# Community Nurseries and the Nutritional Status of Poor Children. Evidence from Colombia*

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June 2012

**Abstract**

In this paper, we use two different datasets and three different instruments to estimate the impact of a long-established pre-school nursery programme (*Hogares Comunitarios*) on the nutritional status of beneficiary children. As placement in the programme is endogenous, we use variables related to cost (fee, distance to the nursery) and programme availability (capacity of the programme in the town) as instruments. One of our datasets is representative of very poor individuals living in rural areas of Colombia, while the other focuses in urban areas and include individuals relatively less poor. We find evidence that programme participation increases the height of participating children, with the size of the effect being remarkably consistent across the three instruments and the two datasets, which is informative about the external validity of our estimates. We also pay careful attention to scrutinize the internal validity of the effects that we find.

Keywords: *Hogares Comunitarios*, Instrumental Variables, Child Nutrition

JEL: C21, I12, I38

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* We are particularly grateful to the staff of *Instituto Colombiano de Bienestar Familiar (*ICBF), including Maria Francisca Concha, Zulma Fonseca, Ana Maria Peñuela and Yaneth Romero, for answering many questions about the details of the programme *Hogares Comunitario*. Special thanks to Jere Behrman, Raquel Bernal, Alejandro Gaviria, Luis Carlos Gómez, Aureo de Paula, Ximena Peña, Jim Smith, Petra Todd, and The Editor for providing us with useful comments. Excellent research assistance was provided by Soledad Giardili. Any errors are the only responsibility of the authors. The authors acknowledge financial support from the ESRC-DFID Grant RES-167-25-0124 and Advanced ERC Grant 249612.

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# 1. Introduction

Malnutrition amongst children is a very prevalent phenomenon in developing countries. According to Onis *et al.* (2000) approximately one third of children below the age of five are stunted in growth. Malnutrition and ill health in infanthood are not only welfare decreasing, but they are associated with poor health outcomes, cognitive and educational performance (Behrman 1996, Strauss and Thomas 1998, Glewwe *et al.* 2001, Alderman *et al.* 2001, Maluccio *et al.* 2006, Lindeboom et al 2006, Walker *et al.* 2007, Currie and Stabile 2010, Lindeboom et al 2010, van den Berg 2010) as well as low productivity later on in life (Strauss and Thomas 1998, Schultz 2005, Hoddinott *et al.* 2008). Therefore, to establish which interventions are the most effective in improving child nutrition and development in poor and middle income countries is an important research and policy question (Bhutta *et al.* 2008, Engel *et al.* 2007, Horton *et al.* 2008).

The objective of this paper is to estimate how children's nutritional status is affected by participating in *Hogares Comunitarios* (HC), a community nursery programme established by the Colombian government to provide childcare and food to pre-school children. The programme expanded rapidly since its introduction in 1986 and is currently one of the largest welfare programmes in Colombia: there are approximately 80,000 HC nurseries distributed across all towns in the country and about one million children, from the poorest Colombian families, attend a HC nursery. The cost of the programme, which is financed by a 3% tax on the wage bill, is approximately 250 million US$, or almost 0.2% of Colombian GDP.

Programmes similar to HC are also being implemented in Bolivia, Guatemala, India, México, Perú and other countries.[1] Their attractiveness arises from the fact that these programmes use community (human) resources and can be relatively inexpensive. Despite their importance, little is known about their effect on children's nutritional status or development. Recent reviews on strategies to improve child nutrition in developing countries are noticeably silent about their possible effects (Bhutta *et al.* 2008). In this respect, *HC* is no exception: little is known about its impacts on children nutritional status and development. Such lack of impact estimates is possibly associated with the fact that many of these programmes were established a long time ago, at which time an impact evaluation was not factored into their design, such as for instance by potentially exploiting the roll out of the programme,

[1] It is worth highlighting the size of the Indian programme which shares features with HC, the Integrated Child Development Service. The programme covers 70 million children through a network of over one million village-level *Anganwadi* Centres (Adhikari and Bredenkamp 2009).

as has been the case for recent conditional cash transfer and micronutrient supplementation programmes.

A credible evaluation of HC (or similar programmes) is challenging for all of the reasons for which targeted programmes are difficult to evaluate. The comparison between children attending a community nursery and children not attending one is problematic because participants and non-participants might be different in unobservable variables that, simultaneously, drive participation and the outcomes of interest. Conducting a randomized trial, and randomly providing HC to a subset of eligible children, would be challenging because the programme is by now so widespread. Given this situation, we estimate the effect of HC using an instrumental variable approach, using as instruments variables that proxy for the availability of the programme and, therefore, drive participation but do not affect outcomes directly. In particular, we consider several cost variables, including distance to the nearest HC, the fees charged and the capacity of HC in the town. Given our research strategy, we discuss extensively the identification assumptions we make and pay particular attention to issues of both the internal and external validity of the estimates we obtain.

We estimate the impact of HC using two different data sets. The first was collected to evaluate the impact of a conditional cash transfer (CCT) programme in Colombia (*Familias en Acción*, from now on FeA sample) and includes small rural towns. The second is the 2005 *Encuesta Nacional de Demografía y Salud* (ENDS sample), which is nationally representative, and hence includes larger towns. Individuals in the ENDS sample are, on average, less poor than those in the FeA sample. Estimating the effects on two datasets with different characteristics and the availability of three different instruments constitutes an important strength of our approach, as it allows us to address the external validity of our results.

We measure programme participation in two different ways: exposure (the fraction of a child's life that is spent in a HC nursery) and attendance (whether or not the child is attending a HC nursery at the time of the survey). We find that the derivative of child's height with respect to exposure is 88% of one standard deviation in the FeA sample and 123% for the ENDS sample. We find that attendance increases child's height by 40% of a standard deviation in the FeA sample (83% in the ENDS sample). Our estimates imply that the programme has sizeable effects: a 60 month old child that has spent 24 months in a HC nursery would be 35% (FeA) or 49% (ENDS) of one standard deviation of height taller. These estimates are well within experimental estimates of nutrition interventions. A recent meta-analysis concluded that provision of complementary food in food-insecure populations resulted in average increase of 41% of one standard deviation of age-adjusted height (Bhutta *et al.* 2008).

Because policy-makers might favour a given increase in height if it is obtained by improving the lower tail of the distribution than by improving the upper tail, we also estimate quantile treatment effects. We find that the impact of the programme is considerably higher for lower quantiles and almost zero for the top quantiles.

It is well-known that if the effects of a programme are heterogeneous, instrumental variable estimates identify the effect of the programme for those whose participation decision is sensitive to the instrument (Imbens and Angrist, 1994). We use three different instruments (distance from the household to the nearest nursery, fee charged to parents in the town and local availability of slots – filled plus vacant- in HC nurseries) but obtain similar impacts with each of them which favourably speaks of the external validity of our estimates. An attractive feature of our instruments is that they constitute policy variables that policy makers could manipulate to modify coverage of the programme. Hence, our estimates are informative about the effect of the programme on those individuals whose participation status might change as a result of a policy decision.

As it is generally the case, the credibility of our results coincides with the credibility of our identification assumptions. For this reason, we discuss at length the plausibility of our instruments and our identification assumptions as well as the interpretation of our results. Our main arguments are the following. First, we present a plausible model of individual behaviour (sketched in Section 3), that justifies the instruments we use and provide a clear interpretation to the parameters we estimate. The model gives us a conceptual framework that we can use both to spell out the assumptions that are necessary for the instrument to be valid and to interpret the estimates we obtain. Second, as one of our instruments is the distance from the home to the closest HC nursery, we control for distance to other amenities, which helps considerably to reduce the association between the instrument and other observable variables. Third, we find that towns where HC capacity is higher tend to be associated with variables that predict poor nutritional status (i.e. low parental education). This is consistent with the policy maker following a compensatory allocation rule, which usually leads to underestimate the effects of a programme (Rosenzweig and Wolpin 1986). Fourth, while we use different instruments (which show different patterns of associations with observable variables) on different data sets, we obtain similar results. Fifth, using the same instruments, we run so-called placebo regressions on variables that should not be affected by the programme (such as birth weight and mother's height) and show that indeed they are not. Fifth, we also carry out a sensitivity analysis which shows that our conclusions are robust to substantial violations of our identification assumptions.

The HC programme was established a long time ago. While this presents its evaluation with some difficult problems, it has also its advantages. In particular, provided our estimates are credible, we are evaluating a programme in its maturity, after policy-makers have had time to adjust it and modify it as necessary. As with any other programme, it is probably true that the programme is different now compared to when it started. The programme might have deteriorated due to decreased motivation, corruption or programme guidelines not being enforced. Alternatively, policy-makers may have solved initial bottlenecks and the programme may in fact work better now than at the beginning.[2] Either way, we are estimating the impact of HC after the programme has evolved for a long-time and probably reached its maturity. This is hardly possible to do using a randomized experiment because it would be unethical to randomly exclude communities from benefiting of the programme for a long time.[3] Considering this long-term assessment is particularly important in community based programmes because they draw on community resources (which makes them relatively inexpensive to implement), but are difficult to monitor and depend on the motivation of community members.

Our paper is related to at least two different strands of the literature. First, to the evaluation of nutritional policies in developing countries. Within this literature, our paper is closest to Behrman, Cheng and Todd (2004) who considers a matching approach to evaluate PIDI, a programme very similar to HC implemented in Bolivia. Ruel *et al.* (2006) and Cueto *et al.* (2009) study other two nursery programmes in Guatemala and Peru, respectively. We briefly discuss these studies in Section 6.3. Second, our findings are also very relevant in the context of the recent literature that highlights the importance of early child development (see for instance Currie 2001, Heckman and Masterov, 2005 and Grantham-McGregor *et al.* 2007, Almond and Currie 2011). It is argued that early childhood is the most cost effective period in a person's life in which to invest (Carneiro and Heckman, 2005; Heckman and Masterov 2005; Engle *et al.* 2007). Indeed evaluations of the Head Start programme in the US have shown that large-scale pre-school programmes can have impacts on later educational

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[2] See Banerjee *et al.* (2008) and Hanna *et al.* (2012) for examples of programmes that had a positive impact at the beginning but that fades away later on.

3 Experiments would be useful to study how the programme can be improved. See Attanasio *et al.* (2010) for experimental estimates of how improving the physical infrastructure of the nurseries affects children's nutritional status.

attainment (Currie and Thomas 1995 and 1999; Garces, Thomas, and Currie 2002, Ludwig and Miller 2007).

The rest of the paper is organized as follows. In section 2, we describe the operation of the programme. In section 3, we provide a theoretical framework that helps us in choosing an empirical strategy and in interpreting the results we obtain. We stress in particular that the parameter we estimate identifies the overall effect of the programme, including both direct and indirect effects. In section 4, we briefly describe the two data sets we use to estimate the impact of the programme. In Section 5, we present some evidence in support of our identification strategy. Section 6 presents the main empirical results of the paper, section 7 provides support to the credibility of our identification strategy. Section 8 concludes.

## 2. The *Hogares Comunitarios* programme

In the late 1970s, the Colombian government proposed a new nutrition intervention targeted towards poor families. The programme, called *Hogares Comunitarios de Bienestar Familiar,* was legislated in 1979 as the development of previous initiatives that focussed on community participation and initiatives to target nutrition and child development.

The programme started its operation between 1984 and 1986 and was run by the *Instituto Colombiano de Bienestar Familiar (ICBF).* At the beginning, the ICBF regional office targeted poor neighbourhoods and towns and encouraged eligible parents with children aged 0 to 6 to form 'parents associations'. After a few meetings with programme officials, the parents association was registered with the programme and elected a *madre comunitaria* (or community mother). This mother had to satisfy some criteria, such as having basic education and a large enough house and would be certified by the regional office of the ICBF. The *madre comunitaria* would cook and take care in her house of up to 15 children aged 0 to 6.

ICBF heavily subsidizes the nurseries: ICBF provides the food which is kept in the *madre comunitaria*'s fridge, a food supplement called *bienestarina,* the cost of utilities, the pedagogic material, as well as a monthly payment to the *madre comunitaria* (CO$ 300.000 or £68.6).[4] In addition, the parents

[4] According to the guidelines, the food and *bienestarina* received by the children would provide them with 70% of the daily amount of calories.

association sets a fee for each child attending the nursery. This fee is used to pay the *madre comunitaria* (in addition to the payment that she receives from ICBF). The same fee is set for all the HC nurseries of the same parent association (between 10 and 15 nurseries per parent association). It is then likely that the HC fee is lower in poorer towns because women's reservation wage would be lower.

Eligibility is proxy-means tested using the so-called SISBEN categories. In Colombia, households are assigned a SISBEN category (which ranges from 1 to 6, with 1 being the poorest) on the basis of the value of their SISBEN score, which is constructed using different indicators of economic well being. Most welfare programmes in Colombia are targeted using the SISBEN categories and SISBEN registries are periodically updated by the local authorities. Households can request to be given a SISBEN test to be assigned to a SISBEN category. Children are eligible to participate in HC if they belong to SISBEN 1 and 2 (although we find evidence in the data that SISBEN 3 children also participate in HC).

After the start of the programme and its rapid growth, the turnover among the *madre comunitarias* was substantial. According to officials of the ICBF, between 10% and 15% of the existing *HC* are relocated in each year, in that a *madre comunitaria* ceases to be such and a new *madre* starts to operate it. Moreover, if a household moves to a certain neighbourhood, it can normally register its children in an existing *HC,* if there are spaces available. It seems that over time, the *HC* have evolved into relatively mobile and informal nurseries and have lost some of the tight connection with the original parents association. Nowadays, one parent association is responsible for between 10 and 15 HC nurseries. However, *madres comunitarias* have to be certified by the ICBF, they have a constant contact with it and they have to provide the ICBF, at least in theory, with records of children development and growth.

In rural and sparsely populated areas, an apparently common problem is the difficulty to set up a new *HC* because the ICBF does not start a new nursery unless there are a sufficient number of children who want to attend. This issue seems to be present in many communities. On the other hand, in urban areas, the constraint seems to be the number of places available: in many situations *HC* have waiting lists.

