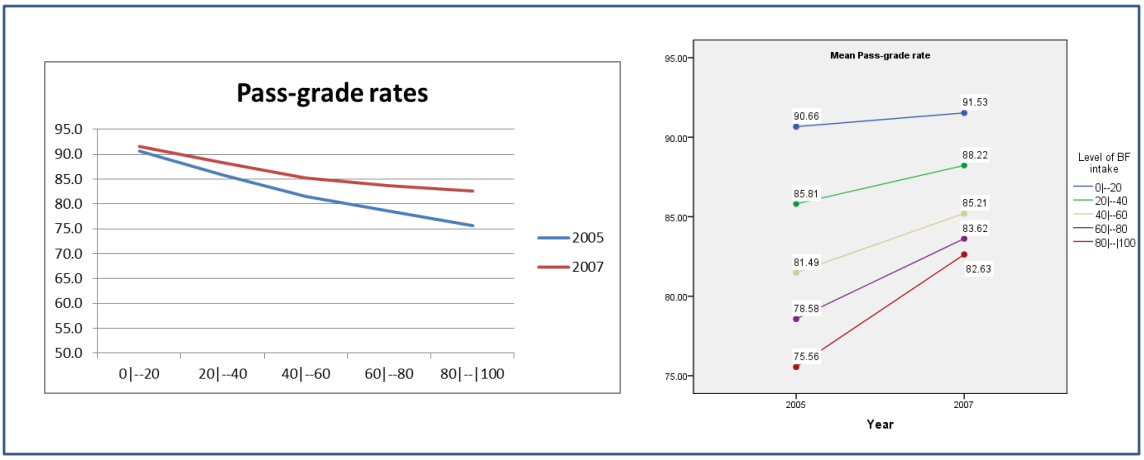
Graph 37: Mean Test Score in PORTUGUESE 2005-2007



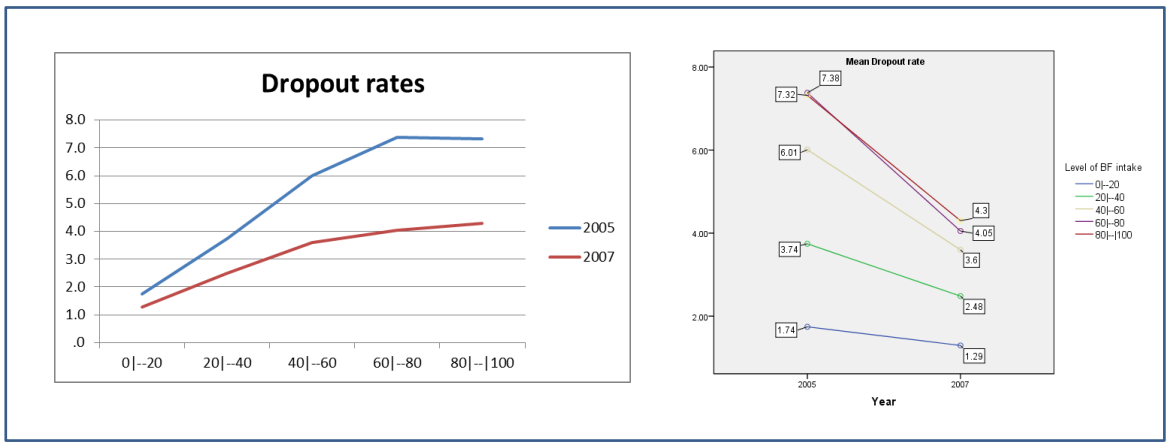
Graph 38: Mean Test Score in MATHS 2005-2007



Graph 39: Mean Pass-grade rate 2005-2007



Graph 40: Mean Dropout rate 2005-2007



The gap between schools in the first and fifth quintiles of *BF* intake narrowed between 2005 and 2007; that is, a reduction in inequality of outcomes between schools primarily serving beneficiary children and their counterparts took place over the two years. The last row of Table 39 shows the figures for the gap reduction in p.p. between the first and fifth quintiles. The gap remains large for test scores in Portuguese Language and Mathematics (around one cognitive level) in 2007, but smaller by 8.3 points in the first case and by 5.5 points in the second case. The gap reduction is also remarkable for pass-grade and dropout rates, reaching 6.2 p.p. and 2.6 p.p. respectively. These gap reductions are good signals and allow us to raise the question of whether *BF* might play a part in helping that progress.

8.3.2 School composition and school resources across years

School outcomes clearly improved between 2005 and 2007, and did so increasingly for schools with higher shares of *BF* beneficiaries. This is reflected in the inter-quintile gap reduction shown in Table 39. An immediate question is what could explain those improvements. Why did schools with higher proportions of beneficiaries improve more than those with lower shares of *BF* intake? Is it an associated effect of *BF* or something else? Looking at changes in school composition and school resources across those years, one can find some plausible explanations for the decreasing inter-quintile gap in school outcomes.

As to school composition (student and household characteristics), basically all the influential factors on school outcomes[251] changed in favour of schools in the top quintiles of *BF* intake. As shown in **Table A - 45**, the inter-quintile gap in characteristics such as the proportions of

251 See Table A - 29 in the previous chapter for the estimated coefficients of school outcomes on school composition and school resources using cross-sectional data for 2007.

overage children, pre-school participation, and grade failure in previous years decreased significantly by 8 p.p., 5 p.p., and 4 p.p. respectively between 2005 and 2007. Although with smaller variations, the inter-quintile gap in child labour also decreased by 0.2 p.p. and demographic characteristics such as gender and race (also correlated with school performance) changed in favour of better outcomes for the last quintile of *BF* intake. Much stronger changes are observed in household characteristics between 2005 and 2007, as shown in **Table A - 46**. The inter-quintile gaps in parents' education dropped by 10 p.p. for mothers and 7 p.p. for fathers. The proportion of children living in large families also saw a significant inter-quintile gap reduction of 6 p.p. and the standard of living of those children in schools with high *BF* intakes improved, as measured by the index for durable goods at home, yielding a 5 p.p. reduction in the gap between the first and the fifth quintiles of *BF* intake. The proportion of female-headed households increased in all groups of schools by basically the same amount (4 p.p.), therefore, very little inter-quintile change is observed. These results indicate that although school composition in 2007 (as already shown in the previous chapter) constituted a more disadvantaged environment in schools with high *BF* intakes, that disadvantage diminished between 2005 and 2007. This can partially explain the different patterns of improvements in school outcomes between those years according to the level of *BF* intake.

The different cohorts of fourth grade students in 2005 and 2007 may explain differences in the levels of socioeconomic disadvantage across schools observed between those years (even in schools with high levels of *BF* intake). As we saw in the previous chapter, beneficiary households' per capita incomes in 2007 were negatively correlated with length of time of participation in *BF*, which means that the poorest families probably had priority in registration for the programme. As a consequence, schools in 2005 with high proportions of beneficiaries were probably enrolling, on average, a poorer group of fourth grade pupils than in 2007. For instance, fourth grade children sitting for the exam in November 2005 might have been included in *BF* only over the previous two years, while attending either the third or fourth grades[252]. On the other hand, fourth grade children in 2007 might have been included after 2005 while also attending the third and fourth grades. The latter third and fourth grade group is likely to be better off than the former, since they were included in the programme later. As we look at more recent national exams for the fourth grade, chances are that the school composition in schools with high *BF* intake is less disadvantaged than that of schools represented in previous exams, which could explain the better school outcomes.

252 It is also possible that children included in *BF* between October and December 2003 were attending the second grade that year.

School resources in terms of infrastructure, teachers' education and class size also changed between 2005 and 2007 across levels of *BF* intake. A trend towards a less unequal educational system finds support in **Table A - 47** and **Table A - 48.** Although schools with high *BF* intakes are less well-resourced than those in the first quintiles in both years, the inter-quintile gap (between first and fifth quintiles) is reduced by significant levels in all but one of the eight school factors analysed between 2005 and 2007. For instance, gaps in the equipment and facilities indices drop by 16 p.p. and 7 p.p., respectively[253]. The existing inter-quintile gap in the proportion of schools with computer rooms, science laboratories, and libraries (or reading rooms) is reduced by 10 p.p., 6 p.p., and 12 p.p. respectively. The inter-quintile gap in class size (for first to fourth grades) changed from an average of one pupil more in the fifth quintile to one pupil less, and the proportion of teachers with higher education (working in the first to fourth grades) increased by 14 p.p. in the fifth quintile, reducing the inter-quintile gap by 5 p.p. These gap reductions tell us a more optimistic story about the distribution of school resources between social groups, as represented by the share of *BF* beneficiaries in each school. As argued in the previous chapter, a *regressive education system* can be identified in 2007 but, when compared to 2005 indicators, a trend towards a less unequal education system cannot be disregarded. Since those school factors are good predictors of school outcomes[254], they may also explain the inter-quintile gap reduction in school outcomes between 2005 and 2007.

The less unequal distribution of school resources amongst schools according to the level of *BF* intake might be the result of a more targeted and deliberate education policy for disadvantaged schools. Although this might be the case in some instances[255], it is also possible that better resourced schools in 2005 received more beneficiaries (or had more of their pupils registered as beneficiaries) in 2007, considering that *BF* expanded between those years. If that were the case, then those schools would migrate to a higher *BF* intake group along with their already better school inputs. It is also possible that less well-resourced schools with high *BF* intakes in 2005 may have had fewer beneficiaries in 2007, either because they took more non-participant enrolments, or because they lost *BF* pupils between those years. In this case, they

253 The utilities index is the only school factor for which the inter-quintile gap increases between 2005 and 2007. However, this is because that index improved for all groups of schools and remained invariant for the fifth quintile, thereby enlarging the gap by 3 p.p.

254 See footnote 251.

255 One cannot disregard the decentralised character of the Brazilian education system, in which state and municipal policies play a definitive role in shaping the patterns of educational inequality observed in the country, but are also in position to promote changes in those patterns. In addition, national policies implemented by the Ministry of Education, although supplementary in character, play an important role in overcoming inequalities between regions.

would migrate to lower *BF* intake groups and contribute to changes in the allocation of school resources between levels of *BF* intake. In the next subsection I look at the transitions schools may have undergone across *BF* intake groups between 2005 and 2007.

8.3.3 Transitions across *BF* intake groups between 2005 and 2007

Table 40 reports the transitions of schools across levels of fourth grade *BF* intake between 2005 and 2007. For instance, the first row shows that amongst schools in the first quintile in 2005, the large majority (62.7%) remained in that group in 2007, 27.7% migrated to the second quintile, 6.8% to the third, and so on. The table also shows the total share of schools in each quintile that migrated downwards, remained in the same group, or migrated upwards between 2005 and 2007. The figures reveal several insights about the transitions between the two years. School composition in terms of *BF* intake changed significantly between 2005 and 2007. With the exception of the first quintile, all groups had more than 50% of schools migrating to another level of *BF* intake. That migration did not always occur towards high *BF* intake groups, as one might presume given that between 2005 and 2007 *BF* was in the process of including more families to achieve the target number of 11.2 million families estimated to be living in poverty. Although a large majority of schools in the first three quintiles in 2005 either remained in the same group or migrated to higher *BF* intake groups, it can be observed that large shares of schools migrated downwards amongst the last two quintiles. The proportion of schools remaining in the same group across years diminishes for high levels of *BF* intake, as can be seen by the figures for the percentage not migrating[256]. The transitions shown in Table 40 could explain the inter-quintile gap reduction in school resources described in the previous subsection.

Table 40: Transition table for 4th grade *BF* intake

Level of BF intake 2005 (%)	Level of BF intake 2007 (%)					% migrating downwards	% not migrating	% migrating upwards	Total
	0-20	20-40	40-60	60-80	80-100				
0-20	**62.7**	27.7	6.8	2.4	0.5	0.0	62.7	37.3	100.0
20-40	22.0	**43.5**	23.8	8.4	2.3	22.0	43.5	34.5	100.0
40-60	6.1	29.7	**38.8**	19.5	5.9	35.9	38.8	25.4	100.0
60-80	5.8	17.3	38.0	**28.7**	10.2	61.1	28.7	10.2	100.0
80-100	1.9	11.1	33.3	37.0	**16.7**	83.3	16.7	0.0	100.0
Total	22.9	34.8	26.5	12.1	3.6				100.0

256 This does not mean that these schools have not seen changes in the proportions of *BF* students, but that the change was not enough to move the schools to another group.

The different cohorts of beneficiaries in the two national exams and the transitions of schools across groups of *BF* intake might fully explain the reduction in the outcome gaps between the first and fifth quintiles of *BF* intake, discussed in subsection 8.3.1. If that is the case, then any claim of *BF* contribution to that improvement could be illusory. The panel data analysis in the next section allows us to examine whether *BF* participation at the school level has any share in explaining those improvements in school outcomes. By controlling for the observed changes in school composition, resources, and proportion of beneficiaries between 2005 and 2007, the panel data analysis can discount the effects of those changes from the observed improvement in school outcomes. Moreover, as I explain in the next section, the panel data analysis also allows us to control for any other factor that differs across schools but that is invariant between those two years, as well as any temporal trend that might affect all schools in the same way.

8.4 Panel Data Model

In this section I use a common set of school outcomes and school context (composition and resources) variables, respectively, as dependent (**Y**) and independent variables (**X**) for the years 2005 and 2007 (as described in Table 38) to estimate a model using *panel data* techniques. The advantage of using panel data is the possibility of estimating models that control for school fixed effects across time ("***fixed effects"*** or "***unobserved effects"*** models), as well as for time effects across schools ("***time fixed effect*** models). In the absence of experimental data, as is the case in this research, using models based on panel data is an important way to reduce the omitted variable problem and to emulate an "experiment" in the absence of one. This possibility represents a clear advantage of panel data analysis over cross-sectional analysis, as developed in the previous chapter. As stressed in the literature (Finkel, 1995; White, Payne and Lakey, 2000), panel data explicitly incorporates change over time in the analysis; it offers a stronger framework for the analysis of causal relationships between variables; and it provides a more robust model for the evaluation of the effects of policies and programmes. By using a panel, each individual unit of analysis (e.g. school) becomes its own effective control if: (a) any relevant characteristic interfering both with the outcome of interest (Y) and with the independent factor being tested (X) is invariant with time within each unit; (b) any relevant time variant factor affecting Y and X equally affects each unit of analysis; and (c) any relevant factor affecting Y and X that may vary across time and across units of analysis are included in the model being estimated. Using the panel data framework, I estimate a model to test the hypothesis that positive changes in fourth grade school outcomes observed between

2005 and 2007 can be associated with *BF* participation. As in the previous chapter, *Time* is considered a relevant variable in assessing potential contributions of *BF* to educational outcomes. However, the ***time-effect*** here is assessed by examining changes in the partial effect of *BF* intake over the two-year period of the panel.

Model 4 describes what is referred to in the econometric literature as the *two-way fixed effect model*. It brings together both the control for any variation across time that affects all schools in the same way and might interfere with the outcome $\mathbf{Y_{it}}$ (represented by the dummy variable $\mathbf{D_t}$) and the control for what could vary between schools and affect the outcome $\mathbf{Y_{it}}$ but is constant across time (represented by $\mathbf{A_i}$). The model also allows for variations in school factors (school composition ($\mathbf{X_{it}}$) and resources ($\mathbf{S_{it}}$)) that might be simultaneously changing over time and across schools, and that could also be correlated with the outcome $\mathbf{Y_{it}}$.

Model 4

$$Y_{it} = \boldsymbol{\alpha}'\boldsymbol{X_{it}} + \boldsymbol{\gamma}'\boldsymbol{S_{it}} + \beta_1 BF_{it} + \beta_2 D_t + \beta_3\, BF_{it}.D_t + A_i + u_{it}$$

In Model 4, any school factors that may affect educational results in any particular school and potentially do not change over time (e.g. administrative level and education system to which the school is linked; management model; school geographic location; etc.) is represented by the term $\mathbf{A_i}$ in the model. Even if not available in the data (unobserved), the influence of these factors on outcomes can be managed by estimating a *school-fixed-effects* model. The term $\mathbf{A_i}$ allows each school to have a difference in its outcomes that is due only to a set of characteristics that are specific to unit **i** (therefore, different across schools) and that do not vary across time. By "fixing" (over time) these school factors in the model, we avoid the omitted variable problem caused if any factor is correlated with any regressor in the model at the same time that it affects the dependent variable under analysis ($\mathbf{Y_{it}}$).

Likewise, any factor that might vary across time and simultaneously affect all schools between 2005 and 2007 (e.g. a new national education funding policy; an extension of the school cycle; a new national textbook programme etc.) is captured by the coefficient $\boldsymbol{\beta_2}$ on the time dummy variable ($\mathbf{D_t}$). However, the marginal effect of the dummy for year ($\mathbf{D_t}$) on school outcomes is allowed to vary according to the level of *BF* intake – a variation captured by the coefficient $\boldsymbol{\beta_3}$. This coefficient can tell us whether there is any significant change in the

marginal effect of *BF* intake between 2005 and 2007 that could suggest a positive contribution of *BF* to school outcomes over time.

Finally, to avoid bias in estimating the parameters of interest β_1, β_2, **and** β_3, a set of school factors (S_{it}) and student and household characteristics (X_{it}) is also included in the model. These are factors that may vary across time and schools and that are simultaneously correlated with students' outcomes and level of *BF* intake in each school (e.g. parents' and teachers' education, incidence of large families, class size, etc.). The full description of the variables entering in Model 4 is presented in **Table 41**.

Table 41: Description of variables used to estimate Model 4

	VARIABLES	DESCRIPTION
Bolsa Família	*BF* (%)	Percentage of 4th grade *BF* beneficiaries in the school.
	Year2007	Dummy variable equal to 1 if year is 2007 and equal to 0 if year is 2005.
School Composition (students' and households' characteristics)	Prop. living in female headed households	Proportion of pupils who live in female headed households amongst the 4th grade pupils who sat for the national exam in a specific year.
	Prop. whose mother has post-primary education	Proportion of pupils whose mothers have completed secondary school or College amongst the 4th grade pupils who sat for the national exam in a specific year.
	Prop. whose father has post-primary education	Proportion of pupils whose fathers have completed secondary school or College amongst the 4th grade pupils who sat for the national exam in a specific year.
	Prop. of boys	Proportion of boys amongst the 4th grade pupils in the school who sat for the national exam in a specific year.
	Prop. of non-whites	Proportion of non-whites amongst the 4th grade pupils in the school who sat for the national exam in a specific year.
	Prop. who attended pre-school	Proportion of pupils who attended pre-school amongst the 4th grade pupils who sat for the national exam in a specific year.
	Prop. living in large families (7 or more)	Proportion of pupils who live in families with 7 or more members amongst the 4th grade pupils who sat for the national exam in a specific year.
	Prop. who have failed any grade	Proportion of pupils who have failed any grade in past years amongst the 4th grade pupils who sat for the national exam in in a specific year.
School Resources	Computer room	Dummy variable equal to 1 if the school has a computer room.
	Laboratory	Dummy variable equal to 1 if the school has a laboratory room.

VARIABLES		DESCRIPTION
	School Food available	Dummy variable equal to 1 if the school offers free school meals to pupils.
	Library/Reading room	Dummy variable equal to 1 if the school has a library or reading room available.
	Facilities index	Index number ranging from 0 to 1 measuring the availability of school facilities amongst 13 items measured by the National School Census in both years (2005 and 2007).
	Equipment Index	Index number ranging from 0 to 1 measuring the availability of school facilities amongst 8 items measured by the National School Census in both years (2005 and 2007).
	Internet available	Dummy variable equal to 1 if the school has Internet available.
	Utilities index	Index number ranging from 0 to 1 measuring whether the school has access to: piped water, power supply, sewage, and garbage collection.
	Class Size (1^{st} to 4^{th} grade)	Number of pupils enrolled in grades 1 to 4 divided by the number of 1^{st} to 4^{th} grade classes.
	Prop. teacher with HE (1^{st} to 4^{th} grade)	Proportion of teachers working in 1^{st} to 4^{th} grade classes who have higher education.
Test Scores	Mean Test Score PORT	School mean test score result for Portuguese Language in a specific year.
	Mean Test Score MATHS	School mean test score result for Mathematics in a specific year.

8.5 Results

The estimation by OLS using the *school-and-time fixed effect model* yields the coefficients and robust standard errors on *BF* intake, on the dummy for year[257], and on their interaction as reported in **Table 42**. The F-statistics show a joint significance of β_1, β_2, and β_3 for all outcomes.

