Migration, the financial crisis, and child growth in rural Guatemala

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Abstract

Migration has been demonstrated by various studies to be closely linked to improvements in individual- and household-level outcomes. It is difficult to identify with confidence the causal effects of migration, however, and likely that some of the observed differences are related to selection. In this paper, rather than examining the "positive" effects of migration, we put the spotlight on how the global financial crisis emanating from the U.S. negatively affected migrant households in rural Guatemala. Treating the financial crisis as a natural experiment affecting migrant and non-migrant households differently, we examine the effect on child anthropometry. Panel data on children, and multiple children in households allow a triple difference estimation in which we control not only for additive selection effects related to migration, but also differences in household-level trends over time. Migrant households fared much worse, in relative terms, than non-migrant households over the period. In particular, our assessment demonstrates that substantial advantages in child anthropometric status for the youngest children in migrant households in 2008, just prior to the crisis, were in large part lost only four years later. This finding emphasizes the potentially tenuous nature of the benefits of migration.

Keywords: Migration, child growth, stunting, financial crisis, Guatemala

JEL Codes:

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1. Introduction

In recent decades, migration and remittances have grown dramatically, becoming important contributing factors to economic growth and development worldwide and there is substantial research investigating the effects on individuals and households in sending countries. The recent global financial crisis, however, temporarily sidetracked those trends with job losses, lower earnings, and changes in migration patterns, resulting in large reductions in remittance flows. In 2008, remittances to Latin America totaled $65 billion, making up more than ten percent of gross domestic product (GDP) in many Latin American and Caribbean countries (Mohapatra et al., 2010). During the third quarter of 2008, remittance flows began to slow, however, declining -11.8 percent to $57 billion in 2009 (World Bank, 2013). Since then, remittances have recovered steadily, reaching $62 billion in 2012, still below their 2008 peak (World Bank, 2013).

In Guatemala specifically, the financial crisis resulted in a 9.3 percent decline in remittance inflows in 2009 (Funaro and Treadway, 2012). Reduced economic transfers had serious repercussions for households that had previously relied on additional remittance income for consumption and investment purposes. In addition, there was an overall slowing of macroeconomic growth, as reflected by a fairly flat rate of GDP per capita growth from 2008-2011.

In this paper, we assess the effects of the crisis beyond the U.S. borders, on rural households in Guatemala, where migration to the U.S. is common and remittances form a substantial proportion of local incomes. Using household panel data, collected in 2008, prior to the crisis, and again in 2012 (three years into the crisis at a point when there were signs of recovery) we focus on relative changes in outcomes for households that had migrants at the baseline compared to those that did not.

After demonstrating that there were substantial decreases in migration, remittances, and expenditures, consistent with the crisis in the U.S. which particularly affected industries in which Guatemalan migrants often work, we focus on early childhood growth, which has been shown to have long-term consequences for human development. Identification rests on the exogenous nature of the financial crisis (from the perspective of rural Guatemalan households), the physiology of child growth, and controls for individual-level fixed-effects and household-level time-varying effects.

In 2008, young children in households with migrants in the U.S. had substantially higher nutritional status than their counterparts in households without migrants. From 2008 to 2012, however, as the economic crisis in the U.S. took its toll on industries and sectors heavily populated by the migrants, those same children lost substantial ground relative to their counterparts in non-migrant households.

The apparent benefits of migration to those children, as measured by height, were in large part, though not completely, lost after four years.

2. Literature Review

[INCOMPLETE] Relevant literatures on identifying effects of migration (Hildebrandt and McKenzie 2005; McKenzie et al. 2006; Yang 2008) and role of migration and remittances in mitigating shocks (Yang and Choi 2007). Also links to literature on effect of aggregate shocks (Yang 2008; Frankenberg et al, 2003; McKenzie 2003; Ferreira and Schady, 2009). Small literature on effect of crisis (Duryea and Morales 2011; Inchauste and Stein 2013 and papers within). Also linked to more recent work showing how destination country GDP shocks have important migration reducing consequences (McKenzie et al, 2014). Our paper at intersection of migration (especially insurance motive) and general crisis literatures, and is not unlike a form of heterogeneity effects analysis.

Carletto et al. (2011) present evidence that migration in this sample in 2008 was associated with better child nutritional status, which under certain assumptions they interpreted as an impact of migration. Those findings, however, were limited by the cross-sectional nature of the data. In this paper, we implement a related identification strategy, extended to panel data. The migration indicator, however, captures something different in the case of the 2008 to 2012, with the global downturn. Whereas migration was largely a benefit prior to this period, the migration indicator during the downturn largely reflects the impact of the crisis (see Section 4 for identification strategy).

3. Data

3.1 Household panel 2008–12

We conducted a household panel survey in 2008 and 2012 in the department of Huehuetenango in the Western Highlands of Guatemala, bordering the Mexican state of Chiapas.[1] The sample was representative of the surveyed areas in four municipalities purposively selected to capture geographical heterogeneity within them.[2] The study area is both remote and rural, populated almost entirely by indigenous Guatemalans, and characterized by high outmigration flows and extremely

[1] The Study of the Impact of Remittances and Migration on Food and Nutrition Security (or IRMISAN, its acronym in Spanish). Carletto et al. (2011) provide additional details.

[2] Most Guatemalan municipalities are composed of one or more micro-regions used for governmental planning and administrative purposes and defined on the basis of geo-climatic affinity. In the two larger municipalities, we selected four micro-regions (1 of 4 in Jacaltenango and 3 of 13 in Cuilco). The two smaller municipalities were covered completely (San Gaspar Ixchil and Santa Ana Huista). Two selected micro-regions in San Mateo municipality had to be replaced prior to the baseline due to security considerations related to the 2008 elections.

high, even for Guatemala, levels of poverty and food insecurity. According to the 2008 school census, 63% of children 6–10 years old in the Western Highlands were stunted, compared to the national average of 46% (*Ministerio de Educación* and SESAN, 2008). Guatemala also is characterized by widespread poverty (World Bank, 2003, 2008) and here again the Western Highlands achieve notoriety. In 2006, 76% of the population in this region was poor, compared to the national average of 51%.[3]

In early 2008, we carried out a census in the survey areas to generate the household listing and determine whether each household had former household members (head, spouse of head, or child of current head or spouse) living abroad. Using this census, a random sample was selected, stratified on migration experience. A total of 24 households, evenly split between those with and without a (former) household member living abroad, were selected from each of 52 communities,[4] yielding a target sample of 1,248 households. Between April and August 2008, 1,222 (97.9%) households were interviewed.

Modeled on the 2006 Guatemalan Living Standards Measurement Survey (LSMS), the household questionnaire solicited information on demographics, health, education, dwelling conditions, full consumption and expenditures, agricultural income activities, non-agricultural income activities, and other income sources such as remittances, and assets. In addition to components of a more standard LSMS survey, however, the questionnaire also included a specially designed module to collect the migration histories of current household members as well as current location (and other) information on all adult children and siblings of the household head and spouse. Lastly, children under six years of age were weighed and measured by specially trained enumerators, using regularly standardized equipment. For children under two years of age, recumbent length was measured and for those two years and over, standing height (WHO, 2006).

From May to August 2012, the follow-up survey targeted all of the households originally interviewed. The 2012 survey instrument was almost identical to that of 2008, with the cut-off for child anthropometric measurements was increased from six to 10 years of age, to provide repeat

[3] Authors' calculations based on the 2006 nationally representative Guatemalan Living Standards Measurement Survey (LSMS).