## 3. Estimating the impact of *Hogares Comunitarios* on eligible children.

The main aim of this paper is to estimate the impact of attending a HC nursery on the nutritional status of eligible children. This exercise is a non trivial one for several reasons. First and foremost, as is common in the evaluation of large programmes that have been operating for a long time, it is difficult to identify a credible counterfactual that would allow us to measure the average nutritional status of eligible children in the absence of the programme. The programme is widely available and many of the eligible children that do not attend do so by choice. Second, the programme changes not only the nutritional input of the children who attend a HC, but also a variety of other variables that are likely to affect their physical (and cognitive) development. In addition to food, the programme provides child care, therefore making it easier for the mother to work (and therefore provide additional resources to the household). The programme is not, by and large, free, so that monetary resources are used for participating into it. Parents are likely to change the allocation of resources and, in particular, food as a consequence of sending a child to a HC. An additional difficulty is that some of the determinants of children nutritional status (that are likely to be affected by participation to HC) are not observed in the data that we use. Third, the programme's impacts are likely to be different for different children and the decision to attend a HC is likely to be driven by the perceived potential benefit to the child. For instance, a child who lives in a very poor household might experience an improvement in her environment when attending a HC, while a child from a not too poor household might be experiencing a worsening of her environment if she attends a HC. This heterogeneity in potential benefits, therefore, might affect our results and the interpretation of the estimates our identification strategy yields. In addition to these conceptual and theoretical issues, there are also a number of practical issues concerning the specification of our empirical exercise. We will be discussing these issues in the second part of this section.

### *3.1. A conceptual framework.*

To explain the empirical strategy we use to tackle these issues and, at the same time, provide an interpretation of the results we will be presenting, it is worthwhile to sketch a simple model of individual behaviour, along the lines considered in Rosenzweig and Schultz (1983). To discuss the issues mentioned above, it is useful to consider a household that maximizes a utility function that depends on consumption and children nutritional status:

$$MaxU(X,H,L) \tag{1}$$

subject to the following restrictions:

$$H = H(A, F, L, z, \varepsilon) \quad (2)$$

$$X = Y - pF - qA + w(L - DA) \quad (3)$$

where *H* is the child's nutritional status, *F* is food fed to the child, *X* is other consumption, *L* is female labour supply, *A* is suitable measure of participation in HC (i.e. whether or not the child attends a HC nursery, the number of days that the child attends a HC nursery, etc), *p* the price of food, *q* is the HC fee per unit of A, D is the distance from the household to an HC and *Y* is other income. The household chooses *A, F,* and *L*. For expositional simplicity we are assuming here that all choice variables are continuous. Equation (3) is the budget constraint that reflects the importance of cost variables (the distance to a HC and fee). Equation (2) is the production function of human capital which is affected by the different inputs *F* and *A,* and by a vector of observable variables *z*, which are assumed to affect the outcome of interest (i.e. maternal height and education). The question about the impact of the programme can be framed in terms of the identification of the production function (2) and, in particular, the partial derivative of the function *H*() with respect to *A,* attendance to a HC, which is seen here as one possible input. The unobservable (to the econometrician) random variable $\varepsilon$ reflects other factors that affect the outcome of interest. The three issues we considered above can be summarized in terms of the features of the model considered. Suppose that the production function in equation (2) can be approximated by a linear function:

$$H_i = \vartheta + \alpha_i A_i + \beta F_i + \gamma L_i + \theta z_i + \varepsilon_i \quad (4)$$

where the subscript *i* indicates the child. The first problem discussed above arises from the fact that the household chooses the variables *A, F,* and *L*. These choices will depend on the exogenous variables of the model (*p, q, z, D, Y, w* and $\varepsilon$). As a consequence, an OLS regression of *H* on the inputs will not yield consistent estimates of the parameters of interest, even when the coefficient on *A* is constant, as households will react to information on $\varepsilon$. The second issue stems from the fact that in many data sets, we have no information on *F*. Finally, impact heterogeneity is reflected in the fact that the parameter $\alpha_i$ varies with *i* and might affect the choice of the inputs.

We have written the model so that, at least for the case in which the coefficient on *A* is a constant, a relatively simple solution is offered by an Instrumental variable approach. Variables that reflect the cost of the various inputs, such as *q, w, D* and *p,* can plausibly be used as 'instruments' for the quantities *F, L* and *A*. To see this, one can solve for the optimal *F, L* and *A* as a function of the exogenous variables and use such equations as a 'first stage', followed by a 'second stage' estimation of equation (4). The plausibility of this identification strategy will then depend on the plausibility of the assumption that the variables used as instruments (*D, q, p, w* and *Y*) are exogenous and can be excluded from equation (4).

The fact that some of the inputs, such as *F*, are not observable implies that the coefficient $\alpha$ cannot be estimated. To see this, abstract from *L* and think of regressing *H* on *A,* instrumenting the latter with 'cost variables' (such as *D* or *q*). The omission of *F* from such a regression, however, induces a correlation between the instruments used and the residual terms that includes *F*. The latter is an alternative input that will react to *A*. Therefore, such a strategy will not yield a consistent estimate of the coefficient $\alpha$, the marginal productivity of *A* in the production function for *H*. Indeed, such a coefficient is not identified without strong and tight parametric assumptions about the separability of the utility function and of the production function. Notice that this lack of identification does not depend on the nature of the instrument used and would hold even with a perfect instrument, such as the random allocation of *A* across children with perfect compliance. The problem stems from the unobservability of *F*.

What is identified in this context, and what we will be reporting in our empirical results, is the overall impact of *A*, including the indirect effect that works through changes induced in other inputs, such as *F* and *L*. To be more precise, write the demand function for *F,* and *L* conditional on the optimal level of *A* as:

$$F_i = f(A_i, p, w, q, D, Y) \tag{5}$$

$$L_i = l(A_i, p, w, q, D, Y) \tag{6}$$

and let define $f_A \equiv \frac{\partial f}{\partial A}$ and $l_A \equiv \frac{\partial l}{\partial A}$. The overall effect of *A*, neglecting for the time being its possible heterogeneity across individual children, is given by $\alpha + \beta f_A + \gamma l_A$ which is composed of a direct effect (measured by the marginal product of *A* in *H*(.) ) and the indirect effect that works through changes in *F* and *L*. In order to estimate the overall effect of *A*, we will use instrumental variables to estimate the coefficient of $A_i$ in the following regression:

$$H_i = \tilde{\vartheta} + \tilde{\alpha} A_i + \tilde{\theta} z_i + \tilde{\varepsilon}_{i,} \tag{7}$$

where we have neglected again the possible heterogeneity of $\tilde{\alpha}$.

Having clarified the meaning of the parameters we will be estimating, we need to deal with the last issue, which is the possible heterogeneity of the impacts that the HC programme might have. The problem, which is particularly serious when selection into the programme depends on the impact heterogeneity, is obviously not new, and has been described extensively in the literature. In terms of our exercise, it affects the interpretation of the results we obtain from our IV specification. In particular, we will be estimating the Local Average Treatment Effect (LATE) which considers the effect for individuals whose treatment participation is sensitive to the instrument used (see Imbens and Angrist 1994, Angrist and Imbens 1995). Since we will be using three different instruments, we will estimate three different LATEs, which are going to be estimates of the treatment effect for groups of individuals who are likely to be different as their participation can be affected in a different way depending on the instrument used. As such, our results are informative about treatment heterogeneity and the external validity of our results.

### *3.2. The empirical specifications: treatment indicators and instruments*

In sketching our conceptual framework, we have treated the use of the HC programme as a continuous variable. In our empirical specification, however, we consider two alternative definitions of 'treatment': *attendance* and *exposure*. *Attendance* is defined according to whether or not the child is currently attending a *HC* nursery. *Exposure* is defined as the number of months in which the child has attended a *HC* during his or her life divided by the child's age in months, therefore defining treatment as the fraction of his or her life spent in a HC nursery. This indicator considers the intensity of treatment as in Angrist and Imbens (1995).

As instruments, we consider three variables: the ratio of the number of HC slots (filled plus vacant) in the town to the total number of children aged 2-6[5] from SISBEN 1 and 2 families in the town (an indicator of programme availability, which we will be referring to as 'capacity'), distance from the household to the nearest HC nursery and median fee paid by children to attend a HC nursery in the town (as indicators of cost of participation both in terms of time and money).[6] We obtain the number of slots in each town directly from the ICBF administrative data and consequently this instrument can

[5] We chose age 2 to 6 because only less than 20% of children enrol in HC below age 2. However, our results are not sensitive to choice of the age range in the construction of the HC capacity variable.

[6] Tuition fees and distance to college has been used as an instrument for schooling by Card (1993), Kane and Rouse (1993), Kling (2001), and Cameron and Taber (2004), Currie and Moretti (2002), Carneiro, Heckman and Vytacil (2006).

be used with both datasets.[7] ICBF does not collect information on the fee paid by children in each town and hence we compute it using a household survey. Both fees and distance to the nearest nursery are only available in the FeA survey. However, conversations with programme official indicated that distance is not an important constraint in large urban towns that make the most of the ENDS sample. Descriptive statistics for the three instruments are shown in Section 4.

## 4. The data

The main data we use in this paper come from two household surveys. The first covers small towns and is part of the survey originally collected to evaluate a conditional cash transfer programme called *Familias en Acción*. The second data source, which we use to evaluate the impact of the programme in urban areas nationwide, is the *Encuesta Nacional de Demografia y Salud,* the Colombian version of a Demographic and Health Survey.

### *4.1. The Familias en Acción Survey*

Between 2001 and 2002, the Colombian government started a conditional cash transfer programme, modelled after the PROGRESA programme in Mexico. This programme, called *Familias en Acción,* has an education, and a health component and is directed to the poorest families (in the SISBEN 1 category) living in the towns targeted by the programme. The targeted communities were relatively small towns (less than 100,000 inhabitants and no departmental capitals) with a bank and "enough" education and health infrastructure.[8] The households included in the survey had to satisfy the eligibility rules of FeA, that is, they had to be registered as SISBEN 1 as of December 1999 and have children aged 0 to 17. This implies that our sample is representative of the poorest households in small towns.[9]

As we are interested in evaluating the impact of the HC programme and we want to avoid contaminations by FeA, in what follows we focus on the towns where FeA was not implemented

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[7] Capacity is only available for 2005 but we use it for the three years of the study because there is little year to year variation (according to our conversations with programme officials).

[8] An additional condition (that turned out to be binding in some situations) was that the mayoral office had to process some documents and have a list of potential beneficiaries ready.

[9] See Attanasio *et al.* 2003 for more information on the survey. The data is publicly available from http://www.dnp.gov.co/PortalWeb/Programas/Sinergia/HerramientasyProductosdelSistema/Basesdedatos/tabid/226/Default.aspx

(towns to serve as controls in the evaluation of FeA).[10] They were chosen as the most similar to the treatment towns according to population size, population living in the urban part of the town, and the value of the official synthetic index for Quality of Life. In the first (summer 2002) and second wave (between July and November 2003), there are 65 towns where FeA was not implemented. Between the second and third wave (between December 2005 and March 2006) of data, the FeA programme started in 13 towns that were part of the control group in the first and second wave. So, only 52 towns are used in the third wave of data. As a consequence, and because of the natural ageing of households, the third wave includes considerably fewer children than the first two.

In addition to a very large number of questions covering consumption, income, school attendance, labour supply and a variety of other variables, the questionnaire also included a number of questions about current and past attendance of each child to a HC. In particular, for each child, we know whether he or she is currently attending a HC, and, for each year of the child's life, how many months he or she had attended a HC. Finally, and importantly for our identification strategy, if a child is attending a HC nursery, we know the distance from the household to the HC nursery. If the child is not attending a HC nursery, we know the distance to the nearest HC. For each child that has ever attended a HC nursery, we also ask for the fee that they currently pay or that they used to pay when they attended. Children aged 0 to 6 were weighed and measured.

The fee paid for attending a HC nursery, and town level wages as reported by the town major were collected in the second and third wave of data but not in the first one. For the first wave, we use the values collected in the second wave. We do not think that this is a major problem as the first and second wave were collected only 12 months apart. The distance to other amenities (schools, the health centres and town hall) was collected for the whole sample in the second and third wave only. For the first wave of data, we use the distance to other amenities collected in the second wave of data.

### *4.2. Encuesta Nacional de Demografía y Salud*

The *FeA* survey gives us an important opportunity to estimate some of the impact of the programme in small towns and rural areas. To explore the external validity of our estimates, we also use the *Encuesta Nacional de Demografia y Salud* (ENDS from now on) and focus in its urban sample. The ENDS includes information on basic household demographics, children anthropometrics, and,

10 Children living in towns where FeA was implemented, had to choose between FeA and HC, they could not be enrolled in both programmes simultaneously. This changed after 2005.

importantly for us, participation in HC. The survey is less detailed than the FeA survey in some aspects, and does not include information on the fee paid to attend a HC nursery, nor the distance from the household the HC nursery.[11] Some other variables, such as distance to other facilities (school, health centre, town hall) and some town level variables are also missing from the ENDS (see Table 4.1 for details).

### 4.3. *Descriptive Statistics.*

The sample of FeA and ENDS differ in two main dimensions: type of town and SISBEN level. The towns in the FeA sample are reasonably small: the average population in 2001 was 25k and even the town at the 75th percentile had less than 30k inhabitants. Only one town is larger than 100k, and none of them are capital of departments. These towns are eminently rural although 52% of the population live in the main part of the town and 48% are spread-out in the countryside. For the towns included in the ENDS sample, the average population in 2005 was 127k. The ENDS includes large metropolitan areas as well as selected capitals of departments.

The population in the FeA sample is extremely poor, as they all belong to the lowest level of SISBEN. The ENDS sample includes all levels of SISBEN but we constrain our sample to 3 or below because of the rules governing eligibility to HC.[12] Hence, the population in the ENDS sample is less poor than the FeA sample. Average family size is 7 (5.5) in the FeA (ENDS) sample. In the FeA sample, most mothers (58%) have not finished primary education, while most mothers (61.4%) finished secondary education in the ENDS sample. The average female daily wage is £2.2 (£2.0) in the main (dispersed) part of the town.[13] The value of a longer list of variables is compared in Table 4.1.

{Table 4.1}

As regards nutritional status indicators, we follow the literature in not using height directly, but we construct the so-called z-scores for these variables standardizing them by age and sex according to the World Health Organization/Center for Disease and Control (WHO/CDC) reference population. In particular, the z-score for height per age is obtained from the height of a child, subtracting the median

[11] The ENDS only asks distance to those that attend the HC nursery, but the question was skipped for non-users.

[12] Though in principle eligibility is constrained to levels 1 and 2, we find that 31% of children with level 3 participate in the HC programme. Because of missing values in the responses to the SISBEN question, we compute the SISBEN level using information in the survey and the SISBEN formula.

[13] During the data collection period, the average exchange rate is £1=CO$ 4371.

height of and dividing by the standard deviation of height of the WHO/CDC reference population of the same age and gender. A child is defined as 'chronically malnourished' if is her or his z-score for height per age is less than -2.

