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Article

The Impact of Positive and Negative Income Changes on the Height and Weight of Young Children

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Abstract

We estimate the impact of changes in unearned income on the height and weight of young children in a developing country. As a source of income variation we use a change in the eligibility criteria for receipt of an unconditional cash transfer in Ecuador. Two years after families lost the transfer, which they had received for seven years, their young children weigh less and are shorter and more likely to be stunted than young children in families that kept the cash transfer. We find no statistically significant effect on young children's height and weight two years after gaining the cash transfer. Information on household expenditures suggests that a reduction of food expenditures by households that lost the transfer is the main mechanism behind this finding.

JEL classification: C31, H51, I14

In 2009, the government of Ecuador revised the poverty index that determines eligibility for the cash transfer that it provides to the 40 percent poorest households. Due to this revision, around 200,000 families lost the transfer, which they had received for seven years. A comparable number of families suddenly became eligible for the transfer, which they had not previously received. This article exploits the movement into and out of the cash transfer program to estimate the causal impact of changes in households' unearned income on the height and weight of young children in a developing country, two years after the change.

The impact of income fluctuations on the height and weight of young children is an important question for development policy. Lower height and weight compared to healthy children of the same age are commonly used as indicators of poor nutritional status and are associated with a range of undesirable economic and health outcomes later in life (de Onis and Blössner 1997).[1] Poor households spend a large

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1 Poor growth and smaller size in children are associated with increased risk of illness and mortality, impaired development, worse school performance and intellectual achievement, and reduced work capacity and economic productivity as adults (de Onis and Blössner 1997). Using Brazilian data, Thomas and Strauss (1997) estimate an elasticity of wages with respect to height of between 2.43 and 3.36 for urban males, controlling for educational achievement.

proportion of their incomes on food, and their incomes are characterized by strong uncertainty and frequent shocks (Banerjee and Duflo 2007). If negative income shocks have an adverse effect on children's nutrition, such shocks may have permanent effects on their health and on a range of later-life outcomes, which directly or indirectly depend on early health conditions (Grantham-McGregor et al. 2007; Martorell 1999).

The results of our study are also important for the design of cash transfer programs. These programs typically serve a limited share of the population. Every now and then, recipients' eligibility for the cash transfer has to be reconsidered. As was the case in Ecuador, this will result in some recipients losing the transfer and some former nonrecipients qualifying for the program. If movement out of the program has adverse effects on the health of children, it might be necessary to supplement movement out of the program by compensating measures.

There is a vast and growing literature on the long-term effects of shocks suffered during the "first 1000 days" (in utero and during the first two years of life). Almond and Currie (2011) survey the literature on the effects of fetal shocks on later life outcomes. They conclude that there is substantial evidence that insufficient nutrition and bad health during pregnancy affect not only later life health but also educational attainment and income. Currie and Almond (2011) review the literature on the effects of shocks early in life (including health shocks) on later life outcomes, concluding that effects are substantial and permanent.

Previous studies on the effect of negative income shocks on health take advantage of aggregate economic fluctuations and historical events to estimate causal effects. Most relevant to our study is Hidrobo (2014) who finds that children who were relatively more exposed to the Ecuadorian crisis (1998–2000) are shorter and have lower cognitive ability. Baird et al. (2011) find that short-term GDP fluctuations in developing countries are correlated with child mortality. Using rainfall shocks as a proxy for early-life income fluctuations, Maccini and Yang (2009) find effects on the adult health of Indonesian women. Papers using historical data include Van Den Berg et al. (2006), who, using data from 1812 to 1912, find that being born during a recession increases child mortality and affects adult health. Banerjee et al. (2007) find that a nineteenth century vine plague in France reduced the adult height of children of wine-growing families, and Lumey et al. (2007) find that individuals born during the Dutch hunger winter of 1944–45 have worse adult health outcomes.

There are a number of studies that use unconditional cash transfers as a source of positive income shocks. Duflo (2000) finds that South African girls whose grandmothers receive pension transfers have large improvements in weight and height, and Agüero et al. (2007) find that an unconditional South African child support grant increases child height. Implementing a randomized field experiment, Schady and Rosero (2008) find that Ecuadorian households increase the food share of their expenditures after receiving an unconditional monthly transfer of 15 US$.[2] Paxson and Schady (2010) using the same data, however, find only weak effects on the height and cognitive development of children who were 20 to 67 months of age at the start of the transfer.

There is also a large literature evaluating conditional cash transfers (Fiszbein et al. 2009). Because the transfers are often conditional on attendance of health care centers, it is difficult to disentangle the impact of the cash transfer from the effect of the conditionality. In a meta-study, Manley et al. (2013) find a small and nonsignificant effect for gaining access to a cash transfer on child height.

2 This contrasts with the common finding in the literature that the income elasticity of food consumption is less than one (e.g., Subramanian and Deaton [1996] and Strauss and Thomas [1990]). Schady and Rosero (2008) attribute this to the fact that the cash transfers are made to women. Recent studies of Angelucci and Attanasio (2013) and Attanasio and Lechene (2014) for Mexico and of Attanasio et al. (2012) for Colombia report a similar result for conditional cash transfers in these countries.

The stand-out feature of our article is that we can estimate the effects of both positive and negative income changes within a single framework. This is a novel contribution to the literature on the impact of income shocks on child health as it enables us to investigate whether positive and negative shocks affect child health symmetrically. We also contribute to the literature on the effects of cash transfer programs by being the first to analyze the effects of losing a cash transfer. Cash transfers are now widely used to fight poverty in developing countries. It is inevitable that over time some households will lose their transfers as they grow marginally richer or as eligibility rules are adapted. How losing a transfer affects the well-being of children in these households is therefore an important policy question.

In the subsample of households that did not receive the cash transfer before the revision, some households qualify for the cash transfer after the revision while other households do not. Using a regression discontinuity approach, this allows us to identify the effect of positive income changes. Likewise, in the subsample of households that did receive the cash transfer before the revision, some households do no longer qualify for the cash transfer after the revision while other households still qualify. Again using a regression discontinuity approach, this allows us to identify the effect of negative income changes. Our analysis is based on novel survey data containing information about the height and weight of almost 1400 children under the age of six years old, as well as on household expenditures including food expenditure.

Our key finding is that two years after families lost the cash transfer, which they had received for seven years, the young children in these families weigh less and are shorter and more likely to be stunted than young children in families that kept the cash transfer. We find no significant effect on young children's height and weight two years after their families started receiving the cash transfer, but confidence intervals are wide enough to include substantial positive effects. We also find that households that lost the transfer spend less on food than comparable households that kept receiving the transfer. The difference in food expenditures is similar to the size of the transfer. This suggests that a reduction of food expenditures by households that lost the transfer is the main mechanism behind the adverse effects of losing the transfer on children's height and weight.

The article proceeds as follows. Section I describes the Ecuadorian context and the cash transfer program. Section II describes the data and how these were collected. Section III explains the empirical strategy, and section IV confirms its validity. Section V lays out the main results, and section VI looks for pathways for the effects of income changes on child height and weight. Section VII concludes.

I. Context and Cash Transfer Program

Ecuador is a lower middle income country that has high poverty levels and high inequality. In sum, 72 percent of its population of around 14 million live in urban areas, 29 percent of the population are poor, and 26 percent are affected by chronic malnutrition.[3]

The cash transfer program is called Bono Desarollo Humano (BDH) and was launched in 2003. It is aimed at the poorest 40 percent of households. Initially, these families received a transfer of 15 US$ per month, which was increased to 30 US$ in 2007 and then to 35 US$ in 2009. The transfers are collected by the mother through local banks. Apart from guaranteeing a minimum level of consumption, the official aims are to increase human capital, reduce the persistence of poverty, and to reduce the levels of chronic malnutrition and preventable diseases for children below 5 years of age. For the households in our sample, which are located around the 40th wealth percentile, the transfer represents about 11 percent of household expenditure.

Most cash transfer programs in developing countries are conditional on some specified behaviors, usually including school attendance, nutritional supplements for children, and health clinic visits for

3 These figures are according to the government of Ecuador.

children. The BDH in Ecuador is different in that, while in theory recipients should send their children to school and to semi-annual health checks, these conditionalities were neither communicated in a sustained way nor effectively controlled or enforced.[4] However, public announcements made at the introduction of the cash transfer have had some effect on the perception of recipients. Schady and Araujo (2006) report that approximately a quarter of recipients believe that sending their kids to school is a requirement. In section VI, we will have a closer look at whether receiving the BDH increases the probability of health center visits for the children in our sample.

