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A Within-Sibling Investigation

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Abstract[1]

This paper examines the causal link between early childhood nutrition and cognition, applying instrumental variables to sibling-differences for a sample of pre-school aged Peruvian children. Child-specific shocks in the form of food price changes and household shocks during the critical developmental period of a child are used as instruments. The analysis shows significant and positive returns to early childhood nutritional investments. An increase in the Height-for-Age z-score of one standard deviation—keeping other factors constant—translates into increases in the Peabody Picture Vocabulary Test (PPVT) score of 17-21 percent of a standard deviation. The period of analysis includes the recent global food price crisis that also affected Peru between 2006 and 2008. This therefore is also a quantification of the nutritional and subsequent cognitive costs of food prices on the sample, which could be magnified in later years.

JEL codes: I12, I20, J13
Keywords: Health, Nutrition, Cognitive Development, Children, Peru

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1. Introduction

A growing literature in economics, nutrition and sociology has built a substantial evidence base on the linkages between early nutritional deficiencies and reduced cognitive ability, educational attainment and ultimately lower market wages later on in life. However, few of these studies have been able to show these correlations to be genuinely causal. In this paper we seek to establish the causal relationship between nutritional achievement and cognitive development in a sample of pre-school aged Peruvian children. Concern about the long-term effects of childhood malnutrition has been amplified by the food price crisis that led to a global rise of 40 percent in food prices during the 2006-2008 period (Von Braun, 2008). While not experiencing the highest rates of inflation in the region, Peru showed some of the most rapid increases, accumulating a 20 percent increase in food prices between 2006 and 2008 (Cuesta and Jaramillo, 2009). Exploiting the variation in nutritional intake resulting from the food price changes as well as other household-specific shocks, we are able to show that nutrition indeed does have a causal impact on cognitive ability. Our results have a further policy meaning, since they are a quantification of the nutritional and subsequent cognitive costs of the global food crisis on pre-school aged Peruvian children.

Understanding causality in the nutrition-cognition nexus is complicated by the endogenous nature of a child's health status. As illustrated by Behrman and Lavy (1994), both a child's health and her cognitive achievement can be understood as the outcomes of a utility-maximization process whereby parents invest in a child's human capital subject to initial conditions—i.e., genetically innate abilities—parental taste for child's quality and budget constraints. Since parental preferences and their ability to turn inputs into outcomes as well as genetic endowments are unobserved, ordinary least squares (OLS) estimations of the cognitive returns to early nutritional investments are likely to be biased.

Grantham-McGregor (1995) and Grantham McGregor and Baker-Henningham (2005) review evidence from the nutrition literature, and they find that school-aged children who were severely malnourished in the early years are more likely to suffer from cognitive deficits. They nonetheless stress that, while the evidence is strong, it is not unequivocal, and that a number of questions remain unanswered.[2] A strand of the literature uses experimental studies of supplementation to address the issue of endogeneity. The INCAP study in Guatemala (Pollitt et al, 1993) showed substantial positive effects of early childhood nutritional supplements on cognitive achievement among teenagers, and later on their adult development (Maluccio et al., 2009). A similar study implemented in Jamaica found that early child stimulation and nutritional supplements were effective in increasing cognitive achievement at the age of 8 (Grantham-McGregor et al., 1991), but by the age of 12, only "stimulation: children had higher cognitive achievement than the control group (Grantham-McGregor et al, 1997). If appropriately randomized, experimental studies are powerful tools for testing causal linkages. However, ethical and budgetary issues limit the replicability of such studies, especially with regard to studying the effects of undernutrition. Using non-experimental data, Alderman et al. (2006) and Glewwe et al (2001)—hereafter AHK and GJK, respectively—show that early childhood nutritional deficiencies in the form of low height-for-age can be linked to poorer cognitive attainment later in life. In doing so, they exploit within-sibling variations to deal with the endogeneity bias resulting from unobserved household heterogeneity. They also address differential parental investments resulting from child heterogeneity in innate abilities by applying instrumental variable estimation.

Following AHK and GJK, in this paper we combine a sibling-difference specification with instrumental variable methods to study the relationship between nutrition and cognitive achievement during the pre-school period for a sample of Peruvian children. We use data from a novel sample of paired siblings to estimate a conditional demand function for cognitive achievement in sibling-difference form, in which all investments common to both siblings are removed. To alleviate concerns of differential parental investments between siblings that might drive differences in nutrition and cognitive outcomes, we incorporate additional controls to the sibling-difference specification in dimensions related to birth order and birth-sex order. We also control for changes in household and community circumstances that might lead to differential outcomes between siblings.

In our instrumentation strategy, we use two sets of instruments for height-for-age differences between siblings, namely food price changes and household shocks occurring during

[2] For a policy review of the relevant evidence, see also World Bank (2006).

the critical developmental period of a child. We exploit differences between siblings by looking at food prices prevalent during the first three years of their life as a source of exogenous variation in nutritional inputs experienced by the siblings. Given that our sample comprises paired-siblings born in the periods 2001-2 and 2003-5, respectively, younger siblings were affected during their early years by the food price crisis, which impacted Peruvian households most severely in 2006 and 2007. In addition, we use household-specific short-term shocks that took place between 2000 and 2002 and between 2007 and 2009 as a further set of exogenous instruments. Because of their timing, these shocks can be considered child-specific, since they affected the household when one of the siblings was in her critical nutritional period, with the counterfactual sibling either relatively old or yet to be born.

We apply this strategy to the Young Lives Peru Survey.[3] The data are a novel sample of paired-siblings born in 2001-2 and 2003-5 respectively, for which anthropometric and cognitive measures were collected at roughly the same age-period—mostly between four and six years of age—at two different points in time, the 2006 and 2009 waves of the Young Lives survey. We use Peabody Picture Vocabulary Test (PPVT) scores as the cognitive outcome measure and contemporaneous height-for-age z-scores as the nutritional measure.

An advantage of our data is that the paired-siblings outcomes are measured roughly at the same age. Cognitive achievement at a particular age can be modeled as a function of a child's innate genetic ability and the cumulative effect of present and past cognitive investments in both the home and school environment (see Todd and Wolpin, 2003 and 2007). The challenge of estimating the effect of the health inputs on cognitive development is that of other inputs being missing. By focusing on a sample of children consisting primarily of pre-school age children, we reduce the sphere of cognitive influence mainly to the home environment. As such, we contribute to the literature on cognitive development during pre-schooling age (see Paxson and Schady, 2007, Berlinski and Galiani, 2007, and Behrman et al., 2004 among others).

Furthermore, our methodology allows us to go beyond previous studies. GJK argue that while instrumentation with birth weight can solve problems of differential parental investments across siblings, it does not deal with unobserved genetic factors affecting both nutritional and cognitive outcomes. Instead, they suggest the use of nutritional shocks as instruments to recreate

[3] www.younglives.org.uk

the identifying conditions of a “natural” experiment, but data limitations prevented the application of their preferred methodology. Using drought and civil war incidence as instruments of a child’s stature, AHK implement the methodology outlined by GJK. However, the validity of their choice of instruments has been contested (Glewwe and Miguel, 2008). We believe that our proposed set of instruments are not only well placed to meet the stringent conditions set forth in GJK, but are arguably sufficiently short-lived to have little impact on later cognitive achievement other than through their impact on anthropometric status.

Our analysis shows that there is a significant and causal impact of early nutrition on cognitive ability. Diagnostics of the first-stage results indicate that both sets of IVs are reasonably strong and valid. As a further robustness check on our instrument validity, we introduce controls for changes in non-food consumption and household assets taking place after the onset of the exogenous events. We find that our results remain stable, ruling out the possibility that the instruments might be affecting cognitive development through a delayed or persistent effect resulting from reduced household assets and consumption. Furthermore, our results are also robust to the inclusion of controls for delayed school and pre-school enrollment, suggesting that our analysis captures a nutrition-cognition parameter beyond the cognitive effects of delayed enrolment.

The effects uncovered appear to be substantial in magnitude; a one standard deviation increase in height-for-age would lead to an increase in the PPVT score of 17-21 percent of a standard deviation. The magnitude of these effects is significant, considering that the cognitive deficits have been accrued only during the first few years of a child’s lifetime. Moreover, they provide a first quantification of the nutritional and subsequent cognitive costs of the food crisis among pre-school age Peruvian children.

The remainder of the paper is organized as follows. Section 2 describes our conceptual framework and lays out in detail the empirical strategy to be used. Section 3 introduces the key features of the sample, the measurement variables and the instrumental variables chosen for the analysis. Section 4 shows our main results, while Section 5 discusses a number of robustness checks. Finally, section 6 concludes.

