MITIGATING THE EFFECTS OF LOW BIRTH WEIGHT: EVIDENCE FROM RANDOMLY ASSIGNED ADOPTEES*

Brian Beach
Department of Economics
University of Pittsburgh
4901 W.W. Posvar Hall
230 South Bouquet Street
Pittsburgh PA, 16260
412-648-1766
brb89@pitt.edu

Martin Saavedra†
Department of Economics
Oberlin College
227 Rice Hall
10 N Professor St
Oberlin OH, 44074
440-775-8483
Martin.Saavedra@Oberlin.edu

Abstract: Infants who are underweight at birth earn less, score lower on tests, and become less educated as adults. Previous studies have found mixed evidence that socioeconomic status mitigates these effects. In this paper, we reconcile these findings using a unique dataset in which adoptees were quasi-randomly assigned to families. We find that median income within a zip code mitigates the effects of low birth weight, as in Currie and Morreti (2007). Interactions between low birth weight and other family characteristics are not statistically significant, which is consistent with Currie and Hyson (1999). These results cannot be explained by differences in genetics, prenatal healthcare or neonatal healthcare.

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† Corresponding author.

I. INTRODUCTION

Infants who are underweight at birth earn less, score lower on tests, are more likely to commit crimes, and become less educated as adults.[1] Researchers have found mixed evidence that socioeconomic status (SES) mitigates the negative effects of low birth weight (LBW). Currie and Hyson (1999) find little evidence that SES, as measured by father's occupational status, mitigates the negative effects of LBW.[2] Lin, Liu, and Chou (2007) show that the decreased likelihood of college attendance is partially mitigated by parental education but only for moderately LBW and full-term LBW children. Zvara and Schoppe-Sullivan (2010) also show that the correlation between LBW and cognitive development at age three is weaker for children whose parents attended college. Cheadle and Goosby (2010), on the other hand, employ a family fixed effects analysis and find little evidence that family characteristics mitigate the effect of LBW on cognitive development or the likelihood of high school graduation. On the other hand, Currie and Moretti (2007) find that LBW women are more likely to have LBW children, and these effects are larger for individuals born in low-income zip codes.

Why these papers find conflicting results remains an open question. Almond and Currie (2011) argue that the differences between Currie and Hyson (1999) and Currie and Moretti (2007) could be due to improvements in technology. They note that Currie and Hyson analyze a 1958 birth cohort and that it is possible that there were few

[1] See Case, Fertig & Paxson (2005), Black et al (2007), Chen, Lin, and Liu (2010) and Currie and Almond (2011).
[2] Black et al (2005) also find no evidence that the effect of LBW is mitigated by mother's education, family income, or birth order, but they cannot rule out small effects because of their sample size.

effective interventions for LBW infants in 1958. Improvements in neonatal technology that might only be accessible to high SES families may explain the results in Currie and Moretti (2007), which uses data from a later period.[3] Alternatively, these papers might differ because they look at different populations or because they use different measures of socioeconomic status. Socioeconomic status is multidimensional and some measures may mitigate the negative effects of LBW whereas other measures might not.

In this paper, we ask whether childhood environment mitigates the effects of LBW using five measures of SES. Specifically, we use the median income in a family's zip code (the measure used by Currie and Moretti, 2007), mother's education, father's education, family income, and family size. To study how these measures interact with LBW, we use data from Sacerdote (2007) in which an adoption agency quasi-randomly assigned Korean orphans to American adoptive families. Because we have random assignment, childhood environment is not confounded by genetics, prenatal health, or neonatal healthcare. Consistent with the findings of Currie and Hyson (1999) and Cheadle and Goosby (2010), the interaction of LBW with parent's education, family income, or family size is not statistically significant. The interaction between LBW and median neighborhood income, however, is positive and significant. This indicates that neighborhood income mitigates the effects of LBW, which is consistent with Currie and Moretti (2007). Specifically, we find that a $4,600 (in 1970 dollars) increase in median neighborhood income from the sample mean would offset the negative effect of LBW on adult earnings. Additionally, a $3,600 increase would mitigate the negative effect on educational attainment.

[3] In the 1960s, the first neonatal intensive care units were developed, as were ventilators for infants and phototherapy for jaundice (Cutler and Meara 2000).

It is important to note that although adoptees were randomly assigned to families, families were not randomly assigned to neighborhoods. Consequently, unobservables correlated with neighborhood income might be driving our results. For instance, many of the symptoms of LBW manifest during childhood (McCormick, 1985; Brooks et al, 2001), and residing in a disadvantaged neighborhood decreases the likelihood of obtaining recommended preventative care (Kirby and Kaneda, 2005). Thus, neighborhood income might only matter to the extent that it is correlated with healthcare quality after the neonatal period.[4] It could also be that neighborhood income is a better proxy for wealth and lifetime earnings, which allow families to make compensatory investments.[5] How parents respond to early-life health shocks is theoretically unclear. Empirically, some have found that parental investments compensate for early-life health shocks (Loughran et al., 2008) whereas others have found that parents reinforce these shocks by allocating resources towards their healthier children (Datar et al., 2010).[6] Although we cannot precisely identify the mechanism, we can rule out genetics, prenatal environment, or neonatal environment as potential mechanisms.[7]

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[4] If the true mechanism is access to health care after the neonatal period, then it is important to note that the correlation between SES and healthcare quality likely differs by country. For example, SES should be a weaker predictor of healthcare quality in countries with universal healthcare.

