Community nurseries and the nutritional status of poor children. Evidence from Colombia*

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Abstract

In this paper, we use two different datasets and three different instruments to estimate the impact of a long-established pre-school nursery program (*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 program availability (capacity of the program 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 program 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, Program Evaluation, Child Nutrition

JEL: C21, I12, I38

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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 cognitive and educational performance (Behrman 1996, Strauss and Thomas 1998, Glewwe *et al.* 2001, Alderman *et al.* 2001, Maluccio *et al.* 2006, Walker *et al.* 2007) as well as low productivity later on in life (Strauss and Thomas 1998, Schultz 2005, Hoddinott *et al.* 2008). Therefore, given the importance of early years status for subsequent development, 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 centres distributed across all municipalities in the country and about one million children, from the poorest Colombian families, attend a HC centre. 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, Mexico, Peru and other countries. Their attractiveness arises from the fact that these programs 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, as has been the case for recent conditional cash transfer and micronutrient supplementation programs.

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 number of places available in a given municipality relative to the eligible population. 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 Accion*, from now on FeA sample) and includes small rural localities. The second is the 2005 *Encuesta Nacional de Demografía y Salud* (ENDS sample), which is nationally representative, and hence includes larger localities. 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 centre) and attendance (whether or not the child is attending a HC centre at the time of the survey). We find that the derivative of child's height with respect to exposure is 95% of one standard deviation in the FeA sample and 123% for the ENDS sample. We find that attendance increases child's height by 50% of a standard deviation in the FeA sample (80% 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 1.6% (2%) taller in the FeA (ENDS) sample or 38% (49%) of one standard deviation of age-adjusted height. 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 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 program 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 locality and local availability of the places in community nurseries) but obtain extremely similar impacts with each of them which favourably speaks of the external validity of our estimates.[1] An attractive feature of our instruments is that they constitute policy variables that policy makers could manipulate to modify coverage of the program. 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 closest HC, we present evidence that households do not move to be closer to HC nurseries, and that our covariates adequately control for location effects. Third, while we use different instruments on different data sets (partly because of the nature of the data and partly because of the different contexts from which the different data are drawn), we obtain very similar results. Fourth, using the same instruments, we run so-called placebo regressions on variables that should not be affected by the program (such as birth weight) 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.

[1] Though fee and availability are correlated with each other, distance is basically uncorrelated with both fee and availability.

The HC programme was established 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 program, 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 program 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 program 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 program very similar to HC implemented in Bolivia. Ruel *et al* (2006) and Cueto *et al.* (2009) study another two nursery programs 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). 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 programs can have impacts on later educational 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

[2] See Banerjee *et al* (2008) for an example of a program that has positive impact in the first six months but the impact disappears after 18 months due to collusion between the authorities and the target of the program.

3 Experiments would be useful to study how the program 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.

estimate identifies the overall effect of the program, 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 localities 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. Each family would pay a small monthly fee, which would be used to complement the *madre comunitaria's* salary. The fee is negotiated between the parent's association and the *madre comunitaria* and is approved by the local office of the ICBF.

The ICBF provides the funds to purchase the food, which is kept at the *madre comunitaria's* fridge. Children are fed three times daily: lunch and two snacks. The children would also be given a nutritional beverage called *bienestarina.* According to ICBF, the food received by the children (including the beverage) would provide them with 70% of the recommended daily amount of calories.

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 do find SISBEN 3 children in one of our data).

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 15 and 20 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 isolated areas, an apparently common problem is the difficulty to set up a new *HC* because the ICBF does not start a new centre 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 the availability of the community nursery on the nutritional status of eligible children who choose to attend them. 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 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 an *HC*. A difficulty arises from the fact that in the data we use some of the determinants of children nutritional status (that are likely to be affected by participation to HC) are not observed. 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 an *HC,* while a child from a not too poor household might be experiencing a worsening of her environment if she attends an *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:

$$H = H(A,F,L,z,\varepsilon) \tag{2}$$

$$X = Y - pF - qA + w(L - DA) \tag{3}$$

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 attendance to the HC, p the price of food, q is the fee for attendance to a HC, 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 ε 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:

$$H_i = \vartheta + \alpha_i A_i + \beta F_i + \gamma L_i + \theta z_i + \varepsilon_i \tag{4}$$

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 ε). 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 ε. 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 α_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 α 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 α, 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 program might have. The problem, which is particularly serious when selection into the program 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 places available in HC to the total number of children aged 2-6[4] from SISBEN 1 and 2 families in the locality (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 locality (as indicators of cost of participation both in terms of time and money).[5] We obtain the number of places available in each municipality directly from the ICBF administrative data and consequently this instrument can be used with both datasets. ICBF does not collect information on the fee paid by children in each locality 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 program 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] We chose age 2 to 6 because below age 2 only less than 20% of children enrol in HC. However, our results are not sensitive to choice of the age range in the construction of the HC capacity variable.

[5] 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).

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 program, called Familias en Acción (FeA from now on) has an education, and a health component and is directed to the poorest families (in the SISBEN 1 category) living in the municipalities targeted by the program. 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.[6] 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.[7]

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 (towns to serve as controls in the evaluation of FeA). They were chosen as the most similar to the treatment towns according to population size, population living in the urban part of the municipality, 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 municipalities 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 municipalities that were part of the control group in the first and second wave. So, only 52 municipalities 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 centre, we know the distance from the household to the HC centre. If the child is not

[6] 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.

[7] 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

attending a HC centre, we know the distance to the nearest HC. For each child that has ever attended a HC centre, 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 centre and municipality 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 the health centre and school was collected for the whole sample in the second and third wave only. For the first wave of data, we use distance to the health centre, and school 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, 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 centre, nor the distance from the household the HC nursery.[8] Some other variables, such as distance to other facilities (school, health centre, town hall) and some municipality 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 municipality 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 75^{th}% percentile had less than 30k inhabitants. Only one town is larger than 100k, and none of them are capital of departments. These localities are eminently rural although 52% of the population live in the main part of the town rather than dispersed 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.

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

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.[9] 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 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/Centre 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 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 -1.25 in the FeA sample and -0.77 in the ENDS sample. Moreover, 23.7% 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.[10] 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 at age 3 and 4 for the FeA and ENDS sample respectively. They are particularly low for very young children. Second, either the

[9] 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.

[10] The percentage of children acutely malnourished –their weight is too low for their height- is only 1.2% and 1.5% in the rural and urban sample, respectively. The percentage of obese children is 1.9% and 0.6%..

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* centre seems to be an important reason in the rural sample, but not in the ENDS sample

{Table 4.3}

In Table 4.4, we report the mean and three percentiles (25^{th}, 50^{th} and 75^{th}) 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 centres and live in municipalities with lower fees and/or more capacity. 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. We start this section providing the results on the first stage regressions, and we devote the rest of the section to justify our identification assumption. In section 7, we provide more evidence to support our empirical strategy and our identifying assumptions.

