University of New South Wales
School of Economics

Honours Thesis

Best Interests at Heart?

Modelling Determinants and Estimating Consequences of Early School Enrolment

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Abstract

Depending on the state of residence, Australian children born between January and July can become eligible for school up to a year earlier than the remainder of their birth cohort. For these children, their parents must decide if they commence school when they are first eligible, and be relatively young for their year, or if they will delay, and be relatively old for their year. Early enrolment has been shown to be consequential for childhood development, however, the reality is that this decision is not made with the welfare of the child solely in mind. Parents are influenced by a conglomerate of factors including the explicit and implicit costs of childcare.

Since 2000, the Australian government has been committed to the Affordable Childcare policy, a series of subsidies and tax rebates with the primary objective of increasing the female labour force participation and reducing cost of living pressures. The introduction of this policy creates an experimental opportunity to analyse both the determinants as well as the consequences of early enrolment. This thesis will utilise the Longitudinal Study of Australian Children (LSAC), a comprehensive data set that focuses on early childhood and adolescent development, to analyse the effect of early enrolment on multiple outcomes. This thesis finds evidence that the decision to enrol early is significantly influenced by the cost of childcare and that this policy has reduced the likelihood of early enrolment. Furthermore, this thesis demonstrates that by providing parents with an alternative to early enrolment through the subsidisation of childcare, children are unambiguously better off.

[0]This research uses data from the Longitudinal Study of Australian Children (LSAC). These data are the property of the Australian Government Department of Social Services. LSAC is an initiative of the Australian Government Department of Social Service (`www.dss.gov.au`), and is being undertaken in partnership with the Australian Institute of Family Studies (`www.aifs.gov.au`).

[0]This paper uses unit record data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey. The HILDA Project was initiated and is funded by the Australian Government Department of Social Services (DSS) and is managed by the Melbourne Institute of Applied Economic and Social Research (Melbourne Institute). The findings and views reported in this paper, however, are those of the authors and should not be attributed to either DSS or the Melbourne Institute.

Declaration

I declare that the work presented in this Honours thesis is, to the best of my knowledge and belief, original and my own work, except where acknowledged in the text, and that the material has not be submitted, either in whole or in part, for a degree at this or any other university

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Stephen Edward Burgess

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Contents

Chapter 1
Introduction

It is extremely difficult to judge the value of environmental factors on the cognitive and non-cognitive development of children prior to commencing school, however, Elder and Lubotsky (2009) argue that its importance is often understated. A child's brain is particularly malleable at these young ages and they develop most rapidly between the ages of 2-5 (Heckman, Elango, Garcia, and Hojman, 2015). Beyond learning language and improving coordination, children of this age also learn social skills, adopt behaviours and begin to develop their unique personality (Love, Kisker, Ross, Raikes, Constantine, and Vogel, 2005). As a result, inequality of opportunity at this young age is of great concern to parents, as well as policy makers, as environmental factors may shape development trajectories and early human capital accumulation.

In Australia, the objective of regulating the minimum school starting age is to ensure that children enter their formal education at comparable levels and are not advantaged or disadvantaged due to age differences. These laws also aim to take the decision out of the hands of parents who face different costs, incentives and circumstances associated with caring for children. These minimum starting ages, however, are not universally applied across states or across individuals. Misalignment between the school calendar and cutoff dates mean children are first eligible for school at different ages. In Australia, depending on the state the family resides in, the cut off date of school enrolment leaves the parents of children born between January and July with some discretion as to the age at which their child commences kindergarten. For all other children born in the second half of the year, parents are not presented with a choice. This discrepancy has generated considerable interest in understanding the impact early enrolment has on outcomes.

For many parents, the decision to hasten or delay school entry is a complex, multifaceted function of a number of variables including costs, perceived ability and maturity, location, access to affordable childcare, labour market participation and the availability of non-cost care. To evaluate the consequences of early enrolment this research must address the correlation between drivers of early enrolment and factors that influence children's outcomes. By consequence of giving parents a choice in the enrolment decision, any simple regression estimations of the impact of age on outcomes will likely be biased due to unobservables (Jha, 2014). As a result, most of the related research has relied on experimental design or instrumental variables to disentangle the consequences from the determinants of early enrolment. These methods, particularly instrumental variable analysis, have relied heavily on using proximity to enrolment cutoffs as a single discontinuity for testing. This method assumes that date of birth is the single determinant of enrolment and thus, if birth dates are randomly selected, provides a suitable testing scenario for the benefits and costs associated with early enrolment. Bedard and Dhuey (2006) suggest such experiments work well in countries with high levels of compliance to these regulations, such the United States, where approximately 95% of children commence school when they are first eligible. However, in Australia, this statistic is much closer to 50% suggesting there are other significant factors contributing to the decision (Edwards, Taylor, and Fiorini, 2011). In this paper, I present a unique opportunity to study different determinants, primarily cost and ability, when considering the impact of early enrolment on several outcomes. Diverging from the existing literature, I use the introduction of a universal tax rebate on childcare to perform a natural experiment to assess the responsiveness of early enrolment to a significant reduction in cost. If, as I assume, parents do hold their children's best interest at heart, the subsidisation of childcare costs will encourage greater attention to be paid to ability when deciding between early or delayed enrolment.

In 2000, the Australian government committed to the "Affordable Childcare Policy", a series of policies aimed at addressing cost of living pressures for Australian families. It is widely regarded that the policy encouraged greater female participation in the labour force (see Doiron and Kalb 2002; Kalb and Wang-Shen 2008), however, the effect it has had on children's outcomes are less understood. Treatment, in this experiment, refers to the introduction of the childcare subsidy in 2005, which provided parents of eligible children with an alternative to early enrolment. This paper finds evidence that cost of childcare is a considerable factor in the decision to enrol children early and the implementation of the rebate has led to a reduced likelihood of early enrolment by approximately 5%. I find evidence of heterogeneity in the model suggesting that the policy impacts certain sub-populations more than others, most notably, boys of low-income families. The comprehensiveness of the Longitudinal Study of Australian Children (LSAC) allows this research to assess the impact early enrolment has on multiple aspects of early childhood development. The treatment cohort is characterised by improved standardised test scores, fewer instances of kindergarten repetition and improved sociability outcomes subsequent to the introduction of the policy. These results suggest that parents do give greater consideration to a child's maturity when determining enrolment after the policy.

Chapter 2
Literature Review

2.1 Early Enrolment, School Starting Age and Children's Outcomes

The literature on early enrolment and, more generally, school starting age is expansive; for brevity, I summarise the consistent findings as well as areas of contention and then focus on a number of papers that present and use similar methodologies to the one conducted here. Children who commence school at 4 instead of 5 will have a year less life experience which influences a number of cognitive and non-cognitive outcomes (Baker, Gruber, and Milligan, 2008). The literature suggests that after controlling for other factors, age alone may influence performance in two ways. The "relative age" theory suggests that children who are relatively young for their year tend to underperform due simply to fewer years of life with which to learn and develop (Elder and Lubotsky, 2009). This experience gap will manifest itself in poorer communication and social skills that may be an impediment to school learning. The "entry-age achievement gap" theory builds on from this to suggest that a maturity gap will persist if, as a result of their additional experience, older children are sorted into higher graded classes, such as reading groups and math classes, that better prepare them for success in later schooling (Jha, 2014). It is posited that the relative age effect will equalise as children mature, yet if the entry-age achievement gap is present then it is possible that this disadvantage will continue throughout their school life.

Elder and Lubotsky (2009) also argue a third reason for ongoing disadvantage: incorrect diagnosis of behavioural disorders and grade repetition. The argument follows that parents and particularly teachers identify maturity gaps in younger children and wrongly attribute them to behavioural disorders (Elder and Lubotsky, 2009). Labels such as Attention Deficit Disorder (ADD) or the stigma of grade retention can have long-lasting impacts on a child's self-esteem as well as parents' and teachers' expectations for the child's potential (Parker, Marsh, and Thoemmes, 2018). Given these three mechanisms, the longevity of the overall disadvantage is what stimulates much of the debate in the literature. Amongst commentators, opinions range from 2-3 years for academic outcomes (Elder and Lubotsky 2009; Jha 2014) to suggestions that behavioural outcomes persist into adulthood (Black, Devereux, and Salvanes, 2011).

Largely seen as one of the pioneering papers in this area of research, Elder and Lubotsky (2009) test changes in American state laws that govern school entrance age and find that children who are relatively old for their year perform better across academic outcomes. Their estimation relies on the construction of an instrumental variable, predicted entry age, a method that has been utilised by other studies as well (Bedard and Dhuey 2006; Black et al. 2011; Datar and Gottfried 2015). Predicted entry age instruments are constructed by assuming that children start school when they are first eligible and that season of birth is unrelated to their educational outcomes. Using this instrument, Elder and Lubotsky (2009) find strong evidence for the relative age theory; younger children perform poorer in early years due to the fewer months they have been alive and learning.

Through a simple model of human capital accumulation, Elder and Lubotsky (2009) highlight the importance of pre-kindergarten education on development and attribute the success of older children to marginally greater life experience at the age they enter school. This life experience is labelled "parental investment" and describes not only parental care but formal childcare investment that equip the child with important skills such as social awareness and language (Cuhna and Heckman 2008; Cuhna, Heckman, and Shennach 2010; Elder and Lubotsky 2009). However, the authors find that this investment only prepares the child for school but does not increase their ability to absorb information. As as a result, the effect of being relatively older diminishes over time and is unobservable in later years of school. Although the effect on formal academic outcomes diminished with time, it also impacts on other early childhood outcomes. The authors found that early enrolment increases the prevalence of ADHD by 25% (Elder and Lubotsky, 2009). Elder and Lubotsky (2009) predict a positive relationship between achievement and socio-economic status as a further consequence of the parental investment model. This idea has implications for public policy and Suziedelyte and Zhu (2015) and Deming (2009) both find evidence that early enrolment in school is successful in closing gaps between advantaged and disadvantaged children in Australia and the United States respectively.

In one of the few papers that focus exclusively on long-term impacts, Black et al. (2011) use the same methodology as Elder and Lubotsky (2009) and utilise a predicted starting age instrument to examine early enrolment in Norway. Using IQ scores and census data the authors seek to tease out any identifiable effect of early enrolment in adulthood. The authors find no evidence that early starters perform worse in standardised IQ tests later in life leading them to argue, consistent with Elder and Lubotsky (2009), that any educational disadvantage is only short term.

They do, however, find evidence of higher risk-taking behaviour measured through observing teen pregnancy. Their conclusion is that early starters are more likely to be forced to mature faster as a result of their relative age. Consequently, they will face adult decisions at an earlier age than their peers and this may lead to excessive risk-taking.

In an Australian context, Suziedelyte and Zhu (2015) attempt to measure the impact of early enrolment using a regression discontinuity (RD) design. Fuzzy RD is possible in this context due to the jump in early enrolment at the cutoff dates with a reasonable, although still debated, assumption that parents are unable to manipulate the running variable of the child's birthday (Angrist and Krueger, 1999). By assumption, children around the cutoff are comparable and treatment (early enrolment) is as good as randomly assigned to children at the threshold. Suziedelyte and Zhu (2015) are primarily interested in testing whether children of differing levels of affluence and opportunity are equally affected by starting early. The measures of affluence and opportunity include income, English as a second language (ESL) status and single-parent status. The study's findings are somewhat mixed. Irrespective of opportunity, social and behavioural outcomes are worse as a result of starting early relative to the counterfactual. This result, however, is less pronounced for disadvantaged children, perhaps as a result of increased discipline and supervision (Suziedelyte and Zhu, 2015). Both advantaged and disadvantaged children display statistically significant improvements in terms of vocabulary measures, however, problem-solving ability improves only for disadvantaged children. These results, they argue, are indicative of differences in parental investment first described by Elder and Lubotsky (2009). Differences in advantaged and disadvantaged results in school are relative to counterfactuals observed for each cohort. Disadvantaged children may have less engagement and stimulation in alternative care measures and therefore sending them to school may improve some, but not all, of their outcomes (Cascio and Lewis (2006); Fitzpatrick, Grissmer, and Hastedt (2011); Gormely, Gayer, Phillips, and Dawson (2005)).

As demonstrated through the papers discussed so far, instrumental variable analysis is the dominating methodology of analysing the outcomes of early enrolment in the literature. It is worth noting that a number of studies (Bedard and Dhuey 2006; Edwards et al. 2011; Jha 2014) question the validity of using predicted starting age as an instrumental variable in countries that have poor compliance to the predicted starting age. In the United States, approximately 95% of students enrol when they are first eligible (Bedard and Dhuey, 2006). In Australia, however, the percentage of eligible children who delay entry is approximately 50% (Edwards et al., 2011). Consequently, a number of studies, including Jha (2014), explore alternative methodologies to estimate the effects of early enrolment on outcomes.

Jha (2014) uses the introduction of preparatory schooling and an increase in the entry age in Western Australia and Queensland as a basis for a natural experiment. Western Australia changed starting age from 5 to 6 in 2001-2002 and Queensland followed suit in 2007-2008 creating two separate observable difference in differences estimations with the other Australian states acting as the control. The author finds a consistent and positive increase in standardised NAPLAN scores across multiple disciplines in Western Australia and Queensland (Jha, 2014). NAPLAN is a national, standardised testing program for school children conducted in years 3,5,7 and 9 of their formal education. NAPLAN covers disciplines of reading, writing, numeracy and language conventions including grammar, punctuation and spelling (ACARA, 2016). Similar to other studies, differences in scores between treatment and control are more pronounced in earlier years of school but are still evident in high school education. Using year 5 NAPLAN scores as a base, Jha (2014) predicts that the children in the control are, on average over the NAPLAN disciplines, 3 months behind their treatment cohort as a result of commencing school earlier in life.

Some papers such as Edwards et al. (2011), have been less concerned with consequences, rather have chosen to focus exclusively on the early enrolment decision itself. From running simple logit models on the binary decision to enrol or not, Edwards et al. (2011) find two important findings that will be relevant to this research. Firstly, girls are more likely to start early than boys and, secondly, income is the primary determinant of enrolment amongst eligible children. Other studies (Cascio and Lewis 2006; Deming 2009; Fyer and Levitt 2004; Gormely et al. 2005) have examined how ethnicity influences enrolment and found non-whites, particularly of lower income families, are considerably more likely to commence school when it is first available. Through these papers that focus on determinants, a noticeable commonality is that socio-economic circumstance strongly influences the decision to enrol early amongst children born within the window. This motivates the use of a policy that affects cost of childcare as a basis for an experiment.

A result of the childcare policy that is not discussed in depth in this thesis is the impact that the policy has had on female participation. For analysis of this topic in Australia and abroad see Connelly 1992; Doiron and Kalb 2002; Gathmann and Sass 2018; Kalb and Wang-Shen 2008; Ribar 1995. Fairly consistently, but with varying degrees of elasticity, these papers find that childcare subsidies have increased levels of female participation as well as the number of children attending formal childcare. Whilst opinion is mixed with regards to infantile and very early childhood development, it is largely posited by the existing literature that childcare has beneficial impacts in terms of preparing children for the transition to school. A number of studies have attributed better school performance to non-primary early childhood education programs such as “Head Start” in the United States (Cuhna and Heckman, 2008; Cuhna et al., 2010; Deming, 2009; Heckman et al., 2015). An important caveat to this, however, is that this does not appear to be the case for universal childcare programs. Such programs have proven to be costly with no significant benefit on later outcomes for children as private, good quality care providers are crowded out by public providers (Baker et al., 2008; Cascio and Schanzenbach, 2013; Lefebvre, Merrigan, and Roy-Desrosiers, 2011). This body of research is significant to this thesis because implicitly assumed is that any impact that the childcare subsidy has on children’s outcomes is a result of an additional year of formal, non-primary education. Some of the effect may be attributable to simply having an additional year in which to mature, however, it is also expected that children this age generally stand to benefit from early childhood education relative to other types of informal care.

Chapter 3
Minimum Starting Age and Australia's Childcare Policies

3.1 Minimum School Starting Age

In Australia, the legislation determining school starting age is overseen by individual state governments. With the exception of Queensland, who until 2008 started school between 6 and 7 years old, the laws fairly universally dictate that children can start school if they turn 5 prior to the state-based cutoff and must start school before their sixth birthday. Table 3.1 summarises the enrolment laws of each state as of 2005. For most children born in the latter half of the year, this means they begin school after their fifth birthday. However, for a large proportion of children born before the cutoff dates, their actual enrolment year is determined by their parents. Parents must decide if the child will start just before the age of five and be relatively young for their cohort, or start just before the age of six and be relatively old for their cohort. The issue is most pronounced in New South Wales where more than 1 in 2 children will be able to enrol in multiple years.

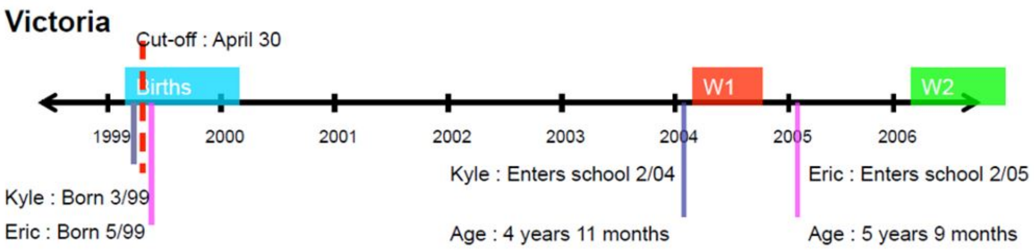
Table 3.1: School entry policies by state in 2005 (Edwards et al., 2011)

State	**Program**	**Mandatory Age**	**Cut-off Date**
	Year before Year 1	Year child turns:	Turns 5 by:
New South Wales	Kindergarten	6	31st July
Victoria	Preparatory	6	30th April
Queensland	Year 1	7	30th June
South Australia	Reception	6	30th April
Western Australia	Pre-Primary	6	30th June
Northern Territory	Transition	6	30th June
Tasmania	Preparatory	6	1st January
Australian Capital Territory	Kindergarten	6	30th April

Figure 3.1 demonstrates the nature of the problem more clearly. Consider two children, Eric and Kyle, who are born in Victoria either side of the cut-off date of April 30th. Although these children are born only days apart, Kyle is eligible for school a year earlier than Eric as he will turn 5 before the cut-off. Kyle's parents will need to decide if Kyle is to start school at 4 years and 11 months or whether they delay his schooling until he is 5 years and 11 months old. Deciding on the latter

will come at the cost of providing his care either personally or externally for another year. "Redshirting" is the term commonly given in the literature to describe when parents intentionally hold back their children. Edwards et al. (2011) refer to this as the "gift of time", the opportunity to take more time to develop emotionally and cognitively in preparation for school. I find, based on the data, that approximately 47% and 51% of eligible children are redshirted in 2004 and 2008.