[5] McClellan and Skinner (2006) note that individual-reports can be noisy measures of lifetime earnings and wealth because of reporting errors, the transitory nature of income, and inadequate measures of financial wealth. On the other hand, Geronimus et al. (1996) and Geronimus and Bound (1997) argue that aggregate measures cannot be interpreted as if they were micro-level variables; aggregate data reflect both characteristics of the individual and characteristics of the areas in which they live.

[6] See Almond and Mazumder (2012) for an overview of this literature.

[7] This, of course, does not imply that genetics, prenatal environment, or neonatal environment do not mitigate the effects of LBW. In fact, Bharadwaj et al (2013) find evidence that LBW infants with access to neonatal healthcare have lower mortality rates and score higher on tests.

Because many of the determinants of prenatal health are unobservable, researchers often use LBW as a proxy for poor prenatal health. Birth weight is determined by gestational age and intrauterine growth rate (Kramer, 1987). Infants born prematurely and the children of mothers who were born LBW are more likely to be LBW (Currie and Moretti, 2007). Furthermore, infants exposed to air pollution or famines while *in utero* have lower birth weights, higher infant mortality rates, and higher adult mortality (Currie, Neidell, and Schmieder, 2009; Almond et al, 2010; Barker, 1995). Maternal behaviors such as smoking or drinking during pregnancy also negatively influence birth weight (Markowitz et al, 2013; Evans and Ringel, 1999). Public policies targeting the health of pregnant women have had modest success at increasing birth weights (Almond et al, 2011; Currie and Gruber, 1996).

In most datasets, the unobserved determinants of LBW (genetics, prenatal environment, and maternal behaviors) are correlated with childhood SES (both observed and unobserved).[8] To identify the causal effect of fetal nutritional intake while holding genetics and family environment constant, many studies have used twin designs (Almond et al, 2005; Oreopoulos et al, 2008; Black et al, 2007; Royer 2009). In our data, family environment is quasi-randomly assigned; therefore, the unobserved determinants of birth weight are orthogonal to childhood SES. Birth weight has the potential to affect long-run economic outcomes through several channels, such as cognitive development and adult health. Consistent with this literature, we find that low birth weight infants earn less and become less educated as adults.

[8] Although children from low SES families are more likely to be LBW, Costa (1998) shows that this difference has narrowed since 1900.

II. DATA

II.a. Survey

Holt International Children's Services is an organization that places Korean orphans into adoptive families.[9] A unique feature of the Holt adoption program is that children were quasi-randomly assigned to families. The only exception to this is that families could avoid special needs children. For this reason, special needs children are excluded from our sample as well as the sample used in Sacerdote (2007). Holt assigned children to families using their application date and a first come, first served rule. Because potential applicants did not know whether the next available child is healthy or not, adoptive families could not have strategically timed their applications to increase the likelihood of adopting a healthier child. To be eligible to adopt, adoptive parents had to be married for at least three years, have no more than four children, have a household income greater than 125% of the Federal Poverty Line, and be between the ages of 25 and 45. Many of these restrictions were imposed by Korean and U.S. law.

Sacerdote (2007) surveyed adoptive families and obtained information regarding their family characteristics at the time of adoption and the adult outcomes for each of their children. Sacerdote surveyed 3,500 of the 7,700 families that adopted a child through Holt's Korea program between 1970 and 1980.[10] Of the surveyed families, 1103 (or 32 percent) responded to the initial survey. Sacerdote re-surveyed 400 of the non-responders, receiving an additional 141 responses. Comparing the two samples,

[9] Much of this paragraph is based off of both Sacerdote (2007) and Holt's website.

[10] Sacerdote also surveyed a subset of children (both adoptees and non-adoptees) to measure the extent to which parents and children reported the same outcome data (e.g. years of education, income, and marital status). The response rate for this survey was 55 percent and the rate of agreement was high.

Sacerdote finds no statistically significant relationship between a child's outcomes and whether a family responded to the initial survey. Using Holt's records from the time of adoption, Sacerdote finds some evidence that higher income and higher educated families were less likely to respond to the survey. However, as evidenced in Table 2 in our paper and Table 3 in Sacerdote's paper, there is still a significant amount of variation in parental characteristics from those that responded.

Sacerdote linked these data to Holt records and showed that family characteristics do not predict an adoptee's initial health, age at time of adoption, or gender. Because family environment was quasi-randomly assigned to adoptees, these data allowed Sacerdote to separately identify the effects of family environment (nature) and genetics (nurture). Sacerdote finds height, weight, and BMI to be determined by genetics; smoking and drinking behavior appear to be influenced by family environment; education and income appear to be influenced by both nature and nurture.