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 locality, and the availability of HC places relative to eligible children in the locality. The results of the first stage regression are in table 5.1. Note that, for each instrumental variable, we have included both linear and quadratic terms. Note also 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. Each regression includes a set of covariates, including the number of children 2-6 in the locality, the distances to health centre, school and town hall, mother's and head's ages and education levels and mother's height, as well as a variety of municipality level variables, which are controlled for in the second step regression. The complete specifications are reported in the Appendix.

In the FeA sample, the three instruments are highly correlated with *exposure* and with the expected signs. The F-statistic for the joint significance of all the instruments is 47.87, The F-statistics for each set are also high (27.7 for capacity, 17.75 for distance, and 11.77 for the fee). For attendance, the joint F-stat in the FeA sample is 14.7 which is larger than 10, the value usually taken as evidence of a weak instrument problem (Staiger and Stock, 1997). However, most of explanatory power is given by distance, with capacity and fee having F-statistics around 4 (this is partially because of the collinearity between fee and capacity, the F-statistic of fee and capacity considered jointly is 8.10). This 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 (see Table 4.3). In the ENDS sample, we can only use 'capacity' as instrument. The F-statistic of capacity in the *exposure* regression is 20.49 and 9.95 in the *attendance* regression. In general, our instruments are strongly correlated with *exposure* and we can rule out a weak instrument problem with this treatment indicator. However, we must be careful in interpreting the results associated with *attendance* in specifications in which we do not use distance as instruments.

The First stage regressions show some other interesting results. In particular, the results indicate that the poorest individual are more likely to participate in the HC program (children are less likely to participate if the mother has finished secondary education in the FeA sample, or if the family is SISBEN 3 and the child leaves in a locality with a large fraction of the houses being equipped with sewage in the ENDS sample). See Table A1 in the Appendix for details.

{Table 5.1}

5.2 Do households move to be closer to a HC centre?

Distance from the household to the nearest HC centre would be correlated with the error term of equation (7) if households who care about their children's nutrition or need more help locate closer to a *HC* centre. However, given the evolution of the *HC* programme, we do not think this constitute a problem. First, conversations with programme officials indicated that, especially in isolated rural areas, which make up a substantial proportion of our *FeA* sample, there might be severe supply restrictions induced by the need of a minimum number of children for ICBF to register a new *HC*. Moreover, after the first few years of the program, the turnover of *madres comunitarias,* induced by a variety of factors, contributed to substantially weaken the link between the original parent association and the location of the *HC* nurseries. It seems that many of the current clients of *HC* are households that move to a given neighborhood and access an existing *HC*. Second, we can provide evidence that households do not move with the purpose to be closer to a HC. Those households that moved location between two consecutive waves of the *FeA survey* but were found and interviewed were asked the reason for changing address. Although 'moving closer to a HC' was explicitly listed as a possible reason to move, only one of the 596 households that moved chose it as an answer.[11] Moreover, comparing the distance from the nearest HC for the movers and those who did not move, (which is done both conditionally on the distance to the nearest school and health centre and unconditionally, see table A.2 in the appendix), we do not find any statistically significant difference.

Finally, among the households who moved, we compare those who have children less than 7 to those who do not, as the latter are not eligible to participate. Once again the distance to the nearest HC is

[11] Responses to the reasons to move are "to find a better equipped house" (32%), "for work related reasons" (14%), to be closer to a relative (8%), to be closer to a school (3%), violence related (2.68%), to be closer to the town centre (0.5%), and to be closer to a HC centre (0.17%), and other reasons (39%).

not statistically different for the two groups.[12] All these pieces of evidence indicate that households do not seem to move to be closer to a HC centre, which could be partly explained because of the high turnover of *madres comunitarias* that we described in section 2.

5.3 Relation between the instrument and other covariates

Our estimates of the effect of participating in HC would be inconsistent if the instruments are correlated with unobserved determinants of child's height. While we cannot compute the correlation between our instrument and unobserved variables, we can try to assess how realistic our assumption is by analyzing the relation between the instrumental variables and observed determinants of child's height. If we find that our excluded variables are correlated with many observed variables, it will be highly unlikely that it will not be correlated with unobserved variables. More importantly, this exercise can 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. This type of argument has recently been proposed by Altonji, Todd and Taber (2005).

HC nurseries tend to be located close to health centers and schools. While we verified in the subsection 5.2 that households do not move to be closer to a HC nursery (probably due to high turnover of nurseries), they might live in areas closer to the town centre, schools, and health centers. Typically, richer households will locate closer to these amenities. As HC nurseries also tend to be located closer to these amenities, we would expect that households that are more educated live closer to HC nurseries. This is what columns 1 and 2 of Table 5.2 show. For instance, a mother that finished secondary education lives in average 8 minutes closer to a HC nursery than a mother that did not finish primary education. While this is obviously a potential problem, we have to stress that our identification assumption states that the instrument we use is uncorrelated with the unobserved components of children nutritional status, *conditional* on the other variables we control for. For our strategy, then, it is important to condition on location variables (whether the household lives in the centre of town, distance to health centre, school, and town hall), for which, fortunately, we have information in our surveys. When we do this, in columns 3 and 4 of Table 5.2, the correlation between education and distance to the nearest HC nursery shrinks dramatically to zero. Conditional on the distance to other amenities, mothers that finished primary education become undistinguishable in terms of distance to the nearest HC nursery from mothers that finished secondary education. The

[12] Non-eligibles are on average 1.9 minutes (se=3.07) closer to a HC nursery.

only statistically significant difference is that a mother that finished primary school lives 2 minutes closer to a HC nursery than a mother that did not finish primary education (and it is statistically different only for current distance but not for wave 1 distance). Other variables such as mother's and head's age or birth order are uncorrelated with distance. There are some municipality level variables that are correlated with distance but the size of the coefficient also shrinks when we condition on location variables (especially wages). In particular, households living closer to a HC tend to live in towns with higher proportion of households with piped water and health insurance.

{Table 5.2}

The regression of the median fee over the other covariates does not show much association with other variables, except a negative correlation with the percentage of households with piped water. From the regressions with capacity, we infer that poorer localities have higher capacity. In the FeA sample, capacity is negatively associated with wages, while in the ENDS sample is negatively associated with SISBEN 3 (which represents richer households than SISBEN 1 and 2).

5.4 How are the instruments correlated between themselves?

Having results obtained with different instrument sets would not be particularly valuable if these instruments are highly correlated between themselves. Figures 1, 2, and 3 show the graphs of one instrument against another. There is a strong clear negative relationship between the median fee paid in the municipality computed using the FeA survey and the capacity variable computed using ICBF data (see Figure 1). Figure 2 and Figure 3 do not show any clear association between either fee and distance or distance and capacity. The correlation between distance and fee (distance and capacity) is 0.13 (-0.18) and its P-value is 0.36 (0.18). Overall, though there is a strong and clear association between fee and capacity, the association between fee and distance and capacity and distance seem rather weak.