Table 42: Coefficients and robust standard errors of school outcomes estimated by fixed-effects

VARIABLES	(1) Portuguese	(2) Maths	(3) Pass-grade	(4) Dropout
BF (β_1)	-0.041***	0.002	-0.015**	0.014***
	(0.007)	(0.008)	(0.007)	(0.004)
Year2007 (β_2)	-1.008***	11.565***	0.894***	-0.004
	(0.231)	(0.256)	(0.242)	(0.122)
Year2007 x *BF* (β_3)	0.028***	-0.040***	0.041***	-0.039***
	(0.006)	(0.007)	(0.006)	(0.003)
Controls	Yes	Yes	Yes	Yes
Constant	176.604***	177.445***	74.141***	4.004***
	(1.589)	(1.759)	(2.056)	(1.102)
F-test (Ho: $\beta_1 = \beta_2 = \beta_3 = 0$)	11.14	2090	89.66	135.0
Prob > F	0.000	0.000	0.000	0.000
Observations	44,359	44,359	44,359	44,359
R-squared	0.47	0.39	0.16	0.10
Number of Schools	23,747	23,747	23,747	23,747

Note: * significant at 10% level; ** significant at 5% level; *** significant at 1% level

As discussed in the previous chapter, coefficients on variables composing interactive terms should not be read as "effects". One must consider the conditional effects as described by the expressions below to properly describe the expected marginal effects of *BF* intake and time on school outcomes. Time in Model 4 is measured by the binary variable $\mathbf{D_t}$, which represents the two-year lag between 2005 and 2007. Due to interaction with *BF* intake, the "slope" of the relationship between school outcomes and the two-year lag will depend on the percentage of beneficiaries in each school, and is expressed by **Equation 13**:

Equation 13

$$\frac{\partial Y}{\partial D_t} = \beta_2 + \beta_3 . BF$$

In the same way, the marginal effect of *BF* intake on school outcomes is expected to vary according to the year we consider and is expressed by the partial derivative given by **Equation 14**:

257 The full set of coefficients can be seen in Table A - 49 in the appendix.

Equation 14

$$\frac{\partial Y}{\partial BF} = \beta_1 + \beta_3 . D_t$$

Equation 13 and Equation 14 can be used to analyse changes in marginal effects related to *BF* intake on school outcomes over time. Consider, for instance, the case in which **Y** represents test scores in Portuguese in Equation 14. Although that marginal effect is assumed to be constant across schools, it will be allowed to vary between 2005 and 2007 according to the coefficient on the dummy $\mathbf{D_t}$ ($\boldsymbol{\beta_3}$). The parameter $\boldsymbol{\beta_1}$ is, thus, an estimate of the *average effect* of *BF* intake on school test scores for the year 2005 ($\mathbf{D_t}$=0). It estimates by how many points the mean test score is expected to change on average for each percentage point increase in the proportion of beneficiaries in a given school in that year. Due to the "programme placement effect", schools with higher *BF* intakes are expected, on average, to perform worse than schools with lower proportions of *BF* beneficiaries – a result confirmed by the negative and significant $\boldsymbol{\beta_1}$ in Table 42 in the case of Portuguese (-0.041). On average, a 10 p.p. increase in *BF* intake is associated with a 0.4-point reduction in the school mean test score in Portuguese in 2005.

The coefficient $\boldsymbol{\beta_3}$ in Equation 14 comes into effect when the dummy variable for time is equal to one ($\mathbf{D_t}$=1), which is true when the year is 2007. This means that $\boldsymbol{\beta_3}$ estimates the average difference of the *BF* intake effect between 2005 and 2007. There are three possible results for $\boldsymbol{\beta_3}$:

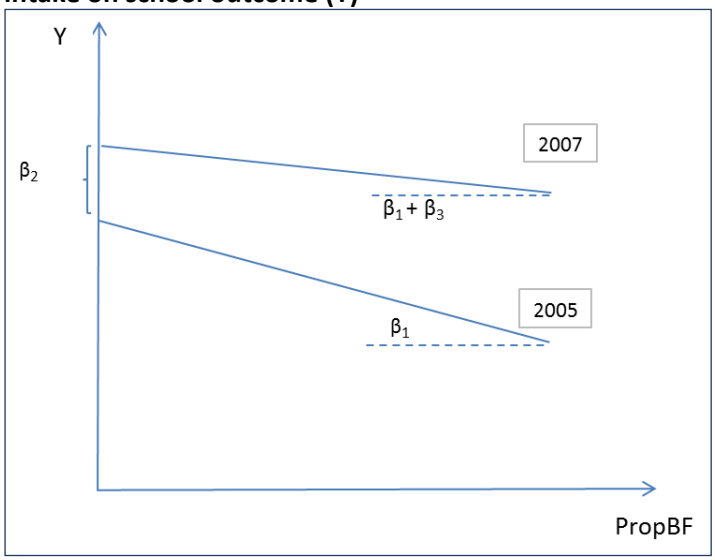
If $\beta_3 = 0$ → there has been no change in *the BF intake effect* on test scores between 2005 and 2007.

If $\beta_3 > 0$ → the negative *BF intake effect* on test scores has been reduced between 2005 and 2007.

If $\beta_3 < 0$ → the negative *BF intake effect* on test scores has increased between 2005 and 2007.

The null hypothesis (H_o) to be tested in the case of test scores is $\boldsymbol{\beta_3 \leq 0}$ against the alternative hypothesis (H_a) $\boldsymbol{\beta_3} > 0$**.** The alternative hypothesis predicts a positive change in test scores associated with *BF* intake between 2005 and 2007. If H_0 is rejected, it implies that the negative marginal effect of *BF* intake on Portuguese test scores in 2005 is reduced in 2007 on average by β_3 for each percentage point in *BF* intake. This would mean that between 2005 and 2007, marginal gains in test scores due to *BF* intake took place, reflecting potential positive effects of participation in *BF*. **Figure 13** illustrates the linear relationship between test scores and *BF* intake in each year in the case $\beta_3 > 0$ given $\beta_1 < 0$.

Figure 13: Hypothesis of diminishing marginal effect of *BF* intake on school outcome (Y)



In order to systematically address the full set of hypothesis to be tested regarding the different school outcomes, three questions are considered based on Equation 14: (i) Does *BF* intake affect the school outcome **Y**?; (ii) Does *BF* intake degrade the school outcome **Y**?; (iii) Does the effect of *BF* intake on the school outcome **Y** improve over time? The last question holds the central hypothesis of interest, which relates to the potential contribution *BF* might make to school outcomes over time. **Table 43** describes the hypothesis to be tested for each school outcome, the statistical test used, the result, and the conclusion based on the estimated models in Table 42.

Table 43: Hypothesis tests for the effects of *BF* intake on school outcomes

School outcome (Y)	Hypothesis to be tested (Ha)	Ho (Null)	Test	Results	Conclusion
Portuguese	Does *BF* intake affect test scores in Portuguese?	$\beta_1 = \beta_3 = 0$	F-test	F = 15.98 Prob > F = 0.000	*BF* intake **affects** Portuguese test scores
	Do test scores decrease with *BF* intake?	$\beta_1 + \beta_3.D_t \geq 0$	One-tail t-tests: i) D_t=0: $\beta_1 \geq 0$ (t-test) ii) D_t=1: $\beta_1 + \beta_3 \geq 0$ (t-test)	i) t = -5.61 Prob.>|t|=0.000 ii) t = -2.47 Prob.>|t|=0.007	i) Test scores **decrease** for Dt=0 (2005) ii) Test scores **decrease** for Dt=1 (2007)
	Does the effect of *BF* intake on test scores improve over time?	**$\beta_3 \leq 0$**	One-tail t-test	t = 4.44 Prob.>|t|=0.000	The effect of *BF* intake on test scores **improves** between 2005 and 2007.
Mathematics	Does *BF* intake affect test scores in Mathematics?	$\beta_1 = \beta_3 = 0$	F-test	F = 30.37 Prob > F = 0.000	*BF* intake **affects** Mathematics test scores
	Do test scores decrease with *BF* intake?	$\beta_1 + \beta_3.D_t \geq 0$	One-tail t-tests: i) D_t=0: $\beta_1 \geq 0$ (t-test) ii) D_t=1: $\beta_1 + \beta_3 \geq 0$ (t-test)	i) t = 0.19 Prob.>|t|=0.425 ii) t = -6.36 Prob.>|t|=0.000	i) Test scores **<u>do not</u> decrease** for Dt=0 (2005) ii) Test scores **decrease** for Dt=1 (2007)
	Does the effect of *BF* intake on test scores improve over time?	**$\beta_3 \leq 0$**	One-tail t-test	t = -5.70 Prob. > |t|=0.999	The effect of *BF* intake on test scores **<u>does not</u> improve** between 2005 and 2007.
Pass-grade	Does *BF* intake affect pass-grade rates?	$\beta_1 = \beta_3 = 0$	F-test	F = 29.78 Prob > F = 0.000	*BF* intake **affects** pass-grade rates
	Do pass-grade rates decrease with *BF* intake?	$\beta_1 + \beta_3.D_t \geq 0$	One-tail t-tests: i) D_t=0: $\beta_1 \geq 0$ (t-test) ii) D_t=1: $\beta_1 + \beta_3 \geq 0$ (t-test)	i) t = -2.01 Prob.>|t|=0.022 ii) t = 4.99 Prob.>|t|=0.999	i) Pass-grade rates **decrease** for Dt=0 (2005) ii) Pass-grade rates **<u>do not</u> decrease** for Dt=1(2007)
	Does the effect of *BF* intake on pass-grade rate improve over time?	**$\beta_3 \leq 0$**	One-tail t-test	t = 6.72 Prob. > |t|=0.000	The effect of *BF* intake on pass-grade rates **improves** between 2005 and 2007.
Dropout	Does *BF* intake affect dropout rates?	$\beta_1 = \beta_3 = 0$	F-test	F = 98.62 Prob > F = 0.000	*BF* intake **affects** dropout rates
	Do dropout rates increase with *BF* intake?	$\beta_1 + \beta_3.D_t \leq 0$	One-tail t-tests: i) D_t = 0: $\beta_1 \leq 0$ (t-test) ii) D_t = 1: $\beta_1 + \beta_3 \leq 0$ (t-test)	i) t = 3.49 Prob.>|t|=0.000 ii) t = -8.15 Prob.>|t|=1.000	i) Dropout rates **increase** for Dt=0(2005) ii) Dropout rates **<u>do not</u> increase** for Dt=1(2007)
	Does the effect of *BF* intake on dropout rate improve over time?	**$\beta_3 \geq 0$**	One-tail t-test	t = -12.39 Prob. > |t|=0.000	The effect of *BF* intake on dropout rates **improves** between 2005 and 2007.

Based on the results in Table 43, it can be concluded that *BF* intake influences all four of the school outcomes analysed. In 2005, all school outcomes, except Mathematics, deteriorate with *BF* intake. However, as assessed by the third hypothesis, the negative impact of *BF* intake on school outcomes is reduced between 2005 and 2007 for test scores in Portuguese, pass-grade rates, and dropout rates. The magnitude of the marginal effects of *BF* intake on school outcomes in both years is given in **Table 44,** nationally and by region[258].

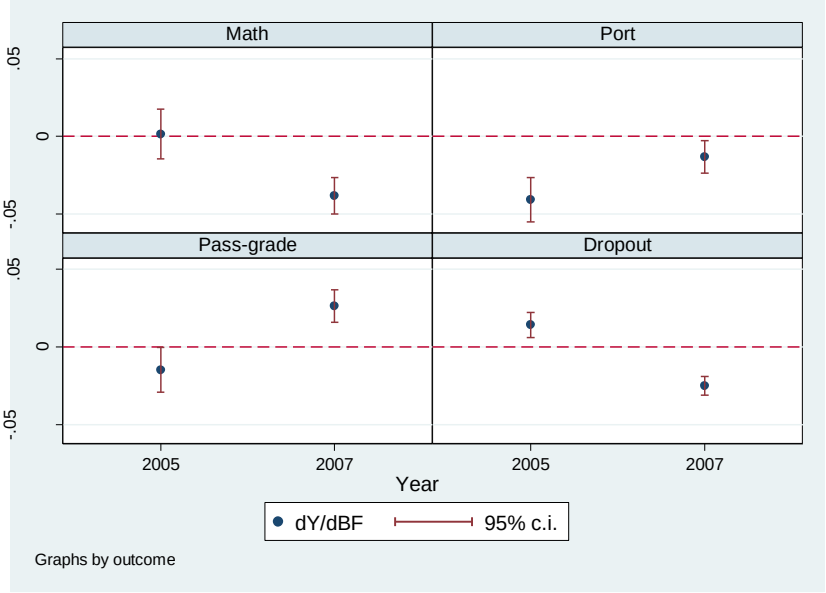
Table 44: Marginal Effect of *BF* intake on school outcomes by region estimated for 2005 and 2007

School Outcome	Brazil		Region									
			North		Northeast		Central-West		Southeast		South	
	2005	2007	2005	2007	2005	2007	2005	2007	2005	2007	2005	2007
Portuguese	-0.041***	-0.013**	-0.043**	0.015	-0.015	-0.005	-0.093***	-0.018	-0.068***	0.000	-0.045***	-0.028**
Maths	0.002	-0.038***	-0.016	-0.009	0.005	-0.002	-0.040	-0.024	-0.027	-0.022	0.000	-0.055***
Dropout rate	0.014***	-0.025***	0.011	-0.017*	0.016	-0.019**	0.020	-0.020	0.002	-0.014***	0.009**	-0.006**
Pass-grade rate	-0.015**	0.026***	-0.054***	0.021	-0.020	0.020*	-0.040*	0.016	0.021	0.051***	-0.045***	0.010

Note: * significant at 10% level; ** significant at 5% level; *** significant at 1% level

Although the point estimates in Table 44 give us the full picture of the marginal effect of *BF* intake in each year[259], a graphical representation of Equation 14 helps to depict not only the point estimates, but also their confidence intervals and the changes that occurred between 2005 and 2007. **Figure 14** shows the average national marginal effects of *BF* intake on school outcomes in each year of the panel. Since the confidence intervals do not overlap for any of the outcomes, the marginal effects are all statistically different between 2005 and 2007.

Figure 14: Marginal effects of *BF* intake on school outcomes by year (NATIONAL)



258 The estimates by region derive from restricting the sample of schools to each region before regressing Model 4.

259 Because the marginal effect of *BF* intake varies only with the dummy Dt, there are only two possible values to be estimated.

For test scores in **Portuguese**, although the marginal effect is still negative in 2007, it clearly diminishes in absolute terms in relation to 2005 results, suggesting that, on average, schools with higher percentages of participants in *BF* improved more than schools with lower *BF* participation rates between 2005 and 2007. That improvement occurred despite schools' time invariant characteristics and despite those factors related to school composition and school resources controlled for (as listed in **Table A - 49**). This trend is even stronger for the North, Central-West, and Southeast regions (see **Figure A- 2**). In the South, although the point estimate seems to improve in Table 44, the marginal effects are not statistically different from each other. In the Northeast, neither in 2005 nor in 2007 is the *BF* marginal effect statistically distinguishable from zero[260]. These results suggest a potential benefit to schools' test scores in Portuguese due to participation in *BF* nationally and in three of the five regions.

The estimated change in the marginal effect of *BF* intake on **pass-grade rates** also shows improvements between 2005 and 2007, becoming positive at the national level in 2007. While in 2005 a 10 p.p. increase in *BF* intake was associated with a 0.15 p.p. *decrease* in pass-grade rates, in 2007 the same variation in *BF* intake is associated with a 0.26 p.p. *increase* in pass-grade rates. The same trend of improvement is statistically distinguishable (at the 5% level) in the North and South regions, and is also noted in the other regions, although with more statistical ambiguity (see **Figure A- 3**). Schools also present lower **dropout rates** nationally by percentage point of *BF* intake in 2007, as compared to 2005. While in 2005 10 p.p. more beneficiaries are associated with an *increase* in dropout rates by 0.14 p.p., in 2007 the same variation in *BF* intake is associated with a *decrease* in dropout rates by 0.25 p.p. All regions perceive improvements by proportion of beneficiaries, although it is only in the Northeast, Southeast, and South that improvements are statistically distinguishable[261] from zero, as shown in **Figure A- 4**.

With test scores in **Mathematics**, there is a different picture, as the national average marginal effect becomes negative in 2007 after being not different from zero in 2005. Looking at the results by region (see **Figure A- 5**), one sees no significant marginal effect for *BF* intake, except in the South in 2007. Technically, in analysing by region, no change in the *BF* intake marginal effect is detected between 2005 and 2007, since all confidence intervals overlap. The more plausible conclusion is that no improvement took place by region related to *BF* intake. The national result, however, may suggest that the relationship between the share of

260 The regression coefficients for the Northeast in the school fixed-effects model (β1 β2 and β3) are jointly statistically insignificant. This means that schools in that region did not observe any change in the mean test score in Portuguese between 2005 and 2007 due either to time trends or to variations in *BF* intake.

261 The South at 5% and the Northeast and Southeast at 10%.

beneficiaries in a school and poor performance in Mathematics is getting stronger over time, instead of attenuating as a potential consequence of participation in *Bolsa Família*. Nevertheless, some caution is necessary in this interpretation, because despite the control factors in estimating Model 4, a large and significant increase in Mathematics test scores took place between those years, as attested by the coefficient β_2 for Mathematics in Table 42. By plotting the marginal effect of the dummy variable for the year by the proportion of beneficiaries (**Equation 13**), we can see how the effect of the two-year lag affects school outcomes differently according to the value of *BF* intake. We can also clearly see what happens with Mathematics (**Figure 15**).

Figure 15: Marginal effects of time on school outcomes by level of *BF* intake (NATIONAL)

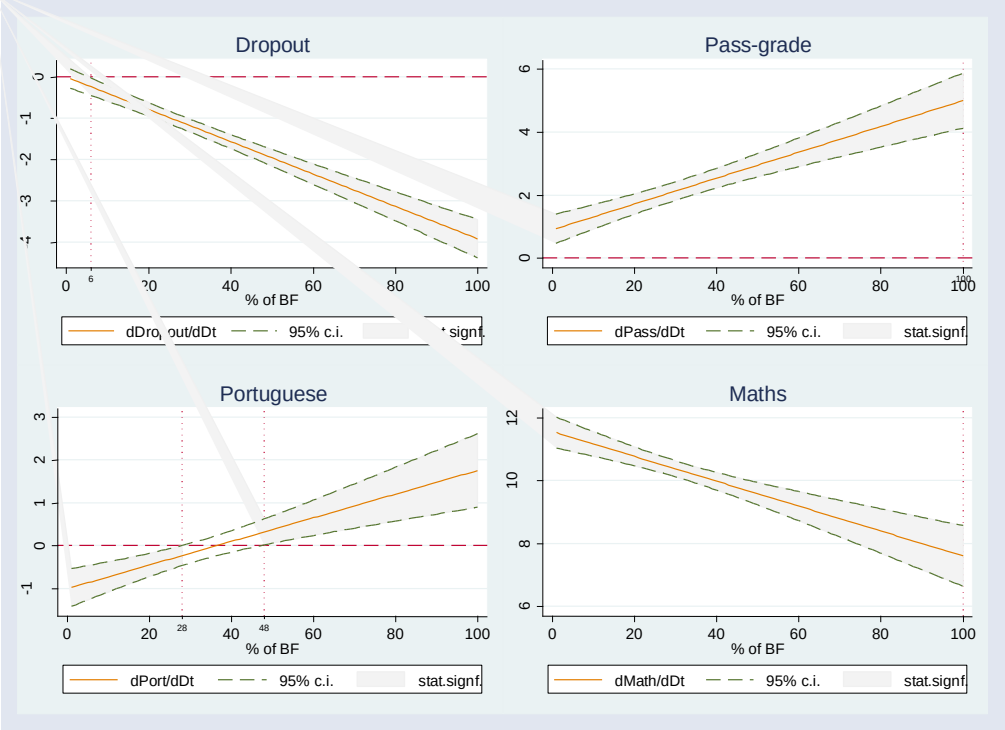


Figure 15 shows the marginal effects of time for different values of *BF* intake for each of the four school outcomes. In fact, the plotted marginal effects of $\mathbf{D_t}$ on school outcomes are the estimated differences in those school outcomes between 2005 and 2007 at different values of *BF* intake. For Portuguese test scores, pass-grade, and dropout rates the marginal effect $\partial Y/\partial Dt$ translates into better results for higher levels of *BF* intake, confirming the results previously discussed. In the case of Mathematics, it can be seen that the two-year lag has a positive and significant effect on test scores for all values of *BF* intake. However, those schools with lower proportions of beneficiaries seem to have gained more of whatever explains the general improvement of test scores in Mathematics between those two years. It is

even possible that the factor(s) causing the improvement of test scores for low *BF* intake schools are different from those explaining the improvement detected for high *BF* intake schools. For instance, if special programmes focused on Mathematics teaching had been introduced, reaching more advantaged schools between those years, it would be possible to observe a faster improvement for schools at the bottom of *BF* intake distribution, while those at the top of the distribution might lag behind despite the possible gains accrued by poor children from participating in *BF*. In sum, the negative marginal effect of *BF* intake estimated for test scores in Mathematics in 2007 reflects the larger improvement obtained by schools from the lower ranks of *BF* intake, rather than the decline of the results of those holding larger shares of beneficiaries, since the latter also observed improvements in test scores. The problem is that in the case of Mathematics, the possible gains from *BF* are probably overshadowed by the gains obtained by more advantaged schools during that period. Without knowing what factor explains the gains over time for low *BF* intake schools, it is not possible to include it in the model in order to purge those effects from the time effect captured by the dummy D_t[262].

8.6 Conclusions and Limitations

The initial analysis of the two years of panel data in this chapter showed that school outcomes improved between 2005 and 2007, with increasing gains for schools at the top of the *BF* intake distribution. As a result, the performance gap between schools in the first and fifth quintiles of *BF* intake diminished over those years. At first glance, these changes may suggest a positive contribution of *BF* to school outcomes. However, it was also shown that school composition in terms of student and household characteristics, as well as school resources, changed between 2005 and 2007 across levels of *BF* intake. Those are influential factors on school outcomes, and the observed changes were shown to benefit schools in the top quintiles of *BF* intake. In addition, schools also changed in terms of their share of beneficiaries between those years, suggesting that the observed improvements at the top of the *BF* intake distribution might be due to transitions of schools to different groups of *BF* intake. The different cohorts of pupils (and beneficiaries) in the two national exams, and the transitions of schools across groups of *BF* intake, might give the impression that larger shares

262 One of the possible explanations for the improvement in test scores in Mathematics in 2007 raised by the Ministry of Education is the Mathematical Olympiad created nationally in 2004 and first run in 2005. In 2005, it reached 31,031 public schools in 93.3% of municipalities in Brazil and in 2007 a total of 38,450 schools took part in the competition covering 98.1% of municipalities in Brazil. By the time this research was finished, the information on which schools took part in each year in the Olympiad was not made available. Therefore, it was not possible to test if the Mathematical Olympiad was acting as an omitted variable for the national exam's results in Mathematics.

of *BF* participation, in itself, was causing the observed improvements in school outcomes across years. A *school-and-time fixed effects model* tested the hypothesis that *BF* participation holds a share in explaining the observed improvements in school outcomes (despite changes in school composition, resources, and proportion of beneficiaries).

