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Essays on income shocks and health in Ecuador

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By

Melissa Lucia Hidrobo

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Committee in charge:

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Abstract

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This dissertation examines how exogenous changes in income affect the most vulnerable populations in Ecuador: poor children and their mothers. It explores the effects of a negative income shock that is due to the 1998-2000 economic crisis and a positive income shock that is due to the introduction of a cash transfer program, the *Bono de Desarrollo Humano*. The outcomes examined are those related to early childhood development and to spousal domestic violence. Both improvements in child development and reductions in domestic violence are crucial for breaking the inter-generational transmission of poverty and for achieving the Millennium Development goals.

The first chapter of this dissertation investigates the effects of Ecuador's 1998-2000 economic crisis on child health and cognitive development. This crisis was characterized by a drastic increase in prices and the eventual adoption of the dollar as its currency. While many reports show that household consumption decreased and poverty increased, there are no studies on the impact of the crisis on child development. I use data from three and five years after the crisis to investigate whether the Ecuadorian crisis had a negative and persistent impact on children's height and language development. In order to estimate the effect of the crisis on child outcomes I take advantage of the variation in children's exposure to the crisis that is due to birth month and province. Results suggest that one year of exposure to the crisis significantly decreased height-for-age z-scores by .1 SD and vocabulary test scores by 2.4 points. The effect of the crisis, however, was not uniform across households. Particularly, in rural areas children from farming households and those with more access to early childhood nutrition programs were significantly more protected from the crisis. Potential pathways through which the crisis impacted child outcomes are also explored.

The second chapter, which is co-authored with Lia Fernald, takes advantage of the randomized roll-out of Ecuador's *Bono de Desarrollo Humano* (BDH) to examine the effects of an exogenous increase in income on the health and development outcomes of very young children. Families enrolled in the BDH received a monthly cash stipend ($15 USD) representing an approximate 6-10%

increase in household income. Participants analyzed in this study are children 12-35 months old from treatment and comparison communities in rural and urban Ecuador. Main outcomes measured are language skills (the Fundación MacArthur Inventorio del Desarollo de Habilidades Comunicativas - Breve), height-for-age z-score, and hemoglobin concentration. Results indicate that in rural areas, being randomized to receive the BDH in very early childhood led to significantly better performance on the number of words a child was saying, and on the probability that the child was combining two or more words. There were no significant effects on language development for children in urban areas and there were no effects on height-for-age z-score or hemoglobin concentration in rural or urban areas. A limited number of potential pathways with respect to cognitive/language stimulation, health behaviors, and parenting quality are also explored. Findings indicate that compared to children in comparison areas, rural children in treatment areas were more likely to have received vitamin A or iron supplementation and have been bought a toy in the past six months.

The third chapter, which is co-authored with Lia Fernald, also takes advantage of the randomized roll-out of the *Bono de Desarrollo Humano* to investigate how an exogenous increase in a mother's income impacts spousal domestic violence. We use existing household bargaining models to predict when a cash transfer will lead to an increase in domestic violence and when it will lead to a decrease. Consistent with predictions, results reveal that for more educated women who had more credible outside-of-marriage options, the BDH decreased psychological violence. However, for less educated women who did not have credible outside options, the effect of the cash transfer was ambiguous and depended on whether the husband or partner had more education than his wife or partner. In particular, for less educated women the cash transfer decreased psychological violence in households where the husband or partner had more education than his wife or partner, and it increased psychological violence in households where the opposite was true. Although the BDH led to significant changes in psychological violence, it did not lead to corresponding changes in physical violence.

Para mi papi, Jorge A. Hidrobo, que con su buen character
y optimismo inspira a todos.
Gracias por todo tu apoyo y amor.

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Preface

At the end of the 20th century Ecuador experienced an economic and political crisis that drastically changed the economic landscape. During this tumultuous time, banks closed, the dollar was adopted as the new currency, and a large new social protection programs was created. The crisis and the subsequent creation of a cash transfer program, *Bono Solidario*, which then became the *Bono de Desarrollo Humano* (BDH), greatly impacted the daily lives of individuals. This dissertation examines the effect of the economic crisis and the BDH on the most vulnerable populations in Ecuador: poor children and their mothers. The three main questions that this dissertation addresses are: 1) How did the 1998-2000 economic crisis impact children's health and cognitive development? 2) Did the BDH improve the health and language development of very young children? and 3) By providing mothers with a new source of income, did the BDH decrease spousal domestic violence?

For analyzing all three questions I use a dataset that was collected by the World Bank and the Government of Ecuador in 2003-2004 (baseline) and 2005-2006 (follow-up). The data were collected in order to investigate the effects of the BDH on children's health and cognitive development, and contain information on household characteristics, the health status and health care usage of children and their mothers, language abilities of children, and anthropometric measures. The sample at baseline consisted of 3,426 households and 5,547 children ages 0-71 months from 6 provinces in Ecuador who were eligible for the BDH. To this BDH sample, 1,630 households and 2,442 children that were slightly too wealthy to be eligible for the BDH were added. Thus, in total there were 7,989 children at baseline.

Given the different focus of the three questions, each analysis uses a different part of the data. For investigating the effect of Ecuador's 1998-2000 crisis the analysis uses children 1-72 months old. For exploring the effects of the BDH on development outcomes of very young children, the analysis focuses on children 12-35 months old at follow-up from the BDH sample. Consequently these children were born after the 1998-2000 economic crisis. Finally for exploring the effect of the BDH on spousal domestic violence, the analysis uses data on all mothers from the BDH sample that responded to the domestic violence questions.

This dissertation proceeds as follows. Chapter one investigates the effect of Ecuador's 1998-2000 economic crisis on child health and cognitive devel-

opment. It begins by providing background information on the 1998-2000 economic crisis and examines the potential pathways through which the crisis could have affected children's development. It describes the econometric strategy used in the analysis which takes advantage of the variation in children's exposure to the crisis that is due to birth month and province; and it presents the results which include the average effects of the crisis on child outcomes and the heterogeneous effects with respect to farming households, access to early childhood nutrition programs, and sensitive periods in a child's development.

Chapter two takes advantage of the randomized roll-out of the BDH to explore how an exogenous increase in a mother's income affects young children's health and language development. It begins by laying out the framework for early childhood development and explains how an increase in income could improve child outcomes. It provides information on the *Bono de Desarrollo Humano*, presents the results for both rural and urban areas, and explores possible mechanisms through which the BDH impacted children's health and language development.

Chapter three also takes advantage of the randomized roll-out of the BDH to explore how an exogenous increase in a mother's income affects spousal domestic violence. It presents theoretical models that explain why an increase in a woman's income affects domestic violence and uses an existing household bargaining model to predict when a cash transfer will lead to an increase in domestic violence and when it will lead to a decrease. It takes the predictions of the model to the data and shows that the results are consistent with the predictions.

The three chapters of this dissertation reveal that exogenous changes in income significantly impact children's health, language development, and spousal domestic violence. The effects, however, are not uniform across households. Chapter one reveals that the 1998-2000 Ecuadorian crisis led to a deterioration in children's health and cognitive development; however, in rural areas children from farming households and those with more access to early childhood nutrition programs were more protected from the crisis. Chapter two shows that the BDH led to improvements in children's language development but only for children from rural areas. Lastly, chapter three reveals that the BDH led to a decrease in domestic violence but only for more educated women and for women who had less education than their husbands or partners.

It is the hope that the theories and results presented in this dissertation not only lead to a better understanding of how exogenous changes in income affect children and their mothers, but that they also help guide policies aimed at improving child development and reducing domestic violence.

Chapter 1

The effect of Ecuador's 1998-2000 economic crisis on child health and cognitive development

1.1 Introduction

In developing countries instruments to smooth consumption such as insurance and credit are often scarce. Thus, during negative economic shocks, households respond by reducing consumption, and/or using sub optimal instruments to smooth consumption such as selling productive assets. If households are forced to reduce consumption or sacrifice nutritional quality during negative economic shocks, this can have detrimental effects on children's health and cognitive development. The impact of negative economic shocks is exacerbated if governments cannot adequately respond to the shock and instead are forced to reduce public spending. My goal is to investigate the impact of the 1998-2000 Ecuadorian crisis on child health and cognitive outcomes.

The 1998-2000 Ecuadorian crisis was triggered by the El Niño phenomenon, a drop in oil prices, and the international financial crisis. These three events led to a banking and financial crisis within Ecuador that caused prices to increase dramatically and public spending to decrease. While many reports show that consumption fell and poverty increased during the crisis, there is little information on how the health and cognition of children were affected. From a policy perspective this is important not only because we care about child outcomes in their own right but also because worse health and cognitive outcomes early in life lead to worse education and labor outcomes later in life [Glewwe and Jacoby, 1995, Glewwe et al., 2001, Alderman et al., 2006]. Thus, it is important to understand how children are affected during economic crises and find ways to mitigate any negative impact.

The possible pathways through which the crisis impacted a child's development are through a deterioration of the health environment (due to decreased public spending and El Niño); a reduction in the consumption of normal goods

such as food, health services and books; and a change in parents time spent on the care of their children. The direction of the change in parent's time is theoretically ambiguous because on the one hand a reduction in current income may lead to a reduction in the opportunity cost of time, and therefore, to parents spending more time with their children [Miller and Urdinola, 2010]. On the other hand, a reduction in current income may lead to distress work and an increase in the labor participation of secondary workers (often mothers)[Bhalotra, 2010]. While disentangling the different pathways is beyond the scope of this paper, results suggest that child health and cognitive development were not pro-cyclical thus ruling out any large increases in parental time.

The data I use to identify the impact of the crisis are health and cognitive development data for children 0-5 years old, collected by the Government of Ecuador and the World Bank in 2003 and 2005. The timing is important because children in the sample who were exposed to the crisis were 0-3 years old when exposed. Many studies show that children's physical growth and cognitive development are the most vulnerable to insults in their first two/three years of life; therefore, any malnutrition and deficits occurring during these years will permanently affect their growth [Shrimpton et al., 2001, Carter and Maluccio, 2003]. Consequently, malnutrition that occurred in children 0-3 years old during Ecuador's economic crisis will lead them to have worse health outcomes after the crisis. Due to the fact that health and cognitive outcomes are measured three and five years after the crisis, this study provides insight on whether the impact of the economic crisis persisted.

In order to estimate the effect of the crisis on health and cognitive outcomes I take advantage of the variation in children's exposure to the crisis due to birth month and province. The two rounds of survey allow me to control for age using indicators and not lose variation in exposure. In other words, a 5 year old child surveyed in 2003 will have been differentially exposed to the crisis when compared to a 5 year old surveyed in 2005. In addition to age fixed effects, I use mother-fixed effects to control for the endogenous response of mothers to the crisis and I control for time trends in a linear fashion. Thus, identification of the impact of the crisis occurs by comparing sibling differences in outcomes across households with children of similar ages but who were differentially exposed to the crisis. Results suggest that one year of exposure to the crisis significantly decreased height for age z-scores by .1 SD and vocabulary test scores by 2.4 points. Compared to baseline averages, this is approximately a 7% reduction in height for age z-scores and a 3% reduction in vocabulary test scores.

While the economic crisis affected all sectors and regions of Ecuador, the impact of the crisis was not the same across households. Consequently, in addition to measuring the average effect of the crisis, I investigate heterogeneous effects with respect to households that were less vulnerable to the crisis. The groups that are explored are farming households and households with more access to early childhood nutrition programs. Farming households that are net producers often gain from increases in food prices. However, even those that

are not net producers are more protected when food prices increase because they can substitute purchased food with home-grown food [de Janvry and Sadoulet, 2009, Thomas and Frankenberg, 2007]. Children with more access to early childhood nutrition programs are also more protected because they can substitute purchased food with free food supplements and vitamins [Yamano et al., 2005]. Consistent with the assertion that children from farming households and those with more access to early childhood nutrition programs are more protected from increases in food prices, I find that children from these households were significantly less affected by the crisis.

The contribution of this investigation to the literature is two fold. First it analyzes the impact of Ecuador's 1998-2000 economic crisis on child health. The evidence of the impact of income shocks on child health is mixed. As Ferreira and Schady [2009] assert, the ambiguous effect is due to the counteracting processes of increased time and decreased private and public spending that result from negative income shocks. In general, studies in developing countries have used weather shocks [Jensen, 2000, Hoddinott and Kinsey, 2001] or macro-economic shocks [Stillman and Thomas, 2008, Frankenberg et al., 1999] and have found a negative relationship between adverse income shocks and health. However, Stillman and Thomas [2008] show that the nutritional status of children in Russia is vulnerable to long term fluctuations in income but not to short term fluctuations. Moreover, Frankenberg et al. [1999] find that while spending on health care and adults BMI decreased during Indonesia's 1998 economic crisis, there was little evidence that the height of children deteriorated. One potential reason for why these two studies did not find short term impacts on children's height is that these studies did not focus on young children whose height is the most vulnerable to insults. Thus my study will contribute to the existing literature by focusing on children who were less than three years old during the crisis.

While the effect of income shocks and health has been well studied, less is known about the the effect of income shocks and cognitive development. There are many studies that explore the relationship between income or social economic status and cognitive development and find a positive relationship [Blau, 1999, Paxson and Schady, 2005]. The handful of studies that investigate how shocks influence children's cognitive development take advantage of the introduction of cash transfer programs, such as *Oportunidades* in Mexico, *Bono de Desarrollo Humano* (BDH) in Ecuador, and *Atención a Crisis* in Nicaragua, and find positive program effects [Macours et al., 2008, Fernald et al., 2008, Paxson and Schady, 2010]. However, the studies mentioned above do not focus on negative income shocks. Thus, the second main contribution of this study to the existing literature is that it investigates the impact of a negative economic shock on early childhood cognitive development. The finding that an economic shock led to a deterioration of cognitive skills cautions researchers from using shocks as an IV for health when investigating the relationship between early childhood health and later academic outcomes because in all likelihood the shock also affected a child's early cognitive development.

The rest of this paper is organized as follows: Section 2 provides background information on the crisis; Section 3 discusses the data I use to conduct my analysis and presents descriptive statistics; Section 4 outlines the conceptual framework and the estimation problem; Section 5 discusses the econometric strategy; Section 6 presents the results; and Section 7 concludes.

1.2 Background

1.2.1 Crisis

The 1998-2000 economic crisis in Ecuador was mainly due to the following exogenous factors: the El Niño phenomenon in 1997-1998, the fall in oil prices in 1998, and the 1997 international financial crisis. The El Niño phenomenon caused extremely heavy rainfall in Ecuador, and as a result, floods and landslides damaged approximately 14% of the total agricultural land in the coastal area [Vos et al., 1999]. In addition to agriculture, El Niño had an adverse effect on the infrastructure and health environment of coastal provinces.

During the same year of El Niño, oil prices dropped and the international financial crisis peaked, all of which exposed the weaknesses of the banking system and precipitated a financial crisis within Ecuador. The banking system was severely affected by these events because they had lent heavily to sectors in coastal agriculture, oil, and other exports. The increase in their nonperforming loans and the decrease in credit from foreign banks led to an intense liquidity pressure which caused the collapse of 1/3 of all Ecuadorian banks by 1999 and a speculative run on the sucre. These events led President Mahuad to freeze bank deposits in March 1999 and in January 2000 the dollar was adopted as the national currency [Jacome, 2004, Parandekar et al., 2002]. The exchange rate chosen for dollarization (25 sucres/dollar) was very undervalued, and thus, there was a sharp reduction in real wages right after dollarization [Parandekar et al., 2002].

The economic crisis was characterized by a drastic increase in yearly inflation rates which began rising in 1998, peaked at 108% in 2000, and then began to decline in 2001 (Figure 1.1). The rapid increase in prices caused real wages to decrease by 40% from January 1999-March 2000 [IADB, 2008]. As a result, aggregate household consumption declined by approximately 33% during 1999-2000 (Figure 1.2) and poverty increased by 65% from 1995 to 1999 (Table 1.1). In addition to the decrease in real wages, unemployment doubled from 8.5% in May 1998 to 16.9% in June 1999. Although unemployment increased, urban labor participation of males and females actually increased in 1998 and 1999 (Figure 1.3), thus leading to an overall increase in occupation rates (Table 1.2). The increase in occupation rates was much larger for women than men which reveals that the crisis led to an increase in secondary workers.

In addition to a large decrease in household consumption, the crisis severely hurt the government's budget. The drop in oil prices, the El Niño, and the

banking crisis all led to a massive increase in the public deficit, which forced the government to reduce social spending and delay payments to local governments and public servants [Vos, 2000]. Specifically, social spending decreased by approximately 50% from 1998 to 2000 and then increased back to pre-crisis levels in 2001 (Table 1.3).

Although all regions and segments of society felt the severity of the crisis, different groups were particularly affected. The urban middle and lower class were among the most affected due to the collapse of the banking system, the decline in employment, and the decline in real wages [WorldBank, 2004]. However, among the very poor, the increase in the severity of poverty was larger in rural areas than urban ones (Table 1.1). Thus, while more individuals were falling below the poverty line in urban areas, the severity of extreme poverty was increasing in rural areas. This distinction is one potential reason why chronic malnutrition actually increased in rural areas and not urban areas during the crisis (Table 1.1). Another distinction between rural and urban areas is that the increase in the percent of individuals occupied during the crisis was greater in rural areas than urban areas (Table 1.2).

The review above on the Ecuadorian crisis reveals three channels through which the crisis affected child health and cognitive development: a decrease in real income, a decrease in the amount of time a parent spent at home, and a decrease in the health environment. A qualitative study by CEPLAES on the crisis shows how the Ecuadorian crisis reduced the quantity and quality of food consumed and health services demanded. In the study there are many quotes such as the following that reveal the decline in consumption of the poor [Leon and Troya, 2000]:

> "Hemos dejado de comprar casi totalmente carne, arroz, azucar, papas. Todavía seguimos comprando, cuando hay plata fideos, sal, condimentos. Las verduras estan muy caras..."
>
> Translation: We have stopped buying almost totally, meat, rice, sugar, and potatoes. We still buy, when we have money, noodles, salt, and condiments. Vegetables are too expensive...
>
> "Si algún miembro familiar se enferma, ya no tenemos la posibilidad de decir: bueno, cualquier rato le llevamos al hospital, para hacerlo atender urgentamente porque la condición economica esta bastante baja..."
>
> Translation: If a member of the family gets sick we can no longer say: ok, at any moment we can take him to the hospital to be attended to urgently because the economic conditions are extremely low.

1.3 Data and descriptive statistics

1.3.1 Data

The health and cognitive ability data that I use to investigate the effect of Ecuador's economic crisis on child outcomes were collected by the World Bank and the Government of Ecuador to conduct an evaluation of a cash transfer program, *Bono de Desarrollo Humano* (BDH). The baseline survey was conducted between October 2003-September 2004 and the first follow-up was conducted between September 2005-January 2006. The sample was constructed by selecting 6 provinces in Ecuador that had not begun receiving the BDH (3 coastal provinces and 3 highland provinces). Parishes within these provinces were stratified into rural and urban groups and then randomly selected.[1] Within the parishes, households that met the following criteria were randomly selected: households had to have preschool age children, no children 6 years old and older, and be eligible for the cash transfer program.[2] In total this sample consisted of 118 parishes, 3,426 households, and 5,547 children and was known as the BDH sample. To this sample 1,630 households (2,442 children) that were slightly too wealthy to be eligible for the transfer were added. Thus there were a total of 7,989 children under the age of 6 years old at baseline. Of these children, 93% were re-surveyed in the follow-up, and 1,285 children who were born after the baseline or were missing during the baseline were added to the follow-up. Therefore, at the follow-up there were 8,702 children of which 7,417 were also in the baseline and 1,285 were born after.

For this study, children who have inconsistent gender or age between the baseline and follow-up are dropped (approximately 5% of sample). Due to the fact that newborns are hard to measure and their length reflects whether or not they are premature babies, I exclude newborns from the study, though the results are robust to their inclusion. I also exclude children older than 71 months at follow-up because they have no comparison group at baseline. However, results are also robust to their inclusion. In the end, the sample I use for this study is 6,977 children 1-71 months old at baseline and 6,787 children 1-71 months old at follow-up.

The main health indicator of interest is children's height. Children's height is converted to age and sex adjusted z-scores using the World Health Organization's (WHO) standards. Children with z-scores greater than 6 or less than -6 are converted to missing as are children who grew less than 2 cm or had negative growth between the baseline and follow-up. 2 cm is a conservative cutoff for minimal growth rates for a 5 year old over a 12 month period taken from Roche and Himes [1980]. The cognitive development indicator of interest for children 36 months and older is the score from a vocabulary test called

[1] Parish is the smallest administrative unit in Ecuador

[2] Households who are in the bottom two quintiles of the SELBEN poverty index are eligible for the transfer.

Test de Vocabulario en Imagenes Peabody (TVIP). This test is administered by showing a child slides that contain 4 pictures. For each slide, the child points to the picture that corresponds to the word stated by the interviewer. The raw scores are age-normed using a reference population of Mexican and Puerto Rican children. These norms set the mean at 100 and the standard deviation at 15.

The timing of the crisis and the structure of the data are key for identifying the impact of the crisis on child health and cognitive outcomes. Table 1.4 provides a time line for the crisis and data collection, and describes the ages of the children at each point in time. In the sample less than 1% of the children are born in 1997 because to be included in the sample a household could not have children older than 71 months at the time of the survey. As you can see from the table, children affected by the crisis are between the ages of 3-5 years during the baseline. Children born after the crisis are 0-3 years old during the baseline and 0-5 years old during the follow-up survey. Having more than one round of data allows comparisons of children's height and TVIP scores to be made for children of the same age who were differentially exposed to the crisis.

In addition to data on child outcomes, I use data on prices to define the timing of the crisis. The price data I use is the consumer price index that is provided by the Instituto Nacional de Estadisticas y Censos (INEC). It is monthly data from 1996-2005 at the province level.[3] I use the price data to define the month a province entered and exited the crisis. Specifically, I estimate the pre-crisis (1996-1997) average growth rate and use it to predict prices during and after the crisis.[4] I then take the deviation of actual prices from predicted prices to calculate when a province entered and exited the crisis. The month a province entered the crisis is defined as the month when actual prices deviated from predicted prices by more than 5 standard deviations. The month a province exited the crisis is defined as the month when the deviation of actual to predicted prices was the largest. Figure 1.4 demonstrates how the dates of the crisis are defined for Azuay province. The vertical dotted lines represent the start and end month of the crisis.

As Table 1.5 reveals, the timing of the crisis differed slightly by province with Los Rios being the first to enter the crisis in February 1998. This pattern is consistent with the fact that Los Rios was the province hardest hit by El Niño, and thus began to experience rising prices earlier than the rest of the sample provinces. Prices in the other provinces began to increase drastically in the summer and fall of 1998. Consequently, instead of defining crisis years as 1998-2000 for all provinces, I specify more precisely the timing of the crisis for each province using the months defined in Table 1.5. Although, I use Table 5 to define the timing of the crisis, results are robust to using the more general January 1998- December 2000 definition of the crisis.

[3]Two series of price data was available. One from 1996-2004 and the other from 2005-2009. I used inflation rates from 2005 to construct one series of prices from 1996-2005.