2. Methodology

2.1 Conceptual Framework

Our conceptual model is a two-period characterization of early child development. Consider a framework in which the first part of early childhood (the first 2-3 years) is considered as Period 1, and the remainder of early childhood as Period 2. Denote nutritional status accumulated at the end of Period 1 of child k from household h as $H_{t-1,k,h}$ and pre-school cognitive achievement at the end of Period 2 as $CA_{t,k,h}$. We assume that $H_{t-1,k,h}$ summarizes all the investment made in the child during Period 1. In turn, $H_{t-1,k,h}$ is assumed to be an input for $CA_{t,k,h}$. Both variables are chosen by parents on the basis of preferences, budget constraints and initial conditions. We focus on the following equation,

$$CA_{t,k,h} = \alpha H_{t-1,k,h} + X_{t,k,h}\Pi + \eta_{CA,h} + \mu_{CA,k,h} + \epsilon_{k,h} \qquad (1)$$

where $X_{t,k,h}$ is a vector that includes Period 2 child and household observable characteristics that have an influence on cognitive achievement; $\mu_{CA,k,h}$ represents child unobservable characteristics; and, $\eta_{CA,h}$ captures unobserved household and environmental characteristics affecting cognitive development. Equation (1) can be interpreted as a conditional demand function for cognitive achievement such that $H_{t-1,k,h}$ is the input of interest and $X_{k,h}$, $\mu_{CA.k.h}$ and $\eta_{CA,h}$ are unobservable determinants of parental cognitive investments. For instance, $\eta_{CA,h}$ reflect aspects such as household intellectual environment, whereas $\mu_{CA.k.h}$ incorporates aspects such as child innate ability (for a similar setup, see Glewwe and Miguel, 2008). As described in Behrman (1996), Behrman and Lavy (1994), GJK and AHK, the main challenge of estimating equation (2.1.1) arises from the possibility that at least one of the following conditions does not hold:

$$E(H_{t-1,k,h}, \eta_{CA,h}) = 0 \qquad (2)$$
$$E(H_{t-1,k,h}, \mu_{CA,k,h}) = 0 \qquad (3)$$

If either condition (2) or condition (3) does not hold, then an OLS estimation of the parameter of interest, α, would be biased. A violation of condition (2) could arise if there are unobservable household characteristics that simultaneously explain why some families are more likely to raise both healthy and well-educated children. Specifically, determinants of child health not included already in $X_{k,h}$ might be correlated with household unobservable characteristics that influence cognitive achievement (e.g., parental health knowledge might be correlated with household intellectual environment). Similarly, unobserved community characteristics, if correlated with health status (e.g., communities with better health services are also likely to have better educational services) would also lead to violations of (2). In turn, a violation of condition (3) could arise if child-specific unobservables are correlated with health status. Two possible mechanisms for this phenomenon have been suggested in the literature. Firstly, parental nutritional investments might be adjusted as a child's innate cognitive abilities are revealed—condition (2). Secondly, the health status and the cognitive ability of a child might be correlated through a common unobserved genetic endowment—condition (3).

Although in principle an instrumental variable approach should suffice to deal with endogeneity due to infringements of conditions (2) and (3), finding a valid, strong instrument for pre-school nutrition is challenging. Instead, the standard approach has consisted of following a two-prong strategy whereby household fixed effects and instrumental variable are jointly implemented. In the context of cognitive returns to investments in early nutrition, this was first applied by GJK and AHK, in turn echoing earlier studies (see Rosenzweig and Wolpin (1995) for an example and references to studies that have used kinship data). Specifically, assuming data on cognitive achievement and nutritional status is available for a pair of siblings i and j, one can estimate a sibling-difference version of equation (1). As illustrated by condition (6), such a strategy allows us to eliminate any factor that is common across siblings.

$$CA_{t,i,h} = \alpha H_{t-1,i,h} + X_{t,i,h}\Pi + \eta_{CA,h} + \mu_{CA,i,h} + \epsilon_{i,h} \quad (4)$$
$$CA_{t,j,h} = \alpha H_{t-1,j,h} + X_{t,j,h}\Pi + \eta_{CA,h} + \mu_{CA,j,h} + \epsilon_{j,h} \quad (5)$$
$$\Delta_{i,j} CA_{t,h} = \alpha \Delta_{i,j} H_{t-1,h} + \Delta_{i,j} X_{t,k,h}\Pi + \Delta_{i,j}\mu_{CA,h} + \Delta_{i,j}\epsilon_h \quad (6)$$

In effect, this sweeps out any potential bias due to departures from condition (2) but leaves endogeneity due to violation of condition (3) unresolved. Parents might still be allocating investments differently across siblings based on sibling differences unobserved to the researcher. Alternatively, inherently healthier siblings might also be more likely to be more intelligent by nature. Thus, an instrumental variable approach is still required in condition (6) for the $\Delta_{i,j} H_{t-1,h}$ term. In addition, the use of instrumental variables helps dealing with the increased noise-to-signal ratios that occurs when implementing sibling-difference methods (Ashenfelter and Krueger, 1994). The data collected on siblings (see below) were collected only in the latest round of the survey, and therefore in the empirical application we are effectively constrained to use contemporaneous height-for-age instead of its lag (e.g., H_t not H_{t-1}). However, the long-term nature of height-for-age as an indicator of nutritional investments, especially in the critical period for each child, combined with the choice of instruments reflecting events that took during Period 1 of our conceptual model, mitigate the shortcomings of the data. Cunha and Heckman (2007) and Heckman (2007) provide a discussion on the theoretical notion of a "critical period" for child development in the early years, when there are complementarities between investments in the early period, and those in subsequent periods.[4]

2.2 Empirical Strategy

Empirically, we estimate equation (6) using data on matched-siblings born in 2001-2 and 2003-5, respectively. The data allow us to compare two siblings at a similar nutritional and cognitive developmental stage. More specifically, we relate differences in cognitive achievement and Height-for-Age between siblings when aged approximately 4-6 years but measured at different points in time, 2006 and 2009 for the index and sibling children respectively. Our baseline econometric specification is represented by equation (7).

$$\Delta_{i,j}^{06,09} PPVT_h = \alpha \Delta_{i,j}^{06,09} HAZ_h + \gamma_1 \Delta_{i,j}^{06,09} Age_h + \gamma_2 Demo_h^{06} + \vartheta_c + \varepsilon_h \quad (7)$$

We include a range of variables designed to capture changes in the conditions that might have effected cognitive investments across siblings, $\Delta_{i,j} X_{t,k,h}$. Equation (7) includes controls for household demographics ($Demo_h^{06}$), changes in community services (ϑ_h), as well as sibling differences in age between 2006 and 2009 ($\Delta_{i,j}^{06,09} Age_h$).

Household demographic controls are designed to capture differences in parental investments that might lead sibling to follow different developmental paths. We address two possible such patterns of differential investments, both linked to parental child preference. First, birth order might play a role in both the time dedicate to a child and the level of resource competition in the household. The index child might have benefited more for being of a lower

[4] There is limited evidence on what precisely the critical period is (are) for aspects of child development, though a consensus that the fetal period and the first three years are extremely important. Almond and Currie (2010) review the limited evidence. See also Raikkonen et al. (2009) for a cohort study.

birth order relative to her sibling[5] or because the household was smaller at the time. We deal with this by including controls for the birth order of the index child and for the number of siblings born after the index. Secondly, the gender of the child might be a determinant of investments. While extreme patterns of gender bias, such as the "missing women" cases found in South Asia, are not known in Peru, task allocations—such as chores, child care and other household production activities—are likely to be linked to both gender and birth-order issues. Accordingly, in our model specification, we include dummies for all gender-birth-order combinations. The baseline model also includes community fixed-effects controls, (ϑ_h), designed to capture changes at the community level, as they could also drive differences between siblings according to their date of birth (e.g., changes in access to and quality of preschool programs).[6]

We apply IV estimation methods to equation (7) to address the remaining endogeneity related to unobserved child-specific investments. The application of IV methods requires the identification of instruments that meet the following conditions:

$$E(\Delta_{i,j} HAZ_h \cdot Z) \neq 0 \quad (8)$$

$$E(\Delta_{i,j} \epsilon_h \cdot Z) = 0 \quad (9)$$

Condition (8)—or the strength condition—states that the vector of instruments (Z) should be correlated with the endogenous variable. "Weak" IVs can lead to biased estimates and invalid standard errors. Condition (9) defines a valid IV. The instrument should not be correlated with the error term in the main equation. In our context, this means that we are looking for events that are exogenous to the determination of a child's PPVT score but are sufficiently strong to affect the stature of the child.

As a potential source of exogenous variation that could meet these requirements, we look at changes in the conditions faced by each of the siblings during their first three years of life. We consider two sets of instruments for within-siblings nutrition. The first set of instruments corresponds to changes in a selected group of food items that together represent around 54

[5] There is indeed evidence pointing out towards this possibility (e.g, Behrman, 1988).