The estimated *school-and-time fixed effects model* (Model 4) manages to control for a set of school compositional characteristics and resources over time, as well as for time invariant school characteristics and school invariant trends over time. By avoiding several potential confounding factors, the panel analysis allows us to capture changes in the marginal effect of *BF* intake across time. The results show that, although the proportion of beneficiaries has a deteriorating effect on school outcomes in 2005 (except in Mathematics), that effect is reduced between 2005 and 2007 for test scores in Portuguese and becomes positive for pass-grade and dropout rates. The two-year period positively affects schools at higher levels of *BF* participation more than those at the bottom of the *BF* intake distribution for those school outcomes, a trend also observed at the regional level. In Mathematics, however, results are blurred by unknown factors that somehow positively affected low *BF* intake schools more than high *BF* intake schools. Despite this, the overall results support the idea that marginal gains associated with *BF* intake over time potentially reflect direct contributions of *BF* to school outcomes.

However, some caveats remain. First, Model 4 does not allow for a clear-cut pre/post policy change assessment, since *Prova Brasil* started in 2005 when *BF* had already been in place for almost two years, amounting to eight million families. The panel data makes use of an alternative post-programme implementation baseline that allows measures of change in relation to 2005. Therefore, the estimated effects are relative to 2005 and do not translate the full picture of how *BF* might be influencing school outcomes over time. Second, the coefficient estimated on the interaction term ($\boldsymbol{\beta_3}$) might be translating other policies targeted towards schools or families in deprived areas coinciding with *BF* targeted families. In this case, the improvements related to the marginal effect of *BF* intake might not be a result of *BF* itself, but of other educational policies driven towards high *BF* intake school communities. Nevertheless, given the highly decentralised political system in Brazil, it is unlikely that local initiatives would create a national effect, if not part of a nationally-induced programme. By 2007, no nation-wide policy was in place to add extra resources to schools based on the level of participation in *BF*. The likelihood is that any marginal gain related to *BF* intake is, in fact, an effect of the cash transfer and its conditionalities. Third, the lack of information on characteristics related to participation in *BF*, such as the value of the cash transfer and length of time of participation for

each family in both years in the panel, prevents any conclusion about the relevance of these factors to the improvements estimated by the *BF* intake marginal effect. However, the consistency between the results found in the panel analysis with those arising from the cross-sectional analysis developed in the previous chapter strongly suggests that the improvements in school outcomes between 2005 and 2007 are, in fact, a direct contribution of the benefits *BF* brings to the educational opportunities of children of low-income families.

Chapter 9. Main Findings

9.1 Introduction

The investigation of whether *Bolsa Família* makes any positive contribution to the educational outcomes of economically disadvantaged children in Brazil followed the analysis of three empirical questions that can be summarised as follows:

- What can explain the achievement gap between *BF* beneficiaries and non-beneficiaries in the national exam?
- Do time of exposure and per capita cash paid to families in *BF* positively influence school results and reduce the achievement gap between high-*BF*-intake schools and low-*BF*-intake schools?
- Did schools observe any improvement in outcomes between 2005 and 2007 that can be associated with the level of *BF* participation in each school?

In pursuing answers to these questions I first explored some of the direct consequences of poverty on children's opportunities and outcomes as reported in the literature, particularly outcomes in education. Several of the mechanisms operating through or moderated by family income discussed in the literature suggest that many of the current attempts to address unequal educational achievements and to promote the education for children of low-income families by merely addressing intra-school factors are likely to fail. How income is distributed in society is at least as important as the way society allocates educational resources to different social groups to promote equal educational opportunities. In this sense, some equality of outcomes is required as a base upon which equality of opportunities can be made effective (Esping-Andersen, 2009). Parent's incomes are, in many respects, the starting point for children's present and future outcomes, and schools cannot compensate for many socially inherited conditions. The playing field in the present is simply not level for millions of children, even when schools are available. It is at this point that the human capital theory collapses. It works for children and youths who are able to benefit from what schools have to offer. But benefitting from schools requires some social minimums that many children may fall short of. This recognition invites a rethinking of the human capital paradigm. Policy-makers should start by asking a different question: *what reduction of child poverty can do for schools and education in the present?* This question presents a new challenge to policy-makers in integrating socio-educational policies to tackle socio-educational inequalities. Perhaps income transfer is not the sole response, but it is a necessary one.

The theoretical models discussed in this thesis explaining the relationship between family income and children's outcomes allow us to envisage at least four main influences relevant to educational outcomes: (i) parents' capacity to invest time and money in children's education; (ii) social experiences mediating and moulding children's values, behaviours, choices, and expectations that affect their development; (iii) parents' capacity to cope with psychological distresses that can affect their ability to support and maintain a healthy environment for children; and (iv) school functioning for economically disadvantaged children. Attacking income inequality and poverty amongst families is not only relevant to directly addressing parents' capacities to provide a better home and social environment for children's education, but also might be very important in resetting the school functioning for the children from the most disadvantaged families.

Empirical studies developed over the last 40 years in the US (reviewed in chapter two) show that cash transfer programmes may have significant effects on the educational outcomes of children of low-income families. Positive impacts of welfare and anti-poverty programmes were found in respect to enrolment and graduation in high school, attendance rates, comportment, academic achievement, and years of schooling. Based on those studies, I identify some general aspects which policy-makers should pay attention to for an effective cash transfer policies. First, **early interventions** are vital in mitigating the potential damages to education, since most evidence recognises that children arrive at school already at very different levels of readiness to benefit from any learning experience. Moreover, young children attending lower grades are more likely to benefit from additional income paid to their families than older children or those in higher grades. This is probably because the youngest are more dependent on their parents; they have less freedom of choice and are the most affected by parents' circumstances in a way that can have an impact on their education. Second, cash transfer policies must be able to affect families' **permanent income**, thereby affecting the patterns of consumption in the household. Any policy in this respect must avoid irregular, insufficient, or transient hand-outs that are incapable of affecting household permanent income. Third, **time of exposure** to cash transfer programmes is relevant in gauging effects on educational outcomes, particularly learning (three or more years in most of the studies). This is consistent with the idea that cash transfer policies must endure so to affect permanent income and patterns of consumption that can, in turn, change the environment for children. It is also consistent with the idea that learning outcomes are probably the most difficult and take the longest to be influenced by cash transfer policies. Fourth, the effect of income increases on educational outcomes also varies with the **level of poverty**. It turns out that the poorer the

family is, the more relevant the impacts of the cash transfer for children. The cash value relative to the household per capita income is a key factor in explaining the potential effect cash transfers may have on children's educational outcomes. These are general findings that should guide policy-makers in designing cash transfer policies in which children's educational outcomes are amongst the main objectives.

I also discussed in this thesis the role conditional cash transfer programmes may have in mitigating poverty's effects on children's educational outcomes in developing countries. A significant amount of research has demonstrated the operational efficacy of these programmes in targeting the poor and in producing expected social outcomes, such as alleviating poverty in the short term, increasing health and nutritional conditions for children, and assuring higher levels of enrolment and attendance in school. If for no other reason, these results alone could justify public investments in CCTs. However, human capital accumulation is at the core of the long-term objectives, as stated in most CCT blueprints. As argued in chapter three, human capital accumulation can only be justified if CCTs can make any positive contribution to the learning outcomes, promotion, and completion rates of beneficiary children. So far, research on CCTs has failed to produce any convincing evidence of these effects, particularly the first, for which null or even negative effects have been reported in the literature. Nevertheless, this absence of results contrasts with the large collection of social experiments and longitudinal studies reviewed in chapter two showing the effects of welfare and anti-poverty programmes on the educational outcomes of beneficiary children. Amongst the many possible reasons that researchers have failed in detecting CCTs' effects on learning outcomes, two stand out. First, the timeframe in which studies on CCTs have been carried out tend to be shorter than the timeframe in which research in the US was able to detect many of the educational effects, particularly learning outcomes. Second, the value of cash paid by CCTs to families has, so far, found very little space in the analysis of CCT effects on education. Working with these two testable hypotheses, I come to the core of the empirical analysis in this thesis: the investigation of whether *Bolsa Família* in Brazil is contributing to achievement in standardised test scores in Portuguese Language and Mathematics of fourth grade students, as well as to pass-grade, and dropout rates.

Bolsa Família (and its predecessor *Bolsa Escola*) has been recognised in the literature as a main contributor to the staggering reduction in poverty and inequality in Brazil since the early 2000s. The question remaining is whether by reducing poverty and inequality in the short term, *BF* can also be an effective policy in creating human capital amongst the poor in the long term. The condition for that to occur relies on the potential effects *BF* might have on

promotion and completion rates, which, in turn, depend on the possibility that beneficiaries will experience improvements in learning outcomes, as described in the socio-educational framework proposed in chapter four. Evidence in the literature so far has shown clear positive effects on increasing school enrolment and attendance, and on reducing dropout rates. These are immediate outcomes derived from the institutional rules requiring families to enrol their children and to ensure that they attend school at a minimum rate per month. The more challenging effect of improvements in learning outcomes had not been studied yet on a national scale in Brazil, due in part to the lack of national test score data at the school level until very recently. When studies of CCT programmes in other countries have tried to evaluate effects on test scores or any other measure of learning outcomes, no effect was detected either. That key *missing link* is at the core of analysis in this thesis.

I have found evidence which strongly suggests that, in the case of *BF*, the effects on learning outcomes do exist, as do effects on pass-grade and dropout rates. I have found this by exploring, in tandem, the two conditions so far absent from research on this topic: time of exposure and cash value paid to families. In the next sections I summarise the main results and limitations of the empirical analysis.

9.2 The achievement gap between beneficiaries and non-beneficiaries

In the first part of the empirical analysis, I examined the achievement gap between beneficiaries and non-beneficiaries in *Prova Brasil* 2005 and how differences in terms of individual, household, and school characteristics explain that gap. My analysis shows that, on average, beneficiaries perform less well than non-beneficiaries by 16 points in Mathematics and by 20 points in Portuguese Language. Those differences are equivalent to two-thirds and three-quarters of one cognitive level, respectively, or 0.4 and 0.5 s.d. from the national mean. These gaps can be translated into months of schooling, as equivalent to five and seven months respectively. The achievement gap between beneficiaries and non-beneficiaries increases with income inequality, as measured by a proxy variable (durable goods index) showing that family income is associated with achievement in test scores in Brazil. The extension of that association was further examined in chapter six by exploring a set of individual, household, and school characteristics that may intervene with children's educational outcomes, and that are also correlated with the condition of being a beneficiary of *BF*.

The analysis in chapter six shows that beneficiaries are at a disadvantage, not only in terms of family income (which is an eligibility criterion to take part in *BF*). In addition to living

in more crowded and materially poorer households, beneficiaries are more likely to be engaged in domestic or paid work and receive less encouragement from parents for school related activities (parents who are generally less educated as well). Beneficiaries are more prevalent amongst those students who did not attend pre-school and have experienced grade failure and dropouts in past years. These students feel more marginalised in the classroom than non-beneficiaries and tend to study in less well-resourced schools, either in terms of infrastructure or teacher quality. Despite all this, beneficiaries share with non-beneficiaries the same expectations regarding achieving secondary education. The multiple disadvantages of beneficiaries are significant and explain part of the achievement gap mentioned above.

Once specific characteristics of students, families, and schools are taken into account, the analysis of test score differences in Portuguese Language according to *BF* participation reveals the relative weight of those factors in explaining the achievement gap. Students' past and present experience in school and students' time allocation when not at school are major contributors, explaining 28% of the achievement gap between beneficiaries and non-beneficiaries (after accounting for gender and race). A difference as large as 28 points in Portuguese Language (equivalent to one year of schooling) is estimated to derive from not attending pre-school, failing any grade in previous years, and experiencing a lack of attention from teachers when attending the fourth grade. Another difference of 20 points is attributed to involvement in domestic and any form of paid job outside the home. By not being able to do homework, students lose another seven points in test scores, on average. Those losing in all these respects are more likely to be beneficiaries.

Household characteristics explain another 15% of the gap between beneficiaries and non-beneficiaries. The material environment at home seems to be even more relevant than parents' characteristics, such as education and participation in school meetings. For instance, a child who lives in a house with bathroom and bedroom, with access to running water and electricity, has, on average, a test score higher by 28 points in Portuguese Language (one year of schooling) compared to a child whose household has none of these facilities. Parents with post-primary education is associated with no more than an additional eight points in test scores, while parental participation in school meetings is associated with an additional four points. Still significant, but less influential than the previous factors, is the family structure in terms of the size and presence of both parents in the household. Together, these factors are associated with seven points more on the average test score in Portuguese. These results show the importance of material poverty to students' performance at school relative to other aspects within the household context.

Students' individual characteristics, together with household characteristics, explain 44% of the existing gap between beneficiaries and non-beneficiaries in Portuguese tests scores. By introducing school-related factors (infrastructure, student-teacher ratio, teachers' education, and practice of assigning homework) the additional proportion to the explained gap is just 4.5%. These results suggest that the achievement gap is, in large part, due to conditions not related to school supplies or to teachers' related variables. Instead, it is what happens (or does not happen) at home and what children bring (or do not bring) to school that makes the bulk of the difference. However, I do not disregard the influence of schools on children's outcomes. What students experience at school in the years prior to and during the fourth grade is relevant, as shown by the large share of the achievement gap explained by the set of variables that I call "students' school history" (pre-school attendance, grade failure in previous years, and feeling of being neglected in the classroom). Nevertheless, socioeconomic disadvantages seem to play a major role in explaining why *BF* beneficiaries lag behind in school achievement compared to non-beneficiaries. By focusing on socioeconomically disadvantaged families, *BF* is expected to mitigate some of the harmful effects of poverty on the educational opportunities and outcomes of children of low-income families, and to contribute to reducing the achievement gap hitherto discussed.

9.3 The influence of *Bolsa Família* on school outcomes

In the second part of the empirical analysis I focused on investigating the central hypothesis of this thesis: that time of participation in *Bolsa Família* (*BF*) and the per capita cash paid to families are central variables influencing the potential effect of *BF* on school results for beneficiary children (test scores, pass-grade, and dropout rates). Using data from *Prova Brasil* 2007, I first explored the differences between schools in terms of composition, resources, and outcomes, asking whether schools' results are associated with the level of *BF* intake, with the mean time of participation in the programme, and with the cash value transferred to families.

The distribution of school resources and outcomes by level of *BF* intake is clearly biased against schools holding large shares of beneficiaries. Once participation in *BF* is considered a proxy variable for poverty incidence by school, it can be said that school conditions (and outcomes) deteriorate as the incidence of poverty increases. I have referred to this as a characteristic of a *regressive education system*, in which the more disadvantaged the socioeconomic context of families, the poorer resourced the schools are for their children. The consequence of both high concentration of poor children and less well-resourced school environment is low performance in all four indicators analysed in this thesis (test scores in

Portuguese and Mathematics, pas-grade, and dropout rates). An achievement gap of one cognitive level (or one year of schooling) exists between schools in the first quintile (0% to 20%) and those in the fifth quintile (80% to 100%) of *BF* intake. Dropout rates are, on average, three times higher and pass-grade rates 9 p.p. lower for schools in the fifth quintile compared to schools in the first quintile. At the same time, the correlations between the mean school outcomes and *BF* households' per capita incomes suggest that changes in the severity of poverty amongst poor families can affect the overall school result. Can those gaps be different if families take part in *BF* for a period of time that changes poverty conditions so as to benefit children's school outcomes?

The analysis in chapter seven shows that *BF* factors at the school level (intake, mean time of participation, and mean per capita cash) interact in predicting changes in school outcomes. All four outcomes at the school level are functions of those three *BF* factors, even after controlling for several household and school characteristics. An "average school" in terms of time of participation (two and a half years) and mean per capita cash (R$ 17.2) has an estimated improvement due to *BF* participation on both dropout and pass-grade rates of 0.2 and 0.1 p.p. respectively for each 10 p.p. increase in *BF* intake. This means that between two schools with same composition (except the level of *BF* participation), the same school resources, and located in the same region, the one with high proportion of beneficiaries will perform better in terms of pass-grade and dropout rates[263]. This evidence suggests a contribution of *BF* to two school outcomes for which the poorest students are clearly at a disadvantage. What about test scores — the *missing link*?

The analysis shows that although schools with higher proportions of beneficiaries tend to perform worse on the national exam, that negative effect reduces as average time of participation and average per capita cash increases. Schools in which families have participated in *BF* for a longer period of time *or* in which families have been paid a higher per capita benefit, beneficiary children perform better on the national exam, since their presence in the school degrades the mean test score by a lower marginal rate. In addition, the variables *Time* and *Cash* were found to have *substitute effects* on test scores; although the results strongly suggest that both *Time* and *Cash* have positive effects on test scores for beneficiaries, these effects interfere with one another in opposite directions. In other words, as per capita cash value increases, beneficiaries' performance improves, but the positive contribution of time of participation to their performance diminishes. In the same way, as time of participation increases, beneficiaries' performance improves, but the positive contribution of

263 A positive marginal effect of *BF* intake is also found for the Northeast region.

per capita cash to their performance diminishes. The net effect, however, is a gain in test scores for beneficiaries due to both time of participation in the programmes and per capita cash transferred to families.

Due to the *substitute effect,* the gains promoted by *Time* and *Cash* on test scores are limited by one another. I estimated the threshold for increases in the variable *Time* beyond which the positive contribution of *Cash* to test scores is completely annulled. For time of participation beyond three years, increases in the mean per capita cash of families have no extra benefit for the average performance of beneficiary children on tests scores. In the same way, a threshold exists for *Cash* beyond which increases in time of participation brings no additional contribution to test scores. That threshold is around R$ 25 per capita. In 2007, the large majority of schools were far from those limits, meaning that the large majority of beneficiaries could still benefit from either more time in the programme or higher cash transfers to their families.

So far, the conclusion that *BF* contributes to the school mean test score through the two factors analysed in this thesis (*Time* and *Cash*) does not tell us the practical significance of that contribution. To determine this significance, I used the interactive model to estimate the predicted gap between schools at extreme points of the distribution of *BF* intake (0% and 100%) when time of participation varies between one and four years[264]. A reduction in that gap of 14.5 points in Portuguese Language test scores (76%), or 0.85 s.d., was estimated when time of participation increased from one to four years. In schools where all children are beneficiaries, an increase of 10.6 points (0.62 s.d.) was estimated when exposure to the programme increased by three years. It was also shown that the mean test score is affected by time of participation only in schools where the proportion of beneficiaries is above 45%. These results show that even small marginal effects can be translated into significant changes of mean results at the school level.

In order to estimate the direct marginal effects of *Time* and *Cash* on school outcomes, I used a subsample of schools from the fifth quintile of *BF* intake (80% to 100%). For this group, the proportion of beneficiaries can be considered invariant across schools in terms of its effect on school outcomes. Pass-grade rates were estimated to improve by 0.6 p.p. for a one *real* increase in the mean per capita cash amount paid to families. This effect is invariant with time of participation in the programme, suggesting that an equivalent effect on pass-grade rates can be expected across schools, independent of the mean time of exposure to *BF*. On the

264 The per capita cash value was fixed at the fifth percentile (R$ 12.20) for these estimates.

other hand, time of participation has no effect on pass-grade rates for this group of schools. Dropout rate is expected to fall by 0.6 p.p. for each additional 10 months of participation in *BF*. Differences in the mean per capita cash paid to families do not matter with respect to dropout rates for the schools in the fifth quintile of *BF* intake. This result suggests that above the minimum per capita cash value observed in the subsample (R$ 11.50), no reduction in dropout is expected to occur as a result of any increase in per capita cash.

Regarding effects on test scores, only Portuguese is affected by *BF* factors in schools of the fifth quintile, not Mathematics[265]. On average, until 27 months of participation, results in Portuguese Language benefit from cash increases by a rate between 0.4 and 1.7 points per *real* increased. The magnitude of this seemingly tiny marginal effect can be seen in the estimated gain in test scores of 0.34 s.d. due to an increase of one s.d. in the per capita cash (R$ 2.16)[266]. The results show that for schools of the fifth quintile of *BF* intake, after two years in the programme, no significant extra gain will be observed in test scores if per capita cash is augmented. Any improvement in test scores for the poorest group of schools seems to come from the initial incentives provided by the programme. Differences in time of participation did not turn out to be positively associated with test scores for this group and, in fact, the results suggest that *Time* has a "discount effect" on any gains obtained from cash transfers for values above R$ 19.40.

These results strongly suggest that *BF* factors (intake, time of participation, and per capita cash) matter for school outcomes, but some caveats must be borne in mind. The relevance of *BF* effects varies across regions and probably across schools. However, the estimates reported in this study assume homogeneous effects across schools. Therefore, the results represent national (or regional) average effects that might differ in more specific contexts. The results are also estimated for schools as a whole, not for the group of beneficiaries in particular. As such, the estimated effects reported can be considered *average treatment effects* (*ATE*), that is, what is expected to happen, for instance, with schools' mean test scores not with beneficiaries' mean test scores in schools. The estimated effects would probably be higher if estimated for beneficiaries only [*average treatment on the treated effect* (ATTE)]; however, this was not possible with the available data. Finally, the validity of the results depends on the ability of the *conditional mean independence* assumption to hold up. In other words, validity depends on the assumption that schools used in the analysis are

265 Although the estimated coefficients for Mathematics had the same sign as those for Portuguese Language, showing the same trend, they were not statistically significant; therefore, no conclusion is possible with respect to *BF* influence on Mathematics.

266 When increased at the very beginning of participation in *BF*.

comparable once relevant differences are accounted for in the regression model. Schools are considered equivalent, on average, with respect to all characteristics that could affect outcomes and at the same time be correlated with *BF* factors whose effects are analysed using cross-sectional data. This last limitation (the comparability between schools) was addressed in a more robust analysis when I moved from cross-sectional to panel data analysis, as described next.