Eligibility for the BDH is determined exclusively by a household's percentile on a wealth index called SELBEN.[5] SELBEN is based on 27 variables including household assets and housing characteristics (access to water, toilet, shower, and household appliances), characteristics of the head of the household (schooling, employment), children's characteristics, and household size. It is calculated using nonlinear principal components analysis. The variables were collected through a census of all households living in poor areas. In 2007–2008, all households in poor neighborhoods were resurveyed and the definition of the index was changed (SELBEN II). The index is now composed of 59 variables covering the same areas (see Fabara [2009] for the complete list of variables).

For many households close to the threshold at the 40th percentile, this redefinition led to changes in eligibility. Around 200,000 households who had received the transfer for over seven years suddenly lost it while around 200,000 other household started receiving the transfer, which until then they had not received. While some change in eligibility had been announced through the media, individual families were not warned in advance and could therefore not prepare for the change. However, the transfer was phased out over the period of a few months, which gave families who lost it some time to adapt.

II. Data

For the data collection, we randomly sampled 2800 households from poor neighborhoods in three urban centers: Guayaquil, Quito, and Santo Domingo.[6] There are three main reasons why we focused on urban areas: First, the majority of the population of Ecuador (72 percent) lives in urban areas. Second, many people living on the countryside produce their own food making a direct link between income and nutrition less likely. And third, data collection is much more costly in remote rural areas. We had limited resources so we decided to concentrate on cities where (a) the impact is expected to be bigger and (b) we get more observations per dollar spent.

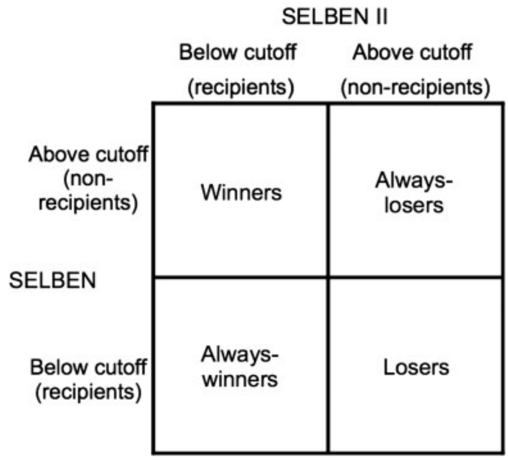
The sample frame consisted of the following households: 1. They scored within 0.3 standard deviations of the cutoff on SELBEN II; 2. they are single-core households so that our sample only contains households that, if eligible, receive the transfer exactly one time per month; 3. they are located in Guayaquil, Quito, or Santo Domingo; 4. they complied with their status of receiver or nonreceiver before the change was implemented (we used administrative data to only include households in our sampling frame that complied with their eligibility status before the change). On the basis of the eligibility statuses of the families before and after the revision of the wealth index we defined four groups (see figure 1): (i) the households who did not receive the transfer before the change and newly gained it (to whom we will refer as "winners"); (ii) those who used to receive the transfer and lost it ("losers"); (iii) those who received it before and after the change ("always-winners"); and (iv) those who never received it ("always-losers").

4 In 2012, well after we collected our data, the government eventually started random checks, which still only cover a small proportion of recipients.

5 SELBEN stands for Selection of Beneficiaries.

6 In population size these are the first, second, and fourth cities in Ecuador. According to the 2010 census, Guayaquil has 2.3 million inhabitants, Quito 1.6 million, and Santo Domingo 305,000. The country's third city, Cuenca, has 330,000 inhabitants. For logistical reasons, we chose Santo Domingo instead of Cuenca.

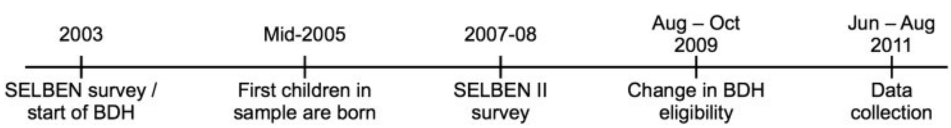
Figure 1. Treatment and Control Groups



The households were randomly sampled within each city while always picking one of each group from the same parish so that the four groups are balanced by parish. We used administrative data to ensure that all households in our sample were compliers before the change. Furthermore, we only picked single-core households so that our sample only contains households which, if eligible, receive the transfer amount one time per month.

Figure 2 shows the timing of the SELBEN surveys and our own data collection. The SELBEN II survey was executed in 2007–2008 and the change in eligibility was implemented between August and October 2009. Our survey thus took place approximately two years after the change. The households in our sample were visited by professional enumerators who were instructed to only conduct the interview with the mother of the house. In case of her absence, the enumerators were to revisit the household several times. In case of repeated absence, a random replacement was drawn from within the same parish. In the end, we received data from 2645 households. The households in our sample comprise 1445 children under six years of age. Every child in the sampled households below the age of six was measured and weighed using professional equipment. We obtained valid measurement of the height and weight of 1374 children.[7]

Figure 2. Timeline



In our analysis we use weight-for-age, height-for-age, and weight-for-height as outcome measures (de Onis 2006). These measures, which are the standard in the child growth literature, were created by the

7 In sum, 681 of these children are from families who initially did not receive the transfer, and 693 are from families who initially received the transfer. Table S2 in the online appendix (available at https://academic.oup.com/wber) reports the numbers of households and children in the different groups.

World Health Organization (WHO) to provide an international standard for detecting malnutrition in children.[8] Weight-for-age gives the difference in standard deviations between the weight of a child and the average weight of an international comparison sample. Height-for-age does the same for height. The height of a child is a cumulative measure of malnutrition. If a child is affected by malnutrition during the first few years of her life, she will fail to grow at a normal pace during that time. Although recent research shows that partial catch-up might be possible later on during childhood (Alderman et al. 2014), the consensus is that it is virtually impossible to fully catch up even if nutrition returns to optimal levels (Martorell 1999). The weight of a child is slightly harder to interpret. Low weight-for-age can either mean a child of normal height who has recently not eaten enough (acute malnutrition) or a child of short stature (who has previously been affected by malnutrition) with normal weight for her height. Finally, weight-for-height gives a more contemporaneous measure of malnutrition, comparing a child's weight to the weight distribution of children of the same height. Low weight-for-height is an indicator of current malnutrition. Children are especially vulnerable to malnutrition in utero and during the first three years of life (Martorell 1999), and malnutrition is thought to impact stature up to age five (de Onis and Blössner 1997).

Apart from measuring and weighing the children, the enumerators conducted a lengthy survey with the mother including questions about education, labor supply, household expenditures, personal well-being, and social indicators. We use information on food expenditures and health center visits to examine potential pathways for our results. We also have access to the data collected by the government through the SELBEN II surveys, most importantly the households' wealth index score, which we link to our own data to implement the regression discontinuity strategy.

Table 1 shows descriptive statistics of the anthropometric measures for the entire sample and for the four subsamples of winners, losers, always winners, and always losers. The average child in our sample is 0.78 standard deviations shorter and 0.37 standard deviations lighter than the average child in the WHO comparison sample, which is an indication of past malnutrition. On the other hand, the average child in the sample has almost the same weight as an average child of the same height, as indicated by the average weight-for-height. Underweight and stunting, defined by the WHO as a weight-for-age or height-for-age score below −2, respectively, are indicators for the most severe undernourishment (de Onis 2006). In our sample, 14 percent of children are stunted, and 6 percent of children are underweight.[9] There is no significant difference in any of the measures between transfer recipients and nonrecipients. This can either mean that the transfers have no impact or that the impact is offset by a positive correlation between wealth and the height and weight measures.

We did not conduct a baseline survey, but the variables contained in the SELBEN II survey can help us get a sense of the economic situation of the households in our sample prior to the eligibility change. Table 2 reports means and standard deviations for a range of characteristics separately for each group. The numbers illustrate that, although the households in our sample are not at the bottom of the wealth distribution, they are still poor and likely vulnerable to income shocks. Between 25 and 35 percent of houses have a roof, floor, or walls that are in a bad state. In sum, 20 to 30 percent do not have access to water, 55 to 65 percent do not have proper sanitation, and 60 to 70 percent do not have a shower.

8 We use the Stata do-files provided by the WHO to calculate the measures: http://www.who.int/childgrowth/standards/en/ (accessed February 26, 2016).