{Figure 1, 2, and 3}

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 (both top and bottom panel), we present our IV estimates of equation (7), using as instruments the local availability of HC places (for both FeA and ENDS sample), the fee in the locality and the distance from the household to the nearest HC nursery (FeA sample). The dependent variable of equation (1) 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 community level. We report the full set of results in Table A.3, A.4 and A.5 in the Appendix. Among our covariates, we include the distance from the household to the nearest school, nearest health centre, and the town hall. In section 5.3, we showed how these variables were important to drastically decrease and almost eliminate the correlation between distance to the nearest HC nursery and some observed household characteristics, such as education. We also include the number of children aged 2-6 in the locality (the denominator of the capacity instrument) to ensure that we only exploit the variability related to the number of HC available slots in the locality 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 municipality 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.[13] These results show that the effect on children's height of HC participation is positive and, in most specifications, statistically different from zero.

13 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 column of Table 6.1 (see Table A.1 in the appendix for the estimated parameters of the models used to compute the prediction). 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,

{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 94.5% of one standard deviation of height taller; and a child that currently attends a HC will be, on average, 44.8% of one standard deviation of height taller.[14] 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 extremely similar to those in column 1. If the returns to program 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 very similar results with three different instruments (although we have shown in Section 5.4 that they are not strongly correlated among themselves). This evidence reinforces the external validity of our estimates. Of course, our results could arise because the returns to program participation are not heterogeneous, or because individuals do not select into the program according to their unobserved returns (Imbens and Angrist 1994, Heckman 1997, Heckman *et al* 2006).[15]

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 20% higher in the ENDS than in the FeA sample, and almost twice as large in the case of attendance

and the standard errors do not need to be corrected (see Wooldridge 2001, pg. 623). 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.

14 The average number of months attending a HC centre is 20 among those children currently attending. The average exposure among those currently attending is 0.42.

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

(although note that, given the size of the standard errors, the confidence intervals overlap).[16] Interestingly, we obtain very sizeable effects of the programme in both datasets.

The bottom panel of Table 6.1 shows IV estimates using standard Two Stage Least Squares. 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 the non-linear IV estimates).[17] This is why we favour the top panel estimates over the bottom panel ones.

Under the assumption of homogenous treatment effects, the Hansen J-statistic can be used to test the overidentifying restrictions in the FeA sample. The Hansen J-statistic is 2.72 (P=0.9) for exposure, and 5.34 (P=0.62) for attendance, and consequently we cannot reject that the exclusion restrictions are valid. This is hardly surprising because the estimates obtained with each instrument separately are very similar. In section 7, we assess the robustness of our results to violations of the exclusion restriction assumptions.

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 program 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 program guidelines explicitly say that children must suffer from "economic vulnerability" in order to be eligible.[18] The first stage equations also confirmed that poorer children are more likely to participate (see section 5.1).

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

[16] 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).

[17] 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.

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

program 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 localities. Table 6.2 (full results are in table A.6 to A.9 in the Appendix) shows the estimates for selected quantiles.

{Table 6.2}

In the ENDS sample, the results show much higher effects at lower quantiles. The point estimate of the effect of the program at the 25th percentile is more than three times as large as the estimate of the impact at the 75th percentile. This almost monotonic pattern indicates that, in the absence of the program, 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.

6.3 Discussion

Our OLS regressions show that participants are slightly shorter than non-participants, but our IV results show sizeable effects of the program. Clearly, the program is allowing the poorest children (that self-select into the program) 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 program.

According to our estimates, the program show sizeable effects: a 60 month old child that has spent 24 months in a HC nursery would be 1.6% taller in the FeA sample (2% in the ENDS sample).[19] Thomas and Strauss (1997) estimate that 1% increase in height leads to 2.4% increase in adult male wages in Brazil.[20] Our estimates are also plausible from the biological point of view. In terms of z-scores, these gains in height correspond to 0.38 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

[19] 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.

[20] 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.

concluded that provision of complementary food in food-insecure populations resulted in average increase of 0.41 height-for-age z-scores (Bhutta el al 2008).

An interesting question is how our estimates compare to results obtained for similar programs. 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* program in Guatemala studied by Ruel *et al*, (2006) and the *Wawa Wasi* programme in Peru, 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 program. 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 application.

In the case of *Hogares Comunitarios* in Guatemala city, Ruel *et al* (2006) used a case-control methodology to estimate the effect of the program on 250 beneficiaries. They report that the program 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* program in Peru', 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 exercise 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 (ii) conducting a sensitivity analysis along the lines of Conley, Hansen and Rossi (2008).

7.1 Falsification exercise using birth weight

Birth weight 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 as an outcome a variable. If we were to find that our instrumental variable procedure indicates an effect of the programme on birth weight, one would suspect that the instruments we are using are correlated with unobservable determinants of nutritional status and are therefore invalid. It is likely that the unobserved determinants of height per age are shared with the determinants of birth weight. Therefore a correlation between these factors with the instruments we use would induce a similar bias both in the specification for height and that for birth weight.

Table 7.1 replicates Table 6.1 but with birth weight as dependent variable (full results are found in table A.10, A.11 and A.12 in the appendix). None of the IV estimates in the FeA sample are statistically different from zero. Perhaps more importantly, the sign of the point estimate varies across instruments and our definition of 'treatment' (exposure and attendance). In particular, the point estimates in column 2 (that uses capacity as an instrument) are negative for exposure and positive for attendance. The point estimates in column 3 (that uses fee as instrument) are mostly negative, and the point estimates in column 4 (that uses distance as instrument) are all positive. 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. Tables 6.1 and 7.1 taken together seem to indicate little scope for bias.

{Table 7.1}

Column 5 of Table 7.1 reports the results for birth weight using the ENDS sample and the capacity instrument. All the point estimates are negative and even statistically significant at 10% when standard two stages least squares is used (bottom panel). This would indicate that, if anything, the results reported for the ENDS would be biased downward, that is, that the results in Table 6.1 constitute a lower bound. This corroborates our results in section 5.3 that poorer households were associated with higher capacity levels.

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 (2008) which consists on estimating α 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 (2008) show how to obtain confidence intervals for α 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 α.

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*.[21] Moreover, we only analyze the effect of relaxing the orthogonality restriction for distance and capacity, for which, in Section 5.3, there are intuitive support restrictions. As we do not have a clear intuition of the sign of the possible correlation between fees and unobserved components of height we do not scrutinize this instrument. Given that the results do not vary much with the instrument we use, this is not particularly worrying.

In subsection 5.3, we showed that conditioning on the distance to other facilities (schools, health centres, and town hall) was important to reduce the correlation between distance 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 centre and the error term. In particular, one might worry that *g* is negative in equation (8).

[21] This is the reason why our results in Table 6.1 differ slightly from those shown in the Figures 4 to 9 when g=0.

Figure 4 and 5 shows the confidence interval for *a* assuming that *g* lies in the interval [*k*,0] for values of *k* ranging from 0 to -0.20.[22] The figure also shows the point estimate for α if *g*=*k*/*2*. The point estimate of α decrease slowly as *g* decreases. The lower bound of the 90% confidence interval for α crosses zero if *k* is smaller than -0.135 for attendance and -0.07 for exposure.