In chapter eight I took a step forward to circumvent the limitation imposed by the cross-sectional analysis by using two years of panel data (2005-2007) to estimate a *school-and-time fixed effects model* and to test the hypothesis of a positive change in school performance between those two years due to participation in *BF*. The initial analysis shows that the unadjusted school outcomes improved between 2005 and 2007, with those schools at the top of the *BF* intake distribution observing higher gains. As a consequence, the performance gap between schools in the first and fifth quintiles of *BF* intake diminished over the two years. However, school composition and resources also changed between those years, benefiting those schools with high levels of *BF* intake proportionally more. This is a positive result in itself, since the *regressive* feature of the education system mentioned earlier seems to be reversing; school resources, for instance, are improving more in schools with high proportions of beneficiaries. Therefore, the changes in resources and composition, inclusive in terms of *BF* intake, might be the driving force behind the improvements in school outcomes for schools on the top of *BF* intake distribution.

The estimation of a *school-and-time fixed effects model* allowed me to control for both time invariant school characteristics and for trends over time that were invariant across schools. In addition, a set of controls for school composition and resources were included in the model to account for possible school characteristics that might change across time and across schools. The panel analysis allowed the exam of how *BF* intake affects school outcomes and, more importantly, whether that effect was changing over time as a potential result of direct benefits from participation in *BF*. The results showed that *BF* intake influences all four school outcomes analysed. More importantly, the negative impact of *BF* intake on school outcomes was found to be *diminishing* between 2005 and 2007 for test scores in Portuguese Language, and became positive for pass-grade and dropout rates.

On average schools with higher proportions of beneficiaries improved more than schools with lower levels of *BF* participation. In Portuguese, the negative effect of 10 p.p. more beneficiaries in a school changed from -0.4 points on average to -0.1 between 2005 and 2007, a 75% reduction. The 10 p.p. more in *BF* intake was associated in 2005 with 0.15 p.p. *decrease*

in pass-grade rates, while in 2007 it was associated with a 0.26 p.p. *increase*; for dropout rates that change was from a 0.14 p.p. *increase* in 2005 to a 0.25 *decrease* in 2007 for each additional 10 p.p. in *BF* intake. The results differ across regions. An anomalous result was found in Mathematics, for which the effect of *BF* intake does not improve between 2005 and 2007, but instead becomes negative after being null in 2005. A possible explanation could be found by looking at how the effect of the two-year period affected test scores in Mathematics across different values of *BF* intake. It was shown that schools with high *BF* intakes improved between 2005 and 2007, although less than schools with low *BF* intakes. The hypothesis raised is that an omitted variable (possibly the National Mathematical Olympiad that started in 2005) benefitted schools at the bottom of the *BF* intake distribution more than schools at the top of the distribution. It is even possible that the reasons for the improvements observed for schools with proportionally fewer beneficiaries is different from those explaining improvements in schools with proportionally more beneficiaries. In this case, any possible gain from *BF* is probably overshadowed by those obtained by more advantaged schools. Apart from that anomaly, the results in general show marginal gains associated with *BF* intake over time, which potentially reflect direct contributions of *BF* to school outcomes.

Again, some caveats must be borne in mind. The panel analysis does not account for a pre/post policy change assessment; all changes are measured in relation to 2005, one year after *Bolsa Família* had started and four years after *Bolsa Escola* had been launched. Second, the positive changes observed in the marginal effect of *BF* intake might still be due to the overlapping of policies other than *BF* with schools or families assisted by *BF*. Nevertheless, that possibility is unlikely unless it is part of a national policy whose targeting mechanism was somehow connected to participation in *BF* and that could affect educational results nationally. There is no evidence that such a policy took place between those years. Finally, I could not test the two key variables (time of participation and per capita cash) in the panel analysis since these variables were not available for 2005. Nevertheless, the consistency of the results related to the marginal effect of *BF* intake between the cross-sectional and panel analyses strongly suggests that at least part of the improvements in school outcomes observed between 2005 and 2007 is a direct contribution of *BF* to the educational outcomes of economically disadvantaged children in Brazil.

Several policy implications can be considered from this set of findings, and several areas for further research can be identified, which I refer to in the next sections.

9.4 Policy implications

In addition to the policy recommendations for an effective cash transfer policy to achieve educational objectives mentioned in section 9.1, this thesis adds some other aspects to be considered by policy-makers in the Brazilian context. **First**, there is an urgent need to address inequality in standards of education supply in Brazil, as evidenced by the impoverished educational conditions of schools with high proportions of *BF* beneficiaries. By extension, *BF* participation by school can be used as a proxy for poverty incidence in schools and to target educational policies focused on improving equity in education systems. **Second**, given the major influence of pre-school participation on children's school outcomes and the clear disadvantage of *BF* recipient children in attending pre-school, special attention should be given to *BF* families in promoting access to pre-school programmes aside from the nutritional and health care assistance already promoted by the programme. **Third**, once per capita cash values seem to make a difference for children's educational outcomes, cash transfers should be tailored based not only on the number of children in the family, but also on the level of per capita poverty and on regional labour market conditions in order to achieve similar relative effects for children living in different circumstances. Regional adjustments in the benefit by proxy variables for the opportunity cost of having children engaged only in studying, such as labour market attractiveness (wage levels), incidence of child labour, and living cost indexes (e.g. basic food basket costs) are possible criteria to be considered. **Fourth**, the *substitute effect* between time of participation and the value of the benefit implies that the *BF cash-effect* on educational outcomes is maximised when families initially start in the programme. After three years, for instance, no benefit for test scores is expected from increases in cash, this threshold being lower (two years) for the poorest schools (with high *BF* intakes). Policy-makers might consider different readjustment policies based on time of participation and level of poverty, granting higher rates to the poorest families with a shorter time in the programme. **Fifth**, it is also possible that the *substitute effect* is reset between school cycles[267] (from primary to lower secondary for example), meaning that a higher benefit per child could be offered once they achieve the fifth grade and the first year of secondary school. This would work as an incentive to progress towards upper grades[268] and potentially maximise impacts on education outcomes. **Sixth**, the large majority of schools in 2007 could still benefit from increases in the value of cash paid to families (99% of the schools) and from families

267 The marginal effect of cash on school outcomes might pick up again after transition to an upper level of education. This hypothesis should be further investigated.

268 A similar scheme was operating in the first CCT programme in Brazil (Federal District 1995) as a savings account that beneficiaries could access after completing secondary school.

continuing for longer periods in the programme (90% of schools). Increases in the value of the benefit occurred on several occasions since 2007[269] and, based on the results of this thesis, probably contributed to improvements in educational outcomes in the large majority of schools.

These are some aspects that might be considered by policy-makers once attention is paid to the educational contributions of *BF* in Brazil. Nevertheless, given the multipurpose characteristics and large-scale coverage of the programme, policy-makers must consider how adjustments in the benefit scheme or in other aspects of the policy are communicated to the population, how they may affect families' perceptions and reactions to the programme, and how eventual changes may affect the programme's objectives other than education. This makes the task of adjusting *BF* a real challenge for policy-makers. However, greater progress for children's educational outcomes may rely on that task.

9.5 Final remarks and indications for further research

Throughout this thesis I have argued that the impact of poverty on children's educational opportunities can be, to some extent, mitigated by welfare and anti-poverty programmes. The analysis of the *BF* case in Brazil has shed some light on how this new policy initiative combining cash transfers with conditionalities on education, health, and nutrition can influence school outcomes and deliver the long-term policy objective of human capital accumulation for children of low-income families. Particularly, I addressed one expected effect so far not found in the literature, which I called the *missing link* to a future free of poverty: learning outcomes. I demonstrated that effects on learning outcomes are related to two parameters of the programme so far not explored by researchers: length of time of exposure and the value of cash transferred to families. Positive effects on learning outcomes were found even in schools of the fifth quintile of *BF* intake; the least resourced schools mostly attended by beneficiaries. In addition, pass-grade and dropout rates were also found to improve in association with those two parameters of *BF*.

Bolsa Família helps to improve educational outcomes by mitigating some of the multiple social disadvantages of beneficiary families that ultimately negatively impact children's outcomes. *BF* is expected to have a direct impact on household patterns of consumption and investment by changing household capacities, behaviours, and choices that can benefit children's health, nutrition, and education. Children's education directly benefits

269 June 2008, July 2009, March 2011, and June 2012.

from healthier and nourished children, from the level of support received from less stressed parents, and from better material and learning conditions at home, which eventually impact children's school performance. In addition *BF* can compensate for direct and indirect costs of education for parents, reduce the incidence and intensity of child labour (which favours more study hours), and help households to manage risks by reducing the incidence of negative coping strategies that can negatively affect children's education. Resource allocation within the household tends to change to benefit children also, because women are empowered as the main recipients of the benefit and tend to make choices in the best interests of the whole family, particularly children. Conditionalities attached to the programme can also impact children's education by creating incentives for enrolment at the proper age and for health and nutritional monitoring, by requiring a minimum attendance (that also contributes to reductions in child labour), and by bringing to school those who otherwise would not be enrolled.

How do *BF* costs compare to its educational benefits? The multiple effects expected from *BF* relates to its capacity of increasing family income and of linking families to social assistance, health, and educational services. To produce a cost-effectiveness analysis of *BF* is not an easy task given the multiplicity of its expected effects, the mobilisation of social services beyond the *BF* budget, and the educational and social costs associated with not intervening to rescue families from conditions of extreme poverty. In 2011 the *BF* budget was equivalent to 0.39% of the GDP and in June 2012 it has raised to 0.46% of the GDP with the creation of an extra benefit for families living below the extreme-poverty line with children aged 0 to 6[270]. It is still a relatively low-cost programme given its scale, and it is compatible with the Brazilian budgetary capacity. In addition, the good targeting, the low risk of deviations (by the use of an electronic card), and the multiple benefits to families contribute to making *BF* one of the most successful programmes in fighting poverty and in promoting social development in the world. By adding the results of this thesis to the now vast literature on CCTs, it can be argued that far from representing an opportunity cost for educational policies, CCTs can be potential allies in creating educational opportunities for those whose access to the benefits of education depends on more than the provision of schools (even good ones).

Research can look further into the case of *Bolsa Família* to investigate the effects on test scores for eighth grade pupils. Is cash transfer still significant for school outcomes once beneficiaries reach the upper grades? Also, by studying interactions between *BF* participation and school characteristics, including teachers' practices, researchers would be able to make

270 It is part of the new initiative called *Caring Brazil* to support extreme poor families with children at pre-school age.

further conclusions about how relevant school factors are in adding value to the benefits of cash transfers on school outcomes. Another relevant extension of research would be an investigation of the impacts of *BF* on transition rates (fourth to fifth grade and eighth grade to the first year of secondary school). Improvements in test scores, reductions of dropout rates, and increases in pass-grade rates, as reported in this thesis, do not guarantee that transition to fifth grade is improving as a consequence of participation in the programme. In addition, studies on the potential effects of *BF* on completion rates are missing in the literature, both for compulsory education[271] and secondary school. These outcomes are important pathways towards the accumulation of human capital and for increasing the opportunities of children to escape poverty in the future.

271 I consider here the compulsory period of nine years, prior to the most recent change in the Constitution, which will come into force in 2016.

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APPENDIX Chapter 6: Tables and Graphs

Table A - 1: Student' characteristics (I)

Demographic Characteristics		Are you in *Bolsa Família*?		(B) – (A) p.p.	Significance level(1)
		No (A)	Yes (B)		
Region	North	9.7%	11.9%	2.2	**
	Northeast	18.5%	31.3%	12.8	**
	Centre-West	9.8%	8.3%	-1.5	**
	Southeast	41.4%	32.7%	-8.7	**
	South	20.6%	15.7%	-4.9	**
Gender	Female	51.0%	47.9%	-3.1	**
	Male	49.0%	52.1%	3.1	**
How do you consider yourself?	White	41.1%	30.9%	-10.2	**
	Mixed-Race	45.2%	49.5%	4.3	**
	Black	7.8%	11.7%	3.9	**
	Asian	2.3%	3.5%	1.2	**
	Indigenous	3.5%	4.4%	0.9	*
How old are you?	8 years-old or less	0.6%	0.9%	0.3	-
	9 years-old	5.1%	4.1%	-1.0	*
	10 years-old	52.9%	36.9%	-16.0	**
	11 years-old	24.7%	30.7%	6.0	**
	12 years-old	8.4%	14.5%	6.1	**
	13 years-old	4.1%	6.9%	2.8	**
	14 years-old	2.2%	3.9%	1.7	**
	15 years-old or more	2.1%	2.1%	0.0	-

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

Table A - 2: Student' characteristics (II)

Students' school history		Are you in *Bolsa Família*?		(B) – (A) p.p.	Significance level(1)
		No (A)	Yes (B)		
Overage	No	91.7%	87.1%	-4.6	**
	Yes	8.3%	12.9%	4.6	**
At which grade did you begin school?	Kindergarten	38.5%	32.5%	-6.0	**
	Pre-school	40.2%	39.0%	-1.2	-
	1st grade	21.3%	28.6%	7.3	**
Have you been studying at this school since 1st grade?	Yes	57.0%	60.0%	3.0	**
	No, but always in public schools	29.7%	30.3%	0.6	-
	No, I studied in private schools	13.4%	9.7%	-3.7	**
Have you ever failed a grade?	No	75.6%	60.2%	-15.4	**
	Yes, once	17.7%	29.1%	11.4	**
	Yes, twice or more	6.7%	10.7%	4.0	**
Have you ever abandoned school during the academic year?	No	92.6%	89.5%	-3.1	**
	Yes, once	5.8%	8.0%	2.2	**
	Yes, twice or more	1.6%	2.5%	0.9	**
Do you feel neglected in the classroom?	Many times	13.8%	22.3%	8.5	**
	Once in a while	30.7%	28.8%	-1.9	*
	Never or rarely	55.5%	49.0%	-6.5	**
(1) – Not significant, * significant at 5% level; ** significant at 1% level.					

Table A - 3: Student' characteristics (III)

Students' habits and expectations		Are you in *Bolsa Família*?		(B) – (A) p.p.	Significance level(1)
		No (A)	Yes (B)		
In a school day, how many hours do you watch TV?	Don`t watch	14.1%	18.0%	3.9	**
	Up to 1h	29.7%	31.9%	2.2	*
	1h to 3h	17.5%	14.5%	-3.0	**
	3h to 4h	10.6%	9.8%	-0.8	-
	More than 4h	28.1%	25.7%	-2.4	**
In a school day, how many hours do you spend in domestic work?	Don`t do	32.7%	29.6%	-3.1	**
	Up to 1h	37.7%	37.9%	0.2	-
	1h to 2h	14.5%	15.0%	0.5	-
	2h to 3h	6.3%	7.2%	0.9	-
	More than 3h	8.7%	10.3%	1.6	**
Do you work?	No	87.8%	80.8%	-7.0	**
	Yes	12.2%	19.2%	7.0	**
Do you do your Portuguese Language homework?	Always or almost always	76.7%	72.5%	-4.2	**
	Once in a while	16.4%	19.1%	2.7	**
	Never or rarely	2.8%	3.7%	0.9	**
	There is no homework	4.1%	4.7%	0.6	-
Do you do your Mathematics homework?	Always or almost always	79.7%	75.6%	-4.1	**
	Once in a while	15.9%	18.7%	2.8	**
	Never or rarely	1.6%	2.9%	1.3	**
	There is no homework	2.8%	2.9%	0.1	-
When you finish fundamental school you intend to:	study only	25.0%	27.7%	2.7	**
	work only	7.6%	8.3%	0.7	-
	study and work	36.7%	32.9%	-3.8	**
	Don`t know	30.7%	31.2%	0.5	-

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

Table A - 4: Household Characteristics (I)

Material Environment		Are you in *Bolsa Família*?		(B) – (A) p.p.	Significance level(1)
		No (A)	Yes (B)		
Power supply at home?	No	2.1%	3.7%	1.6	**
	Yes	97.9%	96.3%	-1.6	**
Piped water supply at home?	No	7.6%	9.3%	1.7	**
	Yes	92.4%	90.7%	-1.7	**
Color TV at home?	Yes, ONE	50.5%	64.2%	13.7	**
	Yes, MORE than ONE	43.6%	26.2%	-17.4	**
	No	5.9%	9.6%	3.7	**
Radio at home?	Yes, ONE	54.1%	60.8%	6.7	**
	Yes, MORE than ONE	38.4%	29.0%	-9.4	**
	No	7.5%	10.3%	2.8	**
Video at home?	No	55.1%	70.0%	14.9	**
	Yes	44.9%	30.0%	-14.9	**
Bathroom at home?	Yes, ONE	69.9%	79.8%	9.9	**
	Yes, MORE than ONE	28.1%	16.6%	-11.5	**
	No	2.0%	3.6%	1.6	**
Bedrooms at home?	Yes, ONE	12.9%	15.0%	2.1	**
	Yes, TWO	39.2%	40.6%	1.4	-
	Yes, THREE	33.4%	29.8%	-3.6	**
	Yes, FOUR or MORE	13.7%	13.0%	-0.7	-
	No	.7%	1.5%	0.8	**
Fridge at home?	Yes, ONE	83.5%	79.0%	-4.5	**
	Yes, MORE than ONE	10.6%	8.4%	-2.2	**
	No	5.9%	12.6%	6.7	**
Freezer at home?	No	63.0%	73.2%	10.2	**
	Yes	37.0%	26.8%	-10.2	**
Washing Machine at home?	No	37.9%	52.1%	14.2	**
	Yes	62.1%	47.9%	-14.2	**
Vacuum Cleaner at home?	No	77.2%	84.7%	7.5	**
	Yes	22.8%	15.3%	-7.5	**
Does your family have a CAR?	Yes, ONE	37.0%	25.9%	-11.1	**
	Yes, MORE than ONE	14.6%	10.1%	-4.5	**
	No	48.4%	64.0%	15.6	**

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

Table A - 5: Household Characteristics (II)

Learning Environment		Are you in *Bolsa Família*?		(B) – (A) p.p.	Significance level(1)
		No (A)	Yes (B)		
Dictionary at home?	No	9.4%	12.6%	3.2	**
	Yes	90.6%	87.4%	-3.2	**
Bookshelf with more than 20 books at home?	No	54.9%	59.1%	4.2	**
	Yes	45.1%	40.9%	-4.2	**
Peaceful place for studying at home?	No	15.1%	16.1%	1.0	-
	Yes	84.9%	83.9%	-1.0	-
Computer at home?	Yes, WITH internet	17.8%	8.4%	-9.4	**
	Yes, WITHOUT internet	7.1%	5.0%	-2.1	**
	No	75.1%	86.6%	11.5	**
(1) – Not significant, * significant at 5% level; ** significant at 1% level.					

Table A - 6: Household Characteristics (III)

Family Structure		Are you in *Bolsa Família*?		(B) – (A) p.p.	Significance level(1)
		No (A)	Yes (B)		
How many people live with you?	Alone or with one person	2.8%	2.6%	-0.2	-
	With 2 people	12.2%	7.0%	-5.2	**
	With 3 people	24.0%	16.8%	-7.2	**
	With 4 or 5 people	40.9%	42.9%	2.0	*
	With 6 to 8 people	14.7%	21.0%	6.3	**
	With more than 8 people	5.4%	9.7%	4.3	**
With whom do you live?	Mother and Father	69.0%	67.3%	-1.7	-
	Mother only	18.2%	20.2%	2.0	**
	Father only	3.1%	3.2%	0.1	-
	Neither Mother nor Father, with other people	9.8%	9.3%	-0.5	-
(1) – Not significant, * significant at 5% level; ** significant at 1% level.					

Table A - 7: Household Characteristics (IV)

Parents' Characteristics		Are you in *Bolsa Família*?		(B) – (A) p.p.	Significance level(1)
		No (A)	Yes (B)		
Which grade does your mother or female carer completed?	No schooling or less than 4th grade	8.7%	13.6%	4.9	**
	4th grade	25.0%	32.5%	7.5	**
	8th grade	14.0%	12.9%	-1.1	-
	Secondary School	16.9%	10.0%	-6.9	**
	College	10.3%	6.7%	-3.6	**
	Don`t know	25.2%	24.5%	-0.7	-
Which grade does your father or male carer completed?	No schooling or less than 4th grade	7.9%	12.2%	4.3	**
	4th grade	19.7%	24.6%	4.9	**
	8th grade	12.6%	11.9%	-0.7	-
	Secondary School	14.0%	8.0%	-6.0	**
	College	10.4%	7.6%	-2.8	**
	Don`t know	35.4%	35.7%	0.3	-
How often do your parents or care givers attend school meetings?	Almost always or always	61.3%	56.8%	-4.5	**
	Once in a while	31.7%	35.2%	3.5	**
	Never or rarely	7.1%	8.0%	0.9	-
Are you incentivised to study by your parents or care givers?	No	4.0%	5.3%	1.3	**
	Yes	96.0%	94.7%	-1.3	**
Are you incentivised to do your homework by your parents or care givers?	No	6.2%	8.0%	1.8	**
	Yes	93.8%	92.0%	-1.8	**
Are you incentivised to read by your parents or care givers?	No	12.9%	14.8%	1.9	**
	Yes	87.1%	85.2%	-1.9	**
Are you incentivised to go to school by your parents or care givers?	No	4.8%	6.9%	2.1	**
	Yes	95.2%	93.1%	-2.1	**
(1) – Not significant, * significant at 5% level; ** significant at 1% level.					

Table A - 8: School Characteristics (I)

School Infra-structure		Are you in *Bolsa Família*?		(B) – (A)	Significance level[(1)]
		No (A)	Yes (B)		
School size [(2)]	Mean	12.5	11.7	-0.8	**
Facility Index	Mean	0.31	0.28	-0.03	**
Equipment Index	Mean	0.49	0.45	-0.04	**
Library/Reading room	No	31.1%	36.4%	5.3 p.p.	**
	Yes	68.9%	63.6%	-5.3 p.p.	**
Computer room	No	62.6%	71.5%	8.9 p.p.	**
	Yes	37.4%	28.5%	-8.9 p.p.	**
Laboratory	No	87.1%	91.2%	4.1 p.p.	**
	Yes	12.9%	8.8%	-4.1 p.p.	**

(1) – Not significant, * significant at 5% level; ** significant at 1% level.
(2) Given by the average number of classrooms in use.

