# Intra-household Resource Allocation:
# Do Parents Reduce or Reinforce Child Ability Gaps?

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**Abstract**

Do parents invest more or less in their high ability children? We provide new evidence on this question by comparing observed ability differences and observed investment differences between siblings living in the United States. To overcome endogeneity issues we use sibling differences in handedness as an instrument for cognitive ability differences. We find that parents invest more in high ability children, with a one standard deviation increase in child cognitive ability increasing parental investments by approximately one-third of a standard deviation. Consequently, differences in child cognitive ability are enhanced by differential parental investments.

Keywords: Children, Cognitive Ability, Parental Investment, Handedness

JEL Classifications: D13, J1

# 1. Introduction

Suppose there are two children in a family and their parents observe that one child is developing more slowly in terms of cognitive skills or ability relative to the other child. The question that arises is: Do parents attempt to reduce these observable differences by investing more resources, in terms of time and/or money, in the less able child or do they act to reinforce the differences? Another way of posing this question is whether parents are more motivated by efficiency or equality concerns when deciding on resource allocations between their children (Becker and Tomes, 1976; Behrman et al., 1982; Rosenzweig and Schultz, 1982). The answer to this question is important because it has direct implications for the intergenerational transmission of human capital and the effectiveness of policies aimed at reducing child inequalities (see, for example, Behrman, 1994; Altonji et al., 1997; Del Bono et al., 2008).

Even though researchers have continuously studied intra-household investment decisions for the last 30 or so years,[1] few have directly examined how parental investments respond to cognitive ability differences between siblings, with the majority instead focused on health differences, especially within families in developing countries. Given the substantial investments made by parents and governments in child development, this relationship requires further consideration. Furthermore, in the existing literature there is no clear consensus on what is the dominant parental motive for within family resource allocation decisions. Some studies find evidence of compensatory behavior, while others find evidence of reinforcing behaviour. For example, Rosenzweig and Schultz (1982) analyze parental allocations in response to differential boy-girl survival data from rural India and conclude that compensating parental investment is not the dominant behavior, but rather parents act to reinforce initial differences. Behrman et al. (1982) on the other hand conclude that parents care about children's earnings inequality and tend to provide more additional resources to the less able child than is consistent with an investment model. More recently, Li et al. (2010) find evidence of multiple motivations at work in China, providing evidence of altruism, favoritism and guilt, towards the children (one of the twins) who experienced more years of rustication following Mao's mass Send-down

[1] Many empirical studies that have shed light on this topic including Griliches (1979), Rosenzweig and Schultz (1982), Rosenzweig and Wolpin (1988), Pitt et al. (1990), Behrman et al. (1994) and more recently Ayalew (2005), Del Bono et al. (2008), Datar et al., (2010) and Li et al. (2010). To control for unobserved family characteristics the literature is mostly based on samples of twins (e.g. Behrman et al., 1982; Royer, 2009; Li et al., 2010) or siblings (e.g. Rosenzweig and Wolpin, 1988; Quisumbing, 1994; Loughran et al., 2008; Datar et al., 2010), and much of the focus has been on health (such as survival rates, birthweight or weight-for-age) of children in developing countries (e.g. Rosenzweig and Schultz, 1982; Rosenzweig and Wolpin, 1988; Pitt et al., 1990). However, some recent research has focused on health endowments in developed countries (e.g. Del Bono et al., 2008; Loughran et al., 2008; Hsin, 2009; Kelly, 2009; Royer, 2009; Datar et al., 2010). More generally, see Almond and Currie (2010) for a review of this research.

Movement in the 1960s and 1970s. These children received higher parental transfers despite having higher earnings.

The conclusions are equally mixed from a series of more recent studies looking at health investments in developed countries. Datar et al. (2010) study a sample of US siblings to see if parents reinforce differences in initial health. They find that normal birth weight children are 5-11% more likely to receive early childhood investments than their lower birth weight siblings. In contrast, Loughran et al. (2008) find that the parents in the NLSY invest more in the siblings with low birth weight. Royer (2009) finds no evidence either way, with parental investments in neonatal care not varying between twins who differed in their birth weight. Similarly, Kelly (2009), examining in-utero exposure of children in the UK National Child Development Study (a 1958 birth cohort) to the Asian influenza pandemic of 1957, finds no evidence in favor of either parental resource reinforcement or equalization.

Importantly, in the case of health investment decisions, at-birth health measures, such as birth weight, can provide a relatively 'clean' measure of initial health differences; however, there is no equivalent measure of initial cognitive ability, making endogeneity the key empirical challenge in this literature (cf. Rosenzweig and Wolpin, 1988; also see Del Bono et al., 2008; and Li et al., 2010, for detailed discussions). In particular, parental investments are very likely to have a direct effect on the cognitive ability of the child, leading to reverse causality, even at a very early age. Additionally, investments made by parents are unlikely to be independent of unobserved characteristics shared by both the parents and the child, such as genetic dispositions or a culture of learning within the family, which makes variation across households difficult to interpret. Moreover, as noted by Almond and Currie (2010), using sibling fixed effects models, while controlling for common family characteristics, does not control for the possibility that children within the family differ in unobservable ways. These issues point towards the combined use of a family fixed-effect estimator and child-specific instrumental variables as an appropriate empirical methodology, as this method is able to control for reverse causality, common family unobserved characteristics and child-specific unobserved characteristics (Del Bono et al., 2008). With a valid instrument the pervasive issue of measurement error is also addressed.

In this paper we make a contribution to the literature on intra-household allocation by establishing the effect of initial cognitive ability differentials on subsequent parental investments, using sibling data from the National Longitudinal Survey of Youth (NLSY). This study therefore provides a strong complement to the much larger literature that has focused on childhood health endowments. Importantly, we are able to estimate the causal effect of child cognitive ability on parental cognitive investments because of our instrumental variables within-family approach. For identification we use the finding that a child's handedness is a strong determinant of his cognitive ability, with left-handed children achieving significantly lower test scores. This is coupled with the finding that parental socioeconomic characteristics at birth have no explanatory power in predicting whether a child is born favoring his left or right-side. Child handedness therefore provides a source of random within-family variation in child cognitive development (Frijters et al., 2009; Johnston et al., 2009; Llaurens et al., 2009). We provide evidence to support these claims.

Our main finding is consistent with the emerging economics literature on the persistence of initial differences between children: we find that a one standard deviation increase in cognitive ability increases the amount of cognitive investments by roughly one third of a standard deviation, implying that initial differences are reinforced by differential parental investments. This finding has implications for education policy aimed at reducing inequalities in child cognitive development, because it suggests that parents could substitute investments between siblings to counteract equalizing public investments. The success therefore of public interventions depends upon whether the cognitive resources supplied by governments are of a type that parents are unable to supply themselves, and whether the cognitive resources are able to change elements of children's cognitive endowments that parents respond to.

## 2. Data and Descriptive Statistics

Our empirical analysis uses data from the NLSY-C, a survey focussed on children whose mothers are respondents in the National Longitudinal Survey of Youth 1979 (NLSY79). The NLSY79 began with a sample of 12,686 Americans who were 14-21 years old in January 1979. These initial respondents were then interviewed annually from 1979 to 1994, and biennially from 1994 to 2006. In 1986 the NLSY-C began and in every even-numbered year since, the NLSY-C collected detailed information on all children born to and living with a female NLSY79 respondent, including information on cognitive development and cognitive resource allocations. The child cognitive development data have been widely used in a number of different literatures (see Argys et al., 1998; Guo and Harris, 2000; James-Burdumy, 2005; and Case and Paxson, 2008), as have the cognitive resources data (see Bradley et al., 2001; Todd and Wolpin, 2007; and Cunha and Heckman, 2008).

