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IZA DP No. 10525

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ABSTRACT

Bonus Skills: Examining the Effect of an Unconditional Cash Transfer on Child Human Capital Formation*

This paper evaluates the impact of the Australian Baby Bonus – a $3000 one-off cash transfer – on various aspects of child human capital development. Using high-quality longitudinal cohort data and difference-in-difference models, we compare the outcomes of cohort members whose younger sibling was born marginally on either side of July 1, 2004, when the Baby Bonus was introduced. Our results suggest that the Baby Bonus was not effective in boosting learning, socio-emotional or physical health outcomes of the average pre-school child. This finding is strengthened by the observation that the Baby Bonus did not impact parental well-being, parental behavior and labor supply, the potential mechanisms via which the cash transfer could have affected human capital formation. The muted effect for the Baby Bonus in comparison to significant effects for similar cash handouts in other countries may be explained by its non-targeted and one-off structure. We conclude that the large financial cost of $3000 per child is not justified as an intervention for the entire population to boost children's skills.

JEL Classification: J24, H53

Keywords: cash transfers, Baby Bonus, cognitive and non-cognitive skills, health, LSAC

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NON-TECHNICAL SUMMARY

Childhood cognitive skills (e.g. intelligence) and non-cognitive skills (e.g. personality) are known to affect subsequent educational attainment, labour-market productivity, savings behaviours, and health outcomes and behaviours. Therefore, a wealth of research has focused on identifying factors that promote the early development of these skills (e.g. family income and parental time investments). Compared to some other factors, little is known about how governmental cash transfers affect the skill development of young children. Income determines the choices households can make regarding whether or not, and how much, to invest in goods and services that foment child development. Therefore, income-support payments may be expected to accelerate children's skill development.

In this study, we examine the effect of the Australian Baby Bonus (a conditional government cash handout of $3,000 introduced on July 1, 2004) on the learning, socio-emotional and physical outcomes of Australian children. Our results suggest that the Baby Bonus was not effective in boosting the learning, socio-emotional or physical health outcomes of pre-school children in Australia. The Baby Bonus also failed to impact parental well-being, parental behaviour and labour supply, which are potential mechanisms via which the cash transfer could have affected children's human capital formation.

Our interpretation is that the income provided by the Baby Bonus was insufficient to help families overcome the 'shock' of the birth of a new-born sibling. Our findings contrast with those for other countries (e.g. the United States and Mexico), in which income-support schemes delivered to new parents had effects on both cognitive and non-cognitive child outcomes. These differences may emerge due to the fact that the Australian Baby Bonus was universal (i.e. it was given to all households conditional on the birth of a new child). In contrast, similar income-support schemes in other countries were targeted at specific sub-populations (e.g. low-income households). In addition to it being non-targeted, the muted effects of the Baby Bonus may also result from its one-off structure.

These findings have important policy implications. They suggest that cash interventions given to the entire population to improve child outcomes and to offset the financial burden of childbirth are at best inefficient and at worst ineffective. The large financial cost of $3,000 per child to the Government budget is not justified as an intervention for the entire population if the goal is to boost children's skills. Government expenditure should either be directed where the marginal return to income is highest (i.e. to the most disadvantaged families) and towards childcare or other services which have a measurable and direct impact on child outcomes.

1 Introduction

The formation of cognitive and non-cognitive skills from early childhood to adolescence crucially determines the long-run productivity of the economy (Fiorini & Keane 2014, Cunha & Heckman 2007). Empirical studies in the field of human capital development have shown that childhood cognitive and non-cognitive skills affect educational attainment, labour-market productivity, savings behaviours, later-life health outcomes and health behaviours (Heckman et al. 2006, Cunha & Heckman 2007, Dahl & Lochner 2012). Although a large literature exists on a variety of determinants of early-life human capital development, there is relatively little known about whether governmental cash transfers to families can causally impact the skill development of young children. In this study we, examine the effect of the Australian Baby Bonus - a conditional government cash handout of $3000 introduced on July 1, 2004 shortly after its announcement in the Australian budget - on the learning, socio-emotional and physical outcomes of an Australian millennium birth cohort.

Our study contributes to the literature and policy debate in three important ways. First, we evaluate the secondary outcomes of the Baby Bonus, which was introduced primarily to boost fertility in Australia. Parr & Guest (2011) and McDonald (2006*b*) suggested that pronatalist policies are justified not because of their 'demographic' effects on fertility, but because of their 'non-demographic' effects of improving equity and alleviating the financial pressures associated with childbirth. Therefore, the broader impacts of the Baby Bonus have clear implications for the design and implementation of future redistribution policies. Second, we contribute to the debate on the effectiveness of cash transfers in reducing economic disadvantage, a question of increasing concern for school systems, state governments and non-governmental organisations such as the OECD (Kautz et al. 2014). In 2014, an estimated 602,604 Australians under the age of 18, equivalent to over one-sixth of all children, were living below the poverty line (ACOSS Poverty in Australia Report, 2014).[1] Indeed, high variation in family income has been linked to variation in children's skills by age five (Fletcher & Wolfe 2016). The capacity of economic disadvantage to limit a child's developmental potential

[1]The estimates provided by ACOSS are based on the OECD threshold of $400 per week.

provides both a social and economic rationale for government intervention to protect children through income support (Khanam & Nghiem 2016, Dahl & Lochner 2012). Hence, we are interested in the extent to which the Baby Bonus can improve children's human capital by reducing financial disadvantage.

Finally, we contribute to a small number of studies that evaluate cash transfers implemented in other countries that are of similar scope, structure and magnitude to the Australian Baby Bonus. These studies find tentative evidence that cash transfers could boost children's skills. Dahl & Lochner (2012) find that an exogenous $3000 increase in family income through the Earned Income Tax Credit improved reading and math achievements of children in the US by one-tenth and one-twentieth of a standard deviation, respectively. Analyzing sixteen welfare-to-work payments in US cities in the 1990s, Duncan et al. (2011) estimate that a $1000 increase in family income boosted early childhood achievement by 5-6% of a standard deviation. Akee et al. (2015) estimate that an annual $4000 payment given by a casino to adult members of the Eastern Cherokee tribe had substantial effects on child non-cognitive outcomes and that the gains were the most significant for children with behavioral and emotional problems. Finally, Mullins (2016), evaluating the effectiveness of several anti-poverty programs in the US, finds that an extra $1000 transferred to the poorest 10% of all families had a significant impact on test scores and high school graduation rates. Our paper builds on these studies; however, the Baby Bonus differed from these interventions because it was conditional on the birth of a child. Hence, our estimates are interpreted not as the direct effect of increased household financial resources on child development, but rather as the impact of the specific policy of the Baby Bonus on child development. Despite this, our study maintains significant policy-relevance, especially since 'Baby Bonuses' of various forms have recently been introduced in other developed countries, such as Singapore, Russia and the Czech Republic.

Our study uses high-quality panel data on children from the Kindergarten cohort (K-Cohort) of the Longitudinal Study of Australian Children (LSAC) in combination with a difference-in-difference estimation method to identify the causal effect of the Baby Bonus.

We track the development of 516 children from ages 4-5 in 2004 to ages 12-13 in 2012, some of whom received an exogenous $3000 cash transfer because of the birth of a new sibling after July 1, 2004 (treatment group), while others did not receive the cash transfer because the birth of a new sibling occurred before the cut-off date (control group). Hence, the treatment effect will be identified for children in families with at least two, but maximal three children, under the assumption that the treatment and control groups differ in no relevant ways pre-treatment other than by the birth date of the new sibling. While larger sample sizes could be achieved with administrative data – e.g. measured by school achievement test scores, a strategy used in Deutscher & Breunig (2016) to evaluate the long-term impact of the Baby Bonus on cognitive skills – LSAC data allows us to explore a greater variety of outcomes that measure child wellbeing and comprehensively understand the potential mechanisms via which the Baby Bonus affected child development.

We find that, at best, the Baby Bonus had a modest positive effect on cognitive skills and at worst, a negative effect on socio-emotional skills and physical health. However, the model is inefficiently estimated, and hence there is no convincing evidence that the Baby Bonus had a significant effect on cognitive skills in the aggregate. The positive treatment effect of the Baby Bonus on cognitive skills is potentially explained by a simultaneous increase in other welfare payments that were introduced around the same time as the Baby Bonus, and which cannot easily be separated from the Baby Bonus in our data. The negative treatment effect of the Baby Bonus on socio-emotional skills is mainly observed in the context of peer problems and driven by a significant effect for boys. One explanation that is consistent with this finding is based on the observation that for the treatment group, the arrival of a new sibling was slightly more recent than in the control group. It could be the case that boys react more sensitively than girls to new siblings who take away resources from the parents. Although we control for the age difference between the study child and the newborn sibling in our estimation model, we cannot rule out for certain this alternative channel. Further, the negative and significant effect of the Baby Bonus on physical health outcomes is driven by the subcomponent of parental assessments of their child's health. This means that it is theoretically possibly that a negative treatment effect is not explained by a real negative effect on the child's health, but by a change in standards according to which parents perceive the health of their child.

Interpreting our findings conservatively – concluding that the Baby Bonus did not boost children's skills – is supported by the finding that it did not change parental behaviors and well-being, the likely causal channels via which cash transfers can affect child outcomes. Our interpretation of the findings is also consistent with the findings of no positive treatment effects of the Baby Bonus on school achievement (Deutscher & Breunig 2016). We conclude that the large financial cost of $3000 per child is not justified as an intervention for the entire population to boost child skills. In addition, the muted effect for the Baby Bonus in comparison to significant effects for similar cash handouts in other countries may be explained by its non-targeted and one-off structure.

The remainder of the paper proceeds as follows. Section 2 briefly outlines the theoretical background and the related literature. Section 3 gives an overview of the Australian Baby Bonus and other statutory schemes aimed at offsetting the costs associated with bearing a child. Section 4 describes the data, outlines the sample selection method and provides pre-treatment summary statistics. Section 5 outlines the empirical strategy used to estimate the relationship between the Baby Bonus and the production of skills. Section 6 presents the results for the effect of the Baby Bonus, a series of robustness checks, an analysis of the possible mechanisms and an analysis of heterogeneous treatment effects. Section 7 concludes and an appendix presents supplementary material.

2 Related Literature

Does income play a role in the way children's skills are being fostered? Income determines the choices households can make to invest in their children in terms of material goods and opportunities to learn. The literature examining the relationship between family income and child development has systematically found positive associations between income and skill development. Also using LSAC data, Khanam & Nghiem (2016) find that family income is significantly associated with cognitive skills, although not with non-cognitive skills, for children in the Kindergarten cohort, while studies based on data from other countries find that income is positively associated with both cognitive and non-cognitive skills (Fletcher & Wolfe 2016, Deckers et al. 2015, Delaney & Doyle 2012). These studies are based on models that deal with the possible endogeneity in income by either conditioning on a large set of control variables including past skill measures or differencing out unobservable, time-invariant components through the use of fixed effects estimation methods. Studies which use quasi-experimental research designs to identify the causal effect of income on learning and socio-emotional outcomes also find a positive impact of family income on children's skills (Akee et al. 2015, Dahl & Lochner 2012, Duncan et al. 2011), especially for children from disadvantaged backgrounds (Adhvaryu et al. 2015).