[4] The sample frame began with 72 communities, a number of which were combined based on size and location into 52 strata prior to sampling. After fieldwork, two additional communities were combined so that there are 51 communities for the purpose of community-level fixed-effects analyses and clustering. Migrants were oversampled and expansion factors (or population weights) constructed. To facilitate direct comparison with regression results, descriptive statistics presented in the paper and in Table 1 do not adjust for the sampling based on this stratification, but differ only slightly from adjusted statistics since the average share of households with a migrant in 2008 across the communities is 40%.

measurements on all children measured in 2008. All height and weight measures are converted into standardized z scores: height-for-age z score (HAZ) and weight-for-age z score (WAZ) (WHO, 2006; de Onis et al., 2007).

Although not statistically representative of the four municipalities, the study region has conditions similar to remote rural areas elsewhere in Guatemala, in other Central American countries, and in Mexico, where in the face of difficult economic situations in their home countries, households also often rely on migration to the U.S. In 2008, household-level poverty in the sample was 72% and more than one-third of the households had a migrant (as defined in detail below) living in the U.S. Child undernutrition was also severe, with 61% of children under five years stunted (HAZ < -2) and 19% underweight (WAZ < -2).[5]

Attrition, always a potential concern in panel surveys, was limited to 8% at the household level for the four-year interval. Of the 1,222 households surveyed in 2008, at least one adult member of 1,127 original households was reinterviewed in 2012. When only some of the members of an original 2008 household were found, all other original members were classified as being in a split-off household (or households) and their present locations and other basic information collected. Split-off members who had moved anywhere in the four study municipalities were tracked. Through this process, we interviewed 181 split-off households; consequently, there were 1,308 separate household interviews in 2012. For most analyses in this paper, which are at the individual child level and control for baseline 2008 characteristics, this feature of the panel data is not utilized. As the primary analyses examine children with valid anthropometric measurements in both 2008 and 2012, in section 5.3 we document individual child-level attrition and examine its potential influence on the findings.

3.2 Measuring migration

At the individual person level, there are a number of ways to measure migration and to define migrants using, for example, time spent away, destination location, and links to those remaining behind (Smith and Thomas, 2003). Consequently, there are an even greater number of ways to define a "migrant household" comprising a number of individuals (Carletto and de Brauw, 2007). Typically, researchers are constrained in how they operationalize migration status by the information available in a general purpose survey not explicitly designed around migration. In contrast, for this research we piloted and implemented comprehensive migration modules specially designed for the Guatemalan context. For example, historically there has been both short- and long-term international

[5] Wasting (height-for-age z score or WHZ < -2), however, is rare in the region and in the sample (3%).

migration from Guatemala, with more seasonal or short-term migration to nearby Mexico (generally from northwestern Guatemala where our study is situated), and longer term, though not always permanent, migration to the US. Migrants often spend years abroad, with some returning home for extended periods in between (Saenz de Tejada 2009).

To identify both past and current migrants, we collected migration histories for all current household members age 15 years or older. We began with a standard household definition including all the individuals who eat and sleep in the same dwelling for a defined minimum period of time. To ensure we captured even the most recent migrants, we defined an individual as a household member if he or she had been present for at least one month in the previous 12 months; many household surveys use a longer minimum residence period, including the Guatemalan LSMS for which an individual needs to have been present for at least three months (within the same 12 month reference period) to be considered a household member. The histories included the first and the most recent migration events for each of these current household members. This allowed us to capture individuals who were currently present but may have been migrants in the past, as well as specific timing, location, principal occupation, and other information related to their migration experiences. Next, we collected similar information on the migration events for the spouse of the current resident head (if not a current household member and therefore not included above), as well as all adult children of the current head or spouse of head.

In this paper, we define a migrant household as one in which there was a migrant to the U.S. in at least three years during the four-year period 2005–08, prior to the baseline survey; we provide further details and our rationale for this definition in Section 4. In the analyses, we also assess whether the results are sensitive to alternative definitions.

4. Methodology

While we argue it is plausible to treat the financial crisis and ensuing economic downturn in the U.S. as exogenous from the perspective of rural Guatemalan households in Huehuetenango, neither their initial migration status in 2008 prior to the crisis, nor any reactions to the crisis (via a number of potential coping mechanisms including, in particular, changes in migration or remittance patterns), can be considered exogenous. In this section, we outline our identification strategy within this context.

We begin with a simple double-difference strategy, and then extend it, to explore the effects of the crisis on households in the 2008–12 panel survey. The intuition underlying the econometric identification is that non-migrant households, as determined in early 2008, were less exposed to, and therefore would have been less affected by, the crisis since they did not have (former) members living in the U.S. These non-migrant households, then, can be treated as a comparison or "control" group against which we contrast migrant households who did have (former) members living in the U.S. If, on average, other developments in the study region influence migrant and non-migrant households in equal measure, then the change over time in outcomes for migrant households, minus the change over time for non-migrant households, yields the effect of the crisis.

More specifically, we begin the empirical exploration of the effects of the crisis on children with a double-difference estimator based on the individual panel data of children under three years (< 36 months) in 2008, as the change between 2008 and 2012 (e.g., in HAZ) for children in migrant households (as determined in 2008 to avoid endogenous changes in migration status resulting from the crisis itself) minus the change between 2008 and 2012 for children in non-migrant households (as determined in 2008). A strength of this approach, by virtue of using panel data, is that it directly controls for all child-specific time-invariant individual-level effects, for example, any fixed genetic differences. Moreover, it also controls for all time-invariant household- and community-level effects, for example, fixed unobservables associated with migration selection effects. In difference form (after removing the fixed-effects), the model is:

$$\Delta y_i = \beta + \delta_{DD} m_{h0} + \Delta u_i \qquad (1)$$

where

Δy_i is the difference over time (2012–2008) in outcome y for individual i (in household h),

m_{h0} is an indicator of whether the household h in which the individual lived in 2008 (i.e., at baseline) was a migrant household, and

Δu_i is the difference over time of an assumed idiosyncratic error term for individual i (in household h)

δ_{DD} yields the double-difference. To interpret this double-difference as the effect of the crisis on migrant households, the maintained assumption is that the change over time in outcomes observed for individuals in non-migrant households accurately represents the change over time that would have been experienced in migrant households had there not been a crisis; i.e., in the absence of the crisis the two groups would have had common trends. Implicit in the common trends assumption in this context is the (further) assumption that non-migrant households were themselves unaffected by the crisis. Both of these assumptions are likely too strong in this context.

First, children in migrant households in 2008 had higher levels of nutritional status (e.g., HAZ) than their counterparts in non-migrant households (due to potential causal effects of migration itself, as well as other possible differences in households that had migrants, i.e., migration selection effects; Mckenzie and Sasin, 2007; Mckenzie, Gibson, and Stillman 2006). Therefore, it is unclear whether we should expect that the two groups of children would have had similar trends (e.g., similar child growth rates) in the absence of the crisis. For example, they could have different trends if migration (or migration selection) not only influences HAZ observed in 2008, but also directly influences growth after that. Rather than a constant additive individual fixed-effect due to migration in each period (which is controlled for in equation (1)), the effect of migration might change over time, directly affecting the growth rate. As HAZ in 2008 is itself the cumulative effect of growth prior to that measurement, such an effect of migration on child growth is plausible, if not likely. Additionally, it is possible that the physiology of human growth influences whether common trends are expected from different starting points in height. Increasing evidence on the potential for catch-up growth (Mani 2012; Prentice et al. 2013; Frankenberg et al. 2013), for example, is consistent with growth being more positive from lower starting points. Therefore, the net influence on child growth, with potential positive effects due to selection but potential negative effects due to the physiology of growth (i.e., catch-up growth among the comparison children), is ambiguous.