9 Overweight, defined as a weight-for-age score above 2, affects less than 3 percent of the children in our sample.

Table 1. Means and Standard Deviations of the Outcome Variables

		Combined sample		Winners vs always-losers		Always-winners vs losers	
	All children	Recipients	Non-rec.	Recipients	Non-rec.	Recipients	Non-rec.
Height-for-age	−0.78	−0.78	−0.78	−0.84	−0.84	−0.73	−0.73
	(1.32)	(1.31)	(1.34)	(1.31)	(1.34)	(1.30)	(1.34)
Weight-for-age	−0.37	−0.37	−0.36	−0.41	−0.39	−0.34	−0.33
	(1.12)	(1.11)	(1.14)	(1.07)	(1.11)	(1.14)	(1.17)
Weight-for-height	0.06	0.09	0.03	0.1	0.03	0.08	0.03
	(1.35)	(1.32)	(1.37)	(1.18)	(1.31)	(1.45)	(1.43)
Stunted	0.14	0.14	0.14	0.16	0.16	0.11	0.12
	(0.35)	(0.35)	(0.35)	(0.37)	(0.37)	(0.32)	(0.33)
Underweight	0.06	0.06	0.05	0.07	0.06	0.05	0.04
	(0.23)	(0.24)	(0.22)	(0.25)	(0.23)	(0.22)	(0.21)

Note: The table shows sample means of anthropometric measure by subgroup. Standard deviations are in parentheses.
Source: Authors' analysis based on own survey data.

Table 2. Background Variables from SELBEN II Survey

	Always-winners	Losers	Winners	Always-losers
Age	40.807	41.078	41.942	42.330
	(10.604)	(10.263)	(11.584)	(10.675)
Education	3.724	3.885	3.713	3.934
	(1.349)	(1.333)	(1.441)	(1.391)
Work	0.822	0.868	0.839	0.858
	(0.383)	(0.338)	(0.368)	(0.349)
Proper roof	0.693	0.772	0.658	0.715
	(0.462)	(0.420)	(0.475)	(0.452)
Proper floor	0.743	0.832	0.667	0.781
	(0.437)	(0.375)	(0.472)	(0.414)
Proper walls	0.740	0.842	0.719	0.798
	(0.439)	(0.365)	(0.450)	(0.402)
Water access	0.720	0.778	0.700	0.755
	(0.450)	(0.416)	(0.459)	(0.431)
Proper toilet	0.380	0.444	0.404	0.465
	(0.486)	(0.497)	(0.491)	(0.499)
Proper shower	0.282	0.427	0.296	0.403
	(0.450)	(0.495)	(0.457)	(0.491)
Household size	3.914	3.893	3.612	3.639
	(1.352)	(1.323)	(1.385)	(1.331)
Crowding	3.124	2.788	2.979	2.686
	(1.396)	(1.352)	(1.444)	(1.345)
Nr of children	1.957	1.838	1.687	1.628
	(1.096)	(1.122)	(1.205)	(1.157)
Nr of children below 6	0.543	0.495	0.522	0.483
	(0.718)	(0.695)	(0.720)	(0.674)
Nr of children below 2	0.139	0.127	0.139	0.122
	(0.359)	(0.342)	(0.359)	(0.327)

Notes: Age and education are age and education level of household head. Work is a binary indicator for whether the household head earns a labor income. Proper floor, roof, and walls are binary indicators for the state of the building where a value of 1 denotes a "good" or "normal" state. Water access is a binary indicator for whether the household is connected to the water network. Proper toilet and shower are binary indicators for the availability of a modern toilet or shower in the household. Crowding denotes number of household members per room. Standard deviations in parentheses.
Source: Authors' analysis based on SELBEN II dataset (Government of Ecuador).

III. Empirical Strategy

Sources of truly exogenous income changes are rare and often limited to specific populations (e.g., buyers of lottery tickets). The revision of the poverty index and the resulting changes in eligibility for receipt of the cash transfer, however, closely mimic exogenous income changes. Conditional on the eligibility status before the revision and the score on the new poverty index (SELBEN II), households that experience a change in their eligibility status are very similar to households whose eligibility status does not change. This creates the four groups we distinguish. Given data on households from all four groups in the vicinity of the new threshold, we can therefore use a regression discontinuity approach to estimate the effects of positive and negative income changes. Compliance with the assigned eligibility status is not perfect; not all household eligible for the transfer receive it, while some households not eligible do receive it. The regression discontinuity design in our setting is therefore fuzzy.

The fuzzy regression discontinuity design is essentially an instrumental variables approach in which a binary indicator Z for having a SELBEN II score below the cutoff is used as an instrument for receiving the monthly cash transfer. Additionally, we condition on a polynomial in the SELBEN II score (s), which is the forcing variable and, in some specifications, on a set of controls X. Comparing the winners to the always-losers yields estimates of the effects of a positive income change and is done by estimating the following equation using a sample of households that were not eligible before the revision (winners and always-losers):

$$Y_i = \alpha_{WvsAL} + \delta_{WvsAL} T_i + f_{WvsAL}(s) + \gamma_{WvsAL} T_i \cdot f_{WvsAL}(s) + X_i \beta_{WvsAL} + \varepsilon_i,$$

where Y is the outcome variable, T is a binary indicator for receiving the transfer, $f_{WvsAL}(s)$ is a polynomial function of s, and α_{WvsAL}, δ_{WvsAL}, γ_{WvsAL}, and β_{WvsAL} are parameters to be estimated. Similarly, comparing always-winners to losers yields estimates for the effects of negative income changes and is done by estimating the following equation using a sample of households that were eligible before the revision (always-winners and losers):

$$Y_i = \alpha_{AWvsL} + \delta_{AWvsL} T_i + f_{AWvsL}(s) + \gamma_{AWvsL} T_i \cdot f_{AWvsL}(s) + X_i \beta_{AWvsL} + \varepsilon_i,$$

where the parameters are potentially different from those of the previous equation. Imposing symmetry of the effects of negative and positive income changes ($\delta_{WvsAL} = \delta_{AWvsL}$), we can also use the combined sample to estimate the effect of more money versus less money:

$$Y_i = \alpha_{All} + \delta_{All} T_i + f_{All}(s) + \gamma_{All} T_i \cdot f_{All}(s) + X_i \beta_{All} + \lambda E_i + \varepsilon_i,$$

where E is a binary indicator for receiving the transfer before the change.[10] In the three equations, T is instrumented by Z, which is a binary indicator for being eligible for the cash transfer after the revision. We will present results for various specifications of $f(s)$.

Children are especially vulnerable to the effects of malnutrition in utero and during the first few years of life. We therefore also conduct the above analyses separately for children who were 24 months or younger at the time of our survey and those who were older than 24 months. This split is intuitive as it roughly separates the sample into those children who were affected by the income shocks before or around birth (and were therefore affected during their whole life) and those for whom the change happened at a later age.

IV. Validity of Regression Discontinuity Approach

For our estimation strategy to be valid, households must not be able to precisely manipulate the assignment variable (Lee and Lemieux 2009). A potential concern is that some households attempt to influence

10 This comes down to $\alpha_{All} = \alpha_{WvsAL}$ and $\lambda = \alpha_{AWvsL} - \alpha_{WvsAL}$.

their score on the poverty index by making themselves look poorer than they actually are. Such "manipulating" households have, conditional on their true poverty index, a higher chance to end up below the program's threshold. If the share of manipulating households is constant along the poverty index, the shares of manipulating households will be equal at either side of the threshold, and the RD approach is valid. If instead the share of manipulating households varies with the poverty index, the RD approach can still accommodate this, provided that the share of manipulating households is a smooth function of the poverty index. In that case the control function $f(s)$ does not only capture any direct relationship between the score on the poverty index and the outcome variable but also the effect of manipulation. If the share of manipulating households is not a smooth function of the poverty index, the RD approach is invalidated if discontinuities in the share of manipulating households coincide with the program threshold. This could occur if households know how the poverty index is constructed and know the location of the program threshold at the moment that they are visited by the enumerators that collect information for SELBEN II. Since neither the variables that are used for the construction of SELBEN II nor their weights were known when the data were collected and since the program threshold was determined ex-post, neither condition is fulfilled. As we can see from figure 3, which shows the distribution of SELBEN II scores for the whole population, there is indeed no indication of bunching.[11]

Another potential problem is noncompliance. Our sampling frame ensures that all households in our sample complied with the assigned eligibility before the change. A low rate of noncompliance after the change can easily be dealt with using the official assignment as an instrumental variable. However, if rates of noncompliance were very high (i.e., if a large proportion of winners fail to collect the transfer or a large proportion of losers continue to receive them), we would face the weak instrument problem. Having access to administrative data, we can determine exactly which families collected a transfer at their local bank.

