Does Money Matter? The Effects of Cash Transfers on Child Health and Development in Rural Ecuador

Christina Paxson
Center for Health and Wellbeing
Princeton University

Norbert Schady
World Bank

January 2007

We thank the Center for Economic and Policy Studies at Princeton University for funding a portion of this study, and Tom Vogl and Lisa Vura-Weiss for excellent research assistance. We also acknowledge the collaboration at every stage of this project with our colleagues at the Secretaría Técnica del Frente Social in Ecuador, in particular Santiago Izquierdo, Mauricio León, Ruth Lucio, Juan Ponce, José Rosero and Yajaira Vázquez.
This paper is a draft: Do not cite or quote without permission from the authors. Comments are welcome.

I. Introduction

In 2003, the government of Ecuador launched a new cash transfer program—the *Bono Desarrollo Humano* (BDH)—targeted to poor families with children. The BDH was originally meant to be a conditional cash transfer program similar in structure to Mexico's *Oportunidades* program, in which eligible mothers receive transfers only if their pre-school children receive regular health checkups and their school-aged children are enrolled in school.[1] However, although conditionality may be imposed at some date in the future, it has not happened yet. Instead, for the past several years, eligible families have received what is referred to as "the Bono" with no strings attached. The transfers are small: only $15 per month per family. However, on average, the transfer represents a non-trivial 10 percent increase in family expenditure for eligible families.

As was the case for *Oportunidades*, random assignment was built into the roll-out of the BDH. Two separate randomized experiments were conducted. One was designed to examine the effects of the Bono on educational attainment among school-aged children. The other experiment—which is the concern of this paper—was designed to examine how the Bono affected the health and development of pre-school aged children. Poor families with pre-school aged children from randomly selected parishes were eligible to receive the Bono early in the roll-out. Eligible families with pre-school aged children from different randomly selected parishes were not offered the Bono until several years later. The families under study were interviewed prior to the introduction of the BDH, and again before the control parishes were included in the program.

The randomized introduction of an unconditional cash transfer program provides an opportunity to answer a basic but important question: how does money affect the health and

[1] The *Opportunidades* program was formerly called the *Progresa* program.

development of young children? A growing body of evidence indicates that, within developing countries, children from lower income families are more likely to experience worse health and to do less well on assessments of cognitive and behavioral development. However, there is much less evidence on whether improvements in income levels will result in healthier children with better developmental outcomes. The difficulties establishing causal effects are clear: while money may improve children's outcomes, it could be that families that are better equipped to earn higher incomes are also better able to produce and nurture healthier and more able children.

The existing literature provides hints that income transfers may improve children's health and developmental outcomes (Duflo, 2003; Case, 2001). For example, Duflo (2003) uses a quasi-experimental design to find that South African girls whose grandmothers become eligible for old-age pensions grow faster. Unfortunately, the data used in this study does not contain measures of other health and developmental outcomes. Recent evaluations of the Mexican conditional cash transfer program have collected data on a broader range of outcomes (see Gertler and Fernald, 2004). Using both matching and randomized-design methods, they find that cash, when paired with health center visits, has small but beneficial effects on children's health and development. However, this study, like other studies of conditional cash transfer programs, cannot distinguish between the effects of cash and the effects of the health center visits and other components of the programs. As is discussed below, better information on the relative contributions of cash versus other elements of conditional cash transfer programs may facilitate the design of more efficient programs.

This paper presents results on the effects of the BDH on the health and development of 2,217 rural children between the ages of 3 and 7. Children in this age range were given a common battery of tests aimed at measuring their nutritional status and their cognitive and motor abilities. Their mothers were asked to report on their behavior problems. Taken together, these data permit a broad assessment of how cash transfers influence health and development.

The following section of the paper provides a brief overview of the associations between economic status and children's health and developmental outcomes, with a focus on the possible mechanisms through which cash transfers might benefit children. Section III describes the Ecuador experiment and data collection. Section IV discusses the specific measures, data and methods we use in our analysis. Results are presented in Section V. We conclude with a discussion of the implications of our results for the design of transfer programs.

II. Economic Status, Cash Transfers and Child Development in Poor Countries

An enormous literature on health in developing countries documents the fact that children from more disadvantaged families—those with lower incomes and less parental education—display higher rates of mortality and morbidity. Within many countries, infants and children from less well-off families are more likely to die, to be stunted or wasted, and to experience a variety of illness conditions such as diarrhea, respiratory infections and measles (see, for example, Desai and Alva, 1998; Haddad et al. 2003, which provide evidence on a large number of countries.) There is a much smaller literature on economics gradients in children's developmental outcomes in poor countries. Results are often based on small samples that are not nationally representative. However, the existing research generally indicates that poverty is associated with developmental deficits across a variety of domains. For example, Gertler and Fernald (2004) provide evidence that, among low-income Mexican children, those that are poorer have smaller vocabularies than other children of the same age, and also score worse on tests of short-term and long-term memory and executive function. Halpern et al. (1996) document that, among Brazilian children, there are clear income gradients in language, social and motor development. Paxson and Schady (2006) show that age-adjusted vocabulary size in Ecuador is smaller among children from less-wealthy families, and the wealth gradient in vocabulary size for older children is larger than that for younger children. However, Fernald et al. (2006) find that, although the nutritional status and mental development of low-income Mexican children falls farther behind U.S. norms as children go from 13 to 24 months of age, these declines are not associated with socioeconomic status. These findings suggest that socioeconomic gradients do not appear until after infancy.

There are several mechanisms through which economic status could affect developmental outcomes. One is that families with lower incomes invest less in goods that promote children's development. Nutrition may be an important "investment good" that changes with income. Poorer children may be more likely to experience nutritional deficits—in calories, iron, and other micronutrients such as zinc and iodine—that adversely effect cognitive development, motor development, and social and behavioral outcomes. A large body of evidence indicates that nutrition and development are related, although distinguishing between the specific effects of different nutritional deficits is difficult due to their frequent co-occurrence (Grantham-McGregor and Baker-Henningham, 2005). Protein energy malnutrition is associated with impaired cognitive performance (Pollitt, 2000). Iron deficiency is associated with lower IQ, poorer memory, altered social and emotional behavior, and less developed motor skills (Grantham-McGregor and Ani, 2001). Animal studies have identified plausible biological mechanisms for these effects (Lozoff et al. 2006). The evidence on the role of other micronutrients is more mixed (Black, 2003; Ani and McGregor, 1999). There is consensus that iodine deficiency, especially during the prenatal period, is related to cognitive impairment, but that the evidence for the importance of zinc and vitamins is quite weak. Choline, a nutrient found in beef liver, chicken liver and eggs, has been shown to be important for brain development in rat pups (Zeisel, 2006) but there is not yet conclusive evidence of its importance in humans.

Poorer families may also invest less in their children's health care, or live in areas with lower-quality health care facilities. This could affect developmental outcomes in several ways. One is that, in poor countries, primary health care is typically aimed at monitoring children's growth and nutritional status and taking remedial actions if children are thought to be inadequately nourished. Interventions could include the use of iron supplements, de-worming treatments, or the provision of supplements to pregnant and lactating women. If health care helps ensure that children are adequately nourished, it could improve developmental outcomes through the mechanisms discussed above. Health care may also treat diseases such as diarrhea, pneumonia, malaria, and vaccinate against others, such as measles. Some of these conditions have been shown to impair development. For example, malaria is associated with cognitive impairments and loss of fine motor control (see cites in Sachs and Malaney, 2002). Finally, health care services may provide mothers with health information that helps them to protect children's nutritional status and prevent illnesses.

Nutrition and health care are not the only routes through which economic status might affect developmental outcomes. In developed countries, research has focused on how the quality of parenting, the home environment, and child care (if relevant) influence early child development. A recent Institute of Medicine report on child development stresses children's needs for close and dependable relationships, and "cognitively and linguistically rich environments" (Shonkoff and Philips, 2000, p. 9) There is no reason to think that these factors are not equally important in poor countries. In one randomized-design study of malnourished Jamaican children, both nutritional supplementation and psychosocial stimulation improved children's developmental outcomes in the short run, and the psychosocial intervention had long-run benefits whereas the nutrition intervention did not (Grantham-McGregor et al. 1991; Grantham-McGregor et al. 1997). There are two routes through which increases in incomes could improve the quality of children's home environments. First, parents might spend more on materials or activities that stimulate children, or enroll them in early educational activities. Second, higher incomes could reduce stress or depression among parents, leading to more nurturing behaviors.

Although there numerous reasons to think that increases in incomes may improve children's health and developmental outcomes, there are also reasons why this may not be the case. For example, the worse health and developmental outcomes of poorer children could be due to parents' lack of information about what could be done to promote health and development. Even in this case, cash transfers could improve children's outcomes if (for instance) they permit families to move to neighborhoods with better-quality services or more well-informed neighbors. However, the operating assumption of the *Oportunidades* program—which requires mothers of young children to attend classes to improve their care of children—seems to be that parental education is complementary to cash transfers. It could also be that children of less healthy and able parents (who are, as a consequence, less wealthy) are themselves less healthy and able. In this case, the association between income and children's developmental outcomes does not represent a causal relationship, and cash transfers will not improve children's outcomes. The randomized intervention we conducted makes it possible to examine whether this is the case.