There are many different ways via which additional income may impact upon child development directly and indirectly. Dahl & Lochner (2012) suggest that the direct mechanism is that cash transfers allow families to purchase educational activities, while the indirect mechanisms are that cash transfers impact parental time with children, maternal labour supply and parental well-being. McLoyd (1990) suggests that poverty is positively correlated with poor health outcomes, high levels of depression and parental stress, and hence welfare payments may be effective in relieving these constraints. Similarly, Mullins (2016) finds that welfare payments significantly improve parental welfare, the stability of the parental relationship and assist mothers in a smooth return to work. Fiorini & Keane (2014) and Bono et al. (2015) find that parental time investments impact positively on cognitive and non-cognitive skill development.

Recent theoretical work on the skill production process has emphasized the importance of dynamic complementarity as a mechanism that explains how small early-life investments can impact on adult outcomes. Dynamic complementarity means that monetary or parental investments are more productive for children at higher levels of initial skill endowments (Cunha & Heckman 2007). One of the few empirical papers evaluating this concept is Aizer &

Cunha (2012), which exploits exogenous variation generated by the launch of the Head Start program in 1966. They find evidence that initial human capital and parental investments are complements rather than substitutes in the human capital production function. Similarly, Attanasio et al. (2015) find that both material and time investments are significantly more productive for children, aged between 12-24 months, with higher initial levels of cognitive and non-cognitive skills.

To the best of our knowledge, this is the first study to provide an empirical analysis of the effect of the Australian Baby Bonus on both cognitive and non-cognitive skill development of children. One exception is Deutscher & Breunig (2016) who examine the effect of the Baby Bonus on school achievement using Australian administrative data of Year 3 (ages 8-9) scholastic achievement scores (NAPLAN). Deutscher & Breunig (2016) focuses on school achievement of children born as a result of the Baby Bonus, while our focuses on a broad range of developmental outcomes of children who benefited from the Baby Bonus because they had a sibling born after the introduction of the Baby Bonus. Previous research, which we will review in the next section, on the Baby Bonus has focused on its impact on fertility only and neglected its impact on child and parental outcomes, which is fundamental for policy analysis.

3 The Institutional Setting of the Baby Bonus

In May 12 2004, the Australian Government announced the introduction of a universal cash payment, the so-called Baby Bonus, in its Federal budget, that was to be implemented from 1 July 2004 onward. It was designed as a non-means tested and non-taxable lump-sum payment of $3000 to encourage family formation, paid to parents following the birth or the adoption of a child after July 1, 2004, regardless of family income, maternal employment status or the number of children in the household. This upfront payment is equivalent to four weekly average disposable household income payments, or 10 weekly disposable income payments for individuals in the lowest income quintile in 2004. Thus, the Baby Bonus payment constitutes a relatively large windfall income especially for low income households.[2]

The amount of the Baby Bonus subsequently rose to $4000 on July 1, 2006 and to $5000 on July 1, 2008. The Baby Bonus underwent major structural changes since January 1, 2009, whereby families could only receive the Baby Bonus if the total family income equalled $75,000 or less in the first 6 months following the birth of the child, and the payment was organized in 12 fortnightly installments starting from birth of the child. Furthermore, from January 2011, a paid parental leave scheme was introduced, providing more generous support to working parents of up to 18 weeks' pay at the minimum wage. The Baby Bonus was abolished entirely on March 1, 2014 (see Sinclair et al. 2012, for a review of the Baby Bonus implementation).

Because of the short time frame between announcement and implementation, the Australian Baby Bonus presents a natural experiment in which families could not change their behavior in response to the Baby Bonus in the initial stage of its implementation up until March 2005. Up until March 2005, the Baby Bonus could be considered as good as a random change in family income. However, as the Baby Bonus targeted fertility by easing the (perceived) costs of raising a child through an up-front lump-sum payment, it may have increased family size in the short to medium run. Indeed, Drago et al. (2011) estimate that through an increase in fertility intentions, there was a 0.7% increase in realized fertility in the first year of the Baby Bonus. Sinclair et al. (2012) show that 0.43% of births (equivalent to approximately 1100 of the 259,800 births in that year[3]) that occurred between March 2005 and January 2006, were directly attributable to the Baby Bonus, indicating that families quickly responded to the financial incentive, although there was no increase in fertility from January 2006 to July 2006. Gans & Leigh (2009) furthermore found an immediate effect of the Baby Bonus, showing a significant spike in births just on July 1, 2004, which suggests that some families who received the Baby Bonus received it through the effort to delay child

[2]Equivalized disposable household income in 2003-2004 was $746 for all persons and $298 for persons in the lowest income quintile. Data taken from the Australian Bureau of Statistics, Household, Income and Wealth 2013-2014.

[3]The number of annual births is reported by the Australian Bureau of Statistics (ABS).

birth through Cesarian section by at least one day.[4]

The introduction of the Baby Bonus also coincided with some important changes in other welfare payments. On the one hand, the Baby Bonus replaced two birth subsidy programs that were in effect before July 1, 2004. One provided women with modest labour-market incomes and who were eligible for Family Tax Benefit payments with a Maternity Allowance of $842.64 per child. The other made some families eligible for the so-called 'First Child Tax Refund' administered by the Australian Tax Office, which granted a tax refund of up to $12,500 over a period of five years (Drago et al. 2011). However, this latter payment had very low utilization rates because it operated as a 'delayed and complicated tax rebate system' (Gans & Leigh 2009, Drago et al. 2011). In addition, most families were not able to claim close to the maximum amount, as the largest payments were accrued to very high-income women that remained out of the workforce for five years. Despite replacing these payments, McDonald (2006*a*) suggests that pronatalist policies like the Baby Bonus may still have an effect because of their directness, certainty, simplicity and immediacy of payment. On the other hand, two additional welfare payments were introduced ex-post implementation of the Baby Bonus. First, on January 1, 2005, the Family Tax Benefit Part B Supplement was introduced as a payment of up to $302.95, which was given at the end of the financial year to households where the primary income earner had an annual income of $100,000 or less. Second, from July 1, 2005, households that placed their child in an approved child care center may have been eligible for the Child Care Rebate (maximum of $4000).

We use the introduction of the Baby Bonus on July 1, 2004 as exogenous variation in family income. Our data which we will describe below allows us to study the medium-term effect of such exogenous increase on children's outcomes for families in which a new-born sibling triggered the Baby Bonus. Hence, we are not studying the impact of the Baby Bonus on the child that triggered the Baby Bonus, but the older sibling of the child which triggered the Baby Bonus. We compare the outcomes of older siblings of the children born just before the threshold (treatment group) with older siblings of the children born just after

[4]Sinclair et al. (2012) also estimates that , which is the end of our sample.

the threshold. Comparing children whose youngest sibling was born just on either side of the threshold of July 1, 2004, exploits an exogenous discontinuity in family household income (where the running variable is birth days). The natural experiment of the Baby Bonus is valid under the assumption that treatment and control group children are similar across a range of important observable and unobservable characteristics. We will discuss in the empirical methods section the details of our specification (Section 5) and the various robustness checks we conduct to shut off all potential concerns over the endogeneity of the Baby Bonus (Section 6.1)

4 Data and Descriptive Statistics

For the analysis, we use five waves of data from the Longitudinal Study of Australian Children (LSAC), which is a biennial representative panel survey of Australian children. LSAC data is collected predominantly through face-to-face interviews with the prime caretaker (Soloff et al. 2005). Response rates of the survey participants in each wave are high (79-90%) and comparable to other recent cohort studies such as the UK Millennium Cohort Study. We use the Kindergarten (K) Cohort, a nationally representative sample of 4983, 4-5 year old children born between March 1999 and February 2000. The respondents have been surveyed biennially since 2003-2004, collecting rich data on their cognitive skills, non-cognitive skills, health, demographic characteristics, school environment, parental background and parental behavior. The detailed panel data thereby allows us to track the evolution of child skills in response to the Baby Bonus from age 4-5 to age 12-13. We exclude the Birth (B) Cohort members from our analysis because their birth window of March 2003 to February 2004 leaves no variation in individual receipt of the Baby Bonus. Instead, there is sufficient variation of Baby Bonus receipt across K-cohort families.[5]

[5]Even if there was variation in the Baby Bonus for the B-Cohort members, it would be difficult to analyze child development for this group. Because early-life childhood measures of cognitive skills and non-cognitive skills are inconsistent across waves, we would introduce measurement error bias of an unknown magnitude and sign in the difference-in-difference estimation.

4.1 Outcome Measures

As measures of human capital development, we use the child's learning, socio-emotional and physical outcomes, which are standard and validated measures that have been used in the literature to evaluate the impact of parenting (Fiorini & Keane 2014), income (Khanam & Nghiem 2016), or childhood obesity (Black & Kassenboehmer 2017). Cognitive skills are measured by the Learning Outcomes Index, which is a composite measure appropriate for the developmental age of the child. The Learning Outcomes Index consists of measures of the child's language skills, literacy skills, numeracy skills and approach to learning. Language skills are assessed using the Peabody Picture Vocabulary Test (PPVT), an interview-administered test where the respondent is required to choose a picture that corresponds to the meaning of a word. Literacy skills - assessed by both parents and teachers - measure the child's capability in reading complex texts as well as the child's interest in reading. Measures of numeracy skills are provided by a teacher rating on a scale of 1 to 5 of the child's ability in counting, simple addition, classifying numbers and recognizing numbers. Finally, approach to learning is assessed through the 'Who Am I?' (WAI) standardized test that evaluates the child's reading, writing, symbol recognition and copying ability. We also analyze cognitive outcomes in the later stages of childhood by using the NAPLAN (reading, writing, language and numeracy) standardized test scores, for which we have available data for students in Years 3, 5 and 7.

Non-cognitive skills are proxied by the Socio-Emotional Index, which is derived from five dimensions of the Strengths and Difficulties Questionnaire (SDQ). We use parent-assessed rather than teacher-assessed SDQ measures due to a substantial amount of missing data in the teacher-assessed SDQ. The five domains of the SDQ are pro-sociality, peer problems, emotional symptoms, hyperactivity and conduct problems. Pro-sociality assesses the child's propensity to behave considerately towards others; peer problems measures the child's ability to form relationships with other children; emotional symptoms evaluate the frequency that the child displays negative emotions; hyperactivity indicates the child's impulsiveness and attention span; and conduct problems assess whether the child exhibits problematic behavior in their interactions with others.

Physical outcomes are proxied by the Physical Outcomes Index, which captures health and motor skills. Child health is determined using a composite measure that consists of (1) a parental assessment of the child's health, (2) an indicator for whether the child had any special health care needs and (3) the child's Body Mass Index. Motor skills are assessed by the parent, based on the Paediatric Quality of Life Physical Health sub-scale. For all outcome measures, larger values indicate stronger outcomes and each outcome measure has been standardised to mean 0 and standard deviation 1.

4.2 Sample

To assess the impact of the introduction of the Baby Bonus on the production of skills, we compare outcomes for a treatment group of children whose younger sibling triggered the $3000 Baby Bonus to a control group of children whose younger sibling did not trigger the Baby Bonus. Both the treatment group and control group consist of K-Cohort children born between March 1999 and February 2000 with at least one sibling born between July 2002 and June 2006 and a maximum of one additional sibling born on any date. The ideal experiment would involve comparing the outcomes of a large sample of children with siblings born on June 30, 2004 to a large sample of children with siblings born on July 1, 2004. However, in our analysis we extend the birth window of the treatment and control groups to up to two years due to concerns over small sample sizes. Therefore, the treatment group consists of children whose youngest sibling was born between July 1, 2004 and June 30, 2006, and thus were eligible for the $3000 Baby Bonus payment. By contrast, the control group consists of children whose youngest sibling was born between July 1, 2002 and June 30, 2004, and hence were not eligible for the Baby Bonus.