The children in our sample have a deficit in height. The average height per age z-score (which should be zero in a healthy sample) is -1.27 in the FeA sample and -0.77 in the ENDS sample. Moreover, 23.9% and 11.2% of children are chronically malnourished in the FeA and ENDS sample respectively. However, they do not have a deficit in 'weight per height' nor problem of obesity.[14] Height is thought to reflect more accurately than other variables the 'stock of nutrition' and therefore is considered a good indicator of long run nutritional status. For these reasons, we focus the analysis in what follows on the impact of the programme on child height.

In Table 4.2 we report the percentage of children who attend a HC by age. Two features are worth stressing. First, attendance rates have an inverted U shape, being highest between 2 and 4 years for the FeA and ENDS sample respectively. They are particularly low for very young children. Second, either the programme does not seem to be extremely popular or the availability is limited, as attendance rates do not achieve 50%, even for children 3 or 4 years old.

{Table 4.2}

Our surveys ask, for each child that does not attend an HC, the main reason for not attending. In Table 4.3, we report the percentages reporting a specific reason, for different age groups. The most common reason for not attending is the availability of child care at home. As to be expected, this is particularly relevant for the youngest children. For the oldest children, the importance of the 'other' reasons is explained by the fact that a significant proportion of these children are in school. Interestingly for our analysis, the distance from the nearest HC is an important reason for not attending a HC in rural areas but much less nationwide. Similarly, the fee that has to be paid to attend a HC nursery seems to be an important reason in the rural sample, but not in the ENDS sample

{Table 4.3}

[14] The percentage of children acutely malnourished –their weight is too low for their height- is only 1.2% and 1.5% in the FeA and ENDS sample, respectively. The corresponding percentages of obese children are 1.8% and 0.6%.

In Table 4.4, we report the mean and three percentiles (25th, 50th and 75th) for our three instruments. In the left-hand panel we consider the statistics computed over the whole sample, while on the right hand panel, we restrict our attention to the sample of participants to HC. As expected, participants tend to live close to HC nurseries and live in towns with lower fees and/or more capacity. The median distance to the nursery is 10 mins. but with substantial variation (25% of the household live at 27 minutes or more from a nursery). In the Fea (ENDS) survey, the median of HC slots (filled plus vacant) to eligible children is 28% (23%). The median monthly fee to attend a HC nursery is CO$ 3500 (£0.8). We return to the importance of these variables as determinants of participation below.

{Table 4.4}

# 5. The identification strategy

Whether a child participates or not in HC is a choice that parents make, and, consequently, we consider it endogenous. To tackle this problem we use an instrumental variable approach. In section 3, we discussed a model that justifies the use of cost variables (and indirectly availability) as instruments and gave an interpretation to the estimates one gets following an IV approach. The crucial assumption, of course, is that the instrumental variables do not enter directly as determinants of *H(.)* in equation (7). In addition, the instruments must be drivers of participation. The latter condition is easy to test using the first stage equations; the former condition remains an important assumption, which we also explore in section 7.

## *5.1. First Stage Regressions.*

As discussed in section 3.2, we use two different variables to measure participation in HC (*attendance* and *exposure)* and three different instruments: distance from the residence to the nearest HC, the median fee in the town, and the capacity of the HC programme in the town (filled plus vacant HC slots divided by the number of eligible children in the town). The results of the first stage regressions are in Tables 5.1-5.3. Note that, in the case of distance, we use both the distance as measured in the most current survey and as measured in the first wave, as it might be possible to have some inertia in the participation decision.[15] Each regression includes a set of covariates, including the number of

[15] To be precise, for participants we use the distance to the HC nursery that they attend, while for non-participants we use the distance to the closest nursery. Ideally we would have liked, for non-participants, the distance from the nursery they would be

children 2-6 in the town, the distance to other amenities (school, health centre and town hall), mother's and head's ages and education levels and mother's height, as well as a variety of town level variables, which all are also controlled for in the second step regression (see Table A1 of the appendix for a complete list of control variables).

As Table 5.1 and 5.2 show, the three instruments are highly correlated with *exposure* and with the expected signs. The F-statistics of the instruments are quite large (all are above 10 except for HC fee which is around 6 when entered jointly with other instruments –Table 5.1- but increases up to 12.30 when it is the only instrument – see Table 5.2). For attendance, the F-statistics are also generally above 10, except for HC fee which exhibits F-stats between 2.90 and 6.9 and consequently might suffer from a weak instrument problem in some specifications (Staiger and Stock, 1997). Overall, the evidence is consistent with the fact that mothers report that "being too far away" and "cannot afford the fee" are important reasons why their children do not attend a HC nursery (see Table 4.3).

In the ENDS sample, we can only use 'capacity' as instrument. When we include quadratic terms as instruments, the F-statistic of capacity in the *exposure* regression is 20.49, and 9.95 in the *attendance* regression. However, the value of the F-statistic is considerably lower if we only use a linear term (6.88 for exposure and 3.43 for attendance). Consequently, we must be careful to interpret results which do not include a quadratic term in the ENDS sample.

{Table 5.1, 5.2, 5.3}

*5.2 Relation between the instrument and other covariates*

In this subsection, we examine the association between our three instruments and the covariates of the model. Clearly, this does not inform directly on the validity of the instruments because it is the correlation between the instrument and unobserved variables what is important for consistency of the IV estimator. However, analyzing the association between the instruments and observed variables can

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using if they were to attend. Unfortunately we do not have this information. Neither we have, for participants, the distance from the closest nursery or whether such nursery is the one actually used. In Appendix C we analyse the bias that this issue could introduce in our impact estimates and conclude that this discrepancy could lead us to underestimate the effect of the programme under the type of selection into the programme that we infer from the data (negative selection). For ease of exposition, we will refer to this distance as the distance between the household and the closest HC nursery, reflecting our belief that this discrepancy is not likely to be important, especially in the sparsely populated areas of the rural towns.

be useful to help us think through the mechanisms that determine the instruments, and hence helping us to assess the direction of the bias if any (Altonji, Todd and Taber 2005).

Figure 1 compares the density of predicted height (as a function of all covariates) according to whether the child lives close or far from a HC nursery (defined as below or above of the median distance). The plot shows that those who live close tend to have covariate values, which predict them to be taller than those who live far. This is probably because HC nurseries tend to be located close to health centers and schools, and richer households might locate closer to these amenities. As HC nurseries also tend to be located closer to these amenities, we would expect more educated households to live closer to HC nurseries. Interestingly, Figure 2, which nets out the relation between height and distances to amenities other than HC nurseries, shows a much smaller difference between those who live close and those who live far from a HC nursery.[16] Consistent with this, the P-value of the Kolmogorov-Smirnov test of equality-of-distributions is 0.000 in Figure 1 but only 0.13 in Figure 2.

{Figures 1-2}

Analogous to Figure 1, Figure 3 and 4 shows the density of predicted height according to whether the child lives in a town where the HC fee is low or high (Figure 3) or whether capacity of HC in the town is also low or high in the FeA sample (Figure 4). They both show negative selection: children living in towns where the HC fee is low or HC capacity is high have covariates values which are associated with lower height. This is consistent with policy makers following a compensatory rule to allocate HC capacity across the type of towns (rural and below 100,000 inhabitants) that the FeA sample represents.[17] That HC fee is also negatively selected is consistent with the way that the HC fee is set.

{Figure 3-4-5}

To sum up, Figures 1 to 5 show the relation between the instruments and the covariates. Distance is, according to this evidence, the least sound of our instruments. Children living closer to a HC nursery tend to have covariate values which predict them to be taller, which could lead to overestimate the

[16] To produce Figure 2, we first run a regression of child's height on distance from the household to other amenities (nearest health centre, school and the town hall), as well as a dummy variable for whether the household lives in the main part of the town. Second, we obtain the residuals from that regression, and we regress them over the other covariates. Figure 2 is the plot the density of the fitted values from this last regression.

[17] In the ENDS dataset, the predicted height densities are very similar (Figure 5) though we must acknowledge that we have far fewer covariates in the ENDS dataset than in the FeA dataset.

effect of HC. We notice however, that this association is attenuated when we condition on distance to other amenities. On the contrary, our findings on HC capacity in the FeA sample are consistent with policy makers following a compensatory behaviour when allocating HC capacity across towns, which typically derives an underestimate the effect of policy (Rosenzweigh and Wolpin, 1986). As with capacity, the median HC fee paid in the town is also negatively selected.

*5.3 Do the instruments covary among themselves?*

Having results obtained with different instrument sets would not be particularly valuable if these instruments are highly correlated among themselves. Figures 6, 7, and 8 show the graphs of one instrument against another. There is a strong clear negative relationship between the median fee paid in the town computed using the FeA survey and the capacity variable computed using ICBF data (see Figure 6). On the contrary, Figure 7 and Figure 8 do not show a clear association between either fee and distance or distance and capacity. This evidence would, therefore, add credibility to similar results obtained with different instrument sets.

{Figure 6, 7, and 8}

## 6. Effects of the programme

In this section, we report and discuss our estimates of the effect of the HC programme on child's height. First, we present average impacts and then we move to present results on selected percentiles of the distribution.

*6.1 Average Treatment Effects*

In columns 1 to 5 of Table 6.1, we present our IV estimates of equation (7), using as instruments the local capacity of HC (for both FeA and ENDS sample), the median HC fee in the town and the distance from the household to the nearest HC nursery (FeA sample). The dependent variable of equation (7) is the *z*-score for height per age. While in the Table we report only the estimates of the programme's effects, in the regression, we also include a large set of covariates at the individual, household, and town level (see the variables reported in Table A1 in the Appendix). Among our covariates, we include the distance from the household to the nearest school, nearest health centre, and the town hall (see subsection 5.2 on why they could be important). We also include the number of children aged 2-6 in the town (the denominator of the capacity instrument) to ensure that we only exploit the variability related to the total availability of HC slots (filled plus vacant) in the town and not population size. In general, the reason for our un-parsimonious specification for this equation is our worry that our instruments could capture some unobserved feature of the environment where the households live and have a direct effect on the outcome of interest. All standard errors are clustered at the town level. We discuss further the robustness of our results in section 7.

The top panel of Table 6.1 (columns 1 to 5) shows IV estimates that uses as instrument the non-linear prediction of the participation variable.[18] These results show that the effect on children's height of HC participation is positive and, in most specifications, statistically different from zero at usual levels.

{Table 6.1}

According to the estimates in column 1, obtained from the FeA sample, a child that spends his/her entire life in a HC (so exposure equals 1) will be 88.5% of one standard deviation of height taller; and a child that currently attends a HC will be, on average, 40.5% of one standard deviation of height taller.[19] These results are significant not only from a statistical point of view: they show that the programme might have a remarkable effect on its beneficiaries.

While in column 1 we use the three instruments simultaneously, columns 2 to 4 report estimates obtained with each set of instruments at a time, still within the FeA sample. These estimates are similar to those in column 1. If the returns to programme participation were heterogeneous, the estimates in Table 6.1 should be equivalent to the so-called Local Average Treatment Effect (LATE), which is sometimes criticised in terms of external validity. In this particular case, we obtain similar results with three different instruments (although we have shown in Figures 7 and 8 that the relation between distance and fee or capacity is rather weak). This speaks favourably about the external validity of our estimates. Of course, our results could arise because the returns to programme participation are not heterogeneous, or because individuals do not select into the programme

18 The prediction is computed after estimating a non-linear model (Probit for attendance and Tobit for exposure) over the complete set of covariates and the variables excluded from equation (1): distance, fee, and capacity according to the subheading of the corresponding column of Table 6.1. This non-linear IV estimation procedure has a number of desirable properties: the estimator is asymptotically efficient under homoskedasticity, it is consistent even if the functional form of the prediction is misspecified, and the standard errors do not need to be corrected (see Wooldridge 2001, pg. 623; Angrist and Pischke 2009, p. 191). Notice that this is not the "prohibited regression" as the prediction is only included in the matrix of instruments, but not in the matrix of regressors.

19 The average number of months attending a HC nursery is 19.3 among those children currently attending. The average exposure among those currently attending is 0.41.

according to their unobserved returns (Imbens and Angrist 1994, Heckman 1997, Heckman *et al.* 2006).[20]

Column 5 of Table 6.1 (top panel) reports the estimates of programme impact obtained in the ENDS urban sample. These results are interesting per se, as they refer to a sample that is substantially different from the FeA one, which is predominantly rural, and are also informative about the external validity of the estimates in columns 1-4. The point estimate of the coefficient on exposure is roughly 35% higher in the ENDS than in the FeA sample, and almost twice as large in the case of attendance (although note that, given the size of the standard errors, the confidence intervals overlap).[21] Interestingly, we obtain very sizeable effects of the programme in both datasets.

The middle and bottom panel of Table 6.1 shows IV estimates using standard Two Stage Least Squares (2SLS). The middle panel uses both linear and quadratic terms as instruments, while the bottom panel simply uses only the linear terms as instruments. For the FeA sample, the results are reasonably similar to the ones in the top panel, but the standard errors for attendance are much larger than those in the top panel (not surprisingly, given the efficiency properties of 2SLS with nonlinear fitted values).[22] Results are only statistically significant at usual levels when we combine the three instruments in the FeA sample. The ENDS estimates are much smaller when we only include the linear term as instrument. This is probably because the F-statistics in the ENDS sample were particularly low when a quadratic term was not included (Table 5.3).

Under the assumption of homogenous treatment effects, the Hansen J-statistic can be used to test the overidentifying restrictions in the FeA sample. The test fails to reject the null hypothesis that the instruments are invalid.[23] This is hardly surprising because the estimates obtained with each instrument separately are similar. In section 7, we assess the robustness of our results to violations of the exclusion restriction assumptions.

20 The sample size in the third wave is considerably smaller because of two main factors: (1) we do not use 13 towns in the third wave because FeA started to be implemented in those towns – see section 4, (2) households have aged since the first wave and they have fewer children between 0 and 6 years old.

21 The ENDS sample is younger (by one year) than the FeA sample. This could potentially explain part of the discrepancy in the attendance results because younger children tend to be more sensitive to nutrition interventions. The FeA sample is older because of natural ageing of the sample (the third wave was collected after three years of the first wave).

22 There is little difference in the standard errors of exposure. This is probably because the prediction generated by the Tobit model is not very different from a linear prediction.

