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Does early schooling narrow outcome gaps for advantaged and disadvantaged children?*

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Abstract

This paper explores how starting school at a younger age affects the developmental score gaps between relatively advantaged and disadvantaged children. While previous findings suggest that delaying school entry may improve school readiness, less is known about whether it has differential effects for advantaged and disadvantaged children. For disadvantaged children, starting school early may be a better alternative to staying at home for longer as school provides a more stable and educational environment than the family home, overcompensating for the penalties of starting school early. This may be less applicable to relatively advantaged children who generally have greater access to resources in the home and who are more likely to utilise formal pre-school services. We use the Longitudinal Study of Australian Children to investigate if there is support for this hypothesis. The endogeneity of school starting age is addressed using the regression discontinuity design. We find that an early school start generally improves children's cognitive skills, which is even more pronounced for disadvantaged children. In contrast, an early school start tends to negatively affect children's non-cognitive skills with both advantaged and disadvantaged children affected in similar ways. Thus, our findings suggest that an earlier school entry may narrow the gaps in cognitive skills, whereas the gaps in non-cognitive skills are not affected by the school starting age.

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1 Introduction

Disadvantaged children begin school academically and behaviourally behind their relatively advantaged peers. They lag behind during the school years, and in adulthood they face weaker economic, social and health outcomes. Since children's scores in cognitive and non-cognitive tests are correlated with better outcomes later in life (Cunha & Heckman 2007), understanding the origins and persistence of these early gaps is a vital step towards reducing later gaps and ensuring the prosperity of future generations. In particular, we care about identifying the factors that can reduce these inequalities and that can be modified by policy.

One example of how the State attempts to modify these inequalities is by providing free schooling to children. However, some researchers argue that the inequalities of opportunity are perpetuated, even accelerated, during the schooling years by the increasing divide between private and public schooling (Preston 2011). Children who attend private schools, compared to those who attend public schools, may have greater access to resources within the school, which can better aid their teachers in delivering the course curriculum and create a more enriching environment for children to learn. Children who attend private schools may also have fewer encounters with children with extreme behavioural problems compared to children who attend public schools.

However, while inequalities exist in our schooling system, how do these inequalities compare to those that exist in the family home context? For example, does the size of the social gradient in achievement scores from attending school earlier narrow compared to the alternative of staying at home for an additional year? And does this apply differently for children from non-English speaking compared to English speaking families, according to the parent's level of education, family structure or the degree that parents involve themselves in different aspects of a child's life? It is important to consider these other dimensions of disadvantage, as opposed to just the conventional measure of income, because heterogeneity in the experiences of disadvantage within low-income families can produce variation in the benefits of early or additional schooling for these children.

These questions are relevant to the policy debate over whether children should delay their age of school start. Starting school early compared to staying at home for longer can impact on children's skill development patterns differently for advantaged and disadvantaged children. A priori, for disadvantaged children, we expect schools to provide greater access to resources and an environment that is more conducive to learning compared to the home environment. Whereas for advantaged children, the relative benefits of going to school to staying at home may be offset (or at least be lower) because their parents tend to invest more time and resources into their children's early education, even before they enter primary school. In this case, we would see lower socioeconomic status related achievement score gaps for children who start school earlier relative to children who start school later. Alternatively, if the inequality in the school environment is greater than the inequality in the home environment, then we will see disadvantaged children lose more ground in achievement scores to advantaged children in the group who start school earlier compared to later.

Most states in Australia admit children into school at the start of the calendar year, with children admitted if they turn five by a specified date. This cut-off rule means that children whose birthdates are one day apart, but lie on either side of the cut-off date, can begin school (nearly) one year apart. We use the interaction between the date-of-birth and the school eligibility rules to identify the causal effect of an early school start. Our approach follows the economics of education literature looking at the impacts of delayed entry into primary school. This is an effective identification strategy if children's dates of birth are random near the school eligibility cut-off dates - as they have been shown to be in the literature (Dickert-Conlin & Elder 2010). This paper contributes to the literature by comparing the net benefits of starting school early for children from disadvantaged and advantaged backgrounds. In other words, we specifically answer the question of how the gaps in cognitive and non-cognitive scores between advantaged and disadvantaged children change depending on whether or not they started school early. As another contribution to the literature, we explore how the results change when we consider different ways of defining disadvantage. We focus on measures such as the parent's education, relationship status, linguistic background as well as the level of educational resources in the home and parental participation in the child's life.

2 Literature review

The literature on the effects of school starting age (SSA) on human capital accumulation is quite large. As in our study, most of the papers in this literature use school starting age rules to separate the causal effect of SSA from the confounding variables.

A number of papers, spanning different countries, analyse the effect of SSA on children's academic performance, usually measured by test scores. For example, Datar (2006), Cascio & Schanzenbach (2007), Elder & Lubotsky (2009), Smith (2009), and Aliprantis (2014) provide evidence for the U.S.; Smith (2009) for Canada; McEwan & Shapiro (2008) for Chile; Crawford et al. (2007, 2010) and Crawford et al. (2011, 2013*a*, 2013*b*) for the U.K.; Strom (2004) for Norway; Fertig & Kluve (2005), Puhani & Weber (2007), Mühlenweg & Puhani (2010), Mühlenweg et al. (2012), and Wolff (2012) for Germany; Ponzo & Scoppa (2011) and Pellizzari & Billari (2012) for Italy, and Hamori & Kollo (2011) for Hungary. Bedard & Dhuey (2006) provide cross-country evidence. Most of these papers find that starting school at an older age improves academic performance, except Cascio & Schanzenbach (2007) and Pellizzari & Billari (2012) who find that being younger in the class has positive effects on academic outcomes (the latter paper focuses on university students).

In most of the cited papers, the effect of SSA is confounded with the effect of age at test or length of schooling, because these three variables are perfectly collinear: age at test = SSA + length of schooling. In addition, all these variables are highly correlated with relative age within a class. A series of papers by Crawford and colleagues (2007, 2010, 2011, 2013*a*, 2013*b*) aim to disentangle these effects by using regional variation in SSA rules, multiple datasets, and econometric techniques. Their results show that age at test accounts for most of the positive effect of SSA. Relative age and length of schooling also have positive effects, but the actual effect of age at which a child starts school is practically zero. Elder & Lubotsky (2009) also find similar results. On the other hand, Datar (2006) finds that starting school later has a positive effect on test scores even after eliminating the age at test effect. The latter findings are, however, based on quite strong functional form assumptions.