Pre-treatment data is obtained from wave 1, while post-treatment data is obtained by wave 3. We have excluded six individuals that were already treated by wave one due to the unavailability of pre-treatment data. Using this sample selection, there are 247 children in the treatment group and 269 children in the control group; however, in our regressions a small number of observations are lost due to missing values in each of the three skill measures. While the number of observations is small relative to descriptive studies that used administrative data sets, it is large relative to various randomized experiments based on the early childhood Perry and Abecedarian programs (Magnuson & Duncan 2016). A further explanation of our sample selection process is detailed in the Online Appendix A2. We deal with the potential of systematic attrition separately in a robustness check.

4.3 Summary Statistics

Table 1 presents pre-treatment (wave 1) summary statistics of the relevant outcome and control variables for both the treatment group and the control group. Given that treatment is randomized conditional on the birth date of the youngest sibling, we expect the treatment group and control group to be similar across a range of observable characteristics, but they should differ in expected ways across some demographic characteristics related to the sibling's birth date. Table 1 suggests that the two groups are indeed similar over demographic characteristics that are independent of the birth date of the study child's youngest sibling. These characteristics include family income, parental education, parental weekly work hours, gender, age of the study child, ethnicity, birth weight and whether both biological parents are present in the household.

By contrast, the two groups are statistically different over the demographic characteristics that are not independent of the birth date of the study child's youngest sibling. First, children in the control group have 0.27 more siblings on average because, in the baseline, families in the treatment group have not yet had the child that allows them to receive the Baby Bonus. Second, mothers in the control group are about 1.3 year older. Third, relative to the control group, cohort members in the treatment group have a larger age difference between themselves and their younger sibling because we have constructed our sample such that their younger sibling is born after July 1, 2004. Finally, children in the treatment group are 13 percentage points less likely to be the first-born child.

Since these variables are fixed at the time that treatment status is determined, we control for them in our regression analysis to eliminate any bias (Angrist & Pischke 2008). However, the number of siblings may be considered an outcome variable in our analysis and hence we only control for the baseline (wave 1) level of siblings. In a robustness check, we test whether our results are sensitive to excluding study children that are not first-born and to modeling a non-linear age difference relationship between the study child and the youngest sibling.

Furthermore, Panels B, C and D of Table 1 demonstrate that the treatment and control groups are similar across all baseline measures of parenting style, parental investment, family stress and neighborhood characteristics with one exception. Children in the treatment group live in a geographic region where a significantly lower percentage of residents have completed secondary education (p-value = 0.005). Hence, we control for this variable in our econometric model.

Although the LSAC data is a nationally-representative, random sample of 4983 children aged 4-5, our estimation sample is only a subset of the representative sample. Hence, we test the external validity of our results by comparing the baseline summary statistics of the selected sample members with the characteristics of all K-Cohort members (see Table A1). While the selected sample and the entire K-Cohort appear to be similar across several measures of parental investment, family stress, family health and neighborhood characteristics with a couple of exceptions, there are significant demographic differences between the two samples. The most important of which are that children in our sample are much more likely to be firstborn, have higher family incomes, higher parental parental education and a smaller number of siblings in the baseline. These differences likely arise because children in our selected sample have a younger sibling born between July 2002 and June 2006, while the entire sample of K-Cohort children do not necessarily have a sibling born in this birth window or even a sibling at all. Therefore, the external validity of our results is constrained to identifying the effect for 4-5 year old children that receive an exogenous cash handout as a result of the birth of a younger sibling.

5 Empirical Strategy

We have so far shown that the Baby Bonus was largely successful in balancing observable characteristics between the treatment and control groups, and thus can be considered as a quasi-natural experiment that we can exploit for causal analysis. We start our empirical analysis with the widely employed value-added model, which controls for relevant observable differences between treatment and control groups in addition to the lagged outcome which acts as a catch-all proxy for ability and unobservable past inputs (Fiorini & Keane 2014, Todd & Wolpin 2003).[6] To overcome concerns created by missing data in LSAC, we estimate the most parsimonious model possible:[7]

$$h_{it} = \beta_0 + \beta_1 h_{i,t-1} + \beta_2 Bonus_i + \beta_3 X_i' + \epsilon_{it}, \tag{1}$$

where h_{it} is the learning, socio-emotional or physical outcome of child i in wave t, $h_{i,t-1}$ is the one-period lagged value of this outcome and $Bonus_i$ is a binary indicator variable that takes the value 1 if the family received the \$3000 Baby Bonus, and 0 otherwise. We test under the null hypothesis that the Baby Bonus had no effect ($\beta_2 = 0$) against the alternative hypothesis is that the Baby Bonus had either a positive or negative effect ($\beta_2 \neq 0$). X_i' is a vector of time-invariant control variables, which include the observed pre-treatment differences between the treatment and control groups related to family composition. In addition, we include the study child's age as a covariate to control for variation in SDQ measures across waves and because children are assessed at different ages. We also control for the study child's gender to account for systematic developmental differences between boys and girls at young ages.

While the value-added regression model allows us to control for observable characteristics that are associated with treatment, the coefficient estimates will likely be biased because

[6]We control for the lagged dependent variable because this is the predominant model in the literature. However, it is arguably not necessary in this setting because we are exploiting a source of exogenous variation where the previous literature does not. Regardless, as we will see, our estimates of the treatment effect are not sensitive to the inclusion of this lagged outcome variable, rather it only affects the precision and explanatory power of our regressions.

[7]We are only comparing outcomes in these value-added regressions and hence we only run these regressions on post-treatment wave 3 data.

they do not control for unobservable characteristics that are associated with treatment. In particular, there may be unobservable time-invariant characteristics related to the observed age differences between the study child and their youngest sibling that cause different developmental outcomes between the treatment and control groups. Therefore, we use as our main empirical strategy a difference-in-difference model that accounts for both differences in the baseline level of skills, as well as any time-invariant, unobserved heterogeneity in the receipt of the Baby Bonus:[8]

$$\Delta h_{it} = \beta_0 + \beta_1 Bonus_i + \beta_2 X_i' + \epsilon_{it}, \tag{2}$$

where Δh_{it} is the change in individual i's outcome from wave 1, measured in 2004 when the child is aged 4-5, to wave three, measured in 2008 when the child is aged 8-9. The same set of control variables are used as in the value-added specification.

Equation 2 differences out an individual-specific fixed effect, which allows β_1 to capture the causal effect of the Baby Bonus on child outcomes if the following assumptions hold. First, the treatment and control groups must be on the same trend in the absence of treatment. We cannot test for this empirically because we do not have two waves of pre-treatment data and because measures of child outcomes in early childhood (0-3) are possibly too noisy and inconsistently measured for a reliable trend analysis. However, this is a reasonable assumption on a priori grounds because children in the treatment and control groups are of the same age, subject to the same economic conditions and there is no statistical difference in their initial skill levels.

Second, we require that there were no other contemporaneous shocks to the treatment group aside from treatment. Children in the treatment and control groups are subject to the same economic environment; hence any economic shocks will affect the treatment and controls groups simultaneously and thus will not violate this assumption. However, this assumption

[8]We employ a standard specification predominantly used in the applied health and labor economics literature. See Angrist & Pischke (2008) for an overview of these difference-in-difference specifications. Note, because we use only two years of data, we are not likely to encounter the problem of serially correlated errors and thus do not need to adjust our standard errors (Bertrand et al. 2004).

could be problematic if there are other child care benefits that are unique to the treatment group, such as the Family Tax Benefit (FTB) Part B Supplement and the Child Care Rebate. Nevertheless, we argue that the FTB Part B Supplement may not dramatically confound the effects of the Baby Bonus because it only increased household income by up to $302.95. The Child Care Rebate, introduced on July 1, 2005, presents more severe complications because it was large in magnitude (up to $4000) and disproportionately available to high-income individuals. We discuss the implications of the Child Care Rebate, as well as a variety of other government payments, for our results in Section 6. Third, we assume that the policy change of the Baby Bonus is as good as random. This assumption is likely to hold because the Baby Bonus was an exogenous and unconditional cash transfer.

6 Results

In this section we present the estimation results on the effect of the Baby Bonus on various outcomes of child development. Table 2 presents estimated coefficients from an OLS model with and without control variables (column 1 and 2), a value-added model with control variables (column 3), and a difference-in-difference model with and without control variables (column 4 and 5).[9] Our preferred specification is the difference-in-difference model with control variables, because it is the least restrictive with respect to modelling assumptions. We consider statistical significance levels of up to 10%, and consider effects as statistically insignificant at higher levels.

The estimation results presented in Panel A suggest that the Baby Bonus had a positive, but statistically insignificant, effect on learning outcomes. In our preferred difference-in-difference model with a full set of controls, we estimate an insignificant treatment effect of 0.26 standard deviations (hereafter, σ) (p-value = 0.15 for a two-tailed test). Our findings are consistent with Khanam & Nghiem (2016) who suggest that raising family income by 1σ

[9] Across all specifications with the exception of the value-added model, the R^2 values are very low. This itself does not present a concern to our empirical methodology because our study is focused on estimating a causal effect rather than prediction.

(equivalent to approximately $70,000) is associated with a statistically significant increase in child cognitive outcomes of approximately $0.2\sigma - 0.3\sigma$ using the same data but a different estimation strategy. Given that the Baby Bonus represents only a fraction of the income increase considered in Khanam & Nghiem (2016), it is not surprising that we find a statistically insignificant impact on learning outcomes.

Further, a statistically insignificant effect of the Baby Bonus is consistent with previous empirical research that considered the Baby Bonus insignificant relative to the life-time costs of bearing a child (Parr & Guest 2011, Lattimore & Pobke 2008, Breusch et al. 2004). One explanation for the insignificant result is that there is a 'labelling effect' of the cash transfer that causes household spending to be directed towards the newborn rather than the K-Cohort member. In this respect, the findings in Deutscher & Breunig (2016) complement our results, as they find no evidence that the Baby Bonus improved Year 3 NAPLAN test scores of children whose birth triggered the Baby Bonus payments.

Notably, we observe a large increase in the treatment effect when adding control variables, which is largely driven by the variable controlling for the age difference between the study child and their sibling. This is caused by the negative correlation between learning outcomes and the 'age difference' variable, combined with the systematically larger age difference for treated children relative to untreated children.

It is difficult to make a like-for-like comparisons between the magnitude of our effect size in this study relative to a broader literature due to the small number of quasi-experimental studies and differences in both methodologies and the number of observations between studies. One the one hand, the learning outcomes effect size appears large relative to other studies that used quasi-natural experiments such as Dahl & Lochner (2012) and Adhvaryu et al. (2015), however these studies use instrumental variable approaches and larger sample sizes. On the other hand, even if the effect had been significant, the effect size represents the lower bound of possible treatment effects found in comparable studies that also use strict birthday cut-offs to evaluate the impact of early child interventions. For example, Gormley Jr et al. (2005) evaluated the Tulsa pre-kindergarten program and estimated effect sizes of 0.38-0.79σ, while Wong et al. (2008) estimate intent-to-treat effects ranging from 0.17-0.68σ. Overall, Magnuson & Duncan (2016) survey of the literature suggests an average effect size of 0.35σ for early childhood interventions on cognitive outcomes, and hence even if an effect existed for our study, it is relatively small in magnitude. In addition, studies with fewer observations – hence are more directly comparable to our study – tended to have more sizeable effect sizes.