Figure 3.1: Hypothetical scenario demonstrating the early enrolment decision

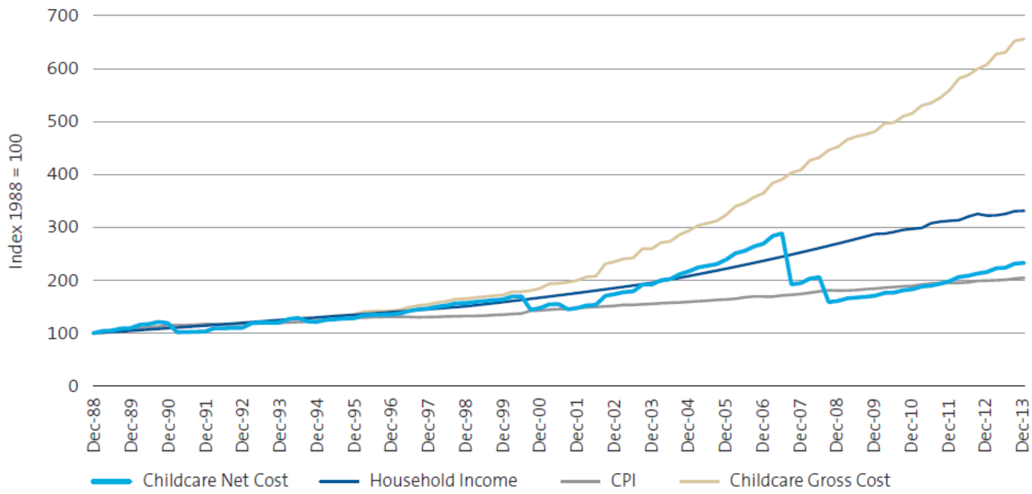


Source: Australian Institute of Family Studies AIFS (2018)

3.2 Childcare Policy In Australia Since 2000

In 2000, the Howard Government committed to "Australia's Affordable Childcare" policy in order to boost labour force participation and ease cost of living pressures. Figure 3.2 shows the increasing net cost of childcare over time. The gross cost of childcare has been steadily increasing over time, although the introduction of the childcare policy appears to have accelerated this trajectory. This is likely due to an increase in demand on account of increased female participation in the labour force and subsidies. The Affordable Childcare policy has consisted of several stages, including the introduction of the CCB in 2000, the CCTR in 2005 and subsequent amendment to the CCTR in 2008. The dips observable in net costs occurring in 2000, 2005-06 and in 2007-08 are all consistent with the incremental introduction of various components of the affordable childcare policy discussed below. This figure demonstrates, through the divergence between net and gross costs, the validity of using the childcare rebate as an experimental tool. The divergence between net and gross costs demonstrates how these policies have affected household budgets. Furthermore, the various amendments to the policy appear to be the only discernible shocks to childcare costs over the past 20 years.

Figure 3.2: Index of childcare costs overtime



Source: AMP NATSEM Income and Wealth Report, Sainsbury (2014)

3.2.1 Child Care Benefit (CCB)

The first instalment to Australia's "Affordable Childcare policy" came in July 2000 with the introduction of a means-tested subsidy for eligible families called the Child Care Benefit (CCB) (McIntosh, 2013). The CCB operated as a means-tested subsidy that was paid directly to the individual. The minimum payment for the CCB was \$21.70 per week until 2002 when the amount was allowed to decline to zero dependent on income level. The maximum amount that could be claimed per week, per child under the CCB was \$144 dollars. In order to be eligible for the CCB, parents were required to be either engaged in or actively seeking part or full-time work, study or unpaid volunteering in excess of 15 hours a week and use a paid childcare provider (McIntosh, 2013). Eligible childcare providers include daycare, after school or occasional care but excluded nannies, relatives or other non-related, unpaid childcare providers.

3.2.2 Child Care Tax Rebate (CCTR)

The second instalment, the childcare tax rebate (CCTR), came in 2005 and was a non-means-tested tax rebate that allowed families to reclaim 30% of out of pocket expenses up to a maximum \$4000 dollars per year, per child (APH, 2005). The CCTR was an election promise made by the Howard government at the end of 2004 and came into effect from 2005 in response to the increasing costs of childcare. Despite the rebate being indexed to inflation, in 2008 the policy was amended to 50% of costs up to \$7500 and furthermore, in 2009 was no longer a tax deduction rather a direct payment (McIntosh, 2013). The CCTR employs the same eligibility criteria as the CCB. Table 3.2 demonstrates how the combined impact of the rebate and the benefit would alleviate childcare costs for one-child families of varying income levels (John, 2013).

Table 3.2: CCB and CCTR entitlement by income level (July 2005)

Taxable Income	CCB/Week	Out Of Pocket	CCTR/Week	Total (CCB &CCTR)	% CC Costs
30,000	144.00	56	16.80	160.80	80.4
50,000	112.00	88.00	26.40	138.40	69.2
70,000	73.54	126.46	37.94	111.48	55.7
100,000	24.15	175.85	52.76	76.91	38.5

Source: Department of Parliamentary Services, John (2013)

Note: These numbers are calculated on the assumption that the family has only 1 child with child care costs amounting to $200 per week

3.2.3 The New Child Care Subsidy (CCS)

Whilst this policy change is not examined by this research, as of July 2018 the CCB and the CCTR were replaced by the CCS. The CCS has returned to a means-tested system, however, the key difference is that payments are made directly to childcare providers rather than family's Centrelink accounts. Families who have a combined income of less than $66,958 will be eligible for a subsidy equating to 85% of childcare costs with this percentage declining up to a combined income of $351,248 with a 20% subsidy (DET, 2018). Eligibility requirements remain unchanged from previous policies.

Chapter 4
Data

4.1 Longitudinal Study of Australian Children (LSAC)

This study utilises the Longitudinal Study of Australian Children (LSAC) conducted by the Australian Institute of Family Studies. LSAC is a panel data set that follows two cohorts of children surveyed every two years over several waves throughout their schooling life. The focus of LSAC is childhood and adolescent development and gathers information from a diverse range of sources. It utilises responses from parents, teachers and the child across multiple areas of child development as well as tracing national standardised tests scores from children's NAPLAN exams. LSAC also provides Medicare information and MySchool data as well as a limited time-use diary. For the purposes of this study, data is primarily drawn from the LSAC survey which includes interviews of parents, carers and teachers conducted every two years. This thesis will also utilise the NAPLAN scores to compare academic outcomes of the control and treatment cohorts. To control for school effects, several variables will be drawn from MySchool data, a comprehensive data set of Australian schools that is also included as part of LSAC. This study compares cohort K from wave 1, interviewed in 2004 and cohort B of wave 3, interviewed in 2008 (see Figure 4.1).

Figure 4.1: LSAC cohort structure

Cohor	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
B cohort	0–1 yrs	2–3 yrs	4–5 yrs	6–7 yrs	8–9 yrs	10–11 yrs
K cohort	4–5 yrs	6–7 yrs	8–9 yrs	10–11 yrs	12–13 yrs	14–15 yrs

Source: Australian Institute of Family Studies AIFS (2018)
Note: For the primary analysis, children in cohort K who are 4-5 years old in Wave 1 are compared with children born in cohort B who are 4-5 years old in wave 3.

We encounter a few issues in using the LSAC data set due to its repeated cross-section structure. The cohorts create inflexibility around testing the enrolment decision as we only observe two years of data in which subjects are 4-5 years old. The first cohort will either be entering school in 2004/05 and the second cohort will be entering school in 2008/09. Whilst this is either side of our policy and creates a testable environment, the length of time between cohorts has the potential to compromise the power of the conclusions. I describe how I account for this in Chapter 5. From the initial 8,232 observations, I exclude children born in Queensland (1,570 observations) and Northern Territory (123 observations) due to the fact that these states did not have compulsory Kindergarten at the time of the survey, which means that these parents make the schooling decision a year later than the rest of Australia. Tasmania is also excluded (198 observations) as the cutoff date of January 1st does not generate a window. An additional 998 observation due to lacking critical information such as state of residence, birth date or school enrolment status leaving 5,343 observations. The number of observations in regressions may differ from 5,343 due to missing dependent variables that are automatically excluded.

4.1.1 Dependent Variables

Early Enrolment Status

The primary question to be answered in this research will be how the likelihood of early enrolment changes after a substantial reduction in childcare costs. Therefore, I generate a dependent variable from the data based on the enrolment status of children. The interviews for LSAC are all conducted between March and November of 2004/08 so it is possible to determine which children are enrolled in school as of January 2004/08 and those that are still in care. This binary variable indicates whether the child has been enrolled in school prior to their fifth birthday.

NAPLAN Scores

The National Assessment Program, Literacy and Numeracy (NAPLAN) is a series of national tests conducted in years 3, 5, 7 and 9 of a child's schooling (ACARA, 2016). They are designed to follow a nationwide curriculum and tests five specific subjects: reading, writing, spelling, grammar and punctuation and numeracy. Scores on each of these subjects are between 0 and 1000 irrespective of year and are designed to measure "absolute competence", meaning scores increase as the student progresses through schooling. On average, this incremental increase between years is approximately 50 points for lower years and 25 points for higher years for each subject area (Jha, 2014). Importantly for our analysis, the test scores are designed be comparable across time, for example, a grade 3 test score in 2008 is comparable to grade 3 score in 2014.

Kindergarten Repetition

As part of the supplementary analysis of children's outcomes, we first examine the relationship between childcare costs and kindergarten repetition. LSAC data includes a dummy variable equal to one if the subject child has repeated kindergarten. Across the two cohorts, approximately 2% of children repeat their first year of school.

Social and Behavioural Characteristics

LSAC data contains survey responses from parents and teachers in relation to the subject child's behaviour, personality and temperament. These survey questions are taken from the Strengths and Difficulties Questionnaire (SDQ), an internationally recognised psychometric questionnaire for children and adolescents (Goodman, 1997). The data includes graded answers to specific questions such as "Please tick one box for each of the following statements to best describe the study childs behaviour over the past six months: Shares readily with other children (treats, toys, pencils, etc)". These survey questions are designed to be compiled to generate a final score across 3 outcomes; pro-social, hyperactivity and emotional responsiveness. LSAC utilises both parents and teachers to give added credibility to these scores and to mitigate issues such as parental bias or self-selection. Each quality receives a score based on responses to 10 questions. The subject may respond to each question with "Not at all", "Sometimes", or "Often". In this analysis, these responses are simplified to binary variables for ease of interpretation. Higher scores represent that the child is more social, hyper or emotional meaning sociability is the only test where higher scores are considered more positive outcomes. Higher scores on hyperactivity and emotional responsiveness indicate poorer outcomes. This thesis will consider the responses of both parents and teachers in the SDQ, however, greater importance will be given to teacher responses due to potential bias of parents.

Anti-social Behaviour (Bullying)

In this study, I also seek to measure children's disposition towards or subjectivity to antisocial behaviour. I take responses from teachers with regards to school bullying and generate two binary variables. The first variable is equal to 1 if the child is known to bully other children, and 0 otherwise. The second variable is equal to 1 if the child is bullied by other children, and zero otherwise. I choose not to use similar variables derived from parent's responses to bullying outcomes due to subjectivity. It is also likely that teachers are better informed about bullying in school than parents.

4.1.2 Independent Variables

Policy Variable (CCTR)

The policy (CCTR) variable divides the sample into treated and non-treated status. As discussed above, LSAC is separated into two cohorts 4 years apart in age that commence school in different years (see Figure 4.1). As treatment in this case refers to the introduction of the tax rebate in 2005, this binary variable is equal to 1 if the child is interviewed in 2008 as part of cohort B and 0 if interviewed in 2004 as part of cohort K (see Figure 4.1). Children in the 2008 cohort would have had access to the tax rebate at the time the decision to commence school was made, whereas children in 2004 cohort would not have had access to the rebate.

Eligibility for Early Enrolment ('Window')

Based on the state-specific enrolment cutoffs discussed in Chapter 3, we generate a dummy variable equal to 1 if the child's birthday falls within the 'window' and 0 otherwise. This variable indicates that the child is eligible for early enrolment and parents will ultimately decide school starting age.

Parental/Socioeconomic Characteristics

I control for a number of relevant family characteristics and socio-economic indicators that may impact the subject child. Firstly, I control for the total family income using a quadratic term. I consider all income at a base year of 2004 using an inflation calculator in order to account for price level differences between the cohorts. Wealth status is more difficult to quantify, however, I include a binary variable that takes the value of 1 if the responding parent(s) owns the home they reside in. I also control for the education levels of the parents which will likely influence the attitudes towards education as well as influencing the labour market participation of both parents. The number of years of education is based on the highest level of qualification obtained by the parent (see Table 4.2). Religion is accounted for by a binary indicator as this may influence decisions around schooling. Also included is a binary variable equal to 1 if the responding parent (main carer) identifies as a single parent. The ethnicity of the child is controlled for by two binary variables that capture if the family identify as being Aboriginal or Torres Strait Islander (TSI) and if the family predominantly speak a language different from English at home (ESL).

Table 4.2: Constructing years of schooling variable based on highest level of completed education

Highest Level of Education	Estimated Years of Schooling
School Certificate	11
Higher School Certificate	12
Certificate III or IV	13
Advanced Diploma	14
Bachelor	16
Graduate Diploma	17
Masters or Doctorate	18

Child Characteristics

Controls for the child range from geographic location to measures of cognitive ability. Firstly, I control for the age in months of the child as of July 2004/08 in order to standardise relative ages of the child irrespective of when they were interviewed. This is in order to control for the expectation that children closer to the cutoff are less likely to enrol early than children further away. In the absence of more granular geographic information, I control for the state in which the child resides as well as a variable if the child does not live in a city or suburban neighbourhood. Included is a binary variable for gender and two separate count variables for the number of older and younger siblings. It is expected that older and younger siblings will impact childcare costs and family arrangements, however, they may do so in different ways. Therefore it is important to make the distinction and not just account for the total number of siblings. To control for any cognitive or physical impairment a binary variable is included which identifies if children have difficulty using limbs, brain impairment or other development issues. This is expected to influence the childcare decision by determining the amount and quality of care required. Furthermore, LSAC contains the results of a Peabody Picture test, a globally-recognised cognitive test used to gauge a child's comprehension of spoken words (Suziedelyte and Zhu, 2015). This variable is included in anticipation that parents will hold back children if they believe their comprehension is underdeveloped or alternatively, will proceed to enrol children they believe to be advanced learners. Also included are the child's reported height and weight measures.

School characteristics

I choose to control for student/teacher ratios and a binary variable equal to 1 if the school is an independent school and zero if a public school. These variables will be important when considering students individual performance in NAPLAN tests.

Table 4.1: List of covariates

Variable	Description
Policy Intervention	
CCTR	Dummy variable equal to 1 if subject child commences school after the policy
Individual Characteristics	
Window	Dummy variable equal to 1 if the subject child is born in the 'window' and eligible for early enrolment
Male	Dummy variable equal to 1 if subject child is male
Age in Months	Age of the child in months as at July 1st
Peabody Score	Peabody Picture Vocabulary Test score
Medical Condition	Dummy variable equal to 1 if the subject child has a pre-existing physical/mental impairment
Hyperactivity	SDQ Hyperactivity score
Emotional Responsiveness	SDQ Emotional Responsiveness score
Sociability	SDQ Sociability score
Height	Height in centimetres
Weight	Weight in Kilograms
Family Characteristics	
Rural	Dummy variable equal 1 if the subject child's postcode is considered rural by the survey
Religion	Dummy variable equal to 1 if affirmative response to religious indicator
Indigenous	Dummy variable equal to 1 if family is Aboriginal or TSI
English Second Language (ESL)	Dummy variable equal to 1 if English is not the main language at home
Younger Siblings	Number of younger siblings
Older Siblings	Number of older siblings
Parental Characteristics	
Single Parent	Dummy variable equal to 1 if one parent only living in the home
Owns Home	Dummy variable equal to 1 if family owns the home they reside in
Log Income	Log of family income including non-labour income
Log Income^2	Log of family income including non-labour income squared
Mother's Education (Years)	Years of education derived from highest level of obtained education (see table 4.2)
Father's Education (Years)	Years of education derived from highest level of obtained education (see table 4.2)
School Characteristics	
Student to teacher ratio	Ratio of full time enrolled students to full time employed teachers
Independent	Dummy variable equal to 1 if school is independent

4.1.3 Summary Statistics And Trend Analysis

HILDA and Trend Analysis

Of primary concern to any researcher seeking to evaluate the effect of public policy is the assumption that, without treatment, the treated and control cohorts follow a comparable trajectory. A further obstacle encountered when using LSAC is the inability to perform trend analysis due to the limited number of cohorts. In order to draw convincing conclusions and substantiate the parallel trend assumption, one would need to observe the behaviour of families at more than two cross-sections. For this, I rely on another comprehensive, Australian-based data set: the Household, Income and Labour Dynamics in Australia (HILDA) survey. HILDA is a household longitudinal data set produced by the Melbourne Institute of Applied Economic and Social Research that commenced in 2001 and is conducted annually. In the first wave, HILDA surveyed 19,914 individuals collecting information on a number of household and individual-specific subject areas.

I use HILDA data for trend analysis due to the difficulty faced in obtaining accurate enrolment statistics for children. A number of alternative datasets were considered, including census data and Department of Education data, however, these data sets only gave age distributions for each year. Age distributions did not provide accurate information regarding whether the child is born in the window, and therefore, whether or not they have commenced school early. From this, only the percentage of children of a certain age at a particular point in time could be discerned. HILDA does not contain comprehensive school data to the same effect as LSAC but does include a variable that indicates the year of school each child in the family is enrolled in as of 2012. Working backwards, it is possible to construct the age distribution of children commencing kindergarten between the year 2000 and 2012. The restricted release of HILDA also provides children's precise birthdays and ages facilitating the generation of variables for the window eligibility status and early enrolment status. For the windows, I apply the same cutoff restrictions as in the LSAC data set and represented in Table 3.1 of the previous section. Again, I exclude the states of Queensland, Northern Territory and Tasmania in accordance with the reasoning given in the LSAC analysis. Using these constructs, I generate the proportion of all school children who enrol early in each year between 2000 and 2012.

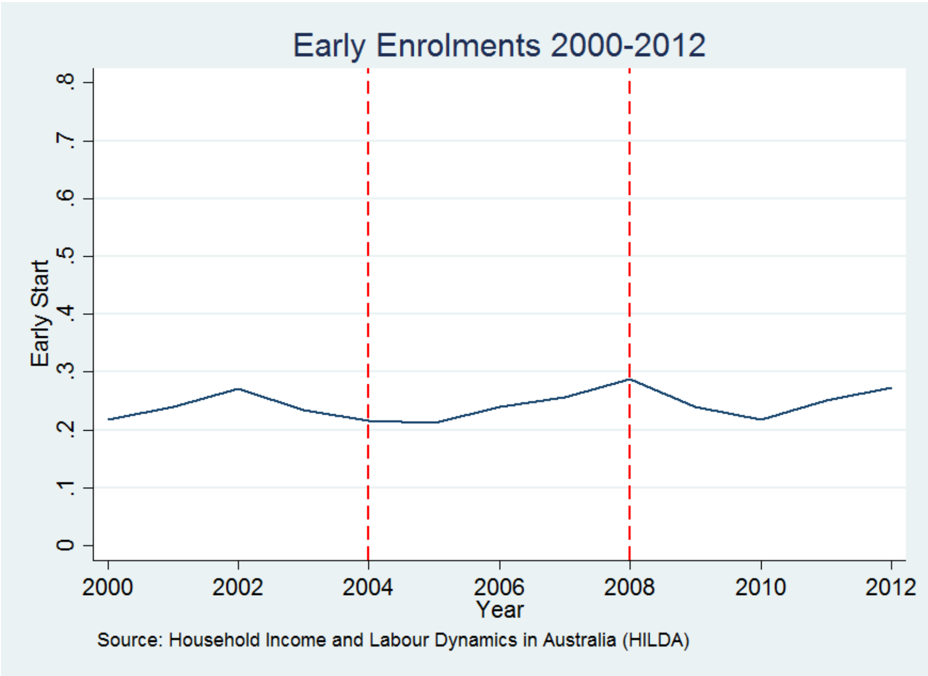
Table 4.3: Proportion of all school children starting early based on HILDA sample for years 2000-2012

	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012
Early	22%	24%	27%	24%	22%	21%	24%	26%	29%	24%	22%	25%	27%
Obs	534	677	660	812	718	883	848	779	834	855	869	842	841

Note: These percentages are calculated by self-reported answers to "year of school as of 2012". With this information, it is possible to work backwards to determine the year they of commencement.