Other papers investigate whether SSA has any long-term effects. The results are mixed. Entering school later is usually found to positively affect educational attainment, but the effect on earnings is either zero or slightly negative. Fredriksson & Öckert (2006, 2013), Crawford et al. (2010), Solli (2012), and Zweimuller (2012) show that delaying school entry positively affects level of education. On the other hand, Fleury (2011) and Lincove & Painter (2006) find no effect of SSA on schooling. Although there is some evidence of negative effect of SSA on earnings (Bedard & Dhuey, 2012; Solli, 2012), most papers (Lincove & Painter, 2006; Dobkin & Ferreira, 2010; Fredriksson & Öckert, 2006, 2013; Crawford et al., 2013*c*) find that SSA does not affect earnings. Zweimuller (2012) shows that there is a positive initial wage gap between late and early school starters, but this wage gap disappears after three years. Moreover, Fredriksson & Öckert (2006, 2013) find a small negative effect of SSA on lifetime earnings and Black et al. (2011) provide evidence of negative SSA effect on wages at age 30.

We contribute to this literature by specifically investigating whether early school entry has differential effects on cognitive and non-cognitive skills of more and less advantaged children. Although some of the above cited papers perform heterogeneity analysis, they only look at a limited number of disadvantage measures and do not investigate this question explicitly. These papers usually find that the negative effect of early school start on academic performance and other outcomes is larger for children from lower socioeconomic status (SES) families (for example, Datar, 2006; Fredriksson & Öckert, 2006, 2013; Crawford et al., 2007; McEwan & Shapiro, 2008; Smith, 2009; Solli, 2012; Aliprantis, 2014). On the other hand, Black et al. (2011) find that children from less cognitively stimulating home environments in fact benefit from starting school early and other papers find no significant differences in SSA effects (Lincove & Painter, 2006; Puhani & Weber, 2007).

The two papers that are most closely related to ours are Cascio & Lewis (2006) and Hamori & Kollo (2011), as they also focus on the variation in SSA effect by disadvantage status. Cascio & Lewis (2006) investigate whether schooling (variation in which is driven by differences in SSA, as age at test is held fixed) may close racial gaps in cognitive skills in the U.S. Their results support this hypothesis. The effect of early school start (and thus length of schooling) is found to be positive for minorities and no effect for whites is found. The results of Cascio & Lewis (2006) are quite imprecisely estimated; therefore, these conclusions are tentative. Hamori & Kollo (2011) analyse variation in SSA effects by mother's education in Hungary and find that negative effects of SSA are larger for children of less educated mothers.

Our paper is different from these two studies in several ways. First, we look at a range of disadvantage outcomes, whereas race and mother's education measure only certain aspects of disadvantage. Second, we analyse differences in early schooling effects on a range of outcomes. On the one hand, disadvantaged children may gain more than advantaged children in terms of cognitive skills by attending school early. On the other hand, transition to school may have larger negative effects on non-cognitive skills of disadvantaged children compared to advantaged children. Third, our sample consists of young (6-7 year old) children, that is, we focus on the effects of early schooling. Cascio & Lewis (2006) and Hamori & Kollo (2011) look at older children (17 year olds and 10-15 year olds, respectively). Finally, to the best of our knowledge, we are the first paper to provide evidence on this question for Australia.

3 Data

The data used in this paper are based on the Longitudinal Survey of Australian Children (LSAC) - a nationally representative longitudinal survey of Australian children and their families conducted every two years beginning from 2004. We combine the two cohorts available in this data: the Kindergarten (born from March 1999 to December 1999) and the Birth (March 2004 to December 2004) cohorts. The available data allows us to examine the effect of early school start (at ages 4 -5) on children's outcomes at ages 6-7 years old. Our sample of children attend school in the following four States or Territories of Australia: New South Wales (NSW), Victoria (VIC), Australian Capital Territory (ACT) and Western Australia (WA).

The wave to wave attrition rate in our sample is small at approximately 9 percent. We also find that the propensity to attrit is not jointly correlated to the vector of demographic and socioeconomic explanatory variables, such as mother's educational attainment, equivalised paternal income, number of siblings, mother's age, jobless household indicator, low child birthweight indicator, parental ethnicity and language spoken in the home, as measured at wave 1. The p-value on the Wald test of joint significance is only 0.14. Together with the small rate of attrition, we argue that the sample attrition is unlikely to bring bias to our estimators.

The education systems vary significantly across the states of Australia. Each state provides funding and regulates the public and private schools within its governing area. Across Australia, Government (public) schools educate approximately 65 per cent of Australian students, with approximately 34 per cent in Catholic and independent (private) schools. Students who are Australian citizens and permanent residents can (largely) attend Government schools for free, whereas Catholic and independent schools usually charge attendance fees. Regardless of the type of system, schools in Australia are largely required to adhere to the same curriculum frameworks of their state or territory. During the second year of schooling, which is the year that this paper focuses on, students are taught basic literacy and numeracy. Students are usually placed in classes with one teacher who is primarily responsible for the student's education and welfare for that year.

As mentioned above, we focus on children residing in four states: NSW, VIC, ACT and WA as the primary school start rules for these four states are similar. This is necessary as we use the school start rules to segment our sample into children who are and are not eligible to begin school in the year that they turn five years old. The other states and territories follow different rules around the school entry process. For example, South Australia and Northern Territory used a rolling admission policy during the analysis period, which means that they are unsuitable to use for this analysis.

LSAC includes a wide array of variables on children's cognitive and non-cognitive outcomes. Also, we have both parent and teacher reported scores of non-cognitive skills, which act as an interesting and useful point of comparison and sensitivity analysis for the parent-response scores. We have fewer observations for teacher responses, as some of the surveys sent to the teachers of children were not returned. The sample size is nearly 4,000 children in the analysis of the parent responses, and just over 3,100 children in the analysis of the teacher responses.

Children's cognitive aptitude is measured by achievement test scores that are administered to the child at the time of the survey. We use the test scores of the Peabody Picture Vocabulary Test (PPVT) and the Matrix Reasoning test. The former is a test designed to measure a child's knowledge of the meaning of spoken words and his or her receptive vocabulary for Standard American English, with a number of changes made for greater applicability to the Australian context. The PPVT entails the interviewer showing the child a book with 40 plates of display pictures and where the child points to (or says the number of) a picture that best represents the meaning of the word read out by the interviewer. The items in this test change to reflect an appropriate difficulty for the respective ages of the children tested. The Matrix Reasoning test is based on the Wechsler Intelligence Scale for Children. It tests the child's problem solving ability by presenting children with an incomplete set of diagrams and requiring them to select the picture that completes the set from five different options. The instrument comprises 35 items of increasing complexity and the child starts on the item that corresponds to his/her age-appropriate start point.