II.b. Birth weights

In addition to the outcome data collected by Sacerdote, we rely extensively on one other variable: the weight that first appears in the adoptees' medical records. These "initial weights" come from Holt records, but they do not always correspond to an adoptees' birth weight. This can be seen more explicitly in Figure 1 where we plot the distribution of these weights. The vertical line corresponds to 4.5 kilograms, which is the 99th percentile of the U.S. birth weight distribution. The thick right tail in the Holt distribution results from adoptees entering Holt with a medical history that does not date back to birth. Unfortunately, it is not possible to restrict our sample to those whose initial weight is their birth weight because the age at which the initial weight was taken is not reported in the Holt data. We would like to treat initial weights as birth weights to determine whether an adoptee was low birth weight, but low birth weight adoptees that cross the LBW threshold before their medical history begins will be misclassified as normal birth weight.[11]

To reduce the likelihood of misclassification, we truncate the sample by excluding adoptees with an initial weight greater than 4.5 kilograms, the 99th percentile of the U.S. birth weight distribution.[12] Truncation also addresses a second problem: higher initial weights are likely correlated with late entrance into the Holt program, and late entrance will be associated with a longer time in their pre-adoption environment. However, children grow rapidly in the first months of life and 4.5 kilograms becomes the 3rd percentile of the weight distribution by age 3 months.[13] Thus, for the set of adoptees whose "initial weight" corresponds to their weight at the time they entered Holt, truncation at 4.5 kilograms also limits the potential time that an adoptee spent in their pre-adoption environment.

Figure 2 illustrates that truncation leaves us with a distribution of weights that resembles a birth weight distribution. In the first panel of Figure 2 we plot the truncated distribution of Holt initial weights against the U.S. birth weight distribution.[14] In the second panel we restrict the U.S. birth weight distribution to the set of children whose

[11] As is standard in the literature, we classify children as low birth weight if they are born weighing less than 2.5 kilograms. This is justified further in Section II.c.
[12] U.S. birth weight distribution comes from a 2012 CDC vital statistics report (Table F, http://www.cdc.gov/nchs/data/nvsr/nvsr62/nvsr62_09.pdf#table25). According to Shin et al (2005) the 95th percentile for Korean children with a gestational age of 42 weeks is 4.2 kilograms.
[13] CDC weight for age tables are available at: http://www.cdc.gov/growthcharts/html_charts/wtageinf.htm.
[14] The U.S. birth weights were obtained from the 1988 Integrated Health Interview Series.

mothers receive Aid to Families with Dependent Children (AFDC). For the sake of comparability, we also truncate the U.S. birth weight distributions at 4.5 kilograms.[15] Although adoptee initial weights are less than the U.S. population as a whole, they are similar to children whose mothers receive AFDC. Specifically, the mean "initial weight" for the truncated sample of adoptees is 3.19 kilograms while the mean birth weight from the truncated sample of children whose mothers receive AFDC is 3.14 kilograms. Furthermore, 14.3 percent of the truncated Holt sample has an initial weight less than 2.5 kilograms, which compares to 13.8 (the percent of children in the truncated AFDC sample that were low birth weight). A Pearson Chi-Squared test comparing LBW in the truncated Holt and AFDC samples produces a test statistic of 0.106 with a p-value of 0.744. For these reasons, we are confident that truncating the sample at 4.5 kilograms allows us to recover a distribution of birth weights with minimal error. Accordingly, we treat "initial weights" as birth weights for the remainder of this paper.

Our results are not sensitive to truncation. We illustrate more precisely in Section IV where we replicate our main findings using the full sample of adoptees. As a robustness check, we truncate the data using age of arrival rather than initial weight, since adoptees that entered Holt during the first year of life would be more likely to have medical histories dating back to their births.[16] Truncating based on age of arrival produces nearly identical results.

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[15] Because 4.5 kilograms corresponds to the 99th percentile of the U.S. birth weight distribution, plotting the truncated distribution of initial weights against the full distribution of U.S. birth weights produces a qualitatively similar picture.

[16] The age at which the adoptee entered Holt does not necessarily correspond to the age at with the "initial weight" was taken. For example, there are adoptees that entered Holt at age two with an initial weight of about 5 kilograms. It is highly unlikely that the weight of 5 kilograms corresponds to the adoptees birth weight or their weight at age two.

II.c. Quasi-random assignment and summary statistics

Sacerdote (2007) shows that parental characteristics do not predict an adoptee's initial health. In Table 1, we show that this remains true for our preferred sample and the other samples used as robustness checks in Section IV. Specifically, we regress an indicator variable for whether the child is LBW (less than 2.5 kg) on parental education, family income, neighborhood income, number of children, parental heights, BMI, and drinking behavior. Parental characteristics are neither individually nor jointly significant at predicting whether an adoptee is LBW.

Table 2 produces summary statistics restricting Sacerdote's dataset to adoptees that weighed less than 4.5 kg at their initial medical history. This dataset contains 535 adoptees, of which nearly 450 were at least 25 years old at the time of the survey. We restrict attention to adoptees over the age of 25 when examining adult income, educational attainment, and college attendance. The sample is only 25 percent male, suggesting that females were more likely to be given up for adoption (see Edlund and Lee (2013) for a discussion on son preference in Korea).[17] Most adoptees arrived to the United States around age one. Information on parental characteristics at the time of adoption was obtained from a retrospective survey conducted by Bruce Sacerdote in the early 2000s (described above). Adoptive mothers had an average of 15.06 years of schooling while adoptive fathers had an average of 15.89. Adoptees over the age of 25 have an average of 15.03 years of schooling. Family income is reported categorically

[17] In the five percent sample of the 2000 Census (Ruggles, et al, 2010), only 43 percent of adopted children born in Korea are male.

for both parents and children.[18] Consequently, three adoptees and one adoptive family are top-coded with family income greater than 200,000. Neighborhood income refers to the median income (in 1980) for those residing in the same zip code as the adopting family. This measure was obtained from the Census; it is not simply an average across Holt families that happen to reside in the same zip code.