[4]Average growth rate was estimated by running the following regression: $log\, price_{tm} = \beta_0 + \beta_1 time_t + \gamma_m + \varepsilon_{tm}$

The exogenous variable of interest is a child's length of exposure to the crisis. Length of exposure is measured by the number of months living in the crisis, with crisis months defined by Table 1.5. Thus, the number of months living in a crisis is jointly determined by province and birth month. Birth months are labeled so that the youngest child born in December 2005 has a birth month of 1 all the way up to the oldest child born in October 1997, and has a birth month of 99. For children born before April 2001, exposure to the crisis is a non-decreasing function of date of birth as labeled by birth month. In other words, the closer a child's birth is to 2001, the less exposed a child is to the crisis. Thus, a child born in Los Rios in January 1998 (birth month 96) is exposed to the crisis for 29 months while a child born in Los Rios in January 1999 (birth month 84) is exposed to the crisis for 18 months. Any child born after March 2001 is exposed to the crisis for 0 months. A table showing how birth month and exposure are calculated can be found in the appendix (A.1).[5]

1.3.2 Descriptive statistics on child health and cognitive outcomes

Due to the sampling design, households in the sample are poorer and younger than the average household in Ecuador [Paxson and Schady, 2010]. These characteristics are reflected in Table 1.6. Specifically, mothers in the sample are young with a mean age of 24.4 years, 59% are not married, and the mean years of schooling is 7.8. Children's cognitive outcome of interest for the analysis is their score on the *Test de Vocabulario en Imágenes Peabody* (TVIP), which was administered to children 36 months and older. The mean normalized TVIP score for the sample is 86.79, which is almost one standard deviation below the mean of the reference population.[6] The mean height-for-age z-score is -1.34, which is more than one standard deviation below the mean of the reference population. The percent of children suffering from chronic malnutrition at baseline is 29.4%.[7] 23% of children in the sample are from farming households, with farming being defined as households that "own, share or rent farms or land for agricultural or livestock use." The mean number of months a child is exposed to the crisis is 6.37 months. However, conditional on being exposed to the crisis, the mean number of months exposed is 14.25.

Table 1.7 shows children's mean height for age z-scores across waves. For the younger children (<36 months), the means are very similar across waves. On the other hand, for the older children (36-71 months), the baseline mean is significantly lower than the follow-up mean. This change in mean height for age z-score between 2003 and 2005 is consistent with the story that older

[5]Unfortunately, I do not know know a child's birth location, and thus, I use vaccination location instead. Since most children received a vaccination before the age of 1, I believe this a good proxy for birth location.

[6]Reference population are Mexican and Puerto Rican children

[7]Chronic malnutrition is measured by the percent of children with height-for-age z-scores less than -2.

children (36-71 months) in the baseline were exposed to the crisis and thus have worse height z-scores than older children (36-71 months) in the follow-up who were for the most part born after the crisis and thus not as affected. The younger children were never exposed to the crisis, and therefore, serve as a comparison group to see how the means in height shift from baseline to follow-up in the absence of the crisis. Unfortunately, since TVIP was only administered to children older than 35 months, the same comparison in TVIP trends between younger and older children cannot be made.

1.4 Conceptual framework

1.4.1 Dynamic health and cognitive development

The following conceptual model for health and cognitive development draws heavily from models outlined in the papers by Currie [2000], Cunha and Heckman [2007], and Strauss and Thomas [2008]. I model health and cognition in a similar manner because they are both stock variables, and thus depend not only on current inputs but also previous health and cognition levels. Specifically, the present stock of a child's health or cognitive development, H_t, is an increasing function of previous health or cognitive development, H_{t-1}, material inputs such as food, N_t, time inputs such as a mother's time breastfeeding her child, K_t, and the environment, D_t. Although health and cognition depend on the same general inputs, their relative sensitivity to these inputs will vary. Consequently, I allow the technology that maps inputs into outcomes to vary by health or cognition which leads to the following production functions:

$$H_t^s = h^s(H_{t-1}^s, N_t, K_t, D_t) \; where \; s = health \; or \; cognition \tag{1.1}$$

Repeated substitution of lag versions of equation 1.1 yields health or cognition as a function of all past and current inputs and of a child's initial health or cognitive endowment.

$$H_t^s = h^s(H_0^s, N_1...N_t, K_1...K_t, D_1...D_t) \tag{1.2}$$

Equation 1.2 reveals the structural relationship between health or cognition and its inputs. Material and time inputs are endogenous variables that depend on prices and wages.[8] Ecuador's crisis led to changes in prices and wages in previous periods, and thus to changes in inputs in previous periods. Specifically, the huge increase in prices and decline in real income during the crisis led to a decrease in consumption of normal goods, and thus N. It also led to an increase in secondary workers, and particularly females, which reduced a mother's time spent with her child, K. In addition to changes in prices and real income, Ecuador's crisis led to the deterioration of the health environment, D,

[8]The appendix (A.2) presents the dynamic household maximization problem in more detail.

that was due to a decrease in public spending and the El Niño. As equation 1.2 shows, reductions in these inputs in previous periods, lead to worse outcomes for children in the current period. Consequently, children born during the crisis who experienced these decreases in inputs will have worse outcomes than children born after the crisis who did not experience these decreases.

Although the crisis was a national phenomenon the impact of the crisis on a child's development will vary by a household's ability to cope with the crisis. In other words, the extent to which changes in prices and income map into changes in inputs depends on household characteristics. Households that are able to smooth their consumption will not be as harmed by the increase in prices and decrease in real income. Households with more access to food aid, will be able to smooth their consumption of food during a negative income shock by substituting purchased food for free food. Households that farm are also more likely to substitute purchased food for home-grown food and thus are more protected from the crisis.

An important extension of the model presented above allows the technology that maps inputs into health and cognition to vary at each time period.

$$H_t^s = h_t^s(H_0^s, N_1...N_t, K_1...K_t, D_1...D_t) \tag{1.3}$$

Using this model, Cunha and Heckman [2007] define a sensitive period as a period t^* where $\frac{\partial H_t}{\partial I_{t^*}} > \frac{\partial H_t}{\partial I_s} \mid_{s \neq t^*} for\ I = N, K, D$. In other words, a sensitive period is defined as a period when inputs in that period are more productive than the same level of inputs in other periods. This notion of sensitive periods is important for my investigation because children exposed to the crisis were exposed when they were 0-3 years old. Within this critical period, there may be sensitive periods when inputs matter more, and consequently, when the impact of the crisis is larger.

1.4.2 Estimating the effect of the economic crisis

Equation 1.2 shows that health and cognitive development are a function of current and past inputs. Ecuador's macroeconomic crisis affected child outcomes through its impact on past inputs. The magnitude of the effect of the crisis depends on the length of a child's exposure to the crisis because every period matters in the production of current health and cognitive development. Thus I exploit the variation in a child's length of exposure to the crisis to estimate the effect of the crisis on child outcomes. In addition to estimating the average effects of the crisis, I investigate heterogeneous effects with respect to household's that are more protected from the crisis. In the main specifications I assume that the effect of the crisis in every time period is equal and thus allow exposure to enter the estimation equation in a linear manner. However, I also relax this assumption in order to investigate whether there are sensitive periods when the effect of the crisis was larger. Before discussing the specific estimation equations, it is important to discuss the issues that need to be addressed when estimating the impact of the crisis on child outcomes.

Although the crisis provides an exogenous source of variation in the economic and health environment, I cannot run a reduced form regression of height or TVIP scores on length of exposure to the crisis and obtain an unbiased estimate of the crisis. The main challenge with estimating the effect of the crisis on health and cognitive outcomes is disentangling the crisis effect from secular time trends and from age trends. Specifically, the timing and structure of the data implies that the older children in the sample are the ones affected by the crisis and thus should have worse outcomes. However, in general height for age z-scores decrease with age because of growth faltering [Shrimpton et al., 2001]. Consequently, I cannot distinguish if older children have worse z-scores because of their age or because they were affected by the crisis or because of both the crisis and their age. Furthermore, if health and cognitive development are improving over time, then older children will also have worse outcomes. One reason why health might improve overtime is if the health technology is improving over time.

The strategy I use to disentangle the age and time trend effects from the crisis effect is to control for age using age fixed effects and control for time trends linearly using month of birth. The age-fixed effect means that I will be comparing outcomes of children of the same age. In order to compare outcomes of children of the same age and still have variation in exposure to the crisis, at least two rounds of data are needed. Thus, a 60 month old child born in Los Rios in January 1999 and surveyed in January 2004 will have been exposed to the crisis for 18 months while a 60 month old child born in January 2001 and surveyed in January 2006 will have had no exposure to the crisis. In order to capture secular time trends I control for birth month. Since improvements in the health and cognitive environment may vary across provinces, I allow the trend to vary across provinces.

The second challenge with estimating the effect of the crisis on child outcomes is that while the crisis is exogenous, the way a household reacts to an economic shock is not. Specifically, mothers who choose to conceive during downturns may be different from mothers who choose not to conceive during this time. As a result the crisis effect may be capturing changes in the composition of women giving birth. In order to control for any compositional changes of women giving birth, I use mother fixed effects to estimate the effect of the crisis on child outcomes. Mother fixed effects will capture any time-invariant characteristics such as preferences that affect both the length of exposure to the crisis and child outcomes.

Consequently, the model I use to estimate the impact of the crisis on child outcomes is an age-fixed effect, mother fixed effect model, that controls for birth month at the province level. Length of exposure to the crisis captures the effect of the crisis on child outcomes. The mother fixed effect controls for any fixed mother, household, or community characteristics. The birth month control captures any trends in community characteristics that affect child outcomes such as improvements in technology. Finally, the age fixed effect controls for age effects and leads to the comparison of outcomes of children of the same

age but who were differentially exposed to the crisis. Thus, the error term is composed of all individual unobservable characteristics, mother time-varying unobservable characteristics, and community time-varying unobservable characteristics not captured by the time trend. As long as the variables in the error term are not related to exposure to the crisis, then the estimates of the effect of the crisis on child outcomes will be unbiased. The exact empirical specification and identification assumptions are discussed in more detail in the following section.

1.5 Empirical strategy

1.5.1 Main specification

In order to estimate the effect of the crisis on child outcomes, I define the exogenous crisis variable as the number of months exposed to the crisis. Thus, the more exposed a child is to the crisis, the larger the effect on child outcomes. For children born after March 2001, exposure is 0. As mentioned in the previous section, I use an age fixed effect-mother fixed effect model and control for linear time trends. The exact equation that I estimate is the following:

$$Y_{impta} = \beta_0 + \beta_1 S_{pt} + \beta_2 X_{impta} + \alpha_0 t + \sum_{p=1}^{5} \alpha_p t * prov_p + \delta_m + \gamma_a + \epsilon_{impta} \quad (1.4)$$

where Y_{impta} is the outcome variable, either height-for-age or cognitive test score, for child i with mother m from province p, born in birth month t and is age a. S_{pt} is the number of months a child from province p and born in month t was exposed to the crisis, and X_{impta} is a vector of individual control variables. Birth month, t, is included in the specification in order to capture linear time trends in height or cognitive development. The birth month trend is interacted with a province indicator, $prov_p$, to allow time trends to vary by province. There are 6 provinces in the sample, thus the trend for the excluded province is captured by α_0. δ_m is the mother fixed effect, and γ_a is the age fixed effect. The age fixed effect is defined as age in months at the time of the survey. The mother fixed effect controls for any endogenous response of mothers to the crisis. The mother fixed effect also controls for all fixed community level characteristics that affect health and cognitive development. Thus, the error term captures unobserved time variant mother characteristics, unobserved time variant community characteristics not capture by the trend, and unobserved individual characteristics.

Although TVIP scores are censored (the lowest score a child can obtain is 55), Ordinary Least Square (OLS) regressions are used instead of Tobit because there does not exist a sufficient statistic allowing the mother fixed effect to be conditioned out of the likelihood. If children who were exposed to the crisis are more likely to have their scores censored at 55, then OLS estimates will

be biased toward zero. Appendix A.3 shows that for models that do not use mother fixed effects, OLS estimates are slightly smaller in magnitude than Tobit estimates. Due to the fact that OLS and Tobit estimates are very similar, I go ahead and conduct all estimates using OLS keeping in mind that the estimates are lower bound estimates.

The effect of the crisis is identified by comparing height or cognitive outcomes of children of the same age but who were differentially exposed to the crisis. The mother fixed effect and birth month control forces this comparison to be made across sibling and across households that are from the same province. Table 1.8 demonstrates exactly how the effect of the crisis is identified. For clarity, estimation of child outcomes is simplified to only including the crisis exposure variable, mother fixed effects, age fixed effects and the birth month interacted with a province indicator. The example below is restricted to only one province because identification occurs by comparing households in the same province. The second to last column calculates the difference in height or cognitive outcomes between siblings, and the last column calculates the difference across households in the sibling differences. As Table 1.8 reveals, the effect of the crisis is identified by comparing the sibling difference in height or cognitive outcomes across households whose children are born the same number of months apart. There are two different avenues through which this identification occurs. The first is demonstrated by household 1 and household 2. Sibling differences of household 1 in January 2004 are compared to the sibling differences of household 2 in January 2006. Identification occurs because household 1 and household 2 have children of the same age, but in 2004 the 60 month old child from household 1 was exposed to the crisis for 18 months whereas in 2006 the 60 month old child from household 2 was exposed to the crisis for 0 months. The second way identification occurs is demonstrated with household 3 and household 4. The sibling difference in household 3 is calculated for siblings that are the same age in different survey months and then compared to the sibling difference of children of the same age in household 4. Identification occurs because siblings of household 3 are the same age in different survey waves and are differentially exposed to the crisis. To disentangle the birth month effect from the crisis effect they are compared to siblings in household 4 that are the same number of months apart as siblings in household 3. As the two examples demonstrate, a necessary condition for identifying the effect of the economic crisis is that the number of birth months apart across siblings is the same across households that are being compared. Due to the fact that it is less likely that siblings are the exact same age across surveys, identification will occur mainly through the first avenue.

The identifying assumption for β_1 to be unbiased is that in the absence of the crisis sibling differences across households whose children are born the same number of months apart would be the same. Referring to the example in Table 1.8, this means that in the absence of the crisis the sibling difference in outcomes for household 1 and household 2 would be the same. In order for this assumption to hold, time trends in height and cognitive development must

be linear. Imposing a linear relationship means that the effect of being born 24 months apart on differences in height is the same no matter when children are born. Thus, differences in sibling outcomes due to being born 24 months apart in 1999 and 2001 is the same as differences due to being born 24 months apart in 2001 and 2003. Given that the time horizon for when children were born is relatively short for this sample (1998-2005), and given that national statistics on malnutrition and infant mortality are fairly linear for short time horizons (Appendix A.4), assuming linearity in trends seems reasonable.

One potential threat, however, to the identifying assumption is a deviation from the trend that is caused by a shock other than the crisis. Specifically, β_1 will be biased if a shock that occurred after the economic crisis differentially affected one age group. If this is the case, then the difference in sibling outcomes from household 1 would no longer be the same as the sibling difference in household 2 in the absence of the 1998-2000 economic crisis. A potential confounding factor is Ecuador's cash transfer program, *Bono De Desarrollo Humano* (BDH). Households in the sample began receiving the BDH in 2004, and if the BDH had a differential impact on child outcomes with respect to age, then it could bias the estimates on the impact of the crisis. Consequently, in some specifications I also control for the BDH.

1.5.2 Heterogeneity

As mentioned in section 4, although the crisis affected all regions and households of Ecuador, there was heterogeneity in a household's ability to cope with the crisis. For households that were more able to smooth their consumption during the downturn, the impact of the crisis on child outcomes should be smaller. Although there are many strategies and institutions that help households smooth their consumption, I focus on a private institution, farming, and a public institution, early childhood nutrition programs. If farmers are net sellers, then large increases in food prices that accompanied the crisis, should actually benefit them. However, even if farmers are not net sellers, the fact that they can produce some of their own food means that when prices increase, farmers are more able to smooth their food consumption by substituting purchased food with home grown food. In order to examine the differential effect of the crisis with respect to farming households, I interact a child's exposure to the crisis with an indicator for whether the household is a farming household. I separate the sample into urban and rural areas, because of differences in farming and the crisis across these two regions. Due to the fact that all households in the data are poor, there should be little heterogeneity in terms of farm size within the farming group.

Another important institution that allows households to smooth their food and health consumption during negative economic shocks is public food and health aid [Yamano et al., 2005]. Specifically, during negative economic shocks households can smooth their consumption by substituting purchased healthcare and food for free healthcare and food. In Ecuador programs related to

early childhood health and nutrition operate through health facilities. Consequently, the closer a family is to health centers, the more access the family has to health-related social programs such as *Maternidad Gratuita, Suplementación con Micronutrientes, Programa Ampliado de Inmunizaciones (PAI),* and *Programa de combate al hambre y la desnutrición (PANN2000).* These programs provide children with free vaccinations, postnatal and prenatal care, nutritional food supplements, and iron and vitamin A supplements. In order to examine whether children with more access to these programs were more protected from the crisis, I interact a child's exposure to the crisis with the number of public health facilities in a child's parish in 1999. Again, I conduct these estimations separately for rural and urban areas because of differences in the quality and availability of health facilities across these areas.

1.5.3 Sensitive periods

Due to the fact that sensitive periods exist in a child's development, I allow exposure to the crisis to enter the estimation equation in a more flexible form. Instead of having a linear relationship between exposure and child outcomes, I break up exposure to the crisis into 6 month periods. Given that 6 month periods are relatively short, I use an indicator for being exposed during a certain period instead of a continuous variable. The new estimation equation is the following:

$$Y_{impta} = \beta_0 + \beta_1 E_{pt}^{prenatal} + \beta_2 E_{pt}^{0t5} + ... + \beta_7 E_{pt}^{30t35} + \beta_8 X_{impta} + \alpha_0 t + \sum_{p=1}^{5} \alpha_p t * prov_p + \delta_m + \gamma_a + \epsilon_{impta} \tag{1.5}$$

where E_{pt}^{0t5} is an indicator for whether a child was exposed to the crisis when they were 0 to 5 months old. Periods are broken up all the way to exposure when a child was 30 to 35 months. There are no children in the sample that were exposed to the crisis after 35 months. In addition to breaking up exposure to 6 months intervals, I allow exposure in utero, $E_{pt}^{prenatal}$, to affect child outcomes.

1.6 Results

1.6.1 Height and TVIP

Tables 1.9 and 1.10 show the results of regressing height for age z-scores or TVIP scores on the number of months exposed to the crisis (Equation 1.4). Due to the fact that the TVIP test is only conducted on children 36 months or older, the sample size for TVIP is approximately half that for height outcomes. Column 1 estimates the effect of the crisis without using mother fixed-effects and columns 2-4 include mother fixed effects. The sample size decreases when mother fixed effects are added because mother fixed effects require at least two

child observations per mother. For TVIP, mother fixed effects require at least two child observations of children older than 35 months per mother. Column 3 adds control variables, and column 4 controls for a potential confounding factor, the *Bono de Desarrollo Humano* (BDH). Control variables included in columns 3 and 4 are an indicator for a child being male, an indicator for whether the child was in the hospital in the last year, an indicator for whether a child's father lives at home, a continuous birth order variable, and 12 indicators for the month in a given year that a child was born. All columns contain age in months fixed effects and control for birth month linearly. Birth month has a value of 1 for a child born in December 2005 and goes to 99 for a child born in October 1997. Standard errors are clustered at the parish level.

The first column in Table 1.9 indicates that the economic crisis had a negative impact on children's height. The magnitude of the coefficient on the number of months exposed to the crisis decreases when mother fixed effects are included in column 2, thus suggesting that the effect of the crisis from column 1 is in part driven by the selection of women giving birth during the crisis. The coefficient then remains fairly stable after the inclusion of control variables (column 3) and the inclusion of the BDH (column 4). Using the coefficient from column 4, the results suggest that an additional month of exposure to the crisis decreased height for age z-scores by .008 standard deviations. Thus, if a child lived in the crisis for a whole year, he/she would be .1 standard deviations shorter. The average height for age z-score of children in the baseline survey was -1.34 standard deviations; therefore, one year of exposure to the crisis led to approximately a 7% reduction in a child's height for age z-score.

Results for TVIP (Table 1.10) indicate that the economic crisis also had a negative impact on cognitive development. Column 1 estimates the effect of the crisis for all children older than 35 months without using mother fixed effects. Similar to the height results, when mother fixed effects are added to the model (column 2) the coefficient decreases thus suggesting that mothers with characteristics associated with worse child outcomes are the ones more likely to have a child during the crisis. When control variables are added (columns 3 and 4) the coefficient increases slightly. Using the coefficient in column 4, the results suggest that an additional month of exposure to the crisis decreased TVIP scores by .2 points. Thus, one year living in the crisis reduced TVIP scores by approximately 2.4 points. The average TVIP score at the baseline is 86.79; therefore, one year in the crisis reduced TVIP scores by approximately 3%.

Although the results are robust to the inclusion of many controls, it is important to understand how selection bias might confound the estimates. Children in the sample are those that were born between 1998 and 2005 and survived long enough to be included in the sample or did not migrate out of the country or 6 provinces included in the sample. If children that were most affected by the crisis died and are thus not in the sample, then this would bias the results toward zero. Consequently, the results presented above would be lower bound estimates of the effect of the crisis. In order to grasp the

magnitude of this bias, I look at mother's birth history, and find that 3.3% of mothers had a child that died during the crisis. This is compared to 1.9% of mothers whose child died in non-crisis years. If we believe that mothers who never had another child and are thus not in the sample have the same probabilities of their child dying, then the extra 1.4% that died during the crisis will lead to the results being biased toward zero, though the bias is likely to be very small.

Selective migration, especially out of the country, will also potentially bias the result, however, the direction of the bias is unclear. If households that migrated out are those that were more affected by the crisis, then the estimates would be biased towards zero. However, if households that migrated are those that were less affected by the crisis, then estimates would be biased away from zero. Although I do not know the type of households that migrated, I do know that 6% of households migrated out of the country during the crisis. While the crisis led to a surge in migration, the surge remained very high up until 2004 [Bertoli et al., 2010]. The fact that migration levels remained high after the crisis is reflected in the data when I observe that only 1.5% of children's fathers at baseline migrated during the crisis, in comparison to the 1.8% of fathers that migrated in non-crisis years. Due to the fact that children had to be born between 1998 and 2005 in order to be included into the sample, there is no reason to believe that the crisis led to differential rates of migration for children born during or children born after the crisis. Thus this eases some concern on selective migration. Furthermore, the low rates of migration of fathers in this sample suggest that any bias due to migration will likely be very small.

1.6.2 Heterogeneous effects

Tables 1.11 and 1.12 reveal the heterogeneous effects of the crisis with respect to farming households and health care availability. The heterogeneous results are presented separately for rural and urban areas since farming and health care availability differ drastically among rural and urban areas. In columns 2 and 5, I investigate the differential effect with respect to any household that owns, rents or shares farms or land for farming or livestock use. In columns 3 and 6, I investigate the differential effect with respect to health care availability. Health care availability is measured by the number of health care facilities in a child's parish in 1999 divided by the parish's population.[9] This variable is multiplied by 10,000 in order to indicate how many health centers are available in a parish for every 10,000 individuals. For both height and TVIP outcomes, the effect of the crisis is large and significant for rural areas but not for urban areas (columns 1 and 4). The difference in magnitude

[9]Health facility and population data is from SIISE 4.5. Health facility data is the number of non-overnight health facilities at the parish level for 1999 and population data is population in 2001.

of the coefficient on exposure between urban and rural areas is not significant for height outcomes but it is significant at the 10% level for TVIP outcomes.[10]

The results for height outcomes (Table 1.11) reveal that children from farming households were more protected from the crisis, however, the differential effect is only significant for rural households (column 2 and column 5). For rural non-farming households, the effect of one month of exposure to the crisis on children's height is -.013 SD, while it is -.004 for rural farming households. For urban non-farming households, the effect of exposure to the crisis is negative but insignificant. Rural children with more access to health centers were also more protected from the crisis (column 3). Specifically, the effect of the crisis is -.016 for children living in parishes without health centers, and the magnitude of this effect decreases by .002 for every additional health center per 10,000 individuals. Interestingly, there is no differential effect with respect to the number of health centers on height outcomes for children in urban areas. One potential reason for the lack of effect is that the variation in the number of health centers is smaller in urban areas than rural ones. Specifically, every urban parish in the sample has at least one health center, but no parish has more than 8 per 10,000 individuals. However, in rural sample parishes 13% do not have any health facility while other rural parishes have as many as 15.4 per 10,000 individuals.

Contrary to the height outcomes, the differential effect of the crisis with respect to farming or access to health centers for TVIP outcomes is close to 0 in both rural and urban areas (Table 1.12). The difference in the heterogeneous outcomes for height and TVIP scores is most likely explained by the differences in the relative sensitivity to time and food inputs. In particular, there is reason to believe that children's cognition is more sensitive to time inputs than food inputs [Grantham-McGregor et al., 1997]. Thus, while access to health centers and farming helped protect against decreases in food consumption during the crisis, they did not help protect against decreases in a mother's time input, and as a result, they did not help protect children's cognition.