A second important concern with the common trends assumption is that because the crisis was a global aggregate shock, it would be inappropriate to assume that non-migrant households were unaffected. Rather, there were potential direct effects related to a general downturn in the Guatemalan economy or to diminished future migration possibilities for these households, as well as indirect effects operating through affected migrant households residing in the same communities. If migrant households experienced lower incomes as a direct result of the crisis (e.g., due to reduced remittances), this could translate into lower spending in the local economy (e.g., purchases from local shops or employment of local labor), which in turn could affect both migrant and non-migrant household neighbors. While any such indirect effects of the crisis on non-migrant households were probably heterogeneous in their severity, it is plausible to assume that few, if any, households in these remote, agriculturally-based communities actually would have benefited on net from the crisis.[6]

[6] This is in contrast to country-specific currency crises with dramatic relative price changes such as in Indonesia (Frankenberg et al., 2003) or the Philippines (Yang 2008) where some households benefited

Consequently, if anything, the direct and indirect effects of the crisis would have a likely negative effect on non-migrant households, leading to underestimates of the impact of the crisis on migrant households.

To relax the strong assumption of common trends, and thereby increase confidence in our estimates of the effect of the crisis on child nutritional status, we extend the basic model outlined above in two ways. First, we introduce additional baseline 2008 controls into the difference model, at the individual (e.g., gender, age, parental education), household (e.g., per capita expenditures and characteristics of the head), and community levels (e.g., distances to health and schooling facilities):

$$\Delta y_i = \beta + \delta_{DD} m_{h0} + \beta_{ind} X_{i0} + \beta_{hh} X_{h0} + \beta_{com} X_{c0} + \Delta u_i \qquad (2)$$

where variables X_{i0}, X_{h0}, and X_{c0} are sets of 2008 baseline individual-, household-, and community-level controls selected on the basis of their potential influence in the production of child health (Marini and Gragnolati, 2003; Strauss and Thomas, 2008) and because they may be different between migrant and non-migrant households.

To the extent that differences in common trends are due to differences in baseline factors in $\boldsymbol{X}$, as seems plausible given what we know about the production technology for child health, controlling for them better isolates in δ_{DD} the effect of the crisis. We also introduce into this specification a consideration of possible spillover effects within communities. Including in X_{c0} the share of migrants in each community, we can explore whether children living in communities with more migrants exhibit different patterns. Larger declines in communities with more migrants, for example, would be consistent with negative local spillover effects or externalities (e.g., due to reduced local consumption). They also would be consistent with more difficulty in locally "insuring" losses from the aggregate shock. Lastly, rather than a set of specific community-level characteristics X_{c0}, we alternatively can control for all community-level factors via a community-level fixed-effect in (2), which in this difference equation is effectively a community-specific time trend.

The second extension we make aimed at relaxing the common trends assumption begins with (2), but expands the sample to include an older age group (those three to six years in 2008) as well. This allows us to introduce an additional difference, in which we contrast younger and older children exploiting key findings from the literature on child growth based both on the physiology of growth and empirical evidence that growth failure occurs primarily in early life.

The nutritional literature emphasizes that undernutrition is most common and severe during periods of greatest vulnerability (Martorell, 1997; UNICEF, 1998). One such period is the first two to three years of life. Young children have high nutritional requirements, in part because they are growing so fast and also because they are fully dependent on others for care. The diets commonly offered to young children to complement breast milk in developing countries like Guatemala are monotonous and have low energy and nutrient density and, as a result, multiple nutrient deficiencies are common. Moreover, young children are highly susceptible to infections because their immune systems (which are both developmentally immature and potentially compromised by poor nutrition) fail to protect them adequately. Foods and liquids are often contaminated and lead to infections, which both reduce appetites and increase metabolic demands. In part for these reasons, WHO guidelines are to breastfeed exclusively until the age of six months—in our sample of children under one year in 2008 less than 40% of mothers had breastfed exclusively for that long.

In a well-known, randomized, food supplementation trial fielded in Guatemala in the 1970s, the gap between the Guatemalan sample and the international reference population increased until about three years of age, but remained fairly constant thereafter (Martorell et al., 1995). Supplementation produced its biggest effects by two years of age and after three years of age did not influence child growth rates (Schroeder et al., 1995).[7]

The physiology of growth, combined with these findings on food supplementation, suggest that if migration affects child growth, then it is more likely to do so if the migration event occurs during this early period of vulnerability. If the migration event occurs later, then it is unlikely (or at least less likely) to affect growth. This pattern sets up a natural difference-in-difference approach to assessing the effect of migration on child nutritional status based on age (Duflo, 2003; Nobles, 2007; Carletto et al. 2011). Having already differenced nutritional status over time in (2), this contrast leads to a triple-difference. The first double difference is from equation (2) for young children. If the only aspect of migration that was selective for child nutritional status was an additive selection term, the double-difference would yield the causal effect of migration. If the (additive) selectivity effect of migration changes over time, however, the double difference would reflect both the causal effect of migration as well as this time varying selectivity effect. In contrast, because the older children are largely past the most important window of opportunity for migration to influence their growth, the

[7] Other work in non-experimental settings provides evidence that catch-up growth at later ages (particularly in adolescence) is possible, with diminished long-term effects (Foster, 1995; Mani, 2008; Prentice et al. 2013). As we emphasize in the text, any catch-up would weaken our identification strategy, almost certainly leading to the possibility of underestimates. Since we only examine children under six years of age in 2008 (and thus under 10 in 2012), adolescent catch-up should not affect our results.

double difference estimate for this group yields only the time varying selectivity effect. Subtracting the double-difference estimate for the older group from that for the younger group, then, removes from the estimate any time varying selectivity effect and provides a more reliable estimate of the causal effect of migration.

To link these ideas to the data, we define migration as follows: a household is a migrant household if there was at least one migrant to the U.S. (including the head, spouse of head, or children of either as described above) in at least three of the four calendar years from 2005 to 2008.[8] This includes migrants who left prior to 2005 and remained away at least three of the four years in between 2005 and 2008. The condition also is filled in (rare) instances with different individuals migrating in different years, as long as during three of the four years there was at least one migrant. As the baseline household survey was implemented from April to July of 2008, the four-year window fully covers the entire life of any child under three years of age at the time of the baseline, including part of the period in utero.[9] The comparison we set up, then, is between these households and households for which this was not the case. Consequently, the comparison group includes households that may have experienced migration, but to a lesser extent.

Building on the evidence that younger children who are growing more rapidly are likely more vulnerable, we expand the sample to include children under age six in 2008 and then modify (2) to include an indicator of whether the child is under three years in 2008, as well as an interaction of that indicator with migrant household status, yielding:

$$\Delta y_i = \beta + \delta_{DD} m_{h0} + \beta_{young} young_{i0} + \gamma_{DDD} m_{h0} \times young_{i0} + \beta_{ind} X_{i0} + \beta_{hh} X_{h0} + \beta_{com} X_{c0} + \alpha_h + \Delta u_i \quad (3)$$

where variables not previously defined include

$young_{i0}$ is an indicator variable for the age in baseline 2008 of individual i in household h (e.g., under 3 years of age), and

α_h is a household-level fixed-effect.

[8] In the years prior to 2008, migration to Mexico has become relatively less important in the region. For example, based on this definition, 36% of households in the overall sample had a migrant to the U.S., but only 12% had one to Mexico (half of which also had one to the U.S.). Since remittance flows from the U.S. are substantially larger on average, we focus on U.S. migrants in the main analysis, but consider the role of migration to Mexico as a robustness check.

[9] In only about one-fifth of the cases is the migrant a parent of young child, and more often it is a grandparent or uncle/aunt.

Ignoring controls ***X*** and the household-level fixed-effect for the moment, γ_{DD} in (3) is now the double-difference for the older age group and γ_{DDD} represents the triple-difference. The latter is the difference between the double-difference estimator as described above for young children, minus the double-difference estimator for older children, captured by γ_{DD} in (3). We treat the double-difference for the young children as the effect on children in migrant households that incorporates both the effect of the crisis on migrant children and the possible migration selection effect on child growth that is due to the selectivity of those households that have a migrant. Assuming that older children are less vulnerable, and therefore less likely to be affected by changes in economic or other conditions between 2008 and 2012, the double-difference effect for them captures only this migration selection effect. The third difference, then, removes the effect of selectivity on growth, leaving only the effect of the crisis. To the extent that older children are still susceptible to influences over this period and there was a negative effect on them as well, γ_{DDD} is likely to be attenuated.