Table A - 9: School Characteristics (II)

School Organisation		Are you in *Bolsa Família*?		(B) – (A)	Significance level(1)
		No (A)	Yes (B)		
Student-teacher ratio	Mean	27.1	27.5	0.4	*
Class size	Mean	29.5	29.3	-0.2	*

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

Table A - 10: School Characteristics (III)

Teachers' Characteristics		Are you in *Bolsa Família*?		(B) – (A) p.p.	Significance level(1)
		No (A)	Yes (B)		
Teachers' education					
Primary Education		0.2%	0.4%	0.2	*
Secondary Education		40.1%	46.9	6.8	**
Higher Education		59.6%	52.6%	-7.0	**
Teacher and Homework					
Does your teacher correct the Portuguese Language homework?	Always or almost always	77.7%	74.6%	-3.1	**
	Once in a while	16.7%	18.0%	1.3	-
	Never or rarely	1.8%	3.4%	1.6	**
	There is no homework	3.8%	4.0%	0.2	-
Does your teacher correct the Mathematics homework?	Always or almost always	80.2%	77.7%	-2.5	**
	Once in a while	15.5%	16.7%	1.2	-
	Never or rarely	1.7%	2.8%	1.1	**
	There is no homework	2.6%	2.8%	0.2	-

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

Table A - 11: Two-sample t-test with unequal variances for differences in Mathematics test scores between *BF* and non-*BF* for the 4th grade

BF Group	Observations	Mean	Std. Err.	Std. Dev.	[95% Conf. Interval]	
No	7417	**191.6**	.470	40.49	190.6	192.5
Yes	3788	**175.4**	.593	36.52	174.2	176.5
Combined	11205	186.1	.377	39.94	185.3	186.8
Diff		**16.2**	.757		14.7	17.7
diff = mean(No) - mean(Yes)					t = 21.41	
Ho: diff = 0					Satterthwaite's degrees of freedom = 8353.67	
Ha: diff < 0			Ha: diff != 0		Ha: diff > 0	
Pr(T < t) = 1.0000			Pr(|T| > |t|) = 0.0000		Pr(T > t) = 0.0000	

Table A - 12: Two-sample t-test with unequal variances for differences in PORTUGUESE test scores between *BF* and non-*BF* for the 4th grade

BF Group	Observations	Mean	Std. Err.	Std. Dev.	[95% Conf. Interval]	
No	7417	**185.6**	.496	42.71	184.7	186.6
Yes	3788	**166.0**	.612	37.65	164.8	167.2
Combined	11205	179.0	.398	42.11	178.2	179.8
Diff		**19.7**	.787		18.1	21.2
diff = mean(No) - mean(Yes)					t = 25.00	
Ho: diff = 0					Satterthwaite's degrees of freedom = 8521.63	
Ha: diff < 0			Ha: diff != 0		Ha: diff > 0	
Pr(T < t) = 1.0000			Pr(T > t) = 0.0000		Pr(T > t) = 0.0000	

A- Graph 1: Mean Household Income Per Capita by Durable Goods (PNAD 2005)

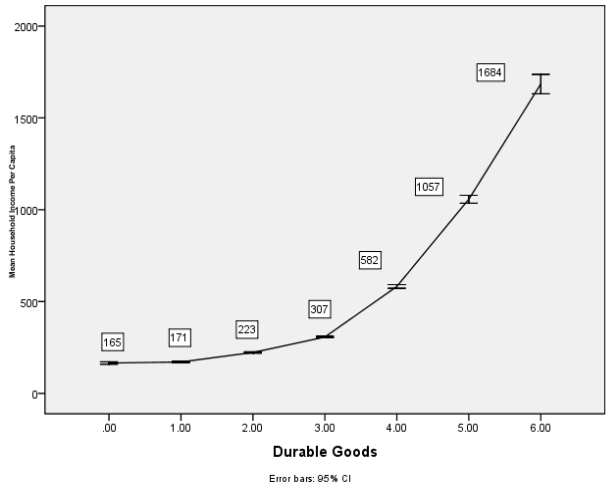
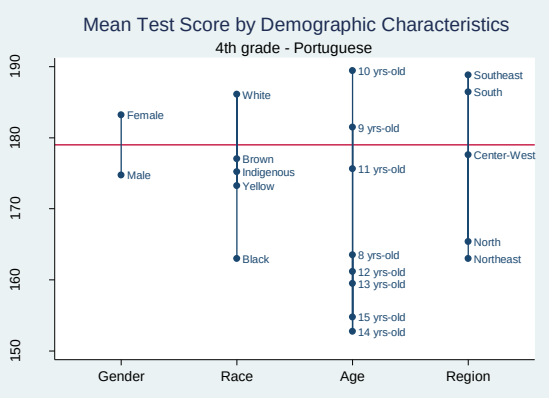


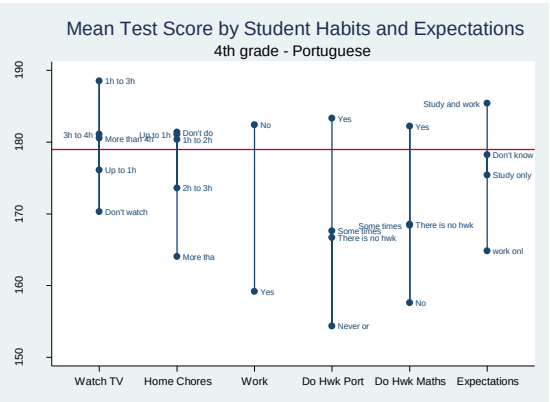
Table A - 13: Summary statistics for household per capita income by durable goods (PNAD 2005)

		Household Per Capita Income (v4621)				
		Valid N	Mean (R$)	Standard Error of Mean	Standard Deviation	Median
Durable Goods	0.00	3,121	165.04	3.30	184.31	108.00
	1.00	8,405	170.87	1.98	181.69	120.00
	2.00	16,051	222.67	2.07	262.70	150.00
	3.00	48,941	306.73	1.92	424.84	213.00
	4.00	25,406	582.08	5.22	832.42	366.00
	5.00	15,521	1,057.29	10.99	1,369.19	645.00
	6.00	6,854	1,683.66	26.83	2,221.21	1,070.50

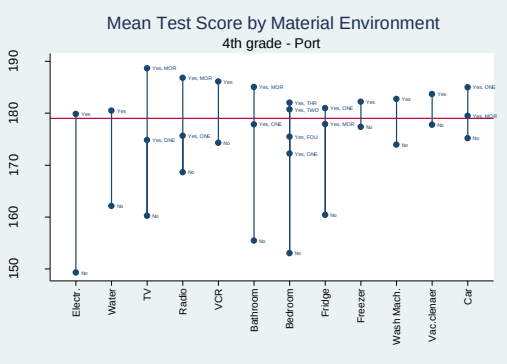
A- Graph 2: Mean test score (Port) by students' demographic characteristics



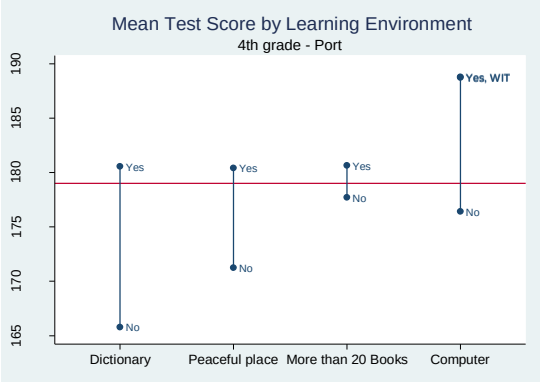
A- Graph 3: Mean test score (Port) by students' habits and expectations



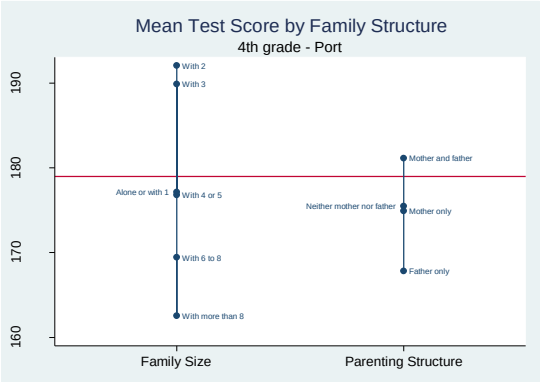
A- Graph 4: Mean test score (Port) by households' material environment



A- Graph 5: Mean test score (Port) by households' learning environment



A- Graph 6: Mean test score (Port) by family structure



A- Graph 7: Mean test score (Port) by parents' characteristics

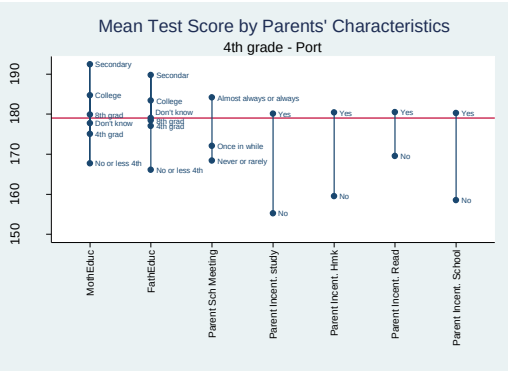


Table A - 14: Students' characteristics

Students' Characteristics		Are you in *Bolsa Família*?		(B) – (A) p.p.	Significance level(1)
		No (A)	Yes (B)		
Demographic Characteristics					
Male	Yes	49.0%	52.1%	3.1	**
Non-white	Yes	58.9%	69.1%	10.2	**
Region	North	9.7%	11.9%	2.2	**
	Northeast	18.5%	31.3%	12.8	**
	Centre-West	9.8%	8.3%	-1.5	**
	Southeast	41.4%	32.7%	-8.7	**
	South	20.6%	15.7%	-4.9	**
School History					
Overage	Yes	8.3%	12.9%	4.6	**
Student did pre-school	Yes	78.7%	71.4%	-7.3	**
Student in the same school since 1st grade	Yes	57.0%	60.0%	3.0	**
Ever failed	Yes	24.4%	39.8%	15.4	**
Ever dropped out	Yes	7.4%	10.5%	3.1	**
Students feeling neglected in the classroom	Yes	44.4%	51.0%	6.6	**
Habits and Expectations					
Watch TV	Yes	85.9%	82.0%	-3.9	**
Do Domestic Work 2h or more a day	Yes	15.0%	17.5%	2.5	**
Do you work?	Yes	12.2%	19.2%	7.0	**
Do homework Port. at least once in a while	Yes	93.0%	91.6%	-1.4	**
Do homework Maths at least once in a while	Yes	95.6%	94.2%	-1.4	**
Intend to study after concluding primary school	Yes	61.7%	60.5%	-1.2	-

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

Table A - 15: Households' characteristics

Households' Characteristics		Are you in *Bolsa Família*?		(B) – (A)	Significance level(1)
		No (A)	Yes (B)		
Material Environment					
Power supply at home?	Yes	97.9%	96.3%	-1.6	**
Tap water at home?	Yes	92.5%	90.7%	-1.8	**
At least one bedroom in the house?	Yes	99.3%	98.5%	-0.8	**
At least one bathroom in the house?	Yes	98.0%	96.4%	-1.6	**
Durable Goods Index	Mean	0.57	0.48	-0.09	**
Dictionary at home?	Yes	90.6%	87.4%	-3.2	**
Peaceful place for studying at home?	Yes	84.9%	83.9%	-1.0	-
Bookshelves with more than 20 books at home?	Yes	45.1%	40.9%	-4.2	**
Computer	Yes	24.9%	13.4%	-11.5	**
Family Structure					
Large family (7 or more)	Yes	20.1%	30.7%	10.6	**
Stable family (Father and Mother present)	Yes	69.0%	67.3%	-1.7	-
Parents' Characteristics					
Mother with post-primary education	Yes	36.3%	22.0%	-14.3	**
Father with post-primary education	Yes	37.7%	24.2%	-13.5	**
Parents often attend school meetings	Yes	61.3%	56.8%	-4.5	**
Parents encouragement for children's education (0-4)	Mean	3.72	3.65	-0.07	**
(1) – Not significant, * significant at 5% level; ** significant at 1% level.					

Table A - 16: Schools' Characteristics

Schools' Characteristics		Are you in *Bolsa Família*?		(B) – (A)	Significance level(1)
		No (A)	Yes (B)		
School Infra-Structure					
School size [(2)]	Mean	12.5	11.7	-0.8	**
Facility Index	Mean	0.31	0.28	-0.03	**
Library/Reading room	Yes	68.9%	63.6%	-5.3	**
Computer room	Yes	37.4%	28.5%	-8.9	**
Laboratory	Yes	12.9%	8.8%	-4.1	**
Equipment Index	Mean	0.49	0.45	-0.04	**
School Organisation					
Student/Teacher ratio (1^{st} - 4^{th} grades)	Mean	27.1	27.5	0.4	*
Teachers' Characteristics					
Proportion of teacher with higher education (1^{st} - 4^{th} grades)	Mean	0.60	0.53	-0.07	**
Teacher always correct homework Port	Yes	77.7%	74.6%	-3.1	**
(1) Not significant, * significant at 5% level; ** significant at 1% level. (2) Given by the average number of classrooms in use.					

Table A - 17: Portuguese test scores and groups of characteristics (students, households and schools)

	VARIABLES	Students' Characteristics				Household Charact.			School Characteristics		Region
		(1) Port	(2) Port	(3) Port	(4) Port	(5) Port	(6) Port	(7) Port	(8) Port	(9) Port	(10) Port
Demographic Characteristics	***Bolsa Família***	**-19.7*** **	**-18.5*** **	**-14.1*** **	**-13.0*** **	**-12.0*** **	**-11.6*** **	**-11.0*** **	**-10.5*** **	**-10.5*** **	**-10.2*** **
		(0.787)	(0.784)	(0.761)	(0.752)	(0.757)	(0.757)	(0.754)	(0.750)	(0.748)	(0.745)
	Male		-7.8***	-5.7***	-4.8***	-5.4***	-5.5***	-5.4***	-5.5***	-5.6***	-5.6***
			(0.768)	(0.736)	(0.738)	(0.736)	(0.734)	(0.728)	(0.725)	(0.724)	(0.719)
	Non-white		-9.5***	-6.3***	-6.0***	-5.0***	-4.6***	-4.5***	-4.1***	-3.8***	-3.1***
			(0.809)	(0.771)	(0.757)	(0.761)	(0.760)	(0.755)	(0.753)	(0.755)	(0.759)
School History	Overage			-7.4***	-4.7***	-3.6***	-3.1**	-2.6**	-1.6	-1.4	-0.6
				(1.266)	(1.261)	(1.265)	(1.272)	(1.266)	(1.269)	(1.268)	(1.261)
	Pre-school			11.9***	10.8***	9.7***	9.2***	8.5***	8.5***	8.3***	8.0***
				(0.831)	(0.823)	(0.823)	(0.824)	(0.821)	(0.816)	(0.816)	(0.813)
	Same school			-0.5	-0.3	-0.7	-0.8	-0.9	-1.4*	-1.6**	-2.1***
				(0.745)	(0.733)	(0.727)	(0.726)	(0.721)	(0.718)	(0.717)	(0.716)
	Fail			-17.3***	-16.2***	-15.0***	-14.5***	-13.7***	-13.5***	-13.3***	-12.7***
				(0.827)	(0.816)	(0.813)	(0.815)	(0.811)	(0.807)	(0.808)	(0.805)
	Dropout			-7.1***	-4.7***	-3.7***	-3.5***	-2.1	-1.9	-1.6	-1.4
				(1.335)	(1.321)	(1.316)	(1.319)	(1.311)	(1.313)	(1.314)	(1.314)
	Neglected			-10.4***	-9.1***	-8.8***	-8.7***	-8.2***	-8.1***	-7.8***	-7.5***
				(0.733)	(0.724)	(0.719)	(0.717)	(0.712)	(0.709)	(0.709)	(0.706)
Habits and Expectations	Watch-TV				6.2***	4.7***	4.6***	4.6***	4.5***	4.5***	4.4***
					(0.990)	(0.997)	(0.995)	(0.986)	(0.980)	(0.978)	(0.972)
	Domestic work (2h or more)				-9.1***	-8.6***	-8.3***	-8.0***	-7.9***	-7.8***	-7.8***
					(0.965)	(0.957)	(0.954)	(0.947)	(0.942)	(0.940)	(0.936)
	Child Labour				-13.3***	-13.1***	-13.0***	-12.4***	-12.1***	-12.0***	-11.6***
					(1.003)	(0.999)	(0.997)	(0.992)	(0.985)	(0.985)	(0.978)
	Do Homework Port.				10.0***	9.0***	8.6***	7.6***	7.5***	6.6***	6.8***
					(1.415)	(1.401)	(1.397)	(1.381)	(1.377)	(1.402)	(1.399)
	Do Homework Maths				3.4*	3.3*	3.3*	2.3	2.2	1.4	1.5
					(1.813)	(1.790)	(1.781)	(1.765)	(1.763)	(1.781)	(1.770)
	Intend to continue studies				3.6***	2.9***	2.9***	2.0***	2.0***	1.9***	2.0***
					(0.735)	(0.731)	(0.729)	(0.726)	(0.722)	(0.721)	(0.717)
Material Environment	Electricity					11.4***	11.0***	9.3***	9.2***	9.1***	9.4***
						(2.217)	(2.209)	(2.192)	(2.184)	(2.185)	(2.171)
	Tap Water					8.1***	7.7***	6.9***	6.0***	5.6***	5.2***
						(1.257)	(1.254)	(1.247)	(1.241)	(1.244)	(1.248)
	Bedroom					10.4***	10.2***	9.4***	9.5***	9.1***	8.2**
						(3.607)	(3.577)	(3.510)	(3.431)	(3.419)	(3.410)
	Bathroom					7.1***	7.1***	6.8***	6.0***	5.6***	4.9**
						(2.139)	(2.134)	(2.100)	(2.099)	(2.110)	(2.108)
	Dictionary					6.1***	6.2***	5.0***	5.0***	5.1***	5.4***
						(1.144)	(1.139)	(1.134)	(1.132)	(1.131)	(1.122)
	Peaceful place to study					1.7*	1.3	-0.3	-0.1	-0.3	-0.3
						(1.008)	(1.006)	(1.008)	(1.008)	(1.009)	(1.003)
	Books ≥ 20					-1.8**	-1.4*	-2.2***	-1.0	-0.6	1.2
						(0.802)	(0.802)	(0.796)	(0.799)	(0.801)	(0.815)
	Computer					3.5***	3.5***	1.9*	2.2**	2.3**	2.9***
						(1.102)	(1.099)	(1.109)	(1.101)	(1.101)	(1.099)
	Goods (0-10)					0.6***	0.5**	0.2	-0.5*	-0.5**	-1.2***
						(0.236)	(0.236)	(0.237)	(0.242)	(0.242)	(0.250)
Family Structure	Family size (≥ 7)						-6.1***	-5.7***	-5.2***	-5.0***	-4.8***
							(0.830)	(0.826)	(0.825)	(0.825)	(0.825)
	Both parents						2.0**	1.8**	2.2***	2.1***	2.1***
							(0.770)	(0.767)	(0.765)	(0.764)	(0.759)
Parents' Characteristics	Mother Educ. (post-primary)							5.8***	5.9***	5.8***	6.5***
								(1.002)	(0.992)	(0.993)	(0.991)
	Father Educ. (post-primary)							1.6*	1.4	1.5	1.7*
								(0.994)	(0.987)	(0.988)	(0.985)
	Parents in school meetings							5.0***	4.8***	4.6***	4.2***
								(0.726)	(0.723)	(0.726)	(0.725)
	Parents encouragem. (0-4)							4.6***	4.3***	4.2***	4.0***
								(0.479)	(0.477)	(0.476)	(0.476)
School Infra-Structure	Facilities (0.0 - 1.0)								12.5***	9.9**	2.4
									(3.812)	(3.868)	(3.956)
	Equipment (0.0 – 1.0)								22.2***	20.0***	12.7***
									(2.810)	(2.869)	(2.930)
Teachers	Student/teacher ratio									-0.2***	-0.0
										(0.043)	(0.045)
	Prop. teachers HE									3.4***	1.5
										(1.143)	(1.169)
	Teacher correct homework (Port.)									2.7***	2.8***
										(0.848)	(0.844)
Region	North										-10.7***
											(1.368)
	Northeast										-12.3***
											(1.122)
	Central-West										-7.1***
											(1.308)
	South										-0.5
											(1.015)
	Constant	185.7***	195.1***	193.1***	174.8***	131.1***	132.9***	121.7***	112.0***	118.5***	129.2***
		(0.496)	(0.778)	(1.153)	(2.380)	(4.828)	(4.821)	(4.847)	(4.857)	(5.058)	(5.137)
	Observations	11,204	11,204	11,204	11,204	11,204	11,204	11,204	11,204	11,204	11,204
	R-squared	**0.049**	**0.070**	**0.162**	**0.191**	**0.206**	**0.210**	**0.224**	**0.232**	**0.235**	**0.244**

Robust standard errors in parentheses
Level of Statistical Significance: *** p<0.01, ** p<0.05, * p<0.1

APPENDIX Chapter 7: Tables and Figures

Table A - 18: Number of schools, students, beneficiaries and students in the exam by region

Region	Schools		Enrolments 4th grade		Students sitting for the 2007 exam		Number of Beneficiaries	
	N	%	N	%	N	%	N	%
North	3142	8.9%	250718	9.6%	204960	9.3%	98541	11.2%
Northeast	10103	28.7%	628202	24.0%	488374	22.1%	318766	36.3%
Central West	2933	8.3%	195977	7.5%	168482	7.6%	56376	6.4%
Southeast	12508	35.6%	1162473	44.3%	1018905	46.1%	304095	34.6%
South	6486	18.4%	384057	14.7%	330327	14.9%	100872	11.5%
Total	35172	100.0%	2621427	100.0%	2211048	100.0%	878650	100.0%