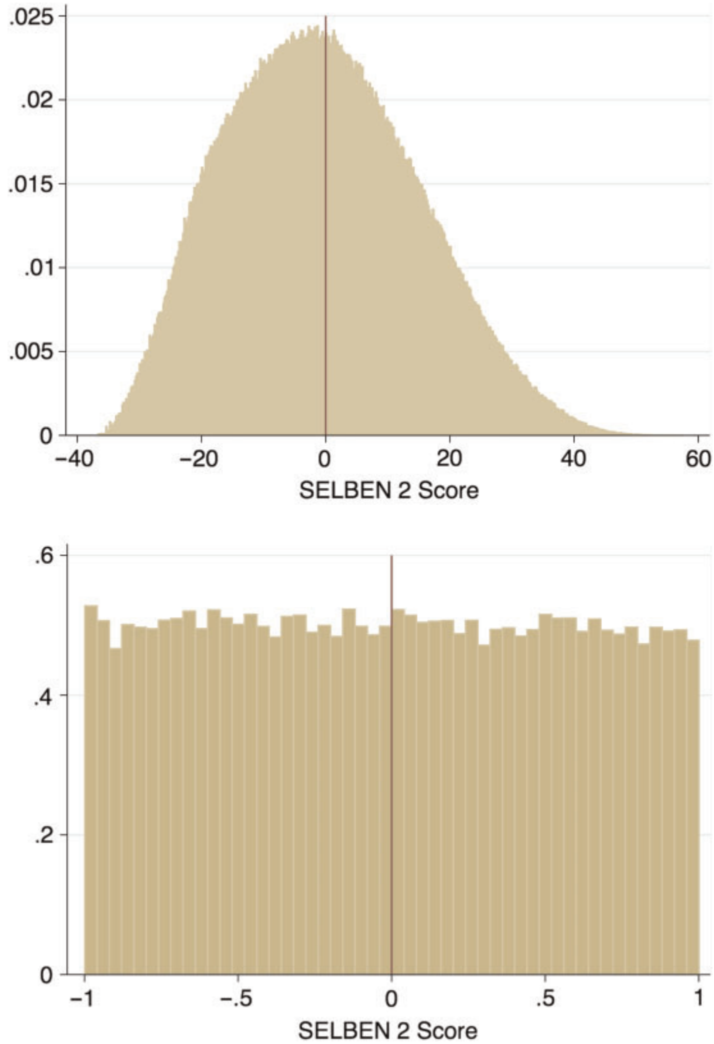
Figure 4 shows collection rates at the household level left and right of the cutoff for the three estimation samples (winners and always-losers, always-winners and losers, and the combined sample). Compliance rates are high: 97 percent of ineligible households do not collect the transfer, while 86 percent of eligible households collect their transfer. Collection rates are higher for always-winners, who were already collecting their transfer before the change, than for winners who are newly eligible. This can be explained with imperfect information. While the eligibility change was announced through the media, there was no personal communication with eligible households, and it was the responsibility of each household who thought they might be eligible to go to a local bank to check for themselves.

The online appendix contains further validity checks. Table S3 reports F-statistics for the first stage (i.e., regressions of a binary indicator of transfer collection on the assignment variable) controlling for first-, second-, or third-degree polynomials in the SELBEN II score for specifications with and without controls for children's gender and age. These regressions are calculated at the individual level using the sample of analysis of children below six years of age. The F-statistics are in all cases very high and well above the rule-of-thumb threshold of around 10 (Angrist and Pischke 2008).

In table S4, we test the assumption of no precise manipulation of the assignment variable by checking whether there is a discontinuity in background characteristics at the cutoff. We did not conduct a

11 When we conduct the McCrary (2008)-test on our sample, we reject the null hypothesis of equal densities near the threshold for Quito and Guayaquil. This is, however, not an indication of manipulation but is an artefact of our sampling scheme in combination with the distribution of the wealth index in these cities. In our sampling scheme we randomly draw equal numbers of observations from below and above the threshold. Because the distribution functions of the wealth index around the program threshold are increasing in Quito and Guayaquil, households just below the threshold have a higher probability to be sampled than households just above the threshold. The distribution function of the wealth index around the program threshold is flat in Santo Domingo. Consistent with that we do not reject the null hypothesis of equal densities near the threshold in Santo Domingo for our data.

Figure 3. Frequency Distribution of SELBEN II (Population)

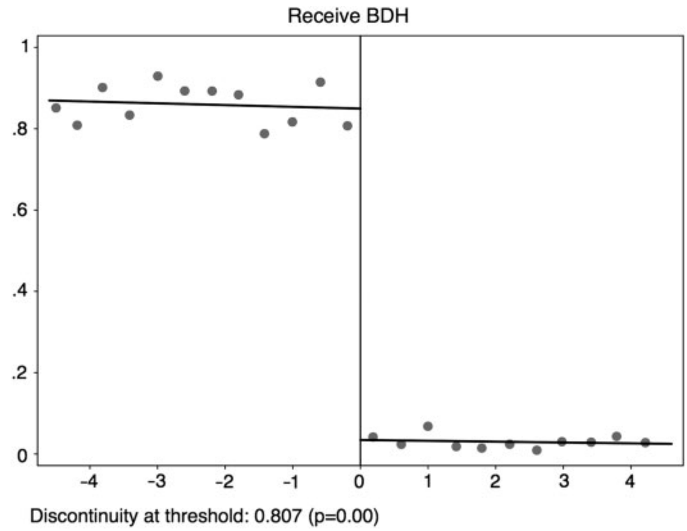


Note: The histograms are generated using the full SELBEN 2 database (2.175.512 households). The cutoff is normalised to zero.
Source: Authors' analysis based on SELBEN II dataset (Government of Ecuador).

baseline survey but we can use the variables contained in the SELBEN I survey. These include age and education level of the household head, a binary indicator for whether the household head earned an income at the time of the survey, indicators for whether the house has a proper floor, proper toilet, and access to a shower; the household size; and the amount of people per room. We report discontinuity estimates separately for the positive and negative income shocks controlling for first-, second-, and third-order polynomials in the assignment variable. None of the variables shows a consistently significant discontinuity.

A further potential worry is selective attrition. Not all the households that we randomly sampled using the SELBEN database could be found. Those who could not be found were replaced with randomly drawn households from the same treatment group and parish. One reason for not being located is the lack of official addresses in poor neighbourhoods, which meant that the enumerators had to rely on

Figure 4. First Stage



Notes: The figure shows the proportion of households who collect the BDH above and below the cutoff for the three estimation samples (winners and always-losers, always-winners and losers, and the combined sample). Observations are divided into bins with a width of 0.4 and the SELBEN II score is normalized to be zero at the cutoff. Households to the left of the cutoff are eligible to receive the transfer while those to the right are not.
Source: Authors' analysis based on administrative data (Government of Ecuador).

sometimes inadequate descriptions of the location of the household. A second reason is repeated absence of the mother. Another reason, however, is households having moved. If the likelihood of moving depends on the treatment and those who move differ from those who stay across relevant dimensions, this could lead to selection bias in our estimates of the treatment effects.

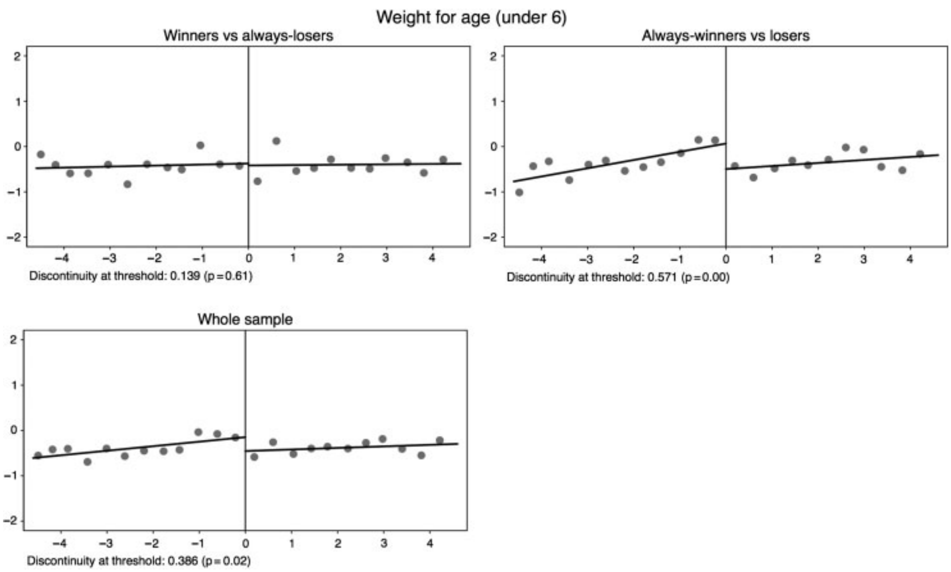
Table S5 shows the proportion of sampled households we could successfully locate. Compared to always-losers (those who never received the transfer), winners (those who suddenly gained the transfer) are less likely to have been located and therefore potentially more likely to have moved since the time of the SELBEN II survey. In sum, 75 percent of always-losers and 70 percent of winners were located. Although this difference is statistically significant ($p = .047$; chi-squared test), it is not very large in magnitude. Losers (those who lost the transfer) and always-winners (those who received the transfer before and after the change) are equally likely to have been located (77 percent vs. 75 percent; $p = .408$). There are no statistically significant differences in the rate of location in the replacement samples. In table S6, we check whether there are observable differences between located and unlocated sample households by looking at differences in average SELBEN I and II scores. Within each treatment group, households who could not be located (and who have therefore potentially moved) look very similar to those households who could be located. Also, the randomly drawn replacements look similar to the households they are replacing. Overall, this indicates that selection is not a big problem for our study.