[6] In our empirical analysis, we use $H_{t,k,h}$ as a proxy for $H_{t-1,k,h}$, because we do not observe the latter for both siblings. Although we acknowledge concerns of a possible simultaneous determination of $H_{t,k,h}$ and $CA_{t,k,h}$, our instrumental variable strategy, where we use shocks that took place during the first three years of life to identify $d_{i,j} CA_{t,h}$, reconciles our estimation this with our conceptual model.

percent of the household consumption basket. Our motivation for this choice of instrument set stems from the 2006-8 food price crisis. Of particular interest is the fact that the older siblings, born between 2001-2, were not affected by the crisis during their critical nutritional period, while their younger siblings, born between 2003-5, were. Thus we compare changes in the prices faced by the siblings between the sixth and the 35th month of life (the first six months of life being excluded as this is the period when a child relies exclusively on breastfeeding).

The second set of instruments we use corresponds to household-level shocks that took place during Period 1 of either the index child or her younger sibling. Specifically, we focus on negative shocks such as frost and illness or death of other household members that took place between 2000 and 2002 and between 2007 and 2009. The former period is linked to the critical period of the older siblings at a time in which their younger siblings had not yet been born, while the latter is associated to the critical period of the younger siblings at a time where the older siblings had already surpassed their critical nutritional period; in fact, given that the cognitive scores for the older siblings were already collected by early 2007, the possibility that the 2007-9 shocks could have had an effect on the older siblings is ruled out. Therefore, we treat these household shocks as child-level shocks. We present the selected set of instruments in more detail in the next section. Conditional on $\Delta_{i,j} X_{t,k,h}$, the selected instruments are assumed to act only through a nutritional channel. GJK list the ideal requirements for the use of shocks as instruments. Shocks should be (a) of sufficient magnitude and persistence to affect a child's Height-for-Age; (b) sufficiently variable across households; and (c) sufficiently transitory not to affect the sibling. We believe that our proposed set of instruments are well placed to meet these stringent conditions and, in particular, are sufficiently short-lived to have little impact on later cognitive achievement other than through their impact on physical growth.

The conceptual framework places particular emphasis on the necessity to control for changes in household circumstances over time ($\Delta_{i,j} X_{t,k,h}$) as a control for differences in cognitive investments across siblings. As a robustness check on our main econometric specification, equation (7), we include information on changes in non-food household real expenditure per capita and household assets measured at period t.[7] The inclusion of these additional controls serves an additional purpose. Shocks that have a persistent and delayed effect on household welfare, through reduced assets and income generating abilities, could be creating a spurious relationship between cognition and nutrition. Controlling for changes in the household following the occurrence of the shocks, ensures that the exclusionary restriction, equation (9), is not violated.

A further set of robustness checks concerns unobserved cognitive investments outside the household. At time of measurement, a substantial number of the children in our sample are enrolled in preschool, while a small proportion of older children have even started primary school education. While the sibling-difference model combined with controls for age-differences should capture the variation generated by the timely enrolment of both siblings, differences between siblings in preschool enrolment as well as the age of preschool enrolment could lead to biases in our estimates. In particular, to the extent that parents decide to delay, or bring forward, enrolment of the sibling because she is smaller, or bigger, than the index child at the same age, this will create a positive correlation between the PPVT and Height-for-Age scores unrelated to the nutrition-cognition causal link (Glewwe and Jacoby, 1995).While we do not have information on preschool enrollment of the siblings, in our robustness checks section, we test the sensitivity of our core results to including controls for preschool enrollment and age of enrollment of the index child only.

3. Data

In our analysis, we make use of the Young Lives Peru Survey, a longitudinal sample of a cohort of children born in 2000-2001. The baseline sample is cluster stratified, with 20 districts randomly selected across the country (seven on the coast, 10 in the highlands and three in the jungle). The districts were chosen from a list of districts that excluded the top 5 percent of districts as measured by a district poverty ranking. This was in line with the policy aim of the project of oversampling children living in poor households (Wilson et al., 2006). Within each selected district, around 100 households with at least one child born between 2001 and 2002

[7] Changes in household assets holding are proxied by changes in an estimated wealth index. This index is the average of three sub-indices: a consumer durables index, an access to services index and a housing quality index. We follow a definition of this index equivalent to that used in the Demographic and Health Surveys.

were chosen randomly to participate in the project. The panel of children that is being followed is 2,000 (hereafter, the index children).[8] The survey collects information about these children, their families and their local communities (*centros poblados* or towns).[9] Currently, three survey waves are available: the baseline round in 2002 and two follow-ups in 2006-7 and 2009. During the time of the surveys, the index children were aged 6-20 months, 4-6 and 7-8 years of age, respectively.

A common problem in longitudinal studies arises due to household attrition. However, this is unlikely to be a problem in this case. Attrition rates between rounds are very low by international standards, with only 3.7 percent of the children lost or dropped out between the two rounds in total, leaving a panel sample of 1,963 children. Further analysis suggests that attrited households are not systematically different from non-attrited households based on observable characteristics (Dercon and Outes-Leon, 2008). While differences in unobservable characteristics cannot be ruled out a priori, the low attrition rates found suggest that potential biases in the results due to attrition are likely to be small.

3.1 Measurement Variables and Analysis Sample

We use the Peabody Picture Vocabulary Test (PPVT), Spanish version, as the measure for cognitive achievement, while nutritional status is proxied by the height-for-age z-score (HAZ). The PPVT is a test of receptive vocabulary. Children were asked to select between four pictures the one that best represented the meaning of a word presented to them orally by the enumerators. The number and the level of difficulty of questions differ according to child's age (see Cueto et al., 2009), for details of the test and its properties in the context of the Young Lives samples). A number of studies have used this test as the basis for investigations into cognitive development in Spanish-speaking countries (e.g., Paxson and Schady, 2007). We standardize the raw PPVT test score by age cohort, e.g., for 3, 4, 5, 6 or 7 years old, to have a mean of zero and a standard deviation of one.

To measure the stock of nutritional achievement of the children we use the height-for-age z-score (HAZ). Height-for-age z-scores are recommended by the World Health Organization

[8] For a detailed description of the sampling design, see Escobal and Flores (2008).

[9] In many instances, the districts selected contain many *centro poblados*. The community-level surveys were administered in the 80 *centros poblados* identified within the 20 districts selected

(WHO) as a measure of child development, in particular as a correlate of long-run investments in child nutrition (i.e., the "stock" of health); they show the height of the child relative to a reference group of healthy children. These measures were updated in 2007.[10]

In the 2009 wave, for each sampled household, the anthropometric module and cognitive achievement test were also administered to the sibling born immediately after the index child—hereafter, the younger sibling—provided he/she was at least four years of age at the time of the survey. The vast majority of these younger siblings were born between 2003 and 2005, so that they were between four and six years of age when the data were collected, a very close match to the age-period of the index children in the 2006-7 survey wave.

As described above, our analysis relates differences in PPVT scores between siblings and their Height-for-Age measure when children were of a similar age but at different points in time. Therefore, in our analysis we exclusively use the sample of matched index and sibling children for which anthropometric and cognitive data was collected, which consists of 900 children in 450 households.[11] As shown in Table 1, most of the children, index or sibling, were aged between 4-6 years of age at the time of measurement, 2006 and 2009 for the index and sibling children, respectively.[12]

Basic descriptive statistics comparing the paired-siblings households to the rest of the Young Lives households are reported in Table 2. We see that there are some significant differences between the households used in the siblings analysis, suggesting that our sample of analysis could be a selected sample. This is not surprising given the sampling frame applied in the collection of the siblings. However, to the extent that we treat our results as representative only of relatively young (and poor) families that have at least two children, sample selection should not be of particular concern. We nevertheless estimate the cross-section for cognitive achievement using OLS and reject the null hypothesis that the cognition effect of nutrition is significantly different between our paired-siblings sample and the excluded sample (not

[10] See http://www.who.int/growthref/ and references on the website.

[11] To remove implausible observations and alleviate the problems of attenuation bias in our sibling-difference specification, the sample of matched-siblings used throughout the paper excludes the top and bottom 2.5 percent tails of the sibling-difference Height-for-Age distribution.

[12] Even though the sampling frame for the siblings would have ruled out children below 4 years of age, Table 1 shows that data was collected for six siblings aged 3 years. To prevent further reductions to an already small sample size, we include these children in our analysis. However, their elimination would be inconsequential to our results.

reported). This alleviates concerns that our results could be driven by sample selection, and suggests that the insights of our analysis could be potentially extrapolated to the wider YL sample.