One issue that arises when analyzing the differential effect with respect to farming households or health center availability is that there could have been selective movement into and out of farming or across parishes with more health centers during the crisis. In Ecuador most of the internal migration occurs across province or across cantons with very little additional migration occurring across parishes [WorldBank, 2004]. Although I do not have information on migration across parishes, I do know that 6.3% of children born during the crisis migrated across cantons compared to 5.4% of children born after the crisis. Thus it is unlikely that the extra .9% of migration that occurred during the crisis is driving the results. Furthermore, it is unlikely that movement into and out of farming is driving the results since in the sample 76% of farming

[10]T-tests were conducted to check whether the effect of the crisis was different in rural and urban areas. Results show that the difference in magnitude of the coefficient is not significant for height outcomes and marginally significant for TVIP outcomes (p-value=.62 for height outcomes; p-value=.06 for TVIP outcomes).

households at baseline owned their land. While I do not have information as to when farmers purchased their land, there are national statistics (ECV 1999) that show that only 3.5% of agricultural plots had been bought or sold within the last 12 months during the crisis. Thus the percentage of households buying and selling their land is likely very low.

Even though it is unlikely that movement into and out of farming or across parishes is driving the results, I conduct the following robustness checks to alleviate some of the concerns mentioned above. First I rerun the specifications on heterogeneity, but restrict the sample to those that have not migrated across cantons, since it is likely that these children have also not migrated across parishes. Second, I investigate the heterogeneous effects with respect to farming households that own land because movement into and out of this category of farming households is more unlikely. As the tables in Appendix A.5 reveal, the results do not change if I restrict the sample to those that did not migrate, although, the interpretation of the results changes to analyzing the heterogeneous effects of the crisis for those that did not migrate. The lack of change in the results when the sample is restricted to those that did not migrate suggests that migration is not driving the results in Tables 1.11 and 1.12. Furthermore, focusing on farmers who own land and thus are less likely to move into and out of farming (columns 3 and 6 in Appendix A.5), reveal the same results as those on farming.

Another concern with estimating the differential effect with respect to farming is that farming might be capturing differential effects of wealth. Specifically, farmers might be wealthier than non-farmers and thus not as affected by the crisis. While I do not have information on income or expenditures for the sample, I do have information on a household's assets, and therefore, create an asset index using Principal Component Analysis. A t-test of equality of means on farmer's versus non-farmer's asset-index reveals that farmers are not wealthier than non-farmers, and thus wealth is not driving the results. Similarly, the differential effect with respect to health center availability could be capturing a parish's remoteness. However, statistics from the 2001 census actually reveal an inverse relationship of health care availability and electrification (an indicator for remoteness). The reason for this inverse relationship is that while small rural parishes have a lower percentage of households with electricity, they have relatively high levels of access to health centers because of the lower number of people per health center. Thus, it is not the case that rural parishes with less access to health facilities are also more remote than those with more access.

1.6.3 Sensitive periods

Due to the fact that the effect of exposure might vary by age, I break up exposure to the crisis into age intervals. Instead of a continuous exposure variable, I use an indicator for whether a child was exposed to the crisis during their prenatal period, 0 to 5 months, 6 to 11 months, 12 to 17 months, 18 to 23

months, 24 to 29 months, and 30 to 35 months. Table 1.13 reveals the results from estimating equation 1.5 for height and TVIP outcomes. One concern with dividing exposure into time intervals is that exposure during later time periods might be picking up length of exposure and not sensitive periods. Thus, in columns 2 and 4 I also control for length of exposure. As Table 1.13 indicates, exposure to the crisis only had a significant negative impact on height for age z-scores during 12-17 months of age. The coefficient on months 12-17 in column 2 is significantly different at the 10% level from the coefficients on all the other time intervals.[11] Therefore, these results imply that 12-17 months is a sensitive period for children's height.

For TVIP scores, exposure to the crisis was significant if exposed when a child was 6-11 months and 18-29 months. Controlling for length of exposure increases the magnitude of the coefficients for these time intervals. The coefficient on months 30-35 is also large and negative but never significant. One potential reason for why it is not significant is that only one percent of the sample was exposed to the crisis when they were 30-35 months. Wald tests of equality of the coefficient on months 24-29 in column 2 with each of the other coefficients reveals that the magnitude of the coefficient on months 24-29 is significantly different from that on prenatal and months 0-5, but it is not significantly different from the rest of the coefficients. In addition, the coefficients on 6-11, 18-23, and 30-35 are significantly different from that on months 0-5 at the 10% level. While not as clear cut as the height results, these results imply that a child's sensitive period for cognitive development begins at around 6 months and lasts until at least 35 months.

Although these results are very interesting, one should interpret them with caution. One concern is that exposure at 24 to 35 months is picking up length of exposure and not critical periods, even after controlling for length of exposure. However, if this was the case, then we would expect the results for height for age to also be negative and significant for this period. Another concern is that these results are sensitive to how I define the timing of the crisis. While the size of the coefficient is sensitive to how the timing of the crisis is defined, for height the coefficient on 12-17 is always the largest and significantly different from some of the other coefficients, and for TVIP the coefficients on 18-29 are always the largest in magnitude and significantly different from another coefficient.

Even though one must interpret the results with caution, there is reassurance in that the results are similar to what others find. Specifically the results on height are similar to what Hoddinott and Kinsey [2001] find in Zimbabwe with respect to a drought having a negative impact on child growth only for children 12 to 24 months old. Maccini and Yang [2009] also analyze the effect of rain at different time periods and find that rainfall in utero had no effects on later health and education outcomes. Glewwe and King [2001] investigate

[11] Wald tests of equality were conducted on the coefficient for months 12-17 and each of the other coefficients.

the issue of timing more precisely by dividing up a child's critical period into 6 months intervals. They find no effect of prices on IQ scores for the first 0-18 month intervals, but they do find a negative and significant effect of prices on IQ scores for the 18-24 month interval.

My results and the ones mentioned above suggest that a sensitive period for height is in a child's second year of life and for language acquisition beginning at 6 months and lasting until 35 months. This is consistent with the assertion that a child is the most vulnerable in their weaning and post-weaning period. Moreover, the results for TVIP scores are consistent with the fact that brain development for receptive language peaks at 8 months [Thompson and Nelson, 2001] and then children undergo a vocabulary growth spurt at the end of their second year [Dapretto and Bjork, 2000, Fischer, 1987]. Due to the fact that TVIP tests a child's vocabulary and receptive language, it seems reasonable that a sensitive period for a child's language acquisition coincides with moments of rapid growth in these areas.

1.6.4 Pathways

The three main pathways through which the crisis led to a deterioration of child health and cognition were through reduced consumption of health and cognitive inputs, reduced time inputs from parents, and a deterioration of the health environment. Unfortunately, the data does not allow for a comprehensive examination of the mechanisms driving the negative crisis effect. The survey does, however, ask a few retrospective questions related to post natal care. I examine the impact of the crisis on these measures to assess some potential pathways.

Information on the number of months a child breastfed and the immunizations a child received was collected for all children in the sample. In addition to this information, the data contain information on the place of birth (home, private clinic, or public clinic) and whether the mother received post-natal care for the last child born in 2003 and 2005. Although a mother receiving post-natal care is not a direct health input for a child, I believe it serves as a good proxy for whether the child received post natal care. Given that child vaccines and postnatal care are free in public clinics in Ecuador, any changes in these variables are more likely to indicate changes in time inputs (time it takes to take a child to get vaccinated) and/or the health environment (vaccine availability in the parish). Given that breastfeeding is time intensive, any changes in breastfeeding patterns resulting from the crisis will provide insight on changes in a mother's time input. The information on a child's place of birth is with respect to being born in a private clinic, public health facility, or at home. Observing whether there are any changes in the frequency of being born in a private clinic will indicate whether there were changes in health inputs that were due to the negative income effect of the crisis.

In order to investigate the impact of the crisis on potential pathways, I estimate equation 1.4; however, instead of using length of exposure to the

crisis, the independent variable is either an indicator for whether the child was born during the crisis or a continuous variable for the number of months exposed to the crisis during a child's first 18 months. The latter variable is used to estimate the effect of the crisis on the number of months a child breastfed, an indicator for whether a child had received at least 3 vaccines, and an indicator for whether a mother received post-natal care.[12] Exposure to the crisis is limited to a child's first 18 months because breastfeeding, vaccinations, and postnatal care occur during this time frame. Extending exposure to a longer time frame does not change the results, but it does not make sense for this analysis since postnatal behavior occurs in a child's first 18 months. An indicator for being born during the crisis is used to analyze the effect of the crisis on a child's place of birth because length of exposure after a child is born does not matter.

Table 1.14 reveals that the crisis had no impact on immunizations and a mother's postnatal care. One potential reason why the crisis had no effect on children's immunizations is that national immunization campaigns sponsored in part by international institutions were conducted in 1999, and thus, any potential negative impact of the crisis on vaccinations would have been mitigated.[13] The crisis did have a negative and marginally significant effect on the number of months a child breastfed. Specifically, for every month in the crisis, breastfeeding was reduced by .02 months; thus if a child was in the crisis for a whole year, they would have .24 months less of breastfeeding. Being born during the crisis also decreased the probability of being born in a private facility by 7%. Instead of being born in a private clinic or at home, households were 8% more likely to use free public health facilities if born during the crisis. Although Table 1.14 just skims the surface of potential pathways through which the crisis operated, the results suggests that the crisis decreased children's time inputs and quality of health inputs, both of which led to worse health and cognitive outcomes.

1.7 Conclusion

The results in the section above indicate that Ecuador's 1998-2000 economic crisis led to a deterioration in child health and cognitive development that persisted 3-5 years later. Specifically, if a child lived in the crisis for a full year then he/she would be .1 standard deviations shorter and have TVIP scores that were 2.4 points lower. The effect of the crisis on children's outcomes was concentrated in rural areas, with rural non-farmers and those without access

[12]The reason for using an indicator for whether or not a child received at least 3 vaccines is that there are 3 vaccines that were recommended by the Ministerio de Salud Pública in 2003 for children under the age of 12 months (BCG, OPT, Pentavalent). Campos, C. (2009) Introducción nuevas vacunas en el esquema de vacunación del Programa Amplaido Inmunaciones. Information on recommended vaccination age found at the following URL: http://www.bvv.sld.cu/ibv/?pg=cip&r=es&country=ec

[13]http://www.paho.org/english/ad/fch/im/sne2104.pdf

to health centers suffering the most. For these sub populations, one year living in the crisis led to a .16 and .19 decrease in height for age z-scores respectively. Interestingly, farming and access to health services had no differential effects on TVIP outcomes. The most likely reason for this discrepancy is the relative sensitivity of cognitive outcomes to time versus material health inputs.

While the main specifications assume a linear relationship between exposure to the crisis and child outcomes, I also break up exposure into specific time intervals, and thereby, explore if there are sensitive periods in a child's development. Findings indicate that children were more sensitive to exposure to the crisis when they were 12-17 months old for height and 6-35 months for cognitive development. Lastly, I investigate potential pathways through which the crisis affected child outcomes. Although only skimming the surface on potential pathways, the results suggest that the negative effect of the crisis was due to reductions in time inputs and substitutions towards cheaper health inputs.

One way to assess the magnitude of the effect of Ecuador's crisis is to compare it with results from other studies. In Russia, Stillman and Thomas [2008] find that a 10% increase in expenditure is associated with a .01 increase in children's height for age z-scores. On the other hand, Alderman et al. [2006] find that a drought in Zimbabwe is associated with a .58 decrease in children's height for age z-score. Thus, the effect of one year of exposure to the crisis on height is larger in magnitude than the effect of a 10% decrease in expenditure in Russia but smaller than the effect of a drought in Zimbabwe. For TVIP there are no comparable studies on the effect of a negative income shock; however, Macours et al. [2008] find that a conditional cash transfer (CCT) program leads to a .22 SD increase in TVIP score. Thus the effect of one year of exposure to the crisis on TVIP scores of 2.4 points (or approximately .16 SD) is slightly smaller in magnitude than the effect of a CCT program.

It is also valuable to consider the implication of these results on later education and labor market outcomes. Using the mothers in the sample, I find that a 1 cm increase in height is associated with an additional .2 years of schooling. Assuming that the effects of the crisis persist until adulthood, then the .1 SD (or approximately .35 cm) decrease in height z-scores that was due to the crisis will be associated with a decrease of .07 years of schooling. For TVIP scores, a 1 point increase in a mother's TVIP is associated with an increase of .1 years of schooling. Again, assuming that the effects of the crisis persist until adulthood, then the 2.4 point decrease in TVIP scores that was due to the crisis will be associated with a decrease of .24 years of schooling. One quarter of a year less of schooling is a sizeable amount, and given that that the returns to education in Ecuador are approximately 8% [Hall and Patrinos, 2006], this implies a decrease in earnings of approximately 2%. Although my calculations for long term impacts on schooling and earning are based on many assumptions and should be interpreted with caution, they are very similar to what Maccini and Yang [2009] find are the effects of early life rainfall on school outcomes in Indonesia and what Hoddinott and Kinsey [2001] predict would

be the consequence of a drought on earnings in Zimbabwe.

It is important to emphasize that the results of the impact of the crisis are specific to the population studied: young families who are in the bottom two poverty quintiles. While these results cannot be generalized to Ecuador's population as a whole, they do provide insight for a large, vulnerable segment of the population. These findings are important because although Ecuador's economy began to recover after 2000, the negative impact of the crisis on child development persisted 3-5 years later. Moreover, the impact of the crisis will most likely persist into the future with exposed children having worse education and labor market outcomes. Fortunately, some households were less affected by the crisis. Successful strategies and institutions that helped protect children from the crisis were farming and early childhood nutrition programs. Given that exposure to the crisis mattered after 6 months of age, interventions that aim to protect children from negative income shocks should target children in their weaning and post-weaning period.

1.8 Figures and tables

Figure 1.1: Yearly inflation rate

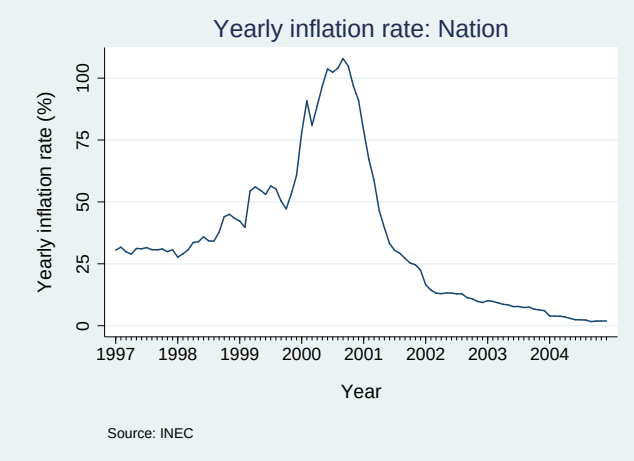


Figure 1.2: Aggregate household consumption

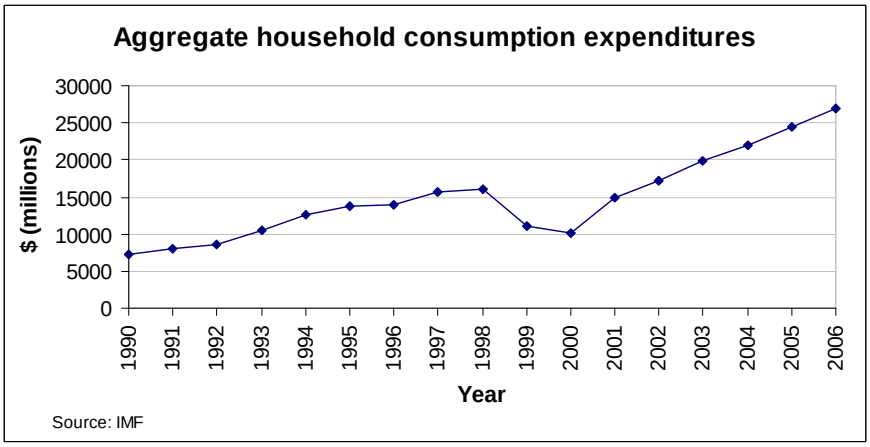
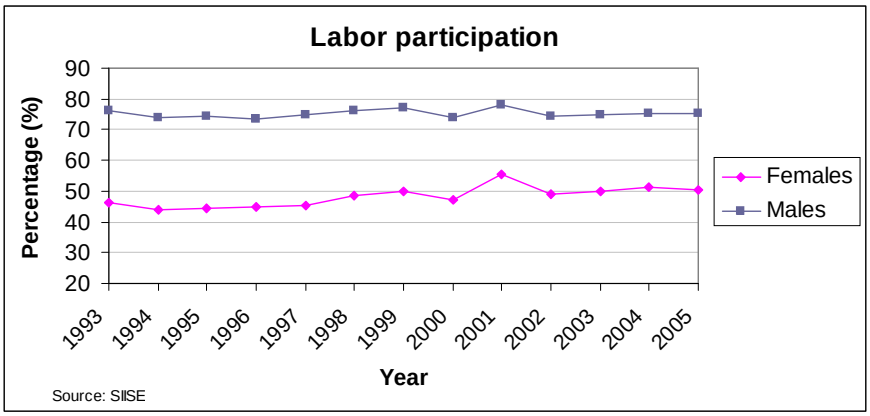
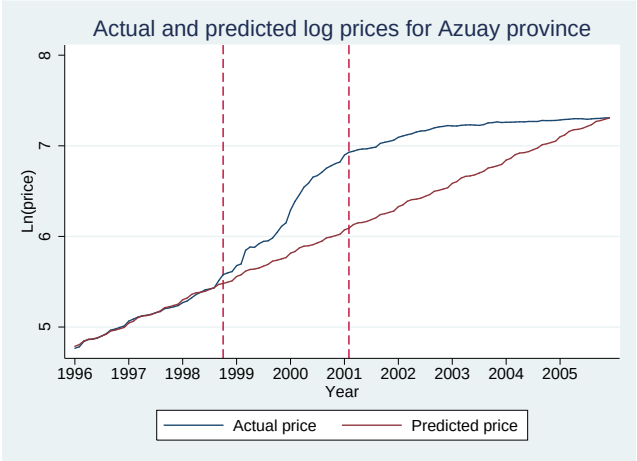


Figure 1.3: Urban labor participation rate



Labor participation is calculated as the ratio of the number of individuals 12 years old and older who are economically active, to the total number of individuals 12 years old and older.

Figure 1.4: Actual and predicted prices



Source: INEC, CPI index

Tables

Table 1.1: Poverty indicators

Poverty (consumption based)						
	Headcount			**Severity**		
	1995	1998	1999	1995	1998	1999
National	34	46	56	5	9	11
Urban	19	30	42	2	4	6
Rural	56	69	77	10	16	20
Extreme poverty (consumption based)						
	Headcount			**Severity**		
	1995	1998	1999	1995	1998	1999
National	12	17	21	1	2	3
Urban	4	7	9	0	1	1
Rural	23	30	38	3	4	6
Chronic Malnutrition (% stunted)						
	1998	1999	2000	2004		
National	26	27	26	21		
Urban	24	18	19	15		
Rural	33	38	36	31		

Source: Poverty indicators from Parandekar et al. [2002] and malnutrition indicators from SIISE 4.5

Table 1.2: Occupation Rates

	1995	1998	1999
National	65	66	69
Female	50	51	57
Male	81	81	83
Urban	62	62	64
Rural	70	72	77

Source: Authors calculations from ECV. Occupation rate refers to individuals over 12 years old with a job over total number of individuals over 12 years old.

Table 1.3: Social spending (millions $) in nominal terms

Year	Health	Other *	Total	GDP	%GDP
1996	198	778	976	21,267.9	4.6%
1997	191	707	898	23,635.6	3.8%
1998	183	729	912	23,255.1	3.9%
1999	119	480	599	16,674.5	3.6%
2000	103	364	467	15,933.7	2.9%
2001	189	760	949	21,249.6	4.5%
2002	268	887	1,155	24,899.5	4.6%

Source: Informe de Desarrollo Social 2007

*Other includes spending on education, social inclusion, work, and urban and housing development

Table 1.4: Time line

	Crisis years					Baseline (Oct 2003-Sept 2004)		Follow-up 1 (Sept 2005-Jan 2006)	
	1998	1999	2000	2001	2002	2003	2004	2005	2006
Age	0	1	2	3	4	5	-	-	-
		0	1	2	3	4	5	-	-
			0	1	2	3	4	5	-
				0	1	2	3	4	5
					0	1	2	3	4
						0	1	2	3
							0	1	2
								0	1
									0

Table 1.5: Monthly price inflation by province

	Non crisis mean	Crisis mean	Crisis months
Azuay	1.27	5.08	Oct 1998-Feb 2001
Del Oro	1.09	4.51	May 1998-Dec 2000
Esmeraldas	1.13	4.98	Sep 1998-Dec 2000
Loja	1.2	5.09	Aug 1998-Oct 2000
Los Rios	1.25	5.41	Feb 1998-Jun 2000
Pichincha	1.07	4.62	Sep 1998-Mar 2001

Source:Prices from INEC

Table 1.6: Descriptive statistics from Baseline

	Mean	SD	N
Child's height for age z-score	-1.34	1.40	6599
Child's standardized TVIP score	86.79	16.82	2745
Child's age in months	34.93	17.64	6977
Child is male	0.51	0.50	6977
Child has no health insurance	0.95	0.23	6977
Mother's years of education	7.82	3.41	6962
Mother's age	24.42	5.44	6977
Mother is married	0.41	0.49	6977
Children 0-5 years in household	1.94	0.81	6977
Father lives at home	0.74	0.44	6976
Farming household	0.23	0.42	6970
Health centers per 10,000 people	2.38	2.14	6977
Number of months exposed to the crisis	6.37	9.25	6977

*Sample consists of all children 1-71 months at baseline that were also in follow-up.

Table 1.7: Mean height for age z-score by age group and year

	2003	2005	Difference
Children 36-71	$\overline{H}$= -1.4	$\overline{H}$= -1.32	-.08***
	(.02)	(.02)	(.03)
Children 1-35	$\overline{H}$= -1.28	$\overline{H}$= -1.29	.01
	(.03)	(.03)	(.04)
Difference	-.12***	-.03	-.09*
	(.03)	(.04)	(.05)

Standard errors in parenthesis

Table 1.8: Identification
Simplifying problem to: $Y_{impta} = \beta_1 S_{pt} + \alpha_0 t + \sum_{p=1}^{5} \alpha_p t * prov_p + \delta_m + \gamma_a$

HH	Child	Survey month	Birth month	Age in months	♯ of months in the crisis	δ_m	γ_a	$\beta_1 S_{pt}$	$\alpha_0 t + \sum_{p=1}^{5} \alpha_p t * prov_p$	Sibling Difference $(Y_{1mpta} - Y_{2mpta})$	HH diff-in-diff
1	1	Jan 04	84 (Jan 99)	60	18	δ_1	γ_{60}	$\beta_1 18$	$\alpha_0 84$		
1	2	Jan 04	48 (Jan 02)	24	0	δ_1	γ_{24}	0	$\alpha_0 48$	$=\gamma_{60} - \gamma_{24} + \alpha_0(84-48) + \beta_1 18$	$= \beta_1 18$
2	1	Jan 06	60 (Jan 01)	60	0	δ_2	γ_{60}	0	$\alpha_0 60$		
2	2	Jan 06	24 (Jan 04)	24	0	δ_2	γ_{24}	0	$\alpha_0 24$	$=\gamma_{60} - \gamma_{24} + \alpha_0(60-24)$	
3	1	Jan 04	84 (Jan 99)	60	18	δ_3	γ_{60}	$\beta_1 18$	$\alpha_0 84$		
3	2	Jan 06	60 (Jan 01)	60	0	δ_3	γ_{60}	0	$\alpha_0 60$	$=\alpha_0(84-60) + \beta_1 18$	$= \beta_1 18$
4	1	Jan 04	48 (Jan 02)	24	0	δ_4	γ_{24}	0	$\alpha_0 48$		
4	2	Jan 06	24 (Jan 04)	24	0	δ_4	γ_{24}	0	$\alpha_0 24$	$=\alpha_0(48-24)$	

Table 1.9: Effect of the crisis on height for age z-score

	(1)	(2)	(3)	(4)
Number of months exposed to crisis	-0.012***	-0.006*	-0.006**	-0.008**
	(0.003)	(0.003)	(0.003)	(0.004)
Male			-0.081***	-0.082***
			(0.028)	(0.028)
Child was in the hospital in the last year			-0.088	-0.090
			(0.060)	(0.060)
Father lives at home			0.152***	0.152***
			(0.056)	(0.056)
Birth order			-0.088*	-0.086*
			(0.051)	(0.052)
Cash transfer program (BDH)				0.057
				(0.105)
Cash transfer program*Age				-0.002
				(0.002)
Constant	-0.332	0.153	0.277	0.253
	(0.204)	(0.160)	(0.256)	(0.261)
Mother fixed effects	no	Yes	Yes	Yes
Birth month trend*Province	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Observations	12,920	12,342	12,342	12,342
Adjusted R2	0.115	0.193	0.197	0.197

Robust standard errors clustered at the parish level. *** p<0.01, ** p<0.05, * p<0.1 All specifications contain age-in-month indicators and specifications 3 & 4 contain 12 indicators indicating the month in a year in which a child was born. Children included are those who have non-missing data on all the control variables and are between the ages of 1-71 months.