Turning to socio-emotional outcomes (Panel B), we consistently estimate across all model specifications a negative impact of the Baby Bonus. However the treatment effect is never statistically different from zero at the 10% level or better, with the exception of the OLS model without control variables. Both value-added model and difference-in-difference model suggests that the Baby Bonus decreased socio-emotional outcomes by 0.23σ (p-values 0.19 and 0.20, respectively). Our finding of no positive impact of the Baby Bonus on socio-emotional skills is consistent with Khanam & Nghiem (2016) who find no statistically significant association between family income and socio-emotional skills.

One explanation that is consistent with the negative treatment effects of the Baby Bonus on socio-emotional outcomes is that study children in the treatment group have experienced the birth of a sibling marginally more recently relative to study children in the control group. It could be the case that the arrival of a new sibling diverts parental time and financial resources away from the study child, or otherwise that simply the presence of a newborn in the household causes the study child in the treatment group to act with more aggression, irritability or impulsivity relative to cohort members in the control group Minnett et al. (1983). While our econometric model controls for the age difference between the study child and their sibling that triggered the Baby Bonus, it is possible that unobserved factors related to this age difference variable or other time varying shocks to families are biasing our estimates downwards.

Estimation results for physical outcomes (Panel C) suggest that the Baby Bonus was significantly associated with worse physical outcomes. Estimates obtained from the value-added model yield a negative treatment effect in the magnitude of 0.31σ (p-value = 0.13), while the difference-in-difference model with controls implies a large negative treatment effect of 0.69σ (p-value = 0.001). This result is striking, and hence in Table 3 we investigate it further by analyzing the individual subcomponents of physical outcomes. The results indicate that the negative treatment effect on physical outcomes is largely driven by a negative effect of the Baby Bonus on a parent-assessed rating of the child's health, rather than the child's obesity or motor skills.

The above specified models consider only a relatively short time window in which the Baby Bonus could have had an impact on child development. It is possible however that the cash transfer had a significant longer-term impact because early childhood interventions can impact the evolution of skills over the life-course (Cunha & Heckman 2007, Attanasio et al. 2015, Mullins 2016). To assess longer-term cognitive outcomes, we use NAPLAN standardized test scores that were collected by schools when the study child was in Year 3, 5 and 7, respectively. Because of our data setting, we have no NAPLAN data available in wave 1, when the study children were not yet in school. For this reason, we can only estimate OLS regressions where the Year 3 (age 8-9) test scores proxy short-term outcomes while the Year 5 (age 10-11) and Year 7 (age 12-13) test scores proxy longer-term outcomes. Table 4 reports the estimation results. We find no statistically significant association between assignment to treatment and NAPLAN test scores in any year.

To further gauge the longer-term impact of the Baby Bonus, we also estimate the impact of the Baby Bonus on socio-emotional skills assessed in waves 4 and 5. In Table 5 we report the treatment effects of the Baby Bonus, obtained from the difference-in-difference model with controls, on each of the five individual components of the parent-assessed SDQ. With the exception of the pro-social scale, a higher value on the subcomponent indicates a worse socio-emotional outcome. The estimation results support our previous findings that the Baby Bonus had no positive effect on socio-emotional outcomes, and at worst may have had a negative and lasting impact on peer problems. The treatment effect of the Baby Bonus on peer problems ranges between 0.37σ and 0.52σ and is statistically significant at least at the 10% level.[10] As discussed previously, our intuition is that this negative treatment effect

[10]Interestingly, we find that the large and significant negative effect for peer problems becomes even larger when only analyzing households with two children, the cohort member and their sibling that triggered the

may be related to the arrival of a newborn sibling. This finding is also consistent with the interpretation that the Baby Bonus was not able to overcome any negative effects of the study child having a newborn sibling in the medium to long run.

6.1 Robustness Checks

6.1.1 Alternative Specifications

Before proceeding to discuss the mechanisms and heterogeneous effects of the Baby Bonus, we will demonstrate that our results are robust to varying sample definitions and alternative model specifications. In the previous analysis, we used a wide birth date window of the youngest sibling to define the treatment and control groups. We now test whether our results are sensitive to stricter classifications of treatment assignment.

In a first robustness check, we narrow the birth window of the last sibling in the treatment group to one year – siblings born between July 1, 2004 and July 1, 2005 – after the Baby Bonus was introduced. This stricter birth date window strengthens the claim that the Baby Bonus was exogenous by making the treatment and control groups even more comparable. Notably, as we will discuss in Section 6.3, this narrower definition also limits the capacity of the Child Care Rebate, introduced on July 1, 2005, to confound our treatment effect. The disadvantage is that it leaves us with a smaller sample of 153 children in the treatment group and 255 children in the control group. We are able to demonstrate in Table A2 that the estimated treatment effects are not sensitive to the treatment sample restriction. We still obtain a positive but statistically insignificant effect for learning outcomes in the magnitude of 0.31σ (Panel A), and a negative but statistically insignificant effects for socio-emotional (-0.18σ) (Panel B) and a significant negative effect for physical outcomes (-0.52σ) (Panel C).

In a second robustness check, we restrict the sample to families with only two children, including the study child and their younger sibling born around the threshold of July 1, 2004. This restriction generates a more homogeneous sample that excludes the possibility

Baby Bonus. These results are provided upon request.

that family size and sibling dynamics drive our main conclusions, however it leaves us with a smaller sample of 53 individuals in the treatment group and 239 individuals in the control group. Table A3 shows that the treatment effect of the Baby Bonus on learning outcomes obtained from the preferred difference-in-difference specification is now 0.49σ, and is statistically significant at the 10% level. The negative effect of the Baby Bonus on socio-emotional outcomes ($-.41\sigma$) and physical outcomes (-1.12σ) become more sizeable, however only the latter is statistically significant.[11] One explanation for this large and statistically significant effect for cognitive skills is that in smaller two-child families, the increased financial resources created by the Baby Bonus may be more likely to be invested in the study child relative to larger families where the benefits of the Baby Bonus may be distributed evenly between multiple children.

In a third robustness check, we test whether the way we control for the age difference between the study child and her youngest sibling influences the treatment effect of the Baby Bonus. In the benchmark models, we assumed a linear relationship between the age difference variable and child outcomes. In Table A4, we demonstrate that relaxing this assumption has an impact on the estimated treatment effect. We now include five dummy variables representing different age gaps to allow for discontinuities in the relationship between age difference and child outcomes.[12] This alternative specification reduces the benchmark treatment effect on learning outcomes marginally from 0.26σ to 0.24σ, an effect which remains statistically insignificant.

In a fourth robustness check, we test whether the results are sensitive to controlling for whether there was a newborn in the household in wave 2 rather than wave 1. While this arguably represents a 'bad control' because it is an outcome variable in the analysis, we present the results because it allows us to shed light on whether there is any 'newborn effect' that is biasing our results. Table A6 shows that the estimated treatment effects remain

[11]If we further restrict the sample definition of the control group sibling to the more restricted birth window of July 1, 2003 to June 30, 2004, we obtain identical results in sign, magnitude, and significance. These results are provided upon request.

[12]The five dummy variables divide age difference into the following categories: (1) less than three years, (2) between three and four years, (3) between four and five years, (4) between 5 and 6 years and (5) between 6 and 7.2 years.

unchanged and that there is no significant effect of a 'newborn'. This suggests that the presence of a newborn in the household in 2008/09 is not biasing our results. Finally, in a fifth robustness check we exclude individuals born very close to July 1st 2004 to purge any bias created by the manipulation of birth dates around the introduction of the scheme (Gans & Leigh 2009). Table A10 demonstrates that the results are not sensitive to this sample selection, however the effect size for learning outcomes increases marginally to 0.34σ.

6.1.2 Other Government Payments

A critical assumption of our difference-in-difference methodology is that there were no contemporaneous shocks to the treatment group other than treatment. We explore the validity of this assumption by examining whether simultaneous government payments differed between the treatment and control groups, thus inflating the importance of the Baby Bonus. This may arise if these government transfers are correlated with the age difference between the study child and their newborn sibling. Panel A in Table A7 shows that pre-treatment (wave 1) receipt of a range of government payments do not differ between the treatment group and the control group. Yet, Panel B shows that post-treatment (wave 3) the treatment group is more likely to have received several different payments, notably among them the Parenting Allowance and the Family Tax Benefit.[13]

This result is striking, and hence we examine it further by analyzing post-treatment wave 3 summary statistics in Table A5. Assignment to the treatment group is associated with both lower family income and lower maternal labor supply, which likely stems from the birth of a newborn sibling in treated households after 2004/2005. In practice, this has the effect of inflating the importance of the Baby Bonus and thus it may upward-bias our treatment effects, which is particularly relevant for the positive treatment effect of the Baby Bonus on learning outcomes. Our robustness checks therefore support our previous conclusion that the Baby Bonus is not likely to have improved learning outcomes or assist with the costs of bearing a child.

[13] Data constraints only allow us to examine the proportion of treated and untreated households that received government payments, rather than the actual amounts that they received.

6.1.3 Attrition

In 2008 (wave 3), only 4387 of the original 4983 children in the K-Cohort were surveyed, representing a dropout rate of 12%. If this attrition is systematic, it could create estimation biases. Therefore, we construct Inverse Probability estimates to correct for selective attrition. We attain these results in two stages: first, in Table A8 we estimate a logit model that shows which variables are important in influencing the dropout rate; second, in Table A9 we replicate our main results using Inverse Probability Weighting. These Inverse Probability Weighting estimates attach more weight to observations that remained in the sample until at least 2008 but who, in the baseline, resemble observations that dropped out of the sample. As shown in Table A9, our results are qualitatively unchanged. The treatment effect on learning outcomes rises to 0.32σ, however it is still statistically insignificant as a natural by-product of Inverse Probability Weighting is that the coefficients are estimated less precisely.

6.2 Parental Outcomes and Mechanisms

So far, we found no robust evidence that the Baby Bonus impacted positively on child developmental outcomes. This conclusion would further be strengthened if the Baby Bonus had no impact on the mechanisms via which family income impacts child development. The previous empirical literature identified various causal channels such as monetary or time investments in the child, parental stress, parental health, parenting styles and financial insecurity. In this section, we therefore test whether the Baby Bonus affected parental behaviors, well-being and household stress using a difference-in-difference estimation model:

$$\Delta m_{it} = \beta_0 + \beta_1 Bonus_i + \beta_2 X_i' + \epsilon_i, \tag{3}$$

where Δm_{it} represents the change in the mechanism from wave 1 in 2004 to wave 3 in 2008, and all other variables are defined in the same way that they were in our main specification. Table 6 presents the estimation results for 13 different outcomes. We do not find statistically significant effects of the Baby Bonus on any of these outcomes, with one important exception. Families in the treatment group are 21 percentage points more likely (significant at the 1% level) to report having no concerns in raising $2000 in response to an emergency or crisis.[14] This represents a clear and economically large effect of both the Baby Bonus and other government handouts; however this effect did not translate into a significant change in child outcomes or parental well-being and behaviours.