An issue that is of particular importance to this study is whether there are "critical periods" in children's development. The effects of nutritional deficits or illness on developmental outcomes are likely to depend on the age of the child at which they are experienced. There is broad consensus that adversity experienced earlier in children's lives—from the prenatal period through infancy—are particularly damaging to children. There is less agreement on how the effects of poor health or nutritional deficits experienced later in early childhood compare to those experienced at earlier ages, or the extent to which developmental "catch up" once children are past infancy is possible. The children we study were all at least 18 months old when the experiment began, and the majority (81 percent) were age two or older. If catch up is not possible, then the program should have no effect on the developmental outcomes of these children, although it may well have beneficial effects for their younger siblings.

III. Experimental Design and Data Collection

The BDH

In 2003, the Ecuadorean government began restructuring its social assistance programs in an effort to improve both child health and education. Between 1998 and 2003, the largest social assistance program in the country was the *Bono Solidario*, which provided unconditional cash transfers of U.S. $15 per month to participating families with children. (Ecuador adopted the U.S. dollar as national currency in January 2000.) This program, which accounted for approximately 0.75 percent of GDP in 2002, has since been phased out. One source of dissatisfaction with the *Bono Solidario* was that it was never tightly means-tested. Although the incidence of the *Bono Solidario* was progressive, there was substantial "leakage" to non-poor families and undercoverage of poor families. In 1999, 49.8 percent of families in the poorest quintile received transfers, and 27.4 percent of families in the top two wealthiest quintiles received transfers. (These statistics are based on our calculations from household survey data.) The "leakage" was primarily due to the fact that, at the program's inception, enrollment was done on a voluntary basis: all women with children were free to enroll. Undercoverage of the poor results from the fact that registration was done on a first-come first-served basis. Many poor families, and especially newly formed families, were unable to register.

Beginning in mid-2003, the *Bono Solidario* was gradually replaced with a new program, the *Bono de Desarrollo Humano* (BDH). The BDH differs from the *Bono Solidario* in that it is means-tested. Starting in 2001, the government of Ecuador invested significant effort into developing a family means-test. Fully 85 percent of families in rural areas and poorer urban areas of Ecuador were surveyed and assigned a poverty index (called the SelBen index) that is used to assess eligibility for the BDH. Only families in the first two quintiles of the SelBen index are eligible for BDH. Like the Mexican *Oportunidades* program, the cash transfer of $15 per month was meant to be conditional on taking children younger than age six for bi-monthly visits to public health clinics, and sending school-aged children to school. However, for a variety of logistical reasons, the conditionality was never implemented. Transfers were distributed through the banking system, and were given directly to mothers rather than fathers.

The experiment

The BDH was rolled out slowly across the country, providing us with an opportunity to randomize parishes into a treatment group and a control group. At the time this project was started, the BDH was already being implemented in several provinces. We selected six provinces—three coastal provinces and three provinces in the highlands—in which to conduct the study. Together, these provinces contained 378 parishes (where the parish is the smallest administrative unit in Ecuador, roughly equal to villages in rural areas.) These parishes were stratified into urban and rural groups. A total of 118 parishes were selected: 51 rural and 28 urban treatment parishes, and 26 rural and 13 urban control parishes.[2] The number of treatment parishes was set to roughly twice the number of control parishes so that, if conditionality was implemented, the treatment parishes could be divided into a group that received conditional cash transfers and a group that received unconditional cash transfers.

The BDH is structured so that all families with children in the first two quintiles of the SelBen index are eligible to receive transfers once the BDH is implemented in their parishes. We refer to these families as "BDH-eligible." Because the purpose of this study was to examine the effects of the program on the health of young children, we studied only a subset of BDH-eligible families. Specifically, to be eligible for inclusion in our sample, families had to be in the first two quintiles of the SelBen index, have at least one child under the age of six, have no children ages 6 or above, and to have not been recipients of the *Bono Solidario*. We refer to these families as "sample eligible" families. We excluded BDH-eligible families with older children because, in the event that the program became conditional, the conditionality would work differently for families with school-aged children than for families with (only) younger children. This exclusion turned out, *ex post*, to be unnecessary. We excluded families who were recipients of the *Bono Solidario* because these families were not newly-eligible for transfers: instead, they were simply being converted from the old program to the new program.

The sample selection criteria that were used mean that the families in our sample are not representative of all BDH-eligible families in the six provinces chosen. A particular concern is that the families that managed to gain access to the *Bono Solidario* were systematically different from those who were newly eligible: they may have been better able to "work the system" to gain entrance to the *Bono*, or conversely may have been more needy and given higher priority.

[2] The numbers of parishes selected was chosen to yield approximately 1200 treatment and 600 control families in rural and urban areas.

We have information from government records for all BDH-eligible families in the parishes we sampled, including their SelBen scores, and so can compare the scores of sample-eligible families with those who were excluded due to former *Bono* receipt, or because of the presence of older children.

Results of these tabulations are shown in Table 1. The table contains information on BDH-eligible families in our sampled parishes: all are poor, and all have at least one child. These families are divided into those who already received the Bono and those who were newly-eligible; and also into those who had only children under the age of 6 and those who had at least one child age 6 and older. Table 1 indicates that 17,987 families—shown in the bottom right quadrant of the table—were "sample-eligible." The families in our survey were selected from this group.

The results in Table 1 indicate that, as expected, "young" families—those with only younger children—are more likely than others to be newly-eligible: 80.2 percent of these families are newly-eligible, in contrast to 26.7 percent of families with older children. The SelBen scores—which range from 11 to 51, with higher values corresponding to greater wealth—indicate that younger families are on average wealthier than older families.[3] This is true for both newly eligible families and those who were former recipients of the *Bono*. More important, however, is the finding that newly-eligible families are only slightly less wealthy (as measured by the SelBen index) than those who are former *Bono* recipients. This difference is wealth is to be expected, given that the *Bono* recipients have been receiving transfers while the newly eligible families have not. Large wealth differences would raise concerns that the newly-eligible families were selectively different from the rest of the population.

[3] We suspect this is because household size was a factor in assigning SelBen scores.

Up to 50 eligible families were selected from each parish (some parishes had fewer than 50 sample-eligible families), resulting in a sample of 3,426 families containing 5,547 children. A baseline survey that collected information on household characteristics and health status was administered between October 2003 and September 2004. Rural families in the treatment parishes became eligible to receive transfers in June 2004, and urban treatment families became eligible to receive transfers in November 2004. A follow-up survey, which collected more detailed information on the health of mother and children and children's developmental outcomes, was conducted between September 2005 and January 2006, with a response rate of 94.1 percent. On average, rural families in the treatment group (who we study in this paper) were eligible for the transfer for 17 months prior to the follow-up survey.

Figure 1 shows information from banking records on the receipt of transfers for all rural families in our sample, through July of 2005. The top panel shows the fraction of families that received the transfer in the month indicated; the bottom shows the average transfer over all families. The figure indicates that take-up of the transfer among families in the control group was nearly non-existent. From June 2004 on, 1.9 percent of families in the control group are reported to have received transfers in any month, indicating very little contamination of the treatment to control areas. In both the treatment and control groups, approximately 1 percent of families were reported to have received transfers prior to June, indicating that some families that were not "newly eligible" were mistakenly included in the sample, presumably because of errors in the government's records of who was and was not receiving the *Bono Solidario*.

The fraction of treatment families who received transfers climbed relatively quickly once the program became available, reaching more than 60 percent by January 2005. Actual program take-up was higher. Eligible families were not required to withdraw their $15 on a monthly basis, but could allow transfers to accumulate for up to 4 months, and the 60 percent figure measures the fraction who withdrew the money in any given month. Overall, approximately 76 percent of families in the treatment parishes enrolled in the BDH, receiving (overall) an average transfer of $12 per month.

IV Data and Methods

Data

The results presented in this paper are based on a sample of 2,217 rural children living in 1,552 families, and were 3 to 7 years old at the follow-up survey.[4] We use this sample to examine the effects of the BDH on eight health and developmental outcomes, including a set of measures of physical health (hemoglobin, height and fine motor control) and cognitive and behavioral measures (short and long term memory, vocabulary, executive function and behavior problems.) These outcomes will be discussed in more detail below.

Table 2 shows characteristics of these families and children from the baseline survey. Descriptive statistics are shown for the treatment and control groups, for two samples we use in our analyses. The first, referred to as the "full sample", includes information for all 2,217 children aged 3 to 7 at follow-up, regardless of whether some of the outcomes we study have missing values. The second, referred to as the "restricted sample", includes information only for the 1,450 children (and their families) for whom all eight measures are non-missing. In a handful of cases (7), missing values resulted from the child being unavailable for testing. The majority of missing values were due to the mother being unwilling to allow the finger prick blood draw required for the hemoglobin measurement (445 cases). Other missing values were

[4] We excluded 46 children whose mothers did not speak Spanish. The language-based developmental tests were not available in indigenous languages.

due to a variety of causes, such as an invalid height measurement or the failure to take a cognitive test.

The baseline data indicate that the children in our sample are disadvantaged. The first row of the table shows means of the imputed logarithm of monthly expenditure. The baseline survey did not include an expenditure module, but did collect information on housing characteristics and household durables. A companion study of the effects of the BDH on the educational attainment of older children (conducted in different parishes) collected the same information on housing and durables, and included an expenditure module. We used data from this study to estimate a regression of the logarithm of monthly expenditure on measures of housing characteristics, durable goods ownership and several household characteristics such as the household head's age and education level, and household size, and used the resulting coefficients to impute the logarithm of expenditure for our sample. The values of imputed expenditure are consistent with what is expected for this sample of low-income families. The average value of the level of expenditure for the sample is $156 or $1.17 per capita per day.