Table A - 19: Number of schools, students, beneficiaries and students in the exam by school proportion of *BF* intake

BF intake (%)	Schools		Enrolments 4th grade		Students sitting for the 2007 exam		Number of Beneficiaries	
	N	%	N	%	N	%	N	%
0|--20	8490	24.1%	775723	29.6%	676779	30.6%	90220	10.3%
20|--40	12014	34.2%	942152	35.9%	788868	35.7%	277383	31.6%
40|--60	9050	25.7%	600605	22.9%	494959	22.4%	292280	33.3%
60|--80	4323	12.3%	238471	9.1%	197107	8.9%	162246	18.5%
80|--|100	1295	3.7%	64476	2.5%	53335	2.4%	56521	6.4%
Total	35172	100.0%	2621427	100.0%	2211048	100.0%	878650	100.0%

Table A - 20: *Bolsa Família* characteristics by school proportion of *BF* intake

BF intake (%)	Number of Beneficiaries		Cash Transfer to Households		Per Capita Cash Transfer		Months of participation in *BF*		*%BF*	
	N	%	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.
0|--20	90220	10.3%	75.84	17.11	16.57	4.13	27.58	8.21	11.56	5.29
20|--40	277383	31.6%	80.09	12.99	16.79	2.84	28.39	5.86	29.70	5.74
40|--60	292280	33.3%	85.37	11.54	17.60	2.51	29.46	5.59	48.97	5.71
60|--80	162246	18.5%	89.85	9.90	18.14	2.26	30.59	5.58	68.14	5.59
80|--|100	56521	6.4%	90.94	9.10	18.22	2.16	31.08	5.76	87.95	5.98
Total	878650	100.0%	82.06	14.17	17.17	3.10	28.85	6.48	37.15	21.39

Table A - 21: *Bolsa Família* household characteristics by school proportion of *BF* intake

BF intake (%)	*BF* Household Income		*BF* Household Per Capita Income		*BF* Household Size	
	Mean	s.d.	Mean	s.d.	Mean	s.d.
0|--20	252.31	90.97	54.98	17.70	4.66	.80
20|--40	243.93	69.61	52.11	14.15	4.81	.58
40|--60	219.70	63.91	46.55	13.35	4.88	.55
60|--80	195.87	58.12	40.85	12.20	4.99	.59
80|--|100	186.97	56.69	38.80	12.00	5.04	.62
Total	231.58	75.10	49.46	15.43	4.82	.65

Table A - 22: School composition as to pupils' characteristics by school proportion of *BF* intake

BF intake (%)	Prop. of boys		Prop. of non-whites		Prop. overage		Prop. who have failed any grade		Prop. who attended pre-school		Prop. in child labour	
	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.
0|--20	.50	.08	.57	.19	.14	.13	.24	.14	.78	.13	.11	.08
20|--40	.50	.10	.65	.17	.22	.15	.33	.15	.74	.14	.14	.08
40|--60	.50	.11	.70	.16	.28	.16	.39	.16	.71	.14	.17	.09
60|--80	.50	.11	.72	.15	.31	.16	.42	.16	.70	.15	.19	.10
80|--|100	.50	.11	.72	.14	.32	.16	.44	.16	.70	.16	.19	.11
Total	.50	.10	.65	.18	.23	.16	.34	.17	.74	.14	.15	.09

Table A - 23: School composition as to households' characteristics by school proportion of *BF* intake

BF intake (%)	Prop. whose mother has post-primary education		Prop. whose father has post-primary education		Durable Goods Index		Prop. living in female headed households		Prop. living in large families (7 or more)	
	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.
0|--20	.39	.17	.41	.18	.64	.11	.23	.10	.19	.10
20|--40	.29	.15	.32	.16	.56	.10	.24	.11	.23	.12
40|--60	.24	.14	.26	.15	.49	.10	.24	.12	.28	.13
60|--80	.21	.14	.22	.15	.44	.09	.24	.11	.31	.13
80|--|100	.21	.14	.20	.15	.43	.09	.23	.11	.32	.14
Total	.29	.16	.31	.18	.54	.12	.24	.11	.25	.13

Table A - 24: School characteristics by school proportion of *BF* intake (I)

BF intake (%)	Classrooms in use		Class Size		School Day (min)		% HE teachers		Computers available	
	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.
0|--20	11	6	28.79	6.17	269.10	46.85	77.90	29.58	11	10
20|--40	11	6	28.21	6.57	263.98	45.41	70.99	34.41	9	9
40|--60	10	6	27.53	6.76	257.20	38.28	64.76	38.04	7	7
60|--80	9	6	26.76	7.14	253.66	32.80	58.73	40.59	6	7
80|--|100	9	5	26.51	7.57	252.24	26.56	58.39	41.87	5	6
Total	11	6	27.94	6.68	261.77	42.44	69.09	36.01	9	9

Table A - 25: School characteristics by school proportion of *BF* intake (II)

BF intake (%)	Internet	School Food available	Utilities Index		Facilities Index		Equipment Index		Mean expenditure per pupil 2008/2009 - Fundamental Education	
	%	%	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.
0|--20	75.5%	96.1%	.93	.14	.52	.17	.71	.18	3637.73	1184.05
20|--40	62.6%	95.1%	.88	.16	.48	.17	.66	.20	3388.03	1191.97
40|--60	50.3%	94.9%	.85	.17	.43	.16	.60	.22	3028.64	1115.25
60|--80	41.2%	94.2%	.81	.18	.39	.16	.54	.23	2743.52	995.85
80|--|100	37.8%	94.0%	.79	.19	.37	.15	.52	.23	2610.67	956.17
Total	60.2%	95.1%	.87	.17	.46	.17	.63	.22	3244.22	1183.14

Table A - 26: School outcomes by school proportion of *BF* intake – 4th grade 2007

BF intake (%)	Portuguese		Mathematics		Pass-grade rate		Dropout rate	
	Mean	s.d.	Mean	s.d.	Mean	s.d.	Mean	s.d.
0|--20	183.89	17.59	201.79	19.29	91.10	9.72	1.39	4.36
20|--40	172.99	16.05	190.47	18.06	87.43	11.81	2.65	5.51
40|--60	165.27	15.45	182.25	17.13	84.78	12.53	3.60	5.72
60|--80	159.90	14.80	176.96	16.39	83.00	13.13	4.06	5.58
80|--|100	158.00	14.51	175.03	16.27	82.42	13.61	4.11	6.25
Total	171.47	18.16	188.86	19.91	86.91	12.12	2.82	5.44

Table A - 27: Test Scores (Portuguese) by *BF* intake and months of participation in the programme

BF intake (%)	Months of Participation							
	0 to 6	6 to 12	12 to 18	18 to 24	24 to 30	30 to 36	36 to 42	42 to 51
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean
0|--20	190.00	186.85	184.96	185.09	183.70	182.42	181.65	182.13
20|--40	.	175.89	173.21	173.81	173.71	172.68	169.55	166.78
40|--60	.	171.18	167.56	166.18	166.37	164.88	161.65	159.32
60|--80	.	171.89	160.63	160.87	161.51	159.77	156.43	154.85
80|--|100	.	162.03	162.25	162.38	159.49	157.45	154.84	151.67
Total	190.00	184.40	177.62	174.45	171.79	169.30	167.12	170.35

Table A - 28: Partial correlation coefficients of socioeconomic variables and school factors with school outcomes and *Bolsa Família* factors (selection of controls)

Variables		School outcomes				*Bolsa Família* factors			Selected Controls
		Portuguese	Maths	Pass-grade rate	Dropout rate	Prop*BF*	Time	Cash	
Socioeconomic Variables	*BF* households' per capita income	0.146***	0.139***	0.080***	-0.008	-0.075***	-0.191***	-0.687***	●
	Prop. of Boys	-0.039***	0.011*	0.001	-0.018***	-0.022***	-0.006	-0.002	●
	Prop. of Non-whites	-0.012**	-0.032***	0.004	-0.026***	0.045***	-0.011*	0.043***	●
	Region	0.147***	0.177***	0.038***	-0.116***	-0.081***	0.094***	-0.079***	●
	Overage Rate	0.056***	0.057***	-0.135***	0.179***	-0.038***	-0.043***	-0.015***	○
	Prop. Pre-school	0.132***	0.152***	-0.007	-0.007	0.013**	-0.006	-0.020***	●
	Prop. Failed	-0.101***	-0.090***	-0.140***	-0.013**	0.082***	0.023***	-0.021***	●
	Prop. in Child Labour	-0.259***	-0.217***	-0.052***	0.058***	0.077***	0.039***	-0.002	○
	Goodsindx	0.080***	0.061***	0.004	0.003	-0.260***	-0.073***	0.044***	○
	Prop. in Large Families (7 or more)	-0.172***	-0.140***	-0.024***	-0.012**	0.038***	0.043***	-0.220***	●
	Prop Mothers with post-primary education	0.127***	0.098***	0.025***	-0.013**	-0.060***	0.011*	0.009	●
	Prop Fathers with post-primary education	0.043***	0.024***	0.004	-0.011*	-0.071***	0.003	0.031***	●
	Prop. Fem. Headed Household	-0.006	-0.017***	-0.015***	0.004	0.009	-0.021***	0.106***	●
School Factors	Class size	-0.040***	-0.045***	-0.054***	-0.004	-0.095***	-0.014**	0.026***	●
	School day (hrs/day)	0.023***	0.027***	-0.013**	-0.021***	-0.033***	-0.037***	-0.017***	●
	Prop. Teachers with HE	0.035***	0.040***	0.004	-0.029***	-0.012**	0.020***	-0.006	●
	Utilities index	0.033***	0.031***	-0.063***	0.066***	-0.016***	0.015***	0.012**	●
	Facilities index	-0.026***	-0.018***	-0.028***	-0.002	0.008	0.019***	0.005	●
	Equipment index	0.065***	0.067***	-0.010*	-0.003	0.036***	-0.009	-0.017***	●
	Expenditure per pupil 2008/2009	-0.009	-0.003	-0.004	-0.039***	-0.090***	-0.096***	0.013**	●
	Prop. Students sitting for the exam	0.099***	0.081***	0.219***	-0.317***	0.086***	-0.004	-0.023***	●

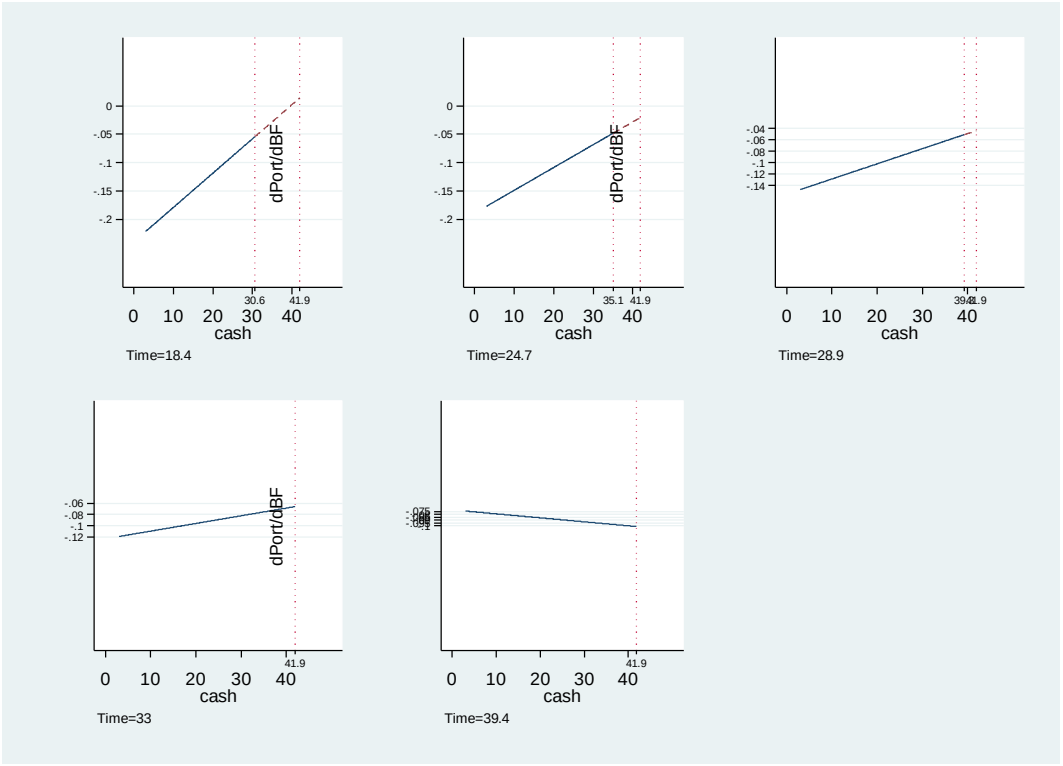
*** p<0.01, ** p<0.05, * p<0.1

Table A - 29: Regression of school outcomes on *BF* factors, school composition and school resources.

	VARIABLES	(1) Portuguese	(2) Maths	(3) Pass-grade rate	(4) Dropout rate
Bola Família Factors	**Prop*BF* (β_1)**	-0.385***	-0.390***	-0.071	0.001
		(0.089)	(0.101)	(0.067)	(0.028)
	Prop*BF* x Cash (β_2)	0.012**	0.011**	0.004	-0.000
		(0.005)	(0.006)	(0.004)	(0.002)
	Prop*BF* x *Time* (β_3)	0.008***	0.007**	0.001	-0.001
		(0.003)	(0.003)	(0.002)	(0.001)
	Prop*BF* x Time x Cash (β_4)	-0.000*	-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)	(0.000)
	Cash (β_5)	-0.172	-0.290*	0.142*	-0.020
		(0.137)	(0.159)	(0.083)	(0.035)
	***Time* (β_6)**	-0.073	-0.093	0.049	0.006
		(0.080)	(0.092)	(0.047)	(0.019)
	Time x Cash (β_7)	-0.003	-0.003	-0.004	-0.000
		(0.005)	(0.005)	(0.003)	(0.001)
School Composition (students' and households' characteristics)	*BF* households' per capita income	0.101***	0.102***	0.057***	-0.011***
		(0.008)	(0.009)	(0.006)	(0.003)
	Prop. of Boys	-9.934***	-2.584***	-0.948	-0.119
		(0.848)	(0.914)	(0.652)	(0.322)
	Prop. of Non-whites	-2.791***	-5.185***	2.131***	-1.193***
		(0.570)	(0.658)	(0.442)	(0.209)
	Prop. Pre-school	12.728***	16.402***	1.166**	-0.871***
		(0.581)	(0.665)	(0.497)	(0.268)
	Prop. Failed	-8.220***	-7.240***	-23.923***	5.128***
		(0.583)	(0.658)	(0.513)	(0.256)
	Prop. in Large Families (7 or more)	-26.301***	-25.021***	-1.449**	0.860***
		(0.779)	(0.867)	(0.655)	(0.309)
	Prop. Fem. Headed Household	0.871	-0.571	-2.341***	0.473*
		(0.774)	(0.846)	(0.580)	(0.266)
	Prop Mothers with post-primary education	16.110***	14.164***	1.003*	-0.929***
		(0.709)	(0.779)	(0.554)	(0.280)
	Prop Fathers with post-primary education	5.814***	3.635***	0.539	-0.882***
		(0.652)	(0.713)	(0.523)	(0.255)
	Prop. Students sitting for the exam	10.497***	9.286***	18.653***	-12.482***
		(0.598)	(0.682)	(0.551)	(0.334)
School Resources	Class size	-0.091***	-0.125***	-0.071***	-0.006
		(0.011)	(0.013)	(0.010)	(0.005)
	School day (hrs./day)	-0.002	-0.002	0.004**	-0.004***
		(0.002)	(0.002)	(0.001)	(0.001)
	Prop. Teachers with HE	0.013***	0.017***	0.001	-0.005***
		(0.002)	(0.002)	(0.002)	(0.001)
	Utilities index	4.083***	3.796***	-1.124***	1.038***
		(0.510)	(0.590)	(0.434)	(0.202)
	Facilities index	-0.593	0.155	-3.493***	0.489***
		(0.505)	(0.573)	(0.394)	(0.170)
	Equipment index	4.503***	5.361***	-0.519	-0.341*
		(0.413)	(0.467)	(0.365)	(0.179)
	Expenditure per pupil 2008/2009	-0.000***	-0.000***	0.000	-0.000***
		(0.000)	(0.000)	(0.000)	(0.000)
Regions	1.North	-5.367***	-8.161***	1.628***	1.138***
		(0.319)	(0.359)	(0.271)	(0.116)
	2.Northeast	-8.019***	-9.992***	-1.120***	2.058***
		(0.256)	(0.284)	(0.219)	(0.100)
	3.Central-West	-4.246***	-6.195***	3.395***	0.675***
		(0.289)	(0.328)	(0.202)	(0.098)
	5.South	-0.094	-0.475	3.752***	-1.471***
		(0.264)	(0.311)	(0.187)	(0.077)
Test Scores	A2007Port			0.060***	-0.008*
				(0.009)	(0.004)
	A2007Math			0.013*	0.002
				(0.008)	(0.004)
	Constant	169.115***	187.238***	62.618***	16.689***
		(2.836)	(3.244)	(1.996)	(0.895)
	Observations	33,805	33,805	33,805	33,805
	Adjusted R-squared	0.523	0.491	0.320	0.294

Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1)
Southeast region (4) is the reference dummy.

Figure A- 1: Marginal Effects of *BF* intake on Portuguese test scores by per capita cash at different percentiles of Time.



Note: The dashed segments indicate non-statistical significant marginal effects.

Table A - 30: Regression of school outcomes on *BF* factors, school composition and school resources for schools in the 5th quintile of *BF* intake

	VARIABLES	Portuguese	Maths	Pass-grade	Dropout
Bola Família Factors	**Cash**	2.258***	1.024	0.445	-0.531
		(0.826)	(0.948)	(0.902)	(0.342)
	Time	1.168**	0.485	-0.038	-0.329*
		(0.503)	(0.575)	(0.558)	(0.199)
	Time x Cash	-0.068**	-0.034	0.004	0.015
		(0.027)	(0.031)	(0.030)	(0.011)
School Composition (students' and households' characteristics)	*BF* households' per capita income	0.162***	0.059	0.013	-0.023
		(0.042)	(0.050)	(0.042)	(0.021)
	Prop. of Boys	-12.524***	-7.552*	-2.000	-0.338
		(3.648)	(4.240)	(3.211)	(1.356)
	Prop. of Non-whites	7.804***	5.178	2.583	-1.421
		(2.744)	(3.338)	(2.771)	(1.114)
	Prop. Pre-school	7.289***	10.830***	2.520	0.001
		(2.399)	(2.814)	(2.839)	(1.832)
	Prop. Failed	-7.132***	-5.234*	-23.991***	5.064***
		(2.471)	(2.947)	(2.721)	(1.687)
	Prop. in Large Families (7 or more)	-21.120***	-19.009***	3.071	0.249
		(3.160)	(3.491)	(3.327)	(1.433)
	Prop. Fem. Headed Household	2.773	-0.096	-8.317**	2.821*
		(3.694)	(4.304)	(4.003)	(1.507)
	Prop Mothers with post-primary education	13.223***	13.970***	2.345	-2.175
		(3.485)	(3.761)	(2.969)	(1.332)
	Prop Fathers with post-primary education	-2.999	-4.121	0.775	-0.594
		(3.038)	(3.363)	(2.716)	(1.282)
	Prop. Students sitting for the exam	10.773***	14.114***	19.023***	-13.505***
		(3.031)	(3.447)	(3.078)	(1.627)
School Resources	Class size	-0.044	-0.075	-0.125**	0.023
		(0.047)	(0.058)	(0.050)	(0.022)
	School day (Min./day)	-0.013	-0.006	0.012	-0.005
		(0.014)	(0.016)	(0.012)	(0.004)
	Prop. Teachers with HE	0.003	-0.002	-0.002	-0.004
		(0.009)	(0.010)	(0.009)	(0.005)
	Utilities index	4.694**	4.067	0.191	0.648
		(2.240)	(2.630)	(2.041)	(1.006)
	Facilities index	6.712**	4.469	0.600	0.313
		(3.097)	(3.522)	(2.875)	(1.172)
	Equipment index	-0.810	3.622	0.715	-0.848
		(1.953)	(2.295)	(2.054)	(0.865)
	Expenditure per pupil 2008/2009	-0.001**	-0.000	-0.001**	-0.000
		(0.000)	(0.001)	(0.000)	(0.000)
Regions	1.North	-5.264***	-8.829***	-0.376	1.638**
		(1.899)	(2.165)	(1.924)	(0.714)
	2.Northeast	-11.181***	-13.502***	-1.504	1.257***
		(1.416)	(1.681)	(1.335)	(0.464)
	3.Central-West	-4.826**	-7.693***	4.999***	0.396
		(1.939)	(2.357)	(1.725)	(0.657)
	5.South	-3.234	-5.016**	2.891	-1.701**
		(1.966)	(2.476)	(2.232)	(0.660)
Test Scores	A2007Port			0.130***	-0.029
				(0.046)	(0.025)
	A2007Math			-0.004	0.015
				(0.039)	(0.022)
	Constant	115.438***	153.806***	48.463**	28.554***
		(17.502)	(19.907)	(19.018)	(7.221)
	F- statistic for joint significance of *Bolsa Família* factors	F(3,1252) = 3.45 ρ = 0.0161	F(3,1252) = 1.50 ρ = 0.2136	F(3,1250) = 2.86 ρ = 0.0359	F(3,1250) = 1.75 ρ = 0.1543
	Observations	1,277	1,277	1,277	1,277
	Adjusted R-squared	0.297	0.256	0.196	0.155