A transmission mechanism that warrants further analysis due to its repeated presence in the literature is whether the Baby Bonus had a causal effect on maternal labour supply, which determines the amount of time mothers can spend with their children (Fiorini & Keane 2014, Bono et al. 2015). There are two theoretical channels via which the Baby Bonus could decrease maternal labour supply; either through its effect on fertility and therefore maternity leave, or generally through creating a disincentive to work because of an increase in unearned household income.[15] Table 7 shows a statistically significant negative effect of the Baby Bonus on maternal labour supply in both OLS specifications (columns 1 and 2) and in the difference-in-difference model without control variables (column 3). Yet, once controlling for differences in observable characteristics in the difference-in-difference model (column 4), the statistically significant effect disappears, suggesting that the negative correlation is driven by a combination of observable characteristics and the fact that families in the treatment group have had a child more recently than the control group, rather than an effect that we can attribute to the Baby Bonus.

We are able to draw two conclusions from our results presented in Tables 6 and 7. First, the non-existent effect of the Baby Bonus on child skills can be explained by a muted impact on the traditional mechanisms – parental investments and styles, and maternal labour supply – that the previous literature has demonstrated to be correlated with the production of childhood skills (Attanasio et al. 2015, Mullins 2016, Bono et al. 2015). Second, parental well-being – measured by parental stress, depression and physical heath – did not improve either as a consequence of the Baby Bonus. Hence, we conclude that the Baby Bonus neither

[14] Given the base probability of 0.81%, this implies a 26% increase from the base probability.

[15] The disincentive to work created by an increase in unearned income is consistent with the predictions of static and dynamic models of labour supply. If leisure is a normal good, then an increase in unearned income raises the 'reservation wage' and thereby decreases the incentive to supply labour Keane (2011).

boosted child outcomes nor parental well-being and behaviours.

6.3 Heterogeneous Treatment Effects

The Baby Bonus was an unconditional cash transfer given to all households. This indiscriminate structure raises the question of whether the Baby Bonus would be more effective if targeted towards specific subsets of the population. Indeed, a number of empirical studies have found that the most significant returns to cash handouts are accrued to children from the most disadvantaged families (Dahl & Lochner 2012, Mullins 2016, Akee et al. 2015, Adhvaryu et al. 2015).[16] Yet, Cunha & Heckman (2007) suggest that due to dynamic complementarities between child investments and initial level of skills, monetary investments should be the most productive for children who have higher levels of skills at the time of the investment. Higher levels of skills are, however, associated with higher levels of economic advantage.[17]

Therefore, in this section, we first evaluate heterogeneous treatment effects across gender, and then explore heterogeneity by economic disadvantage proxied by family income and pre-treatment skill endowment. To estimate heterogeneous treatment effects we add to the preferred difference-in-difference model an interaction term between the Baby Bonus indicator and the relevant interaction variable. Specification details of the estimated models are provided in the Online Appendix A3.

Table 8 shows results for a difference-in-difference model that includes an interaction term between the treatment variable and an indicator for being a male study child. We find a positive treatment effect for girls on learning outcomes of 0.32σ (significant at the 10% level). The interaction effect for boys is negative (-0.098σ), indicating that the Baby Bonus has a smaller effect on learning outcomes for boys. However, the interaction effect is not statistically significant.

In contrast, for socio-emotional outcomes, there is no statistically significant treatment

[16] The theoretical literature also supports this proposition because it suggests that we expect larger returns for disadvantaged children as investments have larger returns at lower levels of available resources (Løken et al. (2012)).

[17] A number of studies have demonstrated this negative correlation between child skills and economic disadvantage including Fletcher & Wolfe (2016), Delaney & Doyle (2012) and Deckers et al. (2015).

effect of the Baby Bonus on girls (-0.0012σ) and we find a negative treatment effect for boys in the magnitude of 0.40σ (where the interaction term is significant at the 5% level). We do not have a clear intuition for the gender heterogeneity in socio-emotional outcomes as a result of the Baby Bonus. Since study children in the treatment group have had a sibling more recently than the control group, one explanation that is consistent with the data is that boys are more sensitive to the competition over scarce parental resources created by a newborn child, relative to girls. Empirical studies examining cash handouts have often ignored heterogeneity by gender; however interestingly our results contradict Dahl & Lochner (2012) who find larger treatment effects for boys.

Panel A in Table 9 presents results for a difference-in-difference model that includes interaction terms between the treatment variable and dummy variables for two terciles of income (medium and high). We find positive treatment effects on learning outcomes for study children in the lowest income tercile (0.25σ), although the effect is not statistically significant. The interaction effect for children in the middle income tercile is not statistically significant either, although its sign indicates that the Baby Bonus had a smaller impact on learning outcomes for this group (-0.20σ). In contrast, the interaction effect for study children in the highest income tercile is positive (0.35σ), indicating that the Baby Bonus had a stronger impact for these children relative to children in the most disadvantaged families.

At first glance, this result for high income individuals contradicts the majority of the empirical literature and provides cursory evidence in favour of dynamic complementarity. However, as shown in Panel B of Table 9, this interaction effect for high-income individuals decreases in magnitude to 0.20σ when we restrict the treatment group to consist of study children with siblings born between July 1, 2004 and July 1, 2005. This suggests that the larger effect for individuals in the highest income tercile could be driven by the additional receipt of the Child Care Rebate, which was introduced on July 1, 2005 and benefited mostly wealthy individuals. In addition, the treatment effect for the lowest income tercile increases from 0.25σ (unrestricted sample) to 0.32σ (restricted sample), and hence our results in the restricted sample are consistent with the empirical literature that postulates larger effects for disadvantaged children.

Further, our results in Table 9 confirm that the sizeable and significant negative treatment effect on physical outcomes is driven by individuals in the lowest income tercile. This gives weight to our hypothesis that families that valued the $3000 enough to manipulate the birth of their newborn were naturally low-income families, which is correlated with poor physical outcomes.

Finally, Table 10 presents the estimation results for dynamic complementarities between skills produced in one stage and the productivity of investments in subsequent stages. To test for dynamic complementarities we estimate a difference-in-difference model in which we include interaction terms between the treatment variable and two dummy variables that capture whether the study child scored in the second or third tercile of the pre-treatment skill distribution (relative to the study children in the first tercile). We focus our analysis of dynamic complementarity on learning outcomes only because this is the outcome for which we have found tentatively positive treatment effects in the previous analysis.

The findings suggest a positive, although insignificant, treatment effect of 0.26σ for children in the lowest skill tercile. The interaction effects between the treatment variable and the second or third tercile of the skill distribution are not statistically significant, although the interaction effect for the third tercile is 0.15σ, suggesting a larger treatment effect for individuals with the highest skill levels relative to the study children in the lowest skill tercile. This is tentative evidence in favour of dynamic complementarities between skill endowment and monetary investments. Similarly, this is tentative evidence against the hypothesis of diminishing marginal returns to investments in human capital (Mullins 2016, Dahl & Lochner 2012).

Hence, while there were no effects for the Baby Bonus in the aggregate, in this section we have presented a wide range of results that find suggestive evidence for heterogeneous treatment effects for different sub-populations, with significant variation in outcomes across income-level and gender. On the balance, it is not immediately clear whether the evidence presented in this section is consistent with the empirical literature that posits larger returns for disadvantaged individuals or with the idea of dynamic complementarities (Cunha & Heckman 2007). The estimation results are possibly compromised by the influence of other government child-care payments that were introduced just after the introduction of the Baby Bonus leading to an upward bias in the treatment effects, particularly for study children of high-income families. However, it is plausible that our evidence for the effect of the Baby Bonus on learning outcomes is consistent with both ideas, because cash interventions had larger returns for the most economically disadvantaged children and the least economically disadvantaged children, relative to children from the middle of the income or skill distribution.

6.4 Discussion and Interpretation

A variety of factors may explain the statistically insignificant impact of the Baby Bonus in the aggregate compared to other studies that found statistically significant effects for similar cash hand-outs.

The first explanation is that the Baby Bonus was non-means tested and not targeted towards sub-groups that have the highest marginal returns to income. Our estimates of heterogeneous treatment effects presented in Tables 9-10 give some weight to this claim, as they provide suggestive but not conclusive evidence that the Baby Bonus may have been more effective had it been targeted towards specific sub-populations. Turning to the literature, Dahl & Lochner (2012) found significant effects of the Earned Income Tax Credit (EITC) on child cognitive achievement; however the EITC differs from the Baby Bonus as it was targeted towards low-income households. Similarly, Adhvaryu et al. (2015) found large returns of the Progresa welfare program in Mexico where, unlike the Baby Bonus, recipients were rigorously selected based on socio-economic and geographic factors. While the $3000 Baby Bonus was relatively small in magnitude relative to the full costs of raising a child, it is comparable in size to cash interventions in other countries that found significant effects. However, it is arguable that the low amount in conjunction with the feature that it was given indiscriminately to all households means that it is not surprising that we find no effect in the aggregate.

Our second explanation is that the Baby Bonus did not have an effect because it was a one-off rather than an ongoing payment. The empirical evidence suggests that household expenditure patterns of one-off lump-sum cash transfers differ substantially from household expenditure patterns of permanent income (Barrow & McGranahan 2000, Goodman-Bacon & McGranahan 2008). Hence, our statistically insignificant results are consistent with the explanation that an increase in permanent household income is more conducive to the production of childhood skills compared to a one-off government handout. Indeed, Akee et al. (2015) find large effects on educational outcomes for a $4000 *annual* payment that in effect permanently increased household income.

A third explanation for why other studies have found positive treatment effects of cash hand-outs on child development is the choice of estimation methods. For instance, Dahl & Lochner (2012) and Duncan et al. (2011) use instrumental variables methods, which resulted in larger estimates than their OLS and first-difference models.[18] Finally, the Baby Bonus, unlike the other cash transfers evaluated in the literature, was accompanied by the 'shock' of the birth of a younger sibling. Thus, the interpretation of our results is that the Baby Bonus was not able to offset the negative impact of the birth of a newborn on a 4-5 year old child in a meaningful way.

7 Conclusion and Implications

Childhood cognitive and non-cognitive skills have important long-run implications for the productivity and prosperity of the economy. It is for this reason that a literature has emerged on the determinants of human capital formation from childhood into adolescence, focusing, among others, on the role of family income. While a positive correlation between family income and child development is well documented in the literature, economists do not agree

[18] Dahl & Lochner (2012) give three explanations for why they estimate larger effects using an instrumental variables technique: (1) OLS estimates suffer from attenuation bias because income is measured noisily, (2) the instrument could be capturing the effect for the most disadvantaged groups who have the highest marginal returns to income, and (3) expectations of future income impact child outcomes which instrumental variable estimates somewhat capture, causing them to be larger than OLS.

on whether there is a causal impact of cash transfers on the production of skills (Duncan et al. 2011). In this study, we contribute to this literature by evaluating the causal effect of the Baby Bonus – a one-off cash transfer of $3000 – on standard measures of cognitive and non-cognitive skill development of preschool children. In comparison to the income-support schemes examined in other studies, the Baby Bonus is unique it was given to all households conditional on the birth of a new child after July 1, 2004. In light of these distinctive features, the findings of this study will have important policy implications. The effectiveness of the Baby Bonus in shaping secondary outcomes may influence the way future cash transfers will be designed and implemented.