Table 2 presents some descriptive statistics for the index children and their younger siblings. On average the younger siblings are better nourished than their older counterparts. As shown in the next sub-section, siblings were exposed to substantial price increases during the 2006-2008 period, prior to their measurement. If these price increases had an effect on nutrition, as would be required for them to be *strong* instruments, this is not apparent from Table 2. However, these differences are hardly surprising considering that they were measured three years after their older *index* siblings; their better nutrition is possibly a reflection of being born at a later stage of the household's life-cycle, benefiting from improved economic conditions, or simply the results of a secular trend. They could also be linked to improvements in access to health services and nutritional programs at the community level. As described above, in our econometric specification we include district dummies and controls for non-food consumption and household assets designed to capture time-varying effects resulting from life-cycle trends.

Before presenting the results of the econometric analysis, it is instructive to plot the correlation between nutrition and cognition in our data. The solid line in Figure 2 presents the kernel density of Height-for-Age sibling-difference, while the dashed line depicts the kernel smoothing estimate of PPVT on Height-for-Age in sibling-difference form. The third line, in dash-dot form, shows the kernel smoothing estimates for the pooled OLS model.[13] We find that the pooled OLS slope is substantially steeper than the nutrition-cognition slope for the sibling-difference model, suggesting that time-invariant household characteristics substantially bias the cognition- nutrition vector. However, as discussed above, the sibling-difference relationship depicted in Figure 2, might at the same time mask substantial endogeneity. While the increased attenuation bias will bias the slope downwards, differential cognitive investments across children could be positively or negative correlated with nutrition, implying that *a priori* it is not possible to sign the direction of the remaining bias. The aim of our instrumentation

[13] Note that the x-axis in Figure 2 is to be interpreted very differently depending on whether we are considering the sibling-difference or pooled OLS relationship. While the ΔHAZ mean is approximately around the value of zero and has no nutritional meaning, the levels line depicts the Height-for-Age in our sample.

strategy is to establish the direction of that bias. We now turn to the discussion of our instruments.

3.2 Instrumental Variables

We use two sets of exogenous instruments; food price changes and idiosyncratic shocks that affected the household in the critical nutritional period. Food prices are clearly relevant to nutrition, exogenous to the household and vary sufficiently over time during the period of study. Moreover, in light of the nature of the food price crisis, we argue that the event was grave and sufficiently short-lived around the critical period of one of the siblings cohorts: the younger sibling. Figure 1 shows that the most dramatic stage of the crisis took place between 2006 and 2008, coinciding with the critical period of the younger siblings born between 2004 and 2005. In contrast, the older siblings born between 2001 and 2002 had already transitioned out of their critical nutritional stage before the beginning of the crisis.

For the purpose of estimating the impact of the food crisis on child nutrition, we do not center exclusively on products consumed by the child, because the increase in prices was generalized. Rather, we look at fluctuations in food price categories deemed important for overall household food consumption and study their effect on within-siblings early nutrition. There are likely to be a number of channels through which food prices impact child nutrition, none of which can be ruled out *a priori*. For households that are net consumers, increases in food prices can lead to a reduction in child food overall intake both in terms of the quantity and in the quality of the food consumed, either because the good is directly consumed by the child or because of a reallocation within the household consumption basket. In turn, for households that are net producers, increases in food prices can have a positive effect on nutrition due to their positive effect on household income.

Taking price data from the Peruvian institute of statistics (*Instituto Nacional de Estadística e Información*, INEI), we create a child-specific variable, representing food prices in the first three years (excluding the first six months of breastfeeding), disaggregated by semester (6-11 months, 12-17 months, 18-23 months, 24-29 months, 30-35 months). This strategy was used by Glewwe and King (2001); food prices were also used by Alderman et al. (2001) with slightly broader age categories. Creating the child-specific variable allows us to introduce as much heterogeneity as possible both across and within households; nonetheless, it should be remembered that, by virtue of the cohort sample design, the index children are all born within a year of each other.

The food prices we use were obtained from data reported by INEI, who collect price data on a monthly basis across the main cities of the country in order to construct regional consumer price indices.[14] For our purposes, we impute prices by matching the 20 clusters sampled by Young Lives to the prices prevalent in the associated capital of the Department where the districts are located. We then match these prices to each child according to date of birth and use as IV the siblings-difference in log-prices. Note that, since our estimation controls for cluster fixed effects, the effect of prices is identified by sibling differences in date of birth within each cluster.

Concerned with parsimony in the IV specification,[15] we include three of the most important food price sub-categories i) : Bread and Cereals (comprising subcategories wheat, rice, maize, pasta as well as bread and biscuits); ii) Meat: (comprising chicken, red meat and meat products, and processed meat); and iii) Dairy: Milk, Eggs and Cheese. The three categories are important for their calorific and quality of protein content, and they also represent just over 50 percent of the purchased consumption basket in the sampled families.[16] We also include the category of Tubers (comprising potatoes, cassava, yucca, and Andean tubers) as the largest category of home production. All four price categories represent 56 percent of the purchased consumption basket in the third round, and 64 percent, including own-consumption, showing that many households produce their own tubers.

Finally, whilst we are convinced that food prices fulfill our econometric requirements in terms of validity, their potential impact on early child nutrition is interesting in itself, given that the study period represents a period of rapidly escalating prices that has caused much concern in policy in the past few years. Figure 1 shows the evolution of prices between 2001 and 2009 for the main food groups used in the analysis (Cereals, Dairy, Meat and Tubers). We can see that the

[14] The geo-political map divides the country into 25 Departments, in turn disaggregated into provinces and districts; INEI collects information for the capital districts of each Department and for other cities.
[15] INEI reports estimates for the main price indices that conform the consumer price index (eight categories), including the food and beverages price index, in turn disaggregated into 14 sub-categories. Using all the sub-categories would generate 70 IVs; with a dataset of only 450, this would be extremely demanding of the data.
[16] Figures computed using information collected in the third wave of data in 2009. Note that we do not have information on food consumption for individual children, only at the household level.

food price "crisis" began in late 2005 and prices rose for most of 2006-2008, and how this corresponds to the age groupings of the cohorts in the sample.

When using price changes as instruments for height-for-age, our underlying assumption is that the price changes had an effect on household expenditure, either negatively because of classical price effects for *goods* or positively because of profits effects among net sellers. We tested this channel explicitly by exploring the extent to which the price data is correlated with changes in household consumption and its potential heterogeneous effects.

We ran household-level regressions of the effect of changes in prices between survey rounds on household food expenditure growth between 2002 and 2006.[17] We include a range of household and community level controls, all measured as changes between rounds, and explore heterogeneity by splitting the prices series by rural/urban areas and farmer/non-farmer households.[18] Column 1 in Table 5 shows the cereal price has a clear, and negative, impact on food expenditure but other categories seem to have little impact. In columns 2 and 3 we explore the heterogeneous impact of the prices. First, we interact prices with an indicator variable of whether the household head's main occupation is agriculture, then, in column 3, we interact them with a rural dummy. Against our expectations, being a farming household appears to have little specific effect on the importance of prices; instead, whether a household lives in a rural community seems to matter for the consumption effect of the price changes. In particular, we find that both Tubers and Dairy products appear to have a significant profit effect that offsets the consumption effect somewhat in rural areas, while—at least in the latter category—having a purely negative consumption effect in urban areas.

We also include idiosyncratic shocks as instruments. While the mechanisms of these shocks is similar to the price changes (that is, reductions in the nutritional intake of a specific child during the critical developmental period), they complement well the price data because they introduce further household-level heterogeneity as well as a different source of nutritional variation. We include three different such events, one from the 2002 wave and two from the 2009 wave. We do not include shocks from the 2006 wave as they could have affected both

[17] Community data for 2009 survey were not available at the time of writing

[18] To run these regressions we construct a different type of prices series from the one used in our sibling-difference IV specification. We compare the changes in household consumption between rounds 2002 and 2006 with the price changes recorded between rounds. We use the local price on the date of interview of a household for constructing the household specific price series.

children during their critical period. From the 2002 wave, we use a dummy for whether any of a list of negative events affected the household between the index child pregnancy period and the time of the survey, when the index children were aged between 6 and 20 months. By definition, these events could only have directly affected the index child, as the younger sibling was yet to be born. Additionally, from the 2009 round, we include indicator variables for whether the household was affected by an event of severe frost, as well as whether a member of the household other than the child died or suffered an illness between 2006 and 2009. Either of these events could not possibly have affected the nutrition of the index child, given that we only use their measurement from the 2006 wave. While household shocks are arguably less exogenous to the household than the price data—their incidence and severity potentially correlated with unobserved household characteristics—this concern is allayed when applying our sibling-difference model specification. In turn, there is no reason to presuppose any of the household shocks to be correlated with child-specific unobservables.