To measure children's handedness the NLSY-C asks mothers about their child's hand preference when writing, brushing teeth and throwing a ball.[2] Three tasks are used because left- and right-handed

[2] These questions were asked in surveys between 1996 and 2006, and so children with multiple responses are allocated handedness based on their latest response (average age handedness is measured is 13). Using the latest response limits measurement error arising from the fact that a small number of children may have not fully revealed their dominant handedness at an early age. Even so, if we estimate our main IV-FE model using handedness based on the earliest recorded

categories are not distinct: most individuals reveal a hand preference for a given task, but it is not always the same hand for each task (Salmaso and Longoni, 1985). We construct a continuous measure of left-handedness by assigning a value of 0 for always right-handed, 0.25 for mostly right-handed, 0.5 for both hands, 0.75 for mostly left-handed, and 1 for always left-handed, and averaging the three responses. This approach corresponds to the theory that handedness is a continuum, with the strength of left- and right-handedness varying across people (Bryden and Steenhuis, 1991; Annett, 2002). According to our measure, roughly 80% of children are strongly right-handed (a value equal to 0), and roughly 5% of children are strongly left-handed (a value equal to 1). The mean value of our handedness measure is 0.107, which is consistent with the finding that around 10% of the population is mainly left-handed.[3]

A measure of overall cognitive ability is generated by averaging scores on the Peabody Individual Achievement Tests (PIAT) of mathematics and reading comprehension at each age. The PIAT of mathematics assesses early mathematic skills, such as recognizing numerals, and also more advanced concepts in geometry and trigonometry. The PIAT of reading comprehension assesses the child's ability to derive meaning from sentences that are read silently. These tests have been found to be correlated with alternative measures of cognitive ability, and each has high completion rates – see Baker et al. (1993) for a detailed discussion of each test. To aid in the interpretation of subsequent estimation results, the average test score has been transformed to have a mean of zero and a standard deviation of one. The tests were repeated every two years since 1986 and so for some children we have five separate measures of cognitive ability (for example at ages 5, 7, 9, 11 and 13).

The NLSY-C quantifies the children's home environment using the Home Observation Measurement of the Environment – Short Form (HOME-SF) survey. The HOME-SF scale was constructed by Bradley and Caldwell (1980, 1984) in order to assess the levels of cognitive stimulation and the levels of emotional support that children receive from their parents and their home environment.[4] In a review, Totsika and Sylva (2004) state that "HOME is without doubt the most commonly used environmental assessment instrument in developmental research" and that "research has proved the instrument's validity in describing the home environments of children at risk and revealing the effect of home experiences in developmental outcomes".

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response, the estimated cognitive ability effect is similar in value and remains statistically significant ($p$-value = 0.045). Thus, our results are robust to the age of measurement.

[3] It has been found that the proportion of people who are left-handed equals 11 percent in Canada (Bryden et al., 1997), 12 percent in the U.S. (Ruebeck et al., 2007), and 12 percent in the U.K. (Denny and O'Sullivan, 2007).

[4] For additional information on the validity and reliability of the HOME-SF see the 1996 *Users Guide*, and citations therein. In addition, see Appendix tables A1-A3 in Todd and Wolpin (2007), which compare the average scores by race/ethnicity for the individual items of the cognitive home scale for children in different age ranges.

The HOME survey instrument includes items obtained by maternal report and interviewer observation, with the number and specific items varying by age of the child (ages 0-2, 3-5, 6-9, and 10 and above). The items reflect the diversity in investment types, with questions measuring the quantity of physical resources (e.g. “About how many children’s books does your child have of his/her own?” ), parental time-investments (e.g. “Do you or have you helped [your child] with the alphabet?”) and procurement of additional tutelage (e.g. “Does child get special lessons or belong to organizations that encourage activities?”). The HOME survey items also differ in the extent to which they measure idiosyncratic child investments (e.g. “How often do you read stories to your child?”) versus investments received by all children within a family (e.g. “Does family get a daily newspaper?”).

The HOME cognitive-stimulation and emotional-support scores are created by summing the modified responses to individual questions, such that each possible response ranges from 0 to 1. Throughout our analysis we use a log transform of these HOME scores such that the transformed scores are increasing in stimulation and support, and have a mean of 10 and a standard deviation of one. As for the test scores, the HOME survey was repeated every two years and so we have up to five separate measures of cognitive resources for each child.

Figure 1 shows the kernel density estimates of our cognitive resources index by handedness. The cognitive resources received by strong left-handers are lower than the cognitive resources received by strong right-handers. Assuming there is no direct effect of handedness on investments, this provides evidence that in the cross-section children with lower cognitive ability receive lower cognitive resources. Figure 2 shows the distribution of differences between siblings at the same age in terms of the cognitive resources they receive. Clearly, large degrees of variation exist between siblings, with a large proportion of children receiving one standard deviation more or less cognitive resources than their siblings at the equivalent age.

The children are described by handedness in Table 1. Column 3 presents mean resources and development outcomes, and mean child and family characteristics for all children who have non-missing handedness and non-missing outcome information. Column 4 presents mean values for the subsample of children who have a surveyed sibling with different handedness than themself. The total number of children in the subsample equals 2,318. It is this subsample that is used to identify the impact of handedness on cognitive ability, and hence the effect of cognitive ability on resource allocation.

The table indicates that the sibling subsample is relatively similar to the full sample of children. The most significant difference between samples is the number of younger and older siblings, which is not surprising since sample selection requires there be at least two siblings with different degrees of handedness. There are also small differences in the proportions who have had a premature birth, a caesarean section birth, and a mother who drank during pregnancy. Mean resources and ability are also lower for the sibling subsample than for the full sample, which can be explained by the differences in household size and by the negative effect of left-handedness on cognitive ability. The bottom panel of Table 1 contains summary statistics for family-level characteristics that are not included as covariates in the family-level fixed-effect models. These statistics are included in order to further explore the differences between the full and sibling samples so that we can better understand the external validity of our estimates. The sample means show that families in the sibling sample are more likely to be African-American and have lower-income, and that mothers in the sibling sample have lower education and cognitive ability.

Figure 3 shows the raw relationship between average cognitive ability and average cognitive resources for each percentile of the cognitive ability distribution. The graph reveals a strong positive relationship that is almost perfectly linear (slope = 0.34), except at the far left tail of the distribution where the relationship is flat, suggesting a minimal level of cognitive investment. In Section 4 we investigate whether this relationship remains once we account for observable and unobservable differences between children.

## 3. Methodology

### 3.1 Model

Our empirical approach is based on a within family comparison of child cognitive ability and parental cognitive investments, or in other words, a family fixed-effects regression model of parental investments (*PI*). For child *i* in family *j* at time *t*, we estimate:

$$PI_{ijt} = \alpha_j + X_{ijt}\beta + CA_{ijt}\delta + \varepsilon_{ijt} \qquad (1)$$

where $\alpha_j$ is a family fixed effect, $X_{ijt}$ is a vector of characteristics that vary across children in the same family, $CA_{ijt}$ is the child's measured cognitive ability, and $\varepsilon_{ijt}$ is a random error term. The coefficient $\delta$ is the parameter of primary interest and represents the impact that cognitive ability has on parental investments. The $X_{ijt}$ vector includes as many observable differences between siblings as our data allow.