Using high-quality panel data on 516 Kindergarten Cohort members of the Longitudinal Study of Children, we estimate a difference-in-difference model comparing cohort members with siblings born marginally on either side of the threshold of July 1, 2004. We assume that the study children in the treatment and control groups have similar observable and unobservable characteristics, with the exception that their siblings were born at different times. In the aggregate, we find that the Baby Bonus was not effective in boosting skills of pre-school children. We estimate a positive treatment effect of the Baby Bonus on learning outcomes and negative treatment effects for both socio-emotional and physical outcomes, but these effects are only statistically significant for physical outcomes in our preferred estimation models. Our interpretation is that the Baby Bonus was not able to overcome the 'shock' of the birth of a newborn sibling for the average cohort member in our data.

While we find no effect in the aggregate, there is suggestive and mixed evidence that the Baby Bonus impacted the study children heterogeneously. We find tentative evidence that cognitive skills improved more for girls than for boys, while socio-emotional skills were unaffected for girls but decreased for boys. Outcomes also differed by income terciles, as we find larger effects for the lowest and highest income tercile relative to the middle income tercile. Despite possible localised treatment effects, our results indicate that the Baby Bonus cannot be justified as an intervention that is directed to the entire population. Our findings are robust to different definitions of the treatment and control group, alternative specifications and other government payments. The policy implications of our results are clear - they suggest that cash interventions given to the entire population to improve child outcomes and to offset the financial burden of childbirth are at best inefficient and at worst ineffective. Thus, future policies either need to be directed towards individuals where the marginal return to income is highest, or financial resources should be directed towards child care or other services which have a measurable and direct impact on child outcomes (Currie 2001).

While our results are consistent across specifications, there are two limitations in our study design that present avenues for further research. First, the data only contains measures on the Kindergarten Cohort from ages 4-5. Therefore, we cannot examine the human capital production function during very early childhood (0-3), a stage of development that Bono et al. (2015) suggest has the highest marginal returns for investment into child skills. Second, the small sample size available in our data prevents us from restricting the treatment and control group simultaneously to study children that had siblings born closer to the July 1, 2004 threshold, that were first-born and that had only one sibling. Therefore, future research could replicate our results using administrative schooling data to track the skill development of those children with siblings that were born just before and after the introduction of the Baby Bonus. Pursuing these avenues of further research may be worthwhile because so little research exists on cash transfers that offset the 'shock' of childbirth.

Tables

Table 1: Summary Statistics

	Treatment	Control	*p*-value of diff.[1]	N
Panel A: Demographic Characteristics				
Annual Family Income	66608.262	70389.274	0.348	480
Mother's Age	32.393	33.705	0.000	515
Mother Completed Year 12 (1=Yes)	0.664	0.714	0.224	516
Father Completed Year 12 (1=Yes)	0.579	0.587	0.855	494
Mother's Weekly Work Hours	14.348	13.900	0.737	516
Father's Weekly Work Hours	43.534	42.825	0.627	516
Sibling's Age Difference (Days)	2065.418	1292.665	0.000	479
Study Child's Age (Months)	56.676	56.353	0.154	516
Gender (1=Male)	0.579	0.580	0.982	516
Child was Firstborn (1=Yes)	0.810	0.941	0.000	516
Number of Siblings	0.794	1.067	0.000	516
Aboriginal or Torres Strait Islander (1=Yes)	0.016	0.022	0.613	516
Both Biological Parents at Home (1=Yes)	0.951	0.963	0.525	516
Low Birth Weight (<2500g)	0.061	0.059	0.953	516
Panel B: Parental Investment				
Housing Tenure (1=Owned Outright)	0.109	0.097	0.638	516
Over 30 Books at Home (1=Yes)	0.879	0.896	0.535	516
Has a Computer at Home (1=Yes)	0.781	0.766	0.673	516
Panel C: Family Stress				
Mother is in Excellent Health (1=Yes)	0.147	0.160	0.698	462
Father is in Excellent Health (1=Yes)	0.104	0.099	0.869	424
Mother's Parenting Style[2]	4.471	4.414	0.126	515
Father's Parenting Style	4.076	4.153	0.134	425
Depression Scale of Mothers[3]	4.366	4.333	0.542	461
Depression Scale of Fathers	4.458	4.373	0.125	409
Panel D: Neighborhood Effects				
Neighborhood Facilities[4]	1.964	1.969	0.936	516
% of Residents Completed Year 12	39.814	43.093	0.005	516
% of Residents Speak English	86.494	86.227	0.809	516

Notes: [1]*p*-values are reported for the pre-treatment (wave one) statistical difference between the treatment and control groups.
[2]Parenting style measures the frequency of parents displaying warmth to their child on a 5-point Likert scale (1 = never, 5 = always).
[3]Depression scale measures the frequency of parents feeling depressed (nervous, hopeless, sad, worthless etc) on a 5-point Likert scale (1 = very depressed, 5 = no depression).
[4]Neighbourhood facilities measures the respondent's ability to access public transportation, shopping facilities and other services such as banks and medical clinics on a 4-point Likert scale. (1 = strongly disagree, 4 = strongly agree).

Table 2: Impact of Baby Bonus on Child Outcomes

	OLS (1)	OLS (2)	Value-Added (3)	Diff-in-Diff (4)	Diff-in-Diff (5)
Panel A. Learning Outcomes					
Treat	-0.059	0.014	-0.035	0.047	0.26
	(0.089)	(0.21)	(0.18)	(0.091)	(0.18)
Panel B. Social/Emotional Outcomes					
Treat	-0.17	-0.19	-0.23	-0.14	-0.23
	(0.095)	(0.21)	(0.17)	(0.094)	(0.18)
Panel C. Physical Outcomes					
Treat	-0.080	-0.22	-0.31	-0.16	-0.69
	(0.091)	(0.23)	(0.20)	(0.098)	(0.21)
Controls	N	Y	Y	N	Y

Notes: Each outcome has been standardised to have mean 0 and standard deviation 1. Standard errors in parentheses. Column (1) reports OLS results with no controls on wave three data. Column (2) reports OLS results on wave three data with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Column (3) reports OLS (value-added) results controlling for both the one-period lagged child outcome and the same set of basic controls. Column (4) reports difference-in-difference results with no controls. Column (5) reports difference-in-difference results with the same basic controls. Observations (Treatment/Control): Learning Outcomes Index - 244/260; Social/Emotional Index - 218/231; Physical Health - 245/260.

Table 3: Impact of Baby Bonus on Physical Health Components

	Child's Health Rating (1)	Special Health Care Needs (2)	BMI (3)	PEDS QL (4)
Treat	0.29	-0.082	-0.21	-0.013
	(0.19)	(0.21)	(0.202	(0.25)
Observations	505	505	284	413

Notes: Each outcome has been standardised to have mean 0 and standard deviation 1. Standard errors in parentheses. All models are estimated using a difference-in-difference strategy with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Column (1) reports the effect of the Baby Bonus on a parent-assessed measure of the child's health, with lower coefficients corresponding to better health outcomes. Column (2) reports the effect of the Baby Bonus on whether the child had any special health child needs. Column (3) reports the effect of the Baby Bonus on the child's Body Mass Index (BMI). Column reports the effect of the Baby Bonus on the PEDS QL physicla health sub-scale which assesses motor skills.

Table 4: Impact of Baby Bonus on NAPLAN Results

	Year 3 (1)	Year 5 (2)	Year 7 (3)
Panel A. Reading			
Treat	-0.13	-0.054	-0.16
	(0.25)	(0.19)	(0.20)
Observations	352	436	416
Panel B. Writing			
Treat	0.26	-0.025	0.017
	(0.21)	(0.20)	(0.22)
Observations	353	436	416
Panel C. Spelling			
Treat	-0.084	-0.10	-0.14
	(0.23)	(0.20)	(0.22)
Observations	352	437	416
Panel D.Grammar			
Treat	-0.0056	0.012	-0.047
	(0.21)	(0.23)	(0.20)
Observations	353	437	416
Panel E. Numeracy			
Treat	-0.11	0.059	-0.11
	(0.21)	(0.19)	(0.20)
Observations	350	437	419

Notes: Each outcome has been standardised to have mean 0 and standard deviation 1. Standard errors in parentheses. Columns (1), (2) and (3) estimate the effect of the Baby Bonus on NAPLAN results in Years 3, 5, and 7 respectively. Each model is estimated using OLS with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education.

Table 5: Long-Term SDQ Results

	Wave 3 (1)	Wave 4 (2)	Wave 5 (3)
Panel A. Pro-Social Scale			
Treat	0.15	0.050	0.20
	(0.22)	(0.18)	(0.22)
Panel B. Hyperactivity Scale			
Treat	0.33	0.057	0.056
	(0.19)	(0.19)	(0.21)
Panel C. Emotional Symptons Scale			
Treat	0.024	-0.077	-0.0017
	(0.19)	(0.19)	(0.20)
Panel D. Peer Problems			
Treat	0.41	0.52	0.37
	(0.21)	(0.19)	(0.22)
Panel E. Conduct Problems			
Treat	0.24	0.071	-0.17
	(0.22)	(0.17)	(0.20)
Panel F. Total SDQ			
Treat	0.39	0.21	0.090
	(0.19)	(0.19)	(0.21)

Notes: Each outcome has been standardised to have mean 0 and standard deviation 1. Standard errors in parentheses. Each model is estimates the effect of the Baby Bonus on parent-assessed SDQ using a difference-in-difference model with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Column (1) represents the difference between wave 3 and wave 1, Column (2) represents the difference between wave 4 and wave 1 and Column (3) represents the difference between wave 5 and wave 1. Observations (Wave 3/4/5) = 449/512/484.

Table 6: Mechanisms

	Treat	Base $P(x)$	N
Panel A. Parental Investment			
Housing Tenure (1 = Owned Outright)	0.0076	0.1027	505
	(0.056)		
Over 30 Books at Home (1 = Yes)	0.032	0.8876	516
	(0.084)		
Computer at Home (1 = Yes)	-0.042	0.7733	505
	(0.077)		
Panel B. Household Environment			
Not Comfortable Financially (1 = Yes)	-0.016	0.0271	505
	(0.037)		
Can Raise $2000 Quickly (1 = Yes)	0.21	0.8112	491
	(0.073)		
Anger and Hostility in the Household (1 = Yes)	-0.15	-	368
	(0.13)		
Panel C. Parental Health			
Stressful Life Events Index	0.023	-	505
	(0.17)		
Mother in Excellent Health (1 = Yes)	0.095	0.1976	411
	(0.082)		
Father in Excellent Health (1 = Yes)	0.02	0.2158	312
	(0.12)		
Mother's Depression Scale	0.022	-	505
	(0.21)		
Father's Depression Scale	0.04	-	505
	(0.21)		
Panel D. Parenting Style			
Mother's Parenting Style	-0.062	-	449
	(0.21)		
Father's Parenting Style	-0.22	-	311
	(0.25)		

Notes: Standard errors in parentheses. All regressions are estimated using a difference-in-difference model comparing wave three to wave one outcomes, with the exception of 'Can Raise $2000 Easily' which compares wave three and wave two outcomes. The following variables are binary outcome variables: Mother and Father in Excellent Health (1=Yes), Not Comfortable Financially (1=Yes), Can Raise $2000 Easily (1=Yes), Housing Tenure (1=Owned outright), Children's Books in the Home (1=More than 30) and Computer in the Home (1=Yes). The following variables are continuous outcome variables: Stress Index, Mother and Father's Depression, Mother and Father's Parenting Style and Anger and Hostility in the Household; all of which are standardised to have mean 0 and standard deviation 1.