Other notable features of the sample are that the children in our sample have, on average, relatively young mothers (around 23 years old) with around 7 years of completed schooling. A large fraction of mothers completed exactly 6 years of schooling, indicating that they did not progress beyond primary school. However, nearly all mothers have attended some school, and only 2 percent report themselves as being unable to read or write. Slightly more than 30 percent of mothers are not living with a husband or partner. This is not the result of migration of male partners: of the 631 children with fathers who did not live in their homes, only 91 had fathers who had migrated elsewhere. In the majority of cases, the child's parents had either separated or never lived together. The results in Table 1 also indicate that many of these children had significant health problems at baseline. The average height-for-age z-score, computed using US norms, was around –1.3 for the full sample.[5] The average level of hemoglobin was 10.4, which is low given that values below 11.0 to 11.5 (depending on the child's age) indicate anemia.

A comparison of means for the treatment and controls groups, using either the full or restricted samples, shows almost no statistically significant differences. In the full sample, slightly more children in the control group were male. Although we suspect this is the product of chance, we control for the child's gender in all specifications. A comparison of the means for the two samples—full and restricted—also shows few differences. The mothers in the restricted sample have slightly higher levels of average education, and the children are slightly taller for their ages. The children are also about a month older on average, which could reflect the fact that mothers were less likely to allow younger children to receive the hemoglobin test. However, other measures—including imputed expenditure, family size, and maternal age—are very similar for the two samples.

Descriptive statistics for the eight child outcomes for the two samples are shown in Table 3. The first, elevation-adjusted hemoglobin, has a mean that is larger than at baseline. Still, the fraction of children who are anemic is 46.1 percent.[6] The average height-for-age z-score indicates that children in Ecuador are, on average substantially shorter than U.S. children of the same age and gender. Fully 23 percent of the children are stunted (i.e. have a z-score less than –2). The final measure of physical development is for fine motor control. Fine motor control was assessed using a pegboard exercise. Children were asked to put pegs into a pegboard, twice using

---

[5] Height-for-age z-scores were computed using growth charts produced by the Centers for Disease Control. The CDC provides programs to compute z-scores: http://www.cdc.gov/nccdphp/dnpa/growthcharts/sas.htm

[6] Using CDC guidelines, the cut-offs for anemia are 11.1 g/dl for children between the ages of 2 and 5, and 11.5 for children between 5 and 8. Hemoglobin is elevation adjusted using CDC guidelines. Generally, hemoglobin levels should be higher at higher altitudes (Centers for Disease Control, 1989),

their dominant hand and twice using their non-dominant hand. These four times were averaged together. The final score in measured in seconds, so that lower values indicate faster times.

We use five measures of cognitive and behavioral outcomes. The first is the *Test de Vocabulario en Imágenes Peabody* (TVIP), the Spanish version of the *Peabody Picture Vocabulary Test* (PPVT), a widely-used test of receptive vocabulary.[7] Children taking this test are shown a series of slides, each of which contains four pictures. For each page, the interviewer says a word (i.e. “dog”, “smile”, etc.) and the child is instructed to point to the picture that goes with the word. The raw scores are converted to standardized scores using age-specific norms that are published by the test developer. The standardized scores are constructed to have a mean (among the sample used for norming) of 100, and a standard deviation of 15. We used three tests of cognitive ability from the Woodcock-Munoz cognitive battery. The first is a test of memory for names (which we denote WJ-1, since it is the first test in the battery). Children are gradually “introduced” to a series of space creatures with nonsensical names, and then are shown groups of space creatures which they are asked to identify. This test taps long-term retrieval, since children must recall the names of creatures they were introduced to early in the test. The second (denoted WJ-2) is memory for sentences. The interviewer reads the child increasingly complex sentences, which the child must repeat back. This test measures immediate recall. The final cognitive test (WJ-5) measures visual integration. Children are shown a series of pictures of common objects that have been distorted in various ways—for example, the child might be shown a picture of a boat with several of the lines missing, or with a pattern superimposed on top of the picture—and are asked to identify the object. This test is meant to measure visual-spatial

[7] See, for example, Desai, Chase-Lansdale, and Michael (1989); Baydar and Brooks-Gunn (1991); Blau and Grossberg (1992); Parcel and Menaghan (1994); Rosenzweig and Wolpin (1994); Blau (1999); and McCulloch and Joshi (2002).

processing. Children's scores on the three Woodcock-Johnson tests are age-normed by converting them to percentiles provided by the test developer. Finally, the behavior problems scale is a commonly-used measure of mother-reported behavior problems. Mothers were read a list of twenty-nine problems and asked to indicate whether the child experiences the problem never or rarely (which earns a score of 0), occasionally (which earns a score of 1), or frequently (which earns a score of 2). The final score is simply the sum of the scores on individual items. Although this test is measure of behavior problems is widely used, we know of no norming procedure and simply use the raw score.

The means in Table 3 indicate that, on average, the children in our sample perform poorly on the tests of language and cognitive ability. The average for the TVIP is 82.3, more than one full standard deviation below the average for the norming sample. The average percentiles on the two tests of memory are 16.3 and 28.8. The test of visual integration was the most difficult for the children in our sample, with a mean percentile of 6.7. We do not have the information required to assess whether the children in our sample have more or fewer behavior problems than less disadvantaged children.

It is unlikely that receipt of the BDH can improve children's outcomes if these outcomes are not themselves related to living standards. To see if this is so, we estimated non-parametric regressions of each outcome on the imputed logarithm of expenditure. We first converted each of the outcomes into a z-score by subtracting the sample mean and dividing by the standard deviation. This within-sample standardization makes it easier to compare results across outcomes. The results, graphed in Figure 2, show dashed lines at the 25^{th} and 75^{th} percentiles of the expenditure measure. Most of the measures have a non-linear relationship with the logarithm of expenditure. Hemoglobin, fine motor control, and all of the language and cognitive test scores improve with expenditure at low expenditure levels. (Recall that, for fine motor control, higher values correspond to worse outcomes.) There are striking increases in the TVIP and the three Woodcock-Johnson tests with expenditure among children in the bottom half of the expenditure distribution. At higher expenditure levels, outcomes either do not improve or sometimes even appear to deteriorate as expenditure rises. Height and behavior problems are exceptions to this pattern. Height is not related to expenditure until expenditure exceeds 5 (the sample mean). Behavior problems decline slightly as expenditure rises.

Some of our analyses use information on four measures of mother's physical and mental health. The first is a measure of the mother's hemoglobin level, which is adjusted for both elevation and pregnancy. The second is the Center for Epidemiological Studies Depression scale (CESD), a widely-used measure of depression. The third is a measure of maternal punitiveness and lack of warmth. This consists of 8 interviewer-assessed items, and is based on the HOME scale (see Bradley 1993; Paxson and Schady 2006.) The last measure is the mother's score on the 4-item version of the Perceived Stress Scale (PSS). This is a widely-used measure of the extent to which life events are perceived to be stressful (Cohen et al. 1983). For all three measures of mental health (the CESD, HOME and PSS), lower scores are better. Two final measures we use are based on the mother's report of her subjective social status, using the MacArthur ladders. Mothers were shown a picture of a ladder with 10 rungs, and were told that higher rungs correspond to higher socioeconomic status. They were asked to place themselves on the ladder in relation to everyone in their communities, and in relation to everyone in Ecuador. We use the ladder scores as crude measures of economic status. The "community" and "Ecuador" ladders provide information on whether the subjective standing of those in the treatment group increases relative to those in the control group.

Figure 3 shows results of nonparametric regressions of the maternal physical and mental health measures on the logarithm of household expenditure. As before, all scores have been converted to within-sample z-scores. The results for maternal hemoglobin are similar to those for children, in that increases in expenditure among the poorest family are associated with increases in hemoglobin. The three measures of mental health all decline (that is, improve) with expenditure: poorer mothers are more likely to be depressed and stressed, and more likely to be rated by interviewers as being harsh and unresponsive to their children. The relationships between raw ladder scores and expenditure are shown in Figure 4. As might be expected, ladder rankings increase with expenditure, and mothers rank themselves higher on the community ladders than on the Ecuador ladders. The predicted value on the community ladder does not exceed 4 even for the wealthiest mothers in out sample, and the predicted ranking on the Ecuador ladder does not exceed 2.5. These low rankings are consistent with the fact that only those in the bottom two quintiles of SelBen index are eligible for the Bono.

Methods

We present intent-to-treat estimates, using regressions of the following form:

(1) $Y_k = T\alpha_k + X\beta_k + \varepsilon_k$, k=1…K,

where Y_k is the kth child outcome (out of 8), T is a treatment indicator, and X is a set of controls (including an intercept). Because the outcomes have been converted to z-scores, the coefficients on the treatment indicator show effect sizes in standard deviation units. (To make it easy to compare results across outcomes, we reverse the signs on variables if necessary so that higher values always correspond to "better" outcomes.) In most specifications, X includes only indicators for the child's age, in 3-month bands, and an indicator for the child's gender. As a robustness check, we also show results that add controls for a set of baseline family characteristics, including the log of imputed expenditure, an indicator for whether the mother lived with a husband at baseline, the mother's years of education and age, and indicators for the numbers of family members in 5 age ranges (0 to 5, 6 to 14, 15 to 44, 45 to 64 and 64 or older) crossed with gender.

