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Abstract

The theoretical and empirical literature on parental investment focuses on whether child-specific parental investments reinforce or compensate for a child's initial endowments. However, many parental investments, such as neighborhood quality and family size and structure, are shared wholly or in part among all children in a household. The empirical results of this paper imply that such household parental investments compensate for low endowments, as proxied by low birth weight.

1. Introduction

It is widely assumed among social scientists that children are born with some set of endowments that have the potential to significantly impact their long-run educational, labor market, and health outcomes. Endowments are typically broadly construed to encompass genetic and prenatal influences that result in basic differences in mental and physical capacities between children at birth. It is also widely accepted among social scientists that parental investment has the potential to reinforce or compensate for these endowments. The theoretical and empirical literature on parental investment focuses almost exclusively on how parents allocate specific investments between their children conditional on their children's endowments. This literature typically finds evidence that parents reinforce endowments, allocating relatively more resources to their higher-endowed children (Rosenzweig and Wolpin 1988; Pitt, Rosenzweig and Hassan 1990; Behrman, Pollak and Taubman 1982; Behrman, Rosenzweig and Taubman 1994; Datar, Kilburn and Loughran 2006), although at least one study presents evidence that parental investment compensates for low endowments (Conley, Strully, and Bennett 2003a).

However, many investments that parents make in their children are shared wholly or in part among all their children. For example, neighborhood choice (and, hence, school quality), family structure, completed family size, parental labor supply, and the availability of books, learning materials, and other technologies in the household are likely to affect more than one sibling. *A priori*, we have no reason to believe that the potential for such household parental investments (i.e., investments available to all siblings) to impact child outcomes is any more or less than the potential for child-specific parental investments (i.e., investments targeted at particular siblings) to impact child outcomes. From a policy perspective, the impact of household parental investment on child outcomes and how it responds to endowments is relevant since many public policies are concerned with raising aggregate levels of investment in children (e.g., raising family income, encouraging family formation) rather than directly affecting the allocation of parental investment within the household.

Since we cannot comprehensively account for household parental investments with specific variables available in nationally representative data sets, like the National Longitudinal Survey of Youth-Child file (NLSY-C), which we use here, nor can we directly measure endowments, we employ an indirect approach in this paper to assess whether household parental investment reinforces or compensates for endowments.[2] This approach compares estimates of the impact of birth weight, which we treat as an observed correlate of endowments,[3] on child test scores derived from ordinary least squares (OLS) and sibling fixed-effect models. Noting sibling fixed-effect models control for both common endowments (i.e., endowments shared in common among all siblings) and household parental investments, we then argue that, if we can control for common endowments in our OLS model, the difference between OLS and fixed-effect coefficient estimates on birth weight can be attributed solely to differences in household parental investments correlated with birth weight and test scores. The sign of this difference in estimated

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[2] The difficulty of measuring parental investment and endowments, child-specific or household, plagues much of the empirical literature on this topic. See Datar, Kilburn, and Loughran (2006) for a critique of this literature.

[3] While the causes of low birth weight are not well understood, it is widely assumed in the empirical literature that genetic endowments and other exogenous prenatal influences, some of which are shared in common across siblings and some of which are specific to particular children, have some effect on birth weight (e.g., Almond, Chay and Lee 2005).

OLS and fixed-effect birth weight coefficients tells us whether household parental investment reinforces or compensates for low birth weight. The key assumptions necessary for this inference are that observed variables in our data control for common endowments, and that OLS and fixed-effect estimates of the impact of birth weight on child outcomes are equally biased by the failure to control for specific endowments (i.e., that part of a child's endowment that is particular to him or her) and child-specific parental investments. Additionally, we must assume that birth weight is measured without error.[4]

The remainder of this paper proceeds as follows. In Section 2, we specify an empirical model of birth weight and child test scores and then use that model to explain how the response of household and child-specific parental investment to birth weight can bias estimates of the impact of birth weight on child outcomes derived from OLS models. Section 3 develops the intuition behind our test for compensating or reinforcing household parental investment by examining recently published estimates of the short- and long-term effects of birth weight derived from data on twins. Section 4 then specifies a sibling fixed-effect model of child outcomes and birth weight and delineates the key assumptions we must make in order to interpret the difference between OLS and fixed-effect models as evidence of whether household parental investment reinforces or compensates for low birth weight. We discuss how we implement our empirical model using data from the NLSY-C in Section 5 and present our empirical results in Section 6. We conclude in Section 7.

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[4] As we explain later, classical measurement error biases our test in favor of finding evidence for reinforcing household parental investment, whereas non-classical measurement error biases our test in favor of finding evidence for compensating household parental investment.

2. An Empirical Model of Birth Weight and Later Outcomes

Controlling for family background characteristics like race, mother's education, and family income, an extensive body of empirical research has shown that low birth-weight children suffer higher levels of neo- and post-natal mortality (McCormick 1985; MacDorman and Atkinson 1999) and childhood morbidity (McCormick 1985; Paneth 1995; Brooks, et al. 2001), score lower on standardized tests (Lee and Barratt 1993; Strauss 2000; Richards, et al. 2001; Matte, et al. 2001; Boardman, et al. 2002), attain fewer years of schooling (McCormick, Gortmaker and Sobol 1990; Corman and Chaikind 1998; Conley and Bennett 2000), and have lower wages and employment as adults (Currie and Hyson 1999) than do normal birth-weight children. It is well known, however, that failure to control for unobserved endowments makes drawing causal inferences from this empirical literature problematic (Almond, Chay and Lee 2005; Behrman and Rosenzweig 2004). For example, low birth-weight children could have lower endowments than high birth-weight children. Consequently, the correlation between birth weight and outcomes could reflect, in part, the correlation between endowments and birth weight. Causal inference is further complicated by the failure to control for parental investments correlated with birth weight, a point that is less frequently acknowledged in the empirical literature, but which we now discuss.

The potential bias in the estimated effect of birth weight on outcomes that results from omitted controls for endowments and parental investments can be derived from the following empirical model:

$$Y_{ij} = \alpha + \beta_{OLS} BW_{ij} + X_{ij}\delta + \varepsilon_{ij} \tag{1}$$

where Y_{ij} is some desirable outcome for child *i* in family *j* (e.g., cognitive test scores), BW_{ij} is a continuous measure of birth weight, and X_{ij} is a vector of child- and family-level explanatory variables. The error term in this model, ε_{ij}, has five components:

$$\varepsilon_{ij} \equiv \gamma_1 e_{ij} + \gamma_2 e_j + \gamma_3 p_{ij} + \gamma_4 p_j + \gamma_5 (BW_{ij}) p_{ij} + \gamma_6 (BW_{ij}) p_j + \phi_{ij} \tag{2}$$

where e_{ij} captures the influence of unobserved specific endowments (i.e., endowments unique to a particular sibling), e_j captures the influence of common endowments (i.e., endowments shared in common among all siblings), p_{ij} captures the influence of child-specific parental investments (i.e., investments targeted at a particular sibling), and p_j captures the influence of household parental investments (i.e., investments shared in common among all siblings). The $\gamma_n 's$, which we assume to be non-negative, measure the correlation between the various error components and the dependent variable. $\gamma_5 (BW_{ij}) p_{ij}$ and $\gamma_6 (BW_{ij}) p_j$ further allow for the possibility that returns to parental investment are functions of birth weight. ϕ_{ij} is an idiosyncratic error term.

In general, we know that if birth weight is correlated with any of the non-idiosyncratic components of ε_{ij}, then an estimate of β_{OLS} derived from estimating Equation (1) will be biased:[5]

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[5] Equation (3) assumes birth weight is measured without error. We consider the implications of measurement error in Section 4.

$$
\begin{aligned}
&\text{plim } \hat{\beta}_{OLS} = \beta_{OLS} + \frac{\gamma_1 \operatorname{cov}(BW_{ij}, e_{ij}) + \gamma_2 \operatorname{cov}(BW_{ij}, e_j)}{\operatorname{var}(BW_{ij})} + \\
&\frac{\gamma_3 \operatorname{cov}(BW_{ij}, p_{ij}) + \gamma_4 \operatorname{cov}(BW_{ij}, p_j)}{\operatorname{var}(BW_{ij})} + \frac{\operatorname{cov}(BW_{ij}\gamma_5(BW_{ij}), p_{ij}) + \operatorname{cov}(BW_{ij}\gamma_6(BW_{ij}), p_j)}{\operatorname{var}(BW_{ij})}
\end{aligned} \tag{3}
$$

It is commonly assumed that birth weight is higher for children with higher specific and common endowments (i.e., $\operatorname{cov}(BW_{ij}, e_{ij}) > 0$ and $\operatorname{cov}(BW_{ij}, e_j) > 0$) and that these unobserved endowments are positively correlated with desirable outcomes, and, so, the presumption in much of the empirical literature is that $\hat{\beta}_{OLS}$ overestimates the effect of birth weight on child outcomes.