4. Results

We report pooled OLS and sibling-difference OLS results in Table 3. For the pooled OLS model, column 1, we include a parsimonious set of child and household-level controls[19] as well as community fixed effects. Recall that the cross-sectional pooled OLS is likely to be biased due to unobserved household heterogeneity. Columns 2 to 4 in turn report results for the sibling-difference model specification following equation (9). Column 2 includes controls for differences between siblings in terms of age, sex and birth order, while column 3 adds cluster fixed effects, which are designed to capture community-level changes.[20]

Table 2 shows that both pooled OLS and sibling-difference models significant and—as expected—positive impact of nutrition on cognitive development and that sibling-difference estimates are robust to the inclusion of controls for community changes. Consistent with the

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[19] These include, child's sex and age, mother's years of schooling, household size, household per capita non-food expenditure expressed in logs and household wealth index

[20] Note that in column 1, robust standard errors are clustered at the household level. All other results throughout the paper, including columns 2 to 4 in table 3 report standard errors corrected for clustering at the *index child age-community* level. All further specifications report standard errors corrected for cluster and index age-cohort specific correlations. In other words, our inference testing is robust to unobserved correlation between children of the same age living in the same district or cluster

descriptive statistics, we find that the nutritional effect is smaller when we control for unobserved household heterogeneity. However, with coefficient estimates of 0.099 and 0.083 respectively, comparisons between the pooled OLS and the baseline sibling-difference model suggest that the importance of unobserved household heterogeneity might be more modest than commonly assumed.[21]

Finally, column 4, re-estimates the core model specification for the sub-sample of paired-siblings aged four to five years only. Siblings in this reduced sample have an age profile that most resembles the index; more importantly, all children are below the schooling age (six years of age in Peru), implying the effect of unobserved schooling investments can be disregarded in this model. We draw comfort from the fact that, in spite of a substantially reduced sample size, 330 paired-siblings, the coefficient remains significant and virtually unchanged. Even though the core model specification does not control for schooling investments, the evidence suggests that schooling investments in the full sample are uncorrelated with height-for-age. We revisit this issue in the next section.

In Table 5 we report the results for the IV GMM sibling-difference model specification. For comparability, in column 1 we duplicate the results from the sibling-difference OLS with full controls (column 3 in table 4). Column 2 reports estimates for the model where we use differences in food prices during the first 36 months of life of a child as IVs, while column 3 presents results when using child-level shocks as excluded instruments. Finally, column 4 combines both sets of instruments.[22]

Diagnostics of the first-stage regressions indicate that our set of instruments is reasonably strong. When using price changes only as instruments, we obtain a Kleibergen-Paap Wald rk F statistic of 14.53, which is above the critical value for a maximum IV bias of 10 per cent (Stock and Yogo, 2005). While this is a strong result by itself, we also tried combining these

[21] In this sample, it would appear that there is little time-invariant household heterogeneity—taken care of by the sibling-difference model—that is not already captured by the household level controls used in the pooled OLS model. In particular, while a bi-variate regression of PPVT on HAZ yields a large coefficient consistent with Figure 2, the pooled OLS coefficient estimates drop substantially when we control for household assets and non-food consumption. In other words, the dramatic differences in slope in Figure 2 are mostly accounted by observed measures of household wealth rather than unobserved household heterogeneity.

[22] In all cases, results were obtained from a two-step efficient generalized method of moments (GMM) estimator using STATA routines created by Baum, Nichols and Schaffer (2010). Due to the clustered nature of our data, a robust Kleibergen-Paap Wald rk F statistic is reported to test for the presence of weak instruments

instruments with the child-level economic shocks, as this introduces more variation across locations; recall our price-shock data are specific to each child, by virtue of their age, but prices are measured at the community level. The combined set of instruments (column 4) is even stronger, with a maximum IV bias of just above 5 per cent. In column 3, we show the contribution of idiosyncratic shocks alone, but we find as instruments they do not pass the "weak IV" test, implying that second-stage inferences will be invalid and point estimates are likely to include a relative bias between 10-20 percent. Additionally, we test for the exogeneity of our instruments and find that all three specifications also pass the over-identification test (Hansen J-test).

Second-stage results uncover a nutritional effect that is large in magnitude and strongly significant. When using food prices, point estimates suggest that one standard deviation increase in height-for-age yields higher PPVT scores by 17 percent of a standard deviation. When combining the two sets of instruments, point estimates rise to 21 percent of a standard deviation. Point estimates from using shocks only as IVs are even higher, at 24 percent, but the coefficient is much less precisely estimated. Even though the latter estimates contain substantial "weak IV" bias, it is interesting to note that the point estimates are not very different from the results using the price data. Even though the two sets of instruments exploit a different source of nutritional variation, the similarity in coefficients suggests that both instruments might be capturing a single mechanism.

Other studies applying similar sibling-difference IV strategies (e.g., AHK), have also obtained coefficients substantially higher than their OLS estimates. The change in parameter estimates could be attributed to the increased attenuation bias that results from applying differentiae methods. However, comparisons of pooled OLS and the sibling-difference models suggest that in our data measurement error might only be part of the story. While one can only speculate, a plausible explanation is that parents allocated household recourses and investments in a compensatory manner (as suggested by Behrman, Pollak and Taubman, 1982). That is, if parental attentions are dedicated to children with poorer health and lower height-for-age, the resulting higher child-specific unobserved cognitive investments will reduce the correlation between cognition and nutrition in the sibling-difference specification. Our instrumentation strategy therefore yields a parameter robust to the mediation of compensatory households.

4.1 Discussion of First-Stage Results

We now move to discuss the effect of the instruments on height-for-age as a proxy for nutritional achievement, a question of intrinsic policy interest. Columns 1 to 3 in Table 4 report the first-stage results for the three IV model specifications reported in Table 5. Because the height-for-age difference variable is constructed as the difference between the Index HAZ and the HAZ of the sibling, a shock affecting the former would have a negative effect on ΔHAZ. First-stage regressions for the idiosyncratic shocks are reported in column 2. We find, as predicted, that adverse shocks to the household that affected the index child only, reduce the difference in height-for-age between siblings, and that illness and death of household members in 2009 increase the nutrition gap. Incidence of frost, a common weather shock in the Andean highlands, has the correct sign but is only imprecisely estimated.

Results for the effect of food prices are reported in column 1. Recall the food prices are split into semesters of early childhood defined for each child. Even though the full set of food price changes are sufficiently strong instruments, only a few items are statistically significant on their own. Cereal prices in the period 30-35 months, and tubers and meat prices appear in the early years to reduce height-for-age of the sibling, resulting in a positive coefficient on height-for-age differences. On the other hand, meat prices in the period 30-35 months have a negative impact on the difference between sibling heights. This is somewhat puzzling, and we suspect multicollinearity might be affecting both the sign and significance of some of the price variables. As a better measure to determine the importance of the price data on nutrition, we use tests of joint significance across groups of variables.

Column 1 in Table 6 shows a selection of joint significance F-tests by food groups (across all semesters). We find that Bread and Cereals and Meat prices are each jointly significant; Dairy and Tubers prices, however, on average do not affect nutrition significantly. To uncover differentiated effects according to whether the household is involved in agricultural activities and by type of location, in columns 2 to 5 we report F-tests for two alternative first-stage specifications, where we interact the price variables with farmer/non-farmer status (columns 2 and 3) and urban/rural dummies (columns 4 and 5), respectively. We find that Tubers prices matter in rural areas—consistent with our findings in the table—and that Dairy prices matter for households where the head of the household is a farmer. Cereals and Meat prices matter for all the sub-groups, though F-tests suggest that their nutritional impact is stronger for rural households and for farmers.

We also tested across food items for joint significance by time period, in order to investigate whether some periods were more important. For example, in searching for a "critical period" Glewwe and King (2001) found tentative evidence that the period 18-24 months was most important for a sample of children in the Philippines. The period 24-35 months has been identified as a more sensitive period for nutritional development (e.g. AHK on Zimbabwe). We find joint significance of the first and last periods under scrutiny (6-11 months, and 30-35 months). However, one should be cautious when interpreting these results, since in our context the significance of a period will also be determined by the magnitude of the price changes experienced. Indeed, in terms of the timing for our cohorts, the 30-35 months period for the younger siblings corresponds with the years 2006-8, the period in which the largest price increases were experienced.

Whilst our data are not comprehensive enough to offer more than suggestive evidence about the precise timing and channels of impact, there is clear evidence that the price crisis has fed through into nutritional outcomes of this sibling sample, and, given our second-stage results, is also affecting cognitive development through the nutritional channel.