We also present the average treatment effect over all 8 child outcomes:

$$(2) \qquad \overline{\alpha} = \frac{1}{K}\sum_{k=1}^{K}\hat{\alpha}_k \ .$$

It may be that the average effect is more precisely estimated than the effects for individual outcomes. The standard errors for all estimates (including $\overline{\alpha}$) are clustered at the parish level.

The non-parametric estimates shown in Figure 2 indicate that the relationship between the outcomes and expenditure are nonlinear. We therefore expect that the effects of the transfer program to also differ across poorer and wealthier families in our sample. To test this hypothesis, we estimate variants of (1) that permit the effects of the transfer to differ across three groups of families: those whose baseline expenditure was in the lowest quartile of the expenditure distribution, the second quartile, and the top two quartiles.[8] Specifically, we estimated:

$$(3) \qquad Y_k = \alpha_k^1 Q_1 T + \alpha_k^2 Q_2 T + \alpha_k^3 Q_3 T + \lambda_k^1 Q_1 + \lambda_k^2 Q_2 + X\beta_k + \varepsilon_k ,$$

where Q_j indicates which group the family belongs to.

V. Results

Take-up rates and transfer amounts

We first present evidence on the take-up rates and transfer levels of the families in our sample. Table 4 shows the results of regressions of the form of (1) and (3), using a variety of measures of

[8] We first separated the top two quartiles into two distinct groups, but found that combining these groups had little effect on the results.

transfers as the dependent variable. The first four columns, based on bank records from June 2004 to July 2005, indicate that treatment children lived in families that received on average of $136.87 more in transfers than did the control group (whose transfers were negligible), and were 76.5 percent more likely to participate in the BDH. The participation rate based on maternal report is slightly higher (80.1 percent, column 5), which makes sense given that the banking records do not cover the complete treatment period. The results also indicate that the families in the bottom two quartiles were slightly more likely than others to take up the BDH, and had slightly higher average transfers. The final four columns indicate that families in the restricted sample look much like those in the full sample: treatment does not predict membership in the restricted sample, and take-up patterns are similar across the two samples. Table 5 shows the average of transfers accumulated over the one-year period from August 2004 to July 2005, as a fraction of imputed annual baseline expenditure. The median transfer is 9.2 percent of expenditure: 12.6 percent for the poorest families and 7.7 percent for the wealthiest. Among those who take up the program, transfers represent more than 10 percent of expenditure.

Children's outcomes

The main results for children's outcomes are in Table 6 (which presents estimates of equation 1) and Table 7 (which presents estimates of equation 3). The results in Table 6, for which the treatment effects do not vary by expenditure group, show at most modest treatment effects. In the specification that controls only for age and gender, the only significant effect is for fine motor control, which is predicted to be 15.5 percent of a standard deviation higher among the treatment group than in the control group. However, all the effects are positive. (Recall that all outcomes are coded that higher values correspond to better outcomes.) The average effects size for the physical outcomes measures (hemoglobin, height, and final motor control) is 12.9 percent of a standard deviation, with a standard error of 5.5 percent. However, average effects sizes for the cognitive and behavioral measures, and for all outcomes combined, are not statistically different from zero. Including the wider set of controls (column 2) does not alter this conclusion.

The results in Table 7 support the idea that treatment effects differ across poorer and wealthier families. The strongest results are for hemoglobin. The treatment effect for elevation-adjusted hemoglobin is 0.504 for those in the bottom quartile, 0.179 for those in the second quartile, and 0.031 for those in the top two quartiles. Note that the treatment effects for those in the bottom two quartiles are roughly equal to (in opposite sign) of the main effects of being in the bottom two quartiles. This implies that there is no expenditure gradient in hemoglobin among children in the treatment group, whereas hemoglobin levels increase with expenditure in the control group. The results for fine motor control display a similar pattern. Estimates for the other individual outcomes are less clear-cut. For two outcomes (height-for-age and memory for names) the effects sizes for the poorest and wealthiest children are similar. For other outcomes, poorest children have the largest effect sizes. In some cases, the 2nd poorest group shows effect sizes that are smaller than those on either side. However, the average effects sizes indicate that, for all subgroups of outcomes and for all outcomes together, the treatment effects are largest for the poorest children. Averages for the physical outcomes and "all" outcomes are large (0.314 and 0.220, respectively) and statistically significant. The average for the cognitive and behavioral outcomes is somewhat smaller and only marginally significant (0.164 with a standard error of 0.092).

Two variants of these results are shown in Table 8. The left-hand panel shows results that include the full set of controls for baseline family characteristics. The right-hand panel shows results with the smaller set of controls, but estimated using the full sample (so that the sample size varies across outcomes.) These results are very similar to those discussed above. The larger sample yields somewhat more precisely-estimated effects than does the restricted sample.

In summary, the results for children provide evidence that eligibility for the cash transfers improved the hemoglobin levels and fine motor control of the poorest children in the treatment group relative to those in the control group. Although the standard errors for the treatment effects for individual cognitive outcomes are large, we find some support for the hypothesis that the treated children are showing cognitive improvements relative to the controls. This general pattern of results may not be surprising. Hemoglobin levels can change relatively quickly in response to changes in diets or reductions in helminth infections, and it may be that improvements in hemoglobin translate directly into better motor control. The cognitive outcomes—especially the TVIP, which reflects both memory as well as exposure to words—might be expected to change more slowly. Furthermore if, as some evidence suggests, cognitive catch-up is difficult to achieve, we might expect that, for children in the age range we examine, transfers will result in larger improvements in physical relative to cognitive outcomes. Still, the findings are supportive enough of improvements in cognitive outcomes to be encouraging.

Mechanisms

As noted above, there are several mechanisms through which cash transfers may influence children's health and development. These include improvements in nutrition; improvements in health care; and improvements in parenting. Although we cannot formally test the mechanisms through which the effects operate—doing so would require further randomized interventions—we can examine whether there is evidence that nutrition, health care and parenting in fact improved.

We first examine several maternal outcomes that reflect nutritional status and wealth, as well as maternal mental health and parenting. The first column of Table 9 shows the treatment effect estimated over all mothers (as in equation 1); the next five columns show results when the effects can vary across expenditure groups (as in equation 3). The first row indicates that mothers in the treatment group experience improvements in their hemoglobin levels, and that the gains are largest for the poorest families. These results for mothers are remarkably similar to those for children, suggesting that improvements in the diets of all family members may have improved. The next two rows indicate that, as expected, the treated mothers perceive themselves to be better-off than those in the control group: they place themselves higher on the "Ecuador" and "community" ladders. We take this as an indication that transfers were spent in a way that made mother's better off.

Although the poorest treated mothers appear to have improvements in their physical health, the same is not true for mental health. We find no treatment effects for depression or stress. The HOME score, which assesses parental responsiveness and harshness, also does not differ between mothers in the treatment and control groups. The lack of improvements in mental health may be surprising, given that mother in the treatment group view themselves as being better off according to the ladders. The finding suggests that mental health and economic status need not go hand-in-hand. These results indicate that it is unlikely that any improvements in children's outcomes are the result of more responsive parenting behaviors.

We also examined whether treated children receive more health care than untreated children. The results are in the lower panel of Table 9. We looked first at whether the child had a "growth control" check-up in the past 6 months. Growth control visits are for preventive care: during the visit, children's growth is monitored, supplements and de-worming treatments are prescribed if necessary, and vaccines are administered. Although visits to public clinics are free, it could be that cash transfers defray transportation costs or make it possible to attend higher-quality private clinics. Furthermore, it is possible that parents mistakenly believed that the conditionality that was supposed to have implemented with the BDH—which would have required regular health center visits—was in fact in effect. However, we find no evidence that this was the case: treatment group children were no more likely to have growth control visits than control group children.

We also examined whether the child had a parasite treatment in the past 12 months. Intestinal parasites are widespread among children in Ecuador, and regular treatments are necessary since re-infection is common. Our results show large and significant treatment effects in parasite treatments, especially among the poorest children. Note that the treatment effects are large enough for the poorest children to completely offset the main effects of being poor. Among those in the control group, the poorest children are predicted to be 12.8 percent less likely than the wealthiest children to receive parasite treatments. Among those in the treatment group, the poorest children and wealthiest children are equally likely to receive parasite treatments. Helminth infections are associated with anemia, so that these results are consistent with our findings for the treatment effects for hemoglobin.

At first blush, the results for growth control visits and parasite treatments appear to at odds: we find no increase in growth control visits among those in the treatment group, but do see increases in parasite treatments. However, health clinics are not the most common source of parasite treatments. When mothers of children who received parasite treatments were asked where the medications were obtained, 40.5 percent said they “bought them,” in comparison to 32.2 percent who received them from health centers, 10.2 percent who said they got them from non-clinic-based public programs, and 10.1 who got them from schools (with the remaining 7 percent replying “other” or “don’t know”.) Therefore, it is possible that the cash transfers were used to purchase treatments for intestinal parasites in the market.

Overall, the evidence suggests that eligibility for the BDH resulted in improved nutrition and greater use of medicines, both of which would have improved hemoglobin levels. The solid evidence for improvements in fine motor control, and the somewhat weaker evidence for improvements in children’s cognitive outcomes, is consistent with previous literature that indicates that low hemoglobin levels impair children’s motor and cognitive development.