{Figures 4 and 5}

Clearly, any assessment on whether our results on the HC program are robust or not depends on whether or not these values for *k* (-0.135 and -0.07) are "small" or "large". To assess this, we run a reduced form regression: child's height over distance, capacity, fee and all the covariates (but exclude their squares). In this reduced form regression, the coefficient on distance to the health centre is -0.05 (standard error = 0.077) and the coefficient on distance to the town hall is -0.07 (standard error = 0.057).[23] It is reasonable to think that distance to health centre and town hall will be more correlated to unobserved determinants of nutritional status than distance to the nearest HC nursery (especially because there are many more HC nurseries than health centres, and it is not uncommon that HC nurseries close because *Madre Comunitarias* do not wish to continue). 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 (-0.14, standard error = 0.08) is about three times larger than the coefficient of distance to the health centre (-0.05, standard error= 0.077). Even if one believed that part of the partial correlation between height and distance to the HC nursery is due to unobserved heterogeneity due to location, we believe that unobserved heterogeneity associated with location should be stronger for health centres, as there are far fewer of those than HC nurseries, and their location is much more stable. As a result, even if one took the extreme assumption that the correlation between height and distance to the health centre 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 health centre 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.

[22] We thank Conley et al (2008) 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.
[23] We do not use distance to school because its coefficient is positive.

Figures 6 and 7 (8 and 9) scrutinize the robustness of our conclusions for the capacity instrument in the FeA (ENDS) dataset. We concentrate on negative values of k because subsection 5.3 concluded that capacity was positively correlated with observed indicators of poverty (the results in Table 7.1 - birth weight regression- also seem to support this). Hence, we will expect children living in localities with higher capacity to be shorter. Figures 6 to 9 show that, if k is negative, our results in Table 6.1 underestimate the effect of the HC program as the point estimate of α increases as k becomes more negative.

{Figures 6, 7, 8, and 9}

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 children relatively less poor. Similar programs exist in Bolivia, Peru, Guatemala and México. Despite their importance, little is known about the effects of these types of programs.

Our focus is on how program 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 program. We also find that program 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 program, such as cost variables: the distance from the household residence to the nearest HC, the ratio of places in a municipality to the number of eligible children and the average level of fees paid in a municipality. Unlike the OLS results, the IV estimates of program participation on child's height are positive and show sizeable effects of program participation. The effects are remarkably similar across three different instruments (distance from the household to the nearest HC nursery, the median fee in the municipality, and the capacity of the HC programme in the municipality). If we consider that results from different instruments are different Local Average Treatment Effects, our results indicate that either the effect of the program is homogenous or households do not self-select into the program 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) households do not move to be closer to a HC, probably because of the high turnover of HC nurseries, (2) we show that controlling for distance to health centres, schools and town halls (as we do in our empirical specifications) dramatically shrinks the partial correlation between distance to the HC centre and household variables that are related to economic well being, (3) distance to the HC centre is uncorrelated with the other two instruments (fee and capacity) which strengthens our case, given that we obtain very similar results with any of the three instruments, (4) when we perform the same exercise on birth weight, which should not be affected by the program, we do not obtain any significant effect (5) capacity seems to be higher in poorer towns which implies that our IV estimates that use capacity as instruments are lower bound estimates, (6) we would obtain positive and statistically significant effects of the program on child's height even if we allow for moderate direct effects on child's height of distance to the nearest HC, and (7) our effects are biologically feasible and lie well within experimental estimates of nutrition interventions with complementary food in food-insecure populations.

Programs evolve with time: staff motivation, accountability; monitoring, guidelines, etc are likely to be different at the start of a program than in the longer term after it has evolved. Contrary to recent evaluations of conditional cash transfer programs, this paper estimates the effect of a program that was established long-ago. 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 program, 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 program. 0.8 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 program. The program 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 program, but is still far from satisfactory. Many of the children attending HC are stills 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 sample Mean	FeA sample SD	ENDS Mean	ENDS SD
age_head	Household head's age in years divided by 100	0.39	0.11	0.42	0.15
age_mot	Mother's age in years divided by 100	0.32	0.07	0.28	0.07
age_m	Child age in months	48.9	23.2	35..53	21.3
altitude	Altitude in thousand meters	0.45	0.68	0.62	0.81
asis_hc	1 if the child is attending a HC centre, 0 otherwise	0.24	0.43	0.32	0.47
capacity	Number of places in HC centres in the town divided by number of children 2 to 6 years old	0.31	0.18	0.25	0.17
numchildren	Number of children 2 to 6 years old in the town, divided by 10000	0.26	0.26	3.23	6.6
edu_m_a	1 if mother did not complete primary education, 0 otherwise	0.58	0.49	0.03	0.16
edu_m_b	1 if mother completed primary education but did not complete secondary education, 0 otherwise	0.35	0.48	0.35	0.48
edu_m_c	1 if mother completed secondary education, 0 otherwise	0.06	0.25	0.61	0.49
edu_h_a	1 if household head did not complete primary education, 0 otherwise	0.65	0.47	0.1	0.29
edu_h_b	1 if household head completed primary education but did not complete secondary education, 0 otherwise	0.29	0.45	0.55	0.49
edu_h_c	1 if household head completed secondary education, 0 otherwise	0.05	0.22	0.35	0.48
exposure	Number of months that the child has attended a HC centre divided by the age of the child in months	0.18	0.24	0.1	0.19
female	1 if child is female, 0 if child is male	0.49	0.5	0.49	0.5
haz	Child’s height. Unit: z-scores	-1.25	1.01	-0.77	0.98
birthweight	Child's weight at birth:	3.37	0.66	3.29	0.54
hc_fee	Median fee to attend a HC nursery in the municipality. Colombian pesos divided by 1000.	3.82	3.18	.	.
height_mot	Mother’s height in metres	1.54	0.06	1.55	0.56
hosp	1 if there is a hospital in the town, 0 otherwise	0.71	0.48	.	.
insur_mun	Proportion of children with formal health insurance in the municipality	0.62	0.22	.	.
ln_age_head	Logarithm of household head's age in years divided by 100	-0.96	0.26	-0.15	0.35
ln_age_mot	Logarithm of mother's age in years divided by 100	-1.17	0.22	-0.97	0.24
ln_order	Logarithm of order of kid in the household	1.15	0.53	0.71	0.62
order	Order of kid in the household	3.6	1.74	2.47	1.66
pipe	Percentage of households with piped water in the municipality	0.85	0.14	0.89	0.31
price_index	Food price index	0.92	0.13	.	.
rural	1 if household lives in the main part of the town, 0 otherwise	0.52	0.5	0	.
sewage	Percentage of households with sewage connection in the municipality	0.44	0.37	0.75	0.43
time_hc	Distance (minutes divided by 100) to the nearest HC	0.21	0.32	.	.
time_hc_b	same as time_hc but in the first wave of data	0.23	0.33	.	.
time_alc	Distance in minutes to the town hall, divided by 100	0.51	0.64	.	.
time_hea	Distance in minutes to the nearest health care provider, divided by 100	0.41	0.55	.	.
time_sch	Distance in minutes to nearest school, divided by 100	0.14	0.15	.	.
time_alc_mun	Average of *time_alc* in the municipality	0.52	0.38	.	.
time_hea_mun	Average of *time_hea* in the municipality	0.3	0.27	.	.
time_sch_mun	Average of *time_sch* in the municipality	0.1	0.05	.	.
wage_fr	Rural female wage in pesos as indicated by the town major divided by 1000 in Colombian pesos (December 2003)	0.91	0.36	.	.
wage_fu	Urban female wage in pesos as indicated by the town major divided by 1000 in Colombian pesos (December 2003)	0.98	0.34	.	.