In the third panel of Table 2 we obtain family characteristics from the 1970 Census neighborhoods sample. This one percent sample was obtained from IPUMS. We restrict to households with at least one child. Relative to the U.S. population, adoptive families are more educated and earn more, but they live in zip codes with similar average incomes. For the sake of comparability, neighborhood income in panel three was adjusted to 1980 dollars and family income is transformed into a categorical variable (where the categories match Sacerdote's survey). As in the second panel, a family's neighborhood income refers to the median income for all households (with or without children) residing in the same zip code as the family. It should be noted that we do not make the same restrictions for the sample of U.S. families that were required for adoptive families. Therefore, the U.S. sample includes single parents, parents of all ages, and those with incomes below 125% of the poverty line.

Table 3 presents the correlations between each of our five measures of SES. Although income, neighborhood income, and parent's education are positively correlated, with the exception of mother's and father's education, the correlation between any of the other measures never exceeds 0.20 in the adoptee sample. The

[18] The categories are as follows: Less than 20,000; between 20,000 and 30,000; between 30,000 and 50,000; between 50,000 and 70,000; between 70,000 and 100,000; between 100,000 and 150,000; between 150,000 and 200,000; and greater than 200,000.

correlations are somewhat stronger for our sample of U.S. families, but they never exceed 0.35, with the exception of mother's and father's education. This highlights the multidimensionality of SES and illustrates why papers that only use one measure of SES might find conflicting results.

III. RESULTS

Figure 3 visually displays our data. Figure 3 presents local linear smooth regressions of the adoptee's years of schooling for each measure of SES: mother's education, father's education, log of family income, the number of children in the family (including the adoptee), and the log of the median income within the neighborhood. Because we are using a small dataset, our data is not well suited for non-parametric analysis, and the standard errors for the estimated difference between these two smoothed regression lines are large.[19] Consequently, Figure 3 should be viewed as a visualization of the data and not as further evidence in support of our findings. Nevertheless, several features of Figure 3 are consistent with our findings. First, LBW adoptees become less educated than normal birth weight adoptees for every level of mother's education, father's education, family income, family size, and neighborhood income. Second, the effect of LBW (measured by the gap between the two nonparametric regression lines) remains approximately constant across levels of mother's education, father's education, family income, and family size. In other words, these measures of socioeconomic status are not mitigating the negative effects of LBW. The birth weight gap does narrow for adoptees from higher income neighborhoods.

[19] For this reason, we use parametric methods for our main analysis.

Figure 3 indicates that a LBW adoptee assigned to a low-income neighborhood would attain one-half fewer years of education, on average, than a normal birth weight adoptee assigned to the same neighborhood. However, that gap almost disappears for adoptees from high-income neighborhoods. Neighborhood income appears to mitigate the effects of low birth weight. Again, this graph should be viewed with caution, as the standard errors of these regressions are large and do not control for other variables. To address these issues, we turn to parametric analysis.

We analyze the effect of LBW on adult outcomes and the extent to which quasi-randomly assigned childhood characteristics mitigate the negative effects of birth weight by estimating variations of the following equation:

$$y_i = \alpha + \beta LBW_i + Z_i{}'\Gamma + (LBW_i * Z_i)'\Delta + \ \theta male_i + age\ FE's + \epsilon_i \qquad (1)$$

$$\text{where } Z_i = \begin{bmatrix} log(zip\ income) \\ log(family\ income) \\ mother's\ education \\ father's\ education \\ number\ of\ children \end{bmatrix}.$$

The outcome variable, y_i, is either years of schooling, log of income, college attendance, or BMI. The variable LBW_i is an indicator equal to one if the adoptee's birth weight is less than 2.5kg. This is the LBW cutoff used by Currie and Hyson (1999) and Currie and Moretti (2007). It is also the cutoff used by the CDC, WHO, and many other academic papers. The vector Z_i contains measures of socioeconomic status. The interaction term measures the extent to which variables in Z_i mitigate the effects of LBW. We are interested in understanding the coefficients within the Δ vector. Each regression also includes indicators for gender and age.

One might be concerned about how misclassification and truncation of the initial weight distribution affect the estimates. Since we are inferring low birth weight status from initial weights, some individuals will be misclassified. This will tend to attenuate our estimates.[20] As noted in the previous section, truncating the distribution at 4.5 kilograms reduces the likelihood of misclassification. This reduction in noise, however, comes at the expense of a reduction in sample size. Because initial weights are only used to infer LBW status, truncation does not reduce the range of our variable of interest.

Table 4 presents our results. For each outcome variable we present three specifications. First, we include the LBW indicator variable with no interactions. Second, we interact LBW with log of neighborhood income. Third, we interact LBW with log of neighborhood income, mother's education, father's education, family income, and the number of children within the family. This is the specification presented in equation (1). As a robustness check, we consider each unique specification that can be constructed by interacting LBW with each measure of socioeconomic status. The results of that exercise (presented in Table 6 and discussed in Section IV) are qualitatively similar to the results presented in Table 4.