The non-cognitive skills are measured by a child's behaviour problems and are derived from the parent and teacher responses to the Strengths and Difficulties Questionnaire (SDQ). There are five subscales of the SDQ, namely, Hyperactivity, Emotional, Peer problems, Pro-social and Conduct problems. Within each of these five subscales, parents are asked five questions and for each specific question they rate whether the incidence of child behavioural issues is "never true","somewhat true" or "certainly true" of a child. Some examples of behavioural issues are "fights with other children", "spiteful to others" and "argumentative with adults". We consider a child to exhibit the specific behavioural issue if parents answer either "certainly true" or "somewhat true" to the specific question. We argue that transforming these variables into binary form, as opposed to making a distinction between the "certainly true" and "somewhat true" responses, allows for a more objective assessment of whether the child does or does not exhibit the specific behavioural problem. Based on these five subscales, we construct three separate measures of non-cognitive skills - (1) internalising behaviours, which combines the emotional and peer subscales; (2) externalising behaviours, which combines the hyperactivity and conduct problems subscales; and (3) pro-social skills. The internalising and externalising behaviour indices are reverse coded so that a higher value indicates a higher level of a non-cognitive skill. For all of our cognitive and non-cognitive outcomes, we standardise them with respect to the weighted sample mean and standard deviation. Thus, the weighted means of these outcomes are equal to zero and their standard deviations are equal to one.

We define disadvantage using a number of indicators. We consider a child to have experienced disadvantage if (1) their mother has a low level of education, which we consider to be mothers without a degree above the high school level (year 12)[1,2]; or 2) if the main language spoken at home is not English as reported by both parents; or 3) if they belong to a single mother household; or 4) if their father's level of income is low, which we consider to be if equalised paternal income is in the bottom two quintiles of the distribution.

Each of these indicators may measure different dimensions in the experience of child disadvantage; therefore, we look at them separately rather than combining them into one index of disadvantage. Together these variables capture a package of traits that proxy for the experience of disadvantage. Income and education are standard measures of socioeconomic status and are correlated with the household's financial resources. Maternal education may also be correlated with the cognitive and emotional stimulation provided to children at home. Single parent status is expected to be linked to both financial and time investments in the child. Children from non-English speaking families are disadvantaged in a sense that they have fewer possibilities to develop their language skills compared to children from English speaking families. Yet this aspect of disadvantage does not necessarily have any implications for their non-verbal skills.

We also measure disadvantage more directly with these two indicators: (1) whether the child has less than 30 books in the household; and (2) whether the level of parental engagement and time spent on various activities with the child such as reading, telling them a story, drawing pictures, musical activities, playing toys and games, everyday activities, and playing outdoors, is low (as reported by the parent). Parental engagement is considered to be low if the average score on the home activities index is in the bottom third of the distribution. It is important to stress that this is a relative measure because even though mothers may be spending substantial amounts of time with the child in absolute terms, it may be perceived as low involvement relative to other children. We find that all of our indirect indicators of disadvantage are correlated with these direct measures of disadvantage.

[1] This does not include mothers who received a certificate or diploma.

[2] We checked sensitivity of the results to redefining the low level of education as "less than high school" instead of "no post-secondary education" and found that the results are robust.

In Table 1, we provide descriptive statistics of the sample by the early school start status. Children who start school early are less likely to have the biological father at home and are more likely to come from larger families, compared to children who start school later. Their parents are more likely to be overseas-born and of a non-English speaking background. They also tend to come from lower socioeconomic status families, where the home ownership rates are lower and their fathers are less likely to be employed and more likely to earn a low level of income. Further, these children receive lower levels of exposure to home or parental investments, as measured by the number of books at home and parental involvement in child activities. Although there are no differences in low birthweight rates by early school start status, early school starters are on average taller than late school starters. Children who start school early are more likely to go to a public school and less likely to go to a Catholic school than children who start school later. These differences in the family and child characteristics by early school start status show that the school starting age is unlikely to be exogenous.

4 Methods

To answer the question of whether or not early school start (ES) can narrow the gap in cognitive and non-cognitive skills between disadvantaged and advantaged children, we estimate the following linear regression for each skill k:

$$s_{k,6-7} = \gamma_{0k} + \gamma_{1k} ES_{4-5} + \gamma_{2k} DISADV + \gamma_{3k} ES_{4-5} * DISADV + X'_{4-5}\gamma_{4k} + v_{k,6-7}, \tag{1}$$

where t denotes time period. Cognitive and non-cognitive skills are measured at ages 6-7. By that time, early school starters have finished the second year of elementary school and late school starters have finished the first year of elementary school. The gap between disadvantaged and advantaged children in a skill k is calculated as: $\gamma_{2k}+\gamma_{3k}ES_{4-5}$. Thus, parameter γ_{2k} is interpreted as the gap in a skill k between disadvantaged and advantaged children in the subsample of late school starters ($ES_{4-5} = 0$) and parameter γ_{3k} shows by how much early school start closes (or widens) this gap. The effect of early school start on a skill k is calculated as: $\gamma_{1k}ES_{4-5}+\gamma_{3k}DISADV$. Thus, parameter γ_{1k} is interpreted as the effect of early school start for advantaged children ($DISADV = 0$). The effect of early school start for disadvantaged children is calculated as $\gamma_{1k} + \gamma_{3k}$. In our model, the effect of early school start cannot be identified separately from the effect of years of schooling, because holding age fixed, school starting age and years of schooling are perfectly correlated. The vector X includes control variables. The error term is denoted as $v_{k,t}$.

The identification of the parameters in equation (1) is complicated by the potential endogeneity of early school start, that is, the correlation between $ES_{k,4-5}$ and $v_{k,6-7}$. For example, parents may decide to delay a child's entry to school if the child has weaker cognitive or non-cognitive skills. For this reason, estimating equation (1) by Ordinary Least Squares (OLS) may result in biased coefficient estimates.

To address the endogeneity of early school start, we use a fuzzy regression discontinuity design (RDD) framework. Specifically, we use exogenous variation in the school starting age generated by the Australian school entry regulations. In Australia, the school year starts in late January or early February. Most Australian states use the child's date of birth to determine if a child is eligible to begin primary school in the year they turn five. A child is eligible to start school if he/she turns five prior to a cut-off date. Thus, these rules segment children into those who are eligible to start school "early" and those who are not. Early entrant children are those born in the first "part" of the year (January to July in NSW, January to April in VIC/ACT, and January to June in WA), and late entrant children are those born in the second "part" of the same calendar year. While at any given date, the early entrants are, on average, older than the late entrants from the same birth year, they are, however, able to enter school up to (nearly) one year earlier and so are younger (on average) when they enter school. This means that once both groups have started school, the late entrant children have spent a longer duration of time in the family home (or in pre-school or childcare) than in primary school, which may imply differing levels of exposure to educational stimuli. We anticipate this to be especially true for disadvantaged children.

These policy features mean that children born just prior to the cut-off date can start school up to one year earlier than children born just after the cut-off date - yet we expect them to be very similar along all background characteristics. Thus, the school starting rules create exogenous variation in the school starting age and we use the interaction between these rules and a child's age as an instrument for early school start. Our instrumental variable (IV) takes the value one if a child is eligible to go to school early (in the year the child turns five) and the value zero, otherwise.