Consistently estimating $\delta$ in equation (1) is complicated by the fact that cognitive ability differences between siblings are not randomly determined. Research by Cook and Evans (2000), Todd and Wolpin (2007), Cuhna and Heckman (2008), amongst others suggest that parental investments have a direct positive impact on child cognitive ability. For example, Todd and Wolpin (2007) find that racial differences in parental investments account for about 10-20% of the black-white and the Hispanic-white test score gap in math and reading. Therefore, a positive $\delta$ estimate may merely reflect the positive impact of parental investments on cognitive ability. Estimation is further complicated by the possibility of unobserved differences between siblings that are related to both ability and investments. For example, if one child is inherently more interested in intellectual activities than his or her sibling, it is likely the child will develop a superior cognitive ability as well as receive greater parental (demand-led) investments.

The strong likelihood of endogeneity bias motivates our use of an instrumental variables (IV) estimation procedure. The first-stage equation in this procedure is:

$$CA_{ijt} = \theta_j + X_{ijt}\gamma + Z_{ij}\pi + u_{ijt} \tag{2}$$

where $\theta_j$ is a family fixed effect, $Z_{ij}$ is an instrumental variable representing the child's degree of left-handedness, and $u_{ijt}$ is a random error term. Importantly, the inclusion of the family fixed-effects in equations (1) and (2), coupled with our IV strategy, implies that identification of $\delta$ is driven by parental investment variation within families where differences in handedness has caused variation in child ability.

In addition to overcoming any endogeneity bias, our IV approach is useful for overcoming attenuation bias related to measurement error in cognitive ability. Our measure of cognitive ability comes from elaborate test score data; however, even the best test is still only a proxy for the cognitive ability that is observable to parents. If parents have a somewhat different perception of their child's cognitive ability than the test scores indicate, measured child-development will suffer from considerable measurement error. If classical, this measurement error can be overcome by our IV approach. The instrumentation is particularly important in the family fixed-effects specification since the attenuation bias arising from measurement error is known to be amplified if one removes the cross-sectional information via fixed-effects (see Bound and Solon, 1999).

### 3.2 Handedness and Cognitive Ability

To legitimately use handedness as an instrument for cognitive ability, handedness must be a source of exogenous variation that significantly affects cognitive ability but does not directly affect parental investments. The first requirement, that handedness is strongly correlated with cognitive ability, can be easily tested. Figure 4 shows kernel density estimates of cognitive ability graphed separately for strongly left- and right-handed children. The figure reveals a sizeable difference in cognitive ability by handedness, with the left-handed distribution more negatively skewed than the right-handed distribution. In other words, left-handedness substantially raises the probability that a child will be poorly developed. Importantly, these significant ability differences are not diminished when we control for family and child characteristics. Furthermore, this is not a finding particular to children in the NLSY. Johnston et al. (2009) using Australian data, Gregg et al. (2008) using English data, and Resch et al. (1997) using German data, also find that left-handedness is associated with lower cognitive development.

The second requirement, that handedness is exogenously determined, though more difficult to confirm is supported by the literature on handedness. The psychology literature proposes two main theories for the determination of handedness that are consistent with the observed differences in cognitive ability (for an excellent review of the literature see Llaurens et al., 2009). One important theory is that handedness is genetically determined through a complex and as yet not fully understood interplay between alleles (Bryden et al., 1997). The best known genetic theory is that handedness is determined by one gene with two alternate forms (called alleles), one dominant and one recessive, and that right-handed individuals who receive one of each allele have a cognitive advantage over left-handed individuals who receive two recessive alleles (Annett, 1985; Annett and Manning, 1989). Therefore, under this theory the lower ability of left-handers is "naturally" occurring and is not caused by parents' socioeconomic status, education, health, or behavior. What might drive the cognitive effect under this genetic model is still a matter for speculation, though it could be related to changes in hemispheric dominance, which affects normal functional localisation through crowding (Lidzba et al., 2006) and/or changes in the inter-hemispheric transfer of information (Häberling et al., 2010; Welcome et al., 2009).

One implication of the genetic model is that mothers and fathers of left-handed children are more likely to be left-handed themselves. This suggests that left-handed parents may have different socioeconomic status, cognitive ability or even parenting styles than right-handed parents. Such a difference, however, is not problematic for our analysis. Our focus is on comparisons between siblings, and obviously parental socioeconomic status, cognitive ability and parenting styles are common to siblings. In other words, any unobserved parental characteristics are captured by our family fixed-effect.

The second main theory for the determination of handedness is that left-handedness is the result of exogenous factors operating on the child before or during childbirth (pathological left-handedness).

For example, an elevated incidence of left-handedness has been reported in children who have suffered severe bacterial meningitis (Ramadhani et al., 2006) and for females with early neurologic insult (Miller et al., 2005). These theories predict lower academic achievement in a sub-group of left-handers, not as a result of their hand preference per se, but because of the brain insult that caused a shift in hand preference and decreased cognitive ability. Johnston et al. (2009, 2012) and Nicholls et al. (2012) test this theory by regressing handedness on extensive sets of explanatory variables representing birth stress and prenatal and postnatal investments and find no strong evidence in support of this theory. Similarly, Bailey and McKeever (2004) find that of 25 potential pregnancy or birth stressors, maternal age is the only variable to have any association with left-handedness.

We investigate the possibility that handedness is determined by differential pre-natal and post-natal investments by comparing differences between siblings in their handedness and a range of characteristics that describe the pregnancy and immediate post-natal health investments (e.g. breast feeding). The results in Table 2 indicate that the only factor significantly related to handedness is gender – it is well known that boys are more likely to be left-handed. All the variables related to pre- and post-natal health are not significantly different from zero at the 5% level. In fact, using an $F$-test we are unable to reject the null hypothesis that all of the factors (apart from gender) are jointly insignificant ($F = 0.91$).

The final requirement of instrument validity is that handedness impacts upon cognitive resources only through its effect on cognitive ability. In other words, our instrument is invalid if parents spend more (or less) time with their left-handed children due to factors unrelated to cognitive development (e.g. health or behavioral differences). Johnston et al. (2012) use NLSY data and sibling fixed-effect models to investigate the effects of handedness on childhood illness, injury, symptoms of behavioral and emotional problems, and cognitive ability test scores. They find that handedness has near-zero, statistically insignificant effects on illness, injury and behavioral-emotional problems, and large, statistically significant effects on test scores, particularly mathematics and reading comprehension scores. Similarly, Gregg et al. (2008) using English data find that left-handed children score significantly lower on cognitive ability tests than right-handed children, but no worse on indices of social-development and behavioral problems. Pekkarinen at al. (2003) analyze a sample of approximately 8,500 men and women from Finland, and find no significant difference in injury involvement between left- and right-handers. Hence, if there are health or behavioral-emotional differences due to handedness, they appear to be small. Even so, in Section 4.2 we show that our results are robust to the inclusion of indicators of child health and behaviour.

Another possibility is that parents spend more time with their children trying to "coax" them into right-handedness, because being left-handed has traditionally been associated with cultural stigma. For example, Teng et al. (1976) observed in China a significant pressure to eat and write right-handed, and Vuoksimaa et al. (2009) find evidence of forced right-handedness in retrospective responses in Finish data, particularly for older cohorts. This is likely to be a minor issue with our young US sample, and the data itself provides some evidence that forced handedness is infrequent. In 1998, 2137 older children from the NLSY-C were asked, "As a child, were you ever forced to change the hand with which you write?" Only 2.6 percent of the children replied yes. However, it is important to note that if parents were to spend more time with their left-handed children coaxing them to becoming right-handed, this would bias on our main results downward.