A final outcome we examined was school enrollment. The results, shown in the bottom row of Table 9, indicate that among the poorest children in the sample, those in the treatment group are 12.4 percent more likely to be enrolled in preschool or grade school than those in the control group. The results are consistent with those of Miguel and Kremer (2004), who found that deworming treatments increased school attendance among Kenyan children. However, we cannot determine whether increased enrollment is the result of improved financial circumstances, better child health, or improved “school readiness”. The improvements in cognitive test scores we see could be a cause or a consequence of increased school enrollment. In any case, our findings suggest that the BDH may have longer-run effects on the schooling outcomes of these children.

VI. Conclusions

There is accumulating evidence that conditional cash transfer programs have beneficial effects for the health and development of pre-school children. The results in this study indicate that conditionality may not be required to obtain positive outcomes. We find that the relatively modest unconditional cash transfer program put into place in Ecuador raised the hemoglobin levels of the poorest children, improved fine motor control and, although the results are more speculative, may have improved cognitive outcomes. Our evidence suggests that these gains were accomplished though better nutrition and the use of medicines to treat intestinal parasites, but not through the use of more preventive health care services or more responsive parenting.

These results are relevant to the design and implementation of cash transfer programs. Conditional cash transfer programs have caught the attention of policy-makers in numerous countries, for good reason. Conditionality may serve to screen less needy families out of the program, reducing budgetary costs. The requirement that children are taken to health clinics makes sense if parents lack knowledge about the value of health care, or if mothers do not have the leverage within families to make sure that children receive appropriate medical care. Furthermore, the imposition of conditionality may increase the political demand for increases in the numbers and improvements in the quality of public health clinics. The abysmal quality of clinics in many poor countries is becoming increasingly well-documented (for example, Banerjee and Duflo, 2006; Banerjee, Duflo and Deaton, 2004; Das and Hammer 2005). Governments that require families to use clinics may be forced to confront problems of absenteeism and the lack of supplies and equipment.

However, conditionality also imposes costs. Requiring families to use health clinics may not be feasible in places where health centers are few and far between, or are of low quality. The families that self-select out of the program, because of the high costs of getting to clinics, may be those whose children are most at risk for poor outcomes. Unconditional transfers will improve the welfare of poor families regardless of how the money is spent and, as the results of this paper indicate, may improve child health.

More research is required in several areas. First, we do not know whether adding conditionality to the BDH would have improved or weakened the beneficial effects—either result is possible in theory. If conditionality is imposed in the future, we plan to extend this study to examine its effects. Second, we do not know whether the BDH will have larger effects on the development of the younger children in our sample, including children who were *in utero* during the treatment. Existing evidence suggests that health and nutrition very early in life have larger effects on children's outcomes than health and nutrition at later ages. We hope to examine this in the future, when these children are old enough to take the cognitive tests examined in this paper.

References

Banerjee, A. and E. Duflo. 2006. "Addressing Absence," *Journal of Economic Perspectives* 20(1): 117-132.

Banerjee, A, E. Duflo and A. Deaton, 2004. "Wealth, Health and Health Services in Rural Rajasthan," *American Economic Review* 94(2): 944-949.

Black, M. M. 2003. "Micronutrient Deficiencies and Cognitive Functioning." *Journal of Nutrition* 133(11 Suppl. 2): 3927S-3931S

Bradley, R.H. 1993. "Children's Home Environments, Health, Behavior, and Intervention Efforts: A Review Using the HOME Inventory as a Marker Measure." *Genetic, Social, and General Psychology Monographs,* 119, 437-490.

Case, A. 2001. "Does Money Protect Health Status? Evidence from South African Pensions." NBER Working Paper No. 8495. Cambridge, MA.

Center for Disease Control. 1989. "Criteria for Anemia for Children and Child-Bearing-Aged Women." *MMWR Morbidity and Mortality Weekly Report* 38: 400-404.

Cohen, S., Kamarck, T., & Mermelstein, R. (1983). A Global Measure of Perceived Stress. *Journal of Health and Social Behavior* 24:385-396.

Das, J. and J. Hammer. 2005. "Money for Nothing: The Dire Straits of Medical Practice in Delhi, India." *World Bank Policy Research Paper 3669,* Washington: World Bank.

Desai S. and S. Alva, 1998. "Maternal Education and Child Health: Is There a Strong Causal Relationship?" *Demography* 35(1): 71-81.

Duflo, E. 2003. "Grandmothers and Granddaughters: Old-Age Pensions and Intrahousehold Allocation in South Africa." *World Bank Economic Review* 17(1): 1-25.

Fernald, L. et al. 2006. "Parallel Deficits in Linear Growth and Mental Development in Low-Income Mexican Infants in the Second Year of Life." *Public Health Nutrition* 9(2): 178-86.

Gertler, P. and L. Fernald. 2004. "The Medium Term Impact of *Oportunidades* on Child Development in Rural Areas." Manuscript, University of California at Berkeley.

Grantham-Mc Gregor, S.M. and C. Ani. 2001. "A Review of Studies on the Effect of Iron Deficiency on Cognitive Development in Children." *Journal of Nutrition* 131: 649-668.

Grantham-McGregor, S.M. and H. Baker-Henningham. 2005. "Review of the Evidence Linking Protein and Energy to Mental Development," *Public Health Nutrition* 8(Special Issue 1): 1191-1201.

Grantham-McGregor, S.M., C.A. Powell, S.P. Walker and J.H. Himes, 1991. "Nutritional Supplementation, Psychosocial Stimulation, and Mental Development of Stunted Children: The Jamaican Study." *Lancet* 338: 1-5.

Grantham-McGregor, S.M., S.P. Walker, S.M. Chang and C.A, Powell. 1997. "Effects of Early Childhood Supplementation With and Without Stimulation on Later Development in Stunted Jamaican Children," *American Journal of Clinical Nutrition* 66: 247-53.

Haddad L., H. Alderman, S. Appleton, L. Song and Y. Yohannes. 2003. "Reducing Child Malnutrition: How Far Does Income Growth Take Us?" *World Bank Economic Review* 17(1): 107-131.

Halpern, R. et al. 1996. "Desnvolvimento neuropsicomotor aos 12 meses de idade em uma coorte de base populacional no Sul do Brasil: Diferenciais conforme peso ao nascer e renda familiar," *Cad. Saúde Púb* 12(suppl, 1): 73-78.

Lozoff, B. et al. 2006. "Long-Lasting Neural and Behavioral Effects of Iron Deficiency in Infancy," *Nutrition Review* 64(5 Pt. 2): S34-S91.

Miguel, E. and M. Kremer. 2004. "Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities," Econometrica 71(1): 159-217.

Paxson, C. and N. Schady, 2006, "Cognitive Development Among Young Children in Ecuador: The Role of Health, Wealth and Parenting," *Journal of Human Resources*.

Pollitt, E. 2000. "Development Sequel from Early Nutritional Deficiencies: Conclusive and Probability Judgements," Journal of Nutrition 130(2): 350S-353S.

Sachs J. and P. Malaney. 2002. "The Economic and Social Burden of Malaria," *Nature* 415:680-685.

Shonkoff, J.P. and D. Phillips, eds. 2000. *From Neurons to Neighborhoods: The Science of Early Childhood Development*, Washington D.C.: National Academy Press.

Zeisel, S.H. 2006. "Choline: Critical Role During Fetal Development and Dietary Requirements in Adults," *Annual Review of Nutrition* 26: 229-250.