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A - 31: Regression of test scores in MATHS on *BF* factors for schools in the 5th quintile of *BF* intake by region

VARIABLES	North Maths	Northeast Maths	Central-West Maths	Southeast Maths	South Maths
Cash (β_1)	2.637	0.900	-6.268	3.920	11.704***
	(3.211)	(1.268)	(4.778)	(4.511)	(4.125)
Time (β_2)	1.800	0.209	-2.831	2.038	7.902***
	(1.904)	(0.764)	(2.767)	(2.352)	(2.326)
Time x Cash (β_3)	-0.109	-0.021	0.169	-0.126	-0.462***
	(0.107)	(0.040)	(0.166)	(0.139)	(0.129)
Controls	Yes	Yes	Yes	Yes	Yes
Constant	92.914	149.884***	250.591**	81.044	97.331
	(59.218)	(26.808)	(100.786)	(85.044)	(116.763)
Observations	99	906	61	159	52
Adjusted R-squared	0.046	0.130	0.207	0.130	0.650
F-test (H_0: $\beta_1=\beta_2=\beta_3=0$)	**0.551**	**2.321**	**0.976**	**0.301**	**7.623**
Prob. > F	**0.649**	**0.074**	**0.413**	**0.824**	**0.001**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A - 32: Regression of Dropout rate on *BF* factors for schools in the 5th quintile of *BF* intake by region

VARIABLES	North Dropout	Northeast Dropout	Central-West Dropout	Southeast Dropout	South Dropout
Cash	0.937	-1.271**	-0.618	0.122	-0.590
	(1.499)	(0.527)	(1.054)	(0.585)	(1.314)
Time	0.929	-0.771**	0.180	-0.114	-0.293
	(0.972)	(0.308)	(0.638)	(0.298)	(0.758)
Time x Cash	-0.052	0.037**	-0.011	0.007	0.018
	(0.054)	(0.016)	(0.037)	(0.018)	(0.043)
Controls	Yes	Yes	Yes	Yes	Yes
Constant	-2.050	47.884***	41.892	7.558	4.588
	(31.325)	(11.252)	(25.703)	(11.185)	(23.741)
Observations	99	906	61	159	52
Adjusted R-squared	0.241	0.128	0.186	0.003	-0.066
F-test (H_0: $\beta_1=\beta_2=\beta_3=0$)	**1.100**	**2.378**	**2.449**	**0.894**	**0.108**
Prob. > F	**0.354**	**0.0685**	**0.0784**	**0.446**	**0.955**

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A - 33: Mean per capita cash values at different points of the distribution by region for schools in the 5th quintile of *BF* intake.

region	min	p5	p25	p50	p75	p95	max
North	12.6	14.6	16.4	17.7	19.0	21.8	24.5
Northeast	12.8	15.6	17.2	18.5	19.9	22.2	25.4
Central-West	11.5	13.5	15.8	17.6	19.7	21.7	22.6
Southeast	11.9	13.6	15.3	16.9	18.0	19.7	22.3
South	11.9	13.9	16.7	17.3	18.9	20.3	21.1
Total	11.5	14.9	16.8	18.2	19.6	21.9	25.4

Table A - 34: Mean time of participation at different points of the distribution by region for schools in the 5th quintile of *BF* intake.

region	min	p5	p25	p50	p75	p95	max
North	13.2	18.1	23.7	27.1	31.4	37.9	38.8
Northeast	14.2	23.0	28.1	31.8	35.6	40.6	47.0
Central-West	8.7	17.2	22.5	24.3	30.1	33.6	37.6
Southeast	15.2	22.7	26.0	30.7	34.4	40.0	48.2
South	18.7	19.8	28.5	32.1	35.8	40.0	45.7
Total	8.7	21.7	27.0	31.3	35.1	40.2	48.2

APPENDIX Chapter 8 (A): Analysis of missing values for the number of *Bolsa Família* beneficiaries by school in 2005

The process of collapsing variables by school to generate school level variables for use in the panel data analysis requires some clarifications as to how missing values are handled. It is important to look at how the original missing values in the student level dataset of socioeconomic variables from *Prova Brasil 2005* are characterised, and how to justify the deletion of cases with missing values, either at the individual or school levels.

There are two potential problems in excluding missing values in the original datasets at the individual level before collapsing variables by school without a clear diagnostic on the extension and distribution of missing values. The first problem is the risk of creating a biased sample with respect to the variables of interest, if cases with missing values systematically differ from cases with valid values. Second is the problem of the potential impact of exclusions on the study's sample size (see Hair et al., 2010, Chapter 2). To produce a diagnostic of the extension of the missing data problem in the original *Prova Brasil 2005* dataset at the student level, attention is given to three considerations: (1) What is the proportion of cases with missing values for each variable? (2) What is the proportion of variables with missing values for each case? (3) What is the overall proportion of cases with missing values?

The original dataset for the results in *Prova Brasil* 2005 has 1,975,635 records of students' test scores and socioeconomic characteristics. However, not all schools included in the original dataset had their results officially recognised and announced by INEP, due to either a reduced number of students writing the exam (affecting representativeness) or for not complying with some of the norms of the national exam. Excluding these schools from the dataset, the number of students' records is reduced to 1,883,779.

Table A - 35 reports the number and percentage of missing values for each of the original variables in the dataset. The relevant variables are those for socioeconomic characteristics (Q*) and those representing school outcomes (A2005*). Amongst the socioeconomic variables, the percentage of missing values reaches about 7% of the observations in most cases. This means that the incidence of missing values is fairly distributed amongst variables. However, there is a problem with the variables Q44 (participation in *BF*), Q20 (mother's education), and Q21 (father's education). These variables have an option "Don't Know" that is coded as missing value (.a), which raises the level of *missingness* in these variables to 24%, 33%, and 42% of the cases, respectively. **Table A - 36** reports the number and

percentage of missing values in the dataset for the new variables (most of them dummies) to be included in the analysis at the school level after aggregating individual data. The new metric variable for durable goods in the household (*Goodsindx*) depends on several of the original variables, and as result is missing in 15% of the cases[272]. The same high proportion of missing values is observed in the derived variables *BF, Motheduc, and Fatheduc*.

Table A - 35: Number and percentage of missing values by original relevant variable

Variable	Obs=.	Obs=.a	%Miss	Obs≠.	Unique values	Min	Max
PK_COD_ENTIDADE				1883779	>500	1.10e+07	5.30e+07
A2005aprov_4ª	1,895		0.1%	1881884	>500	0	100
A2005Math				1883779	>500	113.46	290.94
A2005Port				1883779	>500	106.64	288.65
codigo_alu				1883779	>500	19	6.96e+07
Q1	112,255		6.0%	1771524	2	0	1
Q2	136,543		7.2%	1747236	5	1	5
Q4	161,895		8.6%	1721884	8	1	8
Q7	126,984		6.7%	1756795	3	1	3
Q8	127,077		6.7%	1756702	3	1	3
Q9	129,516		6.9%	1754263	2	0	1
Q12	133,234		7.1%	1750545	3	1	3
Q13	130,410		6.9%	1753369	2	0	1
Q14	132,746		7.0%	1751033	2	0	1
Q15	136,140		7.2%	1747639	2	0	1
Q16	122,349		6.5%	1761430	3	1	3
Q18	140,074		7.4%	1743705	6	1	6
Q19	140,957		7.5%	1742822	4	1	4
Q20	146,697	477,605	33.1%	1259477	5	1	5
Q21	143,704	654,319	42.4%	1085756	5	1	5
Q24	135,897		7.2%	1747882	2	0	1
Q25	135,356		7.2%	1748423	3	1	3
Q28	138,219		7.3%	1745560	2	0	1
Q35	133,391		7.1%	1750388	3	1	3
Q37	141,206		7.5%	1742573	3	1	3
Q44	151,322	306,980	24.3%	1425477	2	0	1

272 The reason that the percentage of missing values is higher for Goodsindx than for its individual component variables is because in any case with a missing component, a missing value is attributed to the index.

Table A - 36: Number and percentage of missing values by new relevant variables

Variable	Obs=.	Obs=.a	%Miss	Obs≠.	Unique values	Min	Max
PK_COD_ENTIDADE				**1883779**	>500	1.10e+07	5.30e+07
A2005aprov_4[a]	1,895		0.1%	1881884	>500	0	100
A2005Math				1883779	>500	113.46	290.94
A2005Port				1883779	>500	106.64	288.65
codigo_alu				1883779	>500	19	6.96e+07
Male(Q1)	112,255		6.0%	1771524	2	0	1
Non_white(Q2)	136,543		7.2%	1747236	2	0	1
Over_age(Q4)	161,895		8.6%	1721884	2	0	1
Pre_school(Q35)	133,391		7.1%	1750388	2	0	1
Dfail(Q37)	141,206		7.5%	1742573	2	0	1
ChildLab(Q28)	138,219		7.3%	1745560	2	0	1
Goodsindx*	288,072		15.3%	1595707	11	0	1
Famsize(Q18)	140,074		7.4%	1743705	2	0	1
Motheduc(Q20)	624,302		33.1%	1259477	2	0	1
Fatheduc(Q21)	798,023		42.4%	1085756	2	0	1
FemHHead(Q19)	140,957		7.5%	1742822	2	0	1
BF(Q44)	151,322	306,980	24.3%	1425477	2	0	1

* Composed by Q7,Q8,Q9,Q12,Q13,Q14,Q15,Q16,Q24,Q25.

Table A - 37 reports the frequency distribution of the percentage of missing variables by student for the variables listed in **Table 38** (Chapter 8). The overall percentage of cases with missing values at the student level is 63%. It also shows that some 82% of the cases have less than 11.8% of missing variables, a relative acceptable level to support different approach methods to circumvent the missing value problem. A "complete case" approach (*listwise* deletion) at the individual level is possible by keeping only 37% of the cases for which there are no missing variables. Because the available dataset is huge, even a high proportion of losses would not be a problem, provided that the resulting sample has no bias, that is, if deleted cases with missing values do not systematically differ from the cases with valid values kept in the sample in respect to the variables of interest[273].

273 This was the procedure adopted in Chapter six, where only cases with no missing values were kept in the dataset from which a smaller sample of 1% of cases was randomly selected to proceed with the analysis.

Table A - 37: Distribution of the percentage of missing variables by students considering the variables in Table A - 36.

Percent_miss	Freq.	Percent	Cum.
0.00	701,153	37.22	37.22
5.88	466,419	24.76	61.98
11.76	383,645	20.37	82.35
17.65	147,911	7.85	90.20
23.53	36,069	1.91	92.11
29.41	18,953	1.01	93.12
35.29	12,440	0.66	93.78
41.18	9,702	0.52	94.29
47.06	7,679	0.41	94.70
52.94	7,942	0.42	95.12
58.82	6,492	0.34	95.47
64.71	6,409	0.34	95.81
70.59	78,777	4.18	99.99
76.47	188	0.01	100.00
Total	1,883,779	100.00	

However, the *listwise* approach is not recommended in the current case since it can potentially compromise the resulting dataset at the school level to be created by data aggregation using students' cases as a base. First, the process of excluding any case with missing variables from the dataset at the individual level may strongly affect the number of valid cases for a particular variable x1 at the school level, implying that the sample size by school can be significantly reduced by that approach. Second, any random pattern of *missingness* for x1 at the individual level does not necessarily reflect at the school level. As a consequence, biased school indicators calculated upon x1 can occur. This is because the *listwise* process excludes not only cases with missing values in the particular variable x1 given a school s1, but also cases in s1 with missing values in any x2, x3…xn, removing cases in s1 with non-missing values of x1. In fact, the more random missing values there are amongst variables at the individual level, the more likely it is that the *listwise* process substantially reduces the number of valid cases for x1 in school s1. Therefore, the collapsing process of variables by school is accomplished without eliminating missing values from the individual level dataset in order to achieve an "all available subset" of cases with valid values for the variables of interest by school.

After collapsing the variables of interest described in **Table 38** by school, a missing table is generated with the number and percentage of missing values in each variable by school. This allows an assessment of the extension and pattern of missing values at the school level. The aggregated variable distribution statistics are reported in **Table A - 38**.

Table A - 38: Mean, standard deviation, and quantiles of the proportion of missing values by relevant variables across schools

variable	mean	sd	p25	p50	p75	p90	p95
Q44Perc	24.56	18.98	14.75	20.31	27.27	37.04	62.69
Q1Perc	6.76	20.73	0.00	1.01	3.23	8.33	37.78
non_whiteP~c	8.04	21.21	0.00	2.00	4.96	12.90	41.67
over_agePerc	9.22	22.00	0.00	2.20	5.56	19.80	61.54
pre_school~c	7.84	20.75	0.00	2.00	4.84	12.07	45.83
DfailPerc	8.33	21.16	0.00	2.27	5.41	13.21	48.00
Q28Perc	8.13	20.80	0.00	2.44	5.56	11.76	40.82
goodsindxP~c	16.05	20.69	6.00	10.26	16.98	28.07	60.00
famsizePerc	8.06	20.97	0.00	1.86	4.49	13.59	49.06
motheducPerc	33.49	18.09	23.44	29.89	37.50	48.48	70.00
fatheducPerc	42.92	16.93	32.86	40.00	48.57	59.32	78.08
FemHHeadPerc	8.27	21.11	0.00	2.47	5.66	12.16	42.31

Number of observations: 27,951 schools.

The main variable of interest to be used in the panel data is the proportion of *BF* beneficiaries by school (*PropBF*), calculated based on the variable Q44, which has 24.6% of missing values on average across schools and a high s.d. (19.0). **Table A - 39** shows a high incidence of missing values for Q44 in schools from all regions, particularly from the Southeast and Northeast regions, as well as a large variation between schools in these regions as shown by the high s.d.

Table A - 39: Mean, standard deviation, and quantiles of the proportion of missing values in Q44 across schools by region

region	mean	sd	min	p25	p50	p75	p90	p95	max
North	16.93	7.63	0.00	11.84	16.26	21.01	26.47	30.00	80.00
Northeast	25.06	20.32	0.00	14.29	20.00	27.42	40.00	87.14	100.00
Central-West	18.43	8.28	0.00	13.21	17.72	22.64	28.21	32.00	100.00
Southeast	29.44	24.32	0.00	16.84	22.39	29.56	50.72	100.00	100.00
South	22.75	10.22	0.00	15.63	21.57	28.57	36.36	41.54	98.41
Total	24.56	18.98	0.00	14.75	20.31	27.27	37.04	62.69	100.00

The differences in the incidence of missing values in Q44 by school suggest that the proportion of beneficiaries will be relatively accurate in schools with a very low percentage of missing values, but will be potentially biased in schools with high rates of *missingness*. For instance, the proportion of beneficiaries will be underestimated in a particular school if participants are overrepresented amongst those not responding the question Q44[274]. This also means that the parameter to be estimated regarding the effect on school outcomes associated with participation in *BF* will be biased, since the inaccuracy in measuring *PropBF* implies a measurement error in the explanatory variable that will be correlated with both the outcome of interest and the explanatory variable itself. For instance, considering the simple regression model below, where school outcome (y_i) is regressed on the 'true' value of *PropBF* for school "i", if *PropBF* is measured with an error then the explanatory variable to be regressed in the model will be the observed (and biased) *PropBF** instead of the 'true' *PropBF.*

$$y_i = \alpha_0 + \alpha_1 PropBF_i + \varepsilon_i$$

$$\underbrace{PropBF_i^*}_{\text{observed}} = \underbrace{PropBF_i}_{\text{'true'}} + \underbrace{\omega_i}_{\text{error}} \quad \rightarrow \quad y_i = \alpha_0 + \alpha_1 (PropBF_i^* - \omega_i) + \varepsilon_i$$

$$y_i = \alpha_0 + \alpha_1 PropBF_i^* + \underbrace{(\varepsilon_i - \alpha_1 \omega_i)}_{u_i}$$

The measurement error of *PropBF* will be discharged in the error term, but will be correlated with both *PropBF** and y_i. Because u_i contains ω_i and Prop*BF** depends on ω_i, there is a correlation between Prop*BF** and u_i that causes bias and inconsistency in the estimator of α_1.

In order to have the best measure of *PropBF* as possible, I employ a technical adjustment to calculate the proportion of beneficiaries by school in 2005 to allow the incorporation of the maximum number of schools with missing values in the variable Q44 into the analysis. The adjustment is based on the procedures adopted by Williams and Flewelling (1987) and Pampel and Williams (2000). Two methods are considered. The first method is a simple adjustment assuming that the distribution of cases with missing values in Q44 equals the distribution of cases with non-missing values within a given school. It is a simple extrapolation of the composition of known cases to unknown cases. The second method uses

274 The previous analysis of missing data carried out to allow the analysis in Chapter six showed that those answering "Don't know" in Q44 are more like beneficiaries than non-beneficiaries in terms of test scores, as well as in relation to other individual and family characteristics. However, because in 2005 *BF* had not yet achieved full coverage of eligible families, that resemblance can, in fact, reflect eligibility instead of participation.

the additional information about the cases with missing values in each school to produce a more accurate estimation of the distribution of Q44. The latter procedure requires two conditions to work: (i) the additional information must be known for a significant proportion of cases with missing values in Q44; (ii) The variable(s) containing the additional information must have a correlation with Q44.

To illustrate the alternatives for dealing with the missing values, I take a particular school[275] where the dummy variable for *BF* (Q44) is distributed as shown below:

[Q44=1]	12 cases (19.3%)
[Q44=0]	44 cases (71%)
[Q44=.]	6 case (9.7%)
[Total]	62 cases (100%)

By simply calculating the proportion of beneficiaries without taking into account the fact that there is a portion of students for which the value of Q44 is missing yields the result in (a).

(a) Prop*BF*=([Q44=1]/Total)x100 = (12/62)x100=19.3%

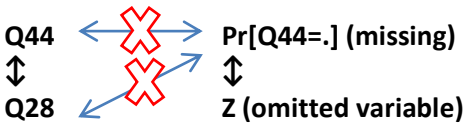
In formula (a) we use all cases to calculate the percentage of children in *BF,* even knowing that some of the cases have missing values. Implicitly, it is assumed that all missing values have Q44=0, since they are counted for the denominator but not for the numerator. It is a strong and certainly wrong assumption, for there would be no particular reason why all missing values would have the same null value. The 'true' value of *PropBF* amongst children who sat for the exam in that school is between 19.3% and 29% depending on how the missing values are distributed as to the binary variable. It is the case that for binary variables the difference in the percentages between the two extreme values for that proportion is precisely the percentage of missing values.

A more reasonable assumption is that missing values would be distributed in the same way as non-missing values in the same school. This is the idea behind the first adjustment method mentioned above. The calculation of the *BF* participation rate for the school in the example using this approach is:

(b) If $PropBF_{adj1}$=([Q44=1]/([Total]-[Q44=.]))x100 = 21.4%

275 School code: 11000201.

This assumption certainly yields a better estimation of the `true' value of *PropBF* than we get from formula (a). This adjustment method simply uses the valid cases for Q44 in each school and calculates their participation rates. Because the result relies only on the valid cases in the school, it is sensitive to the number of those valid cases as a random sample of the students in that school. Inasmuch as the number of valid cases increases (sample size increases), the standard error in estimating the proportion of beneficiaries reduces, yielding a more accurate estimate of *PropBF*. Also, for the valid cases to be a random sample of the students in that school, we must assume that the process leading students to not answer Q44, or the fact that they do "not know" the answer to that question, is neither related to their status as beneficiaries of *BF* nor to any other individual characteristics included in the analysis. In other words, it relies on the assumption that the mechanism behind the missing values is a *missing completely at random* type – MCAR (Little, 1987; Allison, 2002; Enders, 2010). This mechanism can be represented as follows:



This figure is adapted from Enders (2010), in which Z represents omitted variables associated with the unknown process(es) leading to the missing values in Q44. The other associations represented in the figure are the correlation between the probability of missing value in Q44 ([Pr[Q44=.]) and the values of Q44 that are missing, the correlation between Pr[Q44=.] and another variable (Q28), which can also be correlated with Q44. The MCAR assumption first requires that although Z is unknown, it does not include Q44 as an influential factor yielding missing values; that is, there must be no association between the students with missing values in Q44 and their own status as to *BF* participation. Secondly, the MCAR requires that any other student characteristic to be included in the analysis not be associated with the *missingness* in Q44; that is, it is also not part of Z. For instance, the condition of being engaged in child labour (represented by the dummy variable Q28) must not be an associated factor with the student not responding about his/her *BF* participation (Pr[Q44=.]). These two associations are crossed out in the figure as prohibited associations if MCAR assumption is to be held.

The second adjustment method to calculate *PropBF* takes advantage of the association between participation in *BF* and child labour (Q44 ↔ Q28) and the available information on child labour (Q28) for those students with missing values in Q44 to estimate their *Bolsa Família participation rate*. The two conditions that must be satisfied for the second method to be valid hold for Q44 and Q28 (**Table A - 40**):

i) 75% of the students with missing values in Q44 have valid values for Q28[276].

ii) There is a significant correlation between participation in *BF* and being in child labour.