But this presumption ignores the possibility that household and child-specific parental investments respond to birth weight. In the wealth maximization model of parental investment first formally articulated by Becker and Tomes (1976), parents allocate child-specific parental investment in a manner that reinforces specific endowments. Siblings with higher specific endowments receive greater levels of parental investment since the marginal return to a given level of parental investment is thought to be higher for better endowed children. Parental preferences for equality are then satisfied via intervivos transfers and bequests. Alternatively, the Separable Earnings Transfer (SET) model of Behrman, Pollak and Taubman (1982) proposes that parents potentially have separate preferences over the distribution of earnings and wealth across their children, which allows parental investment to reinforce or compensate for child-specific endowments.

The theoretical literature on the response of parental investments to endowments and the empirical literature that seeks to test for reinforcing or compensating investment behavior focuses on the response of child-specific parental investments to endowments. For example, reinforcing child-specific parental investment would be consistent with an observation that parents spend more time with their high birth-weight (i.e., better endowed) children. The empirical model described by Equations (1) and (2) further allows for the possibility that parents make household investments in their children and that those household investments respond to differences in birth weight. For example, compensating household parental investment would be consistent with an observation that parents with relatively low birth-weight children choose to live in neighborhoods with particularly good schools, a resource available to all children in a given family.

In this paper, reinforcing unobserved parental investment occurs if $\gamma_3 \operatorname{cov}(BW_{ij}, p_{ij}) + \operatorname{cov}(BW_{ij}\gamma_5(BW_{ij}), p_{ij}) > 0$ or $\gamma_4 \operatorname{cov}(BW_{ij}, p_j) + \operatorname{cov}(BW_{ij}\gamma_6(BW_{ij}), p_j) > 0$, which would be the case if parents invested more heavily in their higher birth-weight children or if higher birth-weight children exhibited higher marginal returns to a given level of parental investment. Likewise, compensating unobserved parental investment occurs if $\gamma_3 \operatorname{cov}(BW_{ij}, p_{ij}) + \operatorname{cov}(BW_{ij}\gamma_5(BW_{ij}), p_{ij}) < 0$ or $\gamma_4 \operatorname{cov}(BW_{ij}, p_j) + \operatorname{cov}(BW_{ij}\gamma_6(BW_{ij}), p_j) < 0$, which would be the case if parents invested more heavily in their lower birth-weight children or if lower birth-weight children exhibited higher marginal returns to a given level of parental investment. Thus, if both child-specific and household parental investments are reinforcing, then $\hat{\beta}_{OLS}$ is biased upward in magnitude and if both child-specific and household parental investments are compensating, then the net direction of the bias in $\hat{\beta}_{OLS}$ depends on the relative magnitudes of the endowment and parental investment covariance terms.[6]

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[6] In signing the bias of $\hat{\beta}_{OLS}$, we assume that unobserved endowments and parental investments are positively correlated with child outcomes (i.e., $\gamma_{1-4} > 0$). The assumption that endowments and outcomes are positively correlated is relatively innocuous. Regarding parental investments,

3. Interpreting OLS and fixed-effect Estimates of Birth Weight Effects Employing Data on Twins

The intuition behind our empirical test for whether household parental investments reinforce or compensate for low birth weight (developed in Section 4 below) can be conveyed by examining a number of recently published estimates of the impact of birth weight on child and adult outcomes using data on twins. The advantage of using data on twins is that twins experience identical pre-natal parental investments, possess a common endowment, and, in the case of monozygotic (MZ) twins, share identical specific endowments as well. If we ignore the influence of parental investments, the correlation between the difference in birth weight and the difference in outcomes between MZ twins provides us with an unbiased estimate of β_{OLS} under the assumption that differences in birth weight between MZ twins are due solely to exogenous factors (e.g., the difference in nutrition each twin receives in-utero (Zhang, Brenner and Klebanoff 2001)).

Almond, Chay and Lee (2005) and Conley, Strully and Bennett (2003b) use large samples of MZ and dizygotic (DZ) twins to study the impact of birth weight on infant mortality. Almond, Chay and Lee (2005) report that a one standard deviation increase in birth weight (667 grams) lowers one day and one year mortality per 1,000 live births by 0.007 and 0.022, respectively. Using a slightly more recent sample of twins, Conley, Strully and Bennett (2003b) report that a one pound (about 454 grams) difference in birth weight between twins is correlated with a 13 to 22 percent difference in neonatal mortality and a 14 percent difference in post neonatal mortality. Both papers report that their fixed-effect estimates of the effect of birth

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however, we must rule out the possibility that parents make investments in their children that are unintentionally harmful.

weight on infant mortality are substantially smaller in magnitude than their OLS estimates (i.e., the estimate of β_{OLS} from Equation (1) using the same sample of twins but without taking differences between twins).

With data on female MZ twins from the Minnesota Twin Registry, Behrman and Rosenzweig (2004) correlate within-twin differences in birth weight with within-twin differences in adult outcomes. Their estimates imply that a one pound increase in birth weight increases schooling attainment by one-third of one year and hourly wages by about seven percent. Of particular significance for this paper is their surprising finding that their OLS estimates *understate* the positive impact of birth weight on schooling attainment by as much as 50 percent.[7]

Finally, Black, Devereux and Salvanes (2007) use administrative data on a sample of Norwegian MZ and DZ twins to estimate the impact of birth weight on both early and later outcomes. Like Almond, Chay and Lee (2005) and Conley, Strully and Bennett (2003b), they report that their OLS estimates exceed their fixed-effect estimates when examining short-run outcomes like infant mortality and APGAR scores. However, when examining later outcomes,

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[7] In a sample of non-twin siblings from the Panel Survey of Income Dynamics (PSID), Conley and Bennett (2000) also report finding that OLS estimates of the impact of birth weight on high school graduation are half the size of comparable sibling fixed-effect estimates. Royer (2005), however, reports that fixed-effect estimates of the impact of birth weight on educational attainment in a sample of female twins born in California are smaller in magnitude than OLS estimates from the same sample.

like education and wages, they report that their OLS and fixed-effect estimates are similar in magnitude.

Household parental investment provides a potential explanation for why these studies find that, in the case of infant mortality, fixed-effect estimates are smaller in magnitude than OLS estimates, but, in the case of later outcomes, like schooling and wages, fixed-effect estimates are similar or larger in magnitude than OLS estimates.[8] Suppose parents have preferences for equity and so choose to allocate household parental investment in a manner that compensates for low birth weight. The Behrman and Rosenzweig (2004) and Black, Devereux and Salvanes (2007) fixed-effect estimates could equal or exceed the OLS estimates if the household parental investment covariance term balances or outweighs the (presumably positive) endowment covariance terms in Equation (3). That is, household parental investment is strongly compensating, so much so that it could lead to a net *downward* bias in $\hat{\beta}_{OLS}$ in the case of later outcomes.[9] Note that child-specific parental investment cannot account for these differences between the OLS and fixed-effect estimates since both estimates are biased by the failure to

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[8] Of course, alternative explanations for this pattern of results are possible. For example, the samples for these various studies span different countries and time periods. Alternatively it could be that parental investments are reinforcing in the case of infant mortality, but compensating in the case of later outcomes.

[9] A number of other studies have argued that that the estimated effect of early indicators of health (like height in the context of developing countries) on schooling are biased downward in magnitude for this reason as well (Behrman and Lavy 1998; Alderman, et al. 2001; Glewwe and King 2001).

control for child-specific parental investments (this point is explained more thoroughly below in Section 4).

However, in the case of a very short-term outcome such as infant mortality, the potential for a household parental investment to affect outcomes is potentially much less. For example, a household investment such as neighborhood quality might have a relatively small impact on infant mortality attributable to low birth weight when compared to its potential impact on much later outcomes such as schooling attainment and wages. The endowment covariance terms in Equation (3) are likely to dwarf the household parental investment covariance term in the case of very early childhood outcomes like infant mortality, causing fixed-effect estimates to exceed OLS estimates of β. In other words, $\gamma_4 + \gamma_6(BW_{ij})$ is potentially much smaller in magnitude when the dependent variable is an early outcome compared to when it is some later outcome.