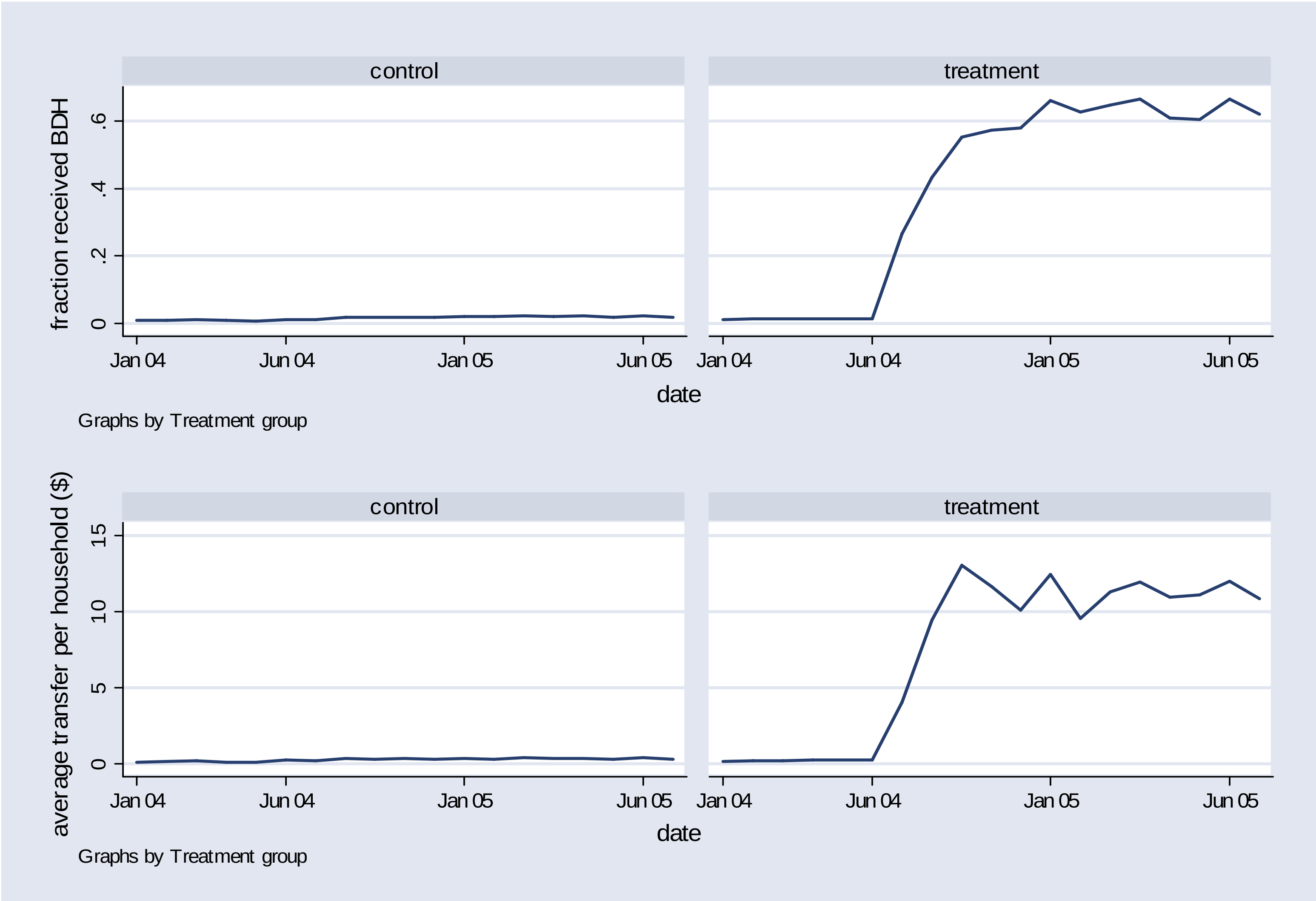

Figure 1: Transfers to rural families during roll-out (bank records)