Our identification strategy relies on the exogeneity of a child's age around the cut-off. This assumption will be violated if some parents manipulate birth timing so that a child's birth date would fall before or after the cut-off. It is especially problematic if the propensity to manipulate birth timing varies with the characteristics that affect children's cognitive and non-cognitive skills. For example, parents may wish to reduce childcare expenditure by sending children to school early. Lower-income parents may be more likely to do so because of their tighter budget constraint. Children of lower income parents may also have lower cognitive and non-cognitive skills. Dickert-Conlin & Elder (2010) directly test the validity of the exogeneity assumption by investigating whether there is a discontinuity in the number of births at the cut-off using U.S. data. Finding such a discontinuity would suggest manipulation of birth timing. These authors find no evidence of such a discontinuity, which provides support for our identifying assumption. To further assess the validity of this assumption, we investigate whether there are any discontinuities in selected family and child characteristics at the cut-off using our data. These characteristics include the father's employment status and income, home ownership, mother's education, main language spoken at home, number of siblings, mother's age, parents' marital status, and child's birthweight and current height. The results are presented in Figure 1. We find no discontinuities at the cut-off in any of the family and child characteristics. We interpret these findings as supporting our identification strategy.

As with other studies, sample size restrictions means that we only have a few observations of children born just before or just after the school entry cut-off date. In order to circumvent this problem, we also use children born further away from the cut-off date. Thus, it is important to flexibly model the running variable, which is the child's age in months[3] (Lee & Lemieux 2010). We expect parents to be more compliant with the school entry rules for cases where the child is older at the point he/she reaches the school-eligible age. Cognitive and non-cognitive skills are also expected to vary with a child's age. Given that we observe age in months and our age range is limited due to survey design, we have few possible age values. For this reason, we control for age non-parametrically: we include dummy variables for each month of age. To avoid collinearity with the constant and the early school start eligibility indicator, we need to omit two age month dummies - one just before the cutoff and one just after the cutoff.

We allow for differential compliance with school starting rules in VIC/ACT, WA and NSW: in the first stage, early school entry eligibility and age are interacted with state; state dummies are also included in the regressions. Thus, the control variable vector X_{4-5} includes age in months dummies, state dummies, and interactions between age and state. No other controls are necessary in the RDD framework when the running variable is randomly determined at the cut-off (Lee & Lemieux 2010). As an instrument for the

[3]We normalise this variable so that it is measured as months above or below five years of age as at April 30 (in the year a child turns five).

interaction between early school start and disadvantage status, we use the interaction between the early school entry eligibility and disadvantage status.

Variation in the school starting rules by state provides an additional source of identification in our analysis. For example, if we have two children who turn five years old on the same day (say May 15), but one lives in VIC and the other one lives in NSW, the first one will be not eligible to go to school that year, whereas the second one will be.

If there is heterogeneity in early schooling effects on cognitive and non-cognitive skills, our IV estimates identify the local average treatment effects (LATE). These estimates are interpreted as the effects of early school start for a sub-population of children who comply with the school starting rules (compliers in the LATE literature), allowing for the compliance to vary with the disadvantage status. In this case, we cannot generalise our results to children who do not go to school once they reach eligible age (red-shirters or never-takers).

5 Results

We start by showing that school entry rules in Australia indeed affect the actual timing of school entry. Figure 2 plots the percentages of children who are in school in the year they turn five by month of age. We present plots for VIC/ACT, NSW and WA separately. The vertical lines denote the relevant age cutoff for each state. Figure 2 shows that the school entry rules affect the timing of school start for children from all states, however, there are differences in the compliance rates across these states. Compliance with the school entry rules is greatest in WA. At the cutoff, the percentage of children who are in school increases from 12 percent to 79 percent. In VIC/ACT, compliance with school entry rules is also substantial with the percentage of children in school increasing from 7 percent for the children just before the cutoff to 40 percent for children just after the cutoff. By contrast, compliance with the school entry rules is weak in NSW with the percentage of children attending school increasing from 0.4 percent to only to 17 percent at the cutoff. Anecdotally, the different compliance-rate profiles across these states may stem from the legacy of their historic rules and practices around when a child began school and the likelihood with which the child repeated a grade. We show that the instruments are strongly related to the early schooling variable (and its interaction with disadvantage status). For example, when low maternal education is used to measure disadvantage, the F-statistic for the joint significance of the instruments in the early school start and its interaction with the disadvantage status equations is equal to 29.12 and 100.25, respectively.

Table 2 presents the main results of the analysis based on the full sample of children. Panel A reports the estimates of the cognitive skill regressions. With the exception of children from Non-English Speaking (NES) families, disadvantaged children are found to have lower Picture Vocabulary and Matrix Reasoning (problem solving) test scores (by close to a quarter of a standard deviation). Children from NES families have substantially lower vocabulary skills than children from English Speaking (ES) families, but do as well in terms of problem solving skills. This suggests that the Non-English Speaking Background (NESB) indicator primarily measures language-related disadvantage, whereas low maternal education and single motherhood proxy for disadvantages affecting different types of cognitive skills.

The answer to one of our key questions - does early school start narrow the gap in cognitive skills - depends on the measure of disadvantage used. The results suggest that early school start may narrow and even close the gap in cognitive skills between children of more and less educated mothers as well as between two-parent and single-parent children. For example, the gaps in cognitive test scores between two-parent and single-parent children are found to be smaller by almost a quarter of a standard deviation among early starters than among late starters. However, none of the positive interactions between these two measures of disadvantage status and early school start are statistically significant.

On the other hand, early schooling is found to widen the gap in vocabulary skills between children from NES and ES families. Among children from ES families, those who started school early do substantially better in the vocabulary test (by almost one half of a standard deviation) than those who started school later. On the contrary, the difference in the vocabulary test score between early and late starters among children from NES families is small ($0.464 - 0.392 = 0.072$) and not statistically significant. This finding is surprising, because early starters have completed one more year of schooling than late starters. Dynamic complementarity between investments is a possible explanation for the small effect of additional schooling on vocabulary skills among children from NES families. Dynamic complementarity refers to the dependence of the return to skills investment on the stock of that skill (Cunha & Heckman 2008). In our context, the effect of reading classes on reading skills may depend on the basic stock of vocabulary skills possessed by the child. If children from NES families have poor basic vocabulary skills, they may not derive much benefit from reading classes at school.