The empirical test we derive in the following section allows us to test whether household parental investment is in fact compensating and whether the degree to which it compensates for low birth weight depends on whether outcomes are measured early or later in life.

4. A Test for Compensating or Reinforcing Household Parental Investment

In this section, we formalize our test for household parental investment and discuss the assumptions we must maintain in order to interpret the difference between OLS and fixed-effect estimates of the impact of birth weight on child test scores as an indication of compensating or reinforcing household parental investment. Estimation of Equation (1) produces our OLS estimate of β. The fixed-effect estimate of β, which we denote $\hat{\beta}_{FE}$, is derived from estimating the following equation:

$$\Delta_j Y_{ij} = \beta_{FE}\Delta_j BW_{ij} + \Delta_j X_{ij}\delta + \gamma_1\Delta_j e_{ij} + \gamma_3\Delta_j p_{ij} + \Delta_j\gamma_5(BW_{ij})p_{ij} + \Delta_j\gamma_6(BW_{ij})p_j + \Delta_j\phi_{ij} \qquad (4)$$

where all variables are now of the form $\Delta_j Y_{ij} = Y_{ij} - \overline{Y}_j$ and $\overline{Y}_j$ is the within-family mean of Y_{ij}. The parameter β_{FE} now measures how within-family differences in birth weight are correlated with within-family differences in outcomes. While the family-level error components, e_j and p_j, have been differenced out of the regression, $\hat{\beta}_{FE}$ remains biased by the failure to control for specific endowments, child-specific parental investments, and the possibility that the effect of parental investment is a function of birth weight:

$$\begin{aligned}&\text{plim } \hat{\beta}_{FE} = \beta + \frac{\gamma_1 \operatorname{cov}(\Delta_j BW_{ij}, \Delta_j e_{ij}) + \gamma_3 \operatorname{cov}(\Delta_j BW_{ij}, \Delta_j p_{ij})}{\operatorname{var}(\Delta_j BW_{ij})} + \\ &\frac{\operatorname{cov}(\Delta_j BW_{ij}, \Delta_j \gamma_5 (BW_{ij}) p_{ij}) + \operatorname{cov}(\Delta_j BW_{ij}, \Delta_j \gamma_6 (BW_{ij}) p_j)}{\operatorname{var}(\Delta_j BW_{ij})}\end{aligned} \tag{5}$$

Under the assumption that

$$\begin{aligned}&\frac{\gamma_1 \operatorname{cov}(BW_{ij}, e_{ij}) + \gamma_3 \operatorname{cov}(BW_{ij}, p_{ij})}{\operatorname{var}(BW_{ij})} + \frac{\operatorname{cov}(BW_{ij}\gamma_5(BW_{ij}), p_{ij}) + \operatorname{cov}(BW_{ij}\gamma_6(BW_{ij}), p_j)}{\operatorname{var}(BW_{ij})} = \\ &\frac{\gamma_1 \operatorname{cov}(\Delta_j BW_{ij}, \Delta_j e_{ij}) + \gamma_3 \operatorname{cov}(\Delta_j BW_{ij}, \Delta_j p_{ij})}{\operatorname{var}(\Delta_j BW_{ij})} + \\ &\frac{\operatorname{cov}(\Delta_j BW_{ij} \Delta_j \gamma_5 (BW_{ij}), p_{ij}) + \operatorname{cov}(\Delta_j BW_{ij} \Delta_j \gamma_6 (BW_{ij}), p_j)}{\operatorname{var}(\Delta_j BW_{ij})}\end{aligned}, \tag{6}$$

the difference between the OLS and fixed-effect estimates of β reduces to

$$\text{plim } (\hat{\beta}_{OLS} - \hat{\beta}_{FE}) = \frac{\gamma_2 \operatorname{cov}(BW_{ij}, e_j) + \gamma_4 \operatorname{cov}(BW_{ij}, p_j)}{\operatorname{var}(BW_{ij})}. \tag{7}$$

If we can further perfectly control for common endowments using observed variables, the difference between the OLS and fixed-effect estimates of β is then attributable solely to the covariance between birth weight and unobserved household parental investments:

$$\text{plim}\,(\hat{\beta}_{OLS} - \hat{\beta}_{FE}) = \frac{\gamma_4 \operatorname{cov}(BW_{ij}, p_j)}{\operatorname{var}(BW_{ij})} \tag{8}$$

Our test for whether household parental investment reinforces or compensates for specific endowments then takes the following form: If $\hat{\beta}_{OLS} - \hat{\beta}_{FE} > 0$, then we infer that household parental investments are reinforcing and if $\hat{\beta}_{OLS} - \hat{\beta}_{FE} < 0$, then we infer that household parental investments are compensating.[10] Note that our test identifies the bias imparted by the possibility that the level of household parental investment varies between parents with relatively low birth-weight children and parents with relatively high birth-weight children. We do not identify the bias imparted by the possibility that the effect of a given level of household parental investment might vary with birth weight. We will return to this point in Section 7 in discussing the interpretation of our findings.

There are three key assumptions behind this test. First, we must assume that Equation (6) holds. In the language of Bound and Solon (1999), the left- and right-hand sides of Equation (6) represent the endogenous variation in birth weight attributable to specific endowments and child-specific parental investments as a proportion of the variance in birth weight between families (on the left-hand side) and within families (on the right-hand side). Since the endogenous variation in birth weight represented by Equation (6) is attributable solely to child-specific factors, we

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[10] As noted in footnote 6, this test assumes that unobserved household parental investments are positively correlated with child outcomes (i.e., $\gamma_4 > 0$). Also note that if $\gamma_4 = 0$, then our test cannot distinguish between the hypothesis that household parental investments are uncorrelated with birth weight and the hypothesis that household parental investments are uncorrelated with outcomes.

have no reason to believe that it should bias our estimate of β_{OLS} any more or less than it biases our estimate of β_{FE}. In other words, we have no reason to believe that endogenous variation in birth weight attributable solely to child-specific factors (i.e., factors that are orthogonal to family-specific factors) across unrelated individuals is any more or less than this type of endogenous variation across siblings.

Second, we must assume that we can control for common endowments using observed variables in our data so that the OLS estimate of β is no longer biased by e_j. Our data contain a number of good proxies for common endowments. The first is the mother's score on the Armed Forces Qualifying Test (AFQT) which is broadly predictive of educational and labor market outcomes later in life (see, for example, Neal and Johnson 1996).[11] Our data also contain information on grandmother's education, which is often employed as a control for family endowments. We include race and ethnicity as a common endowment under the assumption that race and ethnicity could be correlated with physiological differences between women that are correlated with birth weight (Frisbie, Forbes and Pullum 1996). The key feature of each of these common endowment measures is that they are arguably outside the choice set of the mother. We do not include any father-level variables in our analysis. The NLSY-C follows mothers, not fathers, and so father-level variables are generally less available and of questionable quality.

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[11] There is some question whether AFQT measures schooling achievement rather than ability, especially for minority children (see, for example, Cascio and Lewis (2005) and Hansen, Heckman, and Mullen (2004)). Either way, we will treat AFQT as a measure of common endowments in this paper.

Admittedly, these mother-level variables will not perfectly capture common endowments. Under the assumption that $\operatorname{cov}(BW_{ij}, e_j) > 0$ and $\gamma_2 > 0$, failure to control perfectly for common endowments will bias our test statistic in favor of reinforcing household parental investment. Our results, however, suggest compensating household parental investment. Thus, an inability to perfectly control for common endowments suggests that household parental investments may be even more compensating than our results imply.

Finally, our test requires that measurement error not differentially bias $\hat{\beta}_{OLS}$ and $\hat{\beta}_{FE}$. This assumption is difficult to maintain since birth weight is almost certainly measured with some error in our data and, in the case of classical measurement error, fixed-effect estimates of β will suffer more attenuation bias than will OLS estimates (Griliches and Hausman 1986). A recent study in the United Kingdom which compared mother-reported birth weight with registration data on birth weight for the same children found that 82 percent of mothers reported their baby's weight within 30 grams (about one ounce) of the registration weight and 92 percent reported their baby's weight within 100 grams (Tate, et al. 2005). A study of 1,892 U.S. children reports a comparable level of accuracy in mother-reported birth weight (McCormick and Brooks-Gunn 1999).[12] Additionally, measurement error was found to be mean zero in these

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[12] McCormick and Brooks-Gunn (1999) find that birth weight recall was especially accurate (98 percent reporting within 100g) for mothers with children of more than 2500g. Our evidence for compensating household parental investment is strongest in families with children of above average birth weight. See also Seidman, et al (1987) and Gayle, et al (1988) for evidence on the accuracy of birth weight recall. Seidman, et al (1988) note that accuracy diminishes as the recall period increases. In the NLSY-C, the average recall period is 38 months. Our qualitative

studies. If measurement error in birth weight is classical, then our test statistic is biased in favor of finding evidence for reinforcing household parental investment. With classical measurement error, then, household parental investment might be even more compensating than our estimates suggest.