It is of course impossible to definitively declare that handedness impacts upon cognitive resources only through its effect on cognitive ability, as there are many unanswered questions in the scientific field of laterality. For example, it is possible the same genetic process that determines handedness also determines other individual characteristics. However, in our regression models we control for a number of differences between siblings that determine cognitive resources, and we also conduct a number of robustness tests, and our estimated effects are always robust.

### 3.3 Auxiliary assumptions

Our empirical methodology is based upon the difference in cognitive development between siblings of different ages due to variation in handedness, and generates a single estimated effect of cognitive ability on parental investments across different ages. Inherent in this approach are three simplifying assumptions, each of which is important for the interpretation of results. First, the level of development of one sibling is assumed to be unaffected by the cognitive development other siblings. Effectively, this assumption rules out peer effects in learning and the effects of time constraints arising from other siblings also receiving parental time investments. The latter implication is reasonable if the opportunity cost of time is roughly linear at the margin of parental investment trade-offs, because then there is no cross-over effect from spending time on other siblings. The former implication of no peer effects is primarily a matter of interpretation. Our estimate $\delta$ in equation (1) equals the difference in parental investment between two siblings divided by the difference in their cognitive development, meaning we cannot distinguish between the effects of higher own development and lower sibling development. A positive value of $\delta$, however, still reflects parental reinforcing behavior and a negative value of $\delta$ still reflects parental compensating behaviour.

Second, it is assumed that, in expectation, the relative position of individuals in the cognitive development distribution does not change with age. This assumption is mandated by the way the cognitive scores are measured, which is relative to the age-specific distribution of a test: cognitive development is not measured on an absolute scale and so our measures of cognitive development and therefore the differences between siblings essentially measures the relative distance between siblings in their respective age-distributions. Since sibling pairs differ in how far apart they are in age, pooling them together as we do requires us to assume that the distance between them in the cognitive development distribution, brought about by handedness differences, does not change in expectation over time. Finally, the responsiveness of the parents to the cognitive development of their children is assumed to be fixed across ages. This assumption is required for equation (1); otherwise we would need age-specific values of $\delta$. We can relax this assumption by looking for different effects at different ages, but the sample sizes are such that we cannot avoid pooling children in our analysis.

It is important to note that these three assumptions are not specific to this application. Most researchers who pool children of different ages and use cognitive development scores will rely on similar assumptions. We see them as a framework within which we can interpret the estimates of the linear equations that we use and that are the dominant tools in this literature. Within these assumptions, one bypasses the need to measure cognitive development at birth because, by the second assumption, the difference in development between siblings with different levels of handedness persists to later ages. Ideally of course, the perfect solution would be to observe cognitive development from birth to adulthood, but we know of no accepted procedure to do so, particularly since at very early ages there are catch-up processes at work whereby children have differently timed development surges. The fact that we cannot separately identify peer effects is a limitation of this analysis, but to do so would require additional instrumental variables, which is a difficult proposition and something for future research.

## 4. Results

### 4.1 Main Results

Our main estimation results are presented in Table 3. For each specification we have restricted the sample to children with no missing information and to children with a surveyed sibling with different handedness than themselves. Moreover, each specification includes covariates representing gender, number of older and younger siblings, mother's age at birth, birth weight, premature birth, Caesarean-section birth, whether breastfed, and whether mother smoke or drank during pregnancy. Not shown, but also included in all models, are full sets of age and year dummies. The covariates allow for a wide range of observable differences between siblings that may be associated with resources received.

Cross-sectional OLS estimates are reported in Column 1. The estimated cognitive ability coefficient equals 0.251, indicating that a one standard deviation increase in cognitive ability increases cognitive resources by approximately one-quarter of a standard deviations. However, this estimate is likely to be biased. It will be biased upward if cognitive ability is positively influenced by cognitive investments, and biased downward if the cognitive ability test-score measure is a noisy proxy for the cognitive ability that is observable and important to parents.

In Column 2 we present family fixed-effect estimates, in which differences in cognitive ability between children are compared to differences in cognitive resources received. This specification is similar to the fixed-effect models used by Hsin (2009) and Datar et al. (2010) in their analyses of parental investment responses to birth weight differences between siblings. The estimated cognitive ability effect is still significantly positive, but much smaller than the cross-sectional relationship. This could be due to diminished endogeneity bias or increased attenuation bias.

Before presenting our preferred IV-FE estimates, we present a reduced form analysis of cognitive resources in Column 3. We find that left-handed children receive significantly less resources than their right-handed siblings, suggesting cognitive ability is positively related to cognitive resources. An alternative explanation for the significant reduced form effect is that parents incorrectly perceive their left-handed children as having lower ability, perhaps because of the historical social stigma against left-handedness, and react by providing lower resources, which in-turn causes lower measured ability (i.e. a self-fulfilling prophecy). This explanation would render the IV strategy invalid, and so in our robustness section we re-estimate the empirical models using the youngest measure of cognitive ability available and find it has little effect on our IV-FE results.

Column 4 presents the first-stage estimates. Handedness is strongly related to within-family differences in cognitive ability, with left-handed children scoring 0.149 standard deviations lower on the cognitive ability measure than their right-handed siblings. The associated *F*-statistic equals 12.32, which is unusually high for an IV model with fixed-effects. Kleibergen and Paap's (2006) rank test for identification, which is particularly useful when employing a robust variance estimator, also suggests that the model is well identified; the Chi-squared statistic equals 11.92 and the *p*-value equals 0.0006.

Finally, in Column 5 we present our IV-FE estimates. As explained previously, this is our preferred specification because it is robust to reverse causality and classical measurement error. The estimated effect, which has a *t*-statistic of 2.18, suggests that an increase in cognitive ability by one standard deviation increases cognitive resources by 0.34 standard deviations. This estimate coincides exactly with the raw correlation of 0.34, and is slightly larger the OLS estimate of 0.25, but is much larger than the fixed-effect estimate.[5] A possible explanation for this result is that measurement error is non-trivial and is amplified in the fixed-effect model. As discussed in Section 3.1, test scores are only a proxy for the component of cognitive ability observable to parents. For example, parents may respond predominantly to information obtained from the children's teacher and school, which will not be perfectly correlated with the NLSY-C test score data. An alternative explanation is that parental investment responses are heterogeneous and that the population that 'complies' with our handedness instrument is particularly responsive to ability differences. However, given the genetic determination of handedness, it is difficult to surmise why complying households would be very different from the average household.[6]

To better understand the ways in which parents allocate their resources between their children, we also investigate the effects of cognitive ability on dimensions of the HOME cognitive resources index. The HOME survey necessarily varies with the child's age, however, there is a set of seven age-consistent questions relating to cognitive resources. These questions relate to whether the child: (i) has access to a musical instrument; (ii) is encouraged to start and keep doing hobbies; (iii) receives special lessons (e.g. music, art, dance, etc.); (iv) has been taken to a museum (e.g. scientific, art, historical, etc.); (v) has been taken a musical or theatrical performance; (vi) has access to books (and the number of books); (vii) and has access to a daily newspaper. There is no clear *a priori* categorization of these investments, and so we instead conduct a principal components factor analysis on the seven items, which recovered two principal components (eigenvalues equal 1.93 and 1.43). The first predicted factor loads heavily on items (iv) and (v), trips to museums and theatrical productions, and the second predicted factor loads on all remaining items, but particularly on items (i) and (iii), access to musical instrument and receives special lessons. The estimated IV-FE cognitive ability coefficients for factors 1 and 2 equal 0.045 and 0.204, respectively. Given the standardisation of the predicted factors and cognitive ability, this suggests that a one standard deviation increase in cognitive ability increases factor 1 by 4.5% of a standard deviation and increases factor 2 by 20% of a standard deviation.