As with (2), we can introduce additional baseline individual-, household-, and community-level controls. Moreover, in this sample with an expanded age range and consequently more households with more than one child in the relevant age range, we can extend to a household fixed-effects specification of equation (3), for which we necessarily drop X_{h0} and X_{c0} and in which identification comes only from those households in which there was at least one child in each of the two age groups.[10][11] While this leads to identification based on a smaller sample size, it effectively controls for household-specific time trends. Under assumptions that older children are less vulnerable to shocks, then, this these household-specific trends include the selectivity effect of migration on growth.[12]

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[10] It is not possible to incorporate household fixed effects in model (2), since the migrant indicator does not vary within household.

[11] Since it mechanically controls for community-level fixed-effects in the difference equation, the household-level fixed-effects analysis controls for possible spillover effects on growth unless such effects are different in younger versus older age groups between migrant and non-migrant households, in which case they are probably best treated as an effect of the crisis anyway.

[12] While the proposed "current" migration indicator captures relevant aspects of historical migration, it does not capture one important way in which migration patterns over time might threaten the validity of the DDD results. If households with current migrants also are more likely to have had past migrants, then children over 36 months of age may have been in migrant households when they were under 36 months of age, and thus benefited during the vulnerable period in the past. As these children are now in the comparison group, past improvements for them may lead to negative bias in the estimated effects, making the results more conservative. If, on the other hand, those households without current migrants are more likely to have had migrants in the past, the bias would be positive, resulting in a possible overestimation of the effect. Restricting the sample to those households with children over 36 months, examination of past migration indicates that households with current migrants are four times as likely to have had migrants in the past as those without current migrants. Despite this reassuring pattern, an additional check will be done [not yet completed], removing from the estimating sample those children over 36 months who, while not presently in migrant households did live in a migrant household when they were younger than 36 months. The concern is that these children were positively affected by

This allows the possibility of household-level fixed-effects to be included directly in the difference model.

5. Results

5.1 Migration and its correlates in 2008

International migration has been an important phenomena in the Western Highlands region of Guatemala for decades, with the relative importance of migration to the U.S. having grown substantially in the 21st century (Camus 2007; Saenz de Tejada 2009). Carletto et al. (2011) demonstrate that for this sample as a whole, migrant households were substantially better off than non-migrant households on a number of important dimensions, including food security, household expenditures, education, and child nutritional status.

The set of households with a child under three years is not random, however, and consequently may differ somewhat from the overall sample. Since these are the relevant households for the analyses in this paper, we focus our description on them. Compared with the total sample in 2008, households with a child under three are more likely to be poor (84% compared with 72% for the overall sample) and less likely to have had a migrant (24% compared with 36% for the overall sample).

In Table 1A we present characteristics for the subsample of households with a child under three in 2008, to confirm that migration is associated in the cross-section with higher well-being. Various measures of expenditures and per capita expenditures, are clearly higher for migrant households in 2008.[13] For example, per capita expenditures are approximately 25% higher in migrant households in that year (despite such households being larger). This difference, however, appears to be concentrated in non-food expenditures as there is little difference in per capita food expenditures across the two types of households. Nevertheless, nutritional status for children under three also is significantly better in migrant households in 2008, though similar for older children (Table 1B). Of course, better outcomes for migrant households, as compared in the cross-section, while suggestive, is hardly conclusive evidence that migration is the underlying cause of these differences.

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migration when younger, so that by including them in the comparison group as older kids in non-migrant households we inflate the estimated impact of migration.

[13] Because of the relatively large variation in expenditure measures, we also include logarithmic transformations of these measures to confirm significant differences are not being driven by particularly large or small values.

5.2 The crisis, migration, and selected household-level outcomes

In 2009, one year after the baseline survey, we carried out qualitative and quantitative follow-up in four survey communities, one from each of the included municipalities. This complementary study provides a limited snapshot of attitudes and perceptions around migration a year into the financial crisis from focus groups, semi-structured interviews with returned migrants, and a short quantitative survey of approximately 100 of the panel households. From each source, the message was clear—the cost-benefit calculations of migration were undergoing substantial change with increased travel costs, increased travel risk (associated with the Mexican leg of the journey as well as greater hostility in U.S. immigration enforcement), and decreased opportunity as a result of the economic downturn. Focus group discussions spontaneously highlighted return migration. Among the reasons for return, most were coming back because they could not find work, with smaller numbers having been deported (Saenz de Tejada 2009).[14] Last, in the quantitative survey administered in four communities respondents overwhelmingly indicated that they believed obtaining work in the U.S. had become much more difficult and universally felt that laws and attitudes toward immigrants in the U.S. had worsened in the previous year.

The panel data between 2008 and 2012 make clear that there was a drop in migration from the region, consistent with the timing alluded to in the 2009 qualitative study. Figure 1 shows the fraction of households by year who had at least one international migrant anywhere (solid line) and at least one international migrant in the U.S. (dotted), for the balanced panel of 1127 households. The fraction of households with a migrant in the U.S. rose steadily until 2008 when it dropped sharply; it has been approximately stagnant since then.

Prior to turning to the effect of the crisis on children, we explore patterns at the household level for the set of households with children. Simple comparisons make clear that migrant households fared relatively poorly compared to non-migrant households between 2008 and 2012, and double-difference estimates confirm the statistical significance and large magnitude of the differences.

To characterize more explicitly changes over time for migrant and non-migrant households at the household level, we begin by assessing remittance behavior, income, and expenditure patterns. Whereas the total and per capita expenditure indicators showed that migrant households were better off than non-migrant households in 2008, single differences between the two were no longer

[14] Further semi-structured interviews with return migrants in 2010 underscored the potential costs of early return or failed migration when the initial investment (most typically a fee of approximately $5000) had not been recouped. In 2010, however, the strong negative sentiment around migration was less salient.

significant in 2012 (Table 1A). First, we examine patterns of whether a household received any remittances in the previous 12 months and how much. We find dramatic double-difference reductions of over 50 percentage points, as consequently large reductions in remittances received (though not in the amount of remittances received conditional on having received any). Second, we considered measures of income and, consistent with the decline in remittances, they point to a decline in income (results not shown). These declines also translate into reductions in per capita consumption expenditures, on the order of 25% though this result is concentrated in non-food expenditures as there is little change in per capita food expenditures. The large changes in overall expenditures, coupled with little change in food expenditures, led to a significant relative increase in the food share for migrant households.[15] Despite these substantial relative declines in well-being, however, migrant households were roughly even or continued to have a slight absolute advantage over non-migrant households in 2012 on these measures.

Given the discussion of identification with migrant selectivity, we do not interpret these unconditional double-differences based on equation (1) as the unbiased causal effects of the economic crisis on these households. They do, however, comprise strongly suggestive evidence that the economic crisis had substantial negative effects on migrant households relative to non-migrant households, making it clear they appear to have been unable to insure against the shock after four years except, perhaps, with respect to per capita food expenditures. It would seem that the financial crisis in the U.S. reached these rural households in the Western Highlands of Guatemala.

5.3 The crisis and child nutritional status

The previous section highlighted one area where migrant households had not seen relative declines over this period, per capita food consumption, even though the share spent on food in migrant households increased significantly. This raises the possibility that despite the crisis, outcomes further downstream from food consumption, including child anthropometrics, might well have been protected. But it by no means implies this, since children eat substantially smaller quantities of food and child growth depends on many other factors, including sanitation and care (Lancet cite). We turn now to our assessment of child growth.