However, it is possible that measurement error in birth weight is correlated within mothers (e.g., mothers systematically over- or under-estimate the birth weight of their children). In this case, fixed-effect estimation results in less attenuated parameters than estimation by OLS (Bound and Krueger 1991) which would bias our test statistic in favor of compensating household parental investment. We cannot reject this possibility, but we do note that rounding errors are a likely cause of measurement error, which would tend to suggest non-correlated measurement error providing mothers do not always round-up or round-down to the nearest focal point. The Tate, et al. (2005) study identified 27 percent of their misreported cases as resulting from rounding and transcription errors. The remaining misreported cases did not have an identifiable cause.

5. Data

We use data from the NLSY-C, which contains detailed information about the children born to female respondents of the National Longitudinal Survey of Youth 1979 (NLSY79). The NLSY79 began in 1979 with a sample of 12,686 young adults between the ages of 14 and 21. NLSY79 respondents were surveyed annually between 1979 and 1994 and biennially

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conclusions are unaffected when we restrict our sample to women with relatively short recall periods (within two years of birth).

thereafter.[13] The last wave of data we analyzed was collected in 2000. The children of the female NLSY79 respondents have been surveyed biennially since 1986. As of the 2000 survey wave, the NLSY-C had collected data on 11,205 children born to 6,283 mothers.

We exploit three key features of the NLSY-C for the purposes of this paper. First, the NLSY-C collects data on all children born to NLSY79 mothers, which allows us to control for family-level fixed effects. Second, the NLSY-C collects birth weight data for all surveyed children. The third key feature of the NLSY-C is that it conducts a variety of child assessments in each survey wave. In this paper, we use data on test scores collected between birth and age 12.

Our test score data come from the Motor and Social Development scale (MSD) administered to children ages 0-3, the Peabody Picture Vocabulary Test (PPVT) administered to children ages 3 or 4 in our sample,[14] and the Peabody Individual Achievement Test (PIAT), administered to children ages 5-14. The MSD was developed by the National Center for Health Statistics to measure dimensions of the motor, social, and cognitive development of young children. Based on the child's age, mothers answer 15 age-appropriate items out of 48 motor and social development items derived from standard measures of child development (e.g., the Bayley Scales of Infant Development, the Gesell Scale, and the Denver Developmental Screening Test). Children who are administered the PPVT listen to a list of 175 vocabulary items of increasing difficulty and then select one of four pictures which best describes the word's meaning. The test

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[13] Two subsamples, the military subsample, and the poor, non-Hispanic white subsample, were dropped from the survey in 1984 and 1990, respectively.

[14] In the overall NLSY-C sample, the PPVT was administered to children ages 3-5 and 10-11.

is designed to generate a quick assessment of a child's verbal ability or scholastic aptitude (CHHR 2002). The PIAT assessments provide wide-ranging measures of academic achievement in the areas of mathematics and reading.[15] We conducted our analyses separately for the PIAT Mathematics (PIAT-M) and Reading Recognition (PIAT-RR) assessments.

We employ a number of sample restrictions, which we detail in Table 1. First, we drop the military and poor, non-Hispanic white subsamples. Children of the military subsample were never included in the child survey and children of the poor, non-Hispanic white subsamples were surveyed, at most, two times. Next, we drop observations with missing birth weight, which leaves 8,370 children. Depending on the model, we then drop observations with missing data on MSD, PPVT, or PIAT scores. Our MSD regressions are restricted to children ages 0-36 months,[16] our PPVT regressions to children ages 3-5, and our PIAT regressions to children ages 5-12. Since we estimate models with family-level fixed effects, we then drop children with no siblings in the remaining sample.

Finally, we randomly select a single observation for each child that remains in these samples. For example, in the case of the PIAT sample, if a single child is observed multiple times between ages 5 and 12, we select only one of those observations (and, hence, only one of those PIAT score observations). We implement the same procedure when we examine how our results vary by age within the MSD and PIAT samples. We do not use multiple observations per

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[15] In all three of these tests, the entry point to the exam is determined by the child's age. See CHRR (2002) for more about these tests.

[16] We do not examine MSD scores for three-year olds because MSD scores generally top-out by the third birthday leaving little variation across children (CHRR (2002)).

child in the same regression since our regression specification does not need that aspect of the panel for identification and employing multiple observations per child could lead to autocorrelated disturbances in Equations (1) and (4).[17] Our final MSD sample consists of 3,316 children born to 1,408 mothers, our final PPVT sample consists of 2,113 children born to 943 mothers, and our final PIAT sample consists of 5,800 children born to 2,241 mothers.[18]

The key explanatory variable in all three regressions is birth weight (*BW*) measured in 100s of grams. We employ a continuous rather than categorical measure of birth weight for several reasons. First, specific birth weight cut-points (e.g., 2,500 grams) are arbitrary (Solis, Pullum and Frisbie 2000). Second, our fixed-effects strategy requires variation in birth weight across siblings. Employing categorical measures of birth weight reduces within-family variation in birth weight considerably. Third, we wish to compare how the estimated coefficient on birth weight varies across specifications, and employing a single measure of birth weight facilitates those comparisons.

There exists a substantial amount of variation in birth weight within families in our data. A simple one-way analysis of variance estimates that the family-effect accounts for about 39 percent of the overall variance in birth weight in our sibling sample and indicates that the standard deviation of birth weight within families (481 grams) is only 22 percent less than the

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[17] Our regression specifications allow for arbitrary correlation in errors at the mother level.

[18] The PIAT sample restrictions result in a sample with similar observable characteristics as the entire sample of NLSY-C siblings who are not in the poor white or military subsamples. As noted later in this section, the MSD and PPVT samples are generally of higher SES than the overall sample of siblings.

overall standard deviation of birth weight in the overall sample (613 grams). About 18 percent of children in this sample reside in families in which there is at least one child with birth weight in the range traditionally considered low birth weight (<2,500 grams) and one child of normal birth weight (≥2,500 grams).

Birth weight is clearly a strong predictor of cognitive development over a range of ages. This is evident in Figures 1 and 2, which graph MSD, PPVT, PIAT-M, and PIAT-RR percentile scores by birth weight and age. The graphs show that, at almost every age, lower birth-weight children have lower MSD, PPVT, PIAT-M, and PIAT-RR test scores than do heavier birth-weight children.[19]

We implement two sets of OLS regressions corresponding to Equation (1) and one sibling fixed-effect regression corresponding to Equation (4). We label the first two regression specifications "OLS1" and "OLS2" and the third regression specification "FE", for fixed effects. All three regressions include in the vector X a number of child-specific covariates, including indicators for age in years or months at the time the child was surveyed, indicators for gender and whether the child is a first born, the mother's age at first birth, log family income, region of residence, and whether the mother is currently married.[20] The vector X in the OLS2

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[19] The erratic movement of the graph line representing children born <1,500 grams is likely due to small sample sizes. About one percent of our sample was born at <1,500 grams, 7 percent between 1,501-2,499 grams, 18 percent between 2,500-3,000 grams, 51 percent between 3,001-3,700 grams, and 23 percent at >3,700 grams.

[20] We do not control for family size in our regressions (although, recall that all children in our sample have at least one sibling) since we view family size as a form of household parental

specification additionally controls for our proxies for maternal endowments: log AFQT, indicators for grandmother's education (less than high school, high school, some college, college or more), and indicators for race/ethnicity (white, black, Hispanic). Rather than drop additional observations with missing covariate data, we impute the value of missing observations with the sample mean and include dummy variables for missing data. The means and standard deviations of the regression variables are reported in Table 2 for the MSD, PPVT, and PIAT samples separately.