23 When we only use linear terms as instruments, the Hansen J-statistic is 1.44 (P=0.70) for exposure and 2.53 (P=0.47) for attendance. If we also use quadratic terms as instruments, the Hansen J-statistic is 6.23 (p=0.51) for exposure and 8.94 (P=0.26) for attendance.

For comparison purposes, we also report OLS estimates of the parameters of interest in columns 6 and 7 of Table 6.1. They show a negative correlation (and statistically different from zero in the case of attendance) between programme participation and child's height. The negative bias of the OLS estimates relative to the IV ones is consistent with self-selection into the programme by those individuals with poor nutritional status. An internal ICBF study by Siabato *et al.* (1997) found that children attending *HC* were shorter than children of 'similar socio-economic background'. Indeed, the programme guidelines explicitly say that children must suffer from "economic vulnerability" in order to be eligible.[24] We also confirm that the sample of attendees are "negatively selected' according to observable variables $z_i$ by regression height on the covariates $z_i$ and plotting the fitted values (see Figures A1 and A2 in the Appendix)

*6.2 Treatment effects on conditional quantiles of the height distribution*.

In this section, we provide estimates of how the marginal distribution of height (conditional on covariates) changes with participation in the HC programme. In order to consider the endogeneity of programme participation within a quantile regression framework, we follow Lee (2007) and estimate quantile regressions that are augmented by the residuals of the first stage regression (control function). For both samples, we use a second degree polynomial in the estimated residuals. Higher order polynomials were not significant. The standard errors are estimated by block bootstrap, with block defined as towns. Table 6.2 shows the estimates for selected quantiles.

**{Table 6.2}**

The point estimate of the effect of the programme at the 25th percentile is about three times as large as the estimate of the impact at the 75th percentile. This monotonic pattern indicates that, in the absence of the programme, the left tail of the height distribution would be considerably longer, and consequently, the number of chronically malnourished children would also be larger. We note that the estimates obtained in the ENDS are quite a bit larger than those obtained in the FeA sample, but so are their standard errors (especially for exposure).

[24] http://www.icbf.gov.co/Tramites/primera_infancia.html#I

*6.3 Discussion*

Our OLS regressions show that participants are slightly shorter than non-participants, but our IV results show sizeable effects of the programme. Clearly, the programme is allowing the poorest children (that self-select into the programme) to almost catch-up with their better off peers, but participants are still short according to international standards, and there might be room to improve the programme.

According to our estimates, the programme show sizeable effects: a 60 month old child that has spent 24 months in a HC nursery would be 1.5% taller in the FeA sample (2% in the ENDS sample).[25] Thomas and Strauss (1997) estimate that 1% increase in height leads to 2.4% increase in adult male wages in Brazil.[26] Our estimates are also plausible from the biological point of view. In terms of z-scores, these gains in height correspond to 0.35 z-scores in the FeA sample (0.49 in the ENDS sample). These estimates are well within experimental estimates of nutrition interventions. A recent meta-analysis concluded that provision of complementary food in food-insecure populations resulted in average increase of 0.41 height-for-age z-scores (Bhutta *et al.* 2008).

An interesting question is how our estimates compare to results obtained for similar programmes. As we mentioned above, the evidence on this type of programmes is very limited. However, some estimates do exist, such as those for the *Proyecto Integral de Desarrollo Infantil* (PIDI) in Bolivia, which is studied in Behrman, Cheng and Todd (2004), and the *Hogares Comunitarios* programme in Guatemala studied by Ruel *et al.* (2006) and the *Wawa Wasi* programme in Perú, studied by Cueto *et al.* (2009).

Similarly to HC, the PIDI provides day-care, nutritional, and educational services to children between the ages of 6 and 72 months who live in poor, predominantly urban areas. Its evaluation is based on non-experimental data and a generalized matching estimator, to control for the non-random allocation of the programme. Behrman, Chang and Todd (2004) do not find significant effects of the programme on height. Notice, however, that a linear matching estimator, such as the OLS estimates in Table 6.1, would also give zero or negative estimates in our case.

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[25] According to the WHO/CDC tables, the median height at 60 months for a boy is 109.93 cms and the standard deviation is 4.59.

[26] The estimate is obtained using a regression of wages over height and education, correcting for selection into employment. We are not aware of similar estimates for Colombia.

In the case of *Hogares Comunitarios* in Guatemala city, Ruel *et al.* (2006) used a case-control methodology to estimate the effect of the programme on 250 beneficiaries. They report that the programme significantly improved children's diet, especially their intake of vitamin A, iron, and zinc – essential micronutrients for physical and cognitive development and for protection from infectious diseases, while no results are reported for height.

Finally, in the case of the *Wawa Wasi* programme in Perú, its qualitative evaluation finds that the centers are environments where children are kept safe and fed nutritious meals, freeing mothers of worries and enabling them to work or study; we do not focus on the results of the quantitative evaluation here as they are difficult to interpret being based on a sample of less than 100 children (see Cueto *et al.* 2009).

## 7. Falsification exercises and sensitivity analysis

The credibility of our results and their internal validity relies on the assumption that the instruments are uncorrelated with the error term of equation (1). In this section, we investigate this issue by: (i) conducting a falsification exercise using birth weight and mother's height, and (ii) conducting a sensitivity analysis along the lines of Conley, Hansen and Rossi (2012).

*7.1 Falsification exercise using birth weight and mother's height*

Birth weight and mother's height will be affected by many of the variables that determine child's height but, unlike child's height, it cannot be affected by participation into HC. This makes it a good candidate as an outcome variable for a falsification exercise.

To provide a sense of the plausibility of our identification assumption, we estimate a specification similar to those reported in Table 6.1, but using birth weight or mother's height as dependent variables. If we were to find that our instrumental variable procedure indicates an effect of the programme on birth weight or mother's height, one would suspect that the instruments we are using are correlated with unobservable determinants of nutritional status and are therefore invalid. Therefore a correlation between these factors with the instruments we use would induce a similar bias both in the specification for height per age and that of the variables that we use for the falsification test (birth weight and mother's height).

Table 7.1 replicates Table 6.1 but with birth weight (standardized) as dependent variable. The birth weight estimates are imprecise because of measurement error (it is self-reported) and the smaller sample due to missing values in recording birth weight. The sign of the point estimate varies across instruments and the estimation method used. This variation in signs according to the instrument contrasts with the consistency in the size of the effect on height per age that we reported in Table 6.1. The point estimates in the top panel of column 1 are quite close to zero, and the one corresponding to exposure is negative. The point estimates when we use distance as instrument are positive, while the capacity ones are negative (consistent with the compensatory hypothesis indicated by Figure 4). Note that the capacity ones in the ENDS are negative and statistically significant at 10% when using the ENDS (middle and bottom panel). This is not so worrying because, if anything, if might mean that the estimates in Table 6.1 underestimate the true effect (and indeed, the estimates in the middle and bottom panel of Table 6.1 are smaller than those in the top panel of the same Table).

{Table 7.1}

Table 7.2 is analogous to Table 7.1 but uses mother's height (standardized) as dependent variable. When we combine all the instruments, the point estimates are negative, but not statistically significant (column 1). A substantial fraction of the coefficients in the top panel are reasonably close to zero. Admittedly, the estimates in Tables 7.1 and 7.2 are less precisely estimated than those in Table 6.1 (although the standard errors of some coefficients of Table 7.2 are not much larger than those of Table 6.1), but it is also the lack of a consistent pattern in the sign of the estimates of Tables 7.1 and 7.2 that give us confidence in our main results (reported in Table 6.1).

*7.2 Sensitivity analysis*

In this section, we present evidence on the robustness of our conclusions to deviations of our main identification assumption: that the instruments are uncorrelated with the error term of equation (7). In this regard, we follow the approach of Conley, Hansen and Rossi (2012) which consists on estimating $\alpha$ in the following regression:

$$H_i = \alpha A_i + \theta Z_i + gI_i + \varepsilon_i, \tag{8}$$

where $I_i$ is the instrument under scrutiny and $g$ is a parameter that measures the direct impact of the instrument on the outcome of interest (child's height). In the previous sections, we have assumed that $g$ is equal to zero. Conley, Hansen and Rossi (2012) show how to obtain confidence intervals for $\alpha$ if one either assumes support restrictions on $g$ or assumes a distribution (prior) for $g$. For instance, in the case of distance, one might suspect that children from poorer households live further away from various amenities and, therefore, $g$ would be negative rather than zero, introducing a bias in the estimate of $\alpha$.

In order to simplify the exposition, when we scrutinize instrument $I$ we only use instrument $I$ to compute the prediction for the instrumental variable regressions. In particular, we do not use the square of $I$.[27] Moreover, we only analyze the effect of relaxing the orthogonality restriction for distance and capacity (given the findings of subsection 5.2, the results for HC fee would follow a pattern similar to that of capacity in the FeA dataset).

In subsection 5.2, we showed that conditioning on the distance to other facilities (schools, health centres, and town hall) was important to reduce the association between distance from the household to the HC nursery and other covariates. However, one might worry that conditioning on the distance to other facilities does not completely eliminate the correlation between the distance to the nearest HC nursery and the error term. In particular, one might worry that $g$ is negative in equation (8).

Figure 9 and 10 shows the confidence interval for $\alpha$ assuming that $g$ lies in the interval $[k,0]$ for values of $k$ ranging from 0 to -0.20.[28] The figure also shows the point estimate for $\alpha$ if $g=k/2$. The point estimate of $\alpha$ decrease slowly as $g$ decreases. The lower bound of the 90% confidence interval for $\alpha$ crosses zero if $k$ is smaller than -0.135 for attendance and -0.055 for exposure.

{Figures 9 and 10}

Clearly, any assessment on whether our results on the HC programme are robust or not depends on whether or not these values for $k$ (-0.135 and -0.055) are "small" or "large". To assess this, we run a reduced form regression: child's height over distance to HC, capacity, fee, distance to other amenities (but exclude square terms, and interactions) and all the other covariates. In this reduced form regression, the coefficient on distance to the health centre is -0.061 (standard error = 0.076) and the

[27] This is the reason why our results in Table 6.1 differ slightly from those shown in the Figures 9 to 14 when g=0.

[28] We thank Conley *et al.* (2012) for making their code available on the web. For each value of $k$, the confidence interval is built as the union of the confidence intervals obtained for a grid defined over $[k,0]$. On the basis of our argument below, -0.20 is very small when we compare it with coefficients on the distances to the health centre and to the town hall.

coefficient on distance to the town hall is -0.051 (standard error = 0.056).[29] It is reasonable to think that distance to health centre and town hall will be more correlated to child's unobserved background characteristics than distance to the nearest HC nursery (a HC nursery can easily close down or move location if its *Madre Comunitaria* does not wish to continue, and we expect the town amenities to locate around the town hall). We conclude from this that our conclusions are robust to small and even not so small violation of our identification assumption ($g$=0).

The inspection of the estimated coefficients of the reduced form regression provides similar insights. It is interesting to note that the coefficient of distance to the HC nursery (-0.136, standard error = 0.076) is more than twice the coefficient of distance to the town hall (-0.051, standard error= 0.056). Even if one believed that part of the partial correlation between height and distance to the HC nursery is due to location related unobserved heterogeneity, we believe that unobserved heterogeneity associated with location should be stronger for the town hall. As a result, even if one took the extreme assumption that the correlation between height and distance to the town hall is purely due to unobserved heterogeneity related to location, the fact that the coefficient on distance to the HC nursery is much larger than the coefficient on distance to the town hall seems to support our interpretation of the results that at least part of the correlation between distance to the HC nursery and height is due to the participation in the HC programme and is therefore causal.

Figures 11 and 12 (13 and 14) scrutinize the robustness of our conclusions for the capacity instrument in the FeA (ENDS) dataset. We concentrate on negative values of $k$ because the results of subsection 5.2 were consistent with policy makers allocating capacity across towns in a compensatory way (the results in Table 7.1 -birth weight regressions- also seem to support this). Hence, we will expect children living in towns with more capacity to be shorter. Figures 11 to 14 show that, if $k$ is negative, our results in Table 6.1 underestimate the effect of the HC programme as the point estimate of $\alpha$ increases as $k$ becomes more negative.

{Figures 11, 12, 13, and 14}

[29] We do not use distance to school because its coefficient is positive.

## 8. Conclusions

In this paper, we have studied one of the largest welfare programme in Colombia *Hogares Comunitarios*, which is a community nursery programme, that costs about 250 million US$ per year, using two datasets: one representative of very poor children living in rural areas of Colombia (FeA survey) while the other (ENDS survey) focussing on urban areas and including relatively less poor children. Similar programmes exist in Bolivia, Perú, Guatemala, India, and México. Despite their importance, little is known about the effects of these types of programmes.

Our focus is on how programme participation affects child's height, which is a good indicator of long-run nutritional status. Our results show that, among eligible children, those from the poorest families are more likely to participate in the programme. We also find that programme participation has zero or negative correlation with child's height. To correct for the obvious selection bias in giving a causal interpretation to the simple comparison between participants and non participants, we use an IV approach, where we use as instruments variables that are related to the availability of the programme, such as cost variables: the distance from the household residence to the nearest HC, the ratio of slots available (filled plus vacant) in a town to the number of eligible children and the average level of fees paid in a town. Unlike the OLS results, the IV estimates of programme participation on child's height are positive and show sizeable effects of programme participation. The effects are similar across the three different instruments (distance from the household to the nearest HC nursery, the median fee in the town, and the capacity of the HC programme in the town). If we consider that results from different instruments are different Local Average Treatment Effects, our results indicate that either the effect of the programme is homogenous or households do not self-select into the programme based on unobserved gains. This reinforces the external validity of our estimates.

We provide an array of evidence to support the internal validity of our estimates. (1) we show that controlling for distance to health centres, schools and town halls (as we do in our empirical specifications) reduces the association between distance to the HC nursery and household variables that predict being taller, (2) there is no strong relation between distance to the HC nursery and the other two instruments (fee and capacity) which strengthens our case, given that the point estimates are similar for the different instruments, (3) when we perform the same exercise on birth weight and mother's height, which should not be affected by the programme, we do not obtain any significant positive effect and several estimates are negative (4) capacity (fee) is higher (lower) in towns with covariate values associated with poor nutritional status (5) we would obtain positive and statistically significant effects of the programme on child's height even if we allow for moderate direct effects on child's height of distance to the nearest HC, and (6) our effects are biologically feasible and lie well within experimental estimates of nutrition interventions with complementary food in food-insecure populations.