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[5] The estimated effects from 2SLS models (exploiting across family rather than within family variation) equal 0.143 for the full sample and 0.197 for the sibling subsample (the corresponding instrumental variable *F*-statistics equal 31.6 and 23.7). Given that families with left-handed children are not on-average identical to families with right-handed children, we believe the IV-FE estimates are more robust and hence preferable to the 2SLS estimates.

[6] We have also explored the role of gender by re-estimating our IV-FE model for males and females separately. The subsamples of same-sex siblings with different handedness are quite small and so the estimated effects are less precisely estimated; nevertheless, the estimated female effect is larger than the estimated male effect, suggesting that reinforcing behaviour may be more important for girls than boys. In addition, an alternative model specification that includes birth order dummy variables (e.g. first born, second born, etc.) provides a statistically significant IV-FE estimated cognitive ability effect that is slightly smaller (0.337) than that presented in column 5, Table 3.

A direct comparison of our results with those from other studies is not possible because to the best of our knowledge no previous study has used data from a developed country to estimate the effect of child test scores on parental cognitive investments, and more generally no previous study has used an IV-FE approach to examine the issue of intra-household resource allocation.[7] The closest study to ours is Datar et al. (2010), as it represents one of the few studies that uses a direct measure of endowments; most others treat endowments as unobservable to the researcher. Datar et al. (2010) estimate a family fixed-effect model using NLSY data and find that higher birth weight children are significantly more likely than their lower birth weight siblings to be breastfed, to be taken for well-baby visits, and to receive vaccines. Interestingly, their OLS estimates are much closer to their FE estimates, suggesting that endogeneity and attenuation bias is less severe when examining the role of an at-birth endowment measure compared with a time-varying after-birth endowment measure, such as cognitive test scores.

Another similar study is Frijters et al. (2009), which investigates the relationship between child development and maternal labor force participation using a sample of Australian children aged 4 and 5. Frijters et al. (2009) also controls for the potential endogeneity of child development by using handedness as an instrumental variable. However, they find that lower child development measured for 4 and 5 year olds decreases maternal labor force participation, suggesting compensatory rather than reinforcing behavior. The difference in results could be due to the fact that maternal labor supply is affected differently by cognitive ability than the cognitive resources measured by the HOME survey. It could also be due to the younger age of the sample, and/or different institutional and cultural frameworks in Australia.

More generally, our finding of a strong positive relationship between cognitive ability and cognitive resources is consistent with a number of studies that find that parents reinforce endowments (for examples see Behrman et al., 1982; Rosenzweig and Wolpin, 1988; Pitt et al., 1990; Behrman et al., 1994; Datar et al., 2010, and Akresh et al., 2012). The most commonly proffered explanation for this finding is that parents are concerned with maximizing their children's future wealth and hence invest greater resources in the child for whom the marginal return to investment is highest. Importantly for this explanation, it is usually assumed that the marginal return is highest for better endowed children, which hence generates a reinforcing strategy by parents (Becker and Tomes, 1976).

[7] Del Bono et al. (2008) use an instrumental variable approach in their analysis of household resource allocation, but they instrument for differences in pre-natal inputs between pregnancies using as an instrument prenatal inputs during earlier pregnancies. This is a completely different approach to that used here.

### 4.2 Robustness

We now present some additional robustness tests. First, we investigate whether the estimated cognitive ability effect is smaller if instead of using a time-varying measure of cognitive ability, we use only cognitive ability measured at the earliest observed age, which for most children is either age 5 or 6 (due to the biennial nature of the survey). In other words, the second-stage equation is now the regression of cognitive resources measured at ages 5 to 14, on cognitive ability measured at age 5 or 6. This robustness model tests the proposition that cognitive resources are in fact a function of contemporaneous cognitive ability, *plus* lagged values of cognitive ability and cognitive resources. If this were true then our instrument would violate the exogeneity assumption because it would be associated with the omitted lags. The likely effect of such a violation is that the IV-FE estimate in Table 3 is too large. The estimated effect of ability on resources from the alternative robustness specification is presented in the first row of Table 1. The IV-FE estimate equals 0.305 ($t$ = 1.91), which is only slightly smaller than the IV-FE estimate presented in Table 3.

Next, we re-estimate our model without the long list of covariates (i.e. birth weight, premature birth, Caesarean-section birth, whether breastfed), including only gender and the year/age dummies. Row 2 in Table 4 shows that we get a similar result: the IV-FE estimate equals 0.316 ($t$ = 2.06). This result provides some extra support for our crucial assumption that handedness is exogenously determined (at least within families), because it demonstrates that our estimate is not conditional on the set of control variables used.

As discussed in Section 2, it is important for our IV strategy that handedness affects cognitive resources only through its effect on cognitive ability. Most large-sample empirical studies on this issue support this assumption; nevertheless, we investigate the robustness of our results to the inclusion of additional control variables for whether the child has had any illnesses or injuries requiring medical attention in the past 12 months, and an index of behavioral-emotional problems. We find that the IV-FE estimated cognitive ability effect with these additional controls is 0.336 (Row 3 in Table 4), which is very close to the main IV-FE estimate in Table 3. This finding suggests that even if left-handedness is associated with worse health or behavioral outcomes, it does not impact largely upon the handedness – cognitive ability – cognitive resources relationship.

In Row 4 of Table 4 we investigate the robustness of our results to our definition of handedness, by collapsing our continuous handedness measure into a binary indicator. The estimated effect changes only slightly from our headline figure (0.326 versus 0.338). In Row 5 we investigate the robustness of our results to our definition of cognitive ability. In addition to the Peabody Individual Achievement Tests (PIAT) of mathematics and reading comprehension, the NLSY also includes the PIAT test on reading recognition, which assesses skills such as matching letters, naming names and reading single words aloud; the Peabody Picture Vocabulary Test (PPVT), which assesses receptive vocabulary for Standard American English and provides a quick estimate of verbal ability and scholastic aptitude; and the Memory for Digit Span Test, which assesses short-term memory.[8] If we use a cognitive ability measure equal to the average of all five test scores, the estimated effect is once again almost the same as the main finding (0.351 versus 0.338).[9]

The final row in Table 4 examines which part of the cognitive ability distribution is identifying the main effect. It might be for instance that the actual relationship is non-linear (though this is not suggested by Figure 3) and the findings are mainly due to a particular cognitive ability region. We examine the possibility of non-linearity by defining a low cognitive ability measure that equals one if the child is in the bottom 10% of test scores. The estimates show that left-handed children are 4 percentage points more likely than their right-handed siblings to have a test score in the bottom decile, and that in turn having a test score in the bottom decile means receiving around 1.2 standard deviations less cognitive resources. Therefore, it is not the case that parents compensate for very low ability.

Finally, as an interesting comparison point, we test the effect of cognitive ability on parental 'emotional' resource allocation, which is the alternative index constructed from the HOME survey. An example of a question asked to parents in relation to emotional resources is, "How many times (in the past week) have you shown [him/her] physical affection (kiss, hug, stroke hair, etc.)?". This is interesting because the correlation between the emotional and cognitive resources measures is only 0.38. We might expect that the allocation of emotional resources by parents should not be strongly dependent on the observed ability of the child, as emotional investments are to a larger extent likely to be a measure of the degree to which parents care about their child. Alternatively, we might view emotional resources as an investment by parents into the development of a child's non-cognitive skills. We find no significant relationship between child cognitive ability and the amount of emotional resources given by parents (results available upon request from authors). That is, parents allocate their emotional resources equally across siblings regardless of their cognitive ability.