In Table 4, we find that LBW adoptees earn approximately 20% less and attain one-half fewer years of education. Because the interaction with neighborhood income is positive and significant at the 5% level for all regressions, we conclude that neighborhood income mitigates the effects of LBW. These results imply that a $4,600

[20] Misclassification should attenuate our results if LBW adoptees are randomly misclassified as normal birth weight. If, however, only the relatively heavier LBW adoptees are misclassified, then what we measure is very LBW.

(in 1980 dollars) increase in median neighborhood income from the sample mean would mitigate the negative effect of LBW on adult earnings. Additionally, a $3,600 increase would mitigate the negative effect on educational attainment. None of the other SES measures are significant at the 10% level. We repeat this analysis in Table 5 using BMI as the dependent variable. For BMI, we find no evidence of a LBW effect or mitigation at conventional levels of significance for any measure of SES.

At first these results might appear to conflict with Sacerdote (2007), as he does not find that neighborhood income affects economic outcomes. This difference occurs because neighborhood income only affects LBW adoptees (see Figure 3). The majority of the sample is normal birth weight, which explains why Sacerdote finds no effect from neighborhood income.

As noted in the introduction, it is important to remember that adoptive families were not randomly assigned to neighborhoods. Thus, it is possible that unobservables correlated with neighborhood income are driving our results. Healthy foods, for instance, are not as accessible in lower income neighborhoods (Franco et al. 2008), and growing up in a lower income neighborhood is associated with behavioral problems, teenage pregnancy, and dropping out of high school (Brooks-Gunn et al. 1993; Duncan et al. 1994). Additionally, children that attend schools with high concentrations of low-income students fare worse academically (Lippman et al, 1996). Neighborhood income likely reflects demographic characteristics (e.g. the share of college-educated adults, or the share of divorced/single parents), which might be important given the literature on peer effects (Gaviria and Raphael, 2001; Hanushek et al., 2003; Sacerdote, 2001). Of course, as discussed in the introduction, it is also possible that median neighborhood income only matters to the extent that it is a better proxy for certain family characteristics (e.g. wealth or lifetime earnings). Nevertheless, genetics, prenatal environment, and neonatal environment can be ruled out as potential mechanisms since (for our sample) they are all independent of neighborhood income.

IV. ROBUSTNESS CHECKS

This section examines the robustness of our results. Specifically, we show that our results are not sensitive to the inclusion of specific interaction terms or the truncation of the sample.

Because we use five measures of SES, it is possible to model the effect of LBW and its interaction with SES in many different ways. We can estimate the effect of LBW by itself; we can include an interaction with one measure of SES; we can include all five measures of SES; or we can include any subset of those interactions. There are 31 unique ways to organize our five interactions – 32 if we include the specification with no interactions. In Table 6, we ask whether our results are consistent across these specifications. For each outcome variable, we run each of the 32 specifications. Each interaction appears sixteen times, and Table 6 reports the number of times the interaction produces a p-value less than 0.05 or less than 0.01. Regardless of whether the outcome variable is log of income, educational attainment, or college attendance, the interaction of LBW and log of neighborhood income is significant at the 5% level for every specification that it appears in. For these outcome variables, none of the other interactions are ever significant at the 5% level. When the outcome of interest is BMI, the interaction of LBW and log of neighborhood income is never significant. With regards to the effect on BMI, the only interaction that is significant at the 5% level is the interaction of LBW with mother's education. However, that interaction is only significant in one of the sixteen specifications. These results are consistent with the results presented in Tables 4 and 5 – neighborhood income mitigates the effects of low birth weight for economic outcomes but it does not affect BMI.

As discussed in Section II, we do not observe birth weights for adoptees that entered Holt with an incomplete medical history. Low birth weight adoptees that cross the LBW threshold before their medical history begins will be misclassified as normal birth weight. Normal birth weight children, however, should not be misclassified as LBW because weight tends to increase monotonically during the first years of life. If LBW adoptees are randomly misclassified as normal birth weight then this should attenuate our results and should bias our results towards zero. However, if only the relatively heavier LBW infants are misclassified as normal birth weight because they are closer to the LBW threshold, then what we are really measuring is very LBW. In Tables 4 and 5 we limit this bias by restricting our analysis to adoptees with an initial weight less than 4.5kg. This restriction allows us to recover a birth weight distribution resembling that of the United States. To illustrate that our results are not sensitive to this cutoff, we relax this restriction in Table 7.

In the first four columns of Table 7 we estimate the effect of LBW and its interaction with each measure of SES for the non-truncated sample. In the last four columns we restrict analysis to adoptees that entered Holt before age one. This restriction is a natural alternative to truncating by initial weight because an adoptee that enters Holt closer to their date of birth is more likely to arrive with a complete medical history. In both cases, we find results similar to those in Tables 4 and 5. For economic outcomes, the effect of LBW is always negative and is significant for four of the six specifications. Moreover, the interaction of LBW and log of neighborhood income is positive and significant for each specification. The interaction of LBW and the other four measures of SES appears 32 times in Table 7, but only two of those coefficients are significant at the 10% level.