Programmes evolve with time: staff motivation, accountability; monitoring, guidelines, etc. are likely to be different at the start of a programme than in the longer term after it has evolved. Contrary to recent evaluations of conditional cash transfer programmes, this paper estimates the effect of a programme that was established long-ago (an exception is Behrman *et al.* 2011). While this creates challenges in terms of both internal and external validity of the results, it has the advantage of providing results that are likely to be representative of the programme as it will run in the future.

Our results are credible, economically significant and important. The programme, which has been operating for 20 years and which is targeted to the poorest 30 per cent of Colombian households, seems to improve the nutritional status of the poorest of the eligible children. The nutritional status of children attending HC is only slightly lower than the nutritional status of other eligible children that do not attend. However, as the attendees are from the poorest of the eligible families, their status would be considerably worse in the absence of the programme. 0.40 of a standard deviation in height per age is a large and substantive difference that can have important long run consequences for the development of these children. This result is also important because the programme relies on community resources and it is therefore relatively cheap to run.

These considerations do not mean that the HC nurseries are a perfect programme. The programme takes the poorest of the poor Colombian children and brings them up to a level that is considerably higher than the level that would prevail in the absence of the programme, but is still far from satisfactory. Many of the children attending HC are still stunted in growth and suffer from a number of other problems. There is therefore scope for interventions that try to improve the functioning of such an intervention and their evaluation as well as for the consideration of alternatives that might turn out to be more cost effective.

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**Table 4.1. Descriptive Statistics**

| Variable label | Definition | FeA | | ENDS | |
|---|---|---|---|---|---|
| | | Mean | SD | Mean | SD |
| Age child months | Child age in months | 49.2 | 22.9 | 35.5 | 21.3 |
| Age head | Household head's age in years divided by 100 | 0.39 | 0.11 | 0.42 | 0.15 |
| Age head (Ln) | Logarithm of household head's age in years divided by 100 | -0.97 | 0.27 | -0.94 | 0.35 |
| Age mother | Mother's age in years divided by 100 | 0.32 | 0.07 | 0.28 | 0.07 |
| Age mother (Ln) | Logarithm of mother's age in years divided by 100 | -1.17 | 0.22 | -1.31 | 0.24 |
| Altitude | Altitude in thousand meters | 0.45 | 0.69 | 0.62 | 0.81 |
| Attendance HC | 1 if the child is attending a HC centre, 0 otherwise | 0.25 | 0.43 | 0.32 | 0.47 |
| Birth Weight | Child’s weight at birth | 3.35 | 0.53 | 3.29 | 0.54 |
| Capacity HC | Number of places in HC centres in the town divided by number of children 2 to 6 years old | 0.32 | 0.19 | 0.25 | 0.17 |
| Centre town | 1 if household lives in the main part of the town, 0 otherwise | 0.50 | 0.50 | 1.00 | 0.00 |
| Distance HC (mins) | Distance (minutes divided by 100) to the nearest HC nursery for those not attending a HC nursery, and actual distance to the HC nursery for those attending | 0.20 | 0.32 | | |
| Distance HC First (mins) | same as Distance HC First but in the first wave of data | 0.24 | 0.34 | | |
| Distance health centre (mins) | Distance in minutes to the nearest health care provider, divided by 100 | 0.39 | 0.52 | | |
| Distance health centre town ave. (mins) | Median of time_hea in the municipality | 0.29 | 0.26 | | |
| Distance school centre (mins) | Distance in minutes to nearest school, divided by 100 | 0.14 | 0.15 | | |
| Distance school town ave. (mins) | Median of time_sch in the municipality | 0.10 | 0.05 | | |
| Distance town hall (mins) | Distance in minutes to the town hall, divided by 100 | 0.50 | 0.63 | | |
| Distance town hall town ave. (mins) | Median of time_alc in the municipality | 0.40 | 0.34 | | |
| Educ. head below primary | 1 if household head did not complete primary education, 0 otherwise | 0.66 | 0.47 | 0.10 | 0.29 |
| Educ. head below secondary | 1 if household head completed primary education but did not complete secondary education, 0 otherwise | 0.29 | 0.45 | 0.55 | 0.50 |
| Educ. head secondary | 1 if household head completed secondary education, 0 otherwise | 0.05 | 0.22 | 0.35 | 0.48 |
| Educ. mother below primary | 1 if mother did not complete primary education, 0 otherwise | 0.59 | 0.49 | 0.03 | 0.16 |
| Educ. mother below secondary | 1 if mother completed primary education but did not complete secondary education, 0 otherwise | 0.34 | 0.47 | 0.36 | 0.48 |
| Educ. mother secondary | 1 if mother completed secondary education, 0 otherwise | 0.06 | 0.24 | 0.61 | 0.49 |
| Exposure | Number of months that the child has attended a HC centre divided by the age of the child in months | 0.17 | 0.23 | 0.10 | 0.19 |
| Fee HC | Median fee to attend a HC nursery in the municipality. Colombian pesos divided by 1000. | 3.86 | 3.13 | | |
| Female | 1 if child is female, 0 if child is male | 0.49 | 0.50 | 0.49 | 0.50 |
| HAZ | Child’s height. Unit: z-scores | -1.27 | 1.09 | -0.77 | 0.98 |
| Health insurance (prop. in town) | Proportion of children with formal health insurance in the municipality | 0.68 | 0.19 | | |
| Hospital | 1 if there is a hospital in the town, 0 otherwise | 0.70 | 0.46 | | |
| Mother's height | Mother’s height in metres | 1.54 | 0.06 | 1.55 | 0.06 |
| Num. children | Number of children 2 to 6 years old in the town, divided by 10000 | 0.26 | 0.25 | 3.23 | 6.67 |
| Order | Order of kid in the household | 3.63 | 1.75 | 2.47 | 1.67 |
| Order (Ln) | Logarithm of order of kid in the household | 1.16 | 0.53 | 0.71 | 0.62 |
| Pipe water (prop. town) | Percentage of households with pipe water in the town | 0.85 | 0.14 | 0.71 | 0.30 |
| Price index | Food price index | 0.94 | 0.14 | | |
| Sewage (prop. town) | Percentage of households with sewage connection in the municipality | 0.45 | 0.37 | 0.86 | 0.19 |
| Wage female rural | Rural female daily wage in pesos as indicated by the town major divided by 10000 in Colombian pesos (December 2003) | 0.90 | 0.37 | | |
| Wage female urban | Urban female daily wage in pesos as indicated by the town major divided by 10000 in Colombian pesos (December 2003) | 0.98 | 0.34 | | |

***Notes*** . Statistics are restricted to estimation sample: 2413 children (FeA wave2 only) and 6179 (ENDS)

**Table 4.2. Percentage of Children attending Hogares Comunitarios**

| | Age | | | | | | |
|---|---|---|---|---|---|---|---|
| | **0** | **1** | **2** | **3** | **4** | **5** | **6** |
| **FeA** | | | | | | | |
| Boys | 3.6% | 16.1% | 42.9% | 45.0% | 34.7% | 20.1% | 7.0% |
| Girls | 2.5% | 19.1% | 38.4% | 46.6% | 35.5% | 16.9% | 7.6% |
| **ENDS** | | | | | | | |
| Boys | 1.9% | 17.7% | 38.2% | 48.4% | 50.3% | 45.6% | - |
| Girls | 1.8% | 15.7% | 40.5% | 46.1% | 49.0% | 44.7% | - |

***Notes*** . Sample sizes: 2352 (FeA Wave 1), 2413 (FeA wave 2), 916 (FeA wave 3), 6179 (ENDS)

**Table 4.3 Reasons for not attending HC**

| | FeA | | | ENDS | | |
|---|---|---|---|---|---|---|
| | **Age 0-1** | **Age 2-3** | **Age 4-6** | **Age 0-1** | **Age 2-3** | **Age 4-5** |
| Available caregiver at home | 63% | 46% | 20% | 81% | 79% | 74% |
| No HC facility or too far | 15% | 27% | 15% | 2% | 3% | 3% |
| Cannot afford fee | 4% | 8% | 4% | 1% | 3% | 3% |
| Does not like food | 1% | 4% | 3% | 1% | 4% | 4% |
| Other | 16% | 15% | 57% | 15% | 12% | 17% |

***Notes*** . Sample sizes: 1760 (FeA Wave 1), 1713 (FeA wave 2), 714 (FeA wave 3), 4177 (ENDS)

**Table 4.4. Distribution of the Instruments**

| | Entire Sample | | | | Participants | | | |
|---|---|---|---|---|---|---|---|---|
| | **Distance (minutes)** | **Fee (Pesos)** | **Capacity** | **Capacity** | **Distance (minutes)** | **Fee (pesos)** | **Capacity** | **Capacity** |
| 25th Percentile | 5 | 1803 | 18% | 15% | 3 | 1000 | 22% | 16% |
| Median | 10 | 3500 | 28% | 23% | 5 | 3000 | 33% | 26% |
| Mean | 22 | 3842 | 31% | 25% | 11 | 3135 | 38% | 27% |
| 75th Percentile | 27 | 5500 | 37% | 32% | 15 | 4000 | 53% | 34% |
| Survey | FeA | FeA | FeA | ENDS | FeA | FeA | FeA | ENDS |

***Notes*** . Sample sizes for entire sample: 2352 (FeA Wave 1), 2413 (FeA wave 2), 916 (FeA wave 3), 6179 (ENDS). Sample sizes for participants: 580 (FeA Wave 1), 597 (FeA wave 2), 198 (FeA wave 3), 2002 (ENDS).

**Table 5.1. First Stage Regressions for FeA. All instruments simultaneously**

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| | | EXPOSURE | | | ATTENDANCE | |
| | OLS | OLS with quadratic terms | Tobit | OLS | OLS with quadratic terms | Probit |
| Capacity HC | 0.267*** | 0.022 | 0.400 | 0.340*** | 0.368 | 2.209* |
| | [0.040] | [0.167] | [0.402] | [0.100] | [0.266] | [1.280] |
| Capacity HC^2 | | 0.297 | 0.019 | | -0.036 | -1.152 |
| | | [0.191] | [0.467] | | [0.286] | [1.448] |
| Fee HC | -0.005** | -0.010 | -0.014 | -0.006* | -0.003 | -0.002 |
| | [0.002] | [0.006] | [0.013] | [0.004] | [0.009] | [0.042] |
| Fee HC^2 | | 0.000 | 0.000 | | -0.000 | -0.001 |
| | | [0.000] | [0.001] | | [0.001] | [0.002] |
| Distance HC (current) | -0.034* | -0.126*** | -0.354*** | -0.120*** | -0.328*** | -1.456*** |
| | [0.018] | [0.037] | [0.095] | [0.036] | [0.071] | [0.414] |
| Distance HC (current)^2 | | 0.063*** | 0.119* | | 0.142*** | 0.359 |
| | | [0.018] | [0.064] | | [0.038] | [0.343] |
| Distance HC (wave 1) | -0.067*** | -0.141*** | -0.352*** | -0.067* | -0.144* | -0.645* |
| | [0.017] | [0.043] | [0.114] | [0.035] | [0.075] | [0.380] |
| Distance HC (wave 1)^2 | | 0.053** | 0.082 | | 0.058 | 0.190 |
| | | [0.023] | [0.067] | | [0.040] | [0.249] |
| F-stat (Distance) | F(2,50)=15.38 | F(4,50)=14.63 | | F(2,50)=13.92 | F(4,50)=11.64 | |
| F-stat (Fee) | F(1,50)=6.71 | F(2,50)=5.77 | | F(1,50)=2.88 | F(2,50)=2.92 | |
| F-stat (Capacity) | F(1,50)=44.09 | F(2,50)=24.93 | | F(1,50)=11.56 | F(2,50)=5.83 | |
| F-stat (Capacity &Fee) | F(2,50)=30.27 | F(4,50)=23.05 | | F(2,50)=9.63 | F(4,50)=7.58 | |
| F-stat (Capacity, Fee, Distance) | F(4,50)=35.33 | F(8,50)=29.34 | | F(4,50)=14.35 | F(8,50)=11.83 | |

***Notes*** . Each column reports the coefficients of a regression of either Exposure or Attendance over the variables of the left column (instruments) and additional control variables (same as those in Table A1 in the Appendix). The estimation method is reported at the top of the column. Sample sizes FeA: 2352 (Wave 1), 2413 (wave 2), 916 (wave 3). Standard errors in brackets are clustered at the town level, *** p<0.01, ** p<0.05, * p<0.1

**Table 5.2. First Stage Regressions for FeA. Each instrument set independently**

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| | | **EXPOSURE** | | | **ATTENDANCE** | |
| | **OLS** | **OLS with quadratic term** | **Tobit** | **OLS** | **OLS with quadratic term** | **Probit** |
| ***CAPACITY as instrument*** | | | | | | |
| Capacity HC | 0.310*** | 0.168 | 0.637 | 0.403*** | 0.521* | 2.486** |
| | [0.041] | [0.172] | [0.412] | [0.100] | [0.268] | [1.236] |
| Capacity HC^2 | | 0.185 | -0.150 | | -0.153 | -1.249 |
| | | [0.202] | [0.485] | | [0.298] | [1.425] |
| F-stat (Capacity) | F(1,50)=56.70 | F(2,50)=30.59 | | F(1,50)=16.21 | F(2,50)=8.42 | |
| ***FEE as instrument*** | | | | | | |
| Fee HC | -0.010*** | -0.021** | -0.036** | -0.013** | -0.021 | -0.081 |
| | [0.003] | [0.008] | [0.018] | [0.005] | [0.014] | [0.058] |
| Fee HC^2 | | 0.001 | 0.001 | | 0.001 | 0.002 |
| | | [0.000] | [0.001] | | [0.001] | [0.003] |
| F-stat (Fee) | F(1,50)=12.30 | F(2,50)=5.52 | | F(1,50)=6.87 | F(2,50)=3.04 | |
| ***DISTANCE as instrument*** | | | | | | |
| Distance HC (current) | -0.037** | -0.127*** | -0.368*** | -0.125*** | -0.331*** | -1.495*** |
| | [0.018] | [0.039] | [0.097] | [0.038] | [0.074] | [0.409] |
| Distance HC (current)^2 | | 0.062*** | 0.125* | | 0.141*** | 0.398 |
| | | [0.020] | [0.064] | | [0.039] | [0.325] |
| Distance HC (wave 1) | -0.071*** | -0.141*** | -0.351*** | -0.071* | -0.143* | -0.640* |
| | [0.019] | [0.045] | [0.118] | [0.036] | [0.074] | [0.377] |
| Distance HC (wave 1)^2 | | 0.050** | 0.080 | | 0.055 | 0.191 |
| | | [0.023] | [0.066] | | [0.039] | [0.245] |
| F-stat (Distance) | F(2,50)=16.86 | F(4,50)=15.05 | | F(2,50)=15.05 | F(4,50)=11.13 | |