[8] We do not use all five tests to construct our main measure of cognitive ability because children did not complete all five tests in the same years. Thus, the summary five-test measure changes in composition from year to year.

[9] To attempt to address the potential bias in our estimates that could arise from any forced right-handedness, we have also re-estimated the IV-FE model using only those siblings with a stable hand preference over time. If we restrict the sample to those children who, between first and last handedness measurements have a change of less than 1/6th, which corresponds roughly to moving on one activity (e.g. brushing, throwing, writing) one point on a five-point scale (e.g. from right hand nearly all of the time to right hand more than half of the time), then the estimate is 0.3306. Therefore, eliminating from the estimation those children who switch from left to right (or vice-versa) does not alter our main conclusion.

## 5. Conclusion

A large literature, both theoretical and applied, has contributed to our understanding of the mechanisms underlying intra-household resource allocation over the last 30 years, and one key question in this research is whether or not parents allocate resources aimed at reducing endowment differentials observed between siblings, or whether they allocate more resources to the better endowed child as an investment type model would suggest (Becker and Tomes, 1976). That is, are parents driven mainly by equity or efficiency concerns? While the majority of the literature has focused on parental responses to differences in sibling health endowments, our contribution has been to focus on measured cognitive ability differentials between siblings. In order to overcome the well-known endogeneity issues involved in such an analysis, we have used child handedness as an exogenous predictor of cognitive ability, relying on the result that left-handed children perform worse than their right-handed counterparts across a wide range of cognitive ability tests. Estimating a sibling fixed-effects model, while instrumenting for cognitive ability using child handedness, enables us to simultaneously control for both family and environment characteristics shared by all siblings and idiosyncratic characteristics or traits that differ between siblings.

We show that parents act to reinforce observed child cognitive ability differentials, with a one standard deviation increase in initial cognitive ability leading to one-third of a standard deviation increase in cognitive resources. This suggests the efficiency motive dominates the equity motive, as has been found by a number of previous papers in respect to health differences between siblings. To the extent that our measure of cognitive resources includes items that are likely to be the same across siblings (like whether the family gets a daily newspaper), this already large estimate is likely to be an underestimate of the effect of initial ability on the discretionary part of parental cognitive investments. Interestingly, while we find parents reinforce the allocation of cognitive resources, we do not find this to be the case for emotional resources.

We have also highlighted the practical modelling difficulties that arise when attempting to make strong causal statements in this context. In particular, the circumstances in which it is valid to identify the parental investment function from the cognitive outcomes of siblings of different ages undertaking different tests are by necessity restrictive. A key question for future research is whether a more flexible structural approach that recognises various elements outside of our framework can be developed, such as peer influences, nonlinearities, and age-specific investment strategies. The data requirements for such an exercise are formidable, because it is necessary to have measured outcomes and inputs at every age for all siblings, as well as multiple sources of random variation in order to separate the endogeneity between investments, cognitive development of children, and the interactions between children.

The results we present tentatively suggest that estimates of the effect of cognitive ability on later life outcomes will comprise both a pure biological effect of lower initial cognitive ability, and also the effect of lower parental investments. Such a finding has important policy implications for education policy, where the crucial question is whether government programs are able to provide a complementary investment to that of parents (which Heckman, 2007, calls dynamic complementarities in human capital investment). If government programs aimed at improving child development offer the same type of investments that parents provide themselves, then the dominance of the efficiency motive implies parents would reduce their own levels of investments, nullifying the government program. If, however, government programs offer investments that parents are unable to provide (for instance because the care needed is too specialized), and if the investments are able to improve those cognitive endowments that parents are responding to, then the programs will be particularly effective as they will also stimulate parental investments. Therefore, the main policy implication of our findings is that the success of education policy depends crucially on the type of investments that are provided and how parents react to the subsequent developmental effects.

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Figure 1: Kernel Density Estimates of Left- and Right-handed Children's Cognitive Resources

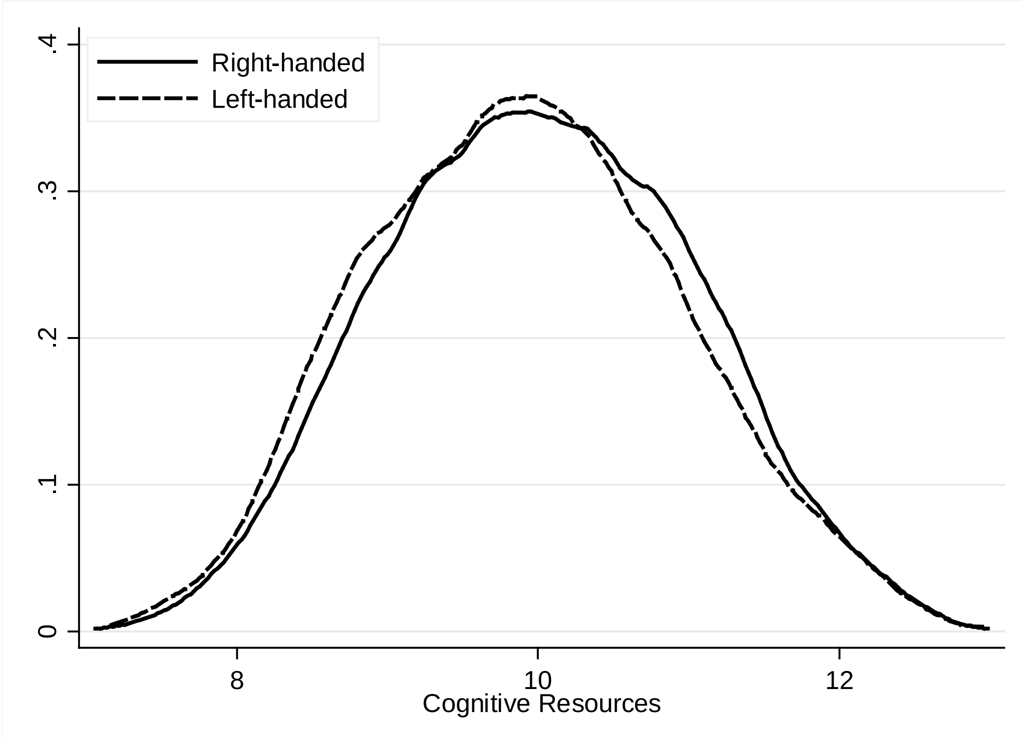


Figure 2: Differences in Cognitive Resources that Siblings Received at the same Age