Turning to the non-cognitive skills, the results vary depending on whether we use parents' or teachers' answers. According to the parents' responses, disadvantaged children have lower non-cognitive skills than advantaged children. The estimated gaps in the non-cognitive skills between single- and two-parent children are especially large and vary from 18.4 percent of a standard deviation in pro-social behaviours to 49 percent of a standard deviation in externalising behaviours. According to teachers' responses the evidence that disadvantaged children have poorer non-cognitive skills is weaker. Consistent with parents' responses, teachers' reports suggest that children of single mothers tend to have more externalising and internalising behaviour problems than children from two-parent families. However, there are no reported differences in the non-cognitive skills between children with relatively high versus low-educated mothers. Moreover, teachers' reports suggest that children from NES families have fewer externalising behavioural problems than children from ES families. The contrast in the results based on the parent and teacher evaluations can be explained in several ways. First, parents may be under-reporting children's behaviour problems and advantaged parents may be more likely to under-report. Teachers are likely to be more objective and consistent in evaluating children's non-cognitive skills than parents. Second, disadvantaged children may be well-behaved well at school but poorly-behaved at home. Finally, teachers may be less likely to notice children's behavioural problems than parents.

According to both parent and teacher evaluations, there are no statistically significant differences in the non-cognitive skill gaps between early and late school starters. Thus, early school start does not appear to affect disadvantaged and advantaged children differently. As the negative coefficients on the early school start variable and insignificant interactions show, both disadvantaged and advantaged children's non-cognitive skills are negatively affected by early schooling. These negative effects are estimated to be generally larger in absolute value and statistically significant, if we use teacher-evaluations of the non-cognitive skills. As discussed in the introduction, the transition into school can be challenging for some children, and especially when this transition occurs at a younger age. Our results support these conjectures.

Next, we present the results for children of married mothers. There are two reasons we are interested in this sub-sample. First, this sub-sample represents families who are, on average, relatively advantaged. For example, a substantial proportion of husbands of low educated mothers have attained education at a level that is higher than high school (61 percent). Also, for 10 percent of children who we classify as coming from a NES family, their fathers are actually Australian-born and this percentage would be higher if we consider fathers born in an English speaking country. These fathers may be more likely to interact with the child in English, even though the survey respondent lists a language other than English as the main language spoken at home. Thus, the difference in the home environment between disadvantaged and advantaged children may be smaller among children of married mothers than among children of single mothers. For this reason, an early school start may affect the gaps in cognitive and non-cognitive skills to a lesser extent in this sub-sample. The results presented in the first two columns of Table 3 show, however, that it is not the case. The estimated gaps in cognitive and non-cognitive skills among children of married mothers are of similar magnitude as in the full sample. The interactions of early school start with disadvantage status are statistically insignificant as in Table 2 (with the exception of the interaction with NESB, which is significant at the 10 percent level and of similar magnitude to the estimate using the full sample).

The second reason we focus on married mothers is that we can use the father's income as a measure of disadvantage. As explained above, household income is not a good measure of disadvantage in our analysis, because mother's employment and, in turn, their income may be affected by whether a child starts school earlier or later (Zhu & Bradbury 2015). Overall, the results based on the father's income as the measure of disadvantage (last column of Table 3) are consistent with the results based on low maternal education. According to the parent-evaluations, children of low-earning fathers have lower cognitive and non-cognitive skills whereas no gaps in non-cognitive skills are found using teacher evaluations. We also do not find any evidence that early school start opens gaps in non-cognitive skills. One difference from the full sample is that an early school start is not found to narrow the gaps in the cognitive skills.

Table 4 presents the results for children of mothers who work part-time (25 hours or less) or do not work at all. We are interested in this sub-sample because children of mothers who do not work full-time are more likely to stay at home rather than attend childcare before starting school. Therefore, larger differences in the home environment between disadvantaged children and advantaged children are expected for this sub-sample. Further, we may also expect there to be less variation in the quantity and quality of childcare by disadvantage status for this sub-sample. Consequently, early school start may have larger effects on the gaps in cognitive and non-cognitive skills among children of mothers who do not work full-time. Another reason to focus on this sub-sample is to net out the effect of early school start on maternal employment. Some mothers may change their employment behaviour in response to their child's early school start (Zhu & Bradbury 2015). Subsequently, differences in children's test scores may in part, reflect the impact of maternal employment differences. We find that 38 per cent of mothers with children who are eligible to start school in the year they turn five work less than or equal to 25 hours per week, compared to 79 per cent for mothers with children who are ineligible to start school early. This difference is also statistically significant. This suggests that maternal employment may be one of the channels that is driving the difference in the test score gaps between early and late starters.

Compared to the full sample, we find some differences in the results for cognitive skills in the sub-sample of mothers who do not work full-time. First, Table 4 shows that an early school start does not appear to widen the gaps in vocabulary skills between children from ES and NES families. It also suggests that early school start has little effect on the vocabulary skills of children from ES families. Second, for this sub-sample we find that early school start widens the gap in problem solving ability between children of two-parent and single-parent families. However, the interaction between early school start and single parenthood status is not statistically significant in either sample. Otherwise, the results for cognitive skills are comparable to the main results based on the full sample. Overall, the results for non-cognitive skills are also similar to the main results. Given these overarching similarities in the results, we infer that changes in maternal employment do not appear to be the main mechanism through which an early school start affects cognitive and non-cognitive skills.

Finally, we investigate how using more direct measures of disadvantage affects the results. The two direct measures that we use are the number of books a child has at home (more than 30 or not) and parental involvement in a child's activities (high or low). The results are presented in Table 5. Similar to the baseline results presented in Table 2, we find that disadvantaged children generally have lower cognitive skills, although there is no statistically significant difference in problem solving ability by parental involvement. Another similar finding is that early schooling does not affect disadvantage gaps in vocabulary skills. However, one point of difference from the baseline results is that Table 5 suggests that early schooling does not close the gaps in either type of the cognitive skills.

For the non-cognitive skills, similar to the findings that use the indirect measures of disadvantage, Table 5 also suggests that the externalising and internalising behaviour gaps are more pronounced according to the parent-evaluations compared with the teacher-evaluations. One point of difference with the baseline results is that there is more evidence to suggest that an early school start affects the non-cognitive skills gaps. For example, we find that an early school start closes (and possibly reverses) the gaps in the parent-evaluated non-cognitive skills when disadvantage is measured by fewer books in the home (for internalising behaviour) and the measure of low parental involvement (for both internalising and externalising behaviour).

6 Conclusion

This paper set out to understand how the cognitive and non-cognitive outcome gaps between advantaged and relatively disadvantaged children fared depending on whether children started school earlier or later. We hypothesised that, on the one hand, these outcome gaps may close for children who start school earlier because the contrast in the quantity and quality of resources provided by the school compared to the family home is likely to be greater for disadvantaged children than advantaged children and thus the gains to entering school earlier compared to staying at home is also likely to be greater. On the other hand, these inequality reducing benefits of early school start may be offset by greater inequalities in the school environment or negative impacts on children's school readiness, which may be more keenly felt by children from disadvantaged backgrounds. We analysed these questions for a variety of outcomes, subsamples and disadvantage indicators.