Table 1.10: Effect of the crisis on TVIP scores

	(1)	(2)	(3)	(4)
Number of months exposed to crisis	-0.27***	-0.16**	-0.17**	-0.20**
	(0.05)	(0.07)	(0.07)	(0.08)
Male			1.83***	1.85***
			(0.49)	(0.49)
Child was in the hospital in the last year			-0.66	-0.67
			(1.10)	(1.10)
Father lives at home			0.85	0.88
			(1.15)	(1.15)
Birth order			1.90	1.87
			(1.26)	(1.25)
Cash transfer program (BDH)				0.45
				(1.42)
Cash transfer program*Age				-0.04
				(0.07)
Constant	91.72***	85.61***	75.91***	75.74***
	(4.01)	(3.33)	(6.59)	(7.21)
Mother fixed effects	no	Yes	Yes	Yes
Birth month trend*Province	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Observations	6,750	4,536	4,536	4,536
Adjusted R2	0.156	0.163	0.168	0.168

Robust standard errors clustered at the parish level. *** p<0.01, ** p<0.05, * p<0.1 Specifications 3 and 4 contain 12 indicators indicating the month in a year in which a child was born. In the 4th column, BDH is interacted with age that is centered at 36 months. Children included are those who have non-missing data on all the control variables and are between the ages of 36-71 months.

Table 1.11: Heterogeneous effects of the crisis: Height for age z-score

	Rural population			Urban population		
	(1)	(2)	(3)	(4)	(5)	(6)
Number of months exposed to the crisis	-0.010**	-0.013***	-0.016***	-0.006	-0.007	-0.004
	(0.004)	(0.004)	(0.005)	(0.007)	(0.007)	(0.008)
Farmer *Number of months exposed to the crisis		0.009**			0.008	
		(0.004)			(0.008)	
Number of health facilities in child's parish in 1999 (per 10,000 individuals)* Number of months exposed to crisis			0.002**			-0.001
			(0.001)			(0.003)
Constant	0.373	0.390	0.360	0.108	0.126	0.098
	(0.361)	(0.356)	(0.360)	(0.362)	(0.358)	(0.357)
Mother fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth month trend*Province	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,096	8,084	8,096	4,246	4,246	4,246
Adjusted R2	0.209	0.209	0.210	0.203	0.203	0.203

Robust standard errors clustered at the parish level. *** p<0.01, ** p<0.05, * p<0.1 All specification contain a full set of controls which includes indicator for whether a child is male, a child's birth order, indicator for whether a child was in the hospital in the last year, indicator for whether a child's father lives at home, 12 indicators indicating the month within a year in which a child was born, indicator for whether a household received the BDH and an interaction of BDH with age. Children included are those who have non-missing data on all the control variables and are between the ages of 1-71 months.

Table 1.12: Heterogeneous effect of the crisis: TVIP score

	Rural population			Urban population		
	(1)	(2)	(3)	(4)	(5)	(6)
Number of months exposed to the crisis	-0.29***	-0.29***	-0.26**	0.00	0.00	-0.01
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.13)
Farmer *Number of months exposed to the crisis		-0.01			0.01	
		(0.08)			(0.09)	
Number of health facilities in child's parish in 1999 (per 10,000 individuals)* Number of months exposed to crisis			-0.01			0.01
			(0.02)			(0.05)
Constant	69.78***	68.53***	69.08***	92.51***	92.61***	92.79***
	(9.11)	(9.97)	(9.84)	(10.06)	(10.29)	(10.39)
Mother fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth month trend*Province	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,048	3,042	3,048	1,488	1,488	1,488
Adjusted R2	0.171	0.171	0.171	0.192	0.192	0.192

Robust standard errors clustered at the parish level. *** p<0.01, ** p<0.05, * p<0.1 All specification contain a full set of controls which includes indicator for whether a child is male, a child's birth order, indicator for whether a child was in the hospital in the last year, indicator for whether a child's father lives at home, 12 indicators indicating the month within a year in which a child was born, indicator for whether a household received the BDH and an interaction of BDH with age. Children included are those who have non-missing data on all the control variables and are between the ages of 36-71 months.

Table 1.13: Effect of the crisis by year of exposure

	Height for age z-scores		TVIP	
Indicator =1 if exposed to the crisis, prenatal	0.05	0.05	-1.73	-0.99
	(0.06)	(0.06)	(1.33)	(1.34)
Indicator =1 if exposed to the crisis, 0-5 months old	-0.03	-0.01	1.25	0.67
	(0.06)	(0.08)	(1.26)	(1.31)
Indicator =1 if exposed to the crisis, 6-11 months old	0.08	0.09	-2.32*	-3.81*
	(0.08)	(0.12)	(1.35)	(2.03)
Indicator =1 if exposed to the crisis, 12 - 17months old	-0.22***	-0.20*	0.14	-1.25
	(0.08)	(0.11)	(1.36)	(1.88)
Indicator =1 if exposed to the crisis, 18-23 months old	-0.01	0.01	-2.63*	-4.07**
	(0.08)	(0.12)	(1.40)	(1.90)
Indicator =1 if exposed to the crisis, 24-29 months old	0.06	0.07	-4.34**	-5.55**
	(0.09)	(0.13)	(1.79)	(2.20)
Indicator =1 if exposed to the crisis, 30 - 35 months old	0.10	0.11	-4.35	-4.22
	(0.14)	(0.15)	(2.84)	(2.81)
Number of months living in the crisis		-0.00		0.31
		(0.02)		(0.27)
Constant	0.26	0.26	70.82***	73.93***
	(0.26)	(0.26)	(8.76)	(8.70)
Mother fixed effects	Yes	Yes	Yes	Yes
Birth month trend*Province	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Number of observations	12,342	12,342	4,536	4,536
Adjusted R2	0.198	0.198	0.175	0.176

Robust standard errors clustered at the parish level. *** p<0.01, ** p<0.05, * p<0.1 All specifications contain controls for a child's sex, whether the child's father lived at home, whether the child was hospitalized in the last year, birth order, 12 indicators indicating the month within a year in which a child was born, an indicator for whether a household received the BDH and an interaction of BDH with age. Children included are those who have non-missing data on all the control variables and are between the ages of 1-71 months for height outcomes and 36-71 months for TVIP outcomes.

Table 1.14: Mechanisms

Independent variable: Number of months exposed to the crisis: 0-18 months	β (SE)	N	Mean or %
Child has at least 3 vaccinations	0.00 (0.00)	11,644	93.2%
Number of months a child was breast fed	-0.02* (0.01)	12,322	5.5
Mother received post-natal care	0.00 (0.00)	9,831	39.7%
Independent variable: Indicator for whether a child was born during the crisis	β (SE)	N	Mean or %
Child was born in private clinic	-0.07* (0.04)	9,833	21.1%
Child was born at home	0.01 (0.03)	9,833	26.7%
Child was born in a public clinic	0.08* (0.04)	9,833	49.1%

Robust standard errors clustered at the parish level. *** p<0.01, ** p<0.05, * p<0.1 All specifications contain age-in-month indicators, linear control for birth month, and mother fixed effects. Each row is represents a new regression.

1.9 Appendix

A.1 Exposure and month of birth

Exposure to the crisis is determined by month of birth and province. Crisis months for each province are defined by Table 1.5. The table below indicates how birth month and province determine a child's exposure to the crisis for two provinces, Los Rios and Azuay.

Table 1.15: Exposure to crisis and birth month

Birth month	Birth month label	Exposure for Los Rios	Exposure Azuay
Dec 05	1	0 months	0 months
.	.	.	.
Jan 05	12	0 months	0 months
.	.	.	.
Jan 04	24	0 months	0 months
.	.	.	.
Jan 03	36	0 months	0 months
.	.	.	.
Jan 02	48	0 months	0 months
.	.	.	.
Feb 01	59	0 months	1 months
Jan 01	60	0 months	2 months
.	.	.	.
Ju1 00	66	0 months	8 months
Jun 00	67	1 months	9 months
.	.	.	.
Jan 00	72	6 months	14 months
Jan 99	84	18 months	26 months
Dec 98	85	19 months	27 months
Nov 98	86	20 months	28 months
Oct 98	87	21 months	29 months
Sept 98	88	22 months	29 months
.	.	.	
Feb 98	95	29 months	29 months
Jan 98	96	29 months	29 months
Dec 97	97	29 months	29 months
Nov 97	98	29 months	29 months
Oct 97	99	29 months	29 months

A.2 Dynamic optimization problem

The following theoretical model for the demand for health draws heavily from models outlined in the papers by Currie [2000], Blau et al. [1996] and MaCurdy [1999]. Given that health is a dynamic process, the model is a dynamic optimization problem where households maximize their expected present discounted value of lifetime utility. Although the model is for health, the same conclusions hold for cognition. A household's expected present discounted value of life time welfare is

$$E_0 \sum_{t=0}^{T} (\frac{1}{1+\rho})^t U(C_t, L_t, H_t; \sigma, \omega_t) \tag{1.6}$$

In other words, life time utility is the expected discounted sum of time-specific utility. Utility in each period is a function of goods consumed by a household in period t, C_t, the health stock of children, H_t, leisure time, L_t and vectors of permanent and time-varying taste shifter variables, σ and ω_t. ρ is the discount factor and E is the expectation operator. The two assumptions made are that preferences are inter-temporally separable and that each utility function is increasing and concave in its arguments. Households choose their consumption, leisure, and health inputs in order to maximize their expected utility subject to the following constraints:

$$A_{t+1} = (1 + r_t)A_t + y_t + w_t l_t - C_t - P_t N_t \tag{1.7}$$

$$H_{t+1} = h(H_t, N_t, K_t; D_t, \upsilon, \mu_t) \tag{1.8}$$

$$L_t + l_t + K_t = 1 \tag{1.9}$$

$$A_T \geq 0 \tag{1.10}$$

Equation 1.7 is the budget constraint, where A are assets, r is the interest rate, y is non-labor income, w is the wage, l is the number of hours of paid labor, and P_t is the price of health goods. Equation 1.8 is a health production function that states that inputs and health in time t affect health in the next period. Thus health in period $t+1$ is a function of current health, H_t, material health inputs, N_t, a household's time spent on health production, K_t, the health environment, D_t, and permanent and time varying productivity shifters,υ and μ_t. υ will encompass all time invariant child and household characteristics that affect production such as gender and μ_t captures time variant characteristics such as age. Equation 1.9 is the time constraint and equation 1.10 is the terminal wealth condition.

In order to solve the maximization problem, dynamic programing methods are used. Specifically, Bellman's Principle of Optimality leads to the following value function and first order conditions:

$$V(A_t, H_t) = max_{\{C_t, L_t, K_t, N_t\}} \left[U(C_t, L_t, H_t; \sigma, \omega_t) + \frac{1}{1+\rho} E_t \left\{ V\left(A_{t+1}, H_{t+1}\right) \right\} \right] \tag{1.11}$$

$$\frac{\partial U(C_t, L_t, H_t; \sigma, \omega_t)}{\partial C_t} = \lambda_{1t} \tag{1.12}$$

$$\frac{\partial U(C_t, L_t, H_t; \sigma, \omega_t)}{\partial L_t} = \lambda_{1t} w_t \tag{1.13}$$

$$\lambda_{2t} \frac{\partial h(H_t, N_t, K_t; D_t, \upsilon, \mu_t)}{\partial N_t} = \lambda_{1t} P_t \tag{1.14}$$

$$\lambda_{2t} \frac{\partial h(H_t, N_t, K_t; D_t, \upsilon, \mu_t)}{\partial K_t} = \lambda_{1t} w \tag{1.15}$$

$$\lambda_{1t} = \frac{1}{1+\rho} E_t[\lambda_{1t+1} * (1 + r_t)] \tag{1.16}$$

$$\lambda_{2t} = \frac{\partial U(C_t, L_t, H_t; \sigma, \omega_t)}{\partial H_t} + \frac{1}{1+\rho} E_t[\lambda_{2t+1} * \frac{\partial H_{t+1}}{\partial H_t}] \tag{1.17}$$

Where λ_{1t} and λ_{2t} are the lagrange multipliers attached to the budget constraint and health production function, and thus they represent the shadow price of wealth and health. The last two equations use the following identities: $\lambda_{1t} = \frac{\partial V_t}{\partial A_t}$ and $\lambda_{2t} = \frac{\partial V_t}{\partial H_t}$. The first order conditions imply the following demand equations[14]:

$$N_t^*, C_t^*, K_t^*, L_t^* = f(P_t, w_t, y_t, r_t, \rho, \lambda_{1t}, \lambda_{2t}, H_t, D_t, \upsilon, \mu_t, \sigma, \omega_t)$$

Where λ_{1t} and λ_{2t} are the lagrange multipliers attached to the budget constraint and health production function, and thus they represent the shadow price of wealth and health. These equations reveal that current period decisions depend on expected future variables only through the shadow values λ_{1t} and λ_{2t}. In other words, decisions in period t, are related to variables outside period t, only through $\lambda_{1t,}\lambda_{2t}$. Substituting the solutions of N_t and K_t back into the health production function leads to the following dynamic health function:[15]

$$H_{t+1} = h^{'}(H_t, P_t, w_t, y_t, r_t, \rho, \lambda_{1t}, \lambda_{2t}, D_t, \upsilon, \mu_t, \sigma, \omega_t) \tag{1.18}$$

[14]Due to the fact that utility is strictly concave, the first order conditions can be inverted to give consumption and labor equations [Browning et al., 1985]

[15]Frisch demand functions are also referred to as marginal-utility-of-wealth constant demand functions. They allow for changes in relative prices to effect inter-temporal substitution in consumption [Kim, 1993].

λ_{1t} and λ_{2t} are functions of every variable relevant to decision making in a lifetime context. In other words, they depend on not only current prices, wages, assets, non-labor income, interest rate, and variables affecting taste and productivity, but also on the moments of the distribution of future wages, prices, non labor income, interest rate, and variables affecting taste and productivity. [MaCurdy, 1985]. Assuming that the moments of the distribution of future variables are determine by current and past variables, and substituting in the determinants of H_t lead to the following reduced form equation for health:

$$H_{t+1} = h(A_0, H_0, P_i, w_i, y_i, r_i, \rho, D_i, \upsilon, \mu_i, \sigma, \omega_i) \; where \; i = 1....t$$

Where A_0 and H_0 are initial assets and health that are assumed to be given. Thus health in time *t+1* is a function of all current and past prices, wages, non-wage incomes, interest rates, health environments, taste and productivity shifters, and a function of initial assets, initial health, and the discount rate.

A.3 Tobit versus OLS

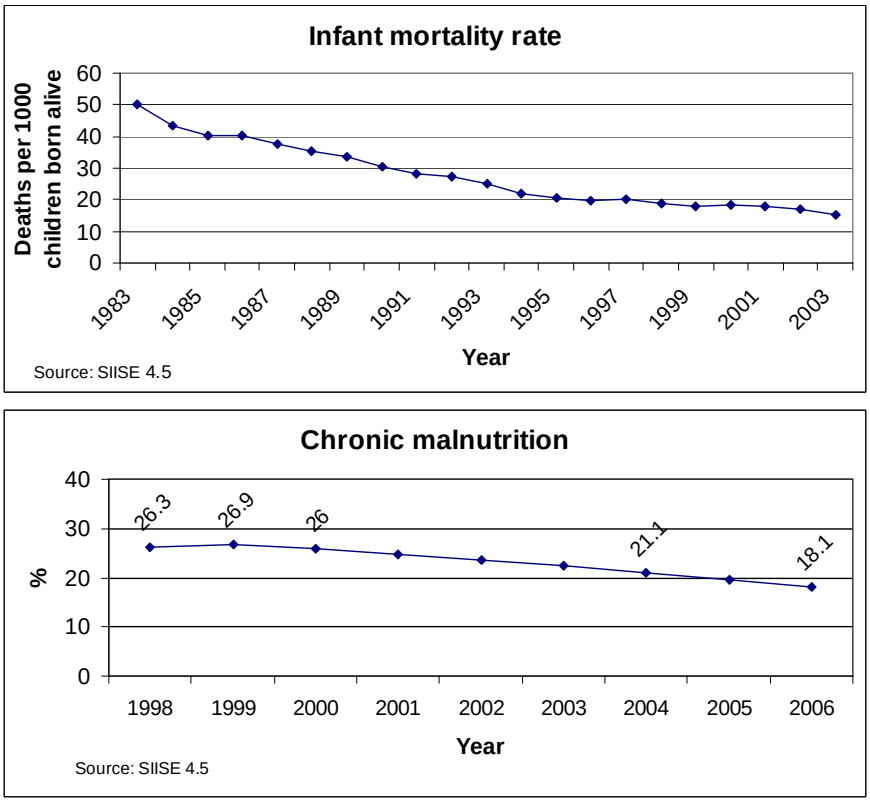
Table 1.16: OLS and Tobit regressions for TVIP

	OLS		TOBIT	
	(1)	(2)	(3)	(4)
Number of months exposed to crisis	-0.27***	-0.24***	-0.28***	-0.25***
	(0.05)	(0.05)	(0.06)	(0.06)
Male		1.05***		1.07***
		(0.35)		(0.35)
Child was in the hospital in the last year		0.76		0.75
		(1.05)		(1.06)
Father lives at home		0.36		0.35
		(0.48)		(0.48)
Birth order		-1.81***		-1.86***
		(0.44)		(0.44)
Constant	91.72***	96.45***	91.40***	94.41***
	(4.01)	(4.17)	(4.06)	(4.07)
Mother fixed effects	no	no	no	no
Birth month trend*Province	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Observations	6,750	6,750	6,750	6,750
Adjusted R2	0.156	0.160	0.021	0.022

Robust standard errors clustered at the parish level. *** p<0.01, ** p<0.05, * p<0.1 Specifications 2 and 4 contain 12 indicators indicating the month in a year in which a child was born. Children included are those who have non-missing data on all the control variables and are between the ages of 36-71 months.

A.4. Trends in health

Figure 1.5: Trends in health in Ecuador



Chronic malnutrition refers to children with height for age z-scores that are less than -2. Malnutrition data was not available for 2001, 2002, 2003, and 2005, so author inputted values for these years.

A.5 Heterogeneous effect

Table 1.17: Heterogeneous effect of the crisis for children that did not migrate: Height for age-z-score

	Rural population				Urban population			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of months exposed to the crisis	-0.011**	-0.013***	-0.013***	-0.015***	-0.005	-0.006	-0.006	-0.003
	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.007)	(0.006)	(0.008)
Farmer *Number of months exposed to the crisis		0.010**				0.008		
		(0.004)				(0.008)		
Farmer owner *Number of months exposed to the crisis			0.010**				0.014	
			(0.005)				(0.011)	
Number of health facilities in child's parish in 1999 (per 10,000 individuals)* Number of months exposed to crisis				0.002**				-0.001
				(0.001)				(0.002)
Constant	0.400	0.420	0.423	0.390	0.133	0.150	0.160	0.123
	(0.385)	(0.380)	(0.380)	(0.384)	(0.373)	(0.369)	(0.370)	(0.371)
Mother fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth month trend*Province	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,594	7,582	7,582	7,594	4,059	4,059	4,059	4,059
Adjusted R2	0.211	0.210	0.210	0.211	0.209	0.210	0.210	0.209

Robust standard errors clustered at the parish level. *** p<0.01, ** p<0.05, * p<0.1 All specification contain a full set of controls which includes indicator for whether a child is male, a child's birth order, indicator for whether a child was in the hospital in the last year, indicator for whether a child's father lives at home, 12 indicators indicating the month within a year in which a child was born, indicator for whether a household received the BDH and an interaction of BDH with age. Children included are those who have non-missing data on all the control variables and are between the ages of 1-71 months.

Table 1.18: Heterogeneous effect of the crisis for children that did not migrate: TVIP score

	Rural population				Urban population			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of months exposed to the crisis	-0.27**	-0.27**	-0.27**	-0.24**	-0.11	-0.11	-0.11	-0.12
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.13)
Farmer *Number of months exposed to the crisis		-0.03				0.01		
		(0.08)				(0.10)		
Farmer owner *Number of months exposed to the crisis			-0.02				-0.05	
			(0.09)				(0.11)	
Number of health facilities in child's parish in 1999 (per 10,000 individuals)* Number of months exposed to crisis				-0.01				0.01
				(0.02)				(0.05)
Constant	65.81***	67.34***	67.40***	66.40***	78.95***	79.04***	86.89***	79.14***
	(10.71)	(9.69)	(9.71)	(10.57)	(10.57)	(10.99)	(11.06)	(10.97)
Mother fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth month trend*Province	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,886	2,880	2,880	2,886	1,396	1,396	1,396	1,396
Adjusted R2	0.165	0.165	0.165	0.165	0.195	0.194	0.194	0.194

Robust standard errors clustered at the parish level. *** p<0.01, ** p<0.05, * p<0.1 All specification contain a full set of controls which includes indicator for whether a child is male, a child's birth order, indicator for whether a child was in the hospital in the last year, indicator for whether a child's father lives at home, 12 indicators indicating the month within a year in which a child was born, indicator for whether a household received the BDH and an interaction of BDH with age. Children included are those who have non-missing data on all the control variables and are between the ages of 36-71 months.

Chapter 2

Effect of Ecuador's cash transfer program (Bono de Desarrollo Humano) on child development in infants and toddlers: A randomized effectiveness trial[1]

Lia Fernald[2] and Melissa Hidrobo

2.1 Introduction

Poverty has negative consequences for young children in terms of immediate and longer term outcomes, with poor children at risk for worse physical, cognitive and behavioral outcomes in both the developed [Ackerman et al., 2004, Brooks-Gunn and Duncan, 1997, Duncan and Brooks-Gunn, 1997, Evans and English, 2002, Owens and Shaw, 2003] and developing world [Engle and Black, 2008]. These negative consequences of poverty are due not just to the lack of household economic resources, but also to the range of risk factors and exposures associated with poverty, and to the accumulation of these exposures [Evans and Kim, 2010]. Compared with children from wealthier households, children living in poverty are exposed to more dangerous neighborhoods,

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greater household crowding, increased pollution, less responsive parenting, and less stimulating learning environments [Bolig et al., 1999, Bradley et al., 2001, Brooks-Gunn et al., 1995, Evans, 2004]. These conditions are exacerbated in the developing world where children are exposed to a greater number of risk factors due to a lack of basic infrastructure and weak social services [Walker et al., 2007]. Poor children in developing countries also suffer from inadequate nutrition and subsequent growth retardation, and thus, do not achieve their developmental potential [Grantham-McGregor et al., 2007]. Children who are unable to fulfill their physical and cognitive growth potential perform more poorly in school and subsequently have lower incomes, fueling the intergenerational transmission of poverty [Alderman, 2010], and perpetuating a cycle of poverty and poor development [Conger and Donnellan, 2007].