Table 7: Mechanisms - Mother's Labour Supply

	OLS (1)	OLS (2)	Diff-in-Diff (3)	Diff-in-Diff (4)
Panel A. Entire Sample				
Treat	-4.28	-8.24	-4.15	-4.79
	(1.36)	(2.93)	(1.37)	(3.03)
Panel B. Narrow Birth Window				
Treat	-3.04	-8.19	-3.05	-5.98
	(1.51)	(3.52)	(1.50)	(3.31)
Panel C. Two Children				
Treat	0.76	-11.9	-7.07	-3.49
	(2.20)	(8.64)	(2.64)	(5.20)
Controls	N	Y	N	Y

Notes: Standard errors in parentheses. The mother's labour supply is measured in the amount of weekly hours worked. Panels A reports the results for the entire sample. Panel B reports results for a restricted sample where the treatment group has siblings born in the narrower birth window of July 1st 2004 to July 1st 2005. Panel reports results from a restricted sample of only two children in the household. Observations (Panel A/Panel B/Panel C) = 505/399/285.

Table 8: Heterogeneous Effects - Gender

	Learning (1)	Social/ Emotional (2)	Physical (3)
Treat (Base = Female)	0.32	-0.0012	-0.70
	(0.21)	(0.22)	(0.24)
Male	0.46	0.21	0.092
	(0.13)	(0.13)	(0.14)
Treat x Male	-0.098	-0.40	-0.023
	(0.18)	(0.19)	(0.20)

Notes: All results are standardised to have a mean of 0 and a standard deviation of 1. Standard errors in parentheses. Each model is estimated using a difference-in-difference strategy with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Observations (Learning/Socio-Emotional/Physical) = 504/449/505.

Table 9: Heterogeneous Effects - Income

	Learning (1)	Social/ Emotional (2)	Physical (3)
Panel A. Entire Sample			
Treat (Base = Tercile 1 of Income)	0.25	-0.28	-0.91
	(0.24)	(0.24)	(0.25)
Treat x Tercile 2 of Income	-0.20	0.035	0.46
	(0.24)	(0.25)	(0.24)
Treat x Tercile 3 of Income	0.35	0.099	0.094
	(0.24)	(0.24)	(0.25)
Panel B. Narrow Birth Window			
Treat (Base = Tercile 1 of Income)	0.32	-0.052	-0.67
	(0.30)	(0.30)	(0.31)
Treat x Tercile 2 of Income	-0.22	0.073	0.44
	(0.29)	(0.31)	(0.26)
Treat x Tercile 3 of Income	0.20	-0.11	0.015
	(0.30)	(0.30)	(0.30)

Notes: All results are standardised to have a mean of 0 and a standard deviation of 1. Standard errors in parentheses. Each model is estimated using a difference-in-difference strategy with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Panel A reports results for the entire sample, and Panel B reports results for a restricted sample where the treatment group only has siblings born in the narrower birth window of July 1st 2004 to July 1st 2005. Panel A Observations (Learning/Socio-Emotional/Physical) = 469/416/469. Panel B Observations ((Learning/Socio-Emotional/Physical) = 367/331/367.

Table 10: Dynamic Complementarity

	Learning (1)	Social/ Emotional (2)	Physical (3)
Treat (Base = Tercile 1 of Initial Skills)	0.26	-0.12	-0.69
	(0.23)	(0.23)	(0.23)
Treat x Tercile 2 of Initial Skills	-0.13	-0.31	0.37
	(0.23)	(0.25)	(0.22)
Treat x Tercile 3 of Initial Skills	0.15	-0.059	0.20
	(0.22)	(0.24)	(0.22)

Notes: All results are standardised to have a mean of 0 and a standard deviation of 1. Standard errors in parentheses. Each model is estimated using a difference-in-difference strategy with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Observations (Learning/Socio-Emotional/Physical) = 503/449/505.

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Appendix

A1: Robustness Tables

Table A1: External Validity - Summary Statistics

	Restricted Sample	Entire Sample	*p*-value for diff.[1]	N
Demographic Characteristics				
Annual Family Income	63829.698	58183.529	0.009	4983
Mother's Age	33.076	34.826	0.000	4944
Mother Completed Year 12 (1=Yes)	0.690	0.569	0.000	4978
Father Completed Year 12 (1=Yes)	0.583	0.525	0.014	4223
Mother's Weekly Work Hours	14.114	13.766	0.625	4983
Father's Weekly Work Hours	43.165	37.096	0.000	4983
Study Child's Age (Months)	56.508	56.953	0.000	4983
Gender (1=Male)	0.579	0.501	0.001	4983
Child was Firstborn (1=Yes)	0.878	0.368	0.000	4983
Number of Siblings	0.936	1.552	0.000	4983
Aboriginal or Torres Strait Islander (1=Yes)	0.019	0.040	0.003	4981
Both Biological Parents at Home	0.957	0.814	0.000	4983
Low Birth Weight (¡2500g)	0.060	0.064	0.721	4982
Parental Investment				
Housing Tenure (1=Owned Outright)	0.103	0.111	0.557	4974
Over 30 Books at Home	0.888	0.808	0.000	4981
Has a Computer at Home (1=Yes)	0.773	0.759	0.468	4982
Family Stress				
Mother is in Excellent Health (1=Yes)	0.154	0.167	0.463	4197
Father is in Excellent Health (1=Yes)	0.101	0.142	0.012	3342
Mother's Parenting Style[2]	4.441	4.441	0.981	4972
Father's Parenting Style	4.117	4.070	0.093	3332
Depression Scale of Mothers[3]	4.349	4.310	0.180	4198
Depression Scale of Fathers	4.413	4.430	0.558	3253
Neighbourhood Effects				
Neighbourhood Facilities[4]	1.967	1.992	0.420	4975
% of Residents Completed Year 12	41.523	40.264	0.043	4983
% of Residents Speak English	86.355	87.550	0.038	4983

Notes: [1]*p*-values are reported for the pre-treatment (wave one) statistical difference between the treatment and control groups.

[2]Parenting style measures the frequency of parents displaying warmth to their child on a 5-point Likert scale (1 = never, 5 = always).

[3]Depression scale measures the frequency of parents feeling depressed (nervous, hopeless, sad, worthless etc) on a 5-point Likert scale (1 = very depressed, 5 = no depression).

[4]Neighbourhood facilities measures the respondent's ability to access public transportation, shopping facilities and other services such as banks and medical clinics on a 4-point Likert scale. (1 = strongly disagree, 4 = strongly agree).

Table A2: Smaller Birth Window

	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	Value-Added	Diff-in-Diff	Diff-in-Diff
Panel A. Learning Outcomes					
Treat	-0.075	-0.0052	0.031	0.0079	0.31
	(0.10)	(0.23)	(0.18)	(0.11)	(0.22)
Observations	399	399	399	399	399
Panel B. Social/Emotional Outcomes					
Treat	-0.17	-0.31	-0.31	-0.22	-0.18
	(0.11)	(0.25)	(0.25)	(0.12)	(0.22)
Observations	360	360	360	360	360
Panel C. Physical Outcomes					
Treat	-0.14	-0.37	-0.37	-0.31	-0.52
	(0.10)	(0.23)	(0.21)	(0.11)	(0.25)
Observations	399	399	399	399	399
Controls	N	Y	Y	N	Y

Notes: Each outcome has been standardised to have mean 0 and standard deviation 1. Standard errors in parentheses. Column (1) reports OLS results with no controls on wave three data. Column (2) reports OLS results on wave three data with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Column (3) reports OLS (value-added) results controlling for both the one-period lagged child outcome and the same set of basic controls. Column (4) reports difference-in-difference results with no controls. Column (5) reports difference-in-difference results with the same basic controls. Observations (Treatment/Control): Learning Outcomes Index - 244/260; Social/Emotional Index - 218/231; Physical Health - 245/260.

Table A3: Two Children

	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	Value-Added	Diff-in-Diff	Diff-in-Diff
Panel A. Learning Outcomes					
Treat	-0.043	0.80	0.40	0.11	0.49
	(0.15)	(0.41)	(0.31)	(0.15)	(0.29)
Observations	285	285	285	285	285
Panel B. Social/Emotional Outcomes					
Treat	-0.43	-0.70	-0.81	-0.38	-0.41
	(0.17)	(0.29)	(0.33)	(0.19)	(0.60)
Observations	260	260	260	259	259
Panel C. Physical Outcomes					
Treat	-0.29	-0.29	-0.13	-0.46	-1.12
	(0.17)	(1.04)	(0.97)	(0.17)	(0.48)
Observations	285	285	285	285	285
Controls	N	Y	Y	N	Y

Notes: Each outcome has been standardised to have mean 0 and standard deviation 1. Standard errors in parentheses. Column (1) reports OLS results with no controls on wave three data. Column (2) reports OLS results on wave three data with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Column (3) reports OLS (value-added) results controlling for both the one-period lagged child outcome and the same set of basic controls. Column (4) reports difference-in-difference results with no controls. Column (5) reports difference-in-difference results with the same basic controls. Observations (Treatment/Control): Learning Outcomes Index - 244/260; Social/Emotional Index - 218/231; Physical Health - 245/260.

Table A4: Non-Linear Age Difference

	OLS (1)	OLS (2)	Value-Added (3)	Diff-in-Diff (4)	Diff-in-Diff (5)
Panel A. Learning Outcomes					
Treat	-0.059	-0.058	-0.17	0.047	0.24
	(0.089)	(0.20)	(0.14)	(0.091)	(0.17)
Panel B. Social/Emotional Outcomes					
Treat	-0.17	-0.30	-0.29	-0.14	-0.25
	(0.095)	(0.21)	(0.16)	(0.094)	(0.18)
Panel C. Physical Outcomes					
Treat	-0.080	-0.18	-0.20	-0.16	-0.66
	(0.091)	(0.24)	(0.17)	(0.098)	(0.22)
Controls	N	Y	Y	N	Y

Notes: Each outcome has been standardised to have mean 0 and standard deviation 1. Standard errors in parentheses. Age-differences was controlled for non-linearly by creating a variable that represented specific age difference ranges. Column (1) reports OLS results with no controls on wave three data. Column (2) reports OLS results on wave three data with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Column (3) reports OLS (value-added) results controlling for both the one-period lagged child outcome and the same set of basic controls. Column (4) reports difference-in-difference results with no controls. Column (5) reports difference-in-difference results with the same basic controls. Observations (Treatment/Control): Learning Outcomes Index - 244/260; Social/Emotional Index - 218/231; Physical Health - 245/260.