Table 2 indicates that the sample of mothers whose children were administered the MSD or PPVT are older at birth (and consequently more likely to be of higher SES) than the sample of mothers whose children were administered the PIAT. The first wave of the NLSY-C was administered in 1986 and the MSD, PPVT, and PIAT were administered to age-eligible children in that year. The children of women who gave birth relatively early in survey's history (e.g., before 1986) were less likely to be eligible to take the MSD (0-36 months) or PPVT (3-4 years in our sample) in 1986 and later years.[21]

6. Results

In this section we report the results of OLS and fixed-effect estimates of the effect of birth weight on four different test scores (MSD, PPVT, PIAT-M, and PIAT-RR) and interpret the difference between these OLS and fixed-effect estimates as evidence of compensating or

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investment. For example, the decision to have a third or higher-order child potentially affects the outcomes of all siblings in a family.

[21] Our qualitative conclusions are unchanged when we restrict our sample to children who took all three exams.

reinforcing household parental investment (i.e., if $\hat{\beta}_{OLS2} - \hat{\beta}_{FE} < 0$, then household parental investment is compensating and if $\hat{\beta}_{OLS2} - \hat{\beta}_{FE} > 0$, then household parental investment is reinforcing). We organize our discussion of results by the dependent variable examined, first discussing results pertaining to test scores at early ages (MSD and PPVT) and then results pertaining to test scores at later ages (PIAT-M and PIAT-RR). We then show how our results vary across families with different family-level endowments (as measured by mother's AFQT and whether all children in a given family have below or above average birth weight).

6.1. MSD and PPVT

Panel A of Table 3 presents estimates of β derived from the OLS1, OLS2, and FE regression specifications in which MSD percentile scores serve as the dependent variable (full regression results are presented in Table A-1). In the overall sample, the OLS1 results indicate that a 100 gram increase in birth weight is associated with an increase in MSD scores of 0.47 percentile points. In the model presented in column (2) of Table 3 (OLS2), we add our proxies for maternal endowments (race, log AFQT, and grandmother's education). The addition of these covariates to the model has little effect on $\hat{\beta}$, which is not surprising since, with the exception of race, these variables have little independent effect on MSD (see Table A-1). In column (3), we present our fixed-effect estimate of β and the final column of Table 3 presents a Hausman test of the difference between the OLS2 and FE estimates. The addition of family-level fixed effects to the model increases the estimate of β somewhat, but the difference between OLS2 and FE estimates is statistically insignificant.

Birth weight has a considerably larger impact on MSD scores at ages 0-18 months than at ages 19-36 months. This makes sense since overall variation in MSD scores across children diminishes with age as children “top-out” on the test. For example, the FE estimate in the 19-36 month sample is 0.26 (and statistically insignificant) compared to 0.67 (and statistically significant) in the 0-18 month sample. In neither case, however, is the difference between the OLS2 and FE estimates of β statistically significant.

Results for PPVT, administered at ages 3 and 4 in our sample, are presented in Panel B of Table 3 (full regression results are presented in Table A-2). The OLS1 specification indicates that a 100 gram increase in birth weight is associated with an increase in PPVT scores of 0.35 percentile points. Unlike in the case of MSD, this correlation diminishes significantly when we add proxies for maternal endowments ($\hat{\beta}$ falls from 0.35 to 0.12 between the OLS1 and OLS2 specifications) and becomes statistically insignificant. The correlation between birth weight and PPVT diminishes even further in the fixed-effect specification. The difference between the OLS2 and FE estimates of β is, therefore, positive, and on the margin of statistical significance.

Thus, in the case of MSD test scores, the difference between OLS2 and FE estimates of β leads us to reject the hypothesis that household parental investment compensates for (or reinforces) low birth weight at very young ages. This finding is consistent with one of two possible explanations. Either the household investments parents make in their children at younger ages are uncorrelated with birth weight or these household investments are not significantly correlated with cognitive development in a child’s early years, at least as measured by the MSD.

In the case of PPVT, however, the difference between OLS2 and FE estimates is positive and marginally significant and so is consistent with reinforcing household parental investment. Recall, however, that a failure to control perfectly for common endowments in the OLS2 specification and classical measurement error in birth weight both bias our test in favor of finding evidence of reinforcing household parental investment.

6.2. PIAT

Panels B and C of Table 3 present estimates of β derived from regression specifications in which PIAT-M and PIAT-RR percentile scores serve as the dependent variables (full regression results are presented in Tables A-3 and A-4). Examining results pertaining to PIAT-M first (Panel C), we see that birth weight is significantly correlated with PIAT-M scores in the OLS1 specification. The coefficient estimates indicate that a 100 gram increase in birth weight is associated with 0.38 (0.06) percentile increase in PIAT-M scores.

Adding proxies for maternal endowments causes the estimate of β to fall to 0.21 (0.06). This suggests that maternal endowments are positively correlated with birth weight and PIAT-M scores. In Table A-3, we see that blacks and Hispanics score considerably lower on the PIAT-M than do whites. PIAT-M scores increase strongly with AFQT scores and grandmother's education.

The addition of fixed effects to the PIAT-M model leads to an increase in the estimate of β to 0.35 (0.08). Thus, the difference between the OLS2 and FE estimates of β is –0.13 (0.05). In Panel D of Table 3 which reports results for the PIAT-RR regressions we also see that the difference between the OLS2 and FE estimates of birth weight is negative, although the standard error on that difference is relatively large. The same pattern in OLS versus FE estimates is evident at both younger (ages 5-8) and older (ages 9-12) ages in the case of PIAT-M. In the case of PIAT-RR the difference in coefficient estimates is positive for younger children and negative for older children.

Under the assumptions outlined in Section 4, we interpret the negative and statistically significant difference in $\hat{\beta}$ between the OLS2 and FE specifications in the overall PIAT-M sample as evidence that household parental investments compensate for low birth weight at those older ages. The PIAT-RR regressions also suggest compensating household parental investment, although the difference between OLS2 and FE estimates in that case is not statistically significant. In these cases, it would appear that family-level fixed effects encompass opposing forces: maternal endowments that are positively correlated with birth weight and test scores (as suggested by the fall in $\hat{\beta}$ between the OLS1 and OLS2 specifications) and some other influence that is negatively correlated with birth weight, yet positively correlated with test scores. We posit that this other component of the family fixed effect that drives the coefficient on birth weight upward in magnitude from the OLS2 to the FE specification is unobserved household parental investment that compensates for low birth weight.

6.3. Results by AFQT and Birth Weight

It is common in the empirical literature on birth weight to examine how the impact of birth weight on outcomes varies across the birth weight distribution and across individuals and families with different characteristics (e.g., Currie and Hyson 1999, Almond, Chay and Lee 2005, Black, Devereux and Salvanes 2007, Royer 2005). With respect to birth weight, the interest is in whether the positive impact of birth weight on outcomes is concentrated among the lightest babies or whether birth weight continues to exert a positive influence even at higher birth weights. It is also hypothesized that the potential for families to reinforce or compensate for low birth weight could be limited in more resource-constrained families (e.g., Behrman 1997).

In this context, though, it is difficult to interpret differences between estimates of β derived from different samples. For example, suppose we were to find that the FE estimate of β

in the PIAT-M regression is much higher among low birth-weight children than it is among high birth-weight children. This difference in coefficient estimates could be attributable to a number of different factors. One possibility is that the causal impact of birth weight on test scores differs across the birth weight distribution for biological reasons. Another possibility is that the impact of specific endowments and child-specific parental investment on test scores (neither of which are controlled for in the FE specification) varies across low and high birth-weight children. Finally, it could be that household parental investment (which is controlled for in the FE specification) compensates for low birth weight more strongly in families with higher birth-weight children.[22]

However, by taking the difference between OLS2 and FE estimates of β derived from the same sample, and then comparing how that difference varies *across* samples, we can make inferences about the relative importance of household parental investment in different samples. In Table 4, we present estimates derived from four samples: high (≥75) and low (<75) AFQT and high (>3,300 grams) and low (≤3,300 grams) birth-weight households. We interpret AFQT here as a convenient summary of ability and socioeconomic status (AFQT is highly correlated with family background, educational attainment, marriage and fertility, income, and other

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[22] Note that even if we obtained estimates of β derived from within-MZ twin comparisons, the difference between β estimated from low and high birth-weight samples could be attributable to differences in the causal impact of birth weight on test scores, the failure to control for child-specific parental investments, or differences in the degree to which household parental investment compensates for low birth weight.

socioeconomic variables).[23] The division of the sample by birth weight results in families in which either all children have birth weight above mean birth weight in our sample or all children have birth weight less than or equal to mean birth weight in our sample.