***Notes*** . Each column reports the coefficients of three different regressions of either Exposure or Attendance over the corresponding instruments (variables in the left column) and additional control variables (same as those in Table A1 in the Appendix). The estimation method is reported at the top of the column. Sample sizes FeA: 2352 (Wave 1), 2413 (wave 2), 916 (wave 3). Standard errors in brackets are clustered at the town level, *** p<0.01, ** p<0.05, * p<0.1

**Table 5.3. First Stage Regressions for ENDS**

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| | EXPOSURE | | | ATTENDANCE | | |
| | OLS | OLS with quadratic term | Tobit | OLS | OLS with quadratic term | Probit |
| Capacity HC | 0.122*** | 0.321*** | 0.814*** | 0.197* | 0.610*** | 2.025*** |
| | [0.047] | [0.054] | [0.127] | [0.107] | [0.142] | [0.503] |
| Capacity HC^2 | | -0.151*** | -0.408*** | | -0.313*** | -1.028*** |
| | | [0.037] | [0.091] | | [0.095] | [0.350] |
| F-stat (Capacity) | F(1,219)=6.88 | F(2,219)=20.49 | | F(1,219)=3.43 | F(2,219)=9.95 | |

***Notes*** . Each column reports the coefficients of a regression of either Exposure or Attendance over the variables of the left column (instruments) and additional control variables (same as those in Table A1 in the Appendix). The estimation method is reported at the top of the column. Sample sizes: ENDS 6170 (Exposure) and 6179 (Attendance). Standard errors in brackets are clustered at the town level, *** p<0.01, ** p<0.05, * p<0.1

**Table 6.1. Effect of HC Participation on Child's Height (z-score)**

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
|---|---|---|---|---|---|---|---|
| | All | Capacity | Fee | Distance | Capacity | Ordinary Least Squares | |
| ***Top Panel: 2SLS with nonlinear fitted values as instruments*** | | | | | | | |
| Exposure | 0.885** | 0.942 | 1.106 | 0.987* | 1.227*** | -0.090 | -0.069 |
| | [0.374] | [0.585] | [0.731] | [0.500] | [0.365] | [0.091] | [0.055] |
| Attendance | 0.405** | 0.419* | 0.499** | 0.468** | 0.826*** | -0.099** | -0.065*** |
| | [0.185] | [0.228] | [0.236] | [0.223] | [0.192] | [0.044] | [0.025] |
| ***Middle Panel: 2SLS with linear and quadratic terms as instruments*** | | | | | | | |
| Exposure | 0.939** | 0.987 | 0.742 | 0.893 | 0.751 | | |
| | [0.373] | [0.593] | [0.735] | [0.660] | [0.692] | | |
| Attendance | 0.496** | 0.627 | 0.558 | 0.422 | 0.442 | | |
| | [0.251] | [0.473] | [0.580] | [0.355] | [0.381] | | |
| ***Botton Panel: 2SLS with linear terms only as instruments*** | | | | | | | |
| Exposure | 1.003** | 0.892 | 0.604 | 1.263 | 0.373 | | |
| | [0.412] | [0.598] | [0.627] | [0.761] | [0.814] | | |
| Attendance | 0.632** | 0.687 | 0.469 | 0.615 | 0.234 | | |
| | [0.270] | [0.478] | [0.503] | [0.392] | [0.487] | | |
| Dataset | FeA | FeA | FeA | FeA | ENDS | FeA | ENDS |

***Notes*** . Each cell reports the coefficient (and standard error clustered at the town level in brackets) of a regression of child's height (z-score) on either Exposure or Attendance and additional control variables (same as those in Table A1 in the Appendix). Columns labelled 1 to 5 reports coefficients estimated using Two Stage Least Squares with instruments being either nonlinear fitted values estimated using the variable at the top of the column and its square term together with all the control variables (top panel), or linear and quadratic terms of the variable at the top of the column (middle panel), or the linear term of the variable at the top of the column (bottom panel). Columns labelled 6 and 7 report OLS coefficients. Sample sizes: FeA wave 1: 2352, FeA wave 2: 2413, FeA wave 3: 916. ENDS sample size is 6170 (for exposure) and 6189 (for attendance). *** p<0.01, ** p<0.05, * p<0.1.

**Table 6.2. Effect of HC Participation on Child's Height at different Quantiles**

| Percentile | 10 | 25 | 50 | 75 | 90 |
|---|---|---|---|---|---|
| ***FeA*** | | | | | |
| Exposure to HC | 1.825** | 1.689** | 1.292** | 0.482 | -0.093 |
| | [0.784] | [0.703] | [0.642] | [0.654] | [0.846] |
| Attendance to HC | 0.597** | 0.572** | 0.384* | 0.133 | -0.003 |
| | [0.268] | [0.233] | [0.224] | [0.224] | [0.283] |
| | | | | | |
| ***ENDS*** | | | | | |
| Exposure to HC | 3.419*** | 3.015*** | 2.284** | 1.667* | 1.697 |
| | [1.214] | [0.926] | [0.977] | [0.884] | [1.084] |
| Attendance to HC | 1.331*** | 1.063*** | 0.987*** | 0.660*** | 0.475 |
| | [0.319] | [0.219] | [0.233] | [0.208] | [0.299] |

***Notes*** . Each cell reports the coefficient (and standard error clustered at the town level in brackets) of a quantile regression of child's height (z-score) on either Exposure or Attendance, additional control variables (same as those in Table A1 in the Appendix), and a second order polynomial of the first stage residual (control function) . The percentile is indicated at the top of the column. Standard errors are computed by block bootstrap at the town level. Sample sizes: FeA 5681, ENDS 6170 (Exposure) 6179 (Attendance). *** p<0.01, ** p<0.05, * p<0.1

**Table 7.1. Effect of HC Participation on Child's Birthweight (standardized)**

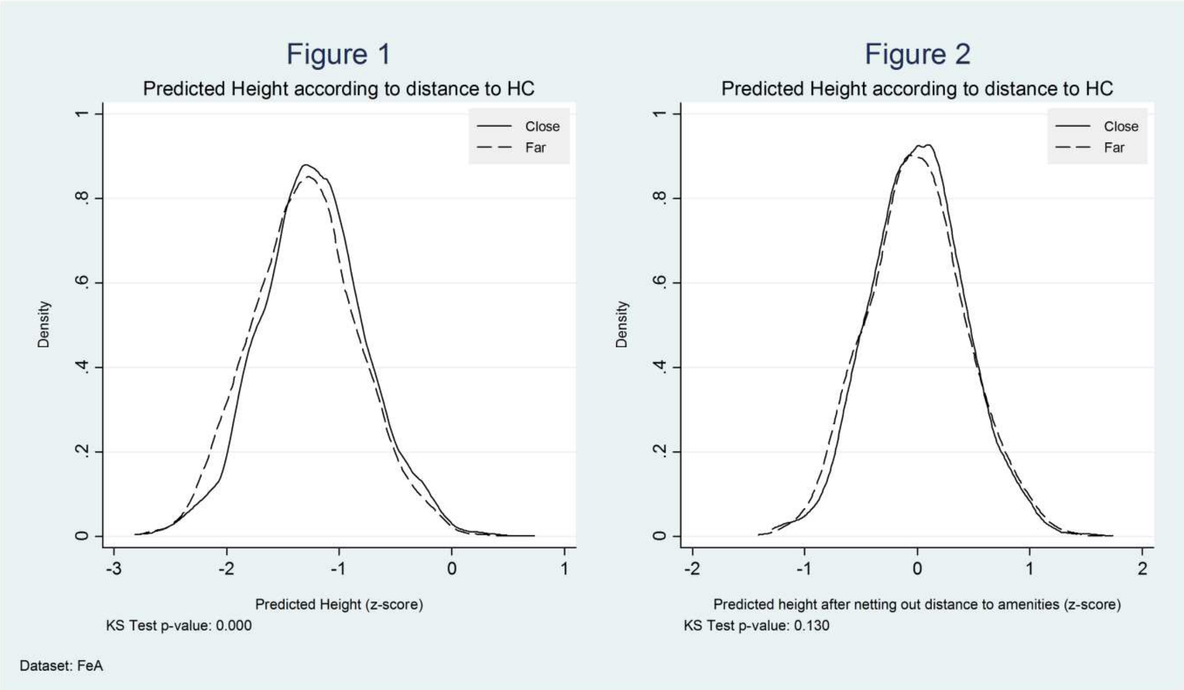
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
|---|---|---|---|---|---|---|---|
| | All | Capacity | Fee | Distance | Capacity | Ordinary Least Squares | |
| ***Top Panel: 2SLS with nonlinear fitted values as instruments*** | | | | | | | |
| Exposure | -0.079 | -0.359 | 0.122 | 0.247 | -0.450 | -0.030 | 0.016 |
| | [0.700] | [0.798] | [1.105] | [0.878] | [0.736] | [0.154] | [0.149] |
| Attendance | 0.004 | 0.195 | 0.477 | 0.127 | -0.102 | -0.085 | -0.018 |
| | [0.319] | [0.369] | [0.437] | [0.360] | [0.310] | [0.099] | [0.052] |
| ***Middle Panel: 2SLS with linear and quadratic terms as instruments*** | | | | | | | |
| Exposure | 0.257 | -0.376 | -0.909 | 1.015 | -2.791* | | |
| | [0.839] | [1.098] | [2.655] | [1.259] | [1.586] | | |
| Attendance | 0.216 | -0.331 | -1.152 | 0.492 | -1.566* | | |
| | [0.551] | [1.013] | [2.609] | [0.676] | [0.935] | | |
| ***Botton Panel: 2SLS with only linear terms as instruments*** | | | | | | | |
| Exposure | 0.439 | -0.384 | -1.057 | 1.482 | -2.797* | | |
| | [0.990] | [1.128] | [2.675] | [1.632] | [1.582] | | |
| Attendance | 0.427 | -0.334 | -0.959 | 0.920 | -1.555 | | |
| | [0.738] | [1.017] | [2.575] | [1.016] | [0.944] | | |
| Dataset | *FeA* | *FeA* | *FeA* | *FeA* | *ENDS* | *FeA* | *ENDS* |

***Notes*** . Each cell reports the coefficient (and standard error clustered at the town level in brackets) of a regression of child's birth weight (standarized) on either Exposure or Attendance and additional control variables (same as those in Table A1 in the Appendix). Columns labelled 1 to 5 reports coefficients estimated using Two Stage Least Squares with instruments being either nonlinear fitted values estimated using the variable at the top of the column and its square term together with all the control variables (top panel), or linear and quadratic terms of the variable at the top of the column (middle panel), or the linear term of the variable at the top of the column (bottom panel). Columns labelled 6 and 7 report OLS coefficients. Sample sizes: FeA 1334, ENDS 2093 (Exposure) and 2097 (Attendance). Standard errors in brackets are clustered at the town level, *** p<0.01, ** p<0.05, * p<0.1.

**Table 7.2. Effect of HC Participation on Mother's Height (standarized)**

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
|---|---|---|---|---|---|---|---|
| | All | Capacity | Fee | Distance | Capacity | Ordinary Least Squares | |
| ***Top Panel: 2SLS with nonlinear fitted values as instruments*** | | | | | | | |
| Exposure | -0.249 | 0.000 | 0.139 | -0.384 | 0.090 | -0.177 | -0.075 |
| | [0.396] | [0.614] | [1.002] | [0.502] | [0.376] | [0.133] | [0.064] |
| Attendance | -0.061 | 0.088 | 0.239 | -0.061 | 0.044 | -0.134 | 0.002 |
| | [0.247] | [0.351] | [0.536] | [0.316] | [0.172] | [0.085] | [0.027] |
| ***Middle Panel: 2SLS with linear and quadratic terms as instruments*** | | | | | | | |
| Exposure | -0.405 | -0.327 | 0.030 | -0.577 | -0.932 | | |
| | [0.434] | [0.660] | [1.518] | [0.592] | [0.803] | | |
| Attendance | -0.229 | -0.127 | 0.208 | -0.372 | -0.527 | | |
| | [0.289] | [0.550] | [1.064] | [0.330] | [0.438] | | |
| ***Botton Panel: 2SLS with only linear terms as instruments*** | | | | | | | |
| Exposure | -0.177 | -0.157 | 1.304 | -0.435 | -1.061 | | |
| | [0.503] | [0.710] | [1.474] | [0.799] | [0.967] | | |
| Attendance | -0.202 | -0.120 | 0.886 | -0.427 | -0.691 | | |
| | [0.341] | [0.551] | [1.032] | [0.461] | [0.633] | | |
| Dataset | *FeA* | *FeA* | *FeA* | *FeA* | *ENDS* | *FeA* | *ENDS* |

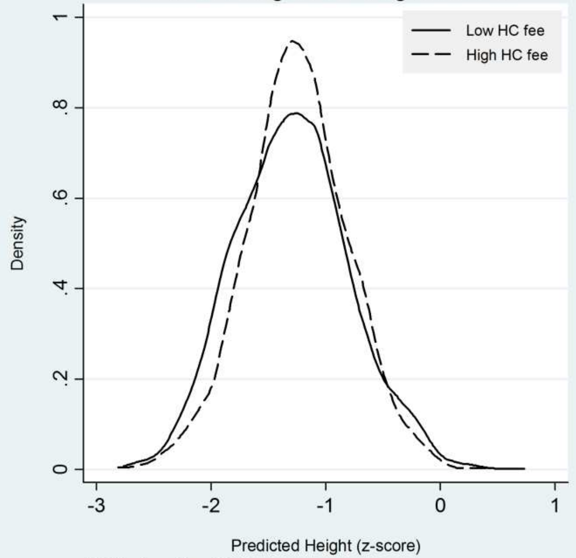
***Notes*** . Each cell reports the coefficient (and standard error clustered at the town level in brackets) of a regression of mother's height (standarized) on either Exposure or Attendance and additional control variables (same as those in Table A1 in the Appendix except mother's height). Columns labelled 1 to 5 reports coefficients estimated using Two Stage Least Squares with instruments being either nonlinear fitted values estimated using the variable at the top of the column and its square term together with all the control variables (top panel), or linear and quadratic terms of the variable at the top of the column (middle panel), or the linear term of the variable at the top of the column (bottom panel). Columns labelled 6 and 7 report OLS coefficients. Sample sizes: FeA 1834, ENDS 2093 (Exposure) and 2097 (Attendance). Standard errors in brackets are clustered at the town level, *** p<0.01, ** p<0.05, * p<0.1.