Table A - 40: Cross-tabulation of Q44xQ28, Pearson's chi-square and Cramer's V statistics

Are you in Bolsa Familia?	Do you work? No	Yes	.	Total
No	779,768	111,547	12,794	904,109
	52.69	42.00	9.26	47.99
Yes	411,859	98,786	10,723	521,368
	27.83	37.20	7.76	27.68
.	28,647	14,202	108,473	151,322
	1.94	5.35	78.48	8.03
Don't Know	259,704	41,047	6,229	306,980
	17.55	15.46	4.51	16.30
Total	1,479,978	265,582	138,219	1,883,779
	100.00	100.00	100.00	100.00

Pearson chi2(6) = 1.0e+06 Pr = 0.000

Cramér's V = 0.5200

The difference with the first method described above is that, instead of applying the unconditional probability distribution function (f(x)) for the non-missing cases over the missing cases (assuming that f(Q44≠.)=f(Q44=.)), we apply the conditional distribution function (f(x1|x2)) for the non-missing cases over the missing cases (assuming that f(Q44≠.|Q28)=f(Q44=.|Q28). The formula for the adjusted rate is[277]:

$$(c)PropBF_{adj2} = \frac{[Q44 = 1] + \sum_{i=0,1,[.]} \left(\frac{[Q44 = 1]|Q28 = i}{n_i} \right) X\, m_i}{N}$$

Where, [Q44=1] is the number of students answering "yes" in Q44 (*BF* participants); "**i**" is the possible conditions related to Q28 (0, 1 or missing); $\mathbf{n_i}$ is the number of valid cases of Q44 for condition "i"; $\mathbf{m_i}$ is the number of missing values of Q44 for condition "i"; and **N** is the total number of students in the school.

Table A - 41 displays the cross-tabulation of Q44 and Q28 for the school in the example.

276 Using data at the student level in dataset \temp\BASE2005_04tem.dta in a cross tabulation of Q44 and Q28 343,600 out of 458,302 students with missing data in Q44 have non-missing values for Q28.

277 This expression is adapted from the study by (Pampel and Williams, 2000)

Table A - 41: Cross-tabulation of Q44xQ28 for school "11000201".

Are you in Bolsa Família?	Do you work?			
	No [Q_{28}=0]	Yes [Q_{28}=1]	. [missing]	Total
No [Q_{44}=0]	41	2	1	44
Yes [Q_{44}=1]	9	3	0	12
.[missing]	4	2	0	6
Total	54	7	1	62

The calculation of *PropBF* using the second adjustment method for the example discussed earlier is described below.

$$PropBF_{adj2} = \frac{12 + \frac{9}{50} * 4 + \frac{3}{5} * 2 + \frac{0}{1} * 0}{62} = 22.5\%$$

The result is very close to the one obtained by using the first simple adjustment method. However, to test for significant differences between the two methods of adjustment across schools, I take a 10% sample of 2,624 schools from the dataset and test the two results as to:

i) differences in the mean value of Prop*BF*;
ii) the correlation coefficient between the two adjusted results;
iii) the seemingly unrelated regression to test for significance difference between the coefficients calculated using different methods to replace missing values in Q44.

The results are reported below (**Table A - 42**, **Table A - 43** and **Table A - 44**):

Table A - 42: Paired t-test

```
------------------------------------------------------------------------------
Variable   |     Obs        Mean    Std. Err.   Std. Dev.   [95% Conf. Interval]
---------+--------------------------------------------------------------------
PropBFadj1 |    2624    .3812203     .003168    .1622799    .3750083    .3874323
PropBFadj2 |    2624    .3578529    .0030174    .1545671    .3519361    .3637696
---------+--------------------------------------------------------------------
      diff |    2624    .0233675    .0004552      .02332    .0224748    .0242601
------------------------------------------------------------------------------
     mean(diff) = mean(PropBF - PropBFadj)                         t =  51.3293
 Ho: mean(diff) = 0                              degrees of freedom =     2623

 Ha: mean(diff) < 0           Ha: mean(diff) != 0           Ha: mean(diff) > 0
 Pr(T < t) = 1.0000         Pr(|T| > |t|) = 0.0000          Pr(T > t) = 0.0000
```

Table A - 43: Pearson's correlation coefficient between *PropBF* by different methods of adjustment

	PropBFadj1	PropBFadj2
PropBFadj1	1.0000	
PropBFadj2	0.9903	1.0000

Table A - 44: Seemingly unrelated regression of Portuguese Test Scores on Prop*BF*

```
------------------------------------------------------------------------------
Equation           Obs  Parms        RMSE    "R-sq"       chi2        P
------------------------------------------------------------------------------
A2005Port         2624      1    16.05802    0.1581     381.16   0.0000
2A2005Port        2624      1     16.1797    0.1453     341.32   0.0000
------------------------------------------------------------------------------

------------------------------------------------------------------------------
             |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
A2005Port    |
  PropBFadj1 |  -18.78054   .9619485   -19.52   0.000    -20.66593   -16.89516
       _cons |   178.6573   .4658606   383.50   0.000     177.7443    179.5704
-------------+----------------------------------------------------------------
2A2005Port   |
  PropBFadj2 |  -18.99094   1.027937   -18.47   0.000    -21.00566   -16.97622
       _cons |   178.2938   .4699224   379.41   0.000     177.3727    179.2148
------------------------------------------------------------------------------
 Testing for equal coefficients  [A2005Port]PropBFadj1 - [2A2005Port]PropBFadj2 = 0

           chi2(  1) =    1.42
         Prob > chi2 =    0.2342
```

Although the mean values for the two adjusted rates are statistically different, the two rates are strongly correlated and the hypothesis for equality of coefficients in the regression analysis cannot be rejected at the 5% level. The results suggest the two methods of counting the missing values for Q44 into the analysis will not yield different results in regression analysis[278]. However, I will opt here for the conditional adjustment based on the child labour variable. During the *BF* implementation process, poor families whose children were at risk of child labour were targeted for inclusion in the programme when not receiving any other benefit. Also, even those families whose children were already participants in the Child Labour Eradication Programme (PETI) but were living below the administrative per capita income threshold for *BF* eligibility were also migrated to *BF*[279]. These policy implementation aspects justify the selection of the child labour variable as a predictor of the distribution of beneficiaries amongst those children not responding about their status as to *BF* participation.

Given the discussion above and the inspection of the data at school level, the following steps are taken regarding the missing value problem in Q44 for 2005 data:

1) Schools with 50% or more missing values in Q44 are excluded from the sample (1,596). Some 85% of these schools are concentrated in two states (RJ and RN). Five hundred and twenty eight schools (33%) out of the excluded 1,596 are schools from Rio Grande do Norte (RN) state. Schools from RN withdrawn from the sample for that state had a major problem with the application of the socioeconomic questionnaire in 2005, as reported by INEP. Some 88% of its schools have more than 50% missing values for Q44. The state of RJ has 832 schools with more than 50% missing values, but it represents only 37% of its schools. The analysis will be carried out without the state of RN.

2) Schools with missing values in any of the outcome variables are excluded from the sample (there are 35 schools missing values for pass-grade rate).

3) Federal schools are excluded from the sample, as there are fewer than 10 and these schools do not represent the average public school in Brazil.

4) The variable *PropBF* is adjusted for the share of missing values according to the school distribution of incidence of child labour (Q28).

---

278 I also used another strong predictor of participation in *BF* to calculate the adjusted Prop*BF*: the dummy variable for failing a grade (Dfail). This variable also generated values of Prop*BF* that were very close to the values obtained by the simple adjustment method, with the same results in the tests for mean differences, correlation, and differences in the regression coefficients.

279 The only families who could remain in PETI were those whose benefits received in the PETI were higher than the prospect benefit to be paid by *Bolsa Família*.

Considering the above steps in dealing with the missing values in Q44, I assume:

(a) The missing values of Q44 in each school are distributed according to the distribution of non-missing values of Q44 conditional on child labour;

(b) The process leading students to not answer Q44 is not related to their status in *BF*. In other words, I assume that missing values in Q44 follow a *missing at random* pattern (MAR);

(c) The percentage of beneficiaries in each school in 2005 (not known) is equivalent to the percentage of beneficiaries sitting for the exam in each school (known through variable Q44);

(d) The last assumption is inverted for 2007 data, that is, the percentage of beneficiaries sitting for the exam in 2007 (not known) is assumed as equal to the percentage of beneficiaries by school (known).

APPENDIX Chapter 8 (B): Tables and Figures

Table A - 45: Student characteristics by *BF* intake 2005-2007

Level of BF intake	Prop. of boys 2005 Mean	Prop. of boys 2007 Mean	Δ	Signif.(1)	Prop. of non-whites 2005 Mean	Prop. of non-whites 2007 Mean	Δ	Signif.(1)	Prop. over-age 2005 Mean	Prop. over-age 2007 Mean	Δ	Signif.(1)
0|--20	.51	.50	-.01	**	.54	.57	.02	**	.13	.14	.01	*
20|--40	.51	.50	-.01	**	.647	.653	.01	*	.21	.21	.00	-
40|--60	.51	.50	-.01	**	.71	.70	-.01	*	.30	.28	-.02	**
60|--80	.51	.50	-.01	**	.73	.72	-.01	**	.35	.31	-.05	**
80|--|100	.49	.50	.01	-	.75	.73	-.03	-	.40	.32	-.08	**
Total	.51	.50	-.01	**	.65	.66	.00	*	.23	.23	-.01	**
Inter-quintile gap (1st-5th)	.024	.005	-.02	**	-.21	-.16	.05	**	-.26	-.18	.08	**

Level of BF intake	Prop. who have failed any grade 2005 Mean	Prop. who have failed any grade 2007 Mean	Δ	Signif.(1)	Prop. who attended pre-school 2005 Mean	Prop. who attended pre-school 2007 Mean	Δ	Signif.(1)	Prop. in child labour 2005 Mean	Prop. in child labour 2007 Mean	Δ	Signif.(1)
0|--20	.24	.24	.00	-	.81	.77	-.04	**	.111	.107	-.004	**
20|--40	.319	.325	.006	**	.74	.73	-.01	**	.15	.14	-.01	**
40|--60	.39	.39	.00	-	.701	.706	.005	*	.18	.17	-.02	**
60|--80	.44	.42	-.02	**	.68	.70	.02	**	.21	.18	-.02	**
80|--|100	.47	.43	-.04	-	.69	.69	.01	-	.193	.186	-.007	-
Total	.335	.338	.003	*	.74	.73	-.01	**	.16	.15	-.01	**
Inter-quintile gap (1st-5th)	-.23	-.19	.04	**	.13	.08	-.05	**	-.082	-.080	.002	**

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

Table A - 46: Household characteristics by *BF* intake 2005-2007

Level of BF intake	Goodsindx 2005 Mean	Goodsindx 2007 Mean	Δ	Signif.(1)	Prop.mother has post-primary education 2005 Mean	Prop.mother has post-primary education 2007 Mean	Δ	Signif.(1)	Prop.father has post-primary education 2005 Mean	Prop.father has post-primary education 2007 Mean	Δ	Signif.(1)
0|--20	.63	.64	.01	**	.42	.40	-.02	**	.43	.43	.00	-
20|--40	.53	.56	.03	**	.295	.300	.005	*	.31	.32	.02	**
40|--60	.46	.49	.03	**	.23	.25	.01	**	.24	.26	.02	**
60|--80	.41	.44	.03	**	.19	.22	.03	**	.20	.23	.03	**
80|--|100	.37	.43	.06	**	.14	.22	.08	**	.15	.22	.07	**
Total	.52	.54	.02	**	.29	.30	.01	**	.30	.32	.01	**
Inter-quintile (1st-5th)	.26	.21	-.05	**	.28	.18	-.10	**	.28	.21	-.07	**

Level of BF intake	Prop. living in female headed households 2005 Mean	Prop. living in female headed households 2007 Mean	Δ	Signif.(1)	Prop. living in large families (7 or more) 2005 Mean	Prop. living in large families (7 or more) 2007 Mean	Δ	Signif.(1)
0|--20	.20	.23	.03	**	.17	.18	.02	**
20|--40	.20	.24	.04	**	.23	.23	.00	-
40|--60	.21	.25	.04	**	.30	.28	-.02	**
60|--80	.20	.24	.04	**	.34	.31	-.04	**
80|--|100	.20	.24	.04	**	.36	.31	-.05	**
Total	.20	.24	.04	**	.25	.24	.00	**
Inter-quintile (1st-5th)	.00	-.01	-.01	**	-.19	-.13	.06	**

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

Table A - 47: School infrastructure by *BF* intake 2005-2007

Level of BF intake	Facilities index 2005 Mean	Facilities index 2007 Mean	Δ	Signif.(1)	Equipment Index 2005 Mean	Equipment Index 2007 Mean	Δ	Signif.(1)	Utilities index 2005 Mean	Utilities index 2007 Mean	Δ	Signif.(1)
0|--20	.526	.535	.01	*	.71	.71	.00	-	.91	.93	.02	**
20|--40	.46	.49	.03	**	.62	.65	.02	**	.87	.88	.01	**
40|--60	.39	.44	.05	**	.53	.59	.06	**	.83	.85	.02	**
60|--80	.32	.40	.08	**	.43	.53	.10	**	.80	.82	.02	**
80|--|100	.30	.39	.08	**	.35	.51	.16	**	.81	.79	-.01	-
Total	.44	.47	.03	**	.60	.63	.03	**	.86	.87	.01	**
Inter-quintile gap (1st-5th)	.22	.15	-.07	**	.36	.20	-.16	**	.10	.14	.03	**

Level of BF intake	Computer room 2005 Prop.	Computer room 2007 Prop.	Δ	Signif.(1)	Sc. Laboratory 2005 Prop.	Sc. Laboratory 2007 Prop.	Δ	Signif.(1)	Library/Reding room 2005 Prop.	Library/Reding room 2007 Prop.	Δ	Signif.(1)
0|--20	.43	.51	.08	**	.21	.18	-.02	**	.75	.77	.01	-
20|--40	.30	.43	.12	**	.12	.12	.00	-	.67	.69	.02	**
40|--60	.19	.34	.14	**	.07	.08	.01	*	.54	.61	.06	**
60|--80	.12	.26	.15	**	.04	.06	.02	**	.41	.53	.12	**
80|--|100	.06	.24	.18	**	.02	.06	.04	-	.37	.51	.14	*
Total	.28	.40	.12	**	.11	.11	.00	-	.63	.66	.03	**
Inter-quintile gap (1st-5th)	.38	.28	-.10	**	.19	.12	-.06	**	.38	.26	-.12	**

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

Table A - 48: Class size and teachers (1st- 4th grades) by *BF* intake 2005-2007

Level of BF intake	Class Size (1st-4th)		Δ	Signif.(1)	Prop. HE teachers (1st-4th)		Δ	Signif.(1)
	2005	2007			2005	2007		
	Mean	Mean			Mean	Mean		
0|--20	27.85	26.99	-.87	**	.67	.76	.09	**
20|--40	28.42	26.51	-1.91	**	.57	.68	.12	**
40|--60	28.67	26.27	-2.40	**	.47	.61	.14	**
60|--80	28.43	26.19	-2.24	**	.39	.54	.15	**
80|--|100	29.06	26.06	-3.00	**	.40	.54	.14	**
Total	28.41	26.50	-1.91	**	.54	.66	.12	**
Inter-quintile gap (1st-5th)	-1.21	.93	2.14	**	.26	.22	-.05	**

(1) – Not significant, * significant at 5% level; ** significant at 1% level.

Table A - 49: Fixed-effects regression coefficients and robust standard errors of school outcomes on *BF* intake, school composition, and school resources – 4th grade.

	VARIABLES	(1) Portuguese	(2) Maths	(3) Pass-grade	(4) Dropout
Bolsa Familia	*BF* (%)	-0.041***	0.002	-0.015**	0.014***
		(0.007)	(0.008)	(0.007)	(0.004)
	Year2007	-1.008***	11.565***	0.894***	-0.004
		(0.231)	(0.256)	(0.242)	(0.122)
	Year2007**BF* (%)	0.028***	-0.040***	0.041***	-0.039***
		(0.006)	(0.007)	(0.006)	(0.003)
School Composition (students' and households' characteristics)	Prop. living in female headed households	1.261	2.141**	-0.254	-0.649
		(0.926)	(0.973)	(0.807)	(0.408)
	Prop. whose mother has post-primary education	6.335***	5.931***	1.699**	-1.238***
		(0.859)	(0.940)	(0.808)	(0.446)
	Prop. whose father has post-primary education	-0.674	-0.410	-0.440	0.854**
		(0.785)	(0.843)	(0.703)	(0.362)
	Prop. of boys	-7.704***	-0.006	-0.574	-0.471
		(0.892)	(0.959)	(0.798)	(0.415)
	Prop. of non-whites	-0.188	0.372	-0.241	-0.278
		(0.716)	(0.801)	(0.630)	(0.317)
	Prop. who attended pre-school	8.650***	8.876***	1.621**	-1.489***
		(0.810)	(0.902)	(0.773)	(0.426)
	Prop. living in large families (7 or more)	-16.332***	-13.122***	-1.706*	0.595
		(1.004)	(1.090)	(0.993)	(0.535)
	Prop. who have failed any grade	-16.206***	-13.258***	-0.630	0.969**
		(0.783)	(0.877)	(0.802)	(0.422)
School Resources	Computer room	-0.170	-0.085	-0.121	-0.035
		(0.237)	(0.260)	(0.230)	(0.115)
	Laboratory	-0.756*	-0.520	-0.666*	0.212
		(0.401)	(0.439)	(0.374)	(0.160)
	School Food available	1.236***	0.736	-0.297	0.092
		(0.423)	(0.451)	(0.408)	(0.230)
	Library/Reading room	0.555**	0.792***	-0.206	0.042
		(0.232)	(0.261)	(0.227)	(0.120)
	Facilities index	-0.423	-3.157***	3.112***	-0.544
		(0.923)	(1.016)	(0.892)	(0.473)
	Equipment Index	1.678***	1.429**	1.158**	-0.922***
		(0.568)	(0.632)	(0.570)	(0.295)
	Internet available	0.598***	1.038***	0.309*	0.002
		(0.162)	(0.178)	(0.160)	(0.086)
	Utilities index	0.953	1.417	0.645	-1.097*
		(0.931)	(1.030)	(0.921)	(0.564)
	Class Size (1st to 4th grade)	-0.050**	-0.044*	-0.055**	0.074***
		(0.022)	(0.025)	(0.025)	(0.016)
	Prop. teachers with HE (1st to 4th grade)	1.233***	0.790**	-1.166***	0.323*
		(0.357)	(0.392)	(0.349)	(0.176)
Test Scores	Mean Test Score PORT			0.063***	-0.015**
				(0.011)	(0.006)
	Mean Test Score MATHS			0.001	0.016***
				(0.010)	(0.005)
	Constant	176.604***	177.445***	74.141***	4.004***
		(1.589)	(1.759)	(2.056)	(1.102)
	Observations	44,359	44,359	44,359	44,359
	R-squared	0.47	0.39	0.16	0.10
	Number of PK_COD_ENTIDADE	23,747	23,747	23,747	23,747

Figure A- 2: Marginal effects of *BF* intake on test scores (Portuguese) by region

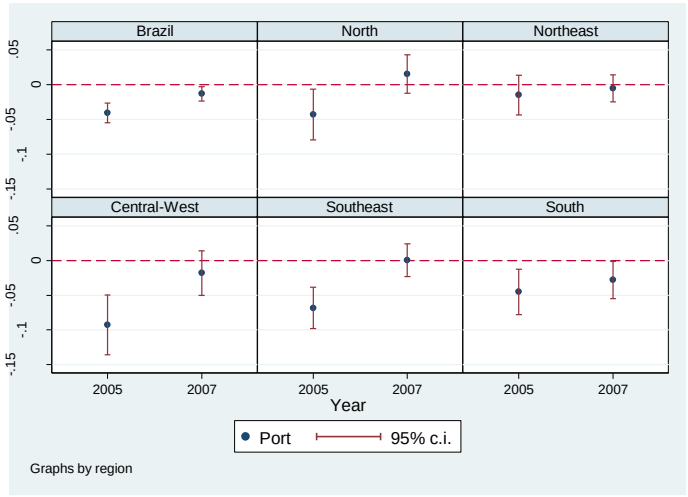


Figure A- 3: Marginal effects of *BF* intake on pass-grade rates by region

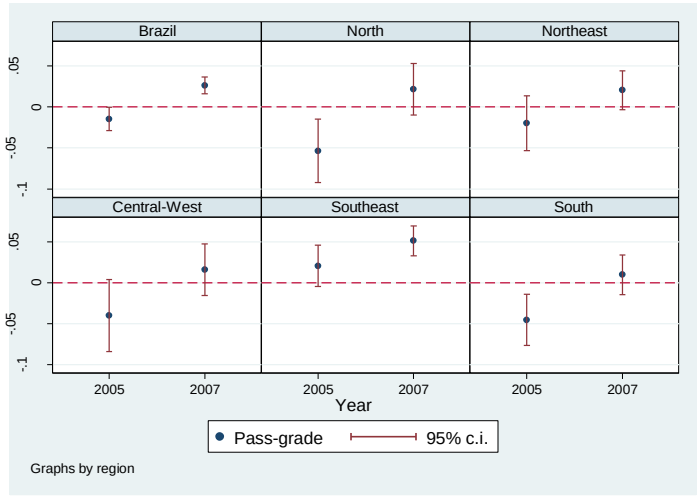


Figure A- 4: Marginal effects of *BF* intake on dropout rates by region

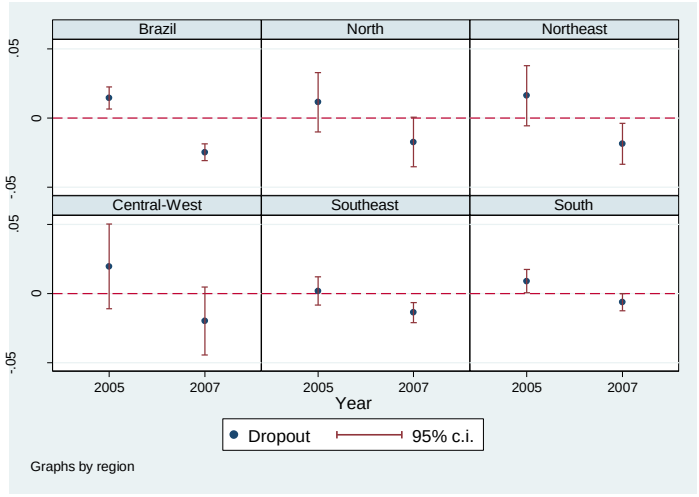


Figure A- 5: Marginal effects of *BF* intake on test scores (Mathematics) by region