Table A5: Summary Statistics - Post-Treatment (Wave 3)

	Treatment	Control	p-value of diff.[1]	N
Demographic Characteristics				
Annual Family Income	92423.127	101178.010	0.114	500
Mother's Age	36.486	37.852	0.000	502
Mother Completed Year 12 (1=Yes)	0.672	0.719	0.252	504
Father Completed Year 12 (1=Yes)	0.555	0.610	0.230	463
Mother's Weekly Work Hours	16.094	20.373	0.002	505
Father's Weekly Work Hours	42.829	40.981	0.256	505
Sibling's Age Difference	2067.704	1294.468	0.000	471
Study Child's Age (Months)	105.306	104.965	0.185	505
Gender (1=Male)	0.576	0.588	0.769	505
Child was Firstborn (1=Yes)	0.812	0.958	0.000	505
Number of Siblings	1.776	1.069	0.000	505
Aboriginal or Torres Strait Islander (1=Yes)	0.016	0.023	0.585	505
Both Biological Parents at Home (1=Yes)	0.922	0.888	0.192	505
Parental Investment				
Housing Tenure (1=Owned Outright)	0.127	0.119	0.803	505
Over 30 Books at Home (1=Yes)	0.922	0.923	0.979	505
Family Stress				
Mother is in Excellent Health (1=Yes)	0.171	0.192	0.554	446
Father is in Excellent Health (1=Yes)	0.193	0.162	0.457	345
Mother's Parenting Style[2]	4.172	4.312	0.011	450
Father's Parenting Style	3.930	4.099	0.010	345
Depression Scale of Mothers[3]	4.425	4.403	0.674	448
Depression Scale of Fathers	4.486	4.365	0.058	344
Neighbourhood Effects				
Neighbourhood Facilities[4]	1.957	1.997	0.513	505
% of Residents Completed Year 12	46.257	50.135	0.001	505
% of Residents Speak English	86.424	85.542	0.466	505

Notes: [1]p-values are reported for the post-treatment (wave three) statistical difference between the treatment and control groups.
[2]Parenting style measures the frequency of parents displaying warmth to their child on a 5-point Likert scale (1 = never, 5 = always).
[3]Depression scale measures the frequency of parents feeling depressed (nervous, hopeless, sad, worthless etc) on a 5-point Likert scale (1 = very depressed, 5 = no depression).
[4]Neighbourhood facilities measures the respondent's ability to access public transportation, shopping facilities and other services such as banks and medical clinics on a 4-point Likert scale. (1 = strongly disagree, 4 = strongly agree).

Table A6: Controlling for Newborn/Pregnancy in the Household in Wave 2

	OLS (1)	OLS (2)	Value- Added (3)	Diff-in- Diff (4)	Diff-in- Diff (5)
Panel A. Learning Outcomes					
Treat	-0.059	0.024	-0.042	0.047	0.27
	(0.089)	(0.21)	(0.17)	(0.091)	(0.18)
Newborn		-0.026	-0.12		0.093
		(0.15)	(0.13)		(0.15)
Panel B. Social/Emotional Outcomes					
Treat	-0.17	-0.19	-0.22	-0.14	-0.21
	(0.095)	(0.20)	(0.17)	(0.094)	(0.18)
Newborn		-0.062	-0.12		0.22
		(0.17)	(0.14)		(0.16)
Panel C. Physical Outcomes					
Treat	-0.080	-0.21	-0.31	-0.16	-0.68
	(0.091)	(0.23)	(0.21)	(0.098)	(0.21)
Newborn		0.024	-0.063		0.060
		(0.16)	(0.13)		(0.15)

Notes: Each outcome has been standardised to have mean 0 and standard deviation 1. Standard errors in parentheses. Column (1) reports OLS results with no controls on wave three data. Column (2) reports OLS results on wave three data with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Column (3) reports OLS (value-added) results controlling for both the one-period lagged child outcome and the same set of basic controls. Column (4) reports difference-in-difference results with no controls. Column (5) reports difference-in-difference results with the same basic controls. Observations (Treatment/Control): Learning Outcomes Index - 244/260; Social/Emotional Index - 218/231; Physical Health - 245/260.

Table A7: Other Government Payments

	Treatment	Control	p-value of diff.	N
Panel A. Pre-Treatment				
Parenting Payment Partnered	0.162	0.119	0.162	516
Parenting Payment Single	0.036	0.026	0.499	516
Carer Allowance	0.045	0.033	0.518	516
Newstart Allowance	0.016	0.011	0.624	516
Disability Pension	0.008	0.015	0.469	516
Other Government Alloances Payments	0.008	0.022	0.184	516
Family Tax Benefit or Family Payment	0.749	0.751	0.960	516
Panel B. Post-Treatment				
Parenting Payment Partnered	0.184	0.104	0.011	504
Parenting Payment Single	0.033	0.066	0.086	504
Carer Allowance	0.078	0.027	0.011	504
Newstart Allowance	0.012	0.008	0.605	498
Disability Pension	0.021	0.012	0.426	498
Other Government Alloances Payments	0.016	0.000	0.045	504
Family Tax Benefit or Family Payment	0.567	0.452	0.009	504

Notes: All government payments are binary variables (1=Individual received the payments, 0=individual did not receive the payment. p-values are reported for the pre-treatment (wave one) statistical difference between the treatment and control groups.

Table A8: Inverse Probability Weighting - Logistic Regression

	Dropout
Annual Family Income	-0.14
	(0.046)
Mother's Age	-0.031
	(0.0066)
Age Difference (Categorical)	-0.045
	(0.15)
Anger and Hostility in Household (1-5 Scale)	-0.058
	(0.056)
Mother's Weekly Work Hours	-0.0060
	(0.0028)
Mother's Health (1-5 Scale)	-0.017
	(0.053)
Father's Health (1-5 Scale)	0.027
	(0.061)
Number of Siblings	0.14
	(0.042)
Child was Firstborn	0.2*
	(0.10)
Bad Financial Situation	0.21
	(0.20)
Stressful Life Index	0.025
	(0.027)
Father's Depression Scale	-0.23
	(0.10)
Constant	1.77
	(0.90)
Observations	4983
Psuedo R-squared	0.1132
AIC	3897.398
BIC	4034.187

Notes: Standard errors in parentheses. We estimate a logit model to construct weights that are used in the following table.

Table A9: Inverse Probability Weighting - Impact of the Baby Bonus on Child Outcomes

	OLS (1)	OLS (2)	Value-Added (3)	Diff-in-Diff (4)	Diff-in-Diff (5)
Panel A. Learning Outcomes					
Treat	0.010	0.16	0.10	0.095	0.32
	(0.10)	(0.26)	(0.21)	(0.10)	(0.22)
Panel B. Social/Emotional Outcomes					
Treat	-0.13	-0.081	-0.15	-0.14	-0.21
	(0.10)	(0.21)	(0.18)	(0.11)	(0.22)
Panel C. Physical Outcomes					
Treat	0.0094	-0.17	-0.14	-0.065	-0.59
	(0.098)	(0.22)	(0.19)	(0.10)	(0.22)
Controls	N	Y	Y	N	Y

Notes: Each outcome has been standardised to have mean 0 and standard deviation 1. Standard errors in parentheses. We account for systematic attrition using inverse probability weights which attach more weight to observations who remain in the sample but who pre-treatment resemble observations that dropped out of the sample. Column (1) reports OLS results with no controls on wave three data. Column (2) reports OLS results on wave three data with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Column (3) reports OLS (value-added) results controlling for both the one-period lagged child outcome and the same set of basic controls. Column (4) reports difference-in-difference results with no controls. Column (5) reports difference-in-difference results with the same basic controls. Observations (Treatment/Control): Learning Outcomes Index - 244/260; Social/Emotional Index - 218/231; Physical Health - 245/260.

Table A10: Excluding Newborns Born Just After July 1st 2004

	OLS (1)	OLS (2)	Value-Added (3)	Diff-in-Diff (4)	Diff-in-Diff (5)
Panel A. Learning Outcomes					
Treat	-0.035	0.14	0.073	0.041	0.34
	(0.091)	(0.22)	(0.18)	(0.093)	(0.21)
Observations	467	467	467	467	467
Panel B. Social/Emotional Outcomes					
Treat	-0.15	-0.083	-0.11	-0.13	-0.22
	(0.097)	(0.21)	(0.21)	(0.099)	(0.22)
Observations	421	421	421	421	421
Panel A. Physical Outcomes					
Treat	-0.049	-0.22	-0.30	-0.079	-0.59
	(0.090)	(0.22)	(0.19)	(0.099)	(0.22)
Observations	468	468	468	468	468

Notes: Each outcome has been standardised to have mean 0 and standard deviation 1. Standard errors in parentheses. Column (1) reports OLS results with no controls on wave three data. Column (2) reports OLS results on wave three data with basic controls for the baseline number of siblings, maternal age, the age difference between the study child and their sibling, whether the study child was first-born, the main language spoken at home and the percentage of residents in the study child's region that completed secondary education. Column (3) reports OLS (value-added) results controlling for both the one-period lagged child outcome and the same set of basic controls. Column (4) reports difference-in-difference results with no controls. Column (5) reports difference-in-difference results with the same basic controls. Observations (Treatment/Control): Learning Outcomes Index - 244/260; Social/Emotional Index - 218/231; Physical Health - 245/260.

A2: Sample Selection Method

A variable representing the household's receipt of the Baby Bonus was not available in the data. Therefore, the sample selection process was coded as follows:

- K-Cohort children with zero siblings or greater than two siblings were dropped from our sample.
- Beginning with wave 4, K-Cohort children were assigned to the treatment group if they had a sibling, half-sibling or adopted sibling born between July 1, 2004 and June 30, 2006.
- Children were assigned to the control group if they had a sibling born between July 1, 2002 and June 30, 2004 and did not have a sibling born after July 1, 2004.
- The implication of this sample selection is that K-Cohort children that have a sibling born between July 1, 2002 and June 30, 2004, and another sibling born between July 1, 2004 and June 30, 2006 are assigned to the treatment group.
- This sample selection is then applied to all other waves.

A3: Heterogeneous Effects Models

In this section, we briefly outline the four models that were used to analyse heterogeneous effects:

1. Income - we created three terciles of income, $Income_i^{T1}$ (low), $Income_i^{T2}$ (medium) and $Income_i^{T3}$ (high), and generated interaction terms to estimate the following model:

$$\begin{aligned}\Delta h_{it} = \beta_0 + \beta_1 Bonus_i + \beta_2 Income_i^{T2} + \beta_3 Income_i^{T3} \\ + \beta_4 Bonus_i \times Income_i^{T2} + \beta_5 Bonus_i \times Income_i^{T3} + \beta_6 X_i' + \epsilon_{it}\end{aligned} \quad (4)$$

2. Gender - we estimated a model with an interaction term between receipt of the Baby Bonus and a dummy variable for gender:

$$\Delta h_{it} = \beta_0 + \beta_1 Bonus_i + \beta_2 Male_i + \beta_3 Bonus_i \times Male_i + \beta_4 X_i' + \epsilon_i \quad (5)$$

3. Region - we estimated a model with an interaction term between receipt of the Baby Bonus and a dummy variable for whether the child resided in a greater capital city:

$$\Delta h_{it} = \beta_0 + \beta_1 Bonus_i + \beta_2 CapitalCity_i + \beta_3 Bonus_i \times CapitalCity_i + \beta_4 X_i' + \epsilon_{it} \quad (6)$$

4. Initial level of skills - we created three terciles of initial skill levels, denoted by h_{i0}^l (low), h_{i0}^m (medium) and h_{i0}^h(high), and generated interaction terms to estimate the following model:

$$\Delta h_{it} = \beta_0 + \beta_1 Bonus_i + \beta_2 {h^m}_{i0} + \beta_3 {h^h}_{i0} + \beta_4 Bonus_i \times {h^m}_{i0} + \beta_5 Bonus_i \times {h^h}_{i0} + \beta_6 X_{it}' + \epsilon_{it} \quad (7)$$