As expected, we find that children from disadvantaged backgrounds tend to do worse in the cognitive skill tests. Disadvantaged children are found to have both lower vocabulary and problem solving skills. The only exception is children from NES families, who while having lower vocabulary skills, do not perform any worse on problem solving skills than children from ES families. This likely reflects the relatively narrow focus of the NES background measure in capturing a child's experience of disadvantage. We also find that disadvantaged children have lower non-cognitive skills than advantaged children, as measured by externalising, internalising and pro-social behaviours. The magnitude of the gaps in the non-cognitive skills vary, however, depending on whether we use the parent or teacher evaluations of non-cognitive skills with smaller gaps reported in the teacher evaluations compared to the parent evaluations. Given these differences, it suggests there is value in collecting and using both the parent and teacher evaluations in analysing non-cognitive skills gaps.

Turning to our primary research question, we find evidence that suggests that an early school start can narrow the gaps in cognitive skills, if disadvantage is measured by low maternal education and single motherhood status. We find that both disadvantaged and advantaged children benefit form early school start in terms of vocabulary skills, but disadvantaged children benefit more so. In the case of problem solving skills, an early school start is found to benefit only disadvantaged children. These results lack, however, statistical significance and thus we are cautious in placing too much emphasis on them. There is no evidence that an early school start narrows the gaps in cognitive skills when we use paternal income as a measure of disadvantage. Additionally, we find that an early school start may widen the gaps in vocabulary skills between children from NES and ES families, with only children from ES families benefiting from an early school start.

For non-cognitive skills, we generally find little evidence that an early school start affects the gaps in these skills between disadvantaged and advantaged children. Both disadvantaged and advantaged children are found to do worse in terms of the non-cognitive skills if they start school early. These findings suggest that early starters are less capable of dealing with the changes related to the transition from home or childcare to school than late starters. Yet, only when we measure disadvantage using more direct measures such as low parental involvement in a child's activities and the number of books in the family home, do we find evidence in parent-evaluations suggesting that an early school start may indeed narrow or even reverse the gaps in non-cognitive skills.

For policy purposes, especially with proposals to delay the age of school start, our results highlight heterogeneity in the effect of school starting age. We find that children's cognitive skills generally benefit from going to school early whereas their non-cognitive skills are likely to be negatively affected. Further, children from different family backgrounds and in particular, with different levels of access to resources in the family home, derive different pay-offs to an early school start. These results highlight the need for a more nuanced approach to assessing the benefits and drawbacks of any policies that purport to assist children with their development - including policies around the school starting age.

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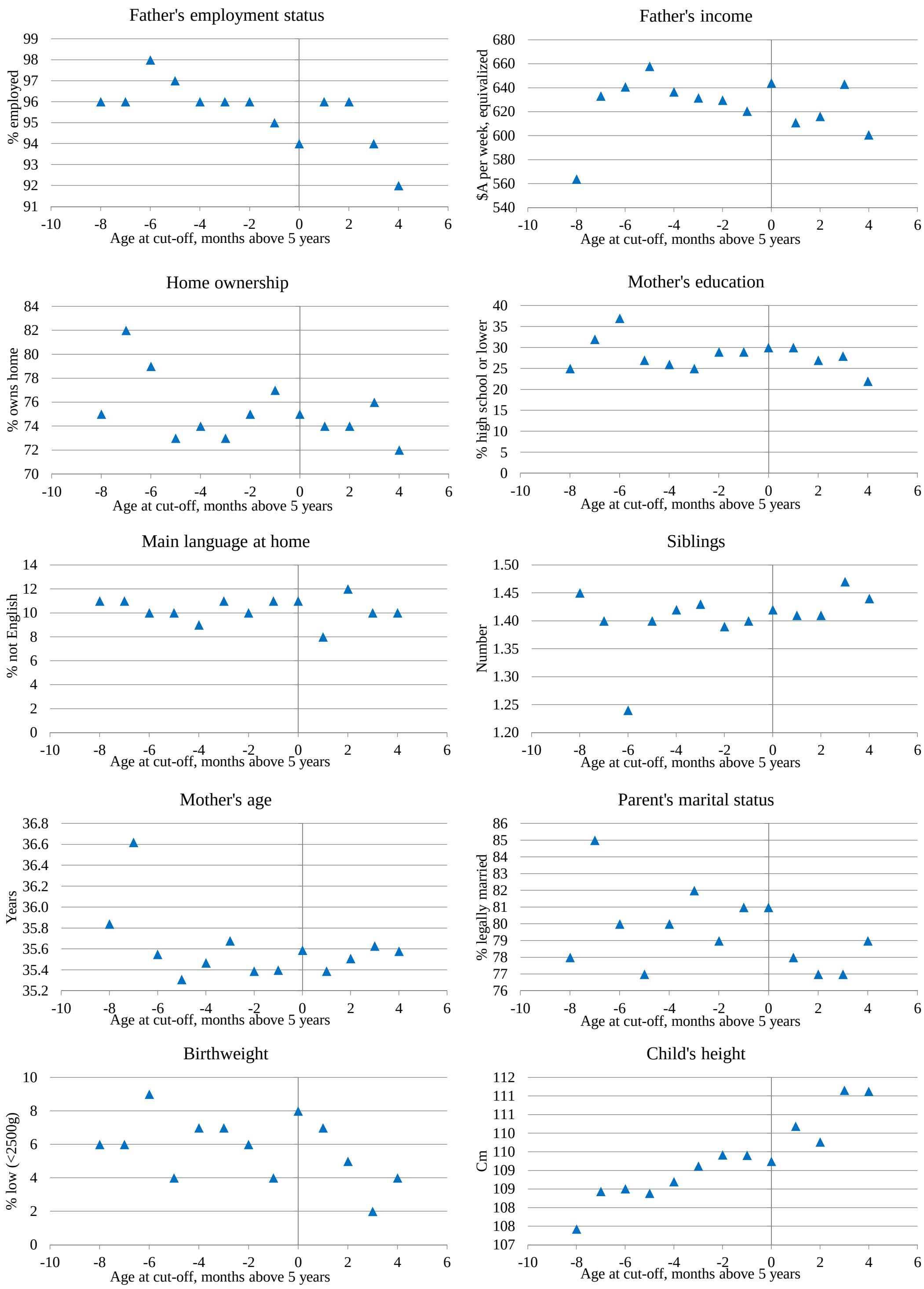

Figure 1: Variation in family and child characteristics by a child's age. *Notes*: age is measured in the year a child turns five.