5. Robustness Checks

An extensive literature on health and nutrition has also explored the fetal and pre-natal period. Most of the studies agree that deficiencies during pregnancy can have health effects in the medium- and long-term (Godfrey and Barker, 2000), although a few studies have also found short-term effects (Maccini and Yang, 2009). In order to keep the number of instruments manageable, we excluded price changes for the pre-natal and breastfeeding period. While appearing to some extent *ad hoc*, this choice is corroborated by further robustness tests. When we re-estimated our main IV model (column 2 in table 5) with the two pre-natal and the breastfeeding semester replacing the two semesters in the third year of the critical period, we found that—with a Kleibergen-Paap F-statistic of 6.0—the revised IV set was sufficiently strong to pass the "weak IV" test. However, the pre-natal and breastfeeding prices had only limited power in explaining height-for-age differences.[23]

Our choice of instruments could be subject to further criticisms (Glewwe and Miguel, 2008). While the exogeneity of our instruments is clear, at least for the price data, price and household level shocks could conceivably have persistent effects on cognitive achievement other than through nutrition through a direct effect on household cognitive investments. In particular, a shock or increases in food prices in one period could crowd out expenditure in educational items in the next period through a reduction in savings or if the household had to sell assets as a result of the event. If this were indeed the case, our instrumentation strategy would be invalid, because our instruments would be correlated with unobserved cognitive investments; our IV GMM estimates would merely be quantifying the effect of such a reduction in investments rather than the nutrition effect. However, it should be noted that our core IV GMM sibling-difference model is robust to certain types of persistence. Indeed, if a shock has not only an immediate effect on household nutritional investments but also a *permanent* effect on household cognitive investments, the within-sibling specification would partly capture this phenomenon. That is, to the extent that a shock is permanent, both siblings should be similarly affected by the reduced household wealth—this would be the case for shocks affecting the index child's critical period, but not that of the sibling (given that we have already measured the index child's cognitive development at an earlier period).

To address this possible concern, we augment our core model specification to include controls for changes in household assets and non-food consumption. To the extent that shocks affect cognitive investments through reduced household wealth, the additional controls "switch off" the argument of the invalidity of our instruments due to their persistent effect.

Table 7 reports estimates for the augmented model specification. Columns 1 and 2 reproduce the core results from the previous section when using prices only and prices and shocks as instruments. Columns 3 and 4 expand the core model to include changes in household assets between 2006 and 2009. Since changes in assets might be an imperfect proxy for changes

[23] In particular, we find the semester (-2), semester (-1) and semester (1) have F-Statistics of 9.4, 5.4 and 3.7 respectively, with only the first group statistically significant. Even though estimates might contain substantial 'weak IV' bias, it is interesting to note that the second-stage estimates yield a nutrition coefficient that was positive and significant. Results not reported in tables, but can be requested from the authors.

in cognitive investments, columns 5 and 6 report results that also include changes in non-food consumption. Finally, columns 7 and 8 also include controls for changes in households' assets between 2002 and 2006.[24] The inclusion of earlier changes is motivated by the concern that some of the price changes might have affected income wealth before 2006; however, by controlling for these early changes we are also eliminating some of the exogenous variation of the instruments that we wanted to exploit. It is therefore not surprising to find that the strength of the IVs is most reduced in columns 7 and 8.

In spite of the importance of controlling for household changes in wealth, as a proxy for changes in cognitive investment, results in Table 7 show little evidence that they affect PPVT sibling differences significantly; while changes in assets have a large coefficient, these are also very imprecisely estimated. When turning to the nutritional effect, we find that our core results remain very robust. On the one hand, we find that the inclusion of the additional controls has only a limited effect on the first-stage strength of the instruments; indeed, the instruments remain "strong" in all model specifications.[25] On the other hand, point estimates remain remarkably stable, largely staying in the 17-21 percent range. We can conclude that if our set of instruments has persistent effects on household welfare, these phenomena do not affect the nutrition-cognition estimates.

A further source of concern in our analysis is with respect to child-specific school investments. On the one hand, some children in our sample are already of schooling age—six years being the standard age of school enrollment. On the other hand, even if not of schooling age, a large proportion of children may already be enrolled in pre-school. Failing to control for differences in schooling between siblings could be biasing our estimates. The concern is that children that are physically small and possibly frail might have their enrollment delayed, and if so, at the time of testing, these children are likely to have a lower PPVT score. The instrumentation strategy is particularly vulnerable to this critique, since it has been shown that early stunting is commonly associated with delayed school enrollment (Glewwe and Jacoby,

[24] Ideally, we would have also included controls for changes in non-food consumption between 2002 and 2006, but no data on non-food consumption were collected in the 2002 wave.
[25] The model in columns 7 has the lowest Kleibergen Paap F-Statistic, 10.68, marginally below the Stock-Yogo critical value for an IV bias of 10 percent.

1995; Alderman et al., 2001; Glewwe, Jacoby and King, 2001). Our IV estimates could therefore be capturing the nutritional effect of delayed enrollment rather than the nutrition-cognition link.

Table 8 reports a range of robustness checks on our core IV model specification aimed at addressing the problem of omitted cognitive investments originating from delayed pre-school or school enrollment.[26] The pre-school and school data available for the index child are extensive, but for siblings these data are largely unavailable. On pre-schooling we only have information for the index child, while for primary school, we only know whether a sibling was enrolled in 2009 but not their age of enrolment. The data limitations imply that for age of enrolment we can only include controls for the index child; we consider this variable a proxy for sibling differences in age of enrollment. If an index child has a particularly early age of enrollment, it is arguably likely that the sibling might have a later enrollment; in our regressions, we would then expect the *levels* measure of age of enrollment of the index child to be positively correlated with the PPVT score *sibling difference* measure.

Columns 2 to 4 in Table 8 report IV estimates when we include pre-school enrollment and age of enrollment for the index child only. On its own, pre-school enrolment has a large and positive effect on PPVT differences (see column 1). Recall that the PPVT difference variable is constructed as the difference between the Index PPVT score minus the PPVT score of the sibling. Pre-school enrollment of the index child therefore increases the gap between the index and its sibling. Similarly, and as expected, ΔPPVT is decreasing in the age of preschool enrolment (column 3), although when both enrolment and age are included, delayed enrolment (from 3 to 5 years of age) does not have a significant effect (column 4). Columns 5 and 6 expand our set of controls to include primary school. Similar to pre-school enrollment, we find that ΔPPVT is decreasing in school enrolment of the sibling and the age of school enrollment of the index child. At the same time, we find that across all of alternative specifications, both first-stage and second stage IV estimates remain robust. When we control for age and enrollment in school and pre-school, we obtain a Kleibergen-Paap F-statistic of 19.99—suggesting an IV bias of just below 5 percent, and find that a one standard deviation increase in height-for-age improves

[26] Note that for our OLS model we reported estimates for the restricted sample of children aged 4 and 5. This sample excludes all children of schooling age but not children that might be enrolled in pre-school. We do not apply this approach to the instrumental variable model, because the restricted sample of 330 observations is too small to produce reliable 2SLS given our large number of instruments.

PPVT by 19.2 percent of a standard deviation.[27] This is a remarkable finding in that, though both schooling and pre-schooling are strong determinants of sibling cognitive differences, their inclusion has no meaningful effect on our estimates of the nutrition-cognition parameter.

6. Conclusions

The importance of a good start in life cannot be overstated, and early child outcomes have strong predictive power for future life chances. In the context of Peru, where many children are malnourished, and affected along with many other countries by the food price crisis of 2006-2008, we have revisited the nutrition-cognition relationship. We have provided evidence on the link between nutrition and cognitive achievement for a group of pre-school age children, by using a within-sibling estimation strategy combined with instrumental variables in order to convincingly determine causality.

We find that an increase in the Height-for-Age z-score of one standard deviation—keeping other factors constant—translates into increases in the Peabody Picture Vocabulary Test (PPVT) score of 17-21 percent of a standard deviation.

Our instruments include both covariate and idiosyncratic shocks, and in robustness checks, we show that controlling for household assets and non-food consumption does not affect our estimates—in other words, if there is a direct and longer-term impact on cognition of the shocks through the household budget, this does not affect the strength of the nutritional channel. Further, we test concerns that the results are driven by shorter children being less likely to enrol in school, by using the data we have on index children's enrolment as a proxy. Our results remain robust to these sensitivity tests, though we do find significant effects of schooling on cognitive development.

The results are of policy concern, not least because this sample of children are only 4-5 years old and we do not yet know the long-term effects of their nutritional deficits. The findings suggest that early nutritional interventions can have substantial cognitive benefits, more so considering that, unless addressed, early deficits are likely to be followed by further deficits in

[27] Table 8 includes both prices and shocks as instruments. Results remain unchanged when using prices only. Indeed, when we re-estimate column 5 with prices only, we also obtain a point estimate of 0.19, and a Kleibergen-Paap F-statistic of 16.7.

human capital accumulation resulting from delayed school enrollment, poor educational progression and early drop-out rates.