Statistics are restricted to estimation sample: 2345 children (Fea wave 1) and 6189 (ENDS)

Table 4.2- Percentage of children attending Hogares Comunitarios

Age	0	1	2	3	4	5	6
FeA							
Boys	4	16	44	44	34	20	8
Girls	3	20	39	46	36	16	7
ENDS							
Boys	2	17	38	48	49	45	38
Girls	2	15	39	46	49	42	40

Observations: 5717 (FeA), 9031 (ENDS)

Table 4.3-Reasons for not attending HC

	Age 0-1	Age 2-4	Age 5-6
FeA			
Available caregiver at home	63%	39%	16%
No HC facility or too far	16%	26%	13%
Cannot afford fee	4%	8%	3%
Does not like food	1%	4%	3%
Other	16%	23%	65%
ENDS			
Available caregiver at home	84%	79%	72%
No HC facility or too far	2%	3%	3%
Cannot afford fee	1%	3%	2%
Does not like food	1%	3%	3%
Other	2%	10%	20%

Observations: 4221 (FeA), 5988 (ENDS)

Table 4.4 Distribution of the instruments

	Entire Sample				Participants			
	Distance (mins.)	**Fee (Pesos)**	**Capacity**	**Capacity**	**Distance (mins)**	**Fee (pesos)**	**Capacity**	**Capacity**
25thPerc	5	1651	18%	15%	3	1000	21%	16%
Median	10	3000	27%	23%	5	3000	33%	25%
Mean	21	3821	31%	25%	10	3059	38%	27%
75thPerc	25	5254	37%	32%	15	4000	53%	33%
Survey	FeA	FeA	FeA	ENDS	FeA	FeA	FeA	ENDS

Observations - Entire sample: 5717 (FeA), 9031 (ENDS), Participants: 1391 (FeA), 3043 (ENDS)

Table 5.1 - First Stage Regressions

	1	2	3	4	5	6	7	8
		FeA				ENDS		
	Linear. Exposure	Linear. Attendance	Non Linear. Exposure	Non Linear. Attendance	Linear. Exposure	Linear. Attendance	Non Linear. Exposure	Non Linear. Attendance
HC capacity	-0.0575	0.217	0.254	1.714	0.321***	0.610***	0.814***	2.025***
	[0.147]	[0.228]	[0.354]	[1.121]	[0.0544]	[0.142]	[0.127]	[0.503]
HC capacity^2	0.377**	0.12	0.152	-0.649	-0.151***	-0.313***	-0.408***	-1.028***
	[0.171]	[0.270]	[0.425]	[1.355]	[0.0366]	[0.0954]	[0.0912]	[0.350]
Median HC fee	-0.0144***	-0.00835	-0.0234**	-0.0235				
	[0.00524]	[0.00905]	[0.0118]	[0.0396]				
Median HC fee^2	0.000423	-2.20E-05	0.000479	-0.000754				
	[0.000304]	[0.000527]	[0.000696]	[0.00231]				
Distance nearest HC_2002	-0.154***	-0.328***	-0.403***	-1.449***				
	[0.0338]	[0.0704]	[0.0911]	[0.426]				
Distance nearest HC_2002^2	0.0729***	0.133***	0.127*	0.304				
	[0.0170]	[0.0392]	[0.0660]	[0.345]				
Distance nearest HC_2003	-0.143***	-0.165**	-0.338***	-0.747*				
	[0.0438]	[0.0783]	[0.110]	[0.395]				
Distance nearest HC_2003^2	0.0556**	0.0719	0.0675	0.262				
	[0.0240]	[0.0434]	[0.0671]	[0.250]				
Observations	5719	5719	5719	5719	6170	6179	6170	6179
R-squared	0.256	0.204	0.323	0.226	0.169	0.221	0.264	2.219
F-test of instruments	F(8,51)=47.87	F(8,51)=14.71			F(2,219)=20.49	F(2,219)=9.95		
[p-value]	[0.0000]	[0.0000]			[0.0000]	[0.0001]		

Standard errors in parentheses are clustered at the town level. The regressions include a number of other controls including region and year (FeA) and state (ENDS) dummies. Complete results are reported in the Appendix.

Table 5.2 - Comparability regressions

	1	2	3	4	5	6	7
			FeA				ENDS
	Current distance	Wave 1 distance	Current distance	Wave 1 distance	Fee	Capacity	Capacity
	without location vbles	without location vbles					
Distance in minutes to the nearest health care provider, divided by 100			0.162***	0.154***	-0.0529	-0.000412	
			[0.0451]	[0.0379]	[0.0897]	[0.00842]	
Distance in minutes to nearest school, divided by 100			0.513***	0.449***	-0.0553	0.00607	
			[0.0505]	[0.0941]	[0.361]	[0.0208]	
Distance in minutes to the town hall, divided by 100			-0.0237	-0.0467*	0.13	0.00618	
			[0.0237]	[0.0238]	[0.0941]	[0.00743]	
Average of time_sch in the municipality			0.537**	0.613**	3.296	-1.103***	
			[0.228]	[0.239]	[5.048]	[0.337]	
Average of time_hea in the municipality			-0.0685	-0.0516	-1.061	0.0163	
			[0.0657]	[0.0559]	[1.302]	[0.0953]	
Average of time_alc in the municipality			0.0195	-0.0127	-1.351	0.137	
			[0.0442]	[0.0478]	[1.228]	[0.0887]	
D(mother completed primary edu)=1	-0.0496***	-0.0414**	-0.0228**	-0.0181	-0.0245	-0.00932	-0.018
	[0.0156]	[0.0201]	[0.0109]	[0.0171]	[0.171]	[0.00720]	[0.0133]
D(mother completed secondary edu)=1	-0.0874***	-0.0881***	-0.0188	-0.022	0.0817	-0.0062	-0.0124
	[0.0188]	[0.0233]	[0.0121]	[0.0175]	[0.230]	[0.0130]	[0.0130]
D(head completed primary edu)=1	-0.0407**	-0.0478**	-0.017	-0.0248	0.0117	-0.00255	-0.003
	[0.0177]	[0.0212]	[0.0145]	[0.0188]	[0.0973]	[0.00639]	[0.00664]
D(head completed secondary edu)=1	-0.0468**	-0.0638***	0.00118	-0.0145	0.0319	0.0115	0.00925
	[0.0182]	[0.0214]	[0.0127]	[0.0165]	[0.197]	[0.0155]	[0.00994]
Child age in months	-0.000600**	-0.000512**	-0.000535***	-0.000455**	-0.0015	-7.01E-05	-4.58E-05
	[0.000226]	[0.000208]	[0.000185]	[0.000171]	[0.00179]	[7.56e-05]	[7.56e-05]
D(child is female)=1	0.014	0.00986	0.014	0.00885	-0.0751	-0.00471	-0.0018
	[0.00996]	[0.0122]	[0.00854]	[0.0111]	[0.0559]	[0.00334]	[0.00277]
Log of order of kid in the household	0.0107	0.00964	-0.00633	-0.00599	-0.15	-0.00142	-0.0101**
	[0.0140]	[0.0177]	[0.0107]	[0.0160]	[0.130]	[0.00808]	[0.00425]
Mother's height in metres	0.122	0.0225	0.0968	0.00213	-0.0241	0.0171	-0.0353
	[0.129]	[0.179]	[0.0904]	[0.144]	[1.064]	[0.0534]	[0.0281]
Log age of household head	0.0477	0.0501	0.0252	0.0295	-0.485**	0.00835	0.01
	[0.0286]	[0.0350]	[0.0209]	[0.0304]	[0.205]	[0.0108]	[0.00847]