Before considering the regression estimation results, we examine the density curves for HAZ for the two age groups in each survey year (Figures 2A-D). In 2008, it is clear that whereas children under

[15] Substantial increases in food prices in late 2008 and 2009 likely underlie the increase in 2012 in per capita food expenditures (despite our deflating with a national CPI) and in the food share.

three in households who had a migrant to the U.S. in at least three of the past four years are generally better off than their counterparts in non-migrant households, this is not the case for those three to six years of age. Following those children under three in 2008 longitudinally, there are two notable changes in the density four years later. First, there has been a narrowing of the distribution, with greater concentration of mass between HAZ of -4 and 0. Second, while migrant children still appear to fare better on average, much of their early life advantage has been lost. Following the older group longitudinally, there is also evidence of a tightening of the distribution and, if anything, small relative gains for the migrant children. These patterns set up the principal findings from our analysis, which demonstrate larger relative gains for young non-migrant children.

For children under three in 2008, we present estimates of the effect of the crisis on children in migrant households for models (1) and (2) in Table 2. All estimates are based on the balanced panel sample of children and standard errors are estimated allowing for clustering at the community level. The estimate in column (1) for each outcome is equation (1)— the simple double-difference estimator. The estimated negative effect on HAZ is statistically significant and substantial (-0.548), more than one-half a standard deviation. Referring back to Table 1B, we see that whereas there was a slight improvement in average HAZ for children in non-migrant households (of approximately 0.1 SD), there was substantial deterioration for children in migrant households (a reduction of 0.4 SD), despite their having had a substantial advantage at baseline. The negative double-difference estimated effect is driven by this decline for migrant children. With that decline, most of their early advantage has been lost, such that in 2012 migrant children have only slightly better, albeit still significant, HAZ than non-migrants (approximately 0.2 SD).

An effect of one-half SD is large; for example, even in carefully controlled program settings effect sizes on HAZ of this magnitude are considered to be substantial (Lancet cite). Moreover, it does not appear to be related to other potential imbalances between migrant and non-migrant children, as it is insensitive to adding controls in columns (2). For example, in (2b) we find a similarly sized effect after controlling for a wide array of baseline individual-, household-, and community-level factors (see Table notes for complete listing). After controlling for all of these factors, growth over the period is higher for boys than for girls. Controls for age at measurement are important and indicate that children under two years at baseline had more positive growth. Conditional on all of the other variables, however, parental education is not significantly associated with changes in HAZ. Finally, although negative as expected, there is no significant relationship between the percent of households in the community who had migrants in 2008 and changes in HAZ, suggesting spillover effects are not substantial (2b).

The finding that boys grew more rapidly than girls raises the possibility that the effects of migration also might differ by sex, which we explore by interacting the migrant indicator with the male dummy variable. While the point estimate on the interaction term between male and migrant (0.245 in column 2d) suggests the effect of the crisis indeed might be smaller (less negative) for boys than for girls, this difference is not statistically significant; moreover, even in this model with an interaction, the total double-difference effect on boys remains a negative and significant half standard deviation.

In the right-side panel of Table 2, we present the results for stunting (HAZ < -2.00), to explore whether the findings are different when we focus on more severe undernutrition. The estimated double-differences similarly demonstrate that the crisis had a substantial, and deleterious, effect on child growth, with an increase in the prevalence of stunting of approximately 20 percentage points. The results for stunting additionally suggest there may have been negative spillovers within communities; a one percentage point increase in the percent of households in the community with a migrant is associated with a marginally significant 0.7 percentage point increase in stunting.[16]

In Table 3, we present results on WAZ and underweight for the same age group, as well as all four measures for the older age group, those age three to six. For ease of comparison, we repeat the DD estimates for HAZ and stunting (from Table 2) on the under three year olds in the first row. While point estimates suggest a possible negative effect of approximately 0.2 SD for WAZ, we fail to reject that the crisis had any statistically significant effect on WAZ or underweight for the under three group. This pattern, of negative effects on the cumulative HAZ measure, but little or no effect on WAZ, which captures short-term fluctuations and is more heavily influenced by current conditions, is consistent with what we know about the crisis—it was deepest in 2009 and 2010, when these children were younger and potentially more vulnerable but by 2012 there had been substantial recovery, as well as more time for effective coping mechanisms on the part of individual households to have taken root. Consequently, the pattern of results for nutritional status reflect the pattern of the crisis, strengthening our causal interpretation.

An examination of the effect on older children (Table 3), whom we argue would have been less sensitive to any shocks, also shows no significant effects. The only estimated coefficients of substantial magnitude are those on stunting (ranging between -0.06 and -0.09) and, if anything,

[16] The share of households in the community with a migrant ranged from 20-60% with a SD of 9.

suggest improvements in nutritional status. That tendency is consistent with positive migration selection, i.e., the possibility that in the absence of the crisis, children in migrant households would have fared better. Also consistent with positive selection is the slightly better (albeit insignificant) HAZ and stunting outcomes for the older group in 2008 (Table 1B).[17]

This possibility of a selection effect, even if statistically insignificant in these specifications, leads us to the final formulation of the model, estimating equation (3). In Table 4, we combine the results for the different age groups and present the triple-difference results. The conclusions are clear and the estimated effects similar across all four specifications. Even when we introduce household-level fixed effects (3d), which in this difference equation control for household-specific trends, the triple-difference estimated negative effect of the crisis on HAZ is greater than one-half SD (-0.594).[18] This effect corresponds to an increase in stunting of more than 20 percentage points.[19]

In 2008, young children in households with migrants in the U.S. had substantially higher nutritional status than their counterparts in households without migrants. From 2008 to 2012, however, as the economic crisis in the U.S. took its toll on industries and sectors heavily populated by the migrants, those same children lost substantial ground relative to their counterparts in non-migrant households. The apparent benefits of migration to those children, as measured by height, were in large part, though not completely, lost after four years. Our interpretation, based on the relatively weak identification assumptions underlying the triple-difference model, is that this was the direct result of the financial crisis.

5.3 Robustness considerations

In this section, we consider a variety of alternative specifications carried out to assess whether the primary results shown in Table 4 are robust. These include: 1) alternative definitions for migrant household in 2008; 2) alternative age groupings; 3) an assessment of the potential effect of attrition on the analysis; and 4) an alternative analysis that uses age cohorts rather than individual panel data.

5.3.1 Alternative definitions of migration

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[17] Since the migration indicator is defined over the previous four years, it does not fully capture the influence of actual migration on these older kids since it does not include migration status during their critical first few years.

[18] Some children in the same households have different parents, which is why parental characteristics are included in 3d; results are essentially unchanged if those variables are not included in the household-level fixed-effects model.

[19] In contrast, estimated triple-difference effects on WAZ and underweight are both small and insignificant (results not shown).

The definition for a migrant household was motivated by the patterns of migration (e.g., that in the years just prior to 2008 migration to the U.S. was relatively more important than migration to Mexico) and by the period of vulnerability in child growth. Modifying how we define migration generally leads to expected changes in the results. We consider the following alternatives: 1) a household has a migrant to either the U.S. *or Mexico* for three of the four years from 2005 to 2008; 2) a household has a migrant to the U.S. in *all three* years 2006 to 2008; 3) a household has a current, in 2008, migrant to the U.S. (ignoring previous migration experiences); and 4) a household has a migrant to the U.S. in *any* year 2005 to 2008.

Preliminary results indicate similar findings consistent with our prior on how each alternative definition changes the migration experience of the household during the four year period prior to 2008 (not yet shown).