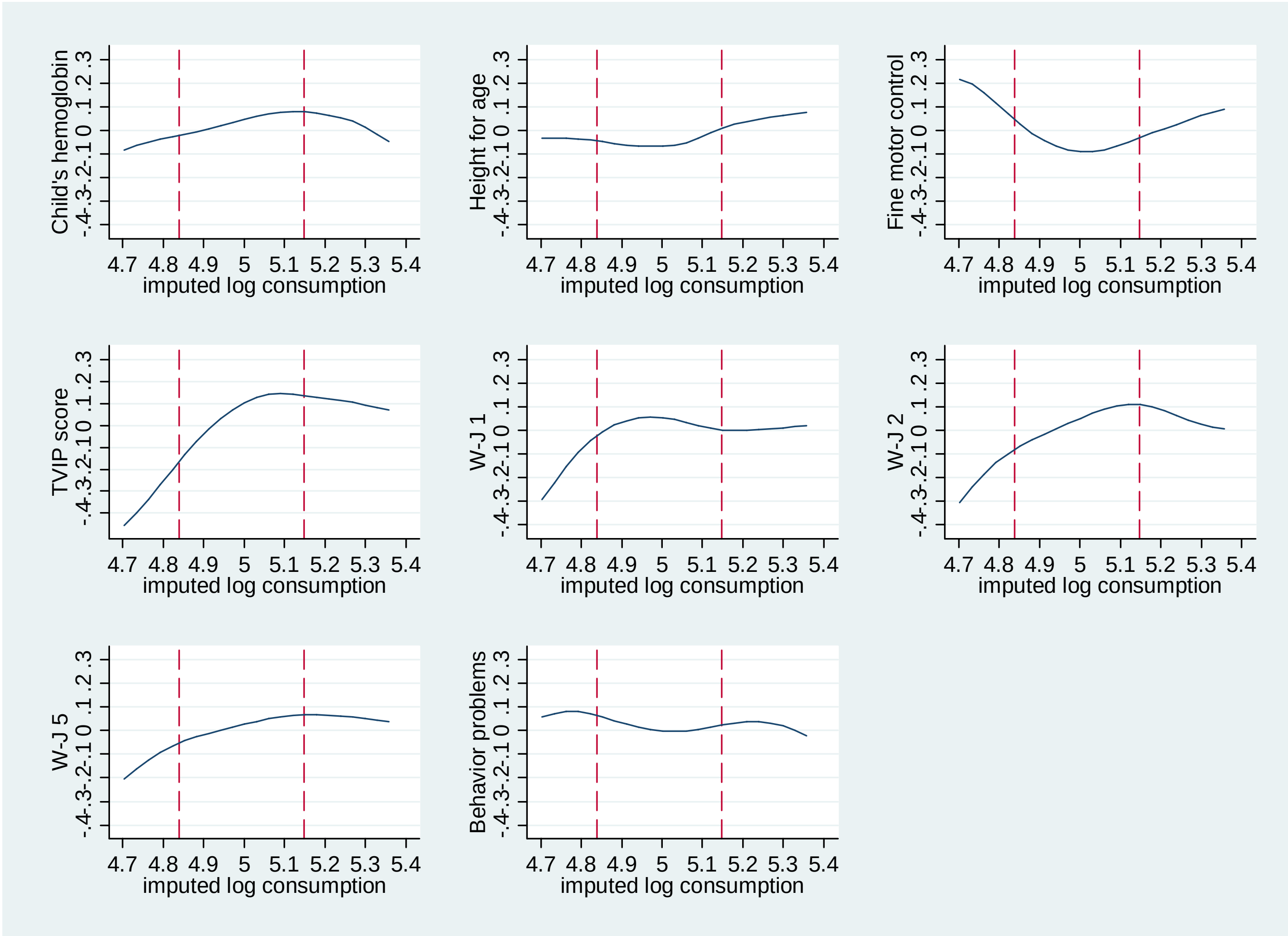

Figure 2: Children's outcomes and imputed baseline expenditure

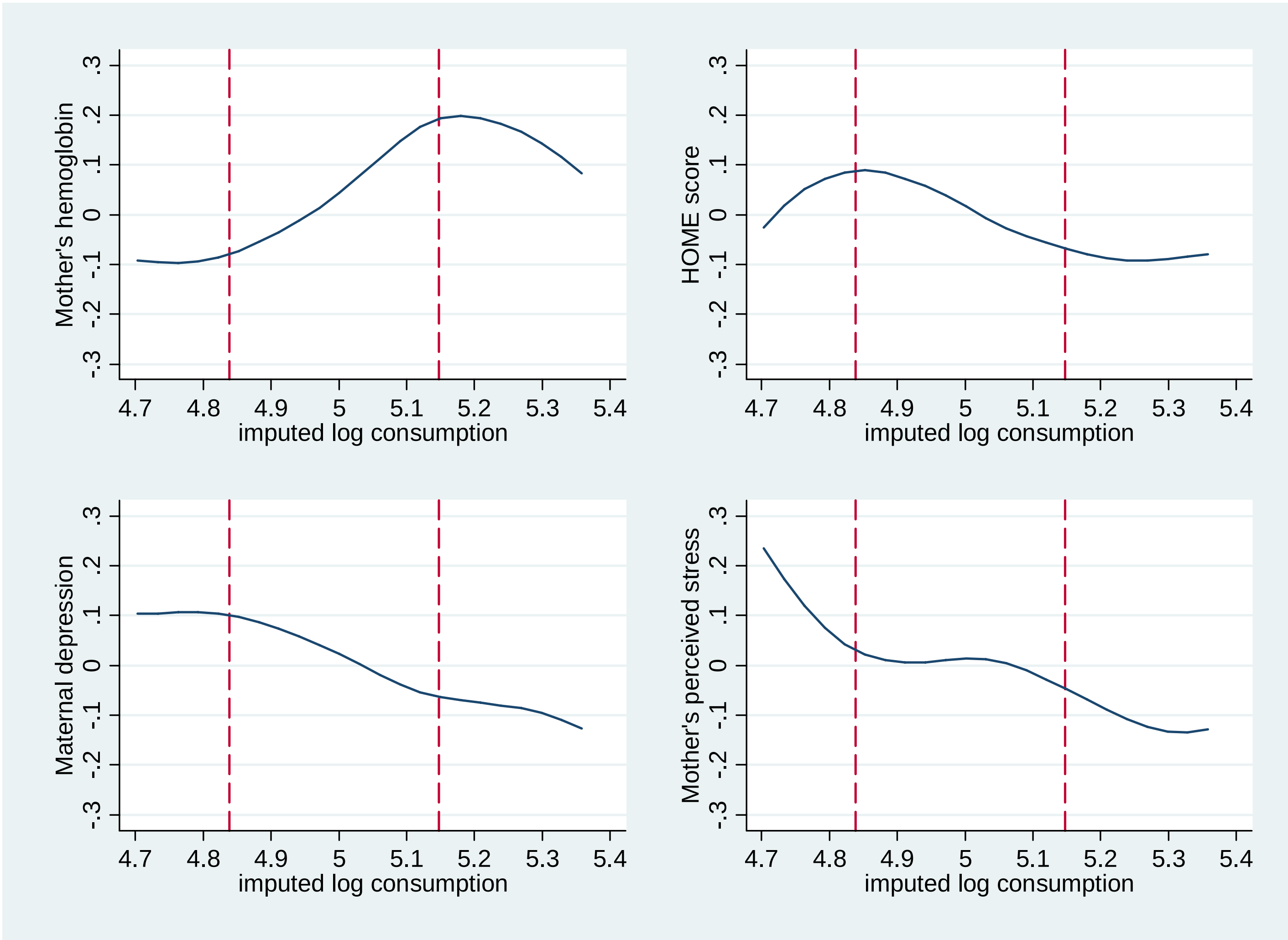

Figure 3: Mother's outcomes and imputed baseline expenditure

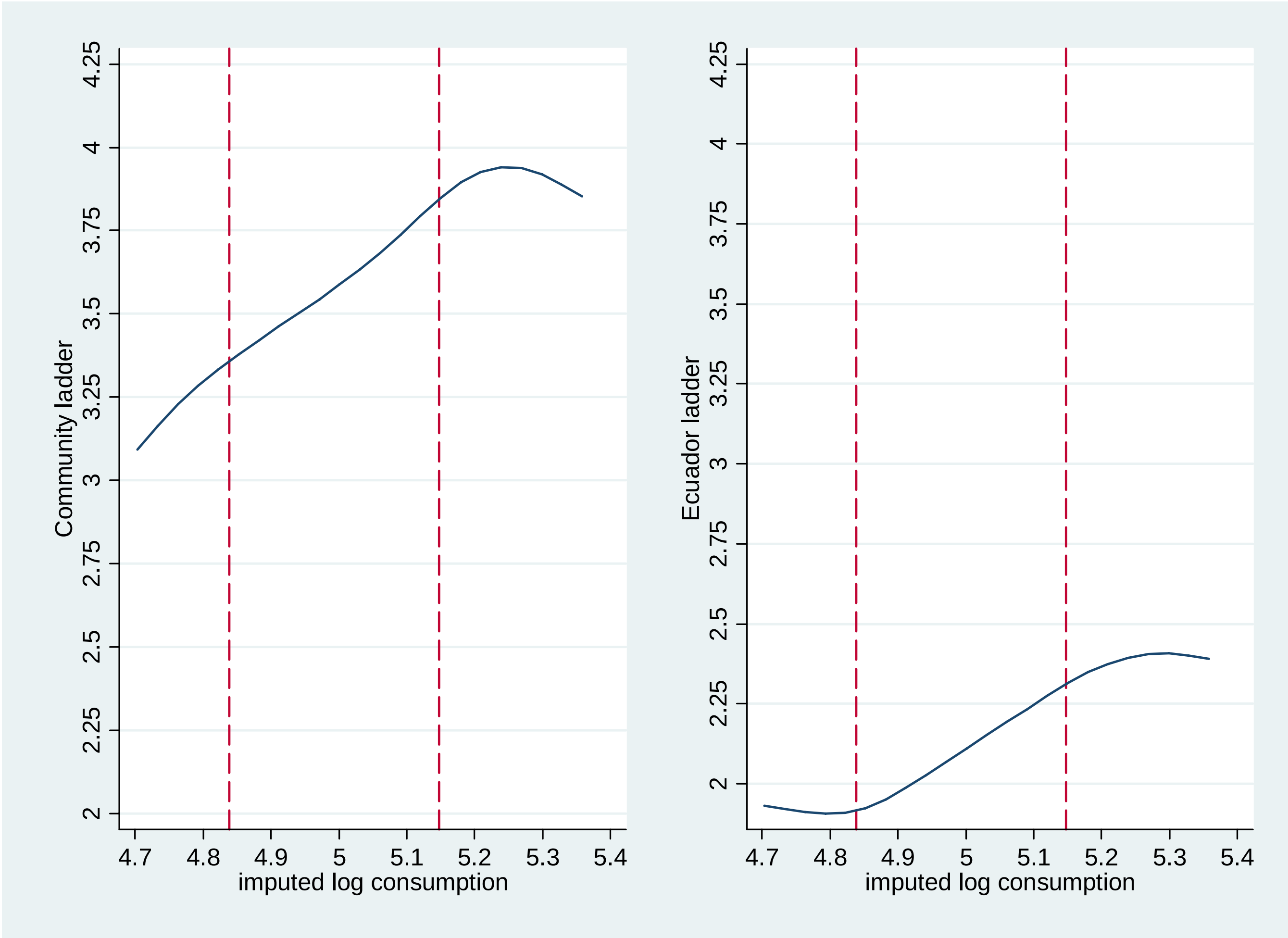

Figure 4: Ladder responses and imputed baseline expenditure

Table 1: Comparison of families which were eligible versus ineligible for the survey

		Former Bono recipients	Newly eligible
At least one child age 6 or older	Observations	26,231	9,573
	% of row	73.3%	26.7%
	Average Selben score (standard deviation)	37.75 (6.28)	36.97 (7.35)
No children age 6 or older	Observations	4,456	17,987
	% of row	19.9%	80.2%
	Average Selben score (standard deviation)	43.37 (4.85)	42.49 (5.05)

Note: These computations are for the group of 58,247 urban and rural families who were in Selben quintiles 1&2 and had at least one child in the household of any age. This group is classified into those who are former Bono recipients and those who are newly eligible for the BDH (across he columns), and those who had at least 1 child age 6 and older and those who had no children age 6 or older (down the rows). Our sample was drawn from the families in the bottom right portion of the table: newly eligible families with no children age 6 or older.

Table 2: Descriptive Baseline Statistics: Full Sample and Restricted Sample

	Full sample			Restricted sample (1,031 mothers and 1,450 children)		
	Treatment	Control	P-value, diff	Treatment	Control	P-value, diff
Family baseline characteristics (1 observation per family)						
Imputed log expenditure [1,552]	5.007 (0.016)	5.029 (0.020)	0.390	5.018 (0.018)	5.041 (0.024)	0.433
Mother's age in years [1,550]	23.574 (0.122)	23.739 (0.215)	0.508	23.710 (0.134)	23.760 (0.264)	0.866
Mother's education in years [1,548]	6.859 (0.169)	6.772 (0.231)	0.763	7.026 (0.162)	6.823 (0.291)	0.544
Mother living with husband [1,552]	0.690 (0.021)	0.690 (0.026)	0.988	0.686 (0.024)	0.659 (0035)	0.527
# Family members [1,552]	4.779 (0.109)	4.709 (0.137)	0.692	4.768 (0.113)	4.704 (0.190)	0.773
Months between interviews [1,552]	22.663 (0.277)	22.415 (0.318)	0.559	22.604 (0.288)	22.479 (0.346)	0.782
Child baseline characteristics (1 observation per child)						
Indicator: Child is male [2,210]	0.494 (0.012)	0.530 (0.017)	0.094	0.492 (0.014)	0.534 (0.024)	0.130
Child's age in months [2,197]	37.689 (0.438)	36.951 (0.607)	0.327	38.700 (0.667)	38.190 (0.740)	0.610
Child's height-for-age z-score [2,106]	–1.226 (0.076)	–1.299 (0.107)	0.842	–1.138 (0.071)	–1.140 (0.125)	0.991
Child's hemoglobin [1,961]	10.415 (0.075)	10.316 (0.087)	0.392	10.411 (0.078)	10.352 (0.096)	0.630

Notes: Both samples consist of children or mothers of children ages 3 to 7 years of age at follow-up. The "restricted sample" consists of the group of children (and their mothers) for whom all 8 child outcomes studied in this paper are non-missing. Standard errors, in parentheses, are clustered at the parish level. The data are drawn from 77 parishes in the full sample and 74 parishes in the restricted sample. The number of observations for each variable for the full sample is in square brackets next to the variable name.

Table 3: Descriptive statistics, child outcomes

	Full sample		Restricted sample (obs=1,450)
	Obs.	Mean (Std. Dev)	Mean (Std. Dev.)
Elevation-adjusted hemoglobin	1,772	11.33 (1.34)	11.36 (1.33)
Height for age (z-score, U.S. norms)	2,119	–1.239 (0.991)	–1.210 (0.996)
Peg board test—average over dominant and non-dominant hands (seconds)	2,127	73.86 (20.44)	71.837 (18.77)
Standardized TVIP score—receptive language (M=100, SD=15)	2,030	82.341 (16.52)	83.041 (16.652)
WJ-1, memory for names (percentile)	2,107	16.344 (21.78)	17.366 (22.308)
WJ-2, memory for names (percentile)	2,077	28.779 (25.91)	31.065 (25.79)
WJ-5, visual integration (percentile)	1,959	6.702 (11.83)	6.777 (11.89)
Behavior problems (mother-rated scale)	2,173	14.177 (5.40)	14.235 (5.33)

Notes: The full sample consists of all children ages 3 to 7 years old at follow-up. The restricted sample includes only those children who have non-missing measures for al of the outcomes.

Table 4: Treatment status, membership in restricted sample, and receipt of the bono

	Total received (bank records)		Receives bono (bank records)		Receives the bono (report)		In restricted sample		Receives the bono (report)	
Treated	136.87 (5.23)		0.765 (0.023)		0.801 (0.022)		0.015 (0.063)		0.808 (0.024)	
Treated x bottom quartile		141.47 (7.64)		0.803 (0.033)		0.818 (0.031)		0.008 (0.077)		0.848 (0.039)
Treated x second quartile		136.37 (8.59)		0.772 (0.035)		0.821 (0.031)		0.060 (0.077)		0.850 (0.030)
Treated x top half		134.93 (5.51)		0.744 (0.026)		0.783 (0.025)		–0.0003 (0.069)		0.771 (0.029)
Bottom quartile		–1.81 (2.61)		–0.007 (0.013)		0.002 (0.020)		–0.101 (0.063)		0.007 (0.032)
Second quartile		0.77 (4.11)		–0.003 (0.016)		0.001 (0.017)		–0.065 (0.051)		–0.021 (0.016)
Observations	2,217		2,217		2,217		2,217		1,450	

Notes: All regressions include indicators for the child's age in 3-month age bands and an indicator for the child's sex. Standard errors are clustered at the parish level.