Log age of mother	-0.0219	-0.0318	0.00641	-0.00925	0.118	-0.0286**	0.0370**
	[0.0314]	[0.0385]	[0.0259]	[0.0338]	[0.309]	[0.0108]	[0.0158]
D(household in sisben 2)=1							-0.0139
							[0.00874]
D(household in sisben 3)=1							-0.0189*
							[0.0102]
Percentage of households with piped water in the municipality	0.159	0.218*	-0.0101	0.0505	-7.333***	0.0373	-0.0644
	[0.106]	[0.118]	[0.0641]	[0.0764]	[2.217]	[0.124]	[0.0548]
Percentage of households with sewage connection in the municipality	-0.101	-0.158**	-0.0452	-0.102**	0.865	-0.0396	-0.123
	[0.0659]	[0.0664]	[0.0412]	[0.0462]	[0.909]	[0.0588]	[0.0877]
Altitude (thousand meters)	0.00384	0.000853	-0.0142	-0.016	1.830*	-0.0297	0.013
	[0.0347]	[0.0312]	[0.0194]	[0.0194]	[1.029]	[0.0311]	[0.0290]
Number of children 2 to 6 years old in the town	0.0202	0.031	0.0148	0.0268	1.367	-0.126	0.0110***
	[0.0552]	[0.0530]	[0.0248]	[0.0288]	[1.561]	[0.0821]	[0.00327]
D(urban)=1			-0.113***	-0.148***	0.248	0.0192	
			[0.0196]	[0.0221]	[0.269]	[0.0210]	
D(hospital in the town)=1	0.000218	0.00399	0.00473	0.00866	0.669	0.00234	
	[0.0299]	[0.0297]	[0.0168]	[0.0178]	[0.628]	[0.0503]	
Percentage of children with health insurance	-0.178*	-0.175*	-0.120*	-0.127*	-0.846	0.164*	
	[0.0963]	[0.101]	[0.0637]	[0.0754]	[1.388]	[0.0976]	
Rural female wage	0.108**	0.107*	0.0286	0.0362	1.327	0.0936	
	[0.0524]	[0.0548]	[0.0533]	[0.0563]	[0.849]	[0.0579]	
Urban female wage	-0.150***	-0.196***	0.016	-0.0362	-0.196	-0.155**	
	[0.0492]	[0.0417]	[0.0565]	[0.0554]	[0.781]	[0.0682]	
Food price index	-0.336***	-0.314***	-0.406***	-0.349***	-0.412	0.549***	
	[0.120]	[0.108]	[0.0717]	[0.0637]	[2.776]	[0.145]	
Observations	5719	5719	5719	5719	5719	5719	6179
R-squared	0.183	0.19	0.391	0.351	0.608	0.587	0.602

Standard errors in parentheses are clustered at the town level. Regressions include region and year (FeA) and state (ENDS) dummies.

*** 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
			Top Panel: IV - non linear[+]			**Ordinary Least Squares**	
	All	*Capacity*	*Fee*	*Distance*	*Capacity*		
Exposure to HC	0.945**	0.977	1.016	1.090**	1.227***	-0.004	-0.0694
	[0.366]	[0.599]	[0.656]	[0.507]	[0.365]	[0.089]	[0.0553]
Attendance to HC	0.448**	0.450*	0.496**	0.504**	0.826***	-0.0913**	-0.0651***
	[0.190]	[0.240]	[0.229]	[0.229]	[0.192]	[0.0443]	[0.0250]
			Bottom Panel: IV - Linear[+]				
	All	*Capacity*	*Fee*	*Distance*	*Capacity*		
Exposure to HC	0.997***	1.002*	0.722	1.001	0.751		
	[0.349]	[0.528]	[0.588]	[0.619]	[0.692]		
Attendance to HC	0.611**	0.709	0.621	0.533	0.442		
	[0.251]	[0.445]	[0.445]	[0.445]	[0.381]		
Dataset	FeA	FeA	FeA	FeA	ENDS	FeA	ENDS

Standard errors in parentheses are clustered at the town level. The regressions include a number of other controls including region and year (FeA) and state (ENDS) dummies.

Complete results are reported in the Appendix. Sample sizes: FeA wave 1: 2345, FeA wave 2: 2395, FeA wave 3: 966.

ENDS sample size is 6170 (for exposure) and 6189 (for attendance). *** p<0.01, ** p<0.05, * p<0.1

[+] linear means instruments are entered in levels, non-linear means instrument set contains also squares.

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

	1	2	3	4	5
Percentile	**10**	**25**	**50**	**75**	**90**
FeA					
Exposure to HC	1.737**	1.884***	1.456**	0.510	0.051
	(0.864)	(0.714)	(0.614)	(0.597)	(0.751)
Attendance to HC	0.649**	0.620**	0.419*	0.125	0.033
	(0.290)	(0.260)	(0.231)	(0.221)	(0.277)
ENDS					
Exposure to HC	3.419***	3.015***	2.284**	1.667**	1.697
	(1.075)	(0.844)	(0.898)	(0.805)	(1.068)
Attendance to HC	1.331***	1.063***	0.987***	0.660***	0.475*
	(0.288)	(0.211)	(0.226)	(0.189)	(0.280)

Standard errors in parentheses are clustered at the town level. The regressions include a number of other controls including year and region (FeA) and state (ENDS) dummies. Complete results are reported in the Appendix. Quantile regressions include a 2nd order polynomial of residuals of a first stage regression of the HC variable on instruments. Sample sizes: FeA 5717, ENDS 6170 (Exposure) 6179 (Attendance)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7.1 -Effect of HC Participation on Child's Birth weight Using Predicted Instrument

	1	2	3	4	5	6	7
			FeA			ENDS	
	OLS		IV - non linear+			OLS	IV - non linear+
		All	Capacity	Fee	Distance		Capacity
Exposure to HC	0.0202	0.399	-0.0947	-0.331	1.01	0.00695	-0.249
	[0.0932]	[0.566]	[0.851]	[1.211]	[0.689]	[0.0812]	[0.402]
						-0.0101	-0.0553
Attendance to HC	-0.0136	0.384	0.0415	-0.59	0.539	[0.0287]	[0.169]
	[0.0627]	[0.359]	[0.808]	[1.643]	[0.384]		
			IV - linear+				IV - linear+
Exposure to HC		0.178	-0.196	-0.016	0.439		-1.558*
		[0.495]	[0.644]	[0.747]	[0.590]		[0.872]
							-0.875*
Attendance to HC		0.175	0.235	0.345	0.223		[0.515]
		[0.230]	[0.301]	[0.299]	[0.239]		

Standard errors in parentheses are clustered at the town level. The regressions include a number of other controls including region and year (FeA) and state (ENDS) dummies.