IV. CONCLUSION

Does socioeconomic status mitigate the negative effects of low birth weight? The answer appears to depend on how socioeconomic status is measured. Currie and Hyson (1999) measure SES with father's occupation and find little evidence that SES mitigates the effects of LBW. Currie and Moretti (2007), on the other hand, measure SES as the poverty rate within a zip code and find that SES does mitigate the effects of LBW. Of the five measures considered, only the interaction of median zip code income with LBW is positive and statistically significant. This result cannot be explained by genetics, prenatal healthcare, or neonatal healthcare being correlated with family environment because adoptees were quasi-randomly assigned to families.

This analysis raises the question of why neighborhood income matters more than other family characteristics. One possibility is schooling. Schools in high-income neighborhoods might offer better remedial programs to help struggling students. Another possibility is that neighborhood characteristics are a better proxy for individual wealth than individual income or education. It could also be that high-income neighborhoods have access to better healthcare during the adoptee's childhood. These explanations are neither mutually exclusive nor jointly exhaustive, but two aspects of our dataset prevent us from addressing these mechanisms. First, detailed neighborhood-level school quality, wealth, and healthcare data are not available for the 1964 to 1985 time period (the years in which these orphans were adopted). Second, although children were randomly assigned to families, families were not randomly assigned to neighborhoods. Therefore, families sorting into neighborhoods along unobservable dimensions could drive this result. However, this sorting must be independent of birth weight, genetics, and prenatal environment. Why neighborhood income mitigates the negative effects of LBW remains an open question for future research.

Although the unique nature of the Holt Institution ensures that genetics and *in utero* environment are orthogonal to SES, it also raises the concern of whether these results are externally valid to the general non-adopted population. The set of families eligible to adopt from Holt are not fully representative of families with young children in the United States. Adoptive families must have had an income of at least 125% of the Federal Poverty Line, must be married, and must be between the ages of 25-45. Any policy attempting to mitigate the negative effects of LBW for non-adoptees would likely affect poorer and younger families as well as single mothers. For these reasons, applying these results to the general population should be viewed with caution.

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Table 1: Relationship between family characteristics and whether the adoptee is LBW

	(1)	(2)	(3)
	Full sample	Arrived at Holt by age one	Holt initial weight <4.5kg
Mother's education	0.004	0.005	0.009
	(0.005)	(0.007)	(0.010)
Father's education	0.002	0.002	0.002
	(0.004)	(0.006)	(0.008)
Log of family income	-0.016	-0.033	-0.042
	(0.017)	(0.024)	(0.030)
Log of neighborhood income	-0.019	-0.032	-0.006
	(0.041)	(0.058)	(0.079)
Number of children	-0.002	-0.000	0.011
	(0.008)	(0.011)	(0.015)
Mother's BMI	0.002	0.002	0.003
	(0.002)	(0.003)	(0.004)
Father's BMI	0.001	0.002	0.003
	(0.002)	(0.004)	(0.005)
Mother drinks	-0.023	-0.030	-0.059
	(0.026)	(0.038)	(0.048)
Father drinks	0.022	0.042	0.062
	(0.026)	(0.038)	(0.048)
Mother's height (inches)	-0.003	-0.004	-0.006
	(0.004)	(0.005)	(0.006)
Father's height (inches)	-0.003	-0.004	-0.004
	(0.004)	(0.006)	(0.008)
R-squared	0.063	0.085	0.112
Observations	936	630	485
F test, parental coeffs=0	0.648	0.665	0.793
p-value: Joint significance	0.663	0.650	0.556

Robust standard errors (clustered at the family level) in parentheses.
* p<0.10; ** p<0.05; *** p<0.01

Table 2: Summary statistics

Characteristics of the adoptee					
	Mean	SD	Min	Max	Observations
Low birth weight	0.144	0.351	0	1	535
Male	0.234	0.424	0	1	535
Age	28.867	3.834	19	40	535
U.S. arrival age	1.108	0.545	1	5	529
Married	0.426	0.495	0	1	530
BMI	23.040	3.828	16.499	38.008	522
Income*	50.252	36.640	10	200	437
Attended college*	0.565	0.496	0	1	448
Education*	15.033	2.110	9	21	448
Characteristics of the adopting family at the time of adoption					
	Mean	SD	Min	Max	Observations
Mother's education	15.060	2.450	9	20	535
Father's education	15.888	2.864	9	20	535
Log of family income	3.165	0.672	2.303	5.298	535
Log of neighborhood income	2.953	0.256	2.179	3.743	535
Number of children	3.2	1.398	1	7	535
Characteristics of US families in 1970					
	Mean	SD	Min	Max	Observations
Mother's education	11.107	2.780	0	17	91881
Father's education	11.010	3.250	0	17	78453
Log of family income	2.403	0.326	2.303	4.094	97276
Log of neighborhood income	2.984	0.328	1.445	4.623	97276
Number of children	2.351	1.449	1	9	97276

Adoptee sample restricted to adoptees with an initial weight of less than 4.5kg. Characteristics of US families obtained from IPUMS 1970 neighborhood sample, restricting to families with at least one child. Family income is binned at the same intervals as the adoptee survey to make the data comparable. However, the US sample is top coded at $50,000 while the adoptee sample is top coded at $200,000. Neighborhood income from 1970 census adjusted to 1980 dollars so that it is comparable with adoptee sample. Although families could not have more than 4 children at the time of adoption, this constraint does not restrict a family from having more children after adoption. Thus it is reasonable to observe families with more than four children.