In Panels A and B of Table 4, we observe some variation across samples in the difference between the OLS2 and FE estimates of the impact of birth weight on MSD and PPVT scores, but this variation in differences appears to be due to sampling variation. Even though there are considerable differences in the FE estimates of β between the low and high birth-weight samples, these differences cannot be attributable to differences in the impact of household parental investment, which leaves either differences in the causal impact of birth weight or differences in the impact of child-specific parental investments and endowments on early test scores.

However, variation in the difference between OLS2 and FE estimates in the low and high birth-weight and low and high AFQT samples in the case of PIAT-M is striking. In the low birth-weight sample, for example, the difference between the OLS2 and FE estimates of β is –0.05 (0.10). In the high birth-weight sample the same difference is –0.42 (0.13). The OLS1 and OLS2 estimates of β in the high birth-weight households are actually negative. The large increase in β between the OLS2 and FE specifications in the high birth-weight and AFQT samples suggest that these well-endowed households strongly compensate for low birth weight. In other words, the impact of birth weight on PIAT-M would be much stronger were it not for the mediating influence of household parental investment in those families. This same pattern of results is observed in the case of PIAT-RR (Panel D). We note here, but do not show in the

[23] See, for example, Neal and Johnson (1996).

tables, that we find evidence of compensating household parental investment at both older and younger ages in the high birth-weight and high AFQT samples.[24]

7. Conclusions

Taken by themselves, our sibling fixed-effect estimates suggest that birth weight is a significant marker of cognitive ability, a marker that is independent of any fixed characteristic of the mother, persists at least through the early teenage years, and holds across a range of subpopulations of our data.[25] Because we cannot control for specific endowments or child-specific parental investments, we do not interpret these fixed-effect estimates as causal, but merely note that whatever birth weight represents, it appears to matter for cognitive development later in life.

We have argued, though, that by comparing OLS and fixed-effect estimates of the effect of birth weight on test scores, we can infer whether the investments parents make in common across their children reinforce or compensate for birth weight. When children are very young

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[24] We also estimate pooled models in which we interact the categorical birth weight and AFQT variables with birth weight to test whether the difference in OLS2 and FE coefficient estimates reported in column 4 of Table 4 are statistically different from one another. These regression results, not reported here, confirm that the differences are statistically distinguishable from one another in the case of PIAT-M and PIAT-RR.

[25] In results not reported here, we show that the fixed-effect estimate of birth weight on MSD, PPVT, and PIAT scores is positive and significant across a number of subpopulations of our data including whites, blacks, and Hispanics, married and unmarried mothers, and in estimates derived from siblings of the same gender.

(less than three), our estimates suggest that household parental investment is neutral with respect to birth weight. At older ages (five and older), we find evidence of compensating household parental investment. In the case of PIAT-M, our estimates imply that household parental investment reduces the impact of birth weight on math scores by a little less than one-third. Our estimates also suggest compensating household parental investment in the case of PIAT-RR, although the estimated effect is relatively small and statistically insignificant.

The finding of neutrality at younger ages is consistent with two possible explanations. The first possibility is that the household investments parents make in their children at younger ages are uncorrelated with birth weight. The second possibility is that these household investments are not significantly correlated with cognitive development in a child's early years, at least as measured by these particular tests.

It is important to note that our estimates identify a particular type of compensating household parental investment. Our estimates imply that families with relatively low birth-weight children spend more (in absolute terms) on household parental investment than do families with relatively high birth-weight children. This seems like a strange finding given that we know average birth weight is positively correlated with household income. But the analyses reported in Table 4 indicate that evidence for this type of compensating parental investment appears only among better-endowed families (i.e., those with children of above average birth weight and with mothers who score relatively high on the AFQT). In these better-endowed families, there is no correlation between average birth weight and household income. So, in these families, our estimates imply that parents with relatively low birth-weight children, but similar resources, will choose to allocate relatively more of those resources to household parental investment than to other consumption.

Many public health and welfare programs seek to reduce the incidence of low birth weight. The present research does not tell us whether such programs, if successful in raising birth weight, will ultimately succeed in improving later child outcomes. However, our research does provide some evidence that household parental investment can compensate for whatever disadvantage low birth weight conveys. This is an important finding since raising birth weight itself may be a difficult (or even inappropriate) target for public policy (Almond, Chay and Lee 2005). The results of this research suggest that policies that raise investment in children in general have the potential to mitigate the adverse consequences of low birth weight, whether birth weight simply reflects endowments and prenatal influences or in fact has a causal impact on later outcomes.

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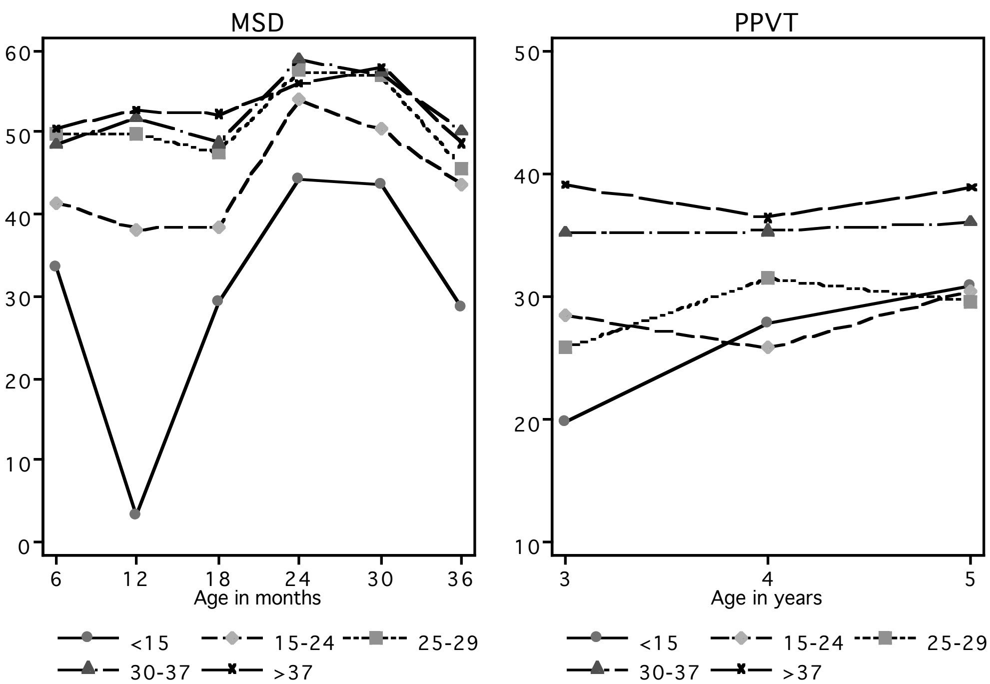
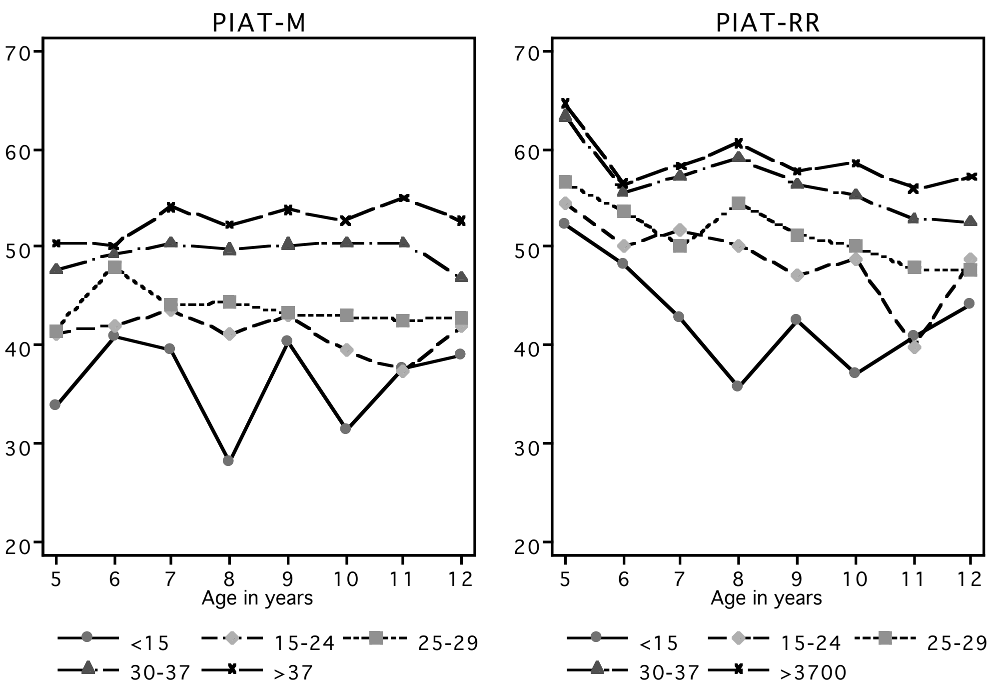


Notes: See Table 1 for sample restrictions. *Data source*: 1986-2000 NLSY-C.