***Notes*:** Figure 1 plots the density of predicted height (z-score) by whether the child lives close to a HC (distance to HC is below the median) or far from a HC (distance is above the median). The prediction is computed using a linear regression of height over the control variables indicated in Table A1 of the Appendix. Figure 2 plots predicted residual of height. First, a regression of height (z-score) over distance to amenities (town hall, school, and health centre, its square terms and two-way interactions; as well as centre town dummy variable) is estimated. Second, the residuals from that regression are regressed over the other control variables (those indicated in Table A1 in the Appendix except distance to amenities and centre town dummy). The prediction from that regression is plot in Figure 2 by whether the child lives close or far from a HC. KS stands for Kolmogorov-Smirnov.

Figure 3

Predicted Height according to HC Fee

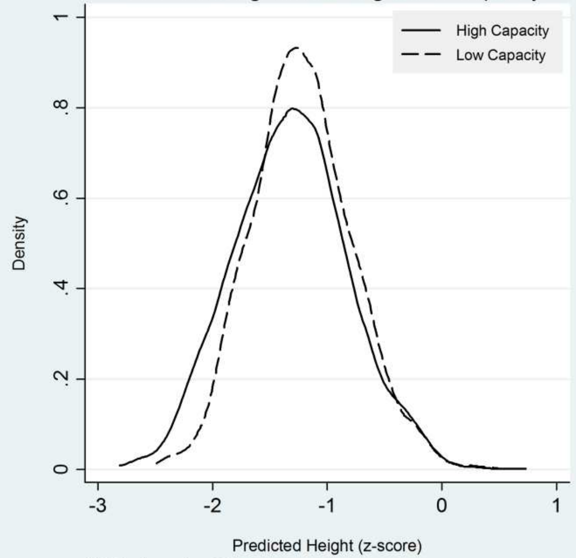



KS Test p-value: 0.000

Dataset: FeA

Figure 4

Predicted Height according to HC Capacity

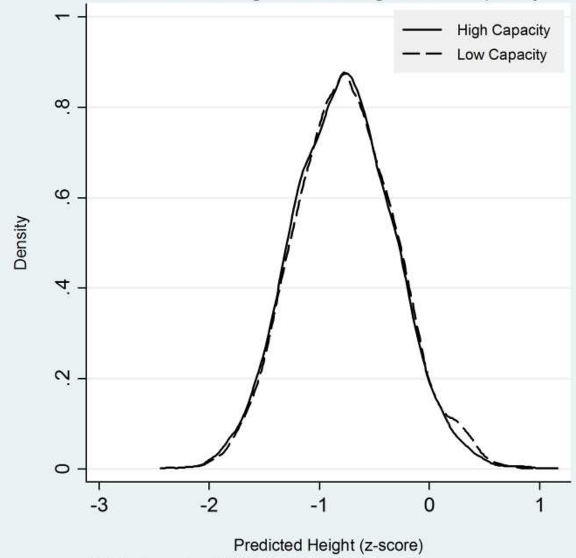



KS Test p-value: 0.000

Dataset: FeA

Figure 5

Predicted Height according to HC Capacity



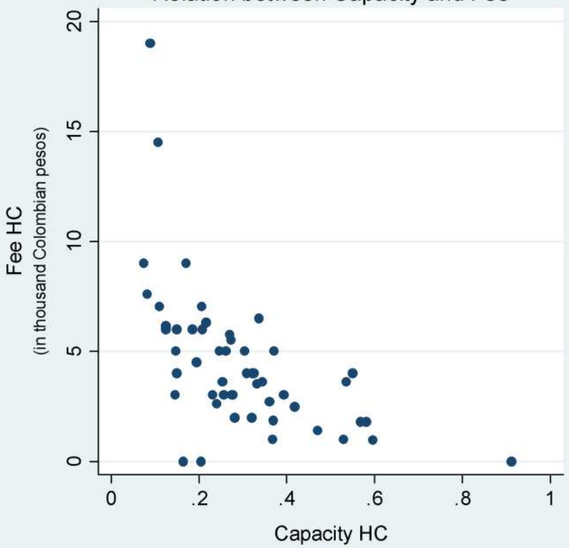

KS Test p-value: 0.257

Dataset: ENDS

***Notes*:** Figures 3 plots the density of predicted height (z-score) by whether the child lives in a town with either low (below the median) or high (above the median) fees. Figures 4 and 5 are analogous to Figure 3 but plot the density of predicted height according to whether the child lives in a town with either low (below the median) or high (above the median) capacity. The prediction is computed using a linear regression of height over the control variables indicated in Table A1 of the Appendix. KS stands for Kolmogorov-Smirnov

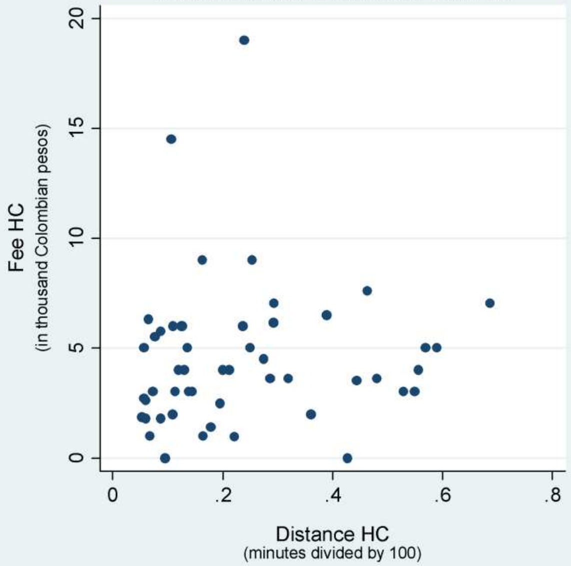
## Figure 6

Relation between Capacity and Fee



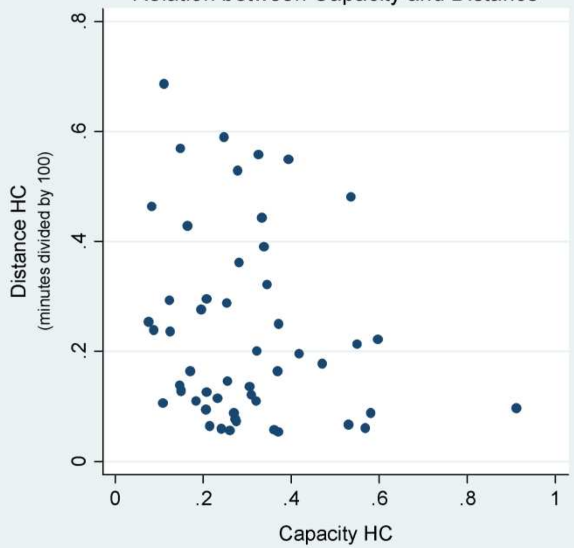

## Figure 7

Relation between Distance and Fee




## Figure 8

Relation between Capacity and Distance

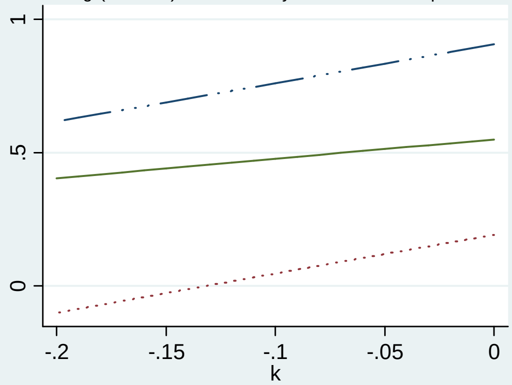



NOTE: Each plot represents the average value in a town

## Figure 9

90% Confidence intervals for Attendance

If g*(distance) enters directly in the outcome equation

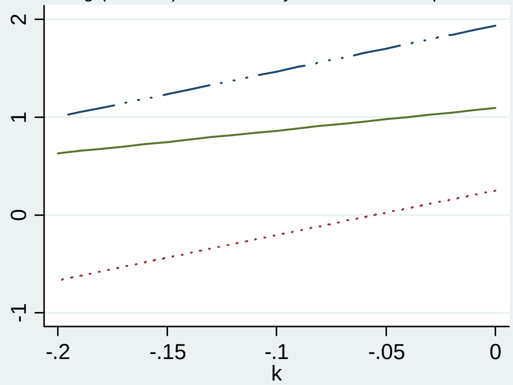



Dataset: FeA

## Figure 10

90% Confidence intervals for Exposure

If g*(distance) enters directly in the outcome equation

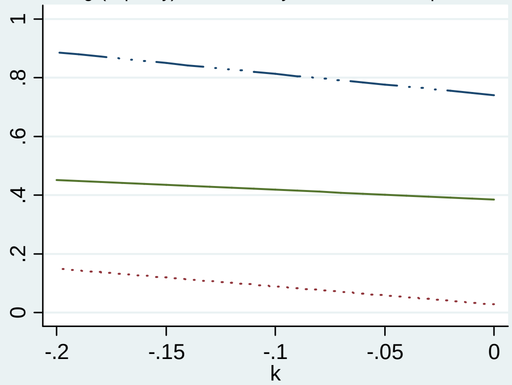



Dataset: FeA

## Figure 11

90% Confidence intervals for Attendance

If g*(capacity) enters directly in the outcome equation

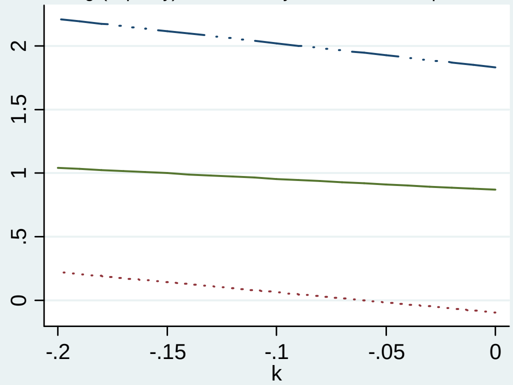



Dataset: FeA

## Figure 12

90% Confidence intervals for Exposure

If g*(capacity) enters directly in the outcome equation

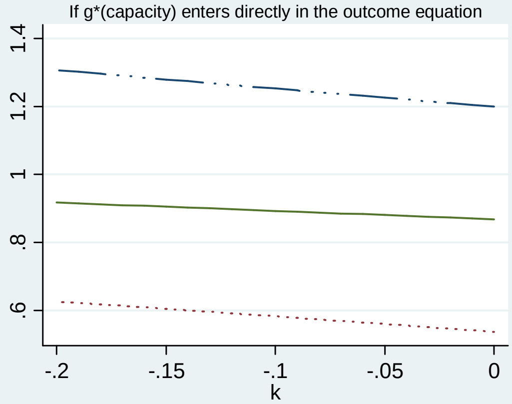



Dataset: FeA

## Figure 13

90% Confidence intervals for Attendance

If g*(capacity) enters directly in the outcome equation

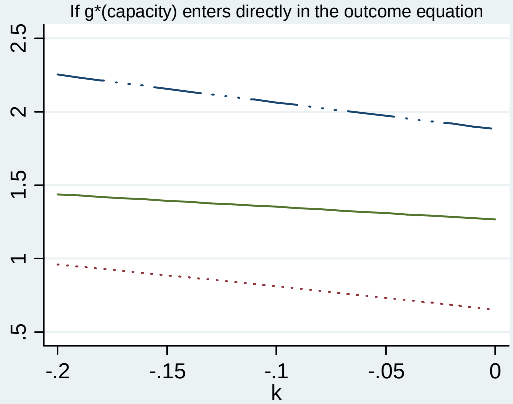



Dataset: ENDS

## Figure 14

90% Confidence intervals for Exposure

If g*(capacity) enters directly in the outcome equation




Dataset: ENDS

upper_ci    lower_ci    point_est

Assumed that g belongs to [k,0]. Point estimate is if g=k/2.

Confidence intervals are built using union of confidence intervals following Conley et al. (2012)

# Appendix A:

# Table A1

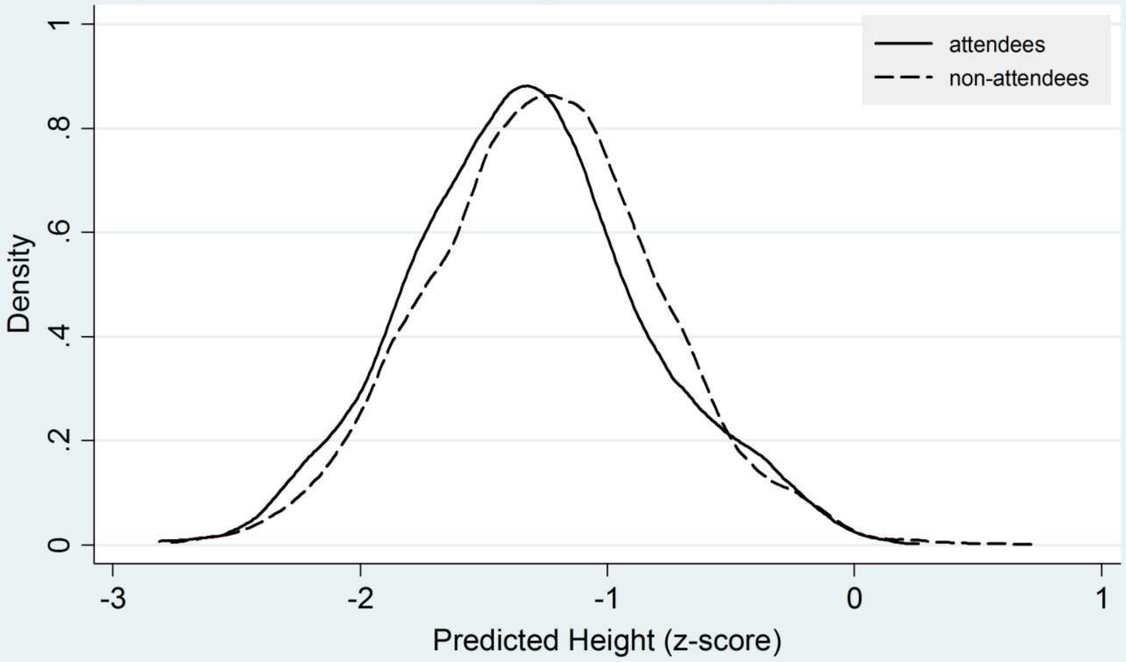
**Table A1. Effect of HC Participation on Child's Height (z-score)**