Figure 4.3 provides a year by year summary of the HILDA early enrolment statistics as well as the number of observations. The sample sizes in each year give weight to this trend analysis, taking observations of over 10,000 children over 12 years. This time frame covers the period of interest where the rebate is introduced, however, earlier years would provide helpful pre-trend analysis before the introduction of the subsidy in 2000. Another potential limitation of this data is that HILDA does not contain statistics for grade repetition, meaning it is possible that a child judged as commencing school on time in fact started early and was subsequently held back. The only implication of this is that it may marginally underestimate the proportion of children who commence school early, however, I anticipate this to be of little consequence to the analysis.

Figure 4.2: Proportion of all school children starting early based on HILDA sample for years 2000-2012



Note: These percentages are calculated by self-reported information about year of school as of 2012. With this information it is possible to work backwards to determine the year they of commencement.

Figure 4.2 shows a reasonably stable trend over the 12 year period with early enrolment fluctuating between 20-30% of children. The percentages observed are corroborated by those found in Edwards et al. (2011) and independent research conducted by Access Economics (2006). Between 2004 and 2008 I observe an increase in the percentage of children enrolling early, however, this is unlikely to harm the analysis. The purpose of this trend analysis is to ensure that the results observed are not because of a consistent trajectory over time. For example, it would be difficult to justify a causal effect if, over time, fewer children were enrolling early due to an increased awareness of the consequences of enrolment. Interestingly, however, there is no immediately identifiable impact of either policy in this figure; the subsidy introduced in 2000 or the rebate in 2005. This suggests that before commencing the analysis, that the estimated effect, if any, is likely to be small and impact children at the margins. None the less, and importantly for the analysis, there appear to be reasonable grounds to support a parallel trend assumption.

Summary Statistics and Balance of Observable Covariates

Table 4.4 demonstrates the proportion of early enrolment in the two cohorts of LSAC. In 2004, the total number of children enrolling early is almost identical between the two datasets, whereas they diverge much more dramatically in 2008.

This doesn't particularly concern any interpretation as table 4.2 demonstrates these percentages are prone to fluctuate year to year.

Table 4.4: Proportion of children enrolling early (LSAC cohorts)

Proportion of Early Starters	2004	2008
% All Children	19%	14%
N	2,286	3,057
% Children in Window	53%	49%
N	825	864

Fundamental to the analysis is that comparison can be made between the cohorts from 2004 and 2008. Table 4.5 presents summary statistics and balance of covariates between children in Cohort 1 (2004) and Cohort 2 (2008). Most of the variables change minimally and support the assumption that these groups are worthy of comparison. After running a balance of covariates tests I find the only variable that might be of concern to the inferences is differences in yearly household income. I observe an increase in household income between 2004 and 2008 after accounting for inflation which may overstate the impact of a tax rebate on the affordability of childcare in 2008. The median scores of income change less dramatically, approximately $10,000, but are still concerning to our conclusions. The best we can do is to control for income and be aware of the potential bias.

Table 4.5: Balance of covariates measuring statistical differences between children in cohort K (2004) and cohort B (2008)

	(1)	(2)	(3)
	Cohort 1 (2004)	Cohort 2 (2008)	
	Mean	Mean	t
Age in Months (July)	59.20	58.62	6.41***
Male	0.51	0.51	0.12
Rural	0.11	0.12	-0.86
Religion	0.79	0.78	0.57
Mother's Education (Years)	13.61	13.95	-5.19***
Father's Education (Years)	13.27	13.45	-2.55*
Medical Condition	0.20	0.09	11.56***
Peabody Score	64.14	65.21	-6.53***
Single Parent	0.13	0.10	2.96**
English Second Language	0.12	0.10	2.05*
Younger Siblings	0.57	0.57	0.02
Older Siblingss	0.83	0.88	-1.92
Indigenous	0.02	0.03	-0.70
Owns Home	0.72	0.73	-0.83
Log Income	10.97	11.17	-11.42***
Hyper	3.46	3.19	4.42***
Emotional	1.68	1.43	5.46***
Social	7.73	7.75	-0.35
Height	132.22	134.36	-6.45***
Weight	31.18	32.09	-4.43***
N	2286	3057	5343

For good measure, I also include a balance of covariates test of children born inside and outside the window. This seeks to rule out any possibility that the month of birth is endogenous and potentially correlated with control variables such as ability. This was an issue raised by (Buckles and Hungerman, 2013) in terms of the validity of using season of birth as an instrument as in Angrist and Krueger (1999). Buckles and Hungerman (2013) argued that parents do manipulate the season of birth in the sense that planned pregnancies tend to fall in particular times of the year which may be correlated with outcomes. This is not as critical to this analysis because this thesis does not utilise instrumental variables. However, it is important to ensure that treatment and control groups are comparable, as is the case in any experimental design. For this purpose, I compare children born in and outside the window. From Table 4.6 it is clear that the two cohorts are comparable for the purposes of analysis. The only statistical differences on these outcomes stem from differences in age. Therefore I am not concerned that children born in the first half of the year are older, as well as taller and heavier on average.

Table 4.6: Balance of covariates measuring statistical differences between children born in and outside the window of early enrolment eligibility

	(1)	(2)	(3)
	Outside Window	Inside Window	
	Mean	Mean	t
Age in Months (July)	57.23	62.42	-98.70***
Male	0.51	0.50	1.07
Rural	0.11	0.13	-1.50
Religious Affiliation	0.77	0.80	-2.42*
Mother's Education (Years)	13.79	13.85	-0.88
Father's Education (Years)	13.40	13.31	1.22
Medical Condition	0.13	0.16	-2.52*
Peabody Score	64.23	65.88	-9.49***
Single Parent	0.11	0.12	-1.23
English Second Language	0.10	0.11	-0.83
Younger Siblings	0.56	0.59	-1.58
Older Siblings	0.86	0.86	0.13
Indigenous	0.02	0.03	-0.73
Owns Home	0.73	0.72	0.80
Log Income	11.09	11.07	0.80
Hyper	3.28	3.38	-1.62
Emotional	1.50	1.63	-2.58**
Social	7.73	7.76	-0.54
Height	133.07	134.24	-3.90***
Weight	31.47	32.19	-3.29**
N	3654	1689	5343

Table 4.6 presents an interesting result in the Peabody scores for children inside and outside the window. From this it is discernible that older children (inside the window) do in fact perform better of cognitive measures even if the difference in age is only several months. In fact, Fitzpatrick et al. (2011) has demonstrated through analysing differences in test scores before and after school holiday periods that even a matter of weeks can have a significant impact on test scores at this young age. This is a significant result with implications for the estimation, as it suggests that there is likely a measurable relative age gap.

Chapter 5
Methodology and Model

5.1 Conceptual Framework

The decision under consideration can be represented as a simple utility maximisation problem and I leverage an existing model proposed by Michalopoulos, Robins, and Garfinkel (1992). In this model, Michalopoulos et al. (1992) constructs the maximisation problem to analyse the childcare choice for the general case of non-school age children. The decision that my paper is considering is slightly more specific and, as a result, I make minor changes to include the early enrolment decision as well as the impact of an exogenous government subsidy. The model is constructed as a Stone-Geary utility maximisation problem between consumption(x), working hours (h) and some perceived measure of quality or benefit from the type of care (Q). I include minimum terms (x_0, Q_0) to represent a necessary amount of consumption and childcare quality for each family. h_0 is specified different to the other subsistence terms, instead, h_0 specifies the maximum amount of working hours in a day and therefore the term being maximised here is leisure ($h_0 - h$). For simplicity, I do not distinguish between leisure and home care hours, despite acknowledging they are not the same. If, however, it is expected that parents do not derive negative utility from time spent with their children, this does not affect the interpretation. Therefore the carer maximises consumption, non-work (leisure) hours and quality of care. The model assumes that this is a decision made by a single primary carer in each family. I begin by considering a child born outside the window to whom school is not an available option of care. I solve this model for an agent who chooses to work and use formal care as this case is most interesting to my analysis. Solutions can be found for the other cases where agents choose either not to work or use informal care, however, I do not consider these results in this paper. The maximisation problem takes the below form and is restricted by two constraint equations:

$$\max_{(f,x,h,Q)} U = \beta_1 ln(x - x_0) + \beta_2 ln(h_0 - h) + \beta_3 ln(Q - Q_0) \tag{5.1}$$

$$\text{s.t. } px + h(1-f)\alpha Q_c = wh + E \tag{5.2}$$

$$TQ = h\Big((1-f)Q_c + fQ_f\Big) + (T-h)Q_h \tag{5.3}$$

The first budget constraint above restricts the value of the consumption bundle (px) and the amount spent on paid childcare to the sum of their wage (wh) and their non-wage income (E). With hourly cost expressed by α, the amount spent on childcare will be positive if labour market hours are greater than zero ($h > 0$) and if free care is not utilised ($f = 0$). The second constraint I impose on this model is a time and quality constraint and captures the decision between types of care based on some quality measure. In the notation, Q_f, Q_c and Q_h stand for quality of free (f) informal care, costly (c) or formal care and parental or home (h) care and Q denotes average quality of care. The first order conditions yield the below equations of interest.

$$\frac{\beta_1(w - \alpha Q_f)}{p(x - x_0)} = \frac{\beta_2}{h_0 - h} + \frac{\beta_3(Q_h - Q_f)}{T(Q - Q_0)} \tag{5.4}$$

$$\frac{\beta_1 \alpha}{p(x - x_0)} = \frac{\beta_3}{T(Q - Q_0)} \tag{5.5}$$

$$px + \alpha Q_f h = wh + E \tag{5.6}$$

The first equation (5.4) represents the decision the carer faces when choosing to work. The utility they derive from an additional hour of work, factoring in prices, cost of care and wages, will be equal to their disutility from working and utility from their child receiving external care. Utility from external care is a function of a number of terms worth mentioning explicitly. The sign of this term, or rather, the utility or the disutility, is determined by how inferior or superior paid child care is to parental care ($Q_h - Q_c$). Secondly, the marginal utility will be dependent on the average quality of care Q which is a sum of the components $\gamma Q_h + (1-\gamma)Q_c$, where γ is the proportion of time spent in home care. If parents are indifferent between quality of care, this term will simply be the marginal utility of care, the preference term β_3 proportional to the average quality. If their preference for good care is low, or the average quality is already quite high, the marginal utility for additional care will be low. The second equation states that all carers, regardless of income, will distribute their budget such that the marginal utility of consumption will be equal to the marginal utility of purchasing care. The third equation is simply the budget constraint faced by a working parent who chooses to purchase care. Solving for consumption x, work hours h and quality Q yields the below results:

$$x = x_0 + \beta_1 \frac{I^*}{p} \tag{5.7}$$

$$h = h_0 - \beta_2 \frac{I^*}{w^*} \tag{5.8}$$

$$Q = Q_0 + \beta_3 \frac{I^*}{\alpha T} \tag{5.9}$$

Where

$$I^* = E + w^* h_0 - px_0 + \alpha T(Q_h - Q_0) \quad (5.10)$$
$$w^* = w - \alpha Q_h \quad (5.11)$$

In equation 5.10, w^* represents the shadow wage, or effective net wage, after subtracting the implicit cost of providing home care (Michalopoulos et al., 1992). I^* is equal to the level of total income once accounting for the minimum amount of consumption and care as well as the maximum number of market hours. Provided that the parent does not leave the child unattended whilst they are at work, it can be expected that the minimum quality of care cannot be provided exclusively at home by a working parent. Therefore this term $\alpha T(Q_h - Q_0)$ will reflect the cost of childcare not provided at home. From this, it can be seen that disposable income increases if there is an exogenous increase in E, shadow wage increases, either through direct wages or increased working hours, or if the cost of childcare declines. The introduction of the rebate is accounted for in the model by redefining the cost of childcare. As the rebate effectively reduces the cost of care, let $\alpha_R = (1 - R)\alpha$ where R represents the proportion of costs that the rebate reimburses, for example, 30% or 50%. Thus there is an exogenous increase in I^*, resulting in an increase in consumption (x) and quality of care (Q) whilst decreasing the need for market hours (h) subject to the conditions stated in equations 5.4-5.6.

Now consider how this model changes for a child born within the window. In this specification, the carer has the additional option of school enrolment which, for simplicity, is financially costless. I assume that the school day is fixed in terms of hours so parents cannot freely substitute school hours for care hours, rather school hours directly deduct the amount of childcare required. Any additional care, such as after or before school care can still be purchased. School care is incorporated into the model through the time/quality constraint (equation 5.14) by the term Q_s. Average quality Q is now determined by the proportion of time the spend in home care, school and childcare $\gamma Q_h + \tau Q_s + (1 - \gamma - \tau) Q_c$. The new maximisation problem incorporating school care is:

$$\max_{(f,x,h,Q)} U = \beta_1 ln(x - x_0) + \beta_2 ln(h_0 - h) + \beta_3 ln(Q - Q_0) \quad (5.12)$$
$$\text{s.t. } px + h(1 - f)\alpha_R Q_c = wh + E \quad (5.13)$$
$$TQ = h\Big((1 - f)Q_c + sQ_s + fQ_F\Big) + (T - h)Q_h \quad (5.14)$$

The new first order conditions with the inclusion of school are only marginally dissimilar from the conditions of the ineligible child but are included in their entirety below.

$$\frac{\beta_1(w-\alpha_R Q_c)}{p(x-x_0)} = \frac{\beta_2}{h_0-h} + \frac{\beta_3(Q_h-Q_c-Q_s)}{T(Q-Q_0)} \tag{5.15}$$

$$\frac{\beta_1\alpha_R}{p(x-x_0)} = \frac{\beta_3}{T(Q-Q_0)} \tag{5.16}$$

$$px+\alpha_R Q_c h = wh+E \tag{5.17}$$

If $Q_s < Q_c$, because childcare and school are substitutes, in 5.16 there will be either a smaller increase or a larger decrease in utility from non-parental care. Because the school day is fixed, if $Q_s < Q_c$, average quality of care Q will decrease, leading to a greater marginal utility of care in Equation 5.16. However, if there is no perceived difference in quality, or parents do not particularly value one form of childcare over another, the partial subsidisation is unlikely to change patterns of childcare usage and the cost savings from reduced childcare will be passed through to consumption. Using the results derived in Equations 5.7 and 5.9 and substituting a number of terms I derive the fundamental implication of this model in Equation 5.18. I argue that the carer will delay school in favour of additional childcare if the weighted benefit of paid childcare is greater than the consumption value of childcare savings inclusive of the rebate.

$$\frac{\beta_1\Big[\alpha_R(Q_0-Q_h-Q_s)\Big]}{p} < \beta_3(Q_0-Q_h) \tag{5.18}$$

The prediction of this case is more ambiguous than when school is not an option. Depending on the child's ability, which informs differences in quality of care, as well as the parent's preferences towards quality care over consumption, their utility can be maximised both by enrolling in school or in childcare.

The intuition derived from this model informs the regression analysis, in particular the variables utilised to control for betas (β_1, β_2 β_3). It is expected that these weights are different functions of individual, parental, socio-economic and other characteristics, of which I can approximate a considerable amount in the LSAC data set. β_3, in particular, I anticipate will be a function of the child's individual characteristics such as maturity levels and general cognition that may influence their care needs. From this equation, I also derive one of the fundamental hypotheses for the study. If it is expected that the childcare rebate increases the average quality of care for certain children, then this should be demonstrated in measurable outcomes. An additional year of higher quality care may prepare a child better for the commencement of school, and therefore, will manifest in improved outcomes relative to comparable children to whom the rebate was not available.

5.2 Regression Model

The foundation of the natural experiment conducted in this thesis is the introduction of the CCTR in 2005. As inferred from the utility model, the CCTR features in all carer's utility functions whether they choose to utilise it or not, as it represents a potential reduction in childcare costs. Ideally, the analysis would be supported by the existence of a control group to which I could compare the results. As this is a nationwide policy, I cannot use different states or regions as controls, nor can I use eligibility status for the rebate. As discussed in Chapter 4, certain families are ineligible for the rebate due to employment or choice of care. However, it is not possible to generate a treatment and control group based on eligibility as this decision is endogenous. By splitting the cohort by eligibility, this will incorrectly attribute ineligible status to parents who have given up work due to costs of care. In order to create the ineligible control, it needs to be assumed that the decision to be a stay-at-home parent and use informal or formal care is exogenous to a model based on the costs of childcare. For obvious reasons this assumption is not particularly persuasive (Connelly 1992; Ribar 1995).

The experiment is generated by the introduction of the childcare rebate which is available for children in wave 3 (treatment), but not wave 1 (control). The CCTR dummy variable determined by the wave (year) that separates the data into those families that are eligible for a tax rebate and those that are not. Of course, there are potential criticisms with this methodology as it is a considerable assumption to make that the only significant difference between 2004 and 2008 is the introduction of the tax rebate. The balance of covariates in Chapter 4 demonstrated that these cohorts are not perfectly comparable, however, the inclusion of suitable controls seeks to redress this issue.

I use a standard linear probability model (LPM) to test the likelihood of early enrolment for children born within the window. The reason for choosing an LPM model, rather than a non-linear binary variable regression model such as logit or probit, is that a considerable amount of our analysis will be based on the interpretation of coefficients on interactions. Due to the non-linearity of logit and probit models, interpreting the coefficients of interaction terms can be problematic and often additional computation is required (Ai and Norton, 2003). There is also an issue with perfect prediction between early enrolment and being born in the window which presents further difficulty in the use of non-linear models. The trade-off of using an LPM compared to a logit or probit is that we will likely observe predicted probabilities below zero or greater than one. As part of my robustness checks (Chapter 7), the issue raised by Ai and Norton (2003) will be further discussed and I will demonstrate the results are robust to linear and non-linear model specification.