V. Results

We will now use the regression discontinuity strategy to estimate the effects of winning and losing the cash transfer on child height and weight. Figures 5 to 7 show the relationship between children's anthropometric measures and the SELBEN II score of the children's households. The observations are divided into bins with a width of 0.4. Each dot represents the average outcome for the children in each bin. The solid lines are the best linear fits through the dots and are calculated separately below and above the cutoff. Below each graph we report an estimate of the discontinuity at the cutoff and its *p*-value. These estimates are from IV regressions, which allow for separate relationships between SELBEN II scores and outcomes on either side of the cutoff and include no control variables apart from the SELBEN II score. Each figure shows the effect of winning the transfer (winners vs. always-losers), losing the transfer (always-winners vs. losers), and for the combined sample, where we implicitly impose that the effects of winning and losing the transfer are symmetric (these regressions additionally control for earlier eligibility).

Figure 5 shows the effects on weight-for-age. The positive income change created by winning the cash transfer has no impact on child weight two years down the line. The negative income change experienced by households that lose the transfer, however, has a large and significant negative impact on child weight. Children from households to the right of the cutoff are more than half a standard deviation lighter compared to children who did not lose the transfer. The lower panel shows that the impact of the income changes is significant for the whole sample. Overall, weight is slightly positively correlated with wealth as measured by SELBEN II.

Figure 5. Impact of Income Changes on Weight-for-Age of Children Below 6 Years of Age

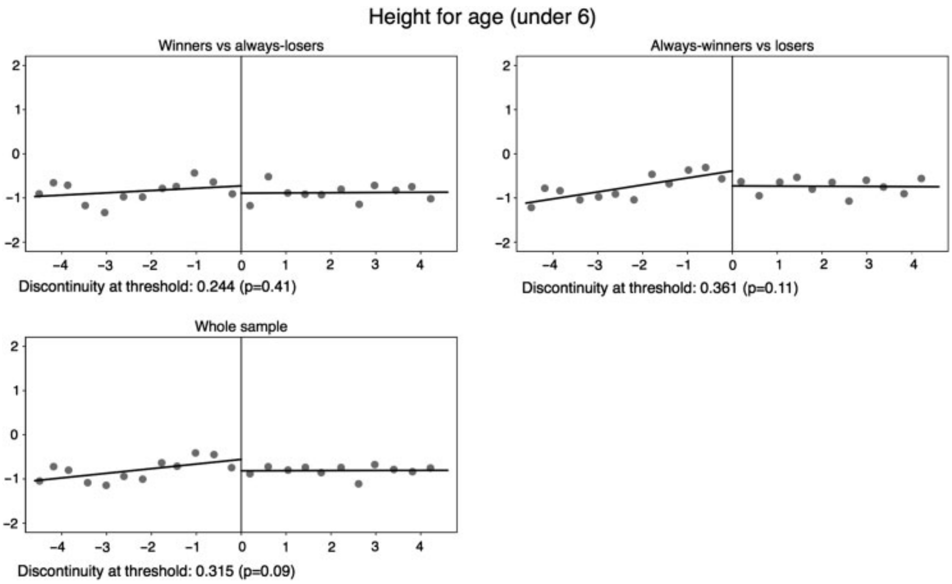


Notes: Observations are divided into bins with width 0.4. Dots represent average outcome in each bin. Solid lines are best linear fits through the dots, calculated separately below and above the cutoff. Discontinuities are from IV regressions, which allow for different slopes on either side of the cutoff and include no controls apart from the SELBEN II score.
Source: Authors' analysis based on own survey data.

Figure 6 shows the impact of the income shocks on height-for-age. We see effects in the expected direction for both positive and negative changes, neither of which, however, is individually significant. The combined effect for the whole sample is statistically significant and equal to 0.315 standard deviations when controlling linearly for the SELBEN II score. This means that over the two years since the change in eligibility, children from ineligible families have accumulated a height deficit relative to the children in eligible families.

Height-for-age is a long-term measure of malnutrition in the sense that a strong nutritional shock just after the eligibility change would be picked up even if children resumed eating normally thereafter.

Figure 6. Impact of Income Changes on Height-for-Age of Children below 6 Years of Age



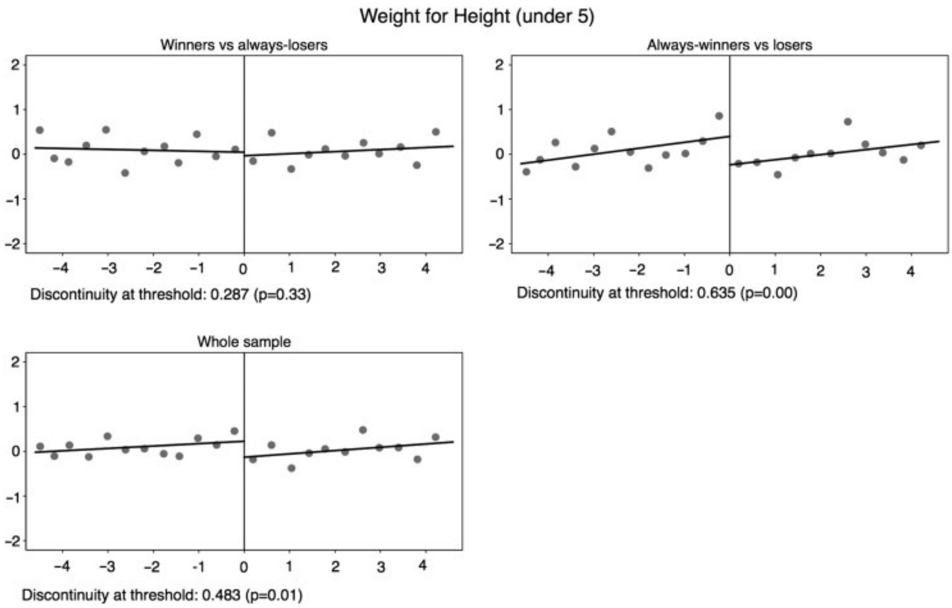
Notes: Observations are divided into bins with width 0.4. Dots represent average outcome in each bin. Solid lines are best linear fits through the dots, calculated separately below and above the cutoff. Discontinuities are from IV regressions, which allow for different slopes on either side of the cutoff and include no controls apart from the SELBEN II score.
Source: Authors' analysis based on own survey data.

Weight-for-age picks up both long and short-term effects. Children can be unusually light either because they are too short for their age or because they recently did not eat enough and are therefore too light for their height. This last point is reflected in the weight-for-height measure (figure 7), which picks up whether children are too light for their height, which would indicate recent malnutrition. We observe the same pattern as for weight-for-age, with no effect for children from positively affected households but a strong negative effect for children in negatively affected households. These results show that two years after the income change, children from families who lost the transfer are still undernourished and that the strong effects of negative income changes on weight are therefore not due solely to a height deficit accumulated in the months immediately following the change.

Six percent of the children in our sample are underweight, and 14 percent are stunted according to the WHO definition. Figure S2 in the online appendix shows the impact of the income shocks on the likelihood of being underweight. Given the low incidence of severe underweight in our sample, it is not surprising that the impact is not significant. Figure S3 in the online appendix shows the impact on the likelihood of being stunted. While the effect is not significant for the sample as a whole, children whose family lost their transfer are 12 percentage points more likely to be stunted, a substantial and significant difference.

We further explore these results in table 3, which shows the results of IV regressions, where eligibility for the cash transfer is used as an instrument for actual receipt of the transfer. We report regressions that control linearly for the forcing variables as well as regressions including a second- and third-order polynomial. All regressions allow for separate relationships between SELBEN II scores and outcomes on either side of the cutoff. In columns 1–3, we only control for the SELBEN II score (and in case of the full sample regressions for earlier eligibility). In columns 4–6, we additionally control for a third-order polynomial in age (measured in months) and a gender dummy. This should not significantly alter the coefficients, as our measures are already scaled for age and gender. Additionally, we report for each regression

Figure 7. Impact of Income Changes on Weight-for-Height of Children below 5 Years of Age



Notes: Observations are divided into bins with width 0.4. Dots represent average outcome in each bin. Solid lines are best linear fits through the dots, calculated separately below and above the cutoff. Discontinuities are from IV regressions, which allow for different slopes on either side of the cutoff and include no controls apart from the SELBEN II score.
Source: Authors' analysis based on own survey data.

the Akaike information criterion (AIC), which is useful for deciding the optimal order of the polynomial (Lee and Lemieux 2009). The preferred specification is the one with the lowest AIC.[12] This criterion as well as visual inspection of the graphs (and the fact that all our observations are very close to the cutoff) indicate that a linear control is superior.