*Restricted to adoptees over the age of 25.

Table 3: Correlation between each measure of socioeconomic status

Panel a: Characteristics of the adopting family at the time of adoption

	Log of neighborhood income	Log of family income	Mother's education	Father's education	Number of children
Log of neighborhood income	1	0.119	0.148	0.119	-0.013
Log of family income		1	0.157	0.043	-0.049
Mother's education			1	0.562	0.057
Father's education				1	0.045
Number of children					1

Panel b: Characteristics of US families in 1970

	Log of neighborhood income	Log of family income	Mother's education	Father's education	Number of children
Log of neighborhood income	1	0.241	0.293	0.350	-0.059
Log of family income		1	0.147	0.169	0.002
Mother's education			1	0.609	-0.081
Father's education				1	-0.073
Number of children					1

Sample of adopting families restricted to adoptees with an initial weight less than 4.5kg. Characteristics of US families obtained from IPUMS 1970 neighborhood sample, restricting to families with at least one child. Family income in US sample is top coded at $50,000, compared to $200,000 in adoptee sample.

Table 4: The effect of low birth weight on adult economic outcomes

	Log of income			Educational attainment			College attendance		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Low birth weight	-0.196**	-2.791***	-2.810**	-0.422	-7.444**	-5.681*	-0.082	-1.836***	-1.562*
	(0.0988)	(1.027)	(1.272)	(0.262)	(3.098)	(3.293)	(0.0622)	(0.701)	(0.844)
Log of neighborhood income * LBW		0.885**	0.863**		2.395**	2.503**		0.598**	0.581**
		(0.343)	(0.341)		(1.066)	(1.131)		(0.239)	(0.249)
Mother's education * LBW			-0.003			0.041			0.008
			(0.039)			(0.129)			(0.029)
Father's education * LBW			0.017			-0.038			0.002
			(0.035)			(0.126)			(0.028)
Log of family income * LBW			-0.036			-0.625			-0.079
			(0.140)			(0.440)			(0.104)
Number of children * LBW			-0.010			-0.056			-0.042
			(0.078)			(0.160)			(0.039)
Marginal effect of LBW	-0.196**	-0.176*	-0.175*	-0.422	-0.371	-0.408	-0.082	-0.069	-0.072
	(0.0988)	(0.095)	(0.096)	(0.262)	(0.261)	(0.270)	(0.0622)	(0.061)	(0.064)
Observations	437	437	437	448	448	448	448	448	448
R-squared	0.146	0.157	0.158	0.089	0.099	0.104	0.080	0.091	0.094

Notes: Sample restricted to adoptees over the age of 25. Robust standard errors (clustered at the family level) reported in parentheses. Each regression controls for log of family's zip code income, mother's education, father's education, and gender. Each regression also includes a set of dummy variables for parent's income category, the child's age, and the number of children within the family. ** p<0.10; ** p<0.05; *** p<0.01*

Table 5: The effect of low birth weight on BMI

	(1)	(2)	(3)
Low birth weight	0.514	-1.967	5.254
	(0.534)	(5.395)	(7.177)
Log of neighborhood income * LBW		0.843	1.709
		(1.826)	(1.820)
Mother's education * LBW			-0.264
			(0.193)
Father's education * LBW			-0.143
			(0.198)
Log of family income * LBW			-0.491
			(0.859)
Number of children * LBW			-0.602
			(0.389)
Marginal effect of LBW	0.514	0.526	0.627
	(0.534)	(0.534)	(0.516)
Observations	522	522	522
R-squared	0.099	0.100	0.115

Robust standard errors (clustered at the family level) reported in parentheses. Each regression controls for log of family's zip code income, mother's education, father's education, and gender. Each regression also includes a set of dummy variables for parent's income category, the child's age, and the number of children within the family. * p<0.10; ** p<0.05; *** p<0.01

Table 6: Proportion of specifications with significant coefficients

	Log of income		Educational attainment		College attendance		BMI	
	$p<0.05$	$p<0.01$	$p<0.05$	$p<0.01$	$p<0.05$	$p<0.01$	$p<0.05$	$p<0.01$
Low birth weight	17/32	2/32	9/32	0/32	12/32	1/32	3/32	0/32
Log of neighborhood income * LBW	16/16	5/16	16/16	0/16	16/16	2/16	0/16	0/16
Mother's education * LBW	0/16	0/16	0/16	0/16	0/16	0/16	1/16	0/16
Father's education * LBW	0/16	0/16	0/16	0/16	0/16	0/16	0/16	0/16
Log of family income * LBW	0/16	0/16	0/16	0/16	0/16	0/16	0/16	0/16
Number of children * LBW	0/16	0/16	0/16	0/16	0/16	0/16	0/16	0/16

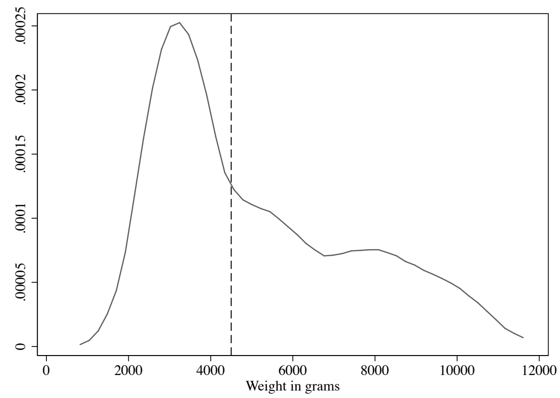
Note: This tables summarizes the results of repeating the analyses in Tables 3 and 4 for every possible subset of the interactions between LBW and the five measures of SES.