The base model takes the below form where X_i represents a vector of character variables. The CCTR variable either takes the form 0 or 1 depending on the wave. I control for state fixed effects, denoted α_s, as education systems are presided over by the state and there may be further unobservables that are related to state-specific education systems beyond school cutoff dates. State fixed effects are consistently significant in the regressions below. All models in this paper will use robust standard errors due to the presence of heteroskedasticity in several of the specified models identified through Breusch-Pagan tests. Due to the size of the sample size, it is unlikely that there are issues in using robust standard errors so I continue to use them in practice throughout the paper. I also argue that there is little motivation to cluster the standard errors, even if there were appropriate levels at which to do so. Due to the expansive nature of LSAC data, postcodes and school identification numbers cover too few observations. Similarly, I already account for unobserved state effects through fixed effects and states provide too aggregate a level and with too few clustering units (Abadie, Athey, Imbens, and Woolridge, 2017). As a result, I argue the most appropriate treatment of standard errors is simply to use robust standard errors. To model the determinants of early start, I estimate the following specification:

$$\begin{aligned} Pr(earlsrt = 1|CCTR, X) = \beta_0 + \beta_1 cctr_i + \beta_2 wndw_i + \\ \beta_3(cctr_i \times wndw_i) + X_i\beta + \alpha_s + \varepsilon_i \end{aligned} \tag{5.19}$$

CCTR is the binary treatment variable, *wndw* indicates eligibility for early enrolment, X a vector of covariates and a_s denotes state fixed effects. The full list of covariates can be found in Chapter 4. Incorporating the interaction effect controls for factors that are specific to children in the window, as well as specific to the year (CCTR). The interaction effect will then only capture only the impact of being eligible for the rebate, given the child is born within the window. For the secondary analysis I employ a difference in differences method to test the impact of the rebate on testable outcomes Y_i including kindergarten repetition, NAPLAN tests, personality scores and anti-social behaviour (bullying).

$$Y_i = \alpha_0 + \gamma_1 cctr_i + \gamma_2 wndw_i + \gamma_3(cctr_i \times wndw_i) + \Delta x_i\beta + \alpha_s + \varepsilon_i \tag{5.20}$$

The other terms in Equation 5.20 remain the same as in Equation 5.19, including binary variables for treatment, eligibility and state fixed effects. Ideally, school fixed effects would be included as it is likely that there considerable unobservables at the school level that affect outcomes, particularly in NAPLAN scores. However, the average number of children in each school in the survey is less than 2, meaning school fixed effects will add no value and, in fact, will nullify important individual differences.

Between both the base model regression and the outcome measurement specifications, the predictions of the empirical model identified at the start of this chapter should be observable. If parents respond to the subsidisation of costs by holding children back an extra year, I should observe a decrease in early enrolment and a general improvement in measurable outcomes. The purpose of the additional difference in differences is to measure the effect of early enrolment, but also to lend credibility to the primary analysis that is not without its shortcomings. If the introduction of the CCTR variable is the primary change that is being captured by the dummy variable over this four year period, then I expect the model to behave as per the empirical analysis. Children who still enrol early in school, despite the rebate, are expected to be more capable children and children who delay should benefit as a result of having more time to mature prior to schooling.

Chapter 6
Results

6.1 Base Model: Determinants of early enrolment

The previous chapter presented a utility model that theorised the impact of a reduction in childcare costs on the likelihood of early enrolment. In the model, a reduction in the relative cost of childcare allowed greater consideration of welfare in the decision. In this chapter, I test this mechanism using the difference in differences framework presented in Equation 5.19. Following on, I estimate the heterogeneous benefit from delaying school entry. I measure the benefits using cognitive measures of year 3 and year 5 NAPLAN scores as well as the likelihood of kindergarten repetition. I also test for an effect on non-cognitive outcomes measured by children's SDQ scores as well the propensity towards antisocial behaviour.

Beginning with the base model, the empirical model is tested by analysing if a reduction in costs does lead to a reduction in early enrolment. Table 6.1 depicts the results with the introduction of the CCTR. The effect of the policy, measured by the interaction term (Window×CCTR), demonstrates a 5% reduction in the probability of early enrolment post the implementation of the policy. This is a large and statistically significant result and suggests that the subsidisation of childcare has had the anticipated impact on enrolment. Interestingly, however, both income variables are not significant at any conventional level. If the cost of childcare is a significant determinant of early enrolment, it would be expected that income and wealth will influence the propensity to enrol early. This is a surprising result given the mechanism being estimated and also the case if the quadratic term is excluded. Wealth, represented by home ownership, is strongly significant in the decision which could cannibalise some of the effect of income.

The base model also reveals interesting results on the coefficients of gender and families that speak English as a second language (ESL). From this model, males appear less likely to start school than females, all else equal, by almost 4%. This result will be discussed in greater detail when considering heterogeneous treatment effects. Notably, the coefficient on ESL is large and statistically significant in this model suggesting that children who speak another language at home are 10% more likely to start school early. It is possible that children with ESL, most likely from migrant backgrounds, could be starting school early for two main reasons: economic reasons or assimilation reasons (Deming, 2009). We observe a negative correlation (-0.1041) between log income and ESL status suggesting that economic factors may be more significant for migrant families. It is also plausible, however, that migrant families have incentive to assimilate their children and develop English skills as early as possible and hence early enrolment is less driven by economic factors, rather intention to develop language skills not taught at home. This may result in greater likelihood of early enrolment if migrant families believe an early start in schooling will accelerate assimilation relative to other forms of care. See appendix Table A.4 for the full regression with all relevant covariates.

Table 6.1: The effect of childcare rebate on the likelihood of early school start

Variables	(1) Early Start
Window	0.472***
	(0.0179)
CCTR	-0.000523
	(0.00355)
Window×CCTR	-0.0505**
	(0.0235)
Age in Months (July)	0.0120***
	(0.00119)
Male	-0.0351***
	(0.00790)
Medical Condition	-0.0302**
	(0.0122)
ESL	0.103***
	(0.0139)
Log Income	-0.161
	(0.160)
Log Income^2	0.00734
	(0.00721)
Own Home	-0.0299***
	(0.00985)
Constant	0.143
	(0.897)
State Fixed Effects	Yes
N	4955
R^2	0.465

Robust standard errors in parentheses

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

Controlling for individual, parental and family characteristics.

6.2 Heterogeneous Treatment Effects (HTE)

The results we derive from the base model in Table 6.1 show characteristic differences between children and families. This suggests that the effect of the policy is heterogeneous in the population. The first HTE we test for is between males and females as this widely examined in the existing literature (Deming 2009; Edwards et al. 2011; Gathmann and Sass 2018; Suziedelyte and Zhu 2015). The results of the base model suggest that similar findings might be observed here as Table 6.1 states boys are 4% more likely to be delayed than girls. This is consistent with the perception that girls are more mature at younger ages (Edwards et al., 2011). Secondly, I test for different responses to subsidisation by income groups. Table 6.1 revealed the perplexing result that income is not significant in determining school starting age amongst children born in the window. However, it is reasonable to expect that childcare costs are not of the same consideration across income levels and therefore may determine enrolment status. The second test will identify if differing income levels respond heterogeneously to the rebate.

6.2.1 Gender

The finding that the determinants of enrolment age are significantly different for boys and girls is quite an interesting and robust result. From the data, 57% of girls enrol early compared to only 45% of boys. Before interpreting the results, I run a balance of covariates to ensure that the differences observed between boys and girls are not driven by statistically significant differences in the cohorts. From Table 6.2 I observe differences between boys and girls that are consistent with a maturity gap at this age. The balance of covariates show that, on average, boys perform worse on the Peabody test than girls and display greater behavioural disorders. The Peabody test score is the variable used to measure cognitive ability prior to school. This test indicates the child's comprehension of language and is the best cognitive measure available in the absence of academic test scores. Boys, on average, show greater levels of hyperactivity and appear less confident with social situations than girls at this age.

In Table 6.3 these differences between boys and girls correspond with a significantly higher propensity to delay schooling for boys. After splitting the cohort by gender, the impact of the policy interaction term for boys is now an 8% decline in the probability of enrolling at the 5% significance level. For girls, the impact of the childcare policy is not statistically significant. This is consistent with the perception of a maturity gap between boys and girls at this age. Consistent with the empirical model, I find that income has become significant for boys and not girls once the cohort is divided by gender. This reassures the model in light of finding no statistical significance in Table 6.1 for the income terms. See appendix Table A.4 for the full regression with all covariates.

Table 6.2: Balance of covariates measuring statistical differences between boys and girls

	(1) Females mean	(2) Males mean	(3) t
Age in Months (July)	58.93	58.89	0.44
Rural	0.12	0.12	0.27
Relig	0.78	0.78	0.72
Mother's Education (Years)	13.77	13.83	-0.87
Father's Education (Years)	13.39	13.34	0.72
Medical Condition	0.11	0.16	-5.79***
Peabody Score	65.17	64.36	5.00***
Single Parent	0.12	0.11	0.35
ESL	0.11	0.10	1.03
# Younger Siblings	0.54	0.59	-2.31*
# Older Siblings	0.89	0.83	2.21*
Indigenous	0.03	0.03	-0.06
Owns Home	0.72	0.72	0.12
Log Income	11.07	11.08	-0.48
Hyper Score	2.97	3.66	-11.67***
Emotional Score	1.60	1.49	2.46*
Sociability Score	8.00	7.49	10.30***
Height	133.57	133.31	0.84
Weight	31.79	31.60	0.96
N	2699	2789	5488

Table 6.3: Heterogeneous treatment effects - effect of the policy on likelihood of early start by Gender

Early Start	Females	Males
Window	0.501***	0.446***
	(0.0259)	(0.0247)
CCTR	0.000568	-0.00131
	(0.00504)	(0.00512)
Window×CCTR	-0.0239	-0.0806**
	(0.0332)	(0.0329)
Age in Months (July)	0.0145***	0.00927***
	(0.00171)	(0.00166)
Medical Condition	-0.0341*	-0.0263
	(0.0185)	(0.0162)
Peabody Score	0.00247**	0.000292
	(0.00110)	(0.000950)
Single Parent	0.0561**	-0.00653
	(0.0232)	(0.0240)
ESL	0.135***	0.0736***
	(0.0197)	(0.0190)
Indigenous	0.0869**	-0.0204
	(0.0376)	(0.0334)
Owns Home	-0.0171	-0.0460***
	(0.0141)	(0.0135)
Log Income	0.00431	-0.373*
	(0.230)	(0.221)
Log Income^2	0.0000819	0.0167*
	(0.0104)	(0.00994)
Constant	-1.019	1.517
	(1.288)	(1.240)
State Fixed Effects	Yes	Yes
N	2,433	2,522
R^2	0.512	0.426

Robust standard errors in parentheses
Controlling for individual, parental and family characteristics
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6.2.2 Income Tertiles

To further asses the heterogeneous effects of the policy on early enrolments, I perform a similar analysis focusing on income levels. In the empirical model, it was posited that cost was the mechanism that determined whether children in the window would commence school early or late. However, it is expected that cost is relative and certain families will have differing budget constraints. Dividing the sample into income tertiles allows for the testing of the decision by varying levels of income. Tertiles were chosen to simplify interpretation as by dividing the sample in thirds yields high, middle and low incomes. Table 6.4 presents how the sample was divided:

Table 6.4: Sample divided into tertiles by yearly household income

Income Level	Observations	Mean
Low	1584	33,472
Middle	1710	68,096
High	1661	135,845

Note: All incomes adjusted for 2004 price level

As shown in Table 6.5, low income families in the first tertile are considerably impacted by the policy compared to higher income families. Demonstrating an 11% reduction in the likelihood of early enrolment at the 1% significance level is consistent with the predictions of the empirical model. Middle and high income families are less responsive to a partial subsidisation of the costs of childcare. Again, the coefficient on males is statistically significant across all income levels. The full model with all covariates can be found in the appendix.

Table 6.5: Determinants of Early Enrolment by Income Tertile

	(1)	(2)	(3)
Early Start	Low	Middle	High
Window	0.537***	0.423***	0.439***
	(0.0291)	(0.0318)	(0.0337)
CCTR	0.00826	-0.00639	-0.0186***
	(0.00549)	(0.00501)	(0.00658)
Window ×CCTR	-0.112***	0.00853	-0.0188
	(0.0421)	(0.0418)	(0.0414)
Age in Months (July)	0.0136***	0.00968***	0.0132***
	(0.00216)	(0.00199) ,	(0.00212)
Male	-0.0330**	-0.0418***	-0.0322**
	(0.0143)	(0.0136)	(0.0135)
Constant	-0.511***	-0.579***	-0.936***
	(0.158)	(0.154)	(0.158)
State FE	Yes	Yes	Yes
N	1584	1710	1661
R^2	0.496	0.430	0.459

Robust standard errors in parentheses

Controlling for individual, parental and family characteristics

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

The analysis of heterogeneous treatment effects affirmed findings of similar papers by displaying distinct differences between males and females. This is posited to be the result of perceivable maturity gaps between males and females at this age as measured in other papers through analysis of test scores (Suziedelyte and Zhu, 2015). The balance of covariates in Table 6.2 supported this idea by demonstrating differences between males and females on items such as hyperactivity, social awareness and the Peabody score. The second test affirmed predictions that the cost of childcare is likely to relative to income level, as not all family face the same budget constraints. It has been demonstrated that by analysing cost determinants of early enrolment, any effect that is discerned will be the average effect of the policy on the cohort as a result of encouraging predominantly boys of low-income families to enrol in school a year later. This is important for the interpretation of outcomes in the below section. Whilst this method analyses the average treatment effect, changes in outcomes are being driven by particular children.

6.3 Assessing the effect of delayed Enrolment on Children's Cognitive and Non-Cognitive outcomes

Through analysing the base model, I demonstrated that the rebate has led to a reduction in the rates of early enrolment for certain children. The purpose of this section is to analyse the impact that this delayed enrolment has had on the averages of several measurable outcomes. Not only do these tests provide an indication as to the consequences of early enrolment, they also assist in supporting the conclusions drawn from the base model. In Data and Methodology, the LSAC cohort structure and length of time between waves were identified as potential criticisms of this analysis. The theory discussed in Chapter 6 predicts that at-risk children will be more likely to be delayed another year after the cost of childcare is reduced. Therefore, if the base model regression is primarily capturing the effect of the rebate, then it should be possible to discern an impact on children's outcomes as a result of an extra year in which to mature. For these specifications I use the difference in differences methodology discussed in Chapter 5. Much of the literature has focused on academic achievement outcomes of an early school start, however, with the richness of our LSAC data source I am able to test multiple additional outcomes. As well as testing children's NAPLAN scores, I test for changes in the likelihood of repeating kindergarten, personality measures as well as the likelihood of engaging in or being subject to antisocial behaviour (bullying).

6.3.1 NAPLAN Results

The first difference in differences regression uses NAPLAN scores to test if children within the window have improved on average as a result of the delayed school entry. The motivation for testing NAPLAN scores derives from the prediction that parents are aware of, and make enrolment decisions based upon their child's ability as well as costs of other types of care. Given an exogenous decrease in the cost of childcare under the CCTR, parent's who are concerned that their children are not yet ready for school will prefer to delay their school entry for another year. Therefore there will be a greater tendency for less capable children to be delayed another year with a measurable difference in the NAPLAN scores between the treated and the control cohorts.

Although the policy term, measured by the interaction, is thought to capture the benefit of an additional year of non-primary education, there is likely to be two mechanisms observable in NAPLAN scores. Firstly, there will be a composition effect as a result of less able children switching from early starters to late starters. If we consider all children on a spectrum of ability, whereby parents of less able children prefer to delay entry, the change in composition could lead to an increase in both the average of early starters and late starters. Secondly, I expect children who start school later due to the policy to receive a positive effect on test scores as a result of an additional year of development prior to commencing school. For brevity, this component will be referred to in the following discussion as the 'childcare effect'. In this section, both mechanisms will be tested and evaluated.

As discussed in Chapter 4 (Data), NAPLAN is a series of tests that measure a child's academic progression across 5 main components: numeracy, spelling, reading, writing and grammar. The tests are conducted in years 3,5,7 and 9 of a child's schooling and are the only nationwide national assessment in Australia. Importantly, the test scores are designed and normalised to allow for comparison across years and cohorts. This allows for the comparison of year 3 results of 2007 with the results of 2010, for example. Furthermore, NAPLAN scores incrementally increase with age for the purpose of tracking a child's development. For each subject, the incremental increase over a two year period is 50 points on average (250 points over all 5 subjects) (Jha, 2014). This will assist in assessing the magnitude of the coefficients. The estimation assumption is that, after controlling for individual, school and family characteristics, the only difference between the treated cohort and the control cohort is that the less able children will be encouraged to delay school entry due to the childcare rebate. Furthermore, since the introduction of the rebate, the additional year of non-primary education is more likely to be in a formal daycare centre rather than other forms of informal childcare. This increase in quality of care could better prepare students for their formal education (Heckman et al., 2015).

Confirming the hypothesis, this regression finds that the CCTR has a positive and significant effect on test scores. Table 6.6 depicts the results of the regression on both Year 3 and Year 5 NAPLAN test scores. The effect of the policy, measured by the coefficient of the interaction term, depicts a 52 point and a 50 point increase in both years 3 and year 5 scores. If 250 points is, on average, the anticipated progress between years 3 and year 5 scores, this suggests that delayed enrolment improves average NAPLAN scores by the equivalent of approximately 5-6 months of learning. This interpretation of magnitude is used by Jha (2014) who finds similar coefficients on a different experiment involving a change in the cutoff dates. Also observable from Table 6.6 is that, unlike the other models we have seen thus far, gender has no statistically significant impact on NAPLAN scores. The LSAC data set also allows controlling for age at test, an issue that has been raised by other studies of early enrolment (Elder and Lubotsky 2009; Fitzpatrick et al. 2011; Suziedelyte and Zhu 2015). The reason controlling for age at test is important is to separate the impact of raw age from relative age. As the results in Table 6.6 suggest, raw age is strongly significant in test results.

Table 6.6: Effect of the CCTR on on Year 3 and Year 5 aggregate NAPLAN scores

Variables	Year 3 NAPLAN	Yr 5 NAPLAN
CCTR	-15.63	-5.094
	(14.86)	(13.33)
Window	-10.24	-26.20
	(20.34)	(17.34)
CCTR × Window	52.44**	50.11**
	(26.05)	(23.54)
Male	5.091	12.24
	(11.74)	(10.85)
Age at test	7.776***	5.055***
	(1.402)	(1.097)
Constant	1576.4	2789.8**
	(1290.5)	(1164.7)
State FE	Yes	Yes
N	3359	3280
R^2	0.017	0.013

Robust standard errors in parentheses
Controlling for individual, parental, family and school characteristics
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

To gain a better understanding of how the benefits of additional non-primary education impact the various components of a child's formal education, the aggregate results are broken into their 5 components. Table 6.7 presents the disaggregated NAPLAN regressions allowing for the identification of the subject areas most impacted an additional year of education. The policy coefficient is positive and statistically significant on writing, grammar and numeracy scores indicating that these components stand to benefit most from delaying enrolment. Again, using the same interpretation of the effect on scores as used by Jha (2014), each subject benefits by the equivalent of approximately 2-3 months of learning. Reading and spelling appear unaffected by the policy. Table 6.8 shows similar magnitudes and significance levels on year 5 NAPLAN scores. Writing and numeracy are still the largest contributors to the overall effect characterised by the strongest magnitude and statistical significance level on the policy variable. The benefit of early start on writing diminished marginally in magnitude from 13.52 in year three to 11.44 in year 5. This could be indicative of the gradual calibration of relative age gaps over time. On the other hand, numeracy has increased in magnitude very slightly from 13.5 to 14. Interestingly, the impact of delayed enrolment on grammar scores is no longer significant, instead spelling has become significant at the 10% level in the year 5 test scores. It is difficult to interpret this result, however, it is likely only a representation of the changing composition of subject material between year 3 and year 5. If this is the case, then this is consistent with an ongoing advantage of delayed school entry. The full list of covariates can be found in Appendix Table A.5.