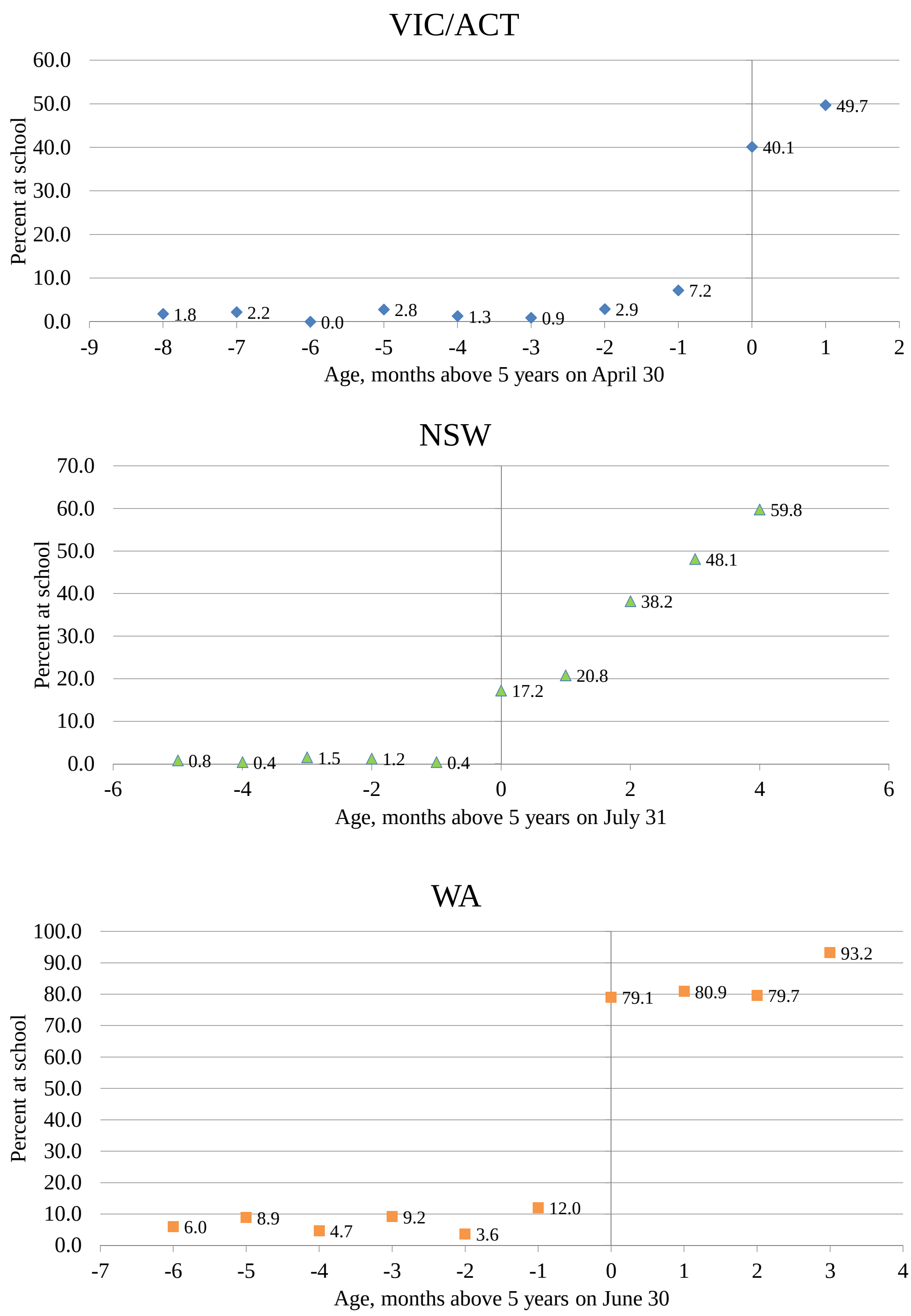

Figure 2: Effects of school starting rules on school entry in VIC/ACT, NSW and VIC. *Notes*: age is measured in the year a child turns five.

Table 1: Descriptive statistics by early school start status

	All	Not in school at age 4-5	In school at age 4-5	t-stat of difference
Family structure:				
Married biological father at home	0.76	0.77	0.73	−1.79
Cohabitating biological father at home	0.09	0.09	0.10	0.74
Stepfather at home	0.02	0.02	0.03	0.65
Single mother	0.13	0.12	0.15	1.51
Family size:				
No siblings	0.11	0.11	0.12	1.09
One sibling	0.49	0.50	0.46	−2.00
Two siblings	0.28	0.28	0.27	−0.61
Three or more siblings	0.12	0.11	0.15	2.05
Mother or father foreign born	0.33	0.31	0.44	5.21
Foreign language spoken at home	0.22	0.20	0.32	4.45
Child indigenous	0.03	0.03	0.03	0.62
Parents own home	0.72	0.73	0.68	−2.00
Father employed	0.95	0.96	0.91	−2.80
Father's equivalised income:				
Bottom quintile	0.17	0.16	0.23	2.84
Second quintile	0.20	0.20	0.20	−0.19
Third quintile	0.21	0.22	0.15	−3.48
Fourth quintile	0.21	0.21	0.22	0.54
Top quintile	0.21	0.21	0.20	−0.36
Educational attainment of mother:				
High school or below	0.30	0.30	0.33	1.46
Vocational education	0.38	0.38	0.37	−0.43
University degree	0.32	0.32	0.30	−0.94
Mother's age, years	35.40	35.41	35.33	−0.28
Less than 30 books at home	0.17	0.15	0.24	3.76
Parental involvement index (standardized):				
Bottom third	0.36	0.34	0.43	3.88
Middle third	0.35	0.35	0.34	−0.51
Top third	0.29	0.31	0.23	−4.16
Child's birthweight low ($<$ 2500g)	0.06	0.06	0.06	−0.43
Child's height, cm	109.27	108.99	110.47	6.10
Type of school:				
Public	0.67	0.66	0.70	1.86
Catholic	0.23	0.24	0.20	−1.89
Independent (private)	0.10	0.10	0.10	−0.36
Sample size	3,952	3,214	738	

Notes: All variables are measured when children are 4-5 years old.

Table 2: IV model estimates: full sample

		Low Maternal Education	NESB	Single mother
A. Cognitive skills				
Vocabulary (std)	DISADV	−0.271***	−0.489***	−0.255***
	ES*DISADV	0.072	−0.392**	0.261
	ES	0.384*	0.464**	0.421*
Problem solving (std)	DISADV	−0.220***	0.032	−0.226***
	DISADV*ES	0.088	0.220	0.287
	ES	−0.041	−0.031	0.017
B. Non-cognitive skills: Parent-evaluated				
Externalising beh - rev (std)	DISADV	−0.166***	−0.176***	−0.490***
	DISADV*ES	−0.025	−0.203	0.165
	ES	−0.256	−0.177	−0.277
Internalising beh - rev (std)	DISADV	−0.087**	−0.063	−0.349***
	DISADV*ES	−0.070	−0.091	−0.176
	ES	−0.337	−0.296	−0.282
Pro-social beh (std)	DISADV	−0.018	−0.161**	−0.184**
	DISADV*ES	−0.117	0.139	0.042
	ES	−0.053	−0.139	−0.065
C. Non-cognitive skills: Teacher-evaluated				
Externalising beh - rev (std)	DISADV	−0.019	0.156**	−0.291***
	DISADV*ES	−0.102	−0.083	−0.024
	ES	−0.485**	−0.504**	−0.525**
Internalising beh - rev (std)	DISADV	−0.059	0.049	−0.258***
	DISADV*ES	0.045	0.226	−0.067
	ES	−0.418*	−0.434*	−0.393*
Pro-social beh (std)	DISADV	0.036	−0.111	−0.029
	DISADV*ES	0.178	0.235	0.027
	ES	−0.426	−0.452	−0.332

Notes: In panels A and B, sample size is 3,952. In panel C, sample size is 3,113. All regressions control for state dummies and age in months dummies (interacted with state). Standard errors are robust to heteroscedasticity. *denotes statistical significance at the 10% level, **denotes statistical significance at the 5% level, and *** denotes statistical significance at the 1% level.