Although there are a substantial number of longitudinal studies that show that poverty is negatively associated with cognitive outcomes [Aughinbaugh and Gittleman, 2003, Taylor et al., 2004, Blau, 1999] and behavior [Morris and Gennetian, 2003], continuing into adulthood [Evans and Schamberg, 2009], there are difficulties with trying to claim causal effects. While there are many reasons to speculate that increased resources to a household will improve child outcomes, it is also plausible that families that are able to earn more money are also more likely to raise children with better language or physical development [Conger and Donnellan, 2007]. Our paper adds to the existing literature by using a randomized trial to isolate the causal effects of income on child development and asking whether an exogenous increase in household income can improve developmental outcomes for very young children (12-35 months old).

The theoretical framework guiding our work specifies that children's development is shaped by positive and negative environmental factors that interact dynamically with biological systems. During the first five years of life, children lay the groundwork for lifelong development. Several kinds of development during this period are malleable and thus can be enhanced by interventions affecting the child and/or the child's environment [Shonkoff and Phillips, 2000]. The first two years of life, in particular, represent a period of great vulnerability for children, especially those in developing countries who are at risk of irreversible linear growth faltering during this time frame [Victora et al., 2010]. During these first two years, rapid development occurs in several cognitive processes, including language development, and in the second year of life a "vocabulary explosion" often occurs [Ganger and Brent, 2004, Kuhl and Rivera-Gaxiola, 2008]. Thus, interventions in very early childhood are critically important not just for practical issues of feasibility and cost-effectiveness [Engle et al., 2007], but also because of the plasticity of physiologic development and the capacity of the child to benefit from interventions during this period [Nelson, 2000].

Designing programs that sustainably improve child development has been a long-term challenge for policy makers. The interventions that seem to have the most promise for improving child development are those such as intensive

parenting programs or high quality day care or preschool, particularly those targeted at the youngest or most disadvantaged children [Engle et al., 2007]. Conditional cash transfer (CCT) programs - now in place in many regions of the world [Fiszbein et al., 2009] - are a popular approach to long term poverty-alleviation, but do not target child development directly. Instead, CCTs operate by giving cash payments to families only if they comply with a set of certain requirements relating to health and education of family members [de Janvry and Sadoulet, 2006]. CCTs use cash transfers both as a mechanism to allow parents to provide for their children and as an incentive for parents to invest in their children's well-being [Das et al., 2005]. Due to evidence that money spent by mothers, in contrast to fathers, is often more child-centric [Thomas, 1993, 1990], many CCTs target the transfer to mothers. Although cash transfer programs have been embraced by the development community [de Janvry and Sadoulet, 2006], they have also been criticized for reasons relating to lack of potential sustainability, lack of a broader focus on rural development, and limited larger scale effects on a country's political economy [Grimes and Wangnerud, 2010, Handa and Davis, 2006, Popay, 2008, Shibuya, 2008].

There are several cash transfer programs already in place in Latin America, including Oportunidades (previously Progresa) in Mexico, *Bolsa Alimentação* in Brazil, *Red de Protección Social* in Nicaragua, *Programa de Asignación Familiar* in Honduras, *Familias en Acción* in Columbia, *Subsidio Unico Familiar* in Chile, and Program of Advancement through Health and Education in Jamaica [Rawlings and Rubio, 2005]. These programs require mandatory attendance at preventative health care services, health and nutrition education sessions designed to promote positive behaviors, and school attendance for school-age children [Lagarde et al., 2007]. Some CCT programs also include the distribution of fortified food and/or micronutrient supplements for vulnerable subgroups in the population (e.g. pregnant women and young children), conditional on the same factors as the cash transfer.

It is exactly the conditional nature of the benefits that separates CCTs from other cash or in-kind distribution programs, such as unconditional cash transfer (UCT) programs, which are those where families receive cash benefits because the household falls below a certain income cut-off or lives within a geographically targeted region [Barrientos and DeJong, 2006]. UCTs assume that parents are income-constrained, and simply do not have the money to meet their families' most pressing needs (e.g. nutritious food or medical treatment). Increasing their purchasing power permits parents to choose what goods they want to buy, and allows them to make choices about the quantity and quality of their purchases. Given that UCTs do not monitor the behavior of households, these programs are operationally less complex and easier for governments to implement than CCTs. A critical research question remains as to the effectiveness of the less studied UCTs in comparison with CCTs [Sherr et al., 2009].

There are several potential mechanisms that could explain why cash trans-

fer programs could improve outcomes for children [Engle and Black, 2008]. Having greater financial resources may allow parents to provide a better environment for their children through improvements in the home (e.g. floor and ceiling, electricity, or water supply) [Guo and Harris, 2000, Yeung et al., 2002] or through the purchase of goods that could directly influence child growth and development (e.g. more expensive nutritious foods, health care, books, or toys). Although public health care may be free for young children, transportation to and from clinics is not; thus, the cash transferred to the household could be used to pay for a bus or taxi. Furthermore, while parasite treatments or nutritional supplements may be provided for free at some health clinics, mothers in developing countries often report that they have to purchase parasite treatments or other medicines. Another possible mechanism by which cash transfers could be linked to improvements in child development is that income could indirectly improve the psychological well-being of family members, through reductions in subjective feelings of financial strain and deprivation [Mistry et al., 2004], which could then be associated with improvements in child well-being and achievement [McLeod, 1990]. Since poverty affects the ways in which parents monitor their children, provide stimulation for their children, and respond to their needs [Brooks-Gunn and Duncan, 1997], increasing access to economic resources may allow parents to be more responsive, warm and consistent [Wachs et al., 2009].

In 2003, Ecuador created a large social assistance program called *Bono de Desarrollo Humano* (BDH), which consisted of giving a cash transfer to low-income mothers. Mothers of children 0-16 years qualified for the program if they were in the bottom two poverty quintiles according to the Sistema de Selección de Beneficiarios (SELBEN). The BDH was initially designed as a CCT requiring certain behaviors of household members in order to receive program benefits. The required behaviors included taking children to preventive health check-ups and requiring a minimum level of attendance at school for school-aged children. The BDH, however, is actually more similar to a UCT due to the lack of verification of compliance with the conditions set out in the program's mandate. Another feature of the BDH is that the transfer size does not depend on the number of children in the household. The size of the transfer in 2006 was \$15/month, which was approximately 6-10% of an average household's pre-transfer expenditure for households in the bottom two poverty quintiles.

Analysis of the effects of CCT programs in Latin America have shown improvements in height among children in rural [Behrman and Hoddinott, 2005, Fernald et al., 2009, Rivera et al., 2004] and urban Mexico [Leroy et al., 2008], in Nicaragua [Maluccio and Flores, 2005], and in Colombia [Attanasio et al., 2005] with no significant effects on height in programs from Brazil [Morris et al., 2004] or Honduras [Moore, 2008]. Studies of UCTs in developing countries are less common, although in South Africa, a UCT was shown to improve child height [Aguero et al., 2010]. Cash transfers in the form of pensions have also been shown to improve anthropometric measures of children but only if

received by an adult female [Duflo, 2003].

We identified only three evaluations of cash transfer programs that included assessments of cognitive or language function in children: Mexico [Fernald et al., 2008, 2009], Nicaragua [Macours et al., 2008], and Ecuador [Paxson and Schady, 2010]. The Ecuadorian study of the BDH revealed very small, positive effects of the program on a composite variable including several development outcomes (including growth, cognition and language), but only for older rural children (36-72 months) from the lowest income quartile [Paxson and Schady, 2010]. In Nicaragua, there were significant program effects of the CCT program, *Atención a Crisis*, on cognitive and language performance in children (0-83 months), with the strongest findings relating to language development in older children [Macours et al., 2008]. In Mexico, there were no main program effects of *Oportunidades* on any cognitive or language measure, but there was a significant, positive, dose-response association between the amount of cash transferred to a household and children's cognitive and language development [Fernald et al., 2008, 2009].

Given the limited number of analyses of cash transfer programs that have included measures of early child development, the first objective of this paper was to use a randomized effectiveness trial in Ecuador to address the question of whether very young children (12-35 months old) benefit in terms of health outcomes or language development if their families receive a cash transfer. Th data set used here is unique because it includes health and development outcomes for some of the youngest and thus most vulnerable children in the population, and because it is one of the first, rigorous examinations of a UCT program in contrast to a CCT.

A second objective of the paper was to separately examine the effects of treatment in rural and urban areas, due to the fact that rural areas differ greatly from urban areas in terms of take-up rates, timing of the intervention, access to health facilities, and livelihoods. It is well-established that determinants of health outcomes, e.g. income/wealth, assets, parental education, and health care infrastructure, are distributed differently in urban and rural settings, and that children in urban areas tend to be healthier as a consequence [Smith et al., 2005, Van de Poel et al., 2007, 2009]. Ecuador is no exception; rural households in Ecuador are poorer than urban households and children from rural areas have higher rates of malnutrition [INEC, 2007]. Consequently, we explored whether the effects of the BDH differed between urban and rural households.

Our third objective was to examine potential pathways through which the BDH could be operating. The pathways we chose to explore were primarily related to cognitive/language inputs, health inputs, and quality of parenting [Engle and Black, 2008, Walker et al., 2007]. As described above, greater financial resources could allow parents to purchase goods that directly influence language and health development (e.g. animal-source-foods, books, or toys); or could improve parenting behavior due to less stress or more optimism about the future [Wachs et al., 2009].

2.2 Methods

2.2.1 Study design

The BDH was rolled-out slowly which allowed two separate randomized evaluations to be conducted: a health and development evaluation (used by Paxson and Schady, 2010 for older children) and an education evaluation (used in Schady and Araujo, 2008). The BDH evaluation was approved by the Princeton University Human Subjects Review Board and the Center for the Protection of Human Subjects at the University of California, Berkeley. All study participants signed an informed consent declaration before being included in the study.

The study reported here uses the data collected for the health evaluation where 378 parishes (the smallest administrative unit in Ecuador) from six provinces (three coastal and three highland provinces) were randomized into treatment and comparison groups. Parishes were stratified into rural and urban groups and from each group, treatment and comparison parishes were randomly selected. In total, 118 parishes were selected for the experiment: 51 in the rural treatment group, 26 in the rural comparison group, 28 parishes in the urban treatment group, and 13 in the urban comparison group (Fig.2.1). Initially the study had been designed to have two treatment groups, one that received the cash transfer if they met the required health and schooling conditions and another that received an unconditional cash transfer. Given that the required health and schooling conditions were never implemented, the two treatment groups were combined into one, resulting in a larger treatment group.

The BDH became available to rural treatment parishes in June 2004 and to urban treatment parishes in November 2004. The transfers were not available to comparison parishes until after 2006. Transfers were administered by the Government of Ecuador and distributed through a large network of private banks (Banred) and the National Agricultural Bank (Banco Nacional de Fomento) [Schady and Araujo, 2008]. According to survey response data, there was very little contamination of the intervention: take-up of the BDH program was 73% for the treatment group and 3% for the comparison group.

Baseline (October 2003-March 2004) and follow-up (September 2005-January 2006) surveys were conducted by the World Bank and the Government of Ecuador. In order to be included into the survey sample, households had to meet the following criteria: have at least one preschool age child, have no children older than 6 years old, be eligible for the cash transfer program, and not have been recipients of a previous welfare program (Bono Solidario). Households in treatment and comparison parishes that met the criteria above were then randomly sampled for the evaluation. The total sample at baseline consisted of 3426 households and 5547 children below the age of 72 months. For this study we focused only on children younger than 36 months at follow-up.

2.2.2 Data

The data collected for the baseline and follow-up surveys included information on household characteristics, the health status and health care usage of children and their mothers, language abilities of children, and anthropometric measures. While anthropometric measures were collected at baseline and follow-up, language tests for children under 36 months were only conducted at follow-up.

Primary outcome measures

Language outcomes. Our primary outcome measure of interest was the child's score on the Fundación MacArthur Inventorio del Desarrollo de Habilidades Comunicativas - Breve (IDHC-B), the Spanish version of the short form of the MacArthur-Bates Communicative Development Inventory (CDI) [Jackson-Maldonado et al., 2003]. The IDHC-B assessment measures the early language skills of children between the ages of 12-35 months using parental report. The Spanish long-form version of this measure was adapted, not translated, from the English versions. Words were added that were linguistically- and culturally-appropriate to Spanish speakers and word classes were adapted to be language-specific. The Spanish long-form of this assessment has been shown to be a valid measure of expressive vocabulary and early language milestones in young children [Jackson-Maldonado et al., 1993, Marchman and Martine-Sussmann, 2002, Thal et al., 2000], and has been widely used [Conboy and Thal, 2006, Mariscal et al., 2007, Vagh et al., 2009]. The version of the IDHC-B used in Ecuador was based on a version of the IDHC-B that had been developed and validated in Mexico [Jackson-Maldonado et al., under review], with some local adaptations to words made as necessary during piloting and pre-testing. IDHC-B scores ranged from 0-100 with 0 indicating that a child had not said any word on the checklist and 100 indicating that a child had said every word on the list. Another outcome measure of interest was an indicator for whether a child had begun to combine words into short sentences. This indicator was constructed by assigning the value of one to children whose mothers reported that they "sometimes" or "frequently" combined two or more words and a value of 0 to children who had "never" combined words.

Health outcomes. Height was measured using stadiometers and then converted to age and sex adjusted z-scores using WHO standards. Height-for-age z-scores (HAZ) for children less than 24 months were based on length, and for children 24 months and older they were based on height. Children with HAZ scores less than 6 or greater than 6 were converted to missing (n =2 observations). Hemoglobin concentrations were obtained using finger-prick blood draws and were adjusted for elevation using standard procedures [CDC, 1989].

Other outcome variables

We examined several pathways through which the BDH could affect language and/or health outcomes in very young children, and the pathways we chose to explore were theoretically justified because they were primarily related to cognitive/language inputs, health inputs, or parenting quality. To assess cognitive/language inputs, dichotomous variables were created that indicated whether a household owned a story book; whether a child was bought a toy in the last 6 months; or whether a child attended day care. For the examination of health inputs, dichotomous variables were created that indicated whether a child had received a parasite treatment in the last 12 months; whether a child received iron or vitamin A supplements in the last 6 months; or whether a child had a visit to a health center during which the child's growth was recorded and monitored. Due to the lack of food expenditure data or detailed food intake data, a food index was constructed using the first component of a Principal Component Analysis (PCA). PCA is a statistical technique that transforms a large collection of correlated variables into a smaller number of components, which are linear weighted combinations of the initial variables. In general, the first principal component, which explains the largest amount of variability in the data, is an adequate measure for consumption [O'Donnell et al., 2008]. Variables included in the PCA were indicators for whether the child ate any of a list of 11 specific food items (liver, chicken, pasta and/or bread, spinach and/or chard, carrots, citrus fruits, non-citrus fruits, ice cream and/or soda, potato chips, cookies and/or crackers, candy) in the last 7 days. As a measure of parental responsiveness, we used a self-reported measure of the mother's depressive symptoms, because maternal depression is a well-known risk factor for poor outcomes in children [Wachs et al., 2009]. Depressive symptoms were assessed using the Center for Epidemiologic Studies Depression Scale (CESD), a 20-item questionnaire (scores range from 0 to 60) [Radloff, 1977] designed to assess the intensity of depressive symptoms in the past seven days (Cronbach's alpha 0.86). We also used two subscales related to harsh parenting of the Home Observation for Measurement of the Environment Inventory (HOME) [Bradley and Caldwell, 1984], which included 11 items where interviewers assessed a mother's interactions with her child during the interview (Cronbach's alpha 0.80). With the exception of CES-D, all variables included in the analysis were associated ($p < 0.10$) with the language development score, height-for-age z-score, or hemoglobin levels of the children in the sample.

2.2.3 Statistical analyses

For the analysis described in this paper, we focused on children 12-35 months old during the follow-up because our language variable of interest, the IDHC-B score, was only assessed for children in this age range. All statistical analyses were performed using STATA 10, and in all the analyses, we clustered the standard errors at the parish level.

We first examined the validity of the randomization of the experiment by comparing the means of the treatment and control groups for a wide variety of variables at baseline using clustered t-tests or chi-squared tests. To estimate the treatment effect of the BDH we took advantage of the randomized roll-out of the program and conducted an intent-to-treat analysis. This approach avoids any bias that might occur due to selection into or out of the program [Montori and Guyatt, 2001]. The treatment indicator took a value of 1 if a child lived in a parish that belonged to the treatment group and a value of 0 if a child lived in a parish that belonged to the control group. For the continuous outcome variables (IDHC-B, HAZ, and hemoglobin concentrations) we used ordinary least squares regressions, and for the dichotomous outcome variable (the indicator representing whether the child had started combining words) we used probit regressions. Given random assignment, the inclusion of baseline characteristics was not necessary to obtain unbiased estimates of the parameters. In order to increase the precision of our estimates, however, we included controls for a child's age and sex; a mother's age, years of education, marital status and an indicator for whether the mother spoke an indigenous language; a household's asset index and the number of children 0-5 years old living in the household. Although we conducted all our estimates with and without control variables, and our results were robust to either specification, we report only the estimates that included control variables since those were more precisely estimated. Based on our a priori assumption that there would be differences in program effects in rural and urban areas, we split the sample and conducted tests of difference. We also examined whether results differed according to belief of conditionality (i.e. whether the participant believed that the BDH was a CCT or a UCT).

To avoid potential endogeneity, we also used an intent-to-treat approach to explore potential pathways through which the BDH could affect children's development. Specifically, we investigated whether the treatment coefficient was significant in regressions where potential mechanistic factors were the outcome variables. We focused on theoretically determined factors that could provide increased cognitive/language stimulation such as books, toys, or childcare; improved health due to parasite treatments, growth monitoring, vitamin supplements or food intake; and/or improved quality of parenting as measured by the HOME or CES-D. Ordinary least square regressions were conducted for the continuous outcome variables and probit regressions were conducted for the dichotomous variables.

2.3 Results

2.3.1 Sample size and composition

There were no differences between children who were in the follow-up survey and those who were not (Table 2.1). Attrition was not significantly associated

with treatment assignment ($\beta = 0.02$, p = 0.42). In addition to the 697 children who were born before the baseline and included in the follow-up, 499 children were born after the baseline survey to mothers who had participated in the baseline survey, and these children were added to the sample. Consequently, at the follow-up there were 1196 (n = 797 in the treatment group and n = 399 in the control group) children 12-35 months old (Fig. 2.1). Of these children, 1192 had complete data for the IDHC-B test, 1074 had height measurements, and 922 had hemoglobin measurements.

2.3.2 Comparison of treatment and control groups at baseline

There were no significant differences between the treatment and control groups with respect to mother or child baseline characteristics (Table 2.2), indicating that the original randomization of treatment and control groups was successful. Mothers in the sample were young and had completed approximately seven years of formal schooling. Three percent of mothers spoke an indigenous language, and two-thirds of this group also spoke Spanish. The percent of children at baseline who owned a book was low (14%) and the mean hemoglobin level was below cut-offs for anemia (WHO [2008] threshold to define anemia is 11 g/dl). These statistics reflect the fact that households in the sample were poorer and younger than national averages due to the inclusion criteria.

2.3.3 Effects of treatment (BDH) on language, growth and hemoglobin

The treatment effect on all child outcomes (combining words, IDHC-B, HAZ, and hemoglobin levels) for the total sample was nonsignificant (Table 2.3). In rural areas, however, there was a significant treatment effect for the measure of language development (IDHC-B), ($\beta = 5.24$, 95% CI = 1.28-9.20, p = 0.01), and a significant increase in the probability that a child was combining words ($\beta = 0.14$, 95% CI = 0.02-0.27, p = 0.03). For HAZ and hemoglobin outcomes, the treatment effects were not significant in rural or urban areas. The effect of the BDH on height-for-age was significant, however, for children whose mothers believed that receiving the benefits of the program was conditional on taking their children to health centers; the results did not differ according to belief of conditionality for any other outcomes.

2.3.4 Effect of treatment (BDH) on potential mechanisms

Participation in the BDH for the whole sample was not significantly associated with the probability of a child being bought a toy in the last six months, attending day care, or owning a children's book (Table 2.4). There was, however, a significant treatment effect in the rural sample on the probability of a child

being bought a toy in the last 6 months (β = 0.09, 95% CI =0.00 to 0.17, p = 0.05). Similarly, the BDH did not significantly increase the probability of receiving parasite treatments, vitamin supplements, or growth controls in the sample as a whole. There was, however, a significant treatment effect in the rural sample on the probability of having received a vitamin supplement (β = 0.11, 95% CI = 0.03 to 0.20, p = 0.01). The BDH had no significant effects on maternal depressive symptoms or on parental harshness.

2.4 Discussion

For infants and toddlers living in rural areas, being randomized to receive the BDH program in very early childhood was associated with a significantly greater number of spoken words in infants and toddlers and a greater probability that a child was combining words; there were no significant effects on language for children in urban areas. Compared with children in control areas, rural children in treatment areas were more likely to have received vitamin A or iron supplementation in the past six months and more likely to have been bought a toy in past six months; these results were not significant in urban areas. There were no effects of the BDH program on height-for-age z-score or hemoglobin concentration in rural or urban areas.

This is the first paper reporting on the effects of Ecuador's welfare program on infant and toddler growth and language development, and is unique because it is one of the only unconditional cash transfer programs that has been evaluated with a rigorous, randomized design. There are, however, several potential limitations to the reported analyses. First, given the inclusion criteria for the survey (e.g. having at least one preschool child and no children over 6 years old), the households in the sample were not representative of all BDH households. Second, we are using maternal report of speech and not directly measuring language production by toddlers. Consequently, the measure for language development may have been subject to reporting bias and measurement error. Third, we are lacking detailed information about potential pathways through which the BDH program could affect outcomes in children. Changes throughout development result from multi-directional interactions between biological factors (genes, brain growth, neuromuscular maturation), and environmental influences (parent-child relationships, community characteristics, cultural norms) over time, and we only have crude measures of some of these variables [Gottlieb, 1991, Shonkoff and Phillips, 2000].

When comparing the BDH to other cash transfer programs, it is important to emphasize the differences in design and implementation of the programs. The BDH program had the lowest cash transfer amount (6-10% of baseline income) when compared with Mexico's *Oportunidades* (~21.8%) or Nicaragua's *Red de Protección Social* (~29.3%) [Fiszbein et al., 2009]. Furthermore, the BDH was considered to be conditional on health care and education requirements by only one quarter of the participants, and unconditional by the re-

mainder of the participants, whereas everyone involved in the programs in Mexico and Nicaragua knew that those programs were conditional on mandatory behavior change. When we analyzed whether the effect of the BDH varied by belief of conditionality, we found that for height-for-age, the effect of the BDH was significant for children whose mother believed it was conditional on taking them to health centers. Thus, it is possible that cash transferred to households living in poverty in Ecuador could have also shown greater effects on multiple domains of development if the cash transfer amount had been larger, or if the receipt of the cash had been linked to positive health behaviors, as found in Mexico [Fernald et al., 2008, 2009] and Nicaragua [Macours et al., 2008].

One possible explanation for why our findings were significant in rural areas and not urban areas is that more mothers in rural areas have little or no schooling in comparison with urban areas (69% v. 52%). Children in Mexico's CCT program who benefitted from early randomization to the program were those whose mothers had received no formal education [Fernald et al., 2009], and these results are consistent with others who had found program effects only in children of uneducated mothers [Leroy et al., 2008, Rivera et al., 2004]. To test this hypothesis we split the sample into rural and urban groups and within these groups we investigated whether there was a differential treatment effect with respect to mother's education. We found that in rural areas the treatment effect on IDHC-B scores ($\beta = 6.93$, $p < 0.01$) and the probability of combining words ($\beta = 0.14$, $p = 0.01$) was large and significant for children whose mothers had less than 7 years of schooling.