Table 5: Bono receipts as a fraction of imputed annual expenditure

	All	Bottom quartile	2nd quartile	Top half
		All families in analysis sample		
Mean	7.8%	10.2%	8.3%	6.4%
Median	9.2%	12.6%	10.9%	7.7%
		Families in analysis sample with positive Bono receipts		
Mean	10.0%	12.5%	10.8%	8.3%
Median	10.3%	13.0%	11.4%	8.6%

Note: The fractions reported are computed as the total Bono receipts reported by banks to the family from August 2004 to July 2005, divided by imputed monthly expenditure times 12.

Table 6: Treatment effects: children's outcomes

	Controls for age and gender	Controls for age, gender and baseline family characteristics
Elevation-adjusted hemoglobin	0.129	0.131
	(0.114)	(0.105)
Height-for-age z-score	0.102	0.078
	(0.106)	(0.094)
Fine motor control (peg board)	0.155	0.164
	(0.077)	(0.075)
TVIP score (receptive vocabulary)	0.015	0.033
	(0.158)	(0.143)
WJ-1 score (memory for names)	0.163	0.201
	(0.118)	(0.120)
WJ-2 score (memory for sentences)	0.005	0.010
	(0.116)	(0.113)
WJ-5 (visual integration)	0.099	0.124
	(0.087)	(0.075)
Behavior problems scale	0.147	0.154
	(0.102)	(0.098)
	Mean effect sizes	
Physical measures	0.129	0.124
	(0.055)	(0.045)
Cognitive and behavioral measures	0.086	0.104
	(0.078)	(0.069)
All measures	0.102	0.112
	(0.060)	(0.048)
Observations	1,450	1,364

Note: All dependent variables have been converted to z-scores by subtracting the sample mean and dividing by the standard deviation. The measures of fine motor control and behavior problems have had their signs reversed so that higher values correspond to better outcomes (i.e. better fine motor control and fewer behavior problems). The sample is restricted to all children ages 3 to 7 years at follow-up, for whom all eight outcomes are measured. The controls for baseline family characteristics include the natural logarithm of imputed family expenditure, an indicator for whether the mother lived with a husband at baseline, the mother's years of education and age, and indicators for the numbers of family members in 5 age ranges (0 to 5, 6 to 14, 15 to 44, 45 to 64 and 64 or older) crossed with gender. Standard errors are clustered at the parish level.

Table 7: Treatment effects with baseline imputed expenditure interactions: children's outcomes

	Treated x bottom quartile	Treated x 2nd quartile	Treated x top half	Bottom quartile	2nd quartile
Elevation-adjusted hemoglobin	0.504 (0.204)	0.179 (0.121)	–0.031 (0.127)	–0.483 (0.206)	–0.197 (0.108)
Height-for-age z-score	0.118 (0.210)	0.040 (0.174)	0.118 (0.102)	0.158 (0.175)	0.096 (0.161)
Fine motor control (peg board)	0.320 (0.101)	0.064 (0.142)	0.138 (0.072)	–0.221 (0.073)	0.049 (0.111)
TVIP score (language)	0.144 (0.149)	–0.144 (0.244)	0.063 (0.134)	–0.528 (0.084)	–0.087 (0.154)
WJ-1 score (memory for names)	0.227 (0.145)	–0.056 (0.191)	0.248 (0.113)	–0.182 (0.113)	0.143 (0.120)
WJ-2 score (memory for sentences)	0.193 (0.159)	–0.020 (0.150)	–0.050 (0.140)	–0.285 (0.133)	–0.049 (0.127)
WJ-5 (visual integration)	0.052 (0.118)	0.035 (0.118)	0.159 (0.118)	–0.142 (0.128)	–0.062 (0.120)
Behavior problems scale	0.201 (0.181)	0.239 (0.180)	0.085 (0.106)	–0.076 (0.174)	–0.130 (0.140)
	Mean effect sizes				
Physical measures	0.314 (0.075)	0.095 (0.073)	0.075 (0.063)	–0.182 (0.062)	–0.018 (0.072)
Cognitive and behavioral measures	0.164 (0.092)	0.011 (0.121)	0.101 (0.077)	–0.242 (0.079)	–0.037 (0.084)
All measures	0.220 (0.076)	0.042 (0.084)	0.091 (0.056)	–0.219 (0.061)	–0.030 (0.051)

Note: 1,450 observations. The sample is restricted to all children ages 3 to 7 years at follow-up for whom all eight outcomes are measured. All dependent variables are measured as z-scores by subtracting the sample mean and dividing by the standard deviation. The measures of fine motor control and behavior problems have had their signs reversed, so that higher values correspond to better outcomes (i.e. better fine motor control and fewer behavior problems). Standard errors are clustered at the parish level.

Table 8: Extensions

	Controls for age, gender and baseline family characteristics, restricted sample (obs=1,364)			Controls for age and gender, full sample (max obs=2,210)		
	Treated x 1st quartile	Treated x 2nd quartile	Treated x top half	Treated x 1st	Treated x 2nd quartile	Treated x top half
Hemoglobin	0.496	0.184	–0.025	0.358	0.132	–0.064
	(0.222)	(0.118)	(0.123)	(0.200)	(0.117)	(0.122)
Height-for-age z-score	0.062	–0.005	0.134	0.022	–0.043	0.014
	(0.206)	(0.166)	(0.084)	(0.173)	(0.157)	(0.098)
Fine motor control	0.313	0.109	0.133	0.142	0.014	0.112
	(0.098)	(0.146)	(0.072)	(0.091)	(0.114)	(0.061)
TVIP score	0.112	–0.120	0.078	0.135	–0.127	0.039
	(0.136)	(0.136)	(0.117)	(0.126)	(0.206)	(0.121)
WJ-1 score	0.192	0.015	0.291	0.180	–0.102	0.209
	(0.147)	(0.201)	(0.109)	(0.121)	(0.154)	(0.104)
WJ-2 score	0.171	–0.035	–0.030	0.145	0.023	–0.073
	(0.162)	(0.162)	(0.131)	(0.135)	(0.139)	(0.135)
WJ-5 score	0.037	0.037	0.222	0.097	–0.020	0.114
	(0.117)	(0.104)	(0.186)	(0.098)	(0.113)	(0.097)
Behavior problems	0.222	0.242	0.080	0.234	0.161	–0.008
	(0.186)	(0.175)	(0.098)	(0.144)	(0.152)	(0.097)
	Mean effect sizes					
Physical measures	0.290	0.096	0.074	0.174	0.034	0.021
	(0.070)	(0.069)	(0.054)	(0.073)	(0.073)	(0.062)
Cognitive & behavioral measures	0.147	0.028	0.124	0.158	–0.013	0.056
	(0.087)	(0.117)	(0.065)	(0.074)	(0.109)	(0.071)
All measures	0.201	0.053	0.105	0.164	0.005	0.043
	(0.070)	(0.079)	(0.044)	(0.065)	(0.079)	(0.054)

Note: 1,450 observations. All dependent variables are measured as z-scores by subtracting the sample mean and dividing by the standard deviation. The measures of fine motor control and behavior problems have had their signs reversed, so that higher values correspond to better outcomes (i.e. better fine motor control and fewer behavior problems).

Table 9: Program effects on maternal health, parenting and health care

	obs.	Treated	Treated x 1st quartile	Treated x 2nd quartile	Treated x top half	1st quartile	2nd quartile
Outcomes for mothers							
Hemoglobin	1,290	0.191 (0.113)	0.442 (0.218)	0.117 (0.153)	0.119 (0.102)	–0.378 (0.194)	–0.009 (0.087)
Ecuador ladder	1,496	0.331 (0.136)	0.239 (0.174)	0.578 (0.163)	0.273 (0.167)	–0.439 (0.143)	–0.423 (0.098)
Community ladder	1,487	0.497 (0.264)	0.587 (0.313)	0.336 (0.320)	0.563 (0.315)	–0.567 (0.244)	–0.234 (0.232)
Depressive symptoms	1,227	0.010 (0.072)	0.200 (0.170)	0.160 (0.129)	–0.137 (0.088)	–0.226 (0.173)	–0.217 (0.106)
Perceived stress	1,433	–0.079 (0.067)	–0.070 (0.123)	–0.077 (0.116)	–0.074 (0.091)	–0.193 (0.115)	–0.060 (0.110)
HOME score	1,546	0.010 (0.136)	0.223 (0.151)	0.158 (0.178)	0.018 (0.152)	–0.268 (0.116)	–0.167 (0.113)
Health care and schooling outcomes							
Child had growth control in last 6 months (mean=0.370)	1,450	0.056 (0.044)	–0.031 (0.065)	0.080 (0.058)	0.084 (0.055)	–0.003 (0.058)	–0.070 (0.049)
Child had parasite treatment in last 12 months (mean=0.592)	1,447	0.129 (0.042)	0.247 (0.067)	0.050 (0.072)	0.121 (0.046)	–0.128 (0.045)	0.061 (0.055)
Child in a preschool or grade school (mean=0.44)	1,450	0.044 (0.046)	0.124 (0.062)	0.042 (0.056)	0.020 (0.045)	–0.178 (0.052)	–0.078 (0.038)

Notes: Regressions for mother's outcomes control for the age of the mother (in 10-year age bands). Regressions for children control for the child's age and gender. Standard errors are clustered at the parish level.