The results confirm what we saw in the graphs. Children affected by a negative income change are significantly lighter compared to unaffected children of the same age and height. With or without controls, the effect is about half a standard deviation, which represents a large impact. These effects are also significant for the sample as a whole, but it is evident that they mainly stem from the negatively affected kids, while we do not find strong evidence for an effect of positive income changes on weight. The effect is also robust to the order of the polynomial.

The picture looks slightly different for height. Again, negatively affected children are shorter than their peers, but this effect is only significant in the linear specification and after the inclusion of controls. Positive income changes have a positive effect on height, but the estimate is much higher when we control for a second-order polynomial and not generally statistically significant. The effects are significant for the sample as a whole and are roughly equal to a third of a standard deviation. Finally, the weight-for-height regressions mirror the weight-for-age results with strong and significant effects for negative income changes and small and insignificant effects for positive changes. For all regressions, the AIC indicates that a linear control for the forcing variable is optimal.

Table S7 in the online appendix shows the results of equivalent analyses for the likelihood of being underweight and stunted. As indicated by the graphs, the probability of being severely underweight is

12 The AIC is defined as $N\ln(\hat{\sigma}^2) + 2p$, where $\hat{\sigma}^2$ is the mean squared error of the regression, and p is the number of parameters in the regression model.

Table 3. Impact of Income Changes on Child Height and Weight

	(1)	(2)	(3)	(4)	(5)	(6)	
Weight-for-age:							
Winners vs always-losers	0.139	0.428	0.430	0.136	0.412	0.476	N = 670
	(0.269)	(0.486)	(0.625)	(0.264)	(0.478)	(0.621)	
	[53.9]	[62.8]	[64.8]	[42.6]	[51.1]	[55.1]	
Always-winners vs losers	0.571***	0.893***	0.910***	0.612***	0.923***	0.926***	N = 672
	(0.186)	(0.271)	(0.311)	(0.185)	(0.274)	(0.315)	
	[37.7]	[50.2]	[54.7]	[27.9]	[40.8]	[44.8]	
Combined sample	0.386**	0.712***	0.700**	0.407**	0.726***	0.729**	N = 1342
	(0.161)	(0.257)	(0.308)	(0.159)	(0.256)	(0.311)	
	[88.5]	[109.7]	[111.9]	[68.6]	[90.0]	[93.7]	
Height-for-age:							
Winners vs always-losers	0.244	0.636	−0.170	0.238	0.593	−0.043	N = 670
	(0.296)	(0.543)	(0.671)	(0.279)	(0.512)	(0.644)	
	[134.1]	[144.3]	[138.7]	[79.3]	[88.8]	[83.7]	
Always-winners vs losers	0.361	0.368	0.289	0.439**	0.447	0.328	N = 672
	(0.227)	(0.334)	(0.369)	(0.220)	(0.327)	(0.358)	
	[112.7]	[116.6]	[118.3]	[82.1]	[85.9]	[86.9]	
Combined sample	0.315*	0.475	0.112	0.355**	0.505*	0.164	N = 1342
	(0.183)	(0.294)	(0.340)	(0.176)	(0.283)	(0.329)	
	[242.0]	[250.8]	[241.4]	[158.5]	[167.5]	[157.8]	
Weight-for-height:							
Winners vs always-losers	0.287	0.350	0.906	0.262	0.315	0.789	N = 571
	(0.298)	(0.543)	(0.701)	(0.297)	(0.531)	(0.683)	
	[101.3]	[105.9]	[123.1]	[96.1]	[100.5]	[114.8]	
Always-winners vs losers	0.635***	1.077***	1.169***	0.625***	1.050***	1.156***	N = 562
	(0.215)	(0.334)	(0.374)	(0.212)	(0.331)	(0.365)	
	[82.3]	[93.2]	[98.0]	[80.3]	[90.7]	[95.7]	
Combined sample	0.483***	0.787***	1.034***	0.466***	0.762***	0.998***	N = 1133
	(0.180)	(0.298)	(0.360)	(0.180)	(0.295)	(0.355)	
	[180.6]	[196.0]	[213.3]	[175.7]	[190.5]	[206.9]	
1st-order polynomial	√			√			
2nd-order polynomial		√			√		
3rd-order polynomial			√			√	
Age and gender cont.				√	√	√	

Notes: Coefficients are from IV-regressions of transfer receipt on the outcome variable. Controls include a third-order polynomial in the child's age (measured in months) and a gender dummy plus a polynomial in the SELBEN II-score. All regressions using the combined sample control for earlier eligibility. Standard errors are in parentheses and Akaike's information criterion in brackets; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$; standard errors are clustered at the household level.

Source: Authors' analysis based on own survey data.

not significantly affected by the cash transfer. On the other hand, children from families who lost the transfer are significantly more likely to be stunted. This effect amounts to 12–14 percentage points when controlling linearly for the forcing variable (the preferred specification according to the AIC).

As mentioned earlier, children are especially vulnerable to the effects of malnutrition in utero and during the first few years of life. Our sample is not large enough to allow for a large number of splits, but in table 4 we conduct the above analyses separately for children who were 24 months or younger at the time of our survey and those who were older than 24 months (see also figures S4 to S6 in the online appendix). This split is intuitive as it roughly separates the sample into those children who were affected by the income shocks before or around birth (and were therefore affected during their whole life) and those for whom the change happened at a later age.

Table 4. Impact of Income Changes on Child Height and Weight by Age

	24 months and younger:			Older than 24 months:		
	(1)	(2)		(3)	(4)	
Weight-for-age:						
Winners vs always-losers	0.419	0.384	N = 196	0.006	0.015	N = 474
	(0.448)	(0.441)		(0.303)	(0.300)	
	[20.4]	[11.7]		[34.6]	[33.1]	
Always-winners vs losers	1.238***	1.402***	N = 214	0.264	0.263	N = 458
	(0.342)	(0.332)		(0.215)	(0.211)	
	[38.1]	[34.4]		−[2.1]	−[5.1]	
Combined sample	0.897***	0.906***	N = 410	0.151	0.156	N = 932
	(0.281)	(0.275)		(0.182)	(0.181)	
	[57.0]	[44.7]		[28.4]	[25.7]	
Height-for-age:						
Winners vs always-losers	0.468	0.463	N = 196	0.124	0.085	N = 474
	(0.535)	(0.499)		(0.337)	(0.329)	
	[55.8]	[40.1]		[41.8]	[39.2]	
Always-winners vs losers	1.113**	1.378***	N = 214	0.007	0.027	N = 458
	(0.439)	(0.399)		(0.261)	(0.262)	
	[59.6]	[47.4]		[41.8]	[39.7]	
Combined sample	0.859**	0.950***	N = 410	0.058	0.059	N = 932
	(0.347)	(0.325)		(0.210)	(0.208)	
	[114.2]	[85.4]		[78.2]	[73.9]	
Weight-for-height:						
Winners vs always-losers	0.173	0.115	N = 196	0.342	0.360	N = 375
	(0.547)	(0.562)		(0.323)	(0.319)	
	[52.3]	[51.3]		[47.0]	[46.7]	
Always-winners vs losers	0.915***	0.996***	N = 214	0.487*	0.439*	N = 348
	(0.347)	(0.344)		(0.257)	(0.255)	
	[49.3]	[48.6]		[36.1]	[33.4]	
Combined sample	0.594*	0.571*	N = 410	0.424**	0.406**	N = 723
	(0.311)	(0.314)		(0.205)	(0.204)	
	[96.1]	[94.5]		[78.7]	[76.4]	
1st-order polynomial	√	√		√	√	
Age and gender cont.		√			√	

Notes: Coefficients are from IV-regressions of transfer receipt on the outcome variable. Controls include a third-order polynomial in the child's age (measured in months) and a gender dummy plus a polynomial in the SELBEN II-score. All regressions use the combined sample control for earlier eligibility. Standard errors are in parentheses and Akaike's information criterion in brackets; $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$; standard errors are clustered at the household level.

Source: Authors' analysis based on own survey data.

The IV regression results show that the effects are indeed concentrated among the younger subgroup. While the effects of the income change on weight-for-age, height-for-age, and weight-for-height are large and significant for the younger children, they are much weaker and mostly insignificant for the older children. As in our analyses using the whole sample, the impact of the negative income shock is much stronger than the impact of the positive shock. For the younger subsample, the effects are highly significant and large in magnitude. When controlling linearly for the forcing variable, children from families who lost the transfer are 1.2–1.4 standard deviations lighter than their peers who did not lose the transfer. They are also 1.1–1.4 standard deviations shorter and have 0.9–1.0 standard deviations lower weight-for-height. The estimated effects are even higher when controlling for a second-order polynomial in the forcing variable.[13] For the

13 We realize that the estimated effects are very large. After presenting results about possible pathways, we discuss in the final section how plausible effects of these magnitudes are.

older subsample, effects are generally much smaller and not significant. Only the effects on weight-for-height are marginally significant for the whole sample. Again, with the one exception of the height-for-age regressions for the younger subsample, the AIC point toward using a linear control for the forcing variable.[14]

A final important consideration is in which part of the height and weight distribution the estimated effects occur. This is especially interesting for weight, where obesity has recently become an important health problem in some middle-income countries. Does losing a cash transfer increase the amount of children with a weight below the international average or rather decrease the amount of children who already weigh more than they should? We explore this question in table S10 in the online appendix by applying our regression discontinuity strategy to binary variables indicating a child has a height-for-age or weight-for-age score below a certain threshold. By running regressions for a series of indicators of z-scores from -2 to 2 (in steps of 0.5), we get a clear picture of where in the height and weight distributions our effects occur.

As indicated from the regression results above, only the negative shocks have significant effects. For the sample as a whole, the effect of losing money shifts the entire distribution of weight-for-age: losers are both significantly more likely to have a weight-for-age score of below -1.5 and below 1.5. The effects on height-for-age, on the other hand, seem to occur more at the lower end of the distribution. For the subsample of children who were affected by the income change before or around birth, the effects on both weight-for-age and height-for-age occur across the entire distribution.

As a further robustness check, in table S11 in the online appendix, we report coefficients from our main specifications using a more narrow bandwidth around the cutoff. The table reports results from regressions using the whole sample of children under six years of age using 75 percent, 50 percent, and 25 percent of the full distance of 0.3 standard deviations left and right of the cutoff. The coefficients are stable across sample widths. Using 50 percent of the original sample width, the effects of losing the transfer on weight-for-age is still significant at the 1-percent level as is the effect on weight-for-height. The effects for the whole sample are significant at the 5 and 10-percent levels, respectively.

VI. Potential Pathways

In this section we look at potential mechanisms for the effect of income changes on child height and weight. First of all, we will check whether receiving the transfer has an impact on the likelihood that a child receives a health check. We then turn to the most straightforward explanation for our results: that families adjust their food expenditures when confronted with income changes. We also look at mothers' labor supply and mothers' mental wellbeing as potential mechanisms.

Health Checks

The BDH is an unconditional cash transfer in the sense that, at least until recently, there was no serious attempt to enforce any conditions. However, it was announced from the beginning that recipients are supposed to enroll children above five in school and take children below five for a free health check at least once every six months. Although these conditionalities were never enforced, families may still feel compelled to comply. The first conditionality is irrelevant for our study as all children in our sample are

14 Because malnutrition has a persistent effect on height-for-age if it occurs during the first 24 months of a child's life and a much weaker effect if it occurs after 24 months, we can run the following robustness check to further eliminate differences between recipients and nonrecipients that are not picked up by the SELBEN II score. Using only children who were affected from birth and those who were at least 24 months when the change happened, we can run a dif-in-dif estimation, regressing height-for-age on age-group, transfer eligibility and the interaction of the two (controlling for the SELBEN II score). The coefficient on the interaction of being young with transfer eligibility is equal to 0.405 (p = .378) for the winners vs. always-losers sample, 0.955 ($p = .054$) for the always-winners vs. losers sample and 0.698 ($p = .039$) for the whole sample. The results are therefore robust to this alternative strategy.

five years or younger. But health checks might lead to an earlier detection of malnutrition and increase parents' awareness of the importance of nutrition for the health and development of their children.

Figure S1 in the online appendix shows the likelihood of having received a health check during the past six months for children on both sides of the cutoff. Children from both eligible and ineligible households are quite likely to have had at least one health check during the previous year (approximately 60 percent of children in our sample). The difference in the likelihood of having gone for a health check between children from eligible and ineligible families is approximately 13 percentage points. This difference is significant for the sample as a whole. The figure also shows results for children who were affected before or around birth, the group for which we find the biggest impact of the income shocks. The graphs for this age group show that health checks cannot explain our findings: children whose families are eligible are neither more nor less likely to have gone for a health check. Table S12 in the online appendix shows IV regressions, which confirm these results. The regressions also show that the impact for the sample as a whole is not robust to controlling for a second- or third-degree polynomial in the forcing variable. We note, however, that the estimates are fairly imprecise so that we can also not completely exclude this channel.

Food Expenditure

The average monthly expenditure of households in our sample with children below six is 320 dollars. Of this amount, 196 dollars are spent on food (see table 5). This amounts to a food share of 61 percent. All the households in our sample are from urban areas and consequently own food production plays a minor role. Households on average only invest two dollars in food production and 93 percent of households invest nothing. This makes it very likely that an income shortfall will have to be at least partially covered by a reduction in food expenditures.

Table 5. Household Expenditure in US$

	Mean:	SD:
Food	196	159
Food production	2	16
Food outside the house	7	20
Rent, housing, utilities	35	33
Education	42	53
Health	34	71
Alcohol, cigarettes	3	11
Other expenses	30	127
Total expenditure	320	159

Source: Authors' analysis based on own survey data.

The top panel of table 6 shows estimates of the impact of the income transfer on household expenditures on food for households with children below six years of age (see also figure S7 in the online appendix). Households that receive the transfer spend on average 31 dollars more on food than households that do not receive the transfer (column 1 for the Combined sample). The effect of losing the cash transfer on food consumption controlling for household size is equal to 16 dollars for the whole sample and 29 dollars for the households with young children. The IV regressions also show an impact of 23 dollars of the positive income shock on food expenditure when controlling for household size, although this effect is not significant for households with children below six years. The effect of the income changes on food expenditure is significant for the sample as a whole and equal to 21 dollars when controlling for household size. On the other hand, there is not much of an impact of the income change on nonfood

Table 6. Impact of Income Changes on Household Expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	
Food expenditure (families with children <6):							
Winners vs always-losers	10.969	42.029	58.901	23.046	57.866	88.726*	N = 551
	(22.610)	(38.303)	(50.557)	(21.447)	(37.160)	(50.584)	
	[2516.0]	[2524.7]	[2533.8]	[2470.8]	[2484.9]	[2505.5]	
Always-winners vs losers	46.140**	37.238	51.514*	29.215*	18.161	26.262	N = 568
	(19.773)	(28.876)	(29.444)	(17.408)	(26.180)	(28.142)	
	[2610.2]	[2612.3]	[2615.5]	[2523.5]	[2524.9]	[2528.4]	
Combined sample	31.411**	39.321*	54.110**	29.857**	38.587*	56.136**	N = 1119
	(15.035)	(23.395)	(27.461)	(13.789)	(21.945)	(26.621)	
	[5124.5]	[5130.6]	[5141.1]	[4997.5]	[5004.4]	[5019.5]	
Nonfood expenditure (families with children <6)							
Winners vs always-losers	−3.169	−3.601	−19.072	0.423	1.122	−10.059	N = 551
	(26.952)	(48.617)	(65.314)	(26.624)	(47.932)	(64.247)	
	[2644.0]	[2647.9]	[2649.8]	[2642.2]	[2646.3]	[2648.4]	
Always-winners vs losers	0.396	18.441	42.852	−4.865	12.580	35.093	N = 568
	(19.505)	(24.462)	(29.572)	(19.114)	(24.485)	(29.838)	
	[2675.0]	[2679.4]	[2685.4]	[2669.3]	[2673.6]	[2679.2]	
Combined sample	−0.725	8.367	18.148	−1.200	8.144	18.761	N = 1119
	(16.394)	(25.441)	(32.409)	(16.235)	(25.151)	(32.059)	
	[5316.8]	[5322.8]	[5329.6]	[5309.2]	[5315.3]	[5322.5]	
1st-order polynomial	√			√			
2nd-order polynomial		√			√		
3rd-order polynomial			√			√	
Household size cont.				√	√	√	

Notes: Coefficients are from IV-regressions of transfer receipt on the outcome variable. Controls include household size plus a polynomial in the SELBEN II-score. All regressions using the combined sample control for earlier eligibility. Standard errors are in parentheses and Akaike's information criterion in brackets; $*p < 0.10$, $**p < 0.05$, $***p < 0.01$; standard errors are clustered at the household level.

Source: Authors' analysis based on own survey data.

expenditure; controlling for household size, the effect for the whole sample is virtually zero (see bottom panel of table 5).

The estimated effects of the cash transfer on food expenditures for the whole sample are quite close to the monthly transfer of 35 dollars. This suggests that the households permanently adjust their food consumption following the change in unearned income. This is especially striking for the families who lost the transfer. Even two years after the change, they are not able to make up for even part of the shortfall in income, and the adjustment is done almost entirely via a reduction in food expenditure. This is reflected in the fact that both long-term and short-term measures of malnutrition are significantly affected by the negative income change. Plausibly, households who gain the transfer also spend a large part of it on food but invest in tastier rather than more nutritious food.[15]

Our food expenditure results are in line with Schady and Rosero (2008) who find that the BDH increases the food share of the expenditure of recipients, in other words, that recipients spend a larger share of the transfer on food than is the case for the rest of their income (albeit for a transfer of 15 US$ and for poorer recipients). A potential reason for this difference in how the money is spent is that the

15 We also estimated regressions with the log of expenditures as the dependent variables. This does not change the precision or the stability of the estimates. We report results in terms of dollars as this allows for the easiest comparison with the size of the cash transfer.

BDH is received by women who might therefore be in control of how to spend it. There is evidence that the nutrition of children increases in the budget share controlled by the mother (Thomas 1990; Hoddinott and Haddad 2011).

Mothers' Labor Supply and Mental Well-Being

A further possibility is that cash transfers have an impact on child health via an impact on the mother. Specifically, we will look at whether mothers who receive the transfer reduce their working hours and therefore have more time to take care of the children and whether mothers who have less money suffer more often from depression.

Figure S8 in the online appendix shows hours worked for mothers on either side of the cutoff, both for the sample as a whole and for mothers of children below six years of age. We can see that if anything, mothers who receive the transfer work more. This is confirmed by the IV regressions in table S13 in the online appendix.

We also measured mothers' mental wellbeing using the CESD depression scale.[16] Figure S9 in the online appendix shows mothers' depression score left and right of the cutoff. There is no significant impact for any specification or subgroup as confirmed by the results in table S14 in the online appendix. This makes it unlikely that a decrease in mothers' mental wellbeing can explain the poor health outcomes of young children in families that lost the transfer.

VII. Discussion and Conclusion

In this article we study how changes in unearned income affect the health of young children raised in poor families in a lower middle income country. In particular, we exploit a recent change in cash transfer eligibility in Ecuador to estimate how the height and weight of children respond to unanticipated movements either into or out of an unconditional cash transfer program. Using self-collected survey information on families close to the new eligibility cutoff, we find that children in families that receive the transfer are taller and weigh more. The differences are large and statistically significant. We also find that these cash transfer effects are largely driven by families who lost the cash transfer before or around the time of birth of their children.

We then explore possible mechanisms to explain why losing the cash transfers is so harmful for very young children; among these are more restrictive health care access, reduction in food expenditures, and reductions in maternal care (in both quantity and quality). Our findings suggest that food expenditures play an important role; that is, we find that the amount families save on food expenditures upon losing the transfer is almost as large as the cash transfer itself.

With the data at our disposal it is impossible to determine the exact origins of the asymmetry in the effects of positive and negative income shocks on child height and weight. Food expenditures make up almost two thirds of the expenditures of the families in our sample. They might also be more easily adjustable downwards than costs for rent, health, and education, which make up most of the rest. Moreover, if before the change the families in our sample were at a margin where food intake was just about sufficient, we would expect larger effects in the negative direction. But it is also important to note that the confidence intervals on our estimates of the effects of positive shocks are quite wide and cannot exclude positive effects of large magnitudes.

The negative effects that we find for the children that are most affected are sizable. Children under two years old in families that lost the cash transfer are on average around 2.9 kilos lighter and

16 For the sake of brevity, instead of using the full 20-questions scale we used the data of data of Rosero and Oosterbeek (2011) to choose the eight variables that best explained the final score. See http://cesd-r.com/cesdr/ (accessed February 26, 2016) for a complete list of question items.

7.5 centimeters shorter than children under two years old in families that kept receiving the cash transfer. Are effects of such magnitudes plausible? For many of these children, their families lost the cash transfer when they were still in utero, and the loss of the cash transfer translated into a reduction of families' food expenditures of around 25 percent. Due to a lack of credible estimates of the effects of similar interventions, it is difficult to benchmark our results. The vast literature on conditional cash transfer programs is not comparable since it focuses exclusively on the effect of receiving a transfer, not on the effect of losing it.

A number of studies present estimates to which we can compare our findings. Almond and Mazumder (2011) estimate the effect of being exposed to Ramadan while in utero on children's birth weight. They find that children are 20 grams lighter at birth when the pregnancy of their Arab mother overlapped with Ramadan. Ramadan lasts one month and prescribes fasting during daylight but pregnant women can postpone this obligation. If we assume: i) that 50 percent of the pregnant Arab mothers actually observe the fast, ii) that the effect of fasting is equivalent to the effect of a 25 percent reduction in food consumption, and iii) that the effect of nine months equals nine times the effect of one month, losing the cash transfer at the beginning of a pregnancy then reduces the weight of a new born by 360 grams. Almond and Mazumder (2011) and Van Ewijk (2011) also document large negative effects of an overlap of pregnancies and Ramadan in Muslim communities on the long-term health outcomes of children. This indicates that the lower birth weight is not undone afterward.

Another study to which we can compare our results is Hidrobo (2014). Using a difference-in-differences approach she estimates that one year of exposure to the 1998–2000 crisis in Ecuador decreased height for age scores by 0.1 of a standard deviation. The effect is much larger when the child was between 12 and 17 months during the year of exposure. To benchmark her results, Hidrobo (2014) refers to results from a study by Alderman et al. (2006), who find that a drought in Zimbabwe is associated with a 0.58 decrease in children's height for age z-score. Finally, using rainfall variation in rural Tanzania as a proxy for income changes, Bengtsson (2010) finds that a transitory 10 percent drop in household income reduces the contemporary body weight of young children by 0.4 kg for girls and 0.2 kg for boys.

While it is hard to compare the 1998–2000 crisis in Ecuador, droughts in African countries, and exposure to Ramadan while in utero with the 25 percent reduction in food expenditures caused by the loss of the cash transfer, the estimates of the health effects of these other events suggest that effects of the magnitudes we find are not completely unrealistic. Another study that finds height effects of similar magnitude is Attanasio et al. (2013) who investigate the effects of a pre-school program in Colombia.

Low child height and weight indicate insufficient nutrition, which has negative effects on later life health and productivity. Income uncertainty is widespread for poor people in developing countries. Our results show that negative income changes have a detrimental effect when they hit during pregnancy or just after birth. Food supplements or cash transfers for pregnant and breast-feeding mothers therefore appear to be a possible policy opportunity.[17] While costs would be low and limited in time, the positive effects would accrue over the child's whole lifetime. The same applies to slightly older children, possibly combined with nutritional advise and regular health check-ups to detect malnourishment early on.

There is no reason to expect that the effects we estimate are unique to reductions in income caused by the loss of a government transfer. Negative income shocks due to bad health, bad weather, natural disasters, or macroeconomic fluctuations probably have similarly devastating health effects on affected children. This indicates that income insurance for poor families could have long-term positive health effects.

17 Cash transfers might be a more efficient way of achieving the same aim. Hidrobo et al. (2014) find that while food transfers increase calories consumed more than cash transfers or vouchers, they are also less cost effective. Cunha (2014) finds that cash transfers are a more efficient way of increasing food intake compared to in-kind transfers.

Nevertheless, our results are of relevance to the design and management of (conditional) cash transfer programs. The families in our sample are situated around the threshold and the effects we estimate are therefore informative about the impact of marginal adjustments to eligibility criteria. For example, the Ecuadorian government is currently considering lowering the wealth threshold to pay for more generous transfers. Our results indicate that this policy change might have detrimental effects on the long-term health of young children in the affected families if no compensatory measures are taken.

It is also important to know what happens if families graduate from cash transfer programs as they become somewhat wealthier. Our results indicate that it might be better to gradually reduce transfers with increasing wealth rather than handle a strict cutoff at which a family suddenly, and potentially unexpectedly, loses the entire transfer. A longer phase-out period is also a possibility but may not have the desired effect, as our results indicate that even two years after the change in eligibility, household expenditure has not yet recovered.

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