Figure 1—MSD and PPVT Percentile Scores, by Birth Weight (100s grams) and Age



Notes: See Table 1 for sample restrictions. *Data source*: 1986-2000 NLSY-C.

Figure 2—PIAT-M and PIAT-RR Percentile Scores, by Birth Weight (100s grams) and Age

Table 1—Sample Restrictions (Numbers of Children)

Sample restriction	Sample MSD	PPVT	PIAT
Universe 1986-2000	11,205	11,205	11,205
Non-military, non-poor white sample	9,460	9,460	9,460
Non-missing birth weight	8,370	8,370	8,370
Non-missing dependent variable	5,558	7,017	6,986
Age 0-36 months, 3-4 years, and 5-12 years	4,857	3,621	6,911
At least one sibling in sample	3,316	2,113	5,800

Notes: *Data source*: 1986-2000 NLSY-C.

Table 2—Sample Means and Standard Deviations, by Sample

Variable	MSD		PPVT		PIAT	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
MSD percentile score	52.098	28.321	—	—	—	—
PPVT percentile score	—	—	34.197	28.820	—	—
PIAT-M percentile score	—	—	—	—	48.110	27.130
PIAT-RR percentile score	—	—	—	—	55.373	27.918
Birth weight (100g)	33.535	6.089	33.565	6.001	33.053	6.107
Child's age in months	19.475	9.925	48.437	6.756	104.806	27.357
Female	0.492	0.500	0.492	0.500	0.485	0.500
First born	0.300	0.458	0.332	0.471	0.378	0.485
Mother age at first birth	27.434	4.208	25.825	4.086	23.758	4.706
Ln family income	9.834	1.200	9.797	1.385	9.693	1.342
Family income missing	0.162	0.368	0.139	0.346	0.171	0.377
Married	0.742	0.437	0.714	0.452	0.612	0.487
Black	0.259	0.438	0.252	0.434	0.343	0.475
Hispanic	0.212	0.409	0.197	0.398	0.220	0.415
Ln AFQT	3.286	1.055	3.349	0.978	3.033	1.127
AFQT missing	0.043	0.202	0.036	0.186	0.039	0.195
Grandmother: dropout	0.436	0.481	0.419	0.479	0.532	0.481
Grandmother: high school	0.392	0.474	0.422	0.479	0.356	0.462
Grandmother: some college	0.097	0.288	0.094	0.283	0.068	0.243
Grandmother's education missing	0.059	0.235	0.059	0.236	0.070	0.255
East	0.165	0.371	0.140	0.347	0.142	0.349
Southeast	0.284	0.451	0.310	0.462	0.260	0.439
Central	0.330	0.470	0.324	0.468	0.386	0.487
West	0.212	0.409	0.217	0.412	0.206	0.404
n	3,316		2,113		5,800	

Notes: Samples are defined as in Table1. *Data Source*: 1986-2000 NLSY-C.

Table 3—The Effect of Birth Weight on MSD, PPVT, and PIAT, by Age

Age	*n*	Specification OLS1	OLS2	FE	OLS2-FE
		A. MSD			
0-36 months	3,316	0.467	0.494	0.539	-0.045
		(0.090)	(0.091)	(0.127)	(0.088)
0-18 months	1,537	0.701	0.747	0.672	0.075
		(0.137)	(0.138)	(0.199)	(0.144)
19-36 months	1,884	0.215	0.216	0.261	-0.045
		(0.119)	(0.120)	(0.155)	(0.099)
		B. PPVT			
3-4 years	2,113	0.349	0.123	-0.054	-0.177
		(0.103)	(0.093)	(0.126)	(0.084)
		C. PIAT-M			
5-12 years	5,800	0.382	0.214	0.348	-0.134
		(0.062)	(0.057)	(0.077)	(0.052)
5-8 years	5,140	0.347	0.211	0.305	-0.094
		(0.063)	(0.060)	(0.081)	(0.055)
9-12 years	4,415	0.476	0.297	0.404	-0.107
		(0.073)	(0.069)	(0.090)	(0.058)
		D. PIAT-RR			
5-12 years	5,800	0.369	0.251	0.332	-0.081
		(0.063)	(0.060)	(0.079)	(0.051)
5-8 years	5,140	0.321	0.255	0.202	0.053
		(0.064)	(0.061)	(0.081)	(0.053)
9-12 years	4,415	0.402	0.239	0.264	-0.025
		(0.078)	(0.075)	(0.092)	(0.053)

Notes: Each cell in columns (3) and (4) reports β estimated from Equation (1). In addition to the covariates included in the OLS1 specification, the OLS2 specification includes race/ethnicity, mother's AFQT, and grandmother's education (see text). Each cell in column (5) reports β estimated from Equation (4). Standard errors (in parentheses) account for arbitrary correlations in error at the mother-level. The standard error on the difference in coefficient estimates reported in column (6) is computed via a Hausman test. See Table 1 for sample restrictions. *Data source*: 1986-2000 NLSY-C.

Table 4—The Effect of Birth Weight on Test Scores by AFQT, and Birth Weight

Sample	n	Specification OLS1	OLS2	FE	OLS2-FE
		A. MSD			
AFQT<75	2,628	0.438	0.455	0.522	-0.067
		(0.098)	(0.100)	(0.137)	(0.094)
AFQT≥75	547	0.299	0.339	0.174	0.165
		(0.213)	(0.213)	(0.337)	(0.261)
BW≤3,300 grams	1,034	0.943	0.919	1.029	-0.110
		(0.192)	(0.193)	(0.260)	(0.174)
BW>3,300 grams	1,480	-0.183	-0.147	-0.363	0.216
		(0.216)	(0.217)	(0.296)	(0.201)
		B.PPVT			
AFQT<75	1,670	0.247	0.059	-0.108	0.167
		(0.110)	(0.099)	(0.134)	(0.090)
AFQT≥75	367	0.416	0.471	0.307	0.164
		(0.274)	(0.270)	(0.361)	(0.239)
BW≤3,300 grams	622	0.507	0.228	0.036	0.192
		(0.214)	(0.200)	(0.230)	(0.113)
BW>3,300 grams	914	-0.152	-0.138	-0.003	-0.135
		(0.262)	(0.236)	(0.322)	(0.220)
		C. PIAT-M			
AFQT<75	4,909	0.397	0.255	0.338	-0.083
		(0.067)	(0.062)	(0.083)	(0.055)
AFQT≥75	662	0.207	0.220	0.629	-0.409
		(0.172)	(0.167)	(0.257)	(0.195)
BW≤3,300 grams	2,099	0.488	0.322	0.369	-0.047
		(0.120)	(0.116)	(0.150)	(0.095)
BW>3,300 grams	2,419	-0.120	-0.136	0.283	-0.419
		(0.164)	(0.147)	(0.196)	(0.130)
		D. PIAT-RR			
AFQT<75	4,909	0.330	0.232	0.231	0.001
		(0.070)	(0.066)	(0.085)	(0.053)
AFQT≥75	662	0.192	0.210	0.430	-0.220
		(0.175)	(0.169)	(0.252)	(0.186)
BW≤3,300 grams	2,099	0.532	0.386	0.476	-0.090
		(0.128)	(0.123)	(0.151)	(0.088)
BW>3,300 grams	2,419	-0.048	-0.041	0.441	-0.482
		(0.161)	(0.144)	(0.202)	(0.143)

Notes: Each cell in columns (3) and (4) reports β estimated from Equation (1). In addition to the covariates included in the OLS1 specification, the OLS2 specification includes race/ethnicity, mother's AFQT, and grandmother's education (see text). Each cell in column (5) reports β estimated from Equation (4). Standard errors (in parentheses) account for arbitrary correlations in error at the mother-level. The standard error on the difference in coefficient estimates reported in column (6) is computed via a Hausman test. *Data source*: 1986-2000 NLSY-C.