Another possible explanation for why our results were only significant for rural children is that program take-up was higher in rural areas (85% for rural areas v. 59% for urban areas). Households in rural areas began receiving the BDH five months before urban areas and thus had more time to adjust or adapt to the program. Households in rural areas were also poorer (asset index of -0.53 for rural areas versus 0.64 for urban areas), and therefore, they could have been in greater need of the transfer. As a result of the differences in timing and take-up rates, the mean accumulated amount that households in treatment parishes had received from the BDH was $336 in rural areas and only $225 in urban areas.

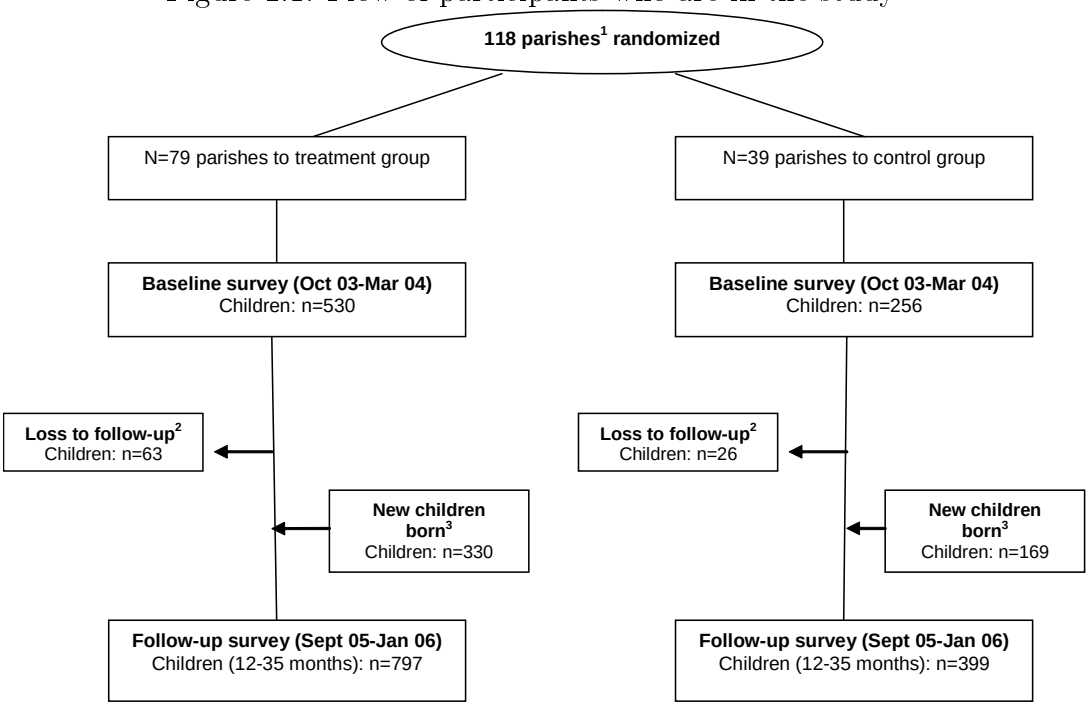
Child development results from a dynamic interplay between biological and environmental factors, which makes it very challenging to isolate what mechanisms could explain our findings. We do not have detailed information on home improvements, purchased inputs for health, or cognitive inputs/stimulation, which limits our ability to make claims relating to the mechanisms by which Ecuador's BDH could influence child language. We do, however, have some information on children's health care use, a rough measure of food consumed, and a few measures of cognitive/ language inputs. Our results suggest that parents of children in rural areas were more likely to have ensured that their children received vitamin A or iron supplementation and more likely to have bought their child a toy, which suggests mechanisms by which the program

could be functioning. We did not find any effect of the program on food consumption, which could be one explanation for why we did not find any effects of the BDH on children's height-forage z-score or hemoglobin levels.

In sum, our analyses support the notion that Ecuador's unconditional cash transfer program, Bono de Desarrollo Humano had significant benefits for very young, rural children in terms of language development, but not for height or hemoglobin and not for urban children. It is possible that the effects of the BDH on language, growth and hemoglobin would have been stronger if the cash were linked to positive health behaviors as it is in CCT programs, if it provided a greater amount of cash to households with young children, or if it were enhanced with more direct benefits for children. A review of studies from countries throughout the world has found that children benefit directly in terms of early child development from a range of interventions, but that interventions with an educational or stimulation component had larger cognitive effects than cash transfer or nutrition-only programs [Nores and Barnett, 2009]. Another previous review has also found that comprehensive programs (including nutrition, health, and parenting components) have the greatest potential for improving child development outcomes [Engle and Black, 2008]. Taken together, these reviews suggest that Ecuador's BDH could have larger effects on children's language and other development domains if it was expanded from being simply a cash transfer program, to including a greater focus on direct child stimulation through parent classes or through high quality day care opportunities.

2.5 Figures and tables

Figure 2.1: Flow of participants who are in the study



Participants in study are those who are 12-35 months at the follow-up. Although the evaluation collected data on children 0-72 months at baseline, our study focuses on children 12-35 months at follow-up due to the availability of language measures (IDHC-B) in this age range.
1 Parishes are the smallest administrative unit in Ecuador.
2 Loss due to enumerator not being able to administer survey to household, or due to child within a household no longer living there.
3 New children that were born after the baseline or became part of the household after the baseline were added to the follow-up survey.

Tables

Table 2.1: Mean (SD) or % (n) from baseline (2003-2004) data by attrition.

	Total (N=786)[1]	Included at follow-up (N=697)[1]	Lost at follow-up (N=89)[1]	P-value
Mother and household characteristics				
Mother's age	22.8 (3.8)	22.8 (3.9)	22.3 (3.1)	0.26
Mother's completed schooling (grades)	7.0 (2.9)	7.0 (2.8)	7.1 (3.3)	0.92
Mother lives with a partner	83% (650)	83% (579)	80% (71)	0.50
Mother speaks indigenous language	2% (19)	2% (17)	2% (2)	0.96
Mother's adjusted hemoglobin level[2]	11.4 (1.5)	11.5 (1.5)	11.3 (1.5)	0.46
Harsh parenting HOME score (0-11 scale)	2.4 (2.3)	2.4 (2.3)	2.5 (2.2)	0.80
Asset Index[3]	0.0 (2.3)	0.1 (2.3)	-0.2 (2.3)	0.60
Number of children 0-5 years old	2.4 (0.7)	2.4 (0.7)	2.5 (0.8)	0.28
Urban[4]	48% (374)	47% (328)	52% (46)	0.41
Child characteristics				
Child's age in months	6.5 (4.2)	6.6 (4.2)	5.7 (3.6)	0.10
Child is male	52% (411)	52% (361)	56% (50)	0.43
Child owns a children's book	14% (111)	14% (100)	12% (11)	0.71
Child's height-for-age z-score[6]	-0.6 (2.1)	-0.6 (2.1)	-0.3 (2.1)	0.32
Child's adjusted hemoglobin[2]	9.6 (1.5)	9.6 (1.4)	9.8 (1.8)	0.55

P-values were calculated to test the equality of means for those who were lost at follow-up and those who were included at follow-up using cluster adjusted t-tests or chi-squared tests.

1 All mother, household, and children variables were obtained at baseline and contain 786 observations (697 at follow-up and 89 lost to follow-up) except for mother's hemoglobin which only contains 748 (667 at follow-up and 81 lost to follow-up), mother's HOME score which contains 754 observations (668 follow-up and 86 lost to followup), child's height for age which contains 718 observations (636 follow-up and 82 lost to follow-up), and child's hemoglobin levels which contains 466 observations (416 follow-up and 50 lost to follow-up).

2 Hemoglobin levels were adjusted for elevation using procedures established by the CDC (1989).

3 HOME score measured harsh parenting quality using subscales of the Home Observation for Measurement of the Environment Inventory (HOME). Higher scores indicate worse outcomes.

4 Asset index was constructed using the first principal from a Principal Component Analysis. Variables used to construct the index were housing infrastructure indicators (e.g. floor, roof, wall, and water source) and asset indicators (e.g. refrigerator, small stove or TV).

5 Urban referred to parishes with 5000 or more inhabitants.

6 Height was converted to age and sex adjusted z-scores using WHO standards.

Table 2.2: Mean (SD) or %(n) from baseline (2003-2004) and follow-up (2005-2006) data by treatment and comparison groups.

Mother and household baseline characteristics	Total (N=1,196)[1]	Comparison (N=399)[1]	Treatment (N=797)[1]	P-value
Mother's age	22.6 (3.8)	22.5 (3.5)	22.7 (4.0)	0.45
Mother's completed schooling (grades)	6.9 (2.9)	6.8 (3.0)	7.0 (2.8)	0.70
Mother lives with a partner	83% (996)	84% (337)	83% (659)	0.47
Mother speaks indigenous language	3% (30)	4% (14)	2% (16)	0.52
Mother's adjusted hemoglobin level[2]	11.3 (1.5)	11.2 (1.5)	11.4 (1.5)	0.43
Harsh parenting HOME score (0-11 scale)[3]	2.5 (2.3)	2.4 (2.3)	2.5 (2.3)	0.74
Asset Index[4]	0 (2.3)	0.01 (2.34)	-0.01 (2.27)	0.96
Number of children 0-5 years old	2.1 (0.8)	2.1 (0.9)	2.1 (0.8)	0.84
Had a child after baseline	42% (499)	42% (169)	41% (330)	0.75
Urban[5]	45% (542)	44% (177)	46% (365)	0.64
Child baseline characteristics	**Total (N=697)[6]**	**Comparison (N=230)[6]**	**Treatment (N=467)[6]**	**P-value**
Child's age in months	6.6 (4.2)	6.9 (4.3)	6.5 (4.2)	0.26
Child is male	52% (361)	51% (117)	52% (244)	0.73
Child owns a children's book	14% (100)	14% (32)	15% (68)	0.86
Child's height-for-age z-score[7]	-0.6 (2.1)	-0.7 (2.0)	-0.5 (2.1)	0.41
Child's adjusted hemoglobin[2]	9.6 (1.3)	9.5 (1.3)	9.7 (1.3)	0.42
Follow-up characteristics	**Total (N=1,196)[8]**	**Comparison (N=399)[8]**	**Treatment (N=797)[8]**	**P-value**
Language IDHC-B score	44.1 (34.8)	42.3 (34.2)	45.0 (35.1)	0.28
Child combines words	59% (704)	55% (219)	61% (485)	0.12
Height-for-age z-score[7]	-1.7 (1.2)	-1.7 (1.2)	-1.7 (1.2)	0.85
Adjusted hemoglobin levels[2]	10.4 (1.4)	10.3 (1.3)	10.4 (1.5)	0.63
Child was bought a toy in the last 6 months	46% (549)	42% (167)	48% (382)	0.17
Household owns a children's book	34% (405)	38% (149)	32% (256)	0.22
Child attended or attends a day care center	32% (372)	30% (115)	33% (257)	0.6
Child had a growth check-up in the last 6 months	51% (602)	49% (197)	51% (405)	0.66
Child received vitamin A or iron in the last 6 months	37% (429)	34% (133)	38% (296)	0.36
Child had parasite treatment in the last 12 months	45% (536)	45% (179)	45% (357)	0.98
Food index[9]	0.0 (1.5)	-0.1 (1.5)	0.0 (1.5)	0.62
Harsh parenting HOME score (0-11 scale)	2.6 (2.5)	2.5 (2.4)	2.7 (2.6)	0.56
Mother's depressive symptoms score (0-60 scale)	19.4 (10.9)	18.9 (10.6)	19.6 (11.1)	0.44

P-Values were calculated to test the equality of means of the treatment and comparison groups using cluster adjusted t-tests or chi-squared tests.

1 All mother or household variables were obtained at baseline and contain 1196 observations (399 comparison and 797 treatment) except for mother's hemoglobin, which only contains 1143 observations (380 comparison and 763 treatment) and mother's HOME score, which contains 1158 observations (389 comparison and 769 treatment).

2 Hemoglobin levels were adjusted for elevation using procedures established by the CDC (CDC, 1989).

3 HOME score measured harsh parenting quality using subscales of the Home Observation for Measurement of the Environment Inventory (HOME). Higher scores indicate worse outcomes.

4 Asset index was constructed using the first principal from a Principal Component Analysis. Variables used to construct the index were housing infrastructure indicators (e.g. floor, roof, wall, and water source) and asset indicators (e.g. refrigerator, small stove or TV).

5 Urban referred to parishes with 5000 or more inhabitants.

6 Of the 1196 children at follow-up only 697 (230 comparison and 467 treatment) were alive or lived in the household at baseline. Of the 697 children, 636 (205 comparison and 431 treatment) had height measurements and 410 had hemoglobin counts (131 comparison and 279 treatment).

7 Height was converted to age and sex adjusted z-scores using WHO standards.

8 Total number of children with IDHC-B scores was 1192 (397 comparison and 795 treatment), with combine words indicator was 1194 (398 comparison and 796 treatment), with height-for-age z-score was 1072 (361 comparison and 711 treatment), and with hemoglobin levels was 922 (293 comparison and 629 treatment). Total number of mothers with CES-D score was 959 (318 comparison and 641 treatment), and with HOME score was 1194 (399 comparison and 795 treatment).

9 Food index was constructed using Principal Components Analysis. Variables included in the analysis were indicators for whether a child ate any of a list of 11 food items in the last 7 days (liver, chicken, pasta and/or bread, spinach and/or chard, carrots, citrus fruits, non-citrus fruits, ice cream and/or soda, potato chips, cookies and/or crackers, candy).

Table 2.3: Effect of the Bono de Desarrollo Humano on child outcomes.

	Total			Rural[1]			Urban[2]		
	Coef.[3]	95% CI	N	Coef.	95% CI	N	Coef.	95% CI	N
Language IDHC-B[4]	2.43	(-1.01 - 5.86)	1192	5.24*	(1.28 - 9.20)	651	-0.92	(-6.83 - 5.00)	541
Combing words[5]	0.08	(-0.02 - 0.18)	1194	0.14*	(0.02 - 0.27)	652	0	(-0.14 - 0.15)	542
Height-for-age z-score[6]	0.01	(-0.18 - 0.19)	1072	-0.09	(-0.37 - 0.19)	582	0.13	(-0.11 - 0.37)	490
Adjusted hemoglobin level[7]	0.04	(-0.21 - 0.29)	922	0.13	(-0.29 - 0.54)	490	-0.1	(-0.38 - 0.18)	432

Robust standard errors clustered at the parish level. **p $<$ 0.01, *p $<$ 0.05, + p$<$ 0.1. All specifications control for a child's age and sex, a mother's age, marital status, years of education, an indicator for whether the mother speaks an indigenous language, the number of children younger than 5 years old, and a household's asset index. OLS regressions conducted for language (IDHC-B), HAZ, and hemoglobin level outcomes. Probit regression conducted for combining words outcome.

1 Rural referred to parishes with less than 5000 inhabitants.

2 Urban refers to parishes with 5000 or more inhabitants.

3 Coefficient represents treatment effect, which was included in model as an indicator that took a value of one if parish had been randomly assigned to the Bono de Desarrollo Humano intervention treatment group.

4 Language IDHC-B was a child's score on the language development indicator Fundacíon MacArthur Inventorio del Desarrollo de Habilidades Comunicativas-Breve.

5 Combining words is an indicator which takes the value of one if a child sometimes or frequently combines two or more words.

6 Height was converted to age and sex adjusted z-scores using WHO standards.

7 Hemoglobin levels were adjusted for parish level elevation using procedures established by the CDC.

Table 2.4: Effect of the Bono de Desarrollo Humano on potential mediating factors by groups.

	Total sample		Rural sample[1]		Urban sample[2]	
	Coef.[3]	95% CI	Coef.	95% CI	Coef.	95% CI
Cognitive inputs						
Child was bought a toy in the last 6 months	0.06	(-0.01 - 0.14)	0.09*	(0.00 - 0.17)	0.03	(-0.12 - 0.17)
Household owns a children's book	-0.06	(-0.13 - 0.02)	-0.07	(-0.16 - 0.02)	-0.01	(-0.13 - 0.10)
Child attended or attends a day care center	0.03	(-0.08 - 0.13)	0.05	(-0.08 - 0.17)	0.01	(-0.15 - 0.17)
Health inputs						
Child had a growth control in the last 6 months	0.02	(-0.06 - 0.09)	0.07+	(-0.01 - 0.16)	-0.05	(-0.17 - 0.07)
Child received vitamin A or iron in the last 6 months	0.04	(-0.03 - 0.11)	0.11**	(0.03 - 0.20)	-0.04	(-0.14 - 0.07)
Child had parasite treatment in the last 12 months	0	(-0.09 - 0.09)	0.01	(-0.11 - 0.13)	-0.02	(-0.14 - 0.11)
Food index[4]	0.09	(-0.11 - 0.28)	0.2	(-0.07 - 0.48)	-0.03	(-0.30 - 0.24)
Parental quality						
Harsh parenting HOME score (0-11 scale)[5]	0.21	(-0.41 - 0.83)	-0.49	(-1.14 - 0.16)	1.06+	(-0.03 - 2.15)
CES-D score (0-60 scale)[6]	0.71	(-0.84 - 2.25)	0.26	(-2.01 - 2.53)	1.16	(-0.99 - 3.32)

Robust standard errors were clustered at the parish level. **p $<$ 0.01, *p $<$ 0.05, + p$<$ 0.1. All specifications control for a child's age and sex, a mother's age, marital status, years of education, an indicator for whether the mother speaks an indigenous language, the number of children younger than 5 years old, and a household's asset index. OLS regressions conducted for continuous variables (Home, CES-D, food index). Probit regression conducted for dichotomous variables (Child had a toy, household owns a children's book, child assisted day care, child had growth control, child received vitamin A or iron, and child had a parasite treatment).

1 Rural referred to parishes with less than 5000 inhabitants.

2 Urban referred to parishes with 5000 or more inhabitants.

3 Coefficient represents the treatment effect, which was included in the model as an indicator that took a value of one if parish had been randomly assigned to the Bono de Desarrollo Humano intervention treatment group.

4 Food index was constructed using Principal Components Analysis. Variables included in the analysis were indicators for whether a child ate any of a list of 11 food items in the last 7 days (e.g. liver, chicken, pasta and/or bread, spinach and/or chard, carrots, citrus fruits, non-citrus fruits, ice cream and/or soda, potato chips, cookies and/or crackers, candy).

5 HOME score measured harsh parenting quality using subscales of the Home Observation for Measurement of the Environment Inventory (HOME). Higher scores indicate worse outcomes.

6 CES-D measured a mother's depression using the Center for Epidemiologic Studies Depression Scale (CES-D) 20-item questionnaire. Higher scores indicate worse outcomes.

Chapter 3

Cash transfers and domestic violence

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3.1 Introduction

Violence against women has been recognized as a major health and human rights problem that is wide-spread and common. It is both a consequence and cause of gender inequality and thus creates an obstacle for achieving the Millennium Development Goals [Garcia-Moreno et al., 2005, WHO, 2005]. Violence against women is multi-dimensional and complex. The United Nations defines violence against women as "any act of gender-based violence that results in, or is likely to result in, physical, sexual or mental harm or suffering to women, including threats of such acts, coercion or arbitrary deprivation of liberty, whether occurring in public or in private life" [Garcia-Moreno et al., 2005]. Recent multi-country studies on domestic violence conducted by international organizations such as the World Health Organization and the Pan American Health Organization, reveal the pervasiveness of domestic violence. In Latin America it is estimated that approximately one in three women have been a victim of sexual, physical, or psychological violence at the hands of domestic partners.[2]

While previous research focused on injury as the primary health outcome of violence, more recent literature has begun to draw attention on the mental, physical, economic and intergenerational consequences of domestic violence. In the United States, the cost of domestic violence is estimated at $5.8 billion per year which includes the cost of medical services and the cost of

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[2]http://www.paho.org/english/dd/pin/pr061121.htm

lost productivity [CDC, 2003]. In addition to the physical and productive damage, domestic violence harms children's development and leads to worse health, social, and academic outcomes [Aizer, 2007, Koenen et al., 2003, Wolfe et al., 2003]. Moreover, children who suffer from social and emotional problems caused by domestic violence create externalities such as decreasing the academic achievement of classroom peers [Carrell and Hoekstra, 2010].

Although the consequences of domestic violence are well documented, there is less evidence on public policies that help reduce domestic violence. In the United States, studies found that unilateral divorce laws [Stevenson and Wolfers, 2006] and reductions in the male-female wage gap [Aizer, 2010] reduce domestic violence; and in South Africa, a micro-finance program that was combined with health education led to reductions in domestic violence [Pronyk et al., 2006]. In developing countries, studies on policies that reduce domestic violence are even more rare and most focus on the impacts of conditional cash transfer (CCTs) programs, which have become a very popular tool for reducing poverty and increasing human capital. Bobonis et al. [2009] find that in the short run Mexico's CCT program, Oportunidades, reduced physical violence, but there were no effects in the long run [Bobonis and Castro, 2010]; and in Peru, Perova [2010] finds that its CCT program, Juntas, significantly decreased physical and emotional violence. Both programs in Peru and Mexico are conditional cash transfer programs and thus it is impossible to completely isolate the income effect from the conditionalities. In particular, the health and education requirements of conditional cash transfer programs could also lead to decreases in domestic violence. By focusing on an unconditional cash transfer we are able to isolate the income effect and thus contribute to the existing literature.

Other studies that have looked at the relationship between income and domestic violence often fail to establish a causal relationship due to issues of omitted variable bias or reverse causality inherent in cross-sectional studies [Aizer, 2010]. We are able overcome these issues by taking advantage of the randomized roll-out of Ecuador's unconditional cash transfer program, the Bono de Desarrollo Humano (BDH), to study the impact of an exogenous increase in a mother's income on domestic violence. We focus on physical violence and psychological violence which we divide up into emotional violence and controlling behaviors by husband or partner. We find that for mothers with more than a primary education (7 years or more of schooling), the BDH significantly decreases emotional violence by 6% and controlling behaviors by 8%. For mothers with less than seven years of schooling, we find that the effect of the BDH depends on whether or not the father has more schooling than the mother. For households where the father does *not* have more schooling, we find that the BDH significantly increases emotional violence by 12% and controlling behaviors by 10%. These results are consistent with the predictions of a household bargaining model developed by Tauchen et al. [1991] and explained in detail in the section below. Although we find significant changes in psychological violence as a result of a cash transfer, we do not find any

corresponding changes in physical violence.

The rest of the paper proceeds as follows: Section 2 provides a description of the theories on domestic violence and develops predictions based on these theories; Section 3 describes the study design and data we use to conduct our analysis; Section 4 presents the estimation strategy and results; and Section 5 concludes.

3.2 Theories on income and domestic violence

Literature in sociology presents theoretical reasons why an increase in a woman's income may help reduce domestic violence. One argument is that institutions in society that promote masculinity and gender inequality lead to domestic violence [Straus and Hotaling, 1980, Jewkes, 2002]. Consequently, a cash transfer that empowers females and leads to a greater degree of financial equality could lower the prevalence of domestic violence. Another influential theory asserts that poverty related stress is one of the driving forces behind domestic violence [Jewkes, 2002, Straus, 1980]. Thus, an improvement in household welfare due to a cash transfer should reduce the incidence of violence. On the other hand, most psychological theories argue that domestic violence is a disorder of power and control [Walker, 2006]. Also known as "male backlash" in the sociological literature, a man may resort to violence as a means for reinstating his authority over his wife [Macmillan and Gartner, 1999]. Consequently, an increase in a woman's earning could actually increase domestic violence if a partner feels threatened as a result of an increase in his wife's income.