5.3.2 Alternative age groups

The choice of age groups was motivated by the nutrition literature, but it is clear from that literature that sharp breaks between, for example, vulnerable and not vulnerable periods are unlikely. In addition, because of the longitudinal nature of the study, even children under three in 2008 are potentially exposed to the crisis at older ages, for example in 2009 and 2010. Consequently, we consider alternative ages for the young group (0 to 24 months and 0 to 30 months), while continuing to define the older age group as three to six years. If exposure is more important during earlier periods, then we would expect to see even larger effects for these ages. For a different reason, related to the difficulty of measuring height for children under 6 months of age (and thus the possibility of greater measurement error), we also considered those 6 to 36 months.

Preliminary results (replicating Table 4) find larger effects (not yet shown).

5.3.3 Attrition in the sample

While household level attrition was only 8%, attrition at the individual level for the age groups we analyze was substantially higher, where we treat as attrited those entirely lost to follow-up as well as those with invalid anthropometric measurements.[20] For children under three, attrition measured in this way was 19.9% and for children three to six, 16.5%. For both age groups, however, the difference in attrition between migrant and non-migrant children was less than two percentage points and insignificant.

[20] We also treat three deceased children as having attrited.

We argue that bias due to attrition is likely minimal for three reasons. First, models include individual level fixed effects and household level time-varying effects. If attrition is related to either of these, as seems plausible, then we control for it. Second, we examine characteristics of attrited versus non-attrited and preliminary results indicate no large or significant differences. Third, we carry out a bounding exercise inspired by Horowitz and Manski (2000), in which we assign to each individual who attrited a change in HAZ from the 95 percentile of the "opposite" group, i.e., assigning the 95 percentile HAZ score change from children in the migrant households to all attrited children from non-migrant households and vice versa. This procedure attenuates the estimated impacts from equation (3) with household-level fixed-effects to -0.460 (SE 0.232) for HAZ and +0.167 (SE 0.095) for stunting, both of which are statistically significant at a 10% significance level.

5.3.4 Cohort analyses

An alternative approach using the same child-age related identification strategy with the panel data, is to compare children of the same age across the two survey rounds. In other words, carrying out the double-difference approach outlined in Carletto et al. (2011) (using age and migration status) for each survey round, and then differencing the two. The advantage of this approach is that it incorporates those children born after 2008, who are children under four in 2012. One disadvantage is that for identification one must assume that fertility patterns for migrants and non-migrants are unchanging (or changing in identical fashion) over the period. A second disadvantage is that one can no longer control for individual level fixed-effects, since only a small portion of children are repeated. For these reasons, we do not treat this as our primary approach but underscore that under stronger maintained assumptions they would yield the same impacts. If the results are substantially different then there would be greater cause for concern, for example, regarding the possibility of differential fertility patterns across groups and over time.

Nevertheless, consideration of this approach leads to similar conclusions regarding the effect of the crisis (results not yet shown).

6. Potential mechanisms

[INCOMPLETE] Will consider double-difference estimates for additional household-level outcomes such as food security and triple difference estimates on individual-level outcomes for children such as breastfeeding, illness, and vaccination. None are as well identified as the primary outcome of liner growth (given the physiology of growth), but nevertheless they should provide suggestive evidence on mechanisms.

7. Conclusions

Migration has been demonstrated by various studies to be closely linked to improvements in individual- and household-level outcomes. It is difficult to identify with confidence the causal effects of migration, and likely that some of the observed differences are related to selection. In this paper, rather than examining the "positive" effects of migration, we put the spotlight on how the global financial crisis emanating from the U.S., negatively affected households in a rural region of Guatemala characterized by high outmigration, using a differences-in-differences strategy on panel data with the baseline measurement prior to the crisis. Literature that has looked at the effects on migrants living in the U.S. has documented negative effects (cites needed). A comprehensive understanding of the impact of the crisis, however, must incorporate effects transmitted elsewhere.

The apparent large gains from migration did not persist during the crisis. Migrant households fared much worse, in relative terms, than non-migrant households over the period. The substantial advantage they had in 2008, however, meant that even with large relative declines migrant households remained, on most measures, slightly better off than non-migrant households even in 2012. Our assessment demonstrates that substantial advantages in child anthropometric status for the youngest children in migrant households in 2008, just prior to the crisis, were largely erased only four years later. While there still may be some long-term benefits for these children who avoided undernutrition at a critical period in their development (for example, related to cognition), for the most part they were put back on the path toward stunting.

Gains from migration can be substantial, but maintaining those gains in the face of a crisis affecting a household's migrants, difficult. Evidence presented here suggests that those gains should not be treated as permanent and underscores the tenuous nature of the benefits of migration. This work demonstrates another way in which the U.S. financial crisis starting in 2008 had important consequences both near and far.

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Table 1A

Household descriptive statistics by migration status and year

	2008				2012				Double-	
	Migrant	Non-migrant	Difference		Migrant	Non-migrant	Difference		Difference	
Total expenditures (Q)	45430.48	30762.01	14668.47	**	39255.49	34257.87	4997.623		-9670.85	**
	(32235.11)	(26677.75)	(4136.54)		(33704.59)	(23569.04)	(3837.35)		(3613.13)	
Log total expenditures	10.52	10.13	0.39	**	10.27	10.25	0.02		-0.38	**
	(0.63)	(0.62)	(0.09)		(0.89)	(0.63)	(0.09)		(0.12)	
Per capita expenditures (Q)	5835.78	4686.71	1149.07	+	5170.24	4966.73	203.51		-945.56	*
	(4467.27)	(3856.78)	(588.15)		(4064.95)	(3806.60)	(456.77)		(468.60)	
Log per capita expenditures	8.45	8.23	0.22	*	8.24	8.29	-0.05		-0.27	**
	(0.66)	(0.65)	(0.09)		(0.89)	(0.66)	(0.09)		(0.10)	
Per capita food expenditures	2481.97	2739.61	-257.64		3042.41	3102.29	-59.88		197.76	
	(1512.66)	(2988.99)	(246.22)		(2214.85)	(2130.56)	(280.27)		(350.98)	
Food share	0.50	0.59	-0.09	**	0.62	0.65	-0.03		0.06	+
			(0.03)				(0.02)		(0.03)	
Household size	8.52	7.14	1.38		8.48	7.59	0.89		-0.49	
	(3.24)	(2.62)	(0.41)		(4.05)	(2.84)	(0.48)		(0.35)	
% receiving remittances	0.83	0.07	0.76	**	0.17	0.05	0.12**		-0.65	**
			(0.05)				(0.04)		(0.06)	
Remittances received (Q)	11574.44	501.18	11073.26	**	3405.75	791.74	2614.008		-8459.249	**
unconditional	(18113.34)	(3002.92)	(2071.74)		(15809.13)	(5350.36)	(1751.01)		2625.58	
Remittances received (Q)	13889.33	7295.00	6594.33	*	19299.25	12435.00	6864.25		269.92	
conditional on receiving	19027.47	9271.97	(3221.98)		34218.84	17946.60	(9638.98)		(10005.4)	
N	84	262			85	267				

Notes: Unless otherwise indicated, units are 2008 equivalent Quetzales. Sample of households with a child under three years in 2008 who was measured in both rounds (415 children in 346 households in 2008). There are six more households in 2012 than in 2008 because of split-offs. Standard deviations in parentheses under means and standard errors estimated allowing for clustering at the community level in parentheses under difference columns. ** indicates significantly different at p<0.01, * at p<0.05, and + at p<0.10.