Table A-1—The Effect of Birth Weight on MSD Percentile Scores

Variable	Specification OLS1	OLS2	FE
Birth weight (100g)	0.467	0.494	0.539
	(0.090)**	(0.091)**	(0.127)**
Child's age in months	0.120	0.127	0.140
	(0.050)*	(0.050)*	(0.058)*
Female	6.920	6.933	6.014
	(0.978)**	(0.974)**	(1.128)**
First born	6.468	6.349	6.325
	(1.136)**	(1.147)**	(1.407)**
Mother age at first birth	-0.228	-0.235	-0.417
	(0.136)	(0.139)	(0.222)
Ln family income	0.377	0.181	0.582
	(0.464)	(0.489)	(0.660)
Married	1.058	2.308	1.905
	(1.353)	(1.472)	(2.415)
East	-2.200	-1.682	-18.974
	(1.698)	(1.741)	(7.698)*
Southeast	-0.313	-0.256	2.258
	(1.381)	(1.424)	(5.810)
West	-3.267	-1.117	3.871
	(1.536)*	(1.635)	(5.624)
Ln family income missing	-0.128	0.202	0.772
	(1.384)	(1.379)	(1.839)
Black		4.414	
		(1.648)**	
Hispanic		-2.386	
		(1.697)	
Ln AFQT		0.732	
		(0.700)	
Grandmother: dropout		1.585	
		(2.312)	
Grandmother: high school		1.925	
		(2.158)	
Grandmother: some college		4.140	
		(2.724)	
AFQT missing		-1.884	
		(3.013)	
Grandmother's education missing		-0.007	
		(2.537)	
Constant	31.658	26.422	32.308
	(6.209)**	(7.530)**	(10.116)**
Observations	3,316	3,316	3,316
R-squared	0.04	0.05	0.06

Notes: Dependent variable is MSD percentile scores. Excluded categories are the central region, white non-Hispanic, and grandmother: college. Standard errors (in parentheses) account for arbitrary correlations in error at the mother-level. * significant at 5%; ** significant at 1%. See Table 1 for sample restrictions. *Data source*: 1986-2000 NLSY-C.

Table A-2—The Effect of Birth Weight on PPVT Percentile Scores

	Specification		
Variable	OLS1	OLS2	FE
Birth weight (100g)	0.349	0.123	-0.054
	(0.103)**	(0.093)	(0.126)
Child's age in months	0.128	-0.351	-1.163
	(1.182)	(1.076)	(1.169)
Female	2.118	1.811	1.222
	(1.110)	(0.999)	(1.123)
First born	10.543	5.626	2.379
	(1.306)**	(1.200)**	(1.386)
Mother age at first birth	0.509	-0.101	-0.637
	(0.190)**	(0.165)	(0.226)**
Ln family income	1.688	0.314	-0.878
	(0.599)**	(0.518)	(0.558)
Married	14.089	4.406	1.079
	(1.600)**	(1.371)**	(2.085)
East	13.967	8.334	-6.319
	(2.174)**	(1.942)**	(6.020)
Southeast	14.028	7.509	-5.139
	(1.801)**	(1.619)**	(5.832)
West	3.818	1.891	0.791
	(1.826)*	(1.638)	(4.946)
Ln family income missing	-2.870	-1.805	0.548
	(1.715)	(1.434)	(1.892)
Black		-16.984	
		(1.670)**	
Hispanic		-8.423	
		(1.966)**	
Ln AFQT		5.805	
		(0.794)**	
Grandmother: dropout		-15.849	
		(2.920)**	
Grandmother: high school		-10.280	
		(2.846)**	
Grandmother: some college		-4.380	
		(3.352)	
AFQT missing		-6.061	
		(3.140)	
Grandmother's education missing		-7.092	
		(2.026)**	
Constant	-28.965	19.845	65.346
	(8.013)**	(8.506)*	(9.756)**
Observations	2,113	2,113	2,113
R-squared	0.19	0.35	0.04

Notes: Dependent variable is PPVT percentile scores. Excluded categories are the central region, white non-Hispanic, and grandmother: college. Standard errors (in parentheses) account for arbitrary correlations in error at the mother-level. * significant at 5%; ** significant at 1%. See Table 1 for sample restrictions. *Data source*: 1986-2000 NLSY-C.

Table A-3—The Effect of Birth Weight on PIAT-M Percentile Scores

Variable	Specification OLS1	OLS2	FE
Birth weight (100g)	0.382	0.214	0.348
	(0.062)**	(0.057)**	(0.077)**
Child's age in months	0.632	0.500	0.750
	(0.171)**	(0.161)**	(0.180)**
Female	0.602	0.689	0.504
	(0.690)	(0.644)	(0.733)
First born	6.910	2.915	-0.625
	(0.746)**	(0.729)**	(0.894)
Mother age at first birth	1.111	0.446	-0.098
	(0.099)**	(0.097)**	(0.139)
Ln family income	1.790	0.492	0.417
	(0.323)**	(0.304)	(0.377)
Married	6.877	1.904	1.494
	(0.901)**	(0.851)*	(1.401)
East	2.804	0.180	-6.570
	(1.283)*	(1.135)	(4.435)
Southeast	3.149	-1.530	-8.144
	(1.019)**	(0.960)	(4.551)
West	-1.495	-1.922	-7.761
	(1.163)	(1.122)	(4.426)
Ln family income missing	-1.820	0.081	0.616
	(0.972)	(0.898)	(1.201)
Black		-6.300	
		(1.071)**	
Hispanic		-4.992	
		(1.185)**	
Ln AFQT		7.003	
		(0.409)**	
Grandmother: dropout		-11.377	
		(1.710)**	
Grandmother: high school		-9.403	
		(1.638)**	
Grandmother: some college		-7.044	
		(2.134)**	
AFQT missing		-1.679	
		(2.182)	
Grandmother's education missing		0.512	
		(1.355)	
Constant	-21.197	11.599	32.299
	(4.463)**	(4.947)*	(6.303)**
Observations	5,800	5,800	5,800
R-squared	0.10	0.21	0.01

Notes: Dependent variable is PIAT-M percentile scores. Excluded categories are the central region, white non-Hispanic, and grandmother: college. Standard errors (in parentheses) account for arbitrary correlations in error at the mother-level. * significant at 5%; ** significant at 1%. See Table 1 for sample restrictions. *Data source*: 1986-2000 NLSY-C.

Table A-4—The Effect of Birth Weight on PIAT-RR Percentile Scores

	Specification		
Variable	OLS1	OLS2	FE
Birth weight (100g)	0.369	0.251	0.332
	(0.063)**	(0.060)**	(0.079)**
Child's age in months	-0.738	-0.833	-0.528
	(0.174)**	(0.165)**	(0.182)**
Female	6.164	6.238	6.061
	(0.679)**	(0.647)**	(0.744)**
First born	10.707	7.157	4.373
	(0.747)**	(0.733)**	(0.907)**
Mother age at first birth	1.051	0.462	0.076
	(0.096)**	(0.095)**	(0.141)
Ln family income	1.699	0.466	0.409
	(0.325)**	(0.297)	(0.383)
Married	6.667	3.078	1.145
	(0.931)**	(0.877)**	(1.421)
East	4.043	1.959	-5.533
	(1.259)**	(1.168)	(4.498)
Southeast	0.776	-2.572	-8.458
	(1.032)	(0.960)**	(4.615)
West	-2.976	-2.684	-6.859
	(1.250)*	(1.224)*	(4.489)
Ln family income missing	-2.456	-0.629	-1.521
	(0.996)*	(0.939)	(1.218)
Black		0.742	
		(1.103)	
Hispanic		-0.610	
		(1.220)	
Ln AFQT		8.100	
		(0.440)**	
Grandmother: dropout		-8.268	
		(1.587)**	
Grandmother: high school		-6.392	
		(1.502)**	
Grandmother: some college		-5.974	
		(1.984)**	
AFQT missing		0.484	
		(2.343)	
Grandmother's education missing		-0.684	
		(1.511)	
Constant	-3.002	14.332	42.353
	(4.442)	(4.942)**	(6.392)**
Observations	5,800	5,800	5,800
R-squared	0.12	0.21	0.03

Notes: Dependent variable is PIAT-RR percentile scores. Excluded categories are the central region, white non-Hispanic, and grandmother: college. Standard errors (in parentheses) account for arbitrary correlations in error at the mother-level. * significant at 5%; ** significant at 1%. See Table 1 for sample restrictions. *Data source*: 1986-2000 NLSY-C.