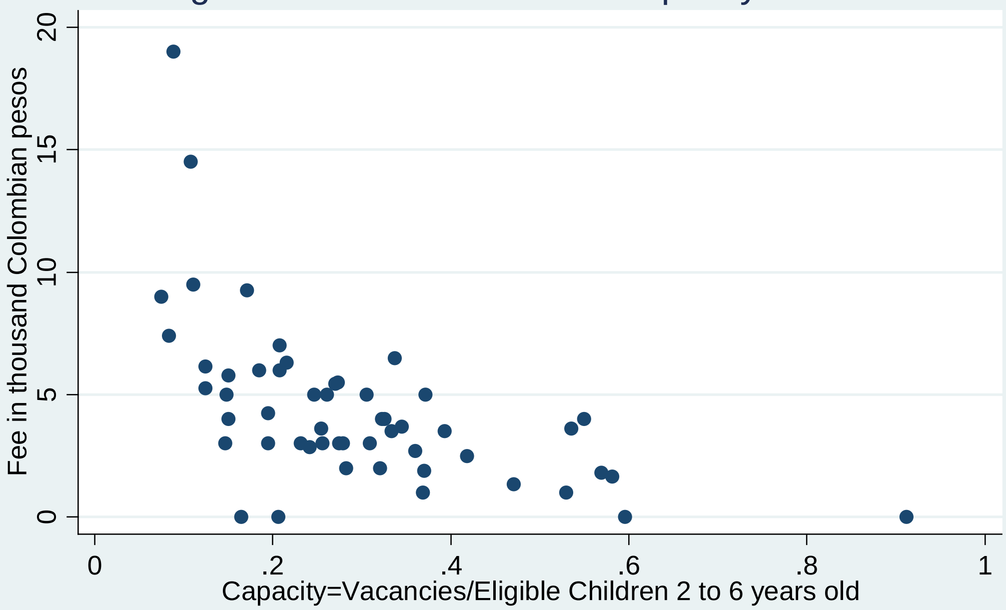
Complete results are reported in the Appendix. Sample sizes: FeA 1371, ENDS 2093 (Exposure) 2097 (Attendance)

+ linear means instruments are entered in levels, non-linear means instrument set contains also squares.

*** p<0.01, ** p<0.05, * p<0.1

Figures

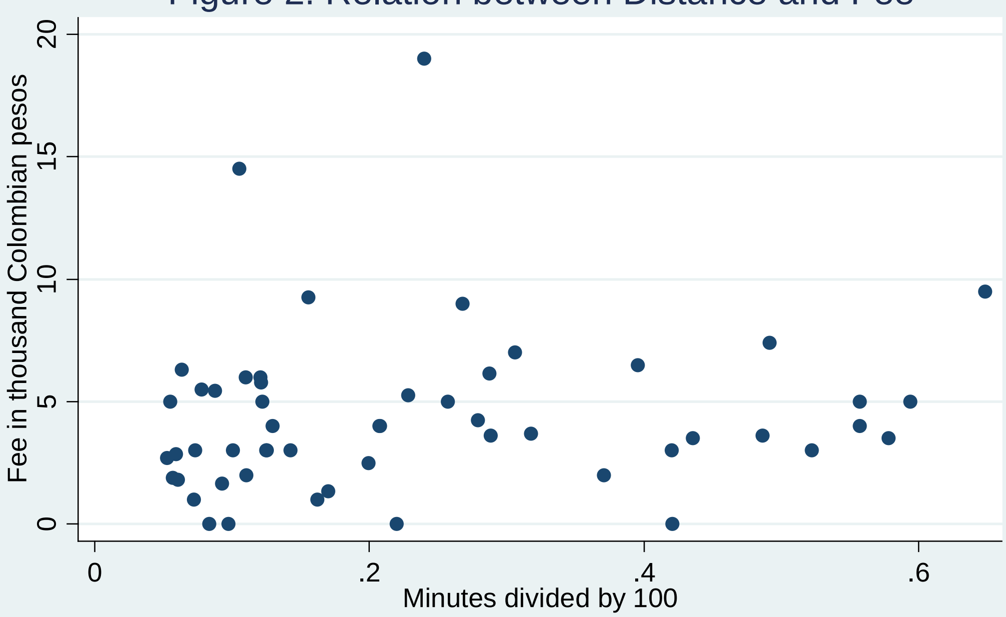
Figure 1: Relation between Capacity and Fees



Correlation=-0.56, P-Value=0.00

Each plot represents the average value in a town

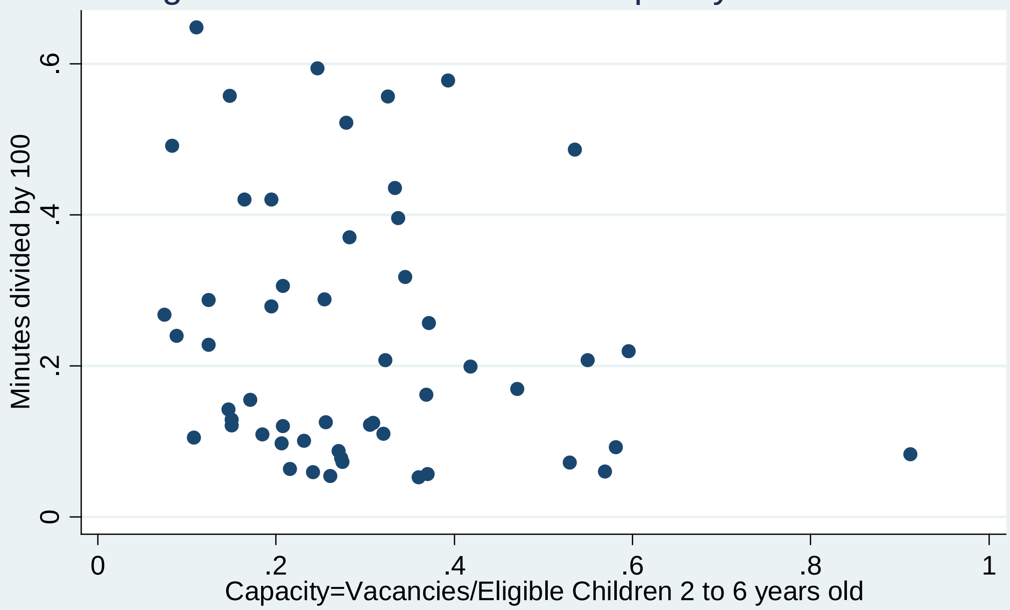
Figure 2: Relation between Distance and Fee