These results could be indicative of the "entry-age achievement gap" discussed by Elder and Lubotsky (2009), whereby older and more mature children are sorted into higher graded classes for subjects such as math. However, if this is the case, then it is surprising that there is no significant impact on reading scores, as in primary school, reading is the most frequently graded subject area (Suziedelyte and Zhu, 2015). Suziedelyte and Zhu (2015) encounter a similar result and argue that reading is most subject to "dynamic complementarity", the dependence of the return to investment on the stock of the skill. Suziedelyte and Zhu (2015) argue that disadvantaged children, particularly from migrant backgrounds, will not derive benefit from reading classes in school due to poorer basic vocabulary that will inhibit their ability to progress at the same pace. Suziedelyte and Zhu (2015) use this as an argument that early enrolment improves outcomes for disadvantaged children by building this stock of skills earlier. Numeracy, on the other hand, is more likely reflective of basic cognition and will benefit from an additional year of cognitive development outside of school (Suziedelyte and Zhu, 2015). The entry age achievement gap does help explain the mechanism to which these results are perpetuated throughout their schooling represented by the exact same disciplines showing similar significance and coefficients in the year 5 scores depicted in Table 6.8. These results strongly suggest that the advantage of an additional year of education continues into at least the later stages of primary school. This is significant as if this effect persists into high school, as it likely does, classes are increasingly structured on ability.

Table 6.7: Effect of the CCTR on Year 3 NAPLAN scores by discipline

Variables	Reading	Writing	Spelling	Grammar	Numeracy
CCTR	6.321	-9.372***	-0.990	-1.170	-11.84***
	(3.928)	(2.898)	(3.424)	(3.985)	(3.306)
Window	3.331	-7.291*	0.358	-1.398	-5.425
	(5.104)	(4.084)	(4.656)	(5.401)	(4.497)
CCTR×Window	4.925	13.52***	9.180	11.94*	13.24**
	(6.707)	(5.054)	(5.954)	(7.096)	(5.668)
Male	2.322	0.965	0.636	1.733	0.775
	(3.091)	(2.243)	(2.688)	(3.202)	(2.547)
Age at test	2.268***	1.037***	1.015***	1.471***	1.938***
	(0.368)	(0.279)	(0.318)	(0.378)	(0.301)
Constant	208.5	374.0	183.8	546.3	247.7
	(318.1)	(230.9)	(287.8)	(361.1)	(280.1)
State FE	Yes	Yes	Yes	Yes	Yes
N	3408	3409	3411	3407	3399
R^2	0.018	0.015	0.009	0.012	0.023

Robust standard errors in parentheses

Controlling for individual, parental, family and school characteristics

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

Table 6.8: Effect of the CCTR on Year 5 NAPLAN scores by discipline

Variables	Reading	Writing	Spelling	Grammar	Numeracy
CCTR	11.44***	-15.50***	6.185**	-0.955	-5.883*
	(3.416)	(2.824)	(3.063)	(3.597)	(3.040)
Window	-0.490	-9.114**	-3.057	-4.246	-6.420*
	(4.443)	(3.810)	(3.789)	(4.653)	(3.802)
CCTR×Window	4.419	11.44**	8.863*	8.000	14.03***
	(5.972)	(4.938)	(5.384)	(6.421)	(5.257)
Male	2.219	0.00580	2.074	4.573	2.121
	(2.777)	(2.251)	(2.489)	(2.946)	(2.461)
Age at Test	1.245***	0.812***	0.569**	0.997***	1.327***
	(0.281)	(0.238)	(0.252)	(0.293)	(0.248)
Constant	558.3*	641.5**	454.7*	675.6**	351.0
	(295.8)	(251.3)	(259.6)	(310.1)	(269.4)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	3329	3314	3322	3322	3308
R^2	0.018	0.017	0.012	0.008	0.017

Robust standard errors in parentheses

Controlling for individual, parental, family and school characteristics

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

Unlike other studies such as Elder and Lubotsky (2009), these results indicate a prolonged impact on academic achievement at least in the medium term. Due to the infancy of the data, I am unable to test year 7 or 9 test scores, however, the impact on aggregate year 5 test scores is only marginally smaller than the effect on year 3 scores. Unless the impact has a rapid decline in the final year of primary school, it is reasonable to suggest that this impact likely continues into high school. The difficulty I encounter when interpreting this result is the considerably small R^2 of less than 2% on both models and the limited significance of other variables. This suggests that the model is a poor fit for the data. When comparing goodness of fit in other studies, this appears to be common (Black et al. 2011; Fitzpatrick et al. 2011). From these studies, as well as the results of this thesis, test scores appear to be difficult for models to fit generally. Black et al. (2011) have similarly low R^2's in their analysis of Norwegian test scores (less than 1%) as do Fitzpatrick et al. (2011) when considering US test scores (less than 4%). It could be that an amount of this variation is driven by innate differences between children that are difficult to measure such as aptitude or, alternatively, by school fixed effects that I am unable to control for. Due to the data set containing very few children from the same school, including school fixed effects will nullify any statistically significant findings from the model.

So far it has been assumed that the improvement in NAPLAN results has been a consequence of an additional year of non-primary education for certain children within the window. Whilst this is certainly the claim, there are likely two effects being captured by these regressions for children in the window. Considering the hypothetical distribution of children's ability, the assumption is that the most able children will commence school early and the less capable children will be delayed. This motivates a test to determine how the changing composition effect and the 'childcare effect' both interact to influence averages of children in the window. Consider first columns 1 and 2 of Table 6.9 and 6.10. These columns show the NAPLAN regressions for children inside and outside the window of early enrolment. From these specifications it is easy to see that the introduction of the CCTR only has a significant impact on the outcomes of children born in the window to whom early enrolment is an option. This captures the overall effect that we are observing; providing certain children an additional year of non-primary education has a positive impact on their test scores. Columns 3 and 4 of Table 6.9 and 6.10 show the effect of the changing composition of those who start early and late for children born within the window. This effect is more difficult to tease out. Column 3 of both tables show the NAPLAN score regressions for children inside the window who start early and column 4 shows the same regressions for children in the window who start late.

In general, I expect the average of both these cohorts to increase simply due to composition effects. For the early starters, their average will improve by the students at the lower end of their specific distribution changing cohort from early to late start leaving only the most capable. In both year 3 and year 5 scores, this effect is positive but not statistically significant. The average of the late starters is presumably brought up by children in the lower tail of early starter's distribution now changing to be the high tail of the late start cohort. This is positive and statistically significant in both years. Thus the overall increase in scores observed can be attributed to the decreased probability of less able children starting school early as well as the positive effect delayed entry may have on their test scores. However, as mentioned, the results of column 1 and 2 are less ambiguous; the average of the window is increasing as a result of the policy and this cannot be driven by composition effect as children cannot move in and out of the window. This is the same effect that is represented by the policy variable in the regression Table 6.6.

Table 6.9: Composition Effect of CCTR on average scores (Year 3)

			Window=1	
	(1)	(2)	(3)	(4)
Year 3 NAPLAN	Window=0	Window=1	Early Start	Late Start
CCTR	-18.47	43.20*	33.01	56.29*
	(15.11)	(22.59)	(32.05)	(32.16)
Constant	1153.3	2101.5	719.1	4300.8
	(1603.7)	(2176.1)	(2659.0)	(3896.4)
N	2293	1066	557	509
R^2	0.018	0.047	0.070	0.046

Robust standard errors in parentheses

Controlling for individual, parental, family and school characteristics

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

Note: Columns 1 represents the estimated impact of the CCTR on children born outside the window. Column 2 estimates the impact of the CCTR on children born inside the window. Columns 3 and 4 estimate the effect of the CCTR on children within the window who start early and those that start late.

Table 6.10: Composition effect of CCTR on average scores (Year 5)

			Window=1	
	(1)	(2)	(3)	(4)
Year 5 NAPLAN	Window=0	Window=1	Early Start	Late Start
CCTR	-9.536	49.17**	14.99	74.41**
	(13.51)	(20.90)	(28.46)	(33.90)
Constant	2281.2	4032.8**	275.2	9641.8***
	(1407.1)	(2041.7)	(2496.1)	(3341.7)
State FE	Yes	Yes	Yes	Yes
N	2222	1058	542	516
R^2	0.015	0.033	0.053	0.068

Robust standard errors in parentheses

Controlling for individual, parental, family and school characteristics

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

Note: Columns 1 represents the estimated impact of the CCTR on children born outside the window. Column 2 estimates the impact of the CCTR on children born inside the window. Columns 3 and 4 estimate the effect of the CCTR on children within the window who start early and those that start late.

6.3.2 Kindergarten Repetition

Much of the existing Australian literature relies heavily on analysing academic achievement in test scores to determine the consequences of early enrolment (Jha, 2014). The contribution of this study is to expand the measures with which the effect of relative age is assessed in both cognitive and non-cognitive measures. Repeating a grade in the early years of school is not uncommon and is a decision made between parents and teachers when a child is underperforming relative to their peers. If less able children are encouraged to delay school entry as a result of the childcare policy, I expect that this will impact the rates of repetition. Similar analysis was conducted by Deming (2009) who found that the Head Start pre-school program in the United States reduced grade repetition for disadvantaged children by approximately 7%.

Grade repetition provides an alternative method of measuring academic benefit obtained from early enrolment. Presumably, any change in the rates of repetition would signify that children are, on average, better prepared for school after the policy and less academically hindered by relative age differences. Not only that, but repetition in early years of school has been shown to have considerably harmful effects on a child's development. The psychological impact is well documented with retention significantly damaging the self-esteem of the child and modifying parents and teachers long-term expectations of the child's potential (Huang, 2014). Evidence in child psychology demonstrates that teachers and parents make judgements on the appropriateness of repetition, intentionally or unintentionally, based on the child's maturity relative to other children in the class (Huang, 2014). This is particularly concerning if these children are relatively young for their year. Elder and Lubotsky (2009) raised similar concerns with regards to diagnosis of hyperactivity. Their reasoning was that teachers, in particular, misdiagnose immaturity as attention deficit disorder or hyperactivity. As a result of the policy, the theory outlined in the empirical model predicts that, on average, only more able children commence school early. This will manifest in fewer instances of repetition in the post-treatment cohort. Table 6.11 show the results of our difference in differences estimate which reveals approximately a 2% reduction in instances of repetition within the treated cohort. The full regression can be viewed in the Appendix Table A.8. This is a significant finding as a reduction in the rate of repetition is indicative of greater readiness for school. Not only are children older when they enter school but are also more likely to have attended formal childcare for that additional year rather than other forms of care. Heckman et al. (2015) have argued the importance of formal care in preparation for the significant transition into formal education.

Table 6.11: Effect of the CCTR on likelihood of repeating Kindergarten

Variables	Repeat Kindergarten
CCTR	0.000
	(0.01)
Window	0.014
	(0.012)
CCTR×Window	-0.019**
	(0.009)
Constant	-0.001
	(0.004)
State Fixed Effects	Yes
R^2	0.036
N	4,837

Robust standard errors in parentheses

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

Controlling for individual, parental and family characteristics

Note: Impact of the policy represented by the coefficient on the interaction between CCTR and Window.

6.3.3 Behavioural and Social Outcomes

Within the existing literature, there has been great enthusiasm for testing the impact of early start on non-cognitive outcomes. Most, however, concentrate on cognitive measures, such as test scores, due to data limitations (Bedard and Dhuey 2006; Fitzpatrick et al. 2011; Jha 2014). A limited number of studies have successfully measured some type of non-cognitive outcome. Elder and Lubotsky (2009) test for diagnosis of Attention Deficit Disorder (ADD) and find that early enrolment increases the likelihood of diagnosis by 25%. Suziedelyte and Zhu (2015) test the same SDQ scores as used in this research and find no statistically significant difference in behavioural outcomes as a result of early enrolment.

The mechanism used as part of this research presents a slightly different approach to Suziedelyte and Zhu (2015). Not only does this study predict that an additional year of non-primary education may improve behavioural outcomes, but also that these outcomes are more likely to be developed in formal childcare rather than other forms of care. In the psychological literature, it is posited that formal childcare and exposure to other children and carers have the ability to improve a number of non-cognitive outcomes (Parker et al., 2018). Relative to other forms of informal care, childcare centres provide an environment more conducive to positive social skills such as sharing and empathy (Love et al., 2005). As a result, it is reasonable to expect that not only delaying school entry but delaying entry in favour of formal childcare may yield demonstrably improved outcomes for children.

Controlling for age, I test the improvement in children's sociability, hyperactivity and emotional responsiveness as perceived by both parents and teachers at the ages of 8-9. Following the literature, I test these outcomes after the children have commenced school in order to measure the impact of an additional year of non-primary education (Elder and Lubotsky, 2009; Suziedelyte and Zhu, 2015). Testing these outcomes at the age that children commence school may be indicative of how these characteristics factor into the parents' decision to enrol early or late, however, will be ineffective at examining the benefit of delaying enrolment. These scores are created through responses to SDQ interview questions, a series of 10 questions for each outcome in which the parents and teachers can answer. As discussed in the data section I have transformed multi-valued responses into binary variables, as done in Suziedelyte and Zhu (2015). As a result, the scores in the sample range from 0-10. Furthermore, higher scores indicate a higher tendency to be social, overly emotional or hyperactive. Therefore, sociability is the only test where higher scores indicate more positive outcomes. It is also difficult to justify using parents' responses to these questionnaires as they are likely to be biased. This report includes parents' responses in the analysis, however, they are used mainly for comparison and are not intensively analysed due to self-selection. Teachers, on the other hand, can be assumed to be less bias in scoring children's behaviour and also perceive the child's behaviour amongst, and relative to, other children in the school. This, in itself, presents an issue with using the teacher's scores as these are likely to be heavily influenced by the particular school, the school type and the geographic location. This is the difficulty with attempting to estimate the effect a non-cognitive and non-standardised outcome which is, by nature, evasive and difficult to measure.

Table 6.12 depicts the impact of the policy on children's SDQ scores. The impact of the CCTR, measured by the coefficient of the interaction, suggests a statistically significant improvement in the child's sociability score as measured by their parent and teacher. Whilst this effect is small, it is consistent with the psychological literature that suggests social outcomes benefit most from formal childcare Love et al. (2005). Despite the bias, it is also reassuring that the teachers' perceptions of sociability improvement are corroborated by the parents. Perhaps unsurprisingly, the R-squared terms are considerably low suggesting that the model is a poor predictor of personality traits. Notwithstanding the potential caveats, Table 6.12 provides some evidence of an improvement in social outcomes as a result of the policy. This stands in contrast to other Australian studies that have not been able to attribute an effect of delayed entry on non-cognitive behavioural outcomes (Suziedelyte and Zhu, 2015). Full output can be seen Appendix Table A.9.

Table 6.12: Effect of the CCTR on Strenghts and Difficulties Questionaire (SDQ) score by parent and teacher responders (children 8-9 years old)

	Parent			Teacher		
	(1)	(2)	(3)	(4)	(5)	(6)
	Social	Hyper	Emotional	Social	Hyper	Emotional
Window	-0.0429	-0.154	-0.0731	0.0518	-0.263**	-0.0940
	(0.0801)	(0.105)	(0.0786)	(0.106)	(0.127)	(0.0827)
CCTR	0.214***	0.265***	0.252***	0.168**	0.0778	0.0221
	(0.0598)	(0.0847)	(0.0640)	(0.0815)	(0.104)	(0.0674)
CCTR × Window	0.192***	0.158	0.138*	0.231**	-0.0733	0.0938
	(0.0745)	(0.107)	(0.0812)	(0.105)	(0.135)	(0.0902)
Constant	19.88***	2.920	-11.12**	21.42***	-5.760	-2.879
	(4.571)	(7.069)	(4.841)	(6.207)	(8.118)	(5.158)
N	5109	5108	5109	4478	4478	4477
R^2	0.006	0.006	0.008	0.005	0.005	0.003

Robust standard errors in parentheses

Controlling for individual, parental and family characteristics

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

Note: Sociability is the only score where higher values correspond with more positive outcomes. A positive score in Hyperactivity or Emotional responsiveness indicate less positive outcomes. SQD scores are measured by the sum of parent/teacher responses to a series of questions relating to each behavioural characteristic.

Whilst the results indicate an increase in sociability scores as a result of the policy for children in the window, it is of interest to test if there is evidence of composition effect. As with NAPLAN scores, composition effect does not detract from the findings of improvement in the window, however, it adds an additional level of analysis to how outcomes improve on average for children in the window. Tables 6.13 demonstrate the composition effect of the CCTR on the social scores. For this analysis, I consider only the teacher responses due to the likely issues with interpreting parents scores of child's non-cognitive ability. From the teacher scores, there is a positive and significant effect of the policy for children in the window. Children born in the first half of the year display more positive social characteristics after the introduction of the policy. As previously when considering NAPLAN scores, this is likely because the average age of children in the window has increased as some children make the switch from early to late start on account of the policy. In contrast to the NAPLAN scores, the effect of the policy in the window appears to be driven by early starters. This is indicative of the composition effect as early starters have not taken up an additional year of childcare prior to commencing school. Instead, what is being observed is less mature children switching from an early to late start which increases the average score of the children who start early. The increase in the average of late starters is not statistically significant. This is consistent with the argument put forward by Elder and Lubotsky (2009) in their examination of hyperactivity. Their stance was that hyperactivity amongst younger children was often a misdiagnosis of the relative age gap (Elder and Lubotsky, 2009). Whilst childcare may improve the sociability outcomes for children, the main effect detected is that teachers observe these children in a more comparable cohort. As opposed to being considered as antisocial in a year 3 class, the child is considered average in a year 2 class.

Table 6.13: **Composition Effect of CCTR on SDQ measures (Teacher Response)**

			Window=1	
	(1)	(2)	(3)	(4)
Social SDQ Scores	Window=0	Window=1	Early Start	Late Start
CCTR	0.172	0.177**	0.337*	0.00703
	(0.121)	(0.0835)	(0.173)	(0.170)
Constant	22.99***	24.08**	14.84	37.83**
	(8.267)	(9.810)	(13.40)	(14.81)
N	3080	1398	704	694
R^2	0.008	0.009	0.016	0.010

Standard errors in parentheses

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

Note: Columns 1 represents the estimated impact of the CCTR on children born outside the window. Column 2 estimates the impact of the CCTR on children born inside the window. Columns 3 and 4 estimate the effect of the CCTR on children within the window who start early and those that start late.

6.3.4 Antisocial Behaviour

Another non-cognitive variable of interest, not formally identified in the above SDQ scores, is the disposition towards, or subjectivity to, antisocial behaviour. For the measure of antisocial behaviour I rely on school-yard bullying. There are some suggestions from child psychology literature that children who commence school relatively young for their year are not only more likely to be bullied but also to become bullies themselves (Parker et al., 2018). The former outcome is more obvious; younger children may be smaller or less mature making them an easier target for older children. However, Parker et al. (2018) often reason that children who are younger may choose to overcompensate for their age and size by demonstrating aggression. These children, who would otherwise be enrolling early, now have the opportunity for an additional year to mature and grow prior to school and will enter as the older children in the cohort.

I construct two tests based on children's disposition towards bullying and the likelihood that a certain child will be bullied. I generate simple indicator variables from information provided by teachers as to whether or not the child is often a victim or perpetrator of bullying. I have similar information from the perspectives of parents, however, I choose not to include this as I expect parents to be less inclined to identify if their child is a bully or is often bullied. Not only that, teachers have greater oversight to the interactions of the child at school amongst their peers and provide a more objective opinion. Again I control for individual as well as socio-economic variables and include height and weight to control for physical components of bullying. Similar to the SDQ scores, I test this variable when the children are ages 8 or 9 years old. The reason for this is to ensure that all children are in school and to mitigate any possible determinism of the dependent variables on the likelihood of starting school early. Similarly, the question of interest is not whether bullies are more likely to start school early, but rather if an early start promotes antisocial tendencies.