| | FeA | | ENDS | |
|---|---|---|---|---|
| | All Instruments | | Capacity | |
| **Exposure** | 0.885** | | 1.227*** | |
| | [0.374] | | [0.365] | |
| **Attendance** | | 0.405** | | 0.826*** |
| | | [0.185] | | [0.192] |
| **Female** | 0.134*** | 0.132*** | 0.039* | 0.048** |
| | [0.033] | [0.034] | [0.021] | [0.022] |
| **Age mother** | -0.034*** | -0.035*** | -0.044*** | -0.055*** |
| | [0.006] | [0.006] | [0.003] | [0.005] |
| **Age mother^2** | 0.029*** | 0.034*** | 0.000*** | 0.001*** |
| | [0.005] | [0.007] | [0.000] | [0.000] |
| **Age head (Ln)** | 0.284*** | 0.266*** | 0.040 | 0.076* |
| | [0.088] | [0.082] | [0.042] | [0.045] |
| **Age mother (Ln)** | 0.319** | 0.313** | 0.789*** | 0.811*** |
| | [0.128] | [0.123] | [0.080] | [0.085] |
| **Mother's height** | 5.571*** | 5.542*** | 5.435*** | 5.362*** |
| | [0.449] | [0.464] | [0.240] | [0.254] |
| **Order (Ln)** | -0.311*** | -0.292*** | -0.338*** | -0.336*** |
| | [0.040] | [0.037] | [0.027] | [0.028] |
| **Educ. mother below secondary** | 0.102* | 0.112** | 0.206** | 0.179* |
| | [0.057] | [0.055] | [0.091] | [0.104] |
| **Educ. mother secondary** | 0.222*** | 0.250*** | 0.194** | 0.157 |
| | [0.076] | [0.077] | [0.092] | [0.107] |
| **Educ. head below secondary** | 0.082 | 0.085 | -0.021 | -0.016 |
| | [0.057] | [0.059] | [0.049] | [0.050] |
| **Educ. head secondary** | 0.303*** | 0.291*** | -0.125** | -0.135** |
| | [0.095] | [0.087] | [0.061] | [0.062] |
| **Centre town** | -0.031 | -0.019 | | |
| | [0.063] | [0.062] | | |
| **Distance health centre (mins)** | 0.150 | 0.107 | | |
| | [0.111] | [0.103] | | |
| **Distance health centre^2** | 0.023 | 0.036 | | |
| | [0.044] | [0.044] | | |
| **Distance school centre (mins)** | 0.015 | -0.005 | | |
| | [0.285] | [0.279] | | |
| **Distance school centre^2** | 0.306 | 0.277 | | |
| | [0.386] | [0.398] | | |
| **Distance health centre*Distance school centre** | 0.153 | 0.164 | | |
| | [0.277] | [0.272] | | |
| **Distance town hall (mins)** | -0.211** | -0.181* | | |
| | [0.102] | [0.097] | | |
| **Distance town hall^2** | 0.083** | 0.079** | | |
| | [0.031] | [0.032] | | |
| **Distance town hall*Distance health centre** | -0.140*** | -0.146*** | | |
| | [0.052] | [0.050] | | |
| **Distance town hall*Distance school centre** | 0.004 | 0.006 | | |
| | [0.337] | [0.354] | | |
| **Distance school town ave. (mins)** | -1.598** | -1.554** | | |
| | [0.601] | [0.628] | | |
| **Distance health centre town ave. (mins)** | -0.097 | -0.120 | | |
| | [0.186] | [0.179] | | |
| **Distance town hall town ave. (mins)** | 0.106 | 0.117 | | |
| | [0.134] | [0.132] | | |
| **Hospital** | 0.146*** | 0.144*** | | |
| | [0.046] | [0.048] | | |
| **Pipe water (prop. town)** | 0.346 | 0.387 | -0.039 | -0.012 |
| | [0.231] | [0.236] | [0.079] | [0.088] |
| **Sewage (prop. town)** | 0.121 | 0.121 | 0.068 | 0.107 |
| | [0.093] | [0.094] | [0.124] | [0.138] |
| **Health insurance (prop. in town)** | -1.422* | -1.365* | | |
| | [0.737] | [0.761] | | |
| **Health insurance^2 (prop. in town)** | 0.895 | 0.876 | | |
| | [0.572] | [0.591] | | |
| **Num. children** | -0.046 | -0.032 | 0.012* | 0.015** |
| | [0.075] | [0.076] | [0.007] | [0.007] |
| **Altitude** | 0.288** | 0.226* | 0.026 | 0.022 |
| | [0.118] | [0.126] | [0.115] | [0.123] |
| **Altitude^2** | -0.148*** | -0.128*** | -0.097** | -0.099** |
| | [0.039] | [0.041] | [0.045] | [0.048] |
| **Wage female urban** | 0.066 | 0.062 | | |
| | [0.101] | [0.095] | | |
| **Wage female rural** | -0.008 | -0.012 | | |
| | [0.086] | [0.083] | | |
| **Price index** | -0.004 | 0.077 | | |
| | [0.244] | [0.228] | | |
| **Sisben level 2** | | | 0.164*** | 0.169*** |
| | | | [0.045] | [0.044] |
| **Sisben level 3** | | | 0.313*** | 0.335*** |
| | | | [0.049] | [0.046] |
| **Observations** | 5,681 | 5,681 | 6,170 | 6,179 |

***Notes*** . Each cell reports the coefficient of a regression of child's height (z-score) on either Exposure or Attendance and additional control variables, using Two Stage Least Squares with instruments being fitted values computed using a non-linear model (Probit or Tobit) estimated over distance to HC, capacity, and HC fee (FeA) or only capacity (ENDS) and all the other control variables. Standard error clustered at the town level in brackets, *** p<0.01, ** p<0.05, * p<0.1.

# Appendix B:
# Additional Figures

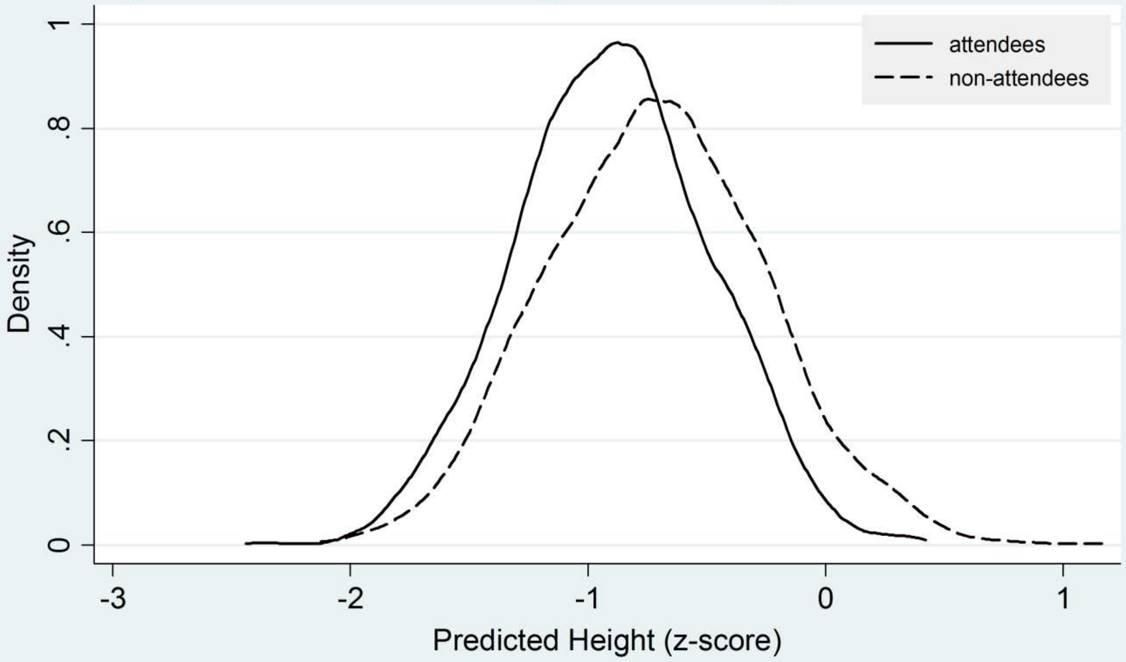
Figure A1: Predicted Height according to HC attendance




KS Test p-value: 0.000

Dataset: FeA

Figure A2: Predicted Height according to HC attendance




KS Test p-value: 0.000

Dataset: ENDS

# Appendix C

## Effect of measurement error in the distance to the HC nursery

In this appendix we discuss the bias introduced by a particular type of measurement error in the distance between the household and the HC nursery. Household whose children do NOT attend a HC nursery were asked to report the distance to the closest HC nursery rather than the distance to the HC nursery that they would go if they attended a HC nursery. This means that the distance collected for the non-attendees is too small. As we will see, the bias depends on the type of selection (positive or negative) into the program. If there is negative selection as in our case (non-attendees are taller), the implication of the measurement error is to underestimate the effect of attending a HC nursery on children's height. To see this, consider that the true Data Generating Process (DGP) is given by:

$$H_i = \vartheta + \alpha A_i + \varepsilon_i,$$

where the variables are defined as in section 3 of the paper (but for simplicity, we omit the other covariates). If $D_i$ is used as instrument for $A_i$ then the IV estimator of $\alpha$ is given by:

$$\widehat{\alpha}_{iv} = \frac{\sum_{i=1}^{N} \left(H_i - \overline{H}\right)\left(D_i - \overline{D}\right)}{\sum_{i=1}^{N} \left(A_i - \overline{A}\right)\left(D_i - \overline{D}\right)}, \tag{1}$$

where $\overline{H}, \overline{A}, \overline{D}$ are the averages of $H_i, A_i$,and $D_i$ respectively. Under standard conditoins, the IV estimator, $\widehat{\alpha}_{iv}$, converges to:

$$\widehat{\alpha}_{iv} \to \frac{Cov(H_i, D_i)}{Cov(A_i, D_i)} = \alpha. \tag{2}$$

Because of the measurement error in distance, we can only use a contaminated instrument, $D_i^e$, whose DGP is assumed to be::

$$D_i^c = \begin{cases} D_i - u_i & \text{, if } A_i = 0 \\ D_i & \text{, if } A_i = 1 \end{cases},$$

where $u_i$ is a non-negative random variable following an unknown distribution but independent of $H_i$. Using the contaminated instrument, $D_i^c$, the feasible IV estimator is:

$$\widehat{\alpha}_{iv}^c = \frac{\sum_{i=1}^{N} \left(H_i - \overline{H}\right)\left(D_i^c - \overline{D^c}\right)}{\sum_{i=1}^{N} \left(A_i - \overline{A}\right)\left(D_i^c - \overline{D^c}\right)}.$$

By decomposing the sum between those with $A_i = 0$ or 1, we obtain that

$$\widehat{\alpha}_{iv}^{c} = \frac{\sum_{i\in\{i:A_i=1\}} \left(H_i - \overline{H}\right)\left(D_i - \overline{D} - (N_0/N)\overline{u}\right) + \sum_{i\in\{i:A_i=0\}} \left(H_i - \overline{H}\right)\left(D_i - u_i - \overline{D} - (N_0/N)\overline{u}\right)}{\sum_{i\in\{i:A_i=1\}} \left(A_i - \overline{A}\right)\left(D_i - \overline{D} - (N_0/N)\overline{u}\right) + \sum_{i\in\{i:A_i=0\}} \left(A_i - \overline{A}\right)\left(D_i - u_i - \overline{D} - (N_0/N)\overline{u}\right)},$$

where $N_0$ is the number of non-attendees, and $\overline{u} = (1/N_0) \sum_{i\in\{i:A_i=0\}} u_i$. After re-arranging terms and taking into account that $\sum \left(H_i - \overline{H}\right)(N_0/N)\overline{u} = 0$ and $\sum \left(A_i - \overline{A}\right)(N_0/N)\overline{u} = 0$, we obtain:

$$\widehat{\alpha}_{iv}^{c} = \frac{\sum \left(H_i - \overline{H}\right)\left(D_i - \overline{D}\right) - \sum_{i\in\{i:A_i=0\}} \left(H_i - \overline{H}\right) u_i}{\sum \left(A_i - \overline{A}\right)\left(D_i - \overline{D}\right) - \sum_{i\in\{i:A_i=0\}} \left(A_i - \overline{A}\right) u_i}.$$

After substituting $A_i = 0$ in the second term of the denominator, we are left with:

$$\widehat{\alpha}_{iv}^{c} = \frac{\sum \left(H_i - \overline{H}\right)\left(D_i - \overline{D}\right) - \sum_{i\in\{i:A_i=0\}} \left(H_i - \overline{H}\right) u_i}{\sum \left(A_i - \overline{A}\right)\left(D_i - \overline{D}\right) + \overline{A}\overline{u}N_0}. \tag{3}$$

In order to obtain the probability limit of (3), we add and substract $\overline{H}_0 = (1/N_0) \sum_{i\in\{i:A_i=0\}} H_i$ in the first parenthesis of the numerator, and we add and substract $\overline{u}$ to $u_i$ also in the numerator. Taking into account that:

$$\left(\frac{1}{N_0}\right) \sum_{i\in\{i:A_i=0\}} \left(H_i - \overline{H}_0\right)\overline{u} = 0,$$

and that $\left(\frac{1}{N_0}\right) \sum_{i\in\{i:A_i=0\}} \left(H_i - \overline{H}_0\right)(u_i - \overline{u})$ converges to zero[1] then:

$$\widehat{\alpha}_{iv}^{c} \to \frac{Cov(H_i, D_i) - (N_0/N)\, E[u_i|A_i = 0]\, (E[H_i|A_i = 0] - E[H_i])}{Cov(A_i, D_i) + (N_0/N)\, E[A_i]E[u_i|A_i = 0]}. \tag{4}$$

Note that the denominator of (4) is larger than the denominator of (2) because $(N_0/N)\, E[A_i]E[u_i|A_i = 0] > 0$.With regard to the numerator, we assume $E[H_i|A_i = 0] - E[H_i] > 0$ because we have found evidence of negative selection into the program, namely the non-attendees are taller.than attendees [2] Hence,

[1] The measurement error is uncorrelated with height because the measurement error does not drive any choices.

[2] Note that the OLS estimate in Table 6.1 is negative, and taht Figures A1 and A2 in the appendix also show evidence of netative selection.

the numerator of (4) is smaller than the numerator of (2). Taking this into account, it is clear that $\widehat{\alpha}_{iv}^{c}$ converges to a number smaller than $\alpha$.In other words, the IV with the contaminated instrument underestimates the true effect of the HC program.