Table 1B

Child descriptive statistics by migration status and year

	2008				2012				Double-	
	Migrant	Non-migrant	Difference		Migrant	Non-migrant	Difference		Difference	
Children < 36m in 2008										
HAZ	-1.582	-2.357	0.775	**	-2.019	-2.246	0.227	*	-0.548	**
	(1.486)	(1.459)	(0.16)		(1.106)	(1.066)	(0.111)		(0.16)	
WAZ	-0.766	-1.147	0.381	**	-1.213	-1.370	0.157		-0.224	
	(1.110)	(1.115)	(0.13)		(1.073)	(1.046)	(0.164)		(0.16)	
Stunted (HAZ < -2)	0.402	0.629	-0.227	**	0.515	0.569	-0.054		0.173	**
			(0.05)				(0.052)		(0.06)	
Underweight (WAZ <-2)	0.155	0.217	-0.062		0.179	0.237	-0.058		0.005	
			(0.04)				(0.057)		(0.05)	
Age in months	17.489	18.649			67.324	68.191				
	(9.732)	(10.320)			(9.789)	(10.397)				
N	97	318			97	318				
Children 36-72m in 2008										
HAZ	-2.281	-2.402	0.121		-2.064	-2.248	0.184	+	0.062	
	(1.054)	(1.200)	(0.14)		(1.179)	(1.119)	(0.104)		(0.14)	
WAZ	-1.274	-1.307	0.033		-1.478	-1.474	-0.005		-0.038	
	0.937	0.885	(0.10)		0.929	1.025	(0.107)		(0.11)	
Stunted (HAZ < -2)	0.634	0.640	-0.006		0.507	0.573	-0.066		-0.060	
			(0.05)				(0.053)		(0.06)	
Underweight (WAZ <-2)	0.189	0.188	0.002		0.311	0.300	0.011		0.009	
			(0.05)				(0.045)		(0.04)	
Age in months	53.301	53.693			103.107	103.150				
	(10.822)	(10.623)			(10.930)	(10.684)				
N	134	342			134	342				

Notes: Standard deviations in parentheses under means and standard errors estimated allowing for clustering at the community level in parentheses under difference columns. ** indicates significantly different at p<0.01, * at p<0.05, and + at p<0.10.

Table 2

Double-difference estimates of the effect of crisis on HAZ and stunting (Children < 3 years old in 2008)

	HAZ					Stunted				
	(1)	(2a)	(2b)	(2c)	(2d)	(1)	(2a)	(2b)	(2c)	(2d)
Migrant (DD)	-0.548**	-0.490**	-0.460*	-0.655**	-0.775*	0.173**	0.158*	0.138+	0.231**	0.187+
	(0.157)	(0.167)	(0.201)	(0.206)	(0.308)	(0.062)	(0.061)	(0.081)	(0.085)	(0.104)
Male		0.184	0.461*	0.556*	0.494*		-0.069	-0.016	-0.079	-0.102
		(0.151)	(0.189)	(0.211)	(0.217)		(0.062)	(0.066)	(0.073)	(0.076)
Male * Migrant					0.245					0.090
					(0.380)					(0.110)
Age in months		0.118**	0.137**	0.132**	0.133**		-0.044**	-0.048**	-0.045**	-0.044**
		(0.029)	(0.029)	(0.028)	(0.028)		(0.014)	(0.013)	(0.013)	(0.013)
Age in months^2		-0.002**	-0.003**	-0.002**	-0.002**		0.001*	0.001**	0.001*	0.001*
		(0.001)	(0.001)	(0.001)	(0.001)		(0.000)	(0.000)	(0.000)	(0.000)
Mother's schooling		-0.135	-0.112	-0.042	-0.037		0.042	0.042	0.055	0.057
		(0.098)	(0.118)	(0.128)	(0.123)		(0.040)	(0.043)	(0.049)	(0.049)
Father's schooling		-0.088	-0.086	0.039	0.037		0.036	0.054	0.027	0.027
		(0.150)	(0.175)	(0.171)	(0.170)		(0.052)	(0.058)	(0.054)	(0.054)
% community migrants			-0.013					0.007+		
			(0.012)					(0.004)		
Intercept	0.111	-1.049**	7.881	10.463	10.772	-0.060	0.335*	5.754	1.272	1.386
	(0.093)	(0.313)	(7.996)	(11.656)	(11.684)	(0.037)	(0.138)	(4.064)	(5.470)	(5.532)
F-Stat p-value										
Male + Male*Migrant					[0.039]					[0.007]
F-Stat p-value										
Overall model	[0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[0.007]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Additional Controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Community-level FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes

Notes: Dependent variable is difference (2012-2008) in height-for-age z score (HAZ) or in binary stunting defined as HAZ < -2.00. Balanced panel sample includes 415 children under age 3 years in 2008. Standard errors estimated allowing for clustering at the community level in parentheses. ** indicates significance at p<0.01, * at p<0.05, and + at p<0.10. Parental education coded as none=0, incomplete primary=1, and primary and above=2. Additional controls (all measured at baseline 2008) not shown include time in months between survey measurements, logarithm of household size, number of males (and separately females) in each of three age groups (0-5, 6-15, and 16-35), household head education and age, whether household head is male, whether household head is indigenous, the log of per capita expenditures (and its square), whether household has: private piped water, cement floor, block walls, and toilet, logarithm of the number of rooms in the house, whether household owns 1 or more hectares of agricultural land, whether anyone in HH benefited in past year from health program, average community level per capita expenditures, and distance to nearest health center, primary school, and market.

Table 3

Double-difference estimates of the effect of crisis on child nutritional status, by age group

	(1)	(2a)	(2b)	(2c)	(1)	(2a)	(2b)	(2c)
Children < 3 years old in 2008								
	HAZ				Stunted			
Migrant (DD)	-0.548**	-0.490**	-0.460*	-0.655**	0.173**	0.158*	0.138+	0.231**
[N=415]	(0.157)	(0.167)	(0.201)	(0.206)	(0.062)	(0.061)	(0.081)	(0.085)
F-Stat p-value	[0.001]	[<0.001]	[<0.001]	[<0.001]	[0.007]	[<0.001]	[<0.001]	[<0.001]
	WAZ				Underweight			
Migrant (DD)	-0.222	-0.221	-0.081	-0.206	0.002	-0.006	-0.032	0.006
[N=412]	(0.161)	(0.157)	(0.184)	(0.178)	(0.053)	(0.054)	(0.062)	(0.061)
F-Stat p-value	[0.173]	[0.073]	[<0.001]	[<0.001]	[0.968]	[0.156]	[<0.001]	[<0.001]
Children 3-6 years old in 2008								
	HAZ				Stunted			
Migrant (DD)	0.062	0.064	0.126	-0.033	-0.060	-0.058	-0.090	-0.062
[N=476]	(0.144)	(0.141)	(0.135)	(0.157)	(0.058)	(0.058)	(0.064)	(0.070)
F-Stat p-value	[0.667]	[0.018]	[<0.001]	[<0.001]	[0.306]	[0.010]	[<0.001]	[<0.001]
	WAZ				Underweight			
Migrant (DD)	-0.032	-0.046	-0.031	-0.028	-0.001	-0.000	0.029	0.019
[N=466]	(0.114)	(0.111)	(0.123)	(0.129)	(0.042)	(0.041)	(0.050)	(0.051)
F-Stat p-value	[0.778]	[0.156]	[<0.001]	[<0.001]	[0.987]	[0.263]	[<0.001]	[<0.001]
Additional Controls	No	No	Yes	Yes	No	No	Yes	Yes
Community-level FE	No	No	No	Yes	No	No	No	Yes

Notes: Dependent variable is difference (2012-2008) in height-for-age z score (HAZ), binary stunting defined as HAZ < -2.00, weight-for-age z score (WAZ), or binary underweight defined as WAZ < -2.00. Balanced panel sample. Standard errors estimated allowing for clustering at the community level in parentheses. ** indicates significance at p<0.01, * at p<0.05, and + at p<0.10. See Table 2 for list of controls.