Economic models are also inconclusive as to the relationship between income and domestic violence. In household bargaining models, individual control of resources matters because bargaining outcomes depend on threat points such as divorce [Manser and Brown, 1980, McElroy and Horney, 1981] or a non-cooperative equilibrium [Lundberg and Pollak, 1993]. The more attractive an individual's opportunities outside the family, the more credible the threat point, and therefore, the more likely that the intrafamily distribution of resources will be more aligned with that individual's preferences. Tauchen et al. [1991] and Farmer and Tiefenthaler [1997] expand on the bargaining models to specifically account for domestic violence and conclude that when a woman's utility outside of marriage or partnership (also known as reservation utility) equals her utility from marriage or partnership, then an increase in her income will decrease domestic violence.[3] Consequently, a cash transfer to mothers would decrease domestic violence by improving their outside options, thus, making their threat points more credible. While Farmer and Tiefenthaler [1997] assume that a woman's reservation utility equals her utility from marriage or partnership, Tauchen et al. [1991] relax this assumption and find that when it is not binding, then the effects of an increase in her income depends on how his marginal utility for violence varies with her consumption.

[3] Assuming that the women is the victim and the husband or partner the injurer.

Although some bargaining models predict that cash transfers paid to women reduce domestic violence, there are also economic models that predict that cash transfers might actually increase it. For example, Bloch and Rao [2002] develop a model where domestic violence is used as a bargaining instrument by a husband to extract rents from the wife's family. Their model predicts that women from richer families are at increased risk of violence because there are more resources to extract. Using this logic, Bobonis et al. [2009] develop a model that predicts that cash transfers paid to women increase the threat of domestic violence with no associated physical violence because there are more resources to extract. A crucial assumption of these extraction models is that wives prefer an intact marriage over anything else. Thus, in contexts where divorce or separation is a feasible and acceptable option, the conclusions of their model do not hold.

Perova [2010] takes aspects from both the bargaining and the extraction models to develop a model where an increase in a woman's income both increases her threat point and creates greater incentives for a husband to extract rents. Her model predicts that an increase in a woman's income will decrease domestic violence if 1) the reservation utility constraint is binding and the marginal utility of income outside marriage is at least as large as that inside the marriage; 2) the reservation utility constraint is not binding and domestic violence becomes less effective; or 3) the reservation utility constraint is not binding and extraction becomes more effective but the wife voluntarily increases the transfer.

The theoretical models of Perova [2010] and Tauchen et al. [1991] predict different outcomes under different scenarios. A sufficient condition in Tauchen et al. [1991] for the relationship between a woman's income and domestic violence to be unambiguously negative is for the reservation utility constraint to be binding. This will either occur if the utility from marriage is so low that it is equal to the utility outside of marriage or if the utility outside of marriage is so high that it equals that inside of marriage. Mothers who are more likely to have a binding reservation utility are those who have more education, and thus, whose dependency on marriage is low [Farmer and Tiefenthaler, 1997]. A woman with more education has better outside options not only because she is more likely to find work and be financially independent, but also because she faces a better marriage market. Consequently, in addition to estimating the average effect of an increase in a woman's income on domestic violence, we also investigate the differential effect of the BDH with respect to a mother's education. Having more education increases a mother's reservation utility, and thus, makes it more likely that her reservation utility constraint is binding and that the relationship between income and domestic violence is negative.

In the non-binding case, Tauchen et al. [1991] assert that an increase in a woman's income will decrease domestic violence except in cases when a husband's or partner's marginal utility for violence is increasing in her consumption. In their analysis they find that an increase in domestic violence occurs in non-binding relationships when the women is the main income earner. One

way to interpret these results is that when the husband is at a disadvantage, it is more likely that his marginal utility for violence increases with her consumption. Using data from Ecuador, we test this assertion. Specifically, we investigate whether domestic violence increases in non-binding relationships where the man's schooling is less than or equal to the woman's schooling. Although we focus on education imbalances instead of income imbalances, the idea is the same: when a husband feels threatened by his wife's position, his marginal utility for violence is most likely increasing in her consumption. It is in these situations that we are most likely to see a "male backlash". Given that women with high education are more likely to be in binding relationships, the increase in violence should only occur for women with low education who do not have a credible outside option, but whose education is the same or higher than their husband's or partner's. The table below indicates the predictions from the Tauchen et al. [1991] model.

Table 3.1: Predictions on how an increase in a mother's income will affect domestic violence

	Mother's edu>=Father's edu	Mother's edu<Father's edu
Low education (non-binding)	Income increases domestic violence	Income decreases domestic violence
High education (binding)	Income decreases domestic violence	Income decreases domestic violence

3.3 Study design and data

3.3.1 The Bono de Desarrollo Humano

In 1998 Ecuador created its largest social assistance program called Bono Solidario. The Bono Solidario consisted of an unconditional cash transfer of 100,000 sucres (approximately $15 USD) per month to poor mothers and 50,000 sucres (approximately $7.5 USD) to elders and handicapped. Due to substantial leakage to non-poor families, the Bono Solidario was phased out and replaced by the Bono de Desarrollo Humano (BDH) in 2003 [Paxson and Schady, 2010]. The major improvement of the BDH over the Bono Solidario was that it would be means-tested. Thus, households who were in the bottom two poverty quintiles according to the Sistema de Selección de Beneficiarios (SELBEN) index qualified for the transfer. In practice the change from Bono Solidario to Bono de Desarrollo Humano meant that 60% of families who were receiving the transfer under the Bono Solidario continued to receive it under the BDH. The other 40% stopped receiving the transfer and a new set of families who were eligible were brought into the program. Our study focuses on this new set of families who were gradually brought into the program.

The BDH was initially designed as a conditional cash transfer (CCT) program that consisted of giving mothers a monthly stipend if their children met certain schooling and health care requirements. However, due to administrative constraints the conditionalities were never implemented, thus making the

BDH more like an unconditional cash transfer. Another distinguishing feature of the BDH is that the transfer size does not depend on the number of children in the household. The size of the transfer in 2005 was $15/month which was approximately 6-10% of an average household's pre-transfer expenditure for households in the bottom two poverty quintiles.

3.3.2 Study Design

The BDH was rolled out slowly across the country to new families that qualified for the transfer. This gradual roll-out allowed two separate randomized evaluations to be conducted: a health evaluation and an education evaluation. The study reported here uses the data collected for the health evaluation where 378 parishes (the smallest administrative unit in Ecuador) from six provinces (three coastal and three highland provinces) were randomized into treatment and control groups. Parishes were stratified into rural and urban groups and from each group treatment and control parishes were randomly selected. In total, 118 parishes were selected for the experiment: 79 in the treatment group and 39 in the control group. Initially the treatment group was divided into a conditional and an unconditional cash transfer group, however, because conditionalities were never implemented, these two groups were combined into one larger treatment group.

The BDH became available to rural treatment parishes in June 2004 and to urban treatment parishes in November 2004. The transfers were not available to control parishes until after 2006. Transfers were administered by the Government of Ecuador and distributed through a large network of private banks (Banred) and through the National Agricultural Bank (Banco Nacional de Fomento) [Schady and Araujo, 2008]. According to survey response data from 2005, there was very little contamination of the experiment: take-up of the BDH program was 73% for the treatment group and 3% for the control group.

Baseline (October 2003-March 2004) and follow-up (September 2005-January 2006) surveys were conducted by the World Bank and the Government of Ecuador. In order to be included in the survey sample, households had to meet the following criteria: have at least one preschool age child, have no children older than 6 years old, be eligible for the cash transfer program, and not have been recipients of the previous welfare program, Bono Solidario. Households in treatment and control parishes that met the criteria above were then randomly sampled for the evaluation. The total sample at baseline consisted of 3,426 households, of which, 94% were re-interviewed at the follow-up (Figure 2.1).

3.3.3 Data

The baseline and follow-up surveys collected information on children's health status and health care usage and on their mother's health status and health

care usage. The information on mothers consisted not only of their physical health, but also their mental health and their cognitive abilities. For this study we concentrate on the spousal domestic violence data that was collected in the baseline and follow-up surveys.

The domestic violence questions were only administered to mothers if they currently lived with their partner and if their partner was not present during the interview. Consequently, of the 3,210 mothers in the follow-up survey, only 2,274 currently lived with their partner and of these mothers, only 1,574 were surveyed when their husband was not present. Of the mothers who could be administered the domestic violence questions, 88.8% were willing to respond (Figure 2.1). The reasons given for not answering the domestic violence questions were: husband's relative was present (54.8%), another person was present (26%), she did not want to answer (5.7%), she is not sure why she did not want to answer (1.1%), and other (12.4%). Of the 1,397 mothers that are eligible and willing to answer the domestic violence questions at follow-up, we drop 38 because of errors in coding.[4] Thus we are left with 1,359 mothers for our analysis. We refer to this sample of mothers as our "In study" sample.

As a consequence of the eligibility requirements for being administered the domestic violence questions, attrition in our sample is high. If attrition is correlated with treatment assignment, then this could potentially bias the estimates of the impact of a cash transfer on domestic violence. There are three main time points when attrition occurred: from baseline to follow-up; at follow-up when the survey was only conducted on mothers who currently lived with their husband or partner and whose husband or partner was not present; and at follow-up for mothers who were eligible to be administered the domestic violence questions and were willing to answer them. Table 3.2 reveals that attrition is not correlated with treatment at any of the three time points.

Table 3.3 examines the baseline characteristics of those that were in the eligible study sample and answered the domestic violence questions ("In study") and those that were not ("Attrited"). Given the eligibility requirements we expect there to be differences between those that attrited and those that did not; however, differential attrition across treatment and control arms would threaten the internal validity of the study. As expected those that attrited are less likely to be married, they are less educated, and they are poorer (as measured by their asset index). Except for the variable "Husband or partner does not allow you to study or work", there are no statistically significant differences in the domestic violence questions at baseline for those in the study and those that attrited (column 3).

In order to examine if differential attrition threatens the internal validity of the study, we focus on columns 7 and 8 of table 3.3. If mothers who experience more domestic violence left the treatment arm in greater proportions than

[4]Errors in coding refer to mothers with inconsistent names and age from baseline to follow-up. Although we drop them from our analysis, our results are robust to their inclusion.

the control arm, then our treatment estimates will be biased because any decreases in domestic violence will be due to both treatment and differential attrition. Except for years of schooling of mothers and an indicator for husband or partner not spending free time with their wife or partner, there are no statistically significant differences at the 5% level in baseline characteristics for those that attrited across treatment and control arms (column 8). The differential attrition with respect to spending free time with wife or partner could potentially bias the treatment effect towards zero because not spending free time with wife is positively correlated with domestic violence. In other words, the higher rate of husbands or partners not spending time with their wives that leave the control group compared to the treatment group suggests that those that remain in the control group will display lower rates of domestic violence, and thus, our estimates will be biased towards zero. We also might worry that the differential attrition with respect to schooling might confound our estimates since less education is associated with higher rates of domestic violence. Given that those with less education are leaving the control arm at higher rates than the treatment arm, the treatment effect would again be biased towards zero since we are left with higher educated mothers in the control group. Table 3.4 examines baseline differences between treatment and control groups for the "In study" sample, and reveals that the differences in schooling and in spending free time with wife or partner are not significant; and therefore, the bias due to the differential attrition of these two variables is likely to be small.

3.3.4 Baseline analysis

The sample of mothers used for this study are those who were married or living with their partner during the follow-up survey, whose husband or partner was not present during the time of the follow-up survey, and who were willing to answer the domestic violence questions at follow-up. Table 3.4 reveals that random assignment was effective at balancing baseline characteristics of treatment and control groups for both the full sample of mothers and for the smaller "In study" sample. In the smaller study sample, the only variable that is significantly different between treatment and control groups is the indicator for whether or not the husband or partner allows his wife or partner to study or work. In the control group 40% of husbands or partners do not allow their wives to study or work compared to 31% in the treatment group. Although worrisome, none of the other 9 domestic violence indicators exhibit any significant differences between treatment and control groups, and the control group actually has a lower mean compared to the treatment group for the other controlling behavior of not allowing wife to see friends or family. Furthermore, both the indicator for not allowing wife or partner to study or work and the indicator for not spending free time with your wife, have the lowest inter-item covariances compared to all the other psychological violence indicators (see appendix for classification). Thus, we do not use either indicators to con-

struct the domestic violence variables we use in the analysis (more details on construction of variables in next section).

Table 3.4 reveals that the prevalence of domestic violence is high, with 28% of all mothers (Full sample) having been pushed, hit or attacked by their husband or partner and 40% having been yelled at by their husband or partner. Mothers in the sample are young and have approximately 7.4 years of schooling. Although formal divorce rates are very low (.6%), 7% of all mothers at baseline are separated, 19% are single, and only 33% are married. The rest of mothers (40%) live with their partners. These statistics on marital status reveal that options outside of marriage are common in Ecuador, and thus, the dissolution of marriage or partnership is a feasible threat point.

3.3.5 Domestic violence indicators

Domestic violence is a multidimensional and complex issue that is usually categorized into physical violence, psychological violence, and sexual violence. Due to the structure of the data, we focus only on psychological and physical violence. Physical violence is defined by the World Health Organization (WHO) as "The intentional use of physical force with the potential for causing death, injury, or harm. Physical violence includes, but is not limited to scratching, pushing, shoving, throwing, grabbing, biting, choking, shaking, poking, slapping, punching, hitting..."[Ellsberg and Heise, 2005]. Given this definition, we create a physical violence indicator that equals one if a woman has been pushed, hit, or physically harmed by her partner. At baseline, 28% of mothers had experienced physical violence.

Psychological violence is a little more complex. It is defined by the WHO as "Any act or omission that damages the self-esteem, identity, or development of the individual. It includes, but is not limited to humiliation, threatening loss of custody of children, forced isolation from family or friends, threatening to harm the individual or someone they care about, repeated yelling or degradation, inducing fear through intimidating words or gestures, controlling behavior,..."[Ellsberg and Heise, 2005]. Operationally, WHO divides this definition of psychological violence into emotional violence and controlling behavior by a partner [Garcia-Moreno et al., 2005]. In the follow-up survey there are 9 questions that can be grouped into the following categories: emotional support, emotional violence, and controlling behavior (see Appendix I for questions). Given that the WHO only uses the latter two categories in their definitions, we concentrate on these latter categories in the construction of our psychological violence variables. For each of the 7 emotional violence or controlling behavior questions, the survey asks if a husband or partner frequently, sometimes, or never exhibits the specific behavior. We create an indicator for emotional violence that equals one if the respondent answered frequently or sometimes to any of the four emotional violence questions. We also create an indicator for controlling behavior that equals one if the respondent answered frequently or sometimes to the following controlling behavior questions: husband or part-

ner does not allow wife or partner to see friends or family; and husband or partner ignores wife or partner. We do not include the indicator for whether or not the husband or partner allows his wife to study or work because it is not balanced across treatment and control groups at baseline and it has a low inter-item covariance with the rest of the psychological indicators. At baseline the percent of mothers suffering from emotional violence was 52.6% and the percent suffering from controlling behavior by their husband or partner was 40.5%.

3.4 Empirical analysis and results

3.4.1 Empirical analysis

To estimate the treatment effect of the BDH on domestic violence we take advantage of the randomized roll-out of the program and conduct an intent-to-treat analysis. This approach avoids any bias that might occur due to selection into and out of the program. The treatment indicator takes a value of one if a mother lived in a parish that belonged to the treatment group and a value of 0 if a mother lived in a parish that belonged to the control group. In all the regressions, we cluster the standard errors at the parish level. The specific equation that we estimate is the following:

$$DV_{ip} = \beta_0 + \beta_1 T_p + \beta_2 X_{ip} + \varepsilon_{ip} \tag{3.1}$$

Where DV_{ip} is the domestic violence indicator for mother i from parish p. T_p is an indicator for whether or not parish p was in the treatment group, and X_{ip} is a vector of control variables. β_1 measures the average effect of the BDH on domestic violence. The successful randomization (Table 3.4) ensures that β_1 is unbiased since it ensures that the treatment and control groups are similar before the intervention. Given the success of the random assignment, the inclusion of baseline controls is not necessary to obtain unbiased estimates of β_1. However, in most estimates we include mother (age, education, race, marital status) and household (number of children 0-5 years old, urban indicator, asset index, province indicators, and father's education) control variables in order to increase the precision of the estimates. All estimations are conducted using probit models.

In addition to estimating the average effect of the BDH, we estimate the differential effect of the cash transfer with respect to a mother's education. The exact equation that we estimate is the following:

$$DV_{ip} = \alpha_0 + \alpha_1 T_p + \alpha_2 T_p * M_{ip} + \alpha_3 M_{ip} + \alpha_4 X_{ip} + \varepsilon_{ip} \tag{3.2}$$

Where M_{ip} is an indicator that equals one if a mother has 7 years or more of schooling (more than primary school education). In our study sample, only 47% of mothers have more than a primary education. We refer to these mothers

as "High education" mothers. α_1 measures the effect of the BDH for mothers with low education (less than 7 years of schooling), while $\alpha_1 + \alpha_2$ measures the effect of the BDH for mothers with high education. Therefore, α_2 is the differential impact of the BDH with respect to a mother's education. Given that mothers with more education have higher reservation utilities, we expect that for these women an increase in income will have a negative impact on domestic violence. Furthermore, for mothers with lower reservation utilities (mothers with less than 7 years of schooling) we expect the impact of income on domestic violence to be smaller in magnitude. In other words, we expect $\alpha_1 + \alpha_2$ to be negative and the differential effect (α_2) to be negative and significant.

In order to test the predictions in table 3.1 we estimate equation 3.2 separately for households where the mother has at least as many years of schooling as the father and for households where the father has more years of schooling than the mother. We label the former group as "Mother's edu>=Father's edu" and the latter group as "Mother's edu<Father's edu". In households where the mother has at least as much education as the father, we expect domestic violence to increase for mothers with low education and for it to decrease for mothers with high education. Specifically we expect α_1 to be positive and $\alpha_1 + \alpha_2$ to be negative. For households where fathers have more education than mothers, we expect domestic violence to decrease as a result of the BDH for both mothers with low and high education. Thus we expect both α_1 and $\alpha_1 + \alpha_2$ to be negative.

3.4.2 Average effects

Table 3.5 presents the results of estimating the effect of a cash transfer on spousal domestic violence. We focus on physical violence and psychological violence. We divide psychological violence into emotional violence (column 1 and 4) and controlling behavior (column 2 and 5) by husband or partner. Physical violence (column 3 and 6) refers to any physical attack on a mother by her husband or partner. In the first three columns we estimate the treatment effect without any controls, and in the last three columns we add control variables. As expected given the successful random assignment, adding control variables does not change the size of the coefficient on treatment. Being in the treatment group has no effect on any of the domestic violence indicators. Table 3.9 in the appendix examines the impact of the BDH on each of the 6 psychological violence indicators used to construct the emotional violence and controlling behaviors indicators. While the treatment effect is negative for all indicators, the BDH only leads to a significant decrease in the probability that a husband or partner does not allow his wife or partner to see her friends or family.

Other factors that are negatively correlated with domestic violence are father's years of schooling, being married, and living in urban areas (Table 3.5). Factors that are positively correlated with domestic violence are being indige-

nous, the number of children 0-5 years old in a household, a household's asset index, and having to use the kitchen for sleeping. Having to use the kitchen for sleeping is likely to be an indication of both poverty and crowding. Even though the provinces that were sampled for the study are not composed of large indigenous populations, and thus the proportion of indigenous mothers in our sample is small (5%), we still observe a large and positive correlation on domestic violence variables. Specifically, indigenous women are 13% more likely to suffer from controlling behaviors and they are 18% more likely to suffer from physical abuse. There are many factors that could explain this strong positive relationship such as the fact that indigenous women are disproportionately poorer than non-indigenous women, they have less education, there are more members living in their household, and they are less likely to separate from their husband [Hall and Patrinos, 2006].

3.4.3 Heterogeneous effects

Given that mothers with more education have better labor market options outside of marriage or partnership, it is more likely that their reservation utilities will be binding; and therefore, it is more likely that an increase in their incomes will lead to a decrease in domestic violence. In order to investigate whether the effect of the cash transfer is negative and larger in magnitude for mothers with more education, we interact treatment with an indicator that equals one if a mother has 7 years or more of schooling ("high education"). Table 3.6 reveals that a cash transfer has no effect on domestic violence for mothers with low education, but for mothers with high education the effect of the BDH is negative for all domestic violence variables and significant for emotional violence and controlling behaviors. Specifically, for mothers with high education the BDH decreases the probability of emotional violence by 6% and the probability that a husband or partner exhibits controlling behaviors by 8%. The treatment effect for mothers with high education is significantly different to that for mothers with low education for both emotional violence and controlling behaviors. The significant decrease in psychological violence for mothers with high education is due to significant decreases in the probability that a husband or partner: does not allow her to see friends or family; ignores her; yells at her; tells her she is worthless, and threatens to leave her (Appendix, Table 3.10). There is no effect of the BDH on physical violence for either mothers with high or low education.

Table 3.7 divides up households by whether or not the father has more education than the mother in order to explore if an increase in a woman's income increases domestic violence in households where the mother has at least as much education as the father. Given that mothers with high education are more likely to have a high reservation utility and thus be in a binding relationship, domestic violence should only increase for mothers with low education but who still have the same or higher education than their husbands or partners. As predicted, Table 3.7 shows that the BDH increases domestic vio-

lence for mothers with low education in households where they have at least as much education as their husbands or partners (columns 1-3). For this group of mothers, an increase in their income significantly increases emotional violence by 12% and controlling behaviors by 10%. In contrast, mothers with high education experience a significant decrease in controlling behavior by 7%. The treatment effect for mothers with high education is significantly different to that of mothers with low education for both psychological violence indicators. In households where the father has more education than the mother (columns 4-6), the BDH decreases domestic violence for all mothers, although the effect is only significant for mothers with high education. Specifically, for this group of mothers an increase in her income decreases emotional violence by 35% and controlling behaviors by 18%. The BDH has no significant effect on physical violence for any of the four groups although the signs of the coefficients are consistent with our predictions.

3.4.4 Robustness

One concern with the estimates reported above is that they may be confounded by self-reporting bias. Domestic violence is a sensitive issue and thus subject to a high degree of under-reporting. The most important factors influencing disclosure are adequate training and safety measures that ensure privacy during interviews [Ellsberg et al., 2001]. Although measures were taken to reduce under-reporting (such as ensuring husband was not in the household at the time of the interview), we cannot dismiss it completely. If under-reporting leads to classical measurement error, then our estimates will be lower bound estimates. However, if under-reporting is associated with treatment, and specifically, if those that are in the treatment group are more likely to under-report, then our estimates will be biased away from zero for the following three groups: 1)high education, mother's education<father's education; 2) low education, mother's education<father's education; 3) high education, mother's education>=father's education. For the fourth group, low education, mother's education>=father's education, the estimates would be lower bound estimates. In order for all estimates to be biased away from zero, mothers who are in this fourth group and in the treatment group would have to over-report domestic violence while the rest of mothers in the treatment group would have to under-report. This pattern seems highly unlikely.

Another concern with our inference is that while the BDH was randomized across treatment and control parishes, the research design did not stratify across education groups. Consequently, there could be pre-existing differences between treatment and control groups across the four groups analyzed. For example, for those in the fourth group (low education, mother's education<=father's education), if the treatment group had a higher proportion of indigenous mothers than the control group, then the higher psychological violence in the treatment group could be due to this higher proportion of indigenous mothers. To show that preexisting differences are not a concern, we

compare baseline characteristics across treatment and control groups for all four analyzed groups. Table 8 reveals that across the 60 difference of means tests (4 groups X 15 variables) only 1 is significant at the 5% level. In the high education, mother's education<=father's education group (columns 4-6), the treatment arm has a higher proportion of Afro-Ecuadorians. However, given that no significant associations exist with being Afro Ecuadorian and any domestic violence indicator (Table 3.5), it is not likely that this will bias our results. Furthermore, we control for being Afro-Ecuadorian in all our regressions.