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Annex 1.

Table 1. Age Distribution: Index versus Younger Siblings, 2006 and 2009 YL Waves

	Age of Younger Sibling, 2009					
	3y	4y	5y	6y	7y	Total
Age of Index, 2006						
4y	1	40	60	17	0	118
5y	7	95	135	77	6	320
6y	0	3	4	5	0	12
Total	8	138	199	99	6	450

Table 2. Descriptive Statistics: Full versus Paired-Siblings Sample

		Paired-Sib. sample	Rest of YL sample	Diff.
Age of mother in 2001	Mean	25.41	27.49	***
	Std.Err.	.287	.180	
Mother's years of schooling	Mean	6.57	8.15	***
	Std.Err.	.216	.115	
Height-for-age of index child	Mean	-1.808	-1.848	
	Std.Err.	.050	.088	
Raw PPVT score of index child	Mean	24.706	31.102	***
	Std.Err.	.821	.475	
n		450	1514	

Table 3. Descriptive Statistics: Index versus Younger Siblings

		Index Children, 2006	Younger Siblings, 2009	Diff.
Height-for-age	Mean	-1.808	-1.613	***
	Std.Err.	.050	.050	
Raw PPVT score*	Mean	-.022	-.0026	
	Std.Err.	.046	.048	
Age (in years)	Mean	4.764	4.907	***
	Std.Err.	.023	.038	
% of male	Mean	.499	.434	*
	Std.Err.	.024	.023	
n		450	450	

*Raw PPVT scores standardized to have mean/var 0/1 across age-groups.

Figure 1. Evolution of Food Prices in Peru: 2000-2009

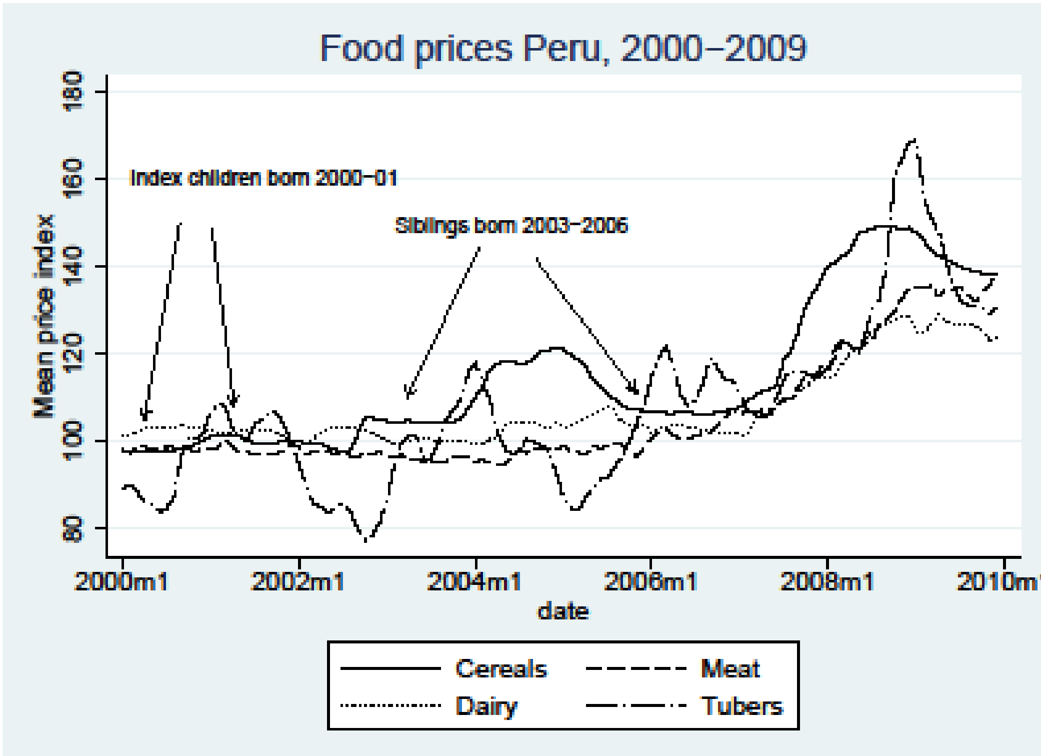

Figure 2. Non-Parametric Nutrition-Cognition Relationship: Pooled Sample vs Siblings-Difference Sample

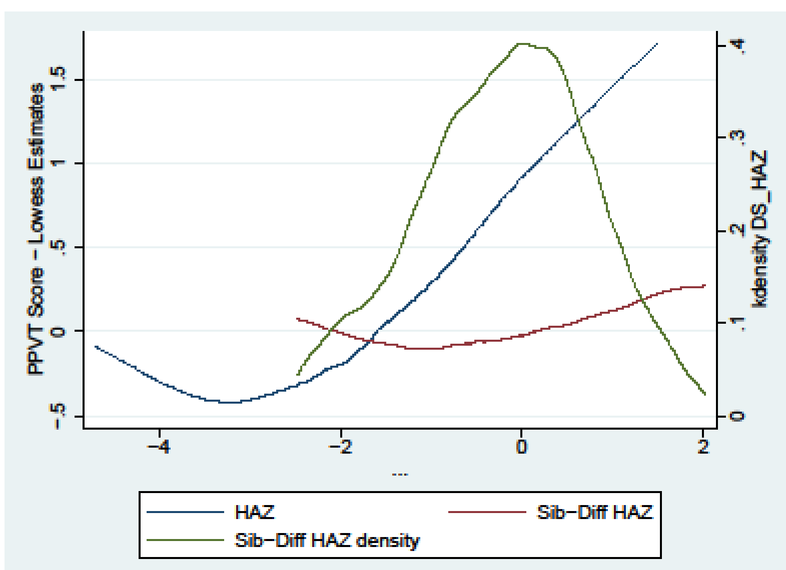

Table 4. Dependent Variable: PPVT Age-Standardized Score

	Pooled OLS	Within-siblings OLS		
	(1)	(2)	(3)	(4)
Height-for-age	0.099			
	(0.029)***			
Siblings-difference height-for-age		0.090	0.083	0.081
		(0.039)**	(0.038)**	(0.044)*
Obs.	898	450	450	330
R^2	0.423	0.076	0.138	0.142
Siblings-difference	No	Yes	Yes	Yes
Child-level controls	Yes	Yes	Yes	Yes
Cluster fixed effects	Yes	No	Yes	Yes
Household level controls	Yes	No	No	No
Age-group	All	All	All	4-5y

Notes: robust standard errors, clustered at the household level in Column (1) and at the *index age-cluster* level in columns (2) to (4); *, **, *** denote significance at 10%, 5% and 1% levels.

Table 5. Dependent Variable: PPVT Age-Standardized Score

	Full sample			
	OLS	Instruments: Changes in food prices (a)	Instruments: Selected shocks (b)	Instruments: (a) + (b)
	(1)	(2)	(3)	(4)
Height-for-age	0.083	0.172	0.239	0.207
	(0.038)**	(0.079)**	(0.318)	(0.059)***
Weak identification test:				
Kleibergen-Paap Wald rk F stat	-	14.53	8.38	19.88
Stock-Yogo weak ID test critical values:				
5% maximal IV relative bias	-	21.38	13.91	21.41
10% maximal IV relative bias	-	11.45	9.08	11.41
Overidentification test:				
Hansen J statistic	-	22.348	0.280	23.622
p-value	-	0.2673	0.8694	0.3674
Obs.	450	450	450	450
R^2	0.138	-0.001	-0.017	-0.008
Nr. Excluded Instruments	-	20	3	23
Siblings-difference	Yes	Yes	Yes	Yes
Child-level controls	Yes	Yes	Yes	Yes
Cluster fixed effects	Yes	Yes	Yes	Yes
Household level controls	No	No	No	No
Age-group	All	All	All	All

Notes: robust standard errors, clustered at the *index age-region* level; *, **, *** denote significance at 10%, 5% and 1% levels.