Table 3: IV model estimates: sub-sample of married mothers

		Low Maternal Education	NESB	Low Paternal Income
A. Cognitive skills				
Vocabulary (std)	DISADV	−0.264***	−0.466***	−0.289***
	DISADV*ES	0.106	−0.413*	−0.163
	ES	0.348	0.401*	0.529**
Problem solving (std)	DISADV	−0.204***	0.006	−0.178***
	DISADV*ES	0.067	0.196	−0.147
	ES	0.037	0.054	0.105
B. Non-cognitive skills: Parent-evaluated				
Externalising beh - rev (std)	DISADV	−0.172***	−0.230***	−0.198***
	DISADV*ES	−0.025	−0.152	0.128
	ES	−0.127	−0.094	−0.126
Internalising beh - rev (std)	DISADV	−0.091**	−0.132*	−0.168***
	DISADV*ES	0.046	−0.082	0.115
	ES	−0.312	−0.223	−0.318
Pro-social beh (std)	DISADV	−0.026	−0.170**	−0.103**
	DISADV*ES	−0.085	−0.022	0.034
	ES	0.069	−0.008	0.057
C. Non-cognitive skills: Teacher-evaluated				
Externalising beh - rev (std)	DISADV	−0.009	0.158**	−0.031
	DISADV*ES	−0.064	−0.089	−0.151
	ES	−0.404	−0.431*	−0.282
Internalising beh - rev (std)	DISADV	−0.050	0.053	−0.006
	DISADV*ES	0.158	0.293	−0.023
	ES	−0.431*	−0.423*	−0.297
Pro-social beh (std)	DISADV	0.026	−0.108	−0.049
	DISADV*ES	0.271	0.214	−0.142
	ES	−0.459	−0.432	−0.352

Notes: In panels A and B, sample size is 3,344. In panel C, sample size is 2,791. All regressions control for state dummies and age in months dummies (interacted with state). Standard errors are robust to heteroscedasticity. *denotes statistical significance at the 10% level, **denotes statistical significance at the 5% level, and *** denotes statistical significance at the 1% level.

Table 4: IV model estimates: sub-sample of mothers working 25 hours per week or less

		Low Maternal Education	NESB	Single mother
A. Cognitive skills				
Vocabulary (std)	DISADV	−0.306***	−0.209*	−0.212*
	DISADV*ES	0.502*	0.033	0.508
	ES	−0.165	−0.010	−0.008
Problem solving (std)	DISADV	−0.204***	0.081	−0.187*
	DISADV*ES	0.112	0.481	−0.234
	ES	0.239	0.110	0.346
B. Non-cognitive skills: Parent-evaluated				
Externalising beh - rev (std)	DISADV	−0.171**	−0.161	−0.548***
	DISADV*ES	−0.108	−0.212	0.413
	ES	−0.104	−0.136	−0.220
Internalising beh - rev (std)	DISADV	0.001	0.015	−0.506***
	DISADV*ES	−0.016	0.033	0.205
	ES	−0.158	−0.196	−0.107
Pro-social beh (std)	DISADV	−0.068	0.025	−0.260**
	DISADV*ES	0.092	−0.253	−0.111
	ES	−0.193	−0.261	−0.341
C. Non-cognitive skills: Teacher evaluated				
Externalising beh - rev (std)	DISADV	−0.081	0.117	−0.169
	DISADV*ES	0.192	−0.209	−0.306
	ES	−0.447	−0.288	−0.529
Internalising beh - rev (std)	DISADV	−0.053	−0.062	−0.255*
	DISADV*ES	0.388	0.178	−0.016
	ES	−0.735*	−0.590*	−0.732*
Pro-social beh (std)	DISADV	0.087	−0.234	−0.139
	DISADV*ES	0.296	0.549	0.273
	ES	−0.505	−0.198	−0.366

Notes: In panels A and B, sample size is 1,588. In panel C, sample size is 1,261. All regressions control for state dummies and age in months dummies (interacted with state). Standard errors are robust to heteroscedasticity. *denotes statistical significance at the 10% level, **denotes statistical significance at the 5% level, and *** denotes statistical significance at the 1% level.

Table 5: IV model estimates using direct measures of disadvantage: full sample

		Less than 30 Books at Home	Low Parental Involvement
A. Cognitive skills			
Vocabulary (std)	DISADV	−0.496***	−0.183***
	DISADV*ES	−0.070	−0.001
	ES	0.358*	0.398*
Problem solving (std)	DISADV	−0.158***	0.016
	DISADV*ES	−0.097	−0.159
	ES	−0.036	0.010
B. Non-cognitive skills (parent-evaluated)			
Externalising beh - rev (std)	DISADV	−0.340***	−0.203***
	DISADV*ES	0.199	0.414***
	ES	−0.356*	−0.437*
Internalising beh - rev (std)	DISADV	−0.372***	−0.172***
	DISADV*ES	0.336*	0.340**
	ES	−0.419**	−0.494**
Pro-social beh (std)	DISADV	−0.103	−0.123***
	DISADV*ES	0.009	0.101
	ES	−0.071	−0.131
C. Teacher-evaluated			
Externalising beh - rev (std)	DISADV	−0.023	−0.041
	DISADV*ES	0.004	−0.009
	ES	−0.569***	−0.469**
Internalising beh - rev (std)	DISADV	0.000	0.030
	DISADV*ES	0.024	−0.249
	ES	−0.331	−0.322
Pro-social beh (std)	DISADV	−0.132*	−0.102**
	DISADV*ES	0.250	0.268
	ES	−0.418	−0.538*

Notes: In panels A and B, sample size is 3,529. In panel C, sample size is 3,113. All regressions control for state dummies and age in months dummies (interacted with state). Standard errors are robust to heteroscedasticity. *denotes statistical significance at the 10% level, **denotes statistical significance at the 5% level, and *** denotes statistical significance at the 1% level.