More importantly, table 8 also reveals that there are no significant differences between treatment and control arms for any of the four groups on any of the baseline domestic violence indicators used in our analysis (emotional violence, controlling behaviors, and physical violence). Unfortunately, only a fraction of mothers who answered the domestic violence questions at follow-up also answered them at baseline. Consequently, there may not be sufficient power to detect small differences between treatment and control arms for the smaller two groups where the father has more education than the mother (columns 7-12). Looking closely at the means across treatment and control arms for these smaller two groups, we find that the largest difference in means occurs with the controlling behavior indicator for the low education, mother's education<father's education group (columns 7-9). For this group, the control arm has a lower proportion of mothers reporting controlling behaviors than the treatment arm (.34 vs .51). Although not significant, this large difference suggests that for this group, the husbands from the treatment arm engaged in more controlling behaviors at baseline, and therefore, estimates of the BDH on controlling behavior are likely to be lower bound estimates. Due to the fact that only a fraction of mothers who answered the domestic violence questions at follow-up also answered them at baseline, we also check to see if in the full sample of mothers at baseline, there are any significant differences in means on any of the domestic violence indicators (emotional violence, controlling behaviors, physical violence) across treatment and control arms for the four groups (Appendix, Table 11). Again, we find no significant differences at the 5% level, although there is an indication that for the two groups where the father has more education than the mother (columns 7-12), husbands in the treatment group exhibit more controlling behaviors at baseline; and thus, our estimates of the BDH on controlling behaviors are likely to be lower bound estimates.

3.5 Conclusion

In this paper we take advantage of the randomized roll-out of a cash transfer program, BDH, to investigate how an increase in a woman's income impacts spousal domestic violence. We use the Tauchen et al. [1991] model to predict when an increase in a woman's income will lead to a decrease in domestic violence and when it will lead to an increase in domestic violence. Our results

are consistent with our predictions. For mothers with high education, the BDH significantly decreases psychological violence as measured by emotional violence and controlling behaviors. For mothers with low education, the effect on domestic violence is ambiguous and depends on the differences in schooling between the mother and father. In particular, for households where the mother has less schooling than the father, the effect on psychological violence is negative although imprecisely measured. In contrast, for households where the mother has at least as much schooling as the father, the BDH significantly increases emotional violence and controlling behaviors.

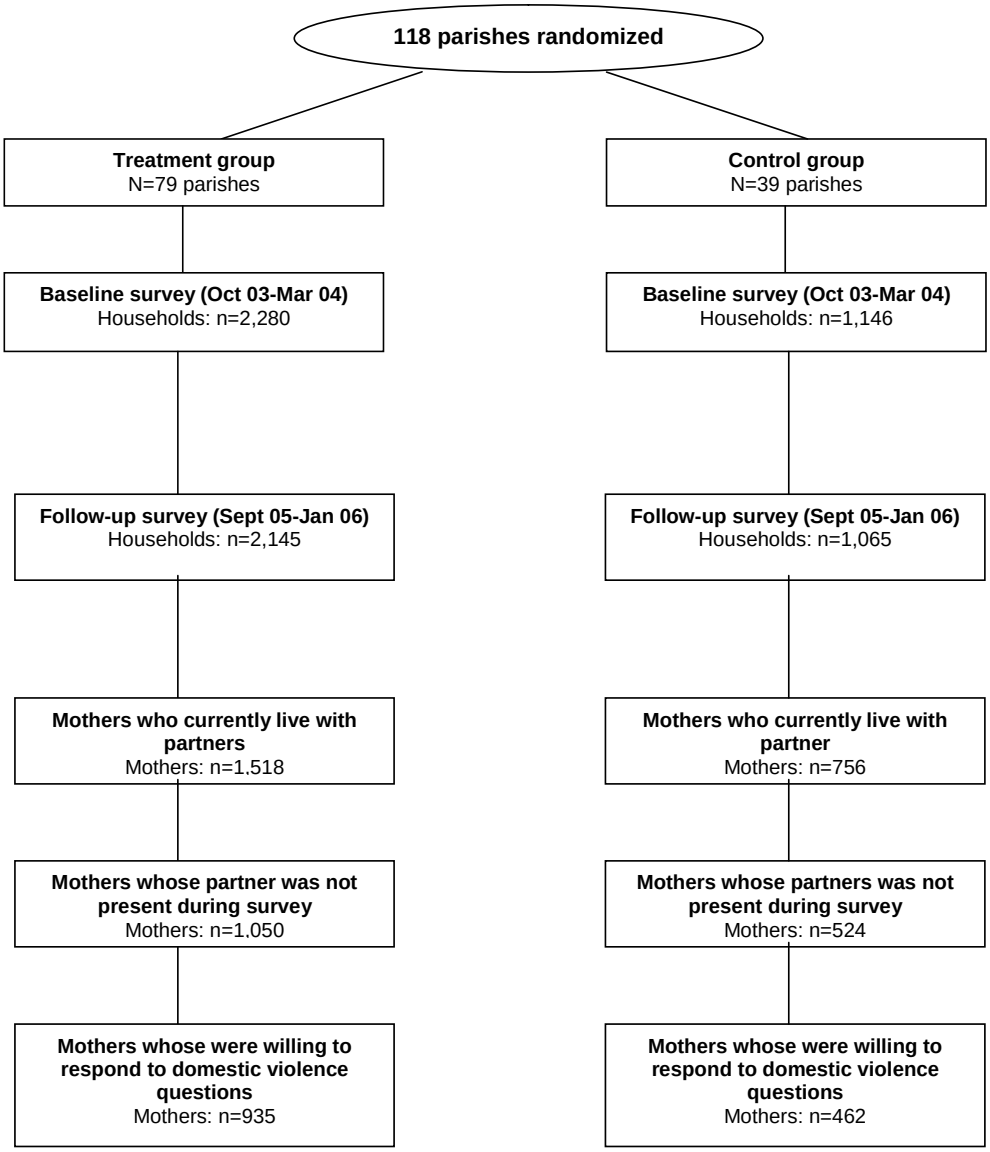
Even though we find that the BDH decreases psychological violence for certain households and increases it for others, we never observe a significant corresponding decrease or increase in physical violence. The most likely reason for this lack of significant impact is that physical violence is more likely to be under-reported. Not only are women more likely to be afraid for their safety if they have been physically abused, but physical violence also has more of a negative stigma attached to it. Given that physical violence is more likely to be under-reported and given that there is a high correlation between physical violence and psychological violence, we should not dismiss the impacts on physical violence completely.[5] Instead, we should use our results from emotional violence and controlling behaviors as suggestive evidence for the direction in which an increase in a woman's income would impact physical violence.

To our knowledge, this is the first paper in a developing country to investigate the effects of an unconditional cash transfer on domestic violence. Given that the cash transfer is not tied to health and education requirements, we are able to isolate the effect of income on domestic violence. In our analysis we show that although an increase in a woman's income leads to a decrease in domestic violence for many households, there are vulnerable households where domestic violence actually increases. These households are those where the mother's outside option is low and where the husband is most likely to feel threatened by an increase in her income. In our study, these vulnerable household are those where a mother's education is low, but she has at least as much education as her husband. In light of these results, it is important for policy makers to identify these vulnerable populations and take measures to reduce their risk. The main goal of the Bono de Desarrollo Humano was never to decrease domestic violence. However, we show yet another dimension of how a cash transfer can impact the everyday lives of the poor.

[5] In our study the correlation coefficient for emotional violence and physical violence at baseline is .34 and the correlation coefficient for controlling behaviors and physical violence is .25

3.6 Figures and tables

Figure 3.1: Flow of participants who are in the study



Tables

Table 3.2: Attrition analysis

	Dependent variable=1 if mother is			
	In follow-up	Eligible	Willing	In study
Treatment	0.01	-0.00	0.01	0.01
	(0.01)	(0.03)	(0.03)	(0.03)
Observations	3426	3210	1574	3426

Clustered standard errors in parentheses. * p<0.10, ** p<0.05, *** p<.01
Column 1: Dependent variable equals one if mother is in the follow-up.
Column 2: Dependent variable equals one if mother is eligible to be administered domestic violence questions. To be eligible mother had to be living with husband or partner and not have them present during the interview. Sample is composed of mothers in the follow-up.
Column 3: Dependent variable equals one if mother is willing to answer domestic violence questions. Sample is composed of mothers that are in follow-up and eligible to answer domestic violence questions.
Column 4: Dependent variable equals one if mother is in the follow-up, is eligible and willing to be administered domestic violence questions, and is in the study sample. Sample is composed of all mothers at baseline.

Table 3.3: Attrition analysis-baseline characteristics

	Control Group			Treatment Group				
	Attrited	In study	P-value	Attrited	In study	P-value	Diff. (1-4)	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	23.54	23.67	0.67	23.64	23.56	0.70	-0.10	0.67
Yrs of schooling (Mothers)	7.07	7.74	0.00	7.43	7.54	0.39	-0.36	0.02
Married	0.29	0.45	0.00	0.25	0.42	0.00	0.04	0.06
Indigenous	0.04	0.05	0.62	0.06	0.05	0.85	-0.01	0.17
Afro-Ecuadorian	0.10	0.09	0.90	0.10	0.11	0.21	-0.00	0.95
No health insurance	0.93	0.95	0.18	0.92	0.94	0.14	0.01	0.62
Number of children 0-5 years old	1.70	1.79	0.08	1.72	1.77	0.18	-0.02	0.59
Husband or partner (domestic violence indicators)								
- pushed, hit, or attacked you	0.29	0.29	0.97	0.28	0.27	0.72	0.02	0.71
- doesn't allow you to see friends or family	0.21	0.20	0.92	0.23	0.25	0.66	-0.02	0.52
- doesn't allow you to study or work	0.32	0.40	0.08	0.34	0.31	0.32	-0.02	0.57
- ignores you	0.30	0.31	0.98	0.32	0.34	0.63	-0.02	0.66
- threatens to leave you	0.20	0.24	0.27	0.24	0.21	0.35	-0.04	0.28
- threatens to take your children away	0.30	0.29	0.78	0.29	0.27	0.65	0.02	0.65
- tells you your worthless	0.27	0.27	0.97	0.28	0.29	0.66	-0.01	0.90
- yells at you	0.35	0.40	0.34	0.41	0.39	0.62	-0.06	0.20
- doesn't spend free time with you	0.45	0.41	0.45	0.36	0.37	0.70	0.10	0.03
- is not affectionate with you	0.32	0.32	0.90	0.38	0.32	0.11	-0.05	0.23
Asset index	-0.09	0.22	0.03	-0.11	0.13	0.01	0.02	0.87
Number of rooms in dwelling	2.12	1.81	0.00	2.13	1.89	0.00	-0.01	0.81
Kitchen is used for sleeping	0.22	0.24	0.42	0.22	0.26	0.07	-0.00	0.97
Urban	0.48	0.54	0.05	0.50	0.51	0.90	-0.03	0.26
Yrs of schooling (Fathers)	7.42	7.45	0.92	7.39	7.35	0.79	0.04	0.83

P-values are reported from t-tests on the equality of means for each variable between In Study vs Attrited groups in columns 3 and 6, and for differences in attrited groups between treatment and control arm in column 8. In study sample consists of mothers that were in the follow-up study, are eligible and willing to be administered the domestic violence questions, and are used in the analysis. Attrited refers to mothers that were in the baseline survey but were not administered the domestic violence questions at follow-up.

Table 3.4: Descriptive Statistics from Baseline

	Full sample (N=3,426)			In study sample (N=1,359)		
	Control	Treatment	P-value	Control	Treatment	P-value
Age	23.59	23.61	0.91	23.67	23.56	0.69
Yrs of schooling (Mothers)	7.33	7.47	0.22	7.74	7.54	0.26
Married	0.35	0.32	0.04	0.45	0.42	0.28
Indigenous	0.04	0.05	0.17	0.05	0.05	0.61
Afro-Ecuadorian	0.10	0.10	0.43	0.09	0.11	0.27
No health insurance	0.94	0.93	0.42	0.95	0.94	0.47
Number of children 0-5 years old	1.74	1.74	0.85	1.79	1.77	0.67
Husband or partner (domestic violence indicators)						
- pushed, hit, or attacked you	0.29	0.27	0.47	0.29	0.27	0.51
- doesn't allow you to see friends or family	0.21	0.24	0.19	0.20	0.25	0.24
- doesn't allow you to study or work	0.36	0.32	0.17	0.40	0.31	0.02
- ignores you	0.31	0.33	0.36	0.31	0.34	0.40
- threatens to leave you	0.22	0.22	0.97	0.24	0.21	0.34
- threatens to take your children away	0.30	0.28	0.48	0.29	0.27	0.59
- tells you your worthless	0.27	0.28	0.67	0.27	0.29	0.65
- yells at you	0.38	0.40	0.47	0.40	0.39	0.85
- doesn't spend free time with you	0.43	0.36	0.03	0.41	0.37	0.29
- is not affectionate with you	0.32	0.35	0.35	0.32	0.32	0.89
Asset index	0.03	-0.01	0.60	0.22	0.13	0.46
Number of rooms in dwelling	2.00	2.04	0.40	1.81	1.89	0.22
Kitchen is used for sleeping	0.23	0.24	0.70	0.24	0.26	0.60
Urban	0.50	0.50	0.81	0.54	0.51	0.30
Yrs of schooling (Fathers)	7.43	7.37	0.62	7.45	7.35	0.60

In study sample refers to mothers who are eligible and willing to answer the domestic violence questions and who are in the analysis. Number of mothers who answered the domestic violence questions at baseline are 1,187 for full sample and 654 for study sample.

Table 3.5: Average effect of the BDH on domestic violence

	(1) Emotional	(2) Controlling	(3) Physical	(4) Emotional	(5) Controlling	(6) Physical
Treatment	-0.01	-0.03	-0.01	-0.01	-0.03	-0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Yrs of schooling (Mothers)				-0.00	-0.00	0.01
				(0.01)	(0.01)	(0.00)
Yrs of schooling (Fathers)				-0.01*	-0.02***	-0.01**
				(0.01)	(0.00)	(0.00)
Married				-0.08**	-0.05*	-0.05
				(0.03)	(0.03)	(0.03)
Age				-0.01	0.02*	-0.02
				(0.01)	(0.01)	(0.01)
Age squared				0.00	-0.00	0.00
				(0.00)	(0.00)	(0.00)
Indigenous				0.10	0.13**	0.18***
				(0.06)	(0.05)	(0.05)
Afro-Ecuadorian				-0.07	-0.01	-0.03
				(0.06)	(0.06)	(0.05)
Asset index				0.02*	0.01	0.02**
				(0.01)	(0.01)	(0.01)
Urban				-0.05*	-0.05*	-0.06**
				(0.03)	(0.03)	(0.03)
Number of children 0-5 years old				0.04**	-0.00	0.06***
				(0.02)	(0.02)	(0.02)
Kitchen is used for sleeping				0.10***	-0.01	0.07**
				(0.03)	(0.04)	(0.03)
Observations	1344	1345	1355	1321	1322	1332

Marginal effects reported from probit models. Standard errors clustered at the parish level. * p<0.10, ** p<0.05, *** p<.01. All estimations contain province indicators.

Table 3.6: Differential effect of the BDH with respect to mother's education

	(1) Emotional	(2) Controlling	(3) Physical
Treatment	0.04	0.02	0.01
	(0.04)	(0.04)	(0.04)
High education X Treatment	-0.11**	-0.09*	-0.03
	(0.05)	(0.05)	(0.05)
High education	0.34	0.24	-0.32
	(0.46)	(0.51)	(0.46)
Observations	1321	1322	1332
Treatment for high education	-0.06*	-0.08**	-0.02
	(0.04)	(0.04)	(0.04)

Marginal effects reported from probit models. Standard errors clustered at the parish level. * p<0.10, ** p<0.05, *** p<.01. High education refers to mothers with 7 years or more of schooling. All estimations control for mother characteristics (age, marital status, years of schooling, race), household characteristics (father's years of schooling, asset index, urban, number of children 0-5 yrs, whether kitchen is used for sleeping, province indicators), and the interaction of all control variables with an indicator for mothers with high education.

Table 3.7: Differential effect with respect to mother's education by differences in education between mother and father

	Mother's edu>=Father's edu			Mother's edu<Father's edu		
	(1)	(2)	(3)	(4)	(5)	(6)
	Emotional	Controlling	Physical	Emotional	Controlling	Physical
Treatment	0.12**	0.10*	0.03	-0.09	-0.11	-0.01
	(0.05)	(0.05)	(0.06)	(0.07)	(0.08)	(0.06)
High education X Treatment	-0.13*	-0.16**	-0.03	-0.27***	-0.08	-0.02
	(0.08)	(0.07)	(0.06)	(0.10)	(0.11)	(0.10)
High education	0.18	0.31	-0.39	0.62	-0.39	0.45
	(0.69)	(0.61)	(0.57)	(0.66)	(0.80)	(1.05)
Observations	928	930	932	393	392	400
Treatment for high education	-0.01	-0.07*	0.00	-0.35***	-0.18*	-0.03
	(0.05)	(0.04)	(0.05)	(0.09)	(0.11)	(0.07)

Marginal effects reported from probit models. Standard errors clustered at the parish level. * p<0.10, ** p<0.05, *** p<.01. High education refers to mothers with 7 years or more of schooling. All estimations control for mother characteristics (age, marital status, years of schooling, race), household characteristics (father's years of schooling, asset index, urban, children 0-5 yrs, whether kitchen is used for sleeping, province indicators), and the interaction of all control variables with an indicator for mothers with high education.

Table 3.8: Baseline comparison of treatment and control groups stratified by education groups-Study sample

	Mother's edu>=Father's edu						Mother's edu<Father's edu					
	Low education (N=453)			High education (N=483)			Low education (N=272)			High education (N=151)		
	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Age	24.25	23.56	0.20	23.61	23.60	0.98	23.18	23.57	0.53	22.85	23.40	0.44
Yrs of schooling (Mothers)	5.74	5.75	0.95	10.82	10.50	0.11	4.48	4.44	0.88	9.08	9.38	0.34
Married	0.49	0.48	0.72	0.42	0.40	0.69	0.45	0.38	0.32	0.40	0.35	0.58
Indigenous	0.06	0.08	0.46	0.02	0.02	0.69	0.08	0.07	0.65	0.02	0.06	0.31
Afro-Ecuadorian	0.07	0.06	0.57	0.08	0.18	0.01	0.10	0.08	0.75	0.19	0.13	0.32
No health insurance	0.93	0.91	0.48	0.95	0.94	0.52	0.96	0.96	0.97	0.96	0.97	0.69
Number of children 0-5 yrs old	1.77	1.88	0.15	1.74	1.62	0.07	2.01	1.85	0.14	1.65	1.77	0.35
Asset index	-0.57	-0.67	0.63	0.98	0.76	0.22	-0.45	-0.33	0.68	1.25	1.38	0.70
Number of rooms in dwelling	1.67	1.82	0.16	1.89	1.87	0.88	1.81	1.79	0.89	2.00	2.34	0.14
Kitchen is used for sleeping	0.27	0.26	0.95	0.25	0.28	0.42	0.25	0.24	0.79	0.13	0.17	0.44
Urban	0.41	0.41	0.96	0.65	0.59	0.18	0.53	0.47	0.37	0.54	0.59	0.56
Yrs of schooling (Fathers)	5.17	5.08	0.62	7.98	7.57	0.14	8.19	8.37	0.62	11.78	12.00	0.58
Domestic violence indicators												
- Emotional Violence	0.56	0.52	0.55	0.53	0.49	0.59	0.59	0.61	0.86	0.50	0.43	0.59
- Controlling Behaviors	0.37	0.43	0.33	0.38	0.43	0.49	0.34	0.51	0.12	0.27	0.31	0.76
- Physical violence	0.28	0.26	0.68	0.33	0.26	0.27	0.28	0.33	0.62	0.18	0.19	0.93

P-values are reported from t-tests of the equality of means for each variable between treatment and control arms for each of the four groups. Low education refers to mothers with less than 7 years of schooling and high education refers to mothers with 7 or more years of schooling. The sample is composed of mothers who answered the domestic violence questions at follow-up. Only a fraction of mothers in the study at follow-up answered the domestic violence questions at baseline. The sample size for mothers who answered the domestic violence questions at baseline is the following for each of the four groups: 1)Low education, Mother's edu>=Father's edu, N=239; 2) High education, Mother's edu>=Father's edu, N=225; 3) Low education, Mother's edu<Father's edu, N=117; 4) High education, Mother's edu<Father's edu, N=64.

3.7 Appendix

Psychological Violence Questions

	Answer	WHO category
Does your husband spend free time with you?	Frequently/Sometimes/Never	NA
Is he affectionate with you?	Frequently/Sometimes/Never	NA
Does he ignore you, is he indifferent to you?	Frequently/Sometimes/Never	Controlling
Does he not allow you to study, work?	Frequently/Sometimes/Never	Controlling
Does he yell at you when he talks to you?	Frequently/Sometimes/Never	Emotional
Does he say things like "You are worthless" or similar phrases?	Frequently/Sometimes/Never	Emotional
Does he not allow you to see your friends/family?	Frequently/Sometimes/Never	Controlling
Does he tell you: "I am tired of you, I am thinking of leaving you"?	Frequently/Sometimes/Never	Emotional
Does he tell you: "If you leave me I will take our children"?	Frequently/Sometimes/Never	Emotional

Physical Violence Questions

	Answer
Has your husband or partner pushed you, hit you or attacked you physically?	yes/no
How often does your husband /partner attack you physically?	Frequently/sometimes

Table 3.9: Average effect of the BDH on all 6 psychological violence questions

	Doesn't allow you to see friends or family	Ignores you	Yells at you	Tells you your worthless	Threatens to leave	Threatens to take kids
Treatment	-0.05**	-0.03	-0.02	-0.03	-0.03	-0.03
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Observations	1332	1326	1333	1334	1332	1325

Marginal effects reported from probit models. Standard errors clustered at the parish level. * p<0.10, ** p<0.05, *** p<.01. All estimations control for mother characteristics (age, marital status, years of schooling, race) and household characteristics (father's years of schooling, asset index, urban, children 0-5 yrs, whether kitchen is used for sleeping, province indicators).

Table 3.10: Heterogeneous effect with respect to mother's education for all 6 psychological violence questions

	Doesn't allow you to see friends or family	Ignores you	Yells at you	Tells you your worthless	Threatens to leave	Threatens to take kids
Treatment	-0.03	0.03	0.05	0.02	0.04	-0.01
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)
High education X Treatment	-0.04	-0.12***	-0.14***	-0.09*	-0.11***	-0.04
	(0.04)	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)
High education	-0.10	0.05	0.18	0.07	0.35	-0.03
	(0.46)	(0.48)	(0.55)	(0.49)	(0.41)	(0.48)
Observations	1332	1326	1333	1334	1332	1325
Treatment for high education	-0.08**	-0.09**	-0.09***	-0.07*	-0.08***	-0.05
	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)

Marginal effects reported from probit models. Standard errors clustered at the parish level. * p<0.10, ** p<0.05, *** p<.01. High education refers to mothers with 7 years or more of schooling. All estimations control for mother characteristics (age, marital status, years of schooling, race), household characteristics (father's years of schooling, asset index, urban, children 0-5 yrs, whether kitchen is used for sleeping, province indicators), and the interaction of all control variables with an indicator for mothers with high education.

Table 3.11: Baseline comparison of treatment and control groups stratified by education groups-Full sample

	Mother's edu>=Father's edu						Mother's edu<Father's edu					
	Low education			High education			Low education			High education		
	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Domestic violence indicators												
- Emotional violence	0.54	0.52	0.62	0.56	0.48	0.16	0.56	0.59	0.62	0.49	0.45	0.71
- Controlling behaviors	0.37	0.43	0.21	0.39	0.38	0.96	0.41	0.50	0.20	0.20	0.37	0.08
- Physical violence	0.27	0.26	0.89	0.33	0.28	0.38	0.32	0.29	0.65	0.20	0.23	0.73

P-values are reported from t-tests of the equality of means for each variable between treatment and control arms for each of the four groups. Low education refers to mothers with less than 7 years of schooling and high education refers to mothers with 7 or more yrs of schooling. The sample is composed of all mothers at baseline. Only a fraction of mothers at baseline answered the domestic violence questions. The sample size for mothers who answered the domestic violence questions at baseline is the following for each of the four groups: 1) Low education, Mother's edu>=Father's edu, N=466; 2) High education, Mother's edu>=Father's edu, N=398; 3) Low education, Mother's edu<Father's edu, N=223; 4) High education, Mother's edu<Father's edu, N=100.

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