Table 7: The effect of low birth weight on adult outcomes

	Non-truncated sample				Adoptees entering Holt before age one			
	Log of income	Educational attainment	College attendance	BMI	Log of income	Educational attainment	College attendance	BMI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low birth weight	-2.366**	-5.455*	-1.446*	5.971	-2.035*	-3.038	-0.910	6.809
	(1.157)	(3.012)	(0.760)	(6.644)	(1.223)	(3.174)	(0.813)	(7.137)
Log of neighborhood income * LBW	0.918***	2.505**	0.574**	1.098	0.857***	2.101*	0.492**	0.755
	(0.305)	(1.042)	(0.225)	(1.624)	(0.327)	(1.154)	(0.249)	(1.794)
Mother's education * LBW	-0.014	-0.026	-0.003	-0.338*	-0.022	-0.012	-0.001	-0.180
	(0.033)	(0.124)	(0.027)	(0.183)	(0.037)	(0.129)	(0.029)	(0.189)
Father's education * LBW	-0.027	-0.018	0.001	-0.059	-0.019	-0.063	-0.003	-0.132
	(0.031)	(0.125)	(0.028)	(0.190)	(0.0347)	(0.126)	(0.029)	(0.197)
Log of family income * LBW	0.054	-0.482	-0.048	-0.457	0.011	-0.788*	-0.133	-0.637
	(0.126)	(0.428)	(0.100)	(0.809)	(0.148)	(0.458)	(0.105)	(0.918)
Number of children * LBW	-0.019	-0.069	-0.050	-0.344	-0.028	-0.019	-0.051	-0.574
	(0.066)	(0.149)	(0.034)	(0.379)	(0.072)	(0.148)	(0.036)	(0.406)
Marginal effect of LBW	-0.172**	-0.446*	-0.085	0.644	-0.183*	-0.531**	-0.094	0.409
	(0.086)	(0.260)	(0.060)	(0.488)	(0.095)	(0.263)	(0.062)	(0.511)
Observations	856	877	877	995	572	585	585	673
R-squared	0.061	0.030	0.022	0.058	0.046	0.031	0.033	0.082

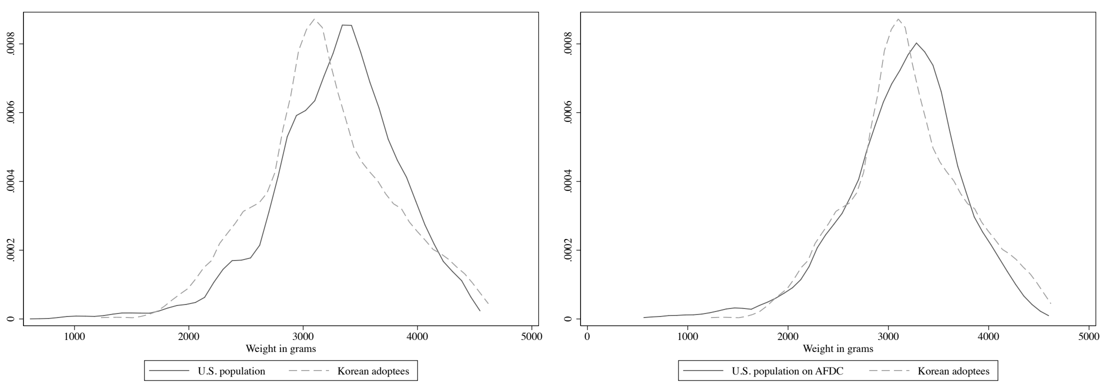
Notes: For log of income, educational attainment, and college attendance outcomes the sample is restricted to adoptees over the age of 25. The non-truncated sample includes all adoptees regardless of initial weight and treats those with an initial weight greater than 2.5kg as normal birth weight. In columns 5-8, uses the full sample but restricts analysis to adoptees that arrived at Holt by age one. Robust standard errors clustered at the family level. Each regression controls for log of family's zip code income, mother's education, father's education, and gender. Each regression also includes a set of dummy variables for parent's income category, the child's age, and the number of children within the family. * $p<0.10$; ** $p<0.05$; *** $p<0.01$

Figure 1: Distribution of Holt initial weights



Note: The vertical line at 4.5 kilograms corresponds to the 99th percentile of the U.S. birth weight distribution.

Figure 2: U.S. and Korean weight distributions



Sources: AFDC birth weight refers to the birth weights of U.S. children whose mothers receive AFDC. These data come from the 1988 sample of the Integrated Health Interview Series. All distributions are truncated at 4.5 kilograms (the 99th percentile of the U.S. birth weight distribution).

Figure 3: The impact of socioeconomic status on educational attainment