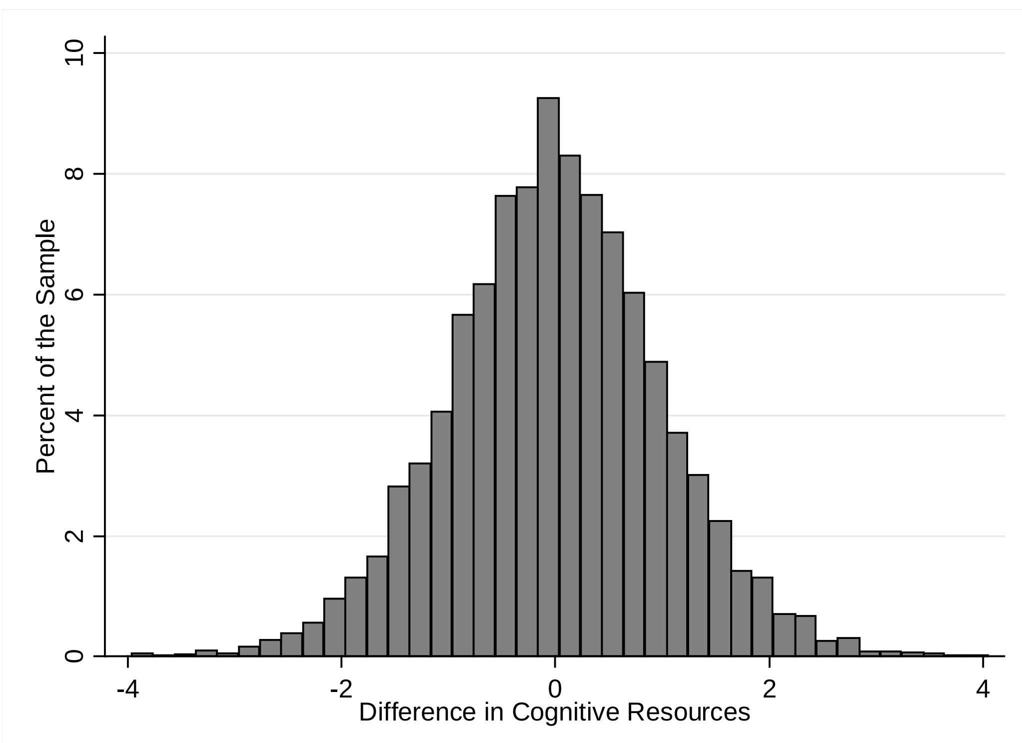

Figure 3: Mean Cognitive Resources and Cognitive Ability for each Percentile of the Cognitive Ability Distribution

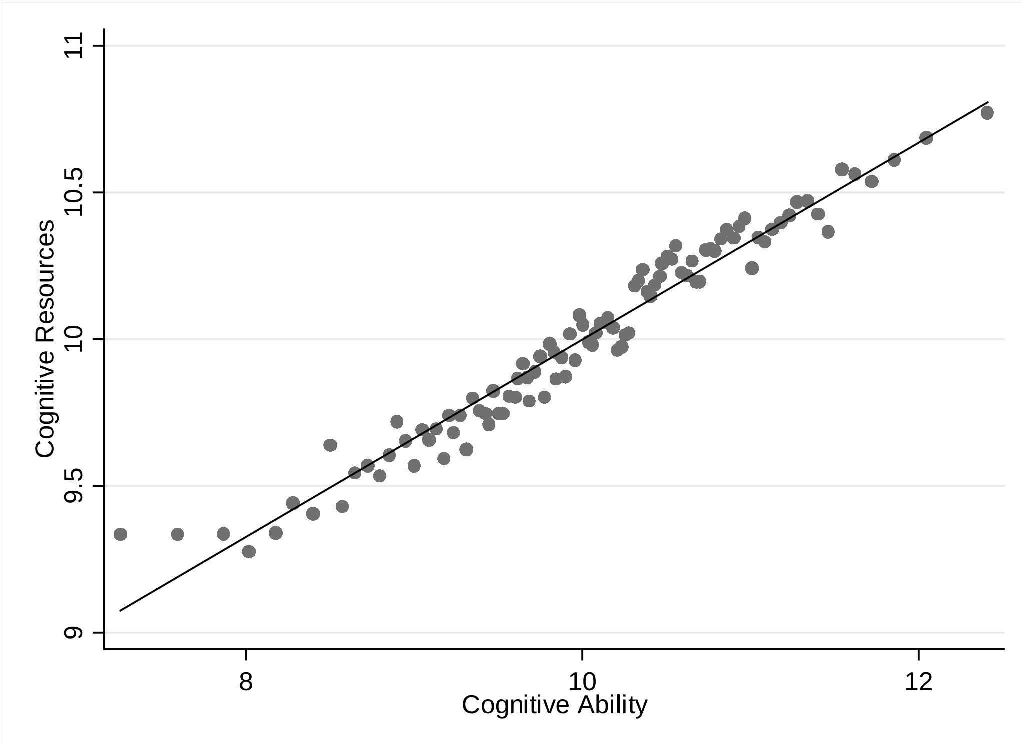


Figure 4: Kernel Density Estimates of Left- and Right-handed Children's Cognitive Ability