Correlation=+0.13, P-Value=0.36

Each plot represents the average value in a town

Figure 3: Relation between Capacity and Distance

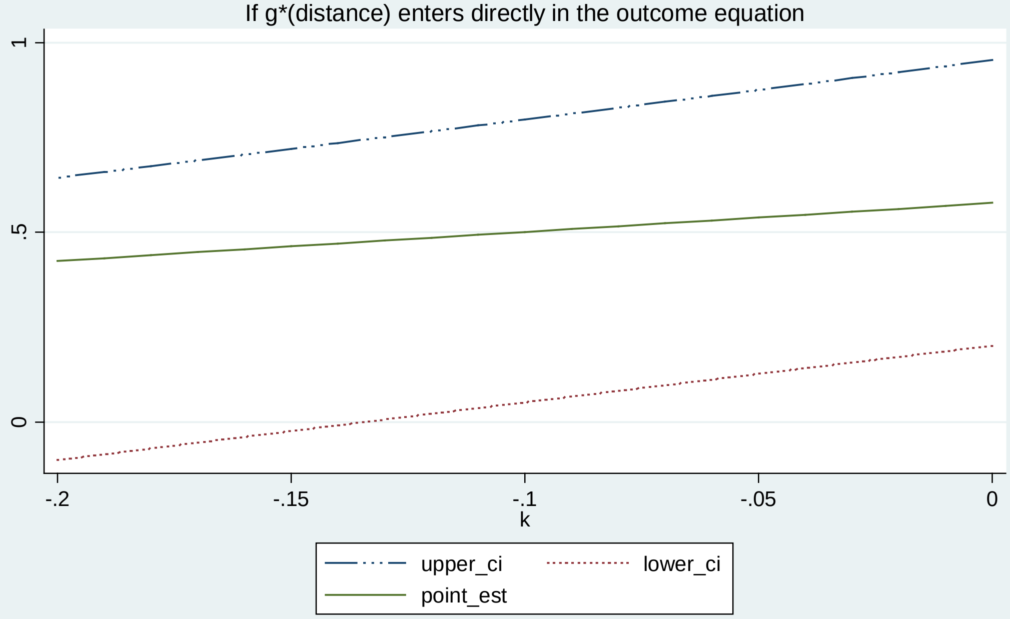


Correlation=-0.18, P-Value=0.18

Each plot represents the average value in a town

Figure 4: 90% Confidence intervals for Attendance

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

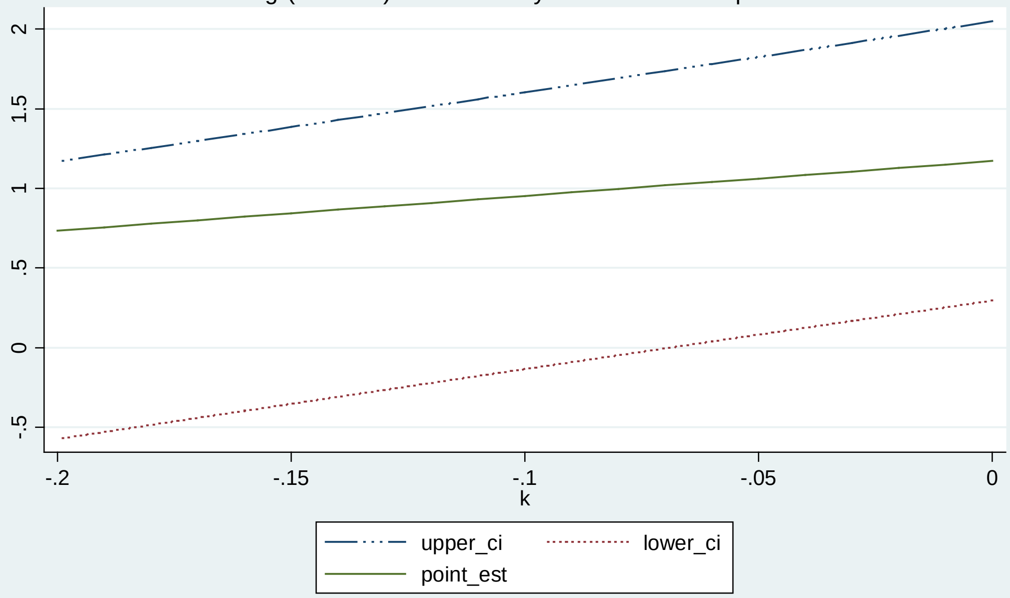


Assumed that g belongs to [k,0]. Point estimate is if g=k/2. Dataset: FeA

Confidence intervals are built using union of confidence intervals following Conley et al (2008)

Figure 5: 90% Confidence intervals for Exposure

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

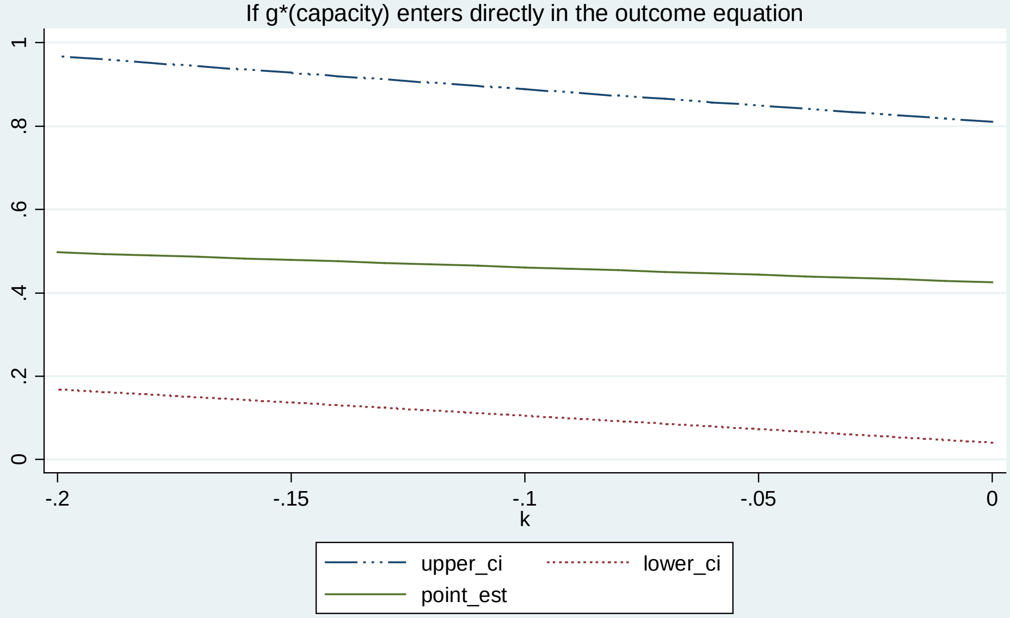


Assumed that g belongs to [k,0]. Point estimate is if g=k/2. Dataset: FeA

Confidence intervals are built using union of confidence intervals following Conley et al (2008)

Figure 6: 90% Confidence intervals for Attendance

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