Examining results in Table 6.14 indicate there is no significant impact of the interaction term on the likelihood of being a bully or being bullied. The two terms that carry the most significance appear to be physical components of height and weight. Interestingly, the coefficients on these terms indicate the same sign for both regressions indicating that smaller children are more likely to be bullied and be bullies themselves. This is consistent with the psychological literature that suggests that smaller children sometimes feel the need to overcompensate with aggression (Parker et al., 2018). Similarly, heavier children are more likely to be bullied as well as bully others. Unsurprisingly, the R^2 indicates our model is ineffective at accounting for much of the variance. This is somewhat expected as personality and behavioural characteristics to be more difficult to define by a formal model. The full model can be viewed in the Appendix Table A.10.

Table 6.14: Effect of the CCTR on prevalence of antisocial behavior (8-9 years old)

	(1) Bully	(2) Bullied
CCTR	-0.00988	0.00148
	(0.0111)	(0.0137)
Window	-0.0169	0.00615
	(0.0186)	(0.0231)
CCTR $\times$Window	-0.0105	-0.0173
	(0.0185)	(0.0240)
Height	-0.00208***	-0.00225***
	(0.000752)	(0.000749)
Weight	0.00286***	0.00698***
	(0.000853)	(0.00117)
Constant	-0.810	1.839
	(0.868)	(1.243)
N	4565	4176
R^2	0.010	0.018

Robust standard errors in parentheses

Controlling for individual, parental and family characteristics

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

Note: Anti-social behaviour is measured by prevalance of bullying at school. In LSAC data set, teachers respond to a question "Child often bullies" or "Child is often bullied"

In analysing the effect of delayed entry on NAPLAN scores, kindergarten repetition and behavioural characteristics, this study has identified clear improvements across most measures. This is consistent with the idea that commencing school early is harmful to development and that many children benefit from an additional year of non-primary education prior to commencing school. Furthermore, the CCTR has promoted formal care over informal care which better prepared children for the transition to school. The analysis of NAPLAN results demonstrated the most robust and easily interpretable benefit of delayed enrolment for children in the window. On average, scores improved by the equivalent of 5-6 months of learning and this effect only marginally diminished by year 5. This was used as evidence that the effect of relative age exists up until the end of primary school and likely persists into high school. Concern is warranted if this effect continues into high school as from as early as year 7, children are sorted into classes based on ability as well as age. This may exacerbate and perpetuate the existence of the entry-age achievement gap, however, data infancy prohibited this analysis (Elder and Lubotsky, 2009). Through the examination of kindergarten repetition, I argued that the observed reduction in this measure is likely indicative of cognitive and non-cognitive improvements. Not only is a 2% reduction in kindergarten repetition indicative of children entering school better prepared for the significant transition, but also indicative of reduced risk of the harmful consequences of grade retention. In terms of non-cognitive outcomes, the analysis was less clear. I found evidence that the introduction of the CCTR lead to greater social awareness and competence among children, however, could not substantiate any decrease in antisocial proclivities. Nonetheless, this thesis finds evidence, consistent with the theoretic model, that the CCTR has led to improved outcomes on average through encouraging delayed school enrolment for children in the window.

Chapter 7
Robustness

Accurate interpretation of the results is dependent on the validity of a number of specifications, assumptions and simplifications made as part of the experimental design. In this section, I begin with testing the robustness of binary models with non-linear functions and analyse any differences between linear probability models and non-linear models. Secondly, I introduce a multinomial logit model that predicts how families' childcare choices are impacted by the introduction of the rebate. This supports the main results as I find that the rebate dummy is only significant in the decision to enrol children in formal childcare and not informal childcare. This gives weight to the validity of the CCTR variable and provides evidence that the main effect being captured by this time dummy is the policy intervention. Lastly, I ignore the endogeneity of childcare and labour market status and divide the sample into eligible and ineligible cohorts to create the desired control group for the base model regression. Using this specification, I find evidence that the rebate is significant in determining school entry only for those who are eligible to receive the rebate. Again, this specification gives credibility to the policy intervention dummy as I observe no significant effect on enrolment for those who cannot obtain the rebate due to labour market status or non-cost childcare preference.

7.1 Model Specification

Base Model

A considerable amount of the results thus far have been produced by linear probability models (LPM). As described in Chapter 5, the justification for using LPM rather than a logit or probit model was a matter of convenience as interaction effects, used to extract the policy impact, cannot be as readily interpreted from coefficients in non-linear models (See, for example, Ai and Norton (2003)). Here I demonstrate that the issue that Ai and Norton (2003) describe does not apply in the case where I conduct difference in differences analysis and demonstrate that non-linear models return similar predictions for the policy impact as the LPM model. The coefficient on interaction terms cannot be interpreted in magnitude or direction as the interaction effect must be calculated as the cross derivative of the expected value of Y (Ai and Norton 2003; Puhani 2012). As a result, it is possible for the interaction effect to be non-zero even if the coefficient is zero. Moreover, standard t-tests and the sign of the coefficient cannot be used or interpreted (Greene, 2010). The reason that this does not apply to difference in difference is because there is only one variable that identifies treatment; in this case, when 'CCTR' and 'Window' are both non-zero. Therefore the treatment effect can be thought of as the difference in potential outcomes (Puhani, 2012). Consider the non-linear model (denoted with F) where C (CCTR) and W (window) represent the components of the interaction effect.

$$E[Y|C, W, X] = F(\alpha C + \beta W + \gamma CW + X\theta) \tag{7.1}$$

According to Puhani (2012), our interaction effect will simplify to

$$F(\alpha + \beta + \gamma + X\theta) - F(\alpha + \beta + X\theta) \tag{7.2}$$

As such, the sign of the coefficient of the non-linear model can be interpreted and magnitudes can be calculated using marginal effects as per the standard logit and probit case (Puhani, 2012).

Although it has been established that these models are usable for this purpose, Table 7.1 indicates another issue with using non-linear models to estimate the likelihood of early enrolment. Immediately obvious is the lack of coefficients on 'Window' and 'CCTR' for the logit and probit models. This is because non-linear models are more sensitive to the perfect prediction between the 'window' variable and the 'early start' variable; only children born in the window can start early. As a result they restrict the sample to only children within the window. Therefore, although there is an interaction term present in Table 7.1, the results are incomparable between the LPM and the non-linear cases due to vastly different samples. This issue with perfect prediction suggests that a linear probability model is the most appropriate model for this particular analysis.

Table 7.1: Effect of CCTR on early enrolment for linear and non-linear model specifications

	(1)	(2)	(3)
	LPM	Logit	Probit
CCTR	0.001		
	(0.00355)		
Window	0.472***		
	(0.0179)		
CCTR × Window	-0.0505**	-0.0503**	-0.0518**
	(0.0235)	(0.0211)	(0.0216)
Constant	0.143		
	(0.897)		
State FE	Yes	Yes	Yes
N	4955	1587	1587
R^2	0.465		

Robust standard errors in parentheses

Controlling for individual, parental and family characteristics

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

Note: Logit and probit models restrict the sample size due to strong correlation between window and early enrolment. Perfect prediction error undermines any comparison between LPM and non-linear models

To accommodate these issues, I show robustness by restricting the sample to only the window and computing marginal effects in this subpopulation. This avoids the use of an interaction term and relaxes the difficulties with perfect prediction in the logit and probit regressions. This does not undermine the analysis, as ultimately I am only concerned with children born in the window to whom early enrolment is an option. Children born outside the window cannot start early, so restricting the sample is valid for the purpose of robustness. It will, however, change how coefficients are interpreted as they are no longer with respect to the entire cohort, rather only children born in the window. From this restricted sample, I compare the results of the LPM to the logit marginal effects and find minimal difference in terms of economic magnitude and greater statistical significance under the logit. The results presented in Table 7.2 demonstrate that, regardless of linear or non-linear model, there is an approximate 5% decline in the probability of early enrolment as a result of the introduction of the tax rebate. This result indicates that our estimations for the policy effect are not dependent on a particular model specification. Furthermore, it suggests that, in light of the issues with non-linear models discussed above, the LPM model is the most appropriate in this case.

Table 7.2: Linear and non-linear (logit) estimates for the effect of CCTR on early enrolment (children in the window only)

	(1)	(2)
	Marginal Effects (Logit)	LPM
CCTR	-0.0565***	-0.0540**
	(0.0215)	(0.0222)
Age in Month (July)	0.110***	0.112***
	(0.00784)	(0.00902)
Male	-0.105***	-0.106***
	(0.0211)	(0.0221)
Migrant	0.271***	0.288***
	(0.0365)	(0.0372)
Owns home	-0.0904***	-0.0899***
	(0.0259)	(0.0264)
Log Income	-0.734**	-0.794**
	(0.347)	(0.371)
Log Income Squared	0.0329**	0.0354**
	(0.0156)	(0.0167)
Hyper	0.0122**	0.0132**
	(0.00559)	(0.00566)
Constant		-2.035
		(2.124)
State FE	Yes	Yes
N	1587	1587
R^2	0.2422	0.285

Robust standard errors in parentheses

Controlling for individual, family and school characteristics

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

Note: Logit estimates reported in marginal effects. Children in the "window" are children born between january first and relevant state-based cutoff.

Children's Outcomes: Repetition

Testing the model specification on children's outcomes does not encounter the same issues with perfect prediction as in the base case discussed above. Unlike the case with early enrolment, kindergarten repetition is not exclusively an option for children in the window, therefore logit and probit will not have difficulty in estimation. To ensure that the results obtained from the analysis of kindergarten repetition are robust to specification, I compute the same specifications with non-linear models. Interpreting the effect of the CCTR on kindergarten repetition through a logit and probit model yields estimates presented in Table 7.3. As in the LPM case, the effect of the policy on the likelihood of repeating kindergarten is approximately 2% across all specifications. These results suggest that the effect obtained under the LPM model are robust to model specification. I do not test for antisocial behaviour as this result failed to yield significant results under the linear model.

Table 7.3: Effect of CCTR on kindergarten repetition for linear and non-linear specifications

	(1) LPM	(2) Logit	(3) Probit
Window	0.0152*	0.0154*	0.0155*
	(0.00888)	(0.00876)	(0.00867)
CCTR	0.00102	0.00140	0.00124
	(0.00538)	(0.00577)	(0.00551)
Window × CCTR	-0.0188**	-0.0197*	-0.0205**
	(0.00943)	(0.0101)	(0.00969)
Age in Months (Jul)	-0.000876	-0.000959	-0.000935
	(0.00112)	(0.00121)	(0.00114)
Male	-0.000602	-0.000645	-0.000511
	(0.00450)	(0.00463)	(0.00451)
Constant	0.132		
	(0.438)		
State FE	Yes	Yes	Yes
N	4837	4837	4837
(Pseudo)R^2	0.010	0.0452	0.0461

Robust standard errors in parentheses

Controlling for individual, family and school characteristics

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

Note: Effect of the policy is represented by the coefficient on the interaction of CCTR and Window. Logit and Probit estimates report the marginal effects.

7.2 Choice of Childcare

The base case regression utilised a linear probability model and found that the childcare rebate led to a statistically significant decline in the likelihood of enrolling early in school. To support this result, and further substantiate the predictions of the empirical model, I test the determinants of childcare choice. If the linear model correctly identifies that parents are choosing to delay school in favour of childcare as a direct result of the policy, then this effect should be detectable in the utilisation of formal childcare but not necessarily informal care.

For this test, I generate a new variable with three choices of childcare: school, formal care and informal care. This allows for the use of a multinomial logit (MNL) to determine how parents decide between formal and informal care after the rebate relative to a base case. I set the numeraire to be early enrolment in school, as this specification has the most appropriate interpretation in this case. For this analysis, I test only children born within the window as only these children have all three types of care available as options. For children outside the window, childcare choice is a binary variable decision between formal and informal care. LSAC provides a number of childcare variables, however, for the construction of the dependent variable I consider only the childcare listed as "Primary childcare". Using these definitions, the number of children and proportion of children for each childcare type are shown in Table 7.6.

Table 7.4: Primary form of care for children born within window

Type of Care	Observations	Percentage
Primary School	865	51.21%
Formal	572	33.87%
Informal	252	14.92%
Total	1689	100%

Note: Table presents the proportion of children inside the window in each form of child care choice. Formal childcare refers to day care centres or other forms of paid childcare excluding nannies. Informal care refers to parents, neighbours grandparents and so on.

A multinomial regression appeals to this analysis because it allows the examination of choice between multiple forms of childcare holding one option as the numeraire. The results of an MNL are interpreted as the change in log odds of using formal or informal care relative to school enrolment, which is set as the base case (Dancer and Fiebig, 2004). For a robustness measure, the parameter direction and significance is more important to the interpretation than understanding changes in log odds.

Therefore, the sign of the coefficient on CCTR is interpreted as indicating an increase or decrease in the likelihood of choosing a form of childcare if the CCTR is available. For analysis, it is important to first establish whether the primary assumption of MNL, the independence of irrelevant alternatives (IIA), holds in this particular case. The IIA assumption simply states that the inclusion or exclusion of an alternative (C) does not affect the likelihood of preferring A over B, or vice versa (Hausman and McFadden, 1984). In this case, the IIA assumption holds if it is reasonable to assume that an agent's preference for formal care over informal care is irrelevant of whether the child is eligible for school or not. This is an intuitive assumption in this particular case and conducting a Hausman-McFadden test ($\chi^2_1 = 0.28, Pr(\chi^2_1 > \chi^2) = 1.000$) yields no evidence that the IIA assumption is violated suggesting that analysis will be suitable.

The results of the MNL in Table 7.5 support the main analysis and predictions of the empirical model. For simplicity, I refrain from interpreting the magnitude of the change in log odds and only infer increases or decreases in probability indicated by the sign of the coefficient and statistical significance. From Table 7.5, I can conclude that after the introduction of the CCTR, the child is more likely to be enrolled in formal day care relative to primary school at a 10% statistical level. On the other hand, the CCTR variable does not impact the likelihood of utilising informal care.

From this model, it is also discernible that males are more likely to use either form of childcare relative to school enrolment and similarly, children who score better on the Peabody test are less likely to use childcare relative to enrolment. ESL families, as found in previous models, are less likely to be in both forms of childcare relative to early enrolment. This is consistent with the notion that migrant families seek to engage their children in education as early as possible in order to assimilate and develop English skills that may not be as well developed in other forms of care. It could also represent cultural differences in the value and significance of education. Income, in this model, is difficult to interpret. As a result of considering household income, there is a predictable relationship between higher incomes and childcare usage as higher incomes will likely imply both parents are working rather than only one. Income coefficients on the probability of using formal care are insignificant as was the case in the base model in Chapter 6. Again, it is possible that wealth is cannibalising some of the statistical significance for the income terms. The coefficients on informal care are in line with what we might expect, the higher the income of a certain household member, the more likely that the family can afford to have a stay at home parent relative to early enrolment. Importantly, none of the results presented in Table 7.5 are remarkably different to the coefficients in the base models. The interpretations remain unchanged between considering school enrolment or childcare choice. Furthermore, the results of this MNL support the assertion that the CCTR variable mainly captures the effect of the policy intervention.

Table 7.5: Multinomial Logit - Testing the change in log likelihood of childcare choice relative to early enrolment after the introduction of the CCTR

	(1) Formal Childcare	(2) Informal Childcare
CCTR	0.223*	0.22
	(0.118)	(0.158)
Male	0.374***	0.639***
	(0.117)	(0.158)
Mother's Education (Years)	0.0392	0.0832**
	(0.0258)	(0.0347)
Medical Condition	0.387**	0.0923
	(0.158)	(0.219)
Peabody Score	-0.0266**	-0.0289**
	(0.0107)	(0.0145)
ESL	-1.021***	-1.193***
	(0.213)	(0.310)
Owns Home	0.332**	0.302
	(0.136)	(0.184)
Log Income	2.734	4.714*
	(1.876)	(2.751)
Log Income^2	-0.118	-0.215*
	(0.0845)	(0.125)
Constant	-15.18	-26.11*
	(10.45)	(15.23)
State FE	Yes	Yes
N	1587	1587
Psuedo R^2	0.0340	0.0340

Robust standard errors in parentheses

Controlling for individual, parental and family characteristics

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

Note: Multinomial logit coefficients estimate the change in log likelihood for formal and informal childcare relative to the base case (early enrolment) after the introduction of the CCTR.

7.3 Eligibility Status

In Chapter 5, I discussed that an endogeneity issue prevents creating a control group for our base model based on ineligible and eligible families. To reiterate, I reasoned that some stay at home parents are eligible for the rebate despite their status because they may have decided to leave the workforce due to the cost, even with the rebate considered. Therefore attributing eligibility based on labour force status excludes parents who would have been eligible had they stayed in work. In this section, I attempt to address this endogeneity issue by isolating parents who would otherwise work were childcare not as costly from those parents who, due to variables such as partner wealth or income, would choose not to work regardless. Similar to the multinomial logit, if there exists a divide between eligible and ineligible parents, this can be used as evidence that the primary effect being captured by the CCTR dummy is the introduction of the policy. If the CCTR variable is not primarily capturing the effect of the rebate and instead capturing another uncontrolled variable, then there is no reason that ineligible families should behave differently to eligible families.

Intention to work is estimated by observing labour market status when the child is 8-9 years old and is a few years into their schooling. If the carer has rejoined the labour force since their child entered school then they are considered 'eligible'. If the carer has not altered their work/study status, it is assumed that their decision to not work is independent of childcare costs and these families are deemed ineligible. I exclude families in which the child of interest has siblings younger than 5 as this will continue to impact the childcare decision.

Furthermore, for this specification only, I assume that families who have access to non-cost childcare are unlikely to alter their childcare decision because of the rebate only covers a fraction of the total cost. This may be a contentious assumption, as it is not clear from this research how parents make the decision between formal and informal care. If childcare is subsidised, it is plausible that parents would alter their childcare choice due to changing constraints. Parents preferences towards childcare, however, are difficult to measure and as a result, I make this reasonably simplistic assumption to make analysis possible. Therefore added to the ineligible cohort are any family who specified their primary care as being non-cost including grandparents, neighbours, friends or other relatives. This includes after-school care if the child has already commenced school. The breakdown of the sample by eligibility status can be shown in table 7.6.

Table 7.6: **Proportion of sample in each form of primary care**

Type of Care	Observations	Percentage
Eligible		
	3,557	66.57%
Ineligible		
Non-Cost Care	1,483	27.78%
Stay-at-home Parents	303	5.67%
Total	5,343	100%

Note: Table demonstrates the proportion of children theoretically eligible and ineligible for the rebate based on labour force participation and childcare choice. Ineligible families are characterised by having a stay-at-home parent or primarily use non-cost during or after school care.

Table 7.6 demonstrates that approximately 30% of families are ineligible for the rebate as specified by labour market status and access to non-cost care. For completeness, I conduct a balance of covariates to test whether eligible and ineligible families are measurably different from one another. Table 7.7 shows that the two groups are not all that dissimilar. Single parents appear to have a greater propensity towards either staying at home with the children or utilising non-cost care and larger families with more siblings tend to have two working parents and use formal care.