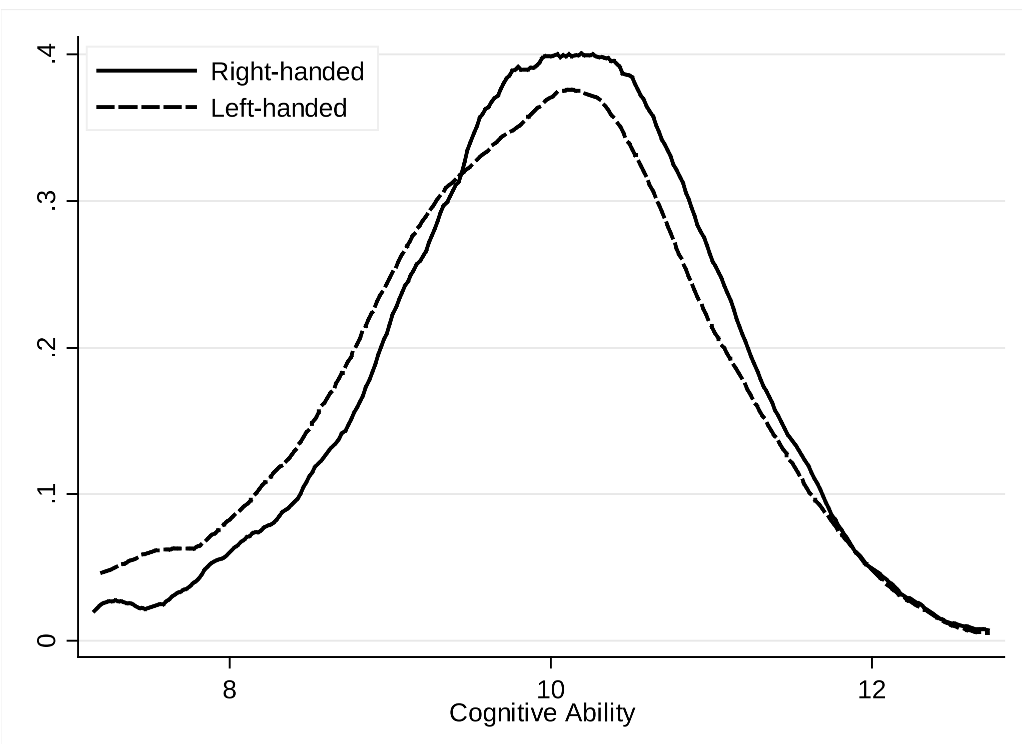

Table 1: Definitions and Sample Means of Key Variables

| **Variables** (1) | **Definition** (2) | **Full Sample** (3) | **Sibling Sub-Sample** (4) |
|---|---|---|---|
| Cognitive resources | Transformed HOME-SF cognitive resources index | 0.000 | -0.069 |
| Cognitive ability | Mean test score | 0.000 | -0.104 |
| Left-handedness | Index ranges from 0 (always right) to 1 (always left) | 0.107 | 0.250 |
| Male | Child is male (dv) | 0.508 | 0.506 |
| Age | Child's age | 9.198 | 9.248 |
| Number of older siblings | Number of older siblings residing in the household | 0.899 | 1.135 |
| Number of younger siblings | Number of younger siblings residing in the household | 0.869 | 1.040 |
| Low birth weight | Birth weight < 2500 grams (dv) | 0.079 | 0.090 |
| Premature birth | Born before 37 weeks of gestation (dv) | 0.122 | 0.134 |
| Caesarean section birth | Born via a Caesarean section (dv) | 0.221 | 0.189 |
| Mother's age at birth | Mother's age at birth | 25.51 | 25.05 |
| Breastfed | Breastfed for at least one week (dv) | 0.457 | 0.428 |
| Smoked during pregnancy | Mother smoked during pregnancy (dv) | 0.265 | 0.295 |
| Drank during pregnancy | Mother drank during pregnancy (dv) | 0.308 | 0.299 |
| Worked during pregnancy | Mother worked during pregnancy (dv) | 0.586 | 0.527 |
| Year of birth | Year child was born | 1986.0 | 1985.5 |
| African-American | Child is African-American (dv) | 0.325 | 0.390 |
| Hispanic | Child is Hispanic (dv) | 0.212 | 0.189 |
| Log income | Natural logarithm of annual family income | 8.905 | 8.674 |
| Mother's education | Mother's years of completed education | 12.45 | 12.15 |
| Mother's AFQT score | Mother's score in Armed Forces Qualification Test | 0.372 | 0.343 |

*Notes*: There are 6593 children from 3281 households in the full sample, and 2318 children from 852 households in the sibling sample. Sibling sample includes only those children who have a sibling with different handedness than themselves. Mean resources, ability, age, number of siblings and log income are time-varying in our sample and so the presented means are averages across children and time; all other variables are time invariant. The abbreviation dv denotes a dummy variable.

Table 2: Family Fixed-Effect Regression Model of Child Handedness

| | **Coef.** | **Std. Error** |
|---|---|---|
| Male | 0.040** | (0.009) |
| Number of older siblings | -0.010 | (0.008) |
| Low birth weight | 0.026 | (0.024) |
| Premature birth | 0.001 | (0.019) |
| Caesarean section birth | 0.019 | (0.024) |
| Mother's age at birth | -0.004 | (0.016) |
| Breastfed | 0.003 | (0.009) |
| Smoked during pregnancy | -0.034 | (0.019) |
| Drank during pregnancy | 0.025 | (0.014) |
| Worked during pregnancy | -0.001 | (0.012) |
| Year of birth | -0.001 | (0.009) |
| Number of children | 2318 | |

*Note*: Dependent variable is measure of left-handedness and ranges from 0 to 1. Standard errors are clustered at the family level. * and ** denote significance at .05 and .01 levels. Sample includes only those children who have an observed sibling with different handedness than themselves. *F*-statistic for joint significance of non-gender covariates equals 0.91.

Table 3: Regression Models of Cognitive Ability and Cognitive Resources

| | **Cognitive Resources (OLS) (1)** | **Cognitive Resources (FE) (2)** | **Cognitive Resources (FE) (3)** | **Cognitive Ability (FE) (4)** | **Cognitive Resources (IV-FE) (5)** |
|---|---|---|---|---|---|
| Cognitive Ability | 0.251** | 0.060** | - | - | 0.338* |
| | (0.010) | (0.007) | | | (0.155) |
| Left-handedness | - | - | -0.050* | -0.149** | - |
| | | | (0.021) | (0.042) | |
| Male | -0.113** | -0.105** | -0.106** | -0.038 | -0.093** |
| | (0.018) | (0.012) | (0.012) | (0.021) | (0.015) |
| Number of older siblings | -0.164** | -0.075** | -0.078** | -0.048 | -0.061* |
| | (0.014) | (0.023) | (0.023) | (0.033) | (0.024) |
| Number of younger siblings | -0.047** | -0.008 | -0.008 | 0.005 | -0.009 |
| | (0.015) | (0.018) | (0.018) | (0.024) | (0.018) |
| Low birth weight | -0.020 | 0.007 | 0.007 | -0.023 | 0.014 |
| | (0.041) | (0.029) | (0.029) | (0.049) | (0.031) |
| Premature birth | 0.061 | 0.004 | -0.002 | -0.102** | 0.032 |
| | (0.034) | (0.022) | (0.023) | (0.039) | (0.029) |
| Caesarean section birth | -0.032 | -0.000 | -0.001 | -0.035 | 0.011 |
| | (0.028) | (0.027) | (0.028) | (0.050) | (0.029) |
| Mother's age at birth | 0.047** | -0.044* | -0.046* | -0.031 | -0.035 |
| | (0.006) | (0.021) | (0.021) | (0.020) | (0.023) |
| Breastfed | 0.255** | -0.013 | -0.014 | -0.013 | -0.010 |
| | (0.024) | (0.021) | (0.022) | (0.036) | (0.022) |
| Smoked during pregnancy | -0.154** | -0.049 | -0.050 | 0.017 | -0.056 |
| | (0.027) | (0.027) | (0.028) | (0.044) | (0.029) |
| Drank during pregnancy | -0.001 | 0.009 | 0.011 | 0.013 | 0.007 |
| | (0.023) | (0.017) | (0.017) | (0.029) | (0.018) |
| Worked during pregnancy | 0.173** | 0.017 | 0.021 | 0.069* | -0.002 |
| | (0.024) | (0.016) | (0.016) | (0.027) | (0.020) |
| Observations | 8474 | 8474 | 8474 | 8474 | 8474 |

*Note*: FE models include family-fixed effects. Full sets of age and year dummies are included in each model but are not shown. Standard errors clustered at the family level are shown in parentheses. * and ** denote significance at .05 and .01 levels. Sample includes only those children who have an observed sibling with different handedness than themselves. The left-handedness instrument ranges from 0 strongly right-handed to 1 strongly left-handed. *F*-statistic on left-handedness in the first-stage equation (column 4) equals 12.32.

Table 4: Robustness Regression Models

| Model Variations | **First-Stage: Effect of handedness on ability** | **Second-Stage: Effect of ability on resources** |
|---|---|---|
| (1) Cognitive ability measured at earliest age | -0.149** | 0.305 |
| | (0.042) | (0.160) |
| (2) Remove control variables | -0.149** | 0.316* |
| | (0.043) | (0.154) |
| (3) Add controls for illness, injury, behavioral-emotional problems | -0.147** | 0.336* |
| | (0.042) | (0.154) |
| (4) Binary measure of left-handedness | -0.135** | 0.326* |
| | (0.037) | (0.150) |
| (5) Cognitive ability measure using 5 tests | -0.144** | 0.351* |
| | (0.044) | (0.164) |
| (6) Binary cognitive ability measure | 0.044** | -1.161* |
| | (0.014) | (0.570) |

*Note*: All figures from FE regressions that include the controls used in Table 2. Sample size equals 8474 in all regressions. Standard errors clustered at the family level are shown in parentheses. * and ** denote significance at .05 and .01 levels. Row 1 uses cognitive ability measured at earliest observed age (usually age 5 or 6). Row 2 removes all the covariates displayed in Table 3 apart from gender. Row 3 includes two indicators for whether child had illness/injury in past 12 months requiring medical treatment, and a continuous measure of behavioral & emotional problems. Row 4 uses a handedness measure equaling one if handedness score is ≥ 0.5 (10.5% of children). Row 5 uses an ability measure equaling average of scores in PPVT test, three PIAT tests and memory digit span test. Row 6 uses an ability measure equaling one if score is in bottom 10% and zero otherwise.