Table 6. First Stage Results: Height-for-Age

	Model 1		Model 2		Model 3	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
Cereal price: 6-11 mths	0.983	0.739			0.936	0.731
Cereal price: 12-17 mths	0.111	0.785			0.128	0.747
Cereal price: 18-23 mths	-0.231	0.944			-0.182	0.905
Cereal price: 24-29 mths	0.257	0.879			0.170	0.868
Cereal price: 30-36 mths	2.666***	0.808			2.746***	0.796
Meat price: 6-11 mths	2.978**	1.381			2.805**	1.388
Meat price: 12-17 mths	2.751**	1.246			2.877**	1.210
Meat price: 18-23 mths	-1.620	1.135			-1.909	1.231
Meat price: 24-29 mths	1.259	1.839			0.897	1.849
Meat price: 30-36 mths	-4.138***	1.545			-4.100***	1.475
Dairy price: 6-11 mths	0.722	1.371			1.290	1.347
Dairy price: 12-17 mths	-1.638	1.393			-1.319	1.339
Dairy price: 18-23 mths	-1.030	1.573			-1.041	1.604
Dairy price: 24-29 mths	2.316	1.552			2.839*	1.474
Dairy price: 30-36 mths	-1.114	1.265			-1.288	1.285
Tubers price: 6-11 mths	0.791*	0.452			0.743*	0.447
Tubers price: 12-17 mths	-0.323	0.428			-0.319	0.441
Tubers price: 18-23 mths	0.032	0.446			0.038	0.442
Tubers price: 24-29 mths	0.008	0.495			0.019	0.516
Tubers price: 30-35 mths	-0.693	0.536			-0.686	0.544
Adverse shocks:2000-02			-0.181**	0.078	-0.181**	0.079
Frosts 2007-09			0.162	0.139	0.171	0.129
Illness (others) 2007-09			0.234***	0.091	0.247**	0.099
Constant	-0.118	0.319	-0.230	0.250	0.145	0.349
R-squared	0.148		0.100		0.165	
N	448		448		448	

Notes: within-household fixed effects estimates. Robust standard errors, clustered at the *index age-region* level; *, **, *** denote significance at 10%, 5% and 1% levels. Food prices groups as used by INEA: (1) Cereals: Bread and Cereals (comprising subcategories wheat, rice, maize, pasta as well as bread and biscuits); (2) Meat (comprising chicken, red meat and meat products, and processed meat); (3) Dairy: Milk, Eggs and Cheese. (4) Tubers: comprising potatoes, cassava, yucca, and andean tubers. Other controls included but not reported: age, sex, birth order, non-food expenditure (2006-2002), change in wealth index (2009-2006), community fixed-effects.

Table 7. Robustness Checks: Assets and Consumption

	IV GMM Core Regressions		Robustness Checks: + Δ Assets, 2006-2009		+ Δ Non-Food Consumption, 2006-2009		+ Δ Assets, 2002-2006	
	IV: Prices	IV: Prices & Shocks	IV: Prices	IV: Prices & Shocks	IV: Prices	IV: Prices & Shocks	IV: Prices	IV: Prices & Shocks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Height-for-age	0.1717**	0.2071***	0.1704**	0.2083***	0.1891**	0.2233***	0.1686**	0.2129***
	(0.079)	(0.059)	(0.079)	(0.057)	(0.077)	(0.056)	(0.083)	(0.059)
Δ Assets, 2006-09			0.4390	0.3866	0.4701*	0.4219	0.3048	0.3096
			(0.268)	(0.260)	(0.270)	(0.261)	(0.302)	(0.293)
Δ Non-Food Cons., 2006-09					-0.0339	-0.0242	-0.0322	-0.0225
					(0.021)	(0.016)	(0.020)	(0.016)
Δ Assets, 2002-06							-0.2225	-0.1550
							(0.199)	(0.180)
Weak identification test:								
Kleibergen-Paap F-Test	14.53	19.88	14.66	19.03	15.03	18.57	10.68	13.08
Overidentification test:								
Hansen J statistic	22.35	23.62	21.99	23.13	22.46	23.46	21.68	22.91
p-value	0.27	0.37	0.28	0.39	0.26	0.38	0.30	0.41
Obs.	450	450	450	450	450	450	450	450
R^2	-0.001	-0.008	0.002	-0.006	-0.001	-0.009	0.001	-0.007
Siblings-difference	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Changes in HH Assets	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Changes in HH Consumption	No	No	No	No	Yes	Yes	Yes	Yes
Age-group	All	All	All	All	All	All	All	All

Notes: robust standard errors, clustered at the *index age-region* level; *, **, *** denote significance at 10%, 5% and 1% levels.

Table 8. Robustness Checks: School Attendance

	Core	Robustness Checks:				
	(1)	(2)	(3)	(4)	(5)	(6)
		coef/se	coef/se	coef/se	coef/se	coef/se
Height-for-age	0,1717**	0.1983***	0.3269***	0.3239***	0.2927***	0.1916***
	(0.079)	(0.064)	(0.061)	(0.061)	(0.063)	(0.064)
Index child attended pre-school		0.1791***		0.2501***	0.3600***	0.3837***
		(0.067)		(0.092)	(0.090)	(0.085)
Index child started preschool at 3yrs.			0.2644***			
			(0.086)			
Index child started preschool at 4yrs.			0.2106***	-0.0483	-0.0534	-0.0320
			(0.075)	(0.052)	(0.051)	(0.042)
Index child started preschool at 5yrs.			0.1520**	-0.1034	-0.0971	-0.0839
			(0.076)	(0.088)	(0.092)	(0.074)
Index was school enroled when interviewed					0.5906*	0.4240
					(0.352)	(0.303)
Siblings was school enroled when interviewed					-0.4403***	-0.4108***
					(0.082)	(0.085)
Age of school enrolment 6yrs.						-0.2629**
						(0.128)
Age of school enrolment 7yrs.						-0.1312
						(0.136)
Age of school enrolment 8yrs.						-0.0067
						(0.117)
R-Squared	-0.001	0.0016	-0.0488	-0.0480	0.0003	0.0407
Nr Observations	450	450	439	439	439	432
Nr Clusters	43	43	43	43	43	43
Hansen J (Overid) test	22.348	23.7786	24.9622	25.0061	24.7080	21.8820
p-value	0.2673	0.3589	0.2989	0.2968	0.3112	0.4670
Kleibergen-Paap F-Test	14.5300	21.8973	28.6230	28.2979	24.3540	19.9923

Notes: robust standard errors, clustered at the *index age-region* level; *, **, *** denote significance at 10%, 5% and 1% levels.

Annex 2.

TableA1. Food Price and Food Expenditure

	F1	FS2	FS3
	(1)	(2)	(3)
Cereals prices	-.005	-.007	-.003
	(.003)*	(.003)*	(.004)
Meat prices	-.0009	.001	.0003
	(.006)	(.005)	(.006)
Dairy prices	-.001	-.010	-.005
	(.004)	(.006)	(.005)
Tubers prices	-.002	-.002	-.004
	(.002)	(.002)	(.002)**
Cereals prices*agriculture		-.002	
		(.006)	
Meat prices*agriculture		-.007	
		(.013)	
Dairy prices*agriculture		.016	
		(.014)	
Tubers prices*agriculture		.0005	
		(.003)	
Cereals prices*rural			-.010
			(.004)**
Meat prices*rural			-.017
			(.011)
Dairy prices*rural			.028
			(.008)***
Tubers prices*rural			.007
			(.002)***
Obs.	449	449	449
R^2	.111	.116	.128

Notes: Variables are the change between 2006-2002 for all categories. Rural is a dummy for living in rural area. Agriculture dummy for main occupation of household head. Significance levels as above.

Table A2. Joint Significance of Food Price Items: F Tests

	Total	By activity		By location	
		Non farmer	Farmer	Urban	Rural
	(1)	(2)	(3)	(4)	(5)
By item (all 5 semesters)					
Bread and Cereals	3.61	2.52	5.13	1.78	3.78
	(0.0084)	(0.0440)	(0.0009)	(0.1377)	(0.0065)
Meat	5.30	1.62	3.05	2.65	3.41
	(0.0007)	(0.1758)	(0.0194)	(0.0362)	(0.0112)
Dairy	1.37	0.99	2.85	1.58	1.10
	(0.2542)	(0.4369)	(0.0266)	(0.1867)	(0.3756)
Tubers	0.89	1.46	0.63	0.43	2.15
	(0.4955)	(0.2231)	(0.6776)	(0.8281)	(0.0786)
By semester (all 4 groups)					
6-11 mth	3.90	2.31	2.98	1.05	12.59
	(0.0088)	(0.0739)	(0.0296)	(0.3594)	(0.0000)
12-17 mth	1.42	0.92	0.85	2.76	0.60
	(0.2430)	(0.4603)	(0.5029)	(0.0400)	(0.6661)
18-23 mth	0.68	1.24	2.21	1.04	1.90
	(0.6097)	(0.3075)	(0.0841)	(0.3978)	(0.1291)
24-29 mth	1.49	0.09	2.75	0.89	1.14
	(0.2216)	(0.9845)	(0.0406)	(0.4805)	(0.3520)
30-35 mth	4.96	4.61	2.16	2.94	3.58
	(0.0023)	(0.0035)	(0.0900)	(0.0314)	(0.0134)
Total (all groups/semesters)					
	14.75	5.59	7.34	11.34	8.64
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)

Notes: Food prices groups as above. p-values reported in brackets. F-tests of joint significance conducted after estimation of model (3).