Table 7.7: Balance of covariates measuring statistical differences in cohorts divided by eligibility for childcare rebate

	(1) Ineligible mean	(2) Eligible mean	t
Age in Months (July)	58.85	58.88	-0.30
Male	0.51	0.51	0.49
Rural	0.12	0.11	0.93
Relig	0.78	0.78	0.17
Mother's Education (Years)	13.87	13.77	1.42
Father's Education (Years)	13.17	13.48	-4.36***
Medical Condition	0.14	0.14	0.17
Peabody Score	64.85	64.70	0.88
Single Parent	0.16	0.09	6.58***
ESL	0.10	0.11	-1.29
Younger Siblings	0.53	0.59	-3.34***
Older Siblings	0.81	0.89	-2.96**
Indigenous	0.02	0.03	-0.85
Owns Home	0.73	0.73	0.02
Log Income	11.07	11.08	-0.65
Hyper Score	3.34	3.30	0.67
Emotional Score	1.56	1.54	0.49
Sociability Score	7.75	7.73	0.43
Height	133.07	133.62	-1.60
Weight	31.51	31.79	-1.30
N	1786	3557	5343

Note: Eligibility status defined by labour market status and choice of childcare. Labour market status is found by observing participation after the child has commenced school to attempt to account for endogeneity between childcare choice and labour market status. Childcare choice described by primary form of during or after school care.

Regressing the impact of the childcare rebate on the likelihood of early enrolment reveals that only eligible families are impacted by the policy. Whilst the statistical significance has diminished from the coefficient, the magnitude (-5%) is similar to previous models. Again, this regression strengthens the assumption that the policy variable is primarily capturing the effect of the rebate. Isolating particular effects that are consistent with this particular policy give confidence that the result observed in the base case is not the product of another policy or event. If the decline in early enrolment observed in Chapter 6 was being driven by another factor other than the rebate, it is less likely that this specification would identify a distinct difference in behaviour between eligible and ineligible families.

Table 7.8: Effect of CCTR on early enrolment by rebate eligibility status

	(1)	(2)
	Ineligible	Eligible
Window	0.435***	0.486***
	(0.0320)	(0.0217)
CCTR	0.00525	-0.00297
	(0.00619)	(0.00452)
CCTR× Window	-0.0586	-0.0463*
	(0.0423)	(0.0283)
Constant	0.427	-0.116
	(1.908)	(1.017)
State FE	Yes	Yes
N	1627	3328
R^2	0.422	0.489

Robust standard errors in parentheses

Controlling for individual, parental and family characteristics

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

Note: Impact of the policy is represented by the interaction of the CCTR and Window. Eligibility described using labour market status and childcare choice.

With these additional tests, this thesis argues that the robustness of the results has been sufficiently demonstrated. Firstly, this chapter addressed concerns about the choice of binary regression specification and showed that logit and probit models are ineffective in the primary regression due to perfect prediction. By restricting the sample to only those in the window, it was shown that the linear and non-linear specifications gave similar significance and magnitude to the impact of the CCTR on the likelihood of early enrolment. Furthermore, non-linear models were shown to be compatible in the kindergarten repetition regression where coefficients comparable with the linear model were again extracted. This demonstrated that the results of the base model and the kindergarten repetition model were not likely to be driven by the choice of specification.

The remainder of the robustness checks explored the possibility of the CCTR variable capturing an effect other than the introduction of the policy. This was shown through a multinomial logit (MNL) as well as the eligibility requirements. The MNL allowed for the modelling of multiple forms of childcare for children in the window. Using a three-option specification, holding early enrolment as the numeraire, the MNL found that the introduction of the rebate increased the likelihood of attending formal childcare relative to school care but had no statistically significant impact on the choice of informal care. This was viewed as supporting the assumption that the CCTR variable is predominantly capturing the effect of the CCTR between 2004 and 2008. As a further robustness check of the CCTR variable, the sample was split by eligibility status. As discussed in the Methodology, this specification is undermined by an endogeneity issue. This regression attempted to address this endogeneity issue by using labour market participation of the carer after the child commences school. This hoped to isolate only those carers who would otherwise choose to work, were they not restricted by childcare cost constraints. Also considered as ineligible were families whose primary forms of childcare, both during or after school hours care, were informal including grandparents, neighbours or other unpaid community members. The results of this regression were that there was no statistical significance of the childcare rebate on ineligible families. This was viewed as evidence that the effect being captured in the CCTR variable was only impacting families with working parents who used costly childcare.

Chapter 8
Discussion and Conclusion

8.1 Discussion

The exact age a child should start their formal education is an area of research that attracts a significant amount of attention. However, it has been extensively demonstrated that it is not only raw age that affects outcomes, but also relative age (Black et al., 2011; Elder and Lubotsky, 2009; Fitzpatrick et al., 2011; Jha, 2014; Suziedelyte and Zhu, 2015). This thesis provides a comprehensive analysis of the causes and effects of early enrolment by utilising the introduction of the childcare rebate in 2005 and its subsequent increase in 2008. I find that, on average, the introduction of the CCTR has led to a 5% decrease in the probability of early enrolment. The mechanism with which the childcare rebate was predicted to impact early enrolment was through the subsidisation of an alternative form of care. By subsidising formal childcare, it was theorised, using the utility model specification, that less able children would delay their enrolment another year in favour of childcare.

Consistent with other studies in this area (Fitzpatrick et al. 2011; Gathmann and Sass 2018; Jha 2014), this research found evidence of the childcare decision does have a heterogeneous effect between boys and girls. Dividing the sample by gender revealed that the enrolment decision for girls was unaffected by the introduction of the rebate. Boys, on the other hand, were 8% less likely to commence school. This is consistent with the notion of a maturity gap between boys and girls at this age (Fitzpatrick et al., 2011). Parents, in general, consider girls more mature at this young age and do not believe that they stand to lose by commencing school relatively younger than their peers (Gathmann and Sass, 2018). This thesis substantiates these findings and as a result, boys appear to be the primary drivers of the change in outcomes. On top of this, I also find that low-income families are particularly responsive to the introduction of the childcare rebate compared to higher incomes. The responsiveness of low-income families to the introduction of the childcare rebate was of a magnitude of an 11% reduction in the likelihood of early enrolment. Medium and high incomes, on the other hand, appear unaffected by the policy. This finding is consistent with the mechanism of the empirical model as it is expected that budget constraints, and hence childcare choice, vary across the population.

Subsequent to analysing the impact the childcare rebate had on early enrolment, this thesis estimated the effect a reduction in early enrolment had on cognitive and non-cognitive outcomes. Not only did these difference in differences support the findings by reconciling results with the theory, but they also provided a comprehensive evaluation of the effect on outcomes that can be attributed to delayed school entry. This thesis found that on average, NAPLAN scores increased by approximately 50 points in years 3 and year 5. A feature of the NAPLAN scores is that they increase incrementally as a child progresses through school allowing parents and teachers to track progress and development. This facilitated the evaluation of the policy effect in terms of months of schooling. Consequently, a 50 point increase was found to be the equivalent of approximately 6 months worth of learning (Jha, 2014). This result was similar in magnitude to another Australian study that analysed a policy change governing enrolment cutoffs on NAPLAN scores (Jha, 2014). Analysing the increase in school entry age by six months, Jha (2014) found a 71.4 point increase in NAPLAN scores in Queensland and a 38.2 point increase in Western Australia. In addition, this thesis found no evidence to suggest the effect of relative age diminished over the short to medium term, as found in other papers (Black et al. 2011; Elder and Lubotsky 2009). On the contrary, the impact on test scores was as pronounced in year 5 as it was in year 3, suggesting that the effect of relative age is likely to persist into high school. Further testing beyond year 5 was prohibited due to the infancy of the LSAC data. By the same mechanism, this thesis found that the rebate reduced the likelihood of kindergarten repetition by 2%. Not only does kindergarten repetition represent a waste of school resources, repetition is considered detrimental to a child's self-esteem and academic potential (Love et al., 2005). To my knowledge, this thesis is the first to examine the impact of early enrolment on kindergarten repetition in Australia. This result was slightly less than comparable tests in the United States. Deming (2009) found that the introduction of the Head Start program that provided preschool for disadvantaged children reduced repetition by 7%.

Beyond the impact of early enrolment on cognitive outcomes, this thesis found evidence to suggest that early enrolment affects behavioural outcomes as well. Using parent and teacher responses to the Strengths and Difficulties Questionnaire (SDQ) - an internationally recognised psychological measure of development - this thesis found evidence of improvement as a result of delayed entry. On the sociability score, which measures traits such as compassion, empathy, ability to share and communicate, parents and teachers recognised an improvement on average. However, the CCTR had no recognisable impact on antisocial behaviour measured through tendencies towards bullying.

8.2 Limitations and Further Research

This research is not without its limitations and has made efforts to be upfront about its potential shortcomings. As LSAC is constructed with only two cohorts of children born four years apart, we can only measure the enrolment decision once for each cohort. This has generated a large space of time between cohorts and potentially compromises the interpretation of the policy variable in both the base model and the difference in differences analysis. Ideally, there would be two cohorts directly either side of the policy intervention which would make for a more compelling assumption that the only difference between cohorts is access to the rebate. Instead, there was a difference of four years between cohorts, with the CCTR being one of, what is more likely many, differences that occurred over this time frame. This assumption was buoyed by the use of the Household Income and Labour Dynamics Australia (HILDA) survey to perform trend analysis and a series of robustness checks. By analysing the proportion of children enrolling early over a 12 year period between 2000 and 2012 indicated that there were no long-term trends that might damage inference of the causal effect of the rebate. For example, the research would be significantly compromised if a general decline in early enrolment was observed, perhaps as a result of community awareness to the consequences that may generate gradual aversion.

The inflexibility of the LSAC cohort structure prevented some additional robustness checks, such as a placebo test, that might have provided greater insight and credibility to the results. With more time it is possible that this could be conducted using HILDA. Additionally, it would be interesting to test the same policy effect on Child Care Benefit (CCB) introduced in 2000, however, there appears to be inadequate data that I am aware of to conduct this experiment. The first wave of HILDA and LSAC commenced in 2001 and 2004 respectively, meaning that neither of these extensive data sets would offer any insight. I have been unable to find Census data or data from other sources including state-based education departments that provide the necessary information, particularly birthdays and extensive independent controls. I expect that the CCB would have had similar results to what I have uncovered in this paper and the presence of a pre-existing benefit scheme may dilute the results determined in this thesis.

This thesis leaves room or further contribution to the topic of early enrolment, particularly in relation to long-term outcomes. As discussed in Chapter 6, large and persistent effects of delayed enrolment were identifiable in year 5 NAPLAN scores, indicating that the effect likely persists into high school. Testing of the this was limited by unavailability year 7 NAPLAN scores in both cohorts, however, it would be interesting and relevant to understand how this effect is captured in the long-term.

The reason for concern is that children are selected into high school classes, more so than primary school classes, based on prior performance and academic achievement. If the relative age effect persists into high school, the magnitude of the entry-age achievement gap is likely to be substantially more important than in studies that observe only primary school achievement. Furthermore, the exploration of non-cognitive outcomes, whilst more comprehensive than most studies that examine only test scores, could be tested in other environments. One of the concerns discussed in Chapter 6 was the unreliability of measuring self-reported information from parents and teachers who possess their own individual circumstances and biases. No Australian study that I have examined whilst writing this thesis has explored non-cognitive, or cognitive outcomes for that matter, beyond the age of 9 years old. Longer-term data, such as census data, may yield interesting measures of non-cognitive ability through risk-taking behaviour similar to that observed in Norway (Black et al., 2011).

Not considered in this thesis, but a natural extension of my work involves examining the benefits and cost of various forms of non-primary education. Although there is an existing body of literature advocating for the importance of early childhood education, see (Cuhna and Heckman 2008; Cuhna et al. 2010; Heckman et al. 2015), research on this topic in Australia is limited. For the purposes of this thesis, I have largely assumed that the quality of formal childcare is superior to informal care for children approaching school starting age. This, however, need not be the case, particularly for younger infants. It is largely posited in the literature that childcare policies in Australia and abroad have generally led to increasing female labour participation (Connelly 1992; Doiron and Kalb 2002). This has generated a growing interest in analysing the costs and benefits of outsourcing early childhood care and decreased parental involvement in infantile development (Liang, Pickles, Wood, and Simonoff 2012; Veramendi and Urzua 2011). Another angle that was not explored in detail in this paper, but has been considered in others, has been the effect of childcare policies on the average quality of care. A number of papers (see Baker et al. (2008); Cascio and Schanzenbach (2013); Lefebvre et al. (2011)) have found that generous subsidies, particularly universal provision of care, have diminished quality and crowded out private care. This is of significant interest to Australian policy makers and analysts as this year, the Australian government committed to providing universal childcare for all 4 years old children, a program estimated to cost $440 million dollars per year once implemented, despite scepticism amongst researchers as to the overall effectiveness of universal programs (APH, 2017). Heckman et al. (2015) unambiguously disclose their position on the matter by arguing “the economic case for universal early childhood programs is weak”.

8.3 Concluding Remarks

In Australia, due to the misalignment of cutoff dates with the start of the school year, the school starting age of up to half of all children in a particular state will be determined by parents. As a result, many children commence school up to a year younger than the other children in the birth cohort. This has resulted in significant interest being devoted to understanding the impact of early school enrolment on a child's short and long-term outcomes. Experimental design, namely instrumental variable analysis, has been the preferred method of measuring this impact in the presence of endogeneity between consequences and determinants of early enrolment. Using birth dates relative to school enrolment cutoff has been the preferred method of analysing the detrimental impact of early enrolment by assuming children start school when they are first eligible. This approach is appropriate in countries with high levels of compliance to enrolment eligibility (Bedard and Dhuey, 2006). However, due to a higher propensity of parents in Australia to 'redshirt', the practice of intentionally holding eligible children back another year, age-based instrumental variables are less informative (Edwards et al., 2011).

In this paper, I proposed using the introduction of the childcare rebate in 2005 as a basis for experimental design. This approach deviates from the literature by focusing on the cost of childcare as the primary determinant of early enrolment. The underlying hypothesis, which was formalised through a utility maximisation specification, was that a substantial reduction in the cost of childcare would provide parents with an economically feasible alternative to early enrolment. This would allow the decision to be based on the ability and relative maturity of their child rather than the cost of alternative forms of care. My regression model estimated that the childcare rebate led to 5% reduction in the likelihood of early enrolment in primary school. Furthermore, using the richness of the LSAC data set, I was able to construct a series of difference in differences models that assessed the average impact of fewer instances of early enrolment on multiple outcomes of interest. The decline in early enrolment has led to consistently higher NAPLAN scores across years 3 and 5 with no evidence supporting claims found by other authors that this effect diminishes over time Elder and Lubotsky (2009).

Furthermore, this thesis attempted to extend the discussion to other measurable outcomes that may be impacted by relative age, rather than focus on academic achievement. Additionally to the results on NAPLAN scores, children were 2% less likely to repeat kindergarten subsequent to the introduction of subsidised childcare, suggesting that children are better prepared for the transition to formal schooling as a result of exposure to formal childcare. Non-cognitive outcomes were also assessed to estimate the impact of the CCTR on behavioural qualities in school-age children. Using teachers perception of social awareness and competence measured by the internationally recognised SDQ measure, a positive increase in sociability was identified in 8 to 9 year-old children as a result of exposure to an additional year of development in formal childcare institutions.

This thesis has identified an alternative method in evaluating the impact of early enrolment by analysing the cost of care. In corroboration with the theoretic model, it was identified that the cost of care was a significant determinant of early enrolment by demonstrating that the introduction of a rebate significantly decreased the likelihood of enrolment. Through analysing the benefits of delayed entry, it was found that the Australian Affordable Childcare policy has improved the average outcomes of children across multiple components of development.

Appendix A

Table A.1: Mean comparison of Kindergarten repetition by treatment status and eligibility for early enrolment

Kindergarten Repetition	Pre-policy	Post-policy	Difference
Window=1	0.030	0.0146	-0.015
(Treatment)	(0.172)	(0.120)	(0.005)
Window=0	0.019	0.022	0.003
(Control)	(0.137)	(0.145)	(0.001)
Difference-in-difference			**-0.019**
			(0.001)

Table A.2: Mean comparison of aggregate NAPLAN scores by treatment status and eligibility for early enrolment for Year 3 Students

Aggregate Y3 NAPLAN	Pre-policy	Post-policy	Difference
Window=1	2109.87	2144.26	34.39
(Treatment)	(16.23)	(14.35)	(21.84)
Window=0	2125.17	2103.53	-21.64
(Control)	(11.62)	(8.74)	(14.69)
Difference-in-difference			**56.03**
			(0.736)

Table A.3: Mean comparison of aggregate NAPLAN scores by treatment status and eligibility for early enrolment for Year 5 Students

Aggregate Y5 NAPLAN	Pre-policy	Post-policy	Difference
Window=1	2484.49	2540.03	55.54
(Treatment)	(14.30)	(13.21)	(19.46)
Window=0	2511.17	2510.70	-0.921
(Control)	(9.05)	(9.32)	(12.99)
Difference-in-difference			**56.46**
			(0.57)

Table A.4: Impact of CCTR on Early start - Base model and HTE Specifications

		Gender		Income		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Male	Female	Low	Middle	High
Window	0.471***	0.500***	0.446***	0.530***	0.427***	0.436***
	(0.0180)	(0.0260)	(0.0248)	(0.0288)	(0.0317)	(0.0338)
CCTR	0.0000490	0.00233	-0.00268	0.0107	-0.00403	-0.0164**
	(0.00366)	(0.00513)	(0.00530)	(0.00651)	(0.00549)	(0.00683)
CCTR × Window	-0.0519**	-0.0272	-0.0792**	-0.117***	0.00137	-0.0167
	(0.0237)	(0.0334)	(0.0331)	(0.0417)	(0.0418)	(0.0415)
Age in Months (July)	0.0123***	0.0149***	0.00916***	0.0140***	0.00933***	0.0138***
	(0.00121)	(0.00175)	(0.00170)	(0.00220)	(0.00203)	(0.00217)
Male	-0.0347***			-0.0304**	-0.0420***	-0.0319**
	(0.00795)			(0.0142)	(0.0137)	(0.0136)
Rural	-0.0142	0.00318	-0.0316*	-0.0149	0.00255	-0.0477*
	(0.0127)	(0.0183)	(0.0175)	(0.0219)	(0.0197)	(0.0259)
Religious	-0.00776	-0.0182	0.00690	-0.0111	-0.00339	-0.0129
	(0.00914)	(0.0130)	(0.0127)	(0.0159)	(0.0163)	(0.0155)
Mother's Education (Years)	-0.00125	-0.000167	-0.00268	-0.00330	-0.000918	-0.000233
	(0.00184)	(0.00275)	(0.00246)	(0.00378)	(0.00293)	(0.00301)
Father's Education (Years)	0.000759	-0.00131	0.00254	-0.00833*	-0.00159	0.00586*
	(0.00197)	(0.00297)	(0.00261)	(0.00451)	(0.00325)	(0.00302)
Medical Condition	-0.0306**	-0.0310*	-0.0285*	-0.0111	-0.0319	-0.0599**
	(0.0122)	(0.0185)	(0.0161)	(0.0197)	(0.0194)	(0.0248)
Peabody Score	0.00107	0.00242**	0.000199	0.000468	0.00139	0.00188
	(0.000725)	(0.00111)	(0.000959)	(0.00124)	(0.00127)	(0.00126)
Single Parent	0.0304*	0.0547**	0.00122	0.0321	-0.00257	0.00560
	(0.0171)	(0.0235)	(0.0243)	(0.0213)	(0.0434)	(0.0689)
ESL	0.103***	0.132***	0.0756***	0.156***	0.0934***	0.0431
	(0.0140)	(0.0198)	(0.0190)	(0.0217)	(0.0240)	(0.0278)
# Younger Siblings	0.00401	0.000509	0.00696	0.00587	-0.00419	0.00968
	(0.00674)	(0.0101)	(0.00901)	(0.0123)	(0.0109)	(0.0119)
# Older Siblings	0.0101**	0.00682	0.0132**	0.00881	0.0159**	0.00264
	(0.00424)	(0.00645)	(0.00554)	(0.00705)	(0.00747)	(0.00783)
Indigenous	0.0261	0.0859**	-0.0319	0.0376	0.00171	0.0645
	(0.0260)	(0.0379)	(0.0321)	(0.0307)	(0.0538)	(0.0744)
Log Income	-0.160	0.0261	-0.381*			
	(0.163)	(0.238)	(0.222)			
Log Income^2	0.00732	-0.000912	0.0171*			
	(0.00735)	(0.0108)	(0.00999)			
Own Home	-0.0303***	-0.0171	-0.0462***			
	(0.00990)	(0.0142)	(0.0136)			
Hyper	0.00393*	0.00481	0.00304	0.000224	0.00547	0.00624*
	(0.00203)	(0.00299)	(0.00275)	(0.00355)	(0.00347)	(0.00352)
Emotional	-0.000333	0.000381	-0.000714	0.000505	0.000834	-0.00248
	(0.00268)	(0.00389)	(0.00366)	(0.00430)	(0.00458)	(0.00496)
Sociability	0.00298	-0.000588	0.00636**	0.00289	-0.0000521	0.00749*
	(0.00239)	(0.00352)	(0.00320)	(0.00407)	(0.00416)	(0.00426)
Height	-0.000120	-0.000715	0.000597	0.000386	0.0000291	-0.000745
	(0.000379)	(0.000599)	(0.000445)	(0.000929)	(0.000609)	(0.000567)
Weight	-0.000214	0.000560	-0.00105	-0.000640	-0.000308	0.000515
	(0.000692)	(0.00106)	(0.000875)	(0.00124)	(0.00120)	(0.00122)
Constant	0.142	-1.084	1.524	-0.779***	-0.590***	-0.906***
	(0.914)	(1.334)	(1.247)	(0.187)	(0.159)	(0.165)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	4897	2403	2494	1565	1689	1643
R^2	0.465	0.511	0.428	0.511	0.435	0.460

Standard errors in parentheses

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

Table A.5: Aggregate NAPLAN Scores

Variables	Year 3 NAPLAN	Yr 5 NAPLAN
CCTR	-15.63	-5.094
	(14.86)	(13.33)
Window	-10.24	-26.20
	(20.34)	(17.34)
CCTR × Window	52.44**	50.11**
	(26.05)	(23.54)
Male	5.091	12.24
	(11.74)	(10.85)
Student to Teach Ratio	4.519**	1.754
	(2.253)	(1.953)
Independent	11.11	5.770
	(12.40)	(11.25)
Age at test	7.776***	5.055***
	(1.402)	(1.097)
Single Parent	1.580	-20.10
	(23.94)	(21.11)
Mother's Education (Years)	-1.379	2.916
	(2.688)	(2.538)
ESL	10.09	18.81
	(19.48)	(18.17)
Medical Condition	-11.43	-9.904
	(16.94)	(15.92)
Log Income	-56.15	-153.8
	(230.5)	(208.1)
Log Income^2	3.159	6.611
	(10.42)	(9.411)
Constant	1576.4	2789.8**
	(1290.5)	(1164.7)
State FE	Yes	Yes
N	3359	3280
R^2	0.017	0.013

Robust standard errors in parentheses

Controlling for individual, parental, family and school characteristics

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

Table A.6: Year 3 NAPLAN Scores by Discipline

Variables	Reading	Writing	Spelling	Grammar	Numeracy
CCTR	6.321	-9.372***	-0.990	-1.170	-11.84***
	(3.928)	(2.898)	(3.424)	(3.985)	(3.306)
Window	3.331	-7.291*	0.358	-1.398	-5.425
	(5.104)	(4.084)	(4.656)	(5.401)	(4.497)
CCTR×Window	4.925	13.52***	9.180	11.94*	13.24**
	(6.707)	(5.054)	(5.954)	(7.096)	(5.668)
Male	2.322	0.965	0.636	1.733	0.775
	(3.091)	(2.243)	(2.688)	(3.202)	(2.547)
Student to Teach Ratio	0.650	0.616	0.752	1.548**	0.923*
	(0.561)	(0.400)	(0.505)	(0.616)	(0.480)
Independent	0.731	5.517**	4.044	1.604	-1.659
	(3.264)	(2.361)	(2.827)	(3.406)	(2.661)
Age at test	2.268***	1.037***	1.015***	1.471***	1.938***
	(0.368)	(0.279)	(0.318)	(0.378)	(0.301)
Single Parent	-3.537	-2.199	2.020	3.123	2.455
	(6.303)	(4.509)	(5.341)	(6.580)	(5.304)
Mother's Education (Years)	-0.384	-0.101	-0.173	-0.384	-0.292
	(0.710)	(0.527)	(0.617)	(0.734)	(0.581)
Indigenous	13.97	13.27**	1.175	13.29	5.543
	(8.994)	(6.082)	(8.293)	(8.828)	(8.234)
Migrant	1.266	1.258	2.814	1.040	2.850
	(5.106)	(3.640)	(4.370)	(5.209)	(4.447)
Medical Condition	-2.509	1.116	-2.829	-4.127	-1.096
	(4.523)	(3.253)	(4.021)	(4.539)	(3.730)
Log Income	-1.249	-11.30	19.29	-51.48	-5.008
	(56.88)	(41.18)	(51.48)	(64.25)	(49.78)
Log Income^2	0.0925	0.615	-0.686	2.438	0.255
	(2.572)	(1.854)	(2.326)	(2.900)	(2.251)
Constant	208.5	374.0	183.8	546.3	247.7
	(318.1)	(230.9)	(287.8)	(361.1)	(280.1)
State FE	Yes	Yes	Yes	Yes	Yes
N	3408	3409	3411	3407	3399
R^2	0.018	0.015	0.009	0.012	0.023

Robust standard errors in parentheses

Controlling for individual, parental, family and school characteristics

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

Table A.7: Year 5 NAPLAN Scores by Discipline

Variables	Reading	Writing	Spelling	Grammar	Numeracy
CCTR	11.44***	-15.50***	6.185**	-0.955	-5.883*
	(3.416)	(2.824)	(3.063)	(3.597)	(3.040)
Window	-0.490	-9.114**	-3.057	-4.246	-6.420*
	(4.443)	(3.810)	(3.789)	(4.653)	(3.802)
CCTR×Window	4.419	11.44**	8.863*	8.000	14.03***
	(5.972)	(4.938)	(5.384)	(6.421)	(5.257)
Independent	0.881	2.874	3.132	1.968	-0.384
	(2.873)	(2.340)	(2.600)	(3.066)	(2.573)
Student to Teach Ratio	0.589	0.217	0.409	0.677	0.406
	(0.498)	(0.427)	(0.447)	(0.572)	(0.427)
Male	2.219	0.00580	2.074	4.573	2.121
	(2.777)	(2.251)	(2.489)	(2.946)	(2.461)
Age at Test	1.245***	0.812***	0.569**	0.997***	1.327***
	(0.281)	(0.238)	(0.252)	(0.293)	(0.248)
Single Parent	-3.040	-6.547	-3.439	-3.679	-0.162
	(5.430)	(4.504)	(5.005)	(5.847)	(4.811)
Mother's Education (Years)	0.990	0.316	0.712	0.642	0.373
	(0.663)	(0.505)	(0.582)	(0.686)	(0.579)
Indigenous	8.370	14.18*	9.132	3.535	-9.663
	(8.437)	(8.238)	(8.652)	(11.90)	(7.821)
Migrant	5.944	0.280	3.917	0.743	7.500*
	(4.619)	(3.811)	(4.073)	(4.979)	(3.955)
Medical Condition	2.088	-1.582	-4.570	-5.631	0.909
	(4.080)	(3.348)	(3.619)	(4.199)	(3.628)
Log Income	-36.60	-42.79	-5.701	-50.99	0.0127
	(52.67)	(44.50)	(46.46)	(55.43)	(48.40)
Log Income^2	1.524	1.889	0.223	2.214	0.000208
	(2.374)	(2.003)	(2.104)	(2.506)	(2.197)
Constant	558.3*	641.5**	454.7*	675.6**	351.0
	(295.8)	(251.3)	(259.6)	(310.1)	(269.4)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	3329	3314	3322	3322	3308
R^2	0.018	0.017	0.012	0.008	0.017

Robust standard errors in parentheses

Controlling for individual, parental, family and school characteristics

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

Table A.8: Kindergarten Repetition

Variables	Repeat Kindergarten
CCTR	0.00103
	(0.00551)
Window	0.0157*
	(0.00892)
CCTR × Window	-0.0197**
	(0.00943)
Age in Months (July)	-0.000856
	(0.00114)
Male	-0.000657
	(0.00454)
Mother's Education (Years)	-0.000582
	(0.00101)
Father's Education (Years)	-0.00177
	(0.00108)
Medical Condition	-0.0125**
	(0.00499)
Single Parent	-0.00761
	(0.00810)
ESL	-0.00282
	(0.00671)
# Younger Siblings	-0.00113
	(0.00310)
# Older Siblings	-0.00530**
	(0.00208)
Own Home	0.00544
	(0.00521)
Log Income	-0.00986
	(0.0824)
Log Income^2	0.000640
	(0.00375)
Hyper	0.000499
	(0.00114)
Emotional	-0.00225*
	(0.00136)
Social	-0.00200*
	(0.00118)
Height	0.000217
	(0.000176)
Weight	-0.000386
	(0.000387)
Constant	-0.001
	(0.004)
State Fixed Effects	Yes
R^2	0.036
N	4,837

Robust standard errors in parentheses

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

Controlling for individual, parental and family characteristics

Table A.9: Social and Emotional Problems Scale by parent and teacher responders (children 8-9 years old)

	Parent			Teacher		
	(1)	(2)	(3)	(4)	(5)	(6)
	Social	Hyper	Emotional	Social	Hyper	Emotional
Window	-0.0429	-0.154	-0.0731	0.0518	-0.263**	-0.0940
	(0.0801)	(0.105)	(0.0786)	(0.106)	(0.127)	(0.0827)
CCTR	0.214***	0.265***	0.252***	0.168**	0.0778	0.0221
	(0.0598)	(0.0847)	(0.0640)	(0.0815)	(0.104)	(0.0674)
CCTR × Window	0.192***	0.158	0.138*	0.231**	-0.0733	0.0938
	(0.0745)	(0.107)	(0.0812)	(0.105)	(0.135)	(0.0902)
Male	-0.000373	-0.0545	0.0656	-0.0434	-0.0348	-0.0101
	(0.0477)	(0.0683)	(0.0513)	(0.0662)	(0.0840)	(0.0545)
Indigenous	-0.160	0.256	0.0671	-0.208	0.133	0.0749
	(0.159)	(0.231)	(0.165)	(0.215)	(0.258)	(0.173)
Rural	0.0331	0.180*	0.0918	-0.0731	0.102	0.136
	(0.0740)	(0.108)	(0.0784)	(0.107)	(0.132)	(0.0890)
Mother's Education (Years)	0.0112	-0.0262	0.00152	0.00862	-0.0297	-0.0104
	(0.0113)	(0.0161)	(0.0122)	(0.0153)	(0.0198)	(0.0133)
Father's Education (Years)	0.000588	0.0302*	0.0101	-0.0389**	0.0543**	0.0135
	(0.0122)	(0.0177)	(0.0133)	(0.0167)	(0.0212)	(0.0141)
Single Parent	-0.159*	0.334**	0.230**	-0.135	0.211	-0.000541
	(0.0945)	(0.145)	(0.109)	(0.135)	(0.175)	(0.108)
ESL	-0.0222	0.0385	-0.0326	0.308***	-0.390***	-0.104
	(0.0792)	(0.112)	(0.0838)	(0.107)	(0.133)	(0.0909)
Own Home	0.00165	0.0351	-0.0594	0.0926	0.0601	-0.0172
	(0.0578)	(0.0838)	(0.0625)	(0.0820)	(0.102)	(0.0690)
# Younger Siblings	-0.0495	0.0408	0.0159	0.0159	-0.0738	-0.0311
	(0.0399)	(0.0562)	(0.0418)	(0.0552)	(0.0685)	(0.0437)
# Older Siblings	-0.00428	-0.0184	-0.0473*	0.00363	-0.0950**	-0.0522*
	(0.0252)	(0.0376)	(0.0277)	(0.0358)	(0.0447)	(0.0286)
Log Income	-1.997**	0.110	2.404***	-2.927***	2.047	0.900
	(0.831)	(1.286)	(0.888)	(1.128)	(1.477)	(0.933)
Log Income^2	0.0885**	-0.00220	-0.107***	0.134***	-0.0978	-0.0432
	(0.0375)	(0.0579)	(0.0401)	(0.0509)	(0.0663)	(0.0419)
Constant	19.88***	2.920	-11.12**	21.42***	-5.760	-2.879
	(4.571)	(7.069)	(4.841)	(6.207)	(8.118)	(5.158)
N	5109	5108	5109	4478	4478	4477
R^2	0.006	0.006	0.008	0.005	0.005	0.003

Robust standard errors in parentheses

Controlling for individual, parental and family characteristics

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

Table A.10: Binary response (teacher) if child is a bully or bullied (8-9 years old)

	(1) Bully	(2) Bullied
CCTR	-0.00988	0.00148
	(0.0111)	(0.0137)
Window	-0.0169	0.00615
	(0.0186)	(0.0231)
CCTR × Window	-0.0105	-0.0173
	(0.0185)	(0.0240)
Age in Months (July)	0.00204	0.00182
	(0.00220)	(0.00265)
Male	-0.00917	0.0109
	(0.00874)	(0.0113)
Rural	0.00577	0.00297
	(0.0140)	(0.0175)
Religious	0.00796	-0.00603
	(0.0105)	(0.0136)
Mother's Education (Years)	0.000668	0.00329
	(0.00209)	(0.00264)
Father's Education (Years)	-0.000319	0.000458
	(0.00215)	(0.00281)
Medical Condition	-0.0198	0.00919
	(0.0122)	(0.0167)
Peabody Score	-0.000578	-0.000953
	(0.000771)	(0.00109)
Single Parent	0.00243	-0.00593
	(0.0186)	(0.0232)
ESL	-0.0117	-0.0173
	(0.0151)	(0.0194)
# Younger Siblings	0.00134	-0.00860
	(0.00710)	(0.00907)
# Older Siblings	-0.00529	-0.0105*
	(0.00473)	(0.00575)
Indigenous	0.0588*	-0.00537
	(0.0352)	(0.0372)
Own Home	0.00548	-0.00995
	(0.0107)	(0.0139)
Log Income	0.169	-0.296
	(0.152)	(0.220)
Log Income^2	-0.00693	0.0131
	(0.00679)	(0.00989)
Hyper	0.00289	-0.00178
	(0.00228)	(0.00292)
Emotional	-0.0000260	0.00451
	(0.00285)	(0.00378)
Social	-0.000232	-0.00370
	(0.00272)	(0.00326)
Height	-0.00208***	-0.00225***
	(0.000752)	(0.000749)
Weight	0.00286***	0.00698***
	(0.000853)	(0.00117)
Constant	-0.810	1.839
	(0.868)	(1.243)
N	4565	4176
R^2	0.010	0.018

Robust standard errors in parentheses

Controlling for individual, parental and family characteristics

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

Table A.11: Multomial Logit Testing the change in log likelihood of childcare choice relative to early enrolment after the introduction of the CCTR

	(1)	(2)
	Formal Childcare	Informal Childcare
CCTR	0.223*	0.22
	(0.118)	(0.158)
Male	0.374***	0.639***
	(0.117)	(0.158)
Mother's Education (Years)	0.0392	0.0832**
	(0.0258)	(0.0347)
Medical Condition	0.387**	0.0923
	(0.158)	(0.219)
Peabody Score	-0.0266**	-0.0289**
	(0.0107)	(0.0145)
ESL	-1.021***	-1.193***
	(0.213)	(0.310)
Single Parent	-0.803***	0.451
	(0.237)	(0.271)
Owns Home	0.332**	0.302
	(0.136)	(0.184)
Log Income	2.734	4.714*
	(1.876)	(2.751)
Log Income^2	-0.118	-0.215*
	(0.0845)	(0.125)
Hyper	-0.0339	-0.0641
	(0.030)	(0.0408)
Emotional	0.001	0.0673
	(0.037)	(0.0476)
Social	-0.030	-0.0601
	(0.0338)	(0.0446)
Height	-0.005	-0.004
	(0.008)	(0.010)
Weight	0.008	-0.008
	(0.010)	(0.0135)
Constant	-15.18	-26.11*
	(10.45)	(15.23)
State FE	Yes	Yes
N	1587	1587
Psuedo R^2	0.0340	0.0340

Robust standard errors in parentheses

Controlling for individual, parental and family characteristics

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

Multinomial logit coefficients estimate the change in log likelihood for formal and informal childcare relative to the base case (early enrolment) after the introduction of the CCTR.

Table A.12: Effect of CCTR on early enrolment by rebate eligibility status

	(1) Ineligible	(2) Eligible
Window	0.435***	0.486***
	(0.0320)	(0.0217)
CCTR	0.00525	-0.00297
	(0.00619)	(0.00452)
CCTR× Window	-0.0586	-0.0463*
	(0.0423)	(0.0283)
Age in Months (July)	0.0118***	0.0132***
	(0.00214)	(0.00151)
Male	-0.0459***	-0.0302***
	(0.0139)	(0.00977)
Rural	-0.0113	-0.0297*
	(0.0214)	(0.0158)
Religious	-0.0228	0.00227
	(0.0164)	(0.0112)
Mother's Education (Years)	-0.00387	0.00180
	(0.00309)	(0.00227)
Father's Education (Years)	0.000829	-0.0000925
	(0.00326)	(0.00224)
Medical Condition	-0.00826	-0.0531***
	(0.0208)	(0.0151)
Peabody Score	-0.000210	-0.000000584
	(0.00128)	(0.000863)
# Younger Siblings	-0.00868	0.00379
	(0.0121)	(0.00801)
# Older Siblings	0.000514	0.0122**
	(0.00732)	(0.00524)
Indigenous	0.0895*	-0.0141
	(0.0512)	(0.0301)
ESL	0.0913***	0.105***
	(0.0254)	(0.0167)
Own Home	-0.0228	-0.0378***
	(0.0176)	(0.0117)
Log Income	-0.346	-0.251
	(0.319)	(0.176)
Log Income^2	0.0155	0.0108
	(0.0145)	(0.00799)
Height	-0.000450	0.0000129
	(0.000525)	(0.000541)
Weight	0.00114	-0.000906
	(0.00121)	(0.000857)
Hyper	0.00969***	0.000633
	(0.00357)	(0.00247)
Emotional	-0.00184	0.00310
	(0.00465)	(0.00328)
Sociability	0.00849**	-0.0000984
	(0.00415)	(0.00294)
Constant	0.427	-0.116
	(1.908)	(1.017)
State FE	Yes	Yes
N	1627	3328
R^2	0.422	0.489

Robust standard errors in parentheses

Controlling for individual, parental and family characteristics

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

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