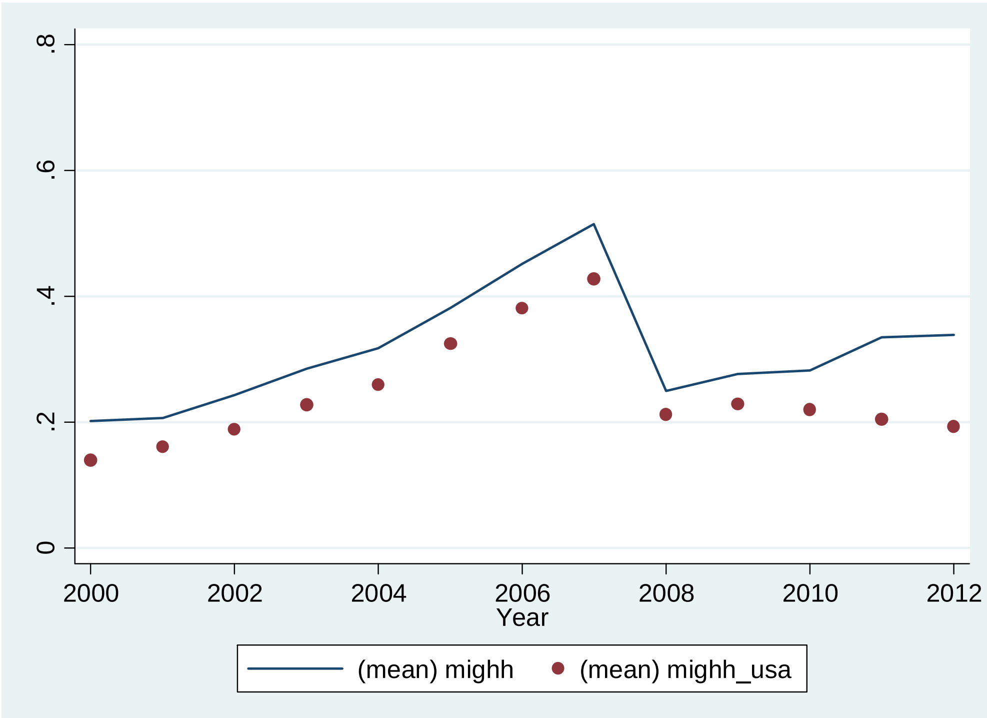
Table 4

Triple-difference estimates of the effect of financial crisis on HAZ and stunting (Children < 6 years old in 2008)

	HAZ				Stunted			
	(3a)	(3b)	(3c)	(3d)	(3a)	(3b)	(3c)	(3d)
Migrant	0.062	0.128	0.026		-0.060	-0.081	-0.061	
	(0.144)	(0.142)	(0.152)		(0.058)	(0.064)	(0.071)	
Child < 3 years	-0.044	0.380+	0.423*	0.538*	0.008	-0.044	-0.059	0.001
	(0.121)	(0.207)	(0.203)	(0.228)	(0.044)	(0.082)	(0.081)	(0.103)
Migrant*Child (DDD)	-0.610**	-0.584**	-0.578**	-0.594*	0.233**	0.216*	0.245**	0.270*
	(0.205)	(0.197)	(0.203)	(0.257)	(0.080)	(0.082)	(0.085)	(0.119)
Male		0.280*	0.330*	0.374*		-0.011	-0.025	-0.069
		(0.137)	(0.145)	(0.143)		(0.057)	(0.059)	(0.068)
Age in months		0.066**	0.068**	0.077**		-0.022**	-0.022**	-0.018**
		(0.014)	(0.013)	(0.014)		(0.005)	(0.005)	(0.005)
Age in months^2		-0.001**	-0.001**	-0.001**		0.000**	0.000**	0.000**
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Mother's schooling		-0.035	0.011	-0.124		-0.004	-0.001	-0.159+
		(0.089)	(0.088)	(0.181)		(0.034)	(0.035)	(0.089)
Father's schooling		-0.059	0.004	-0.047		0.023	0.004	0.054
		(0.115)	(0.123)	(0.218)		(0.040)	(0.039)	(0.121)
Intercept	0.154+	8.851	17.743**	20.037	-0.067+	3.429	-1.880	-3.634
	(0.087)	(5.953)	(5.882)	(17.299)	(0.034)	(2.627)	(3.106)	(4.226)
F-Stat p-value								
Overall model	[0.004]	[<0.001]	[<0.001]	[<0.001]	[0.009]	[<0.001]	[<0.001]	[<0.001]
Additional Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Community-level FE	No	No	Yes	Yes	No	No	Yes	Yes
Household-level FE	No	No	No	Yes	No	No	No	Yes

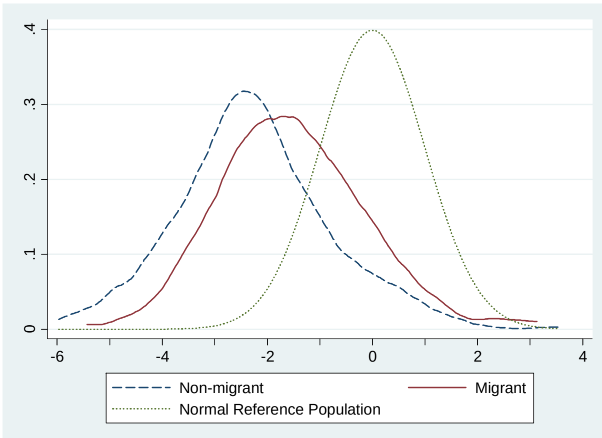
Notes: Dependent variable is difference (2012-2008) in height-for-age z score (HAZ) or in binary stunting defined as HAZ < -2.00. Balanced panel sample includes 891 children under age 6 years in 2008. Standard errors estimated allowing for clustering at the community level in parentheses. ** indicates significance at p<0.01, * at p<0.05, and + at p<0.10. See Table 2 for additional controls.

FIGURES 1: Fraction of households reporting an international migrant

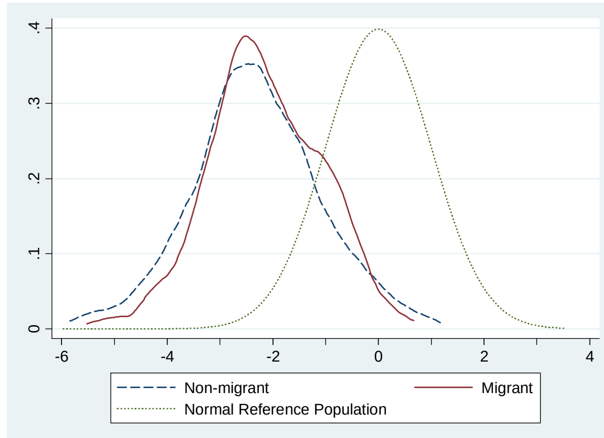


FIGURES 2A-2D Height-for-age z scores

Children less than 3 years in 2008, measured in 2008

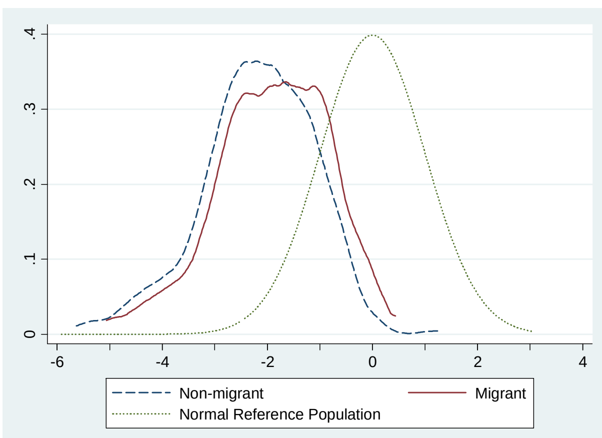


Children 3–6 years in 2008, measured in 2008



Children less than 3 years in 2008, measured in 2012

When 4-7 years of age



Children 3–6 years in 2008, measured in 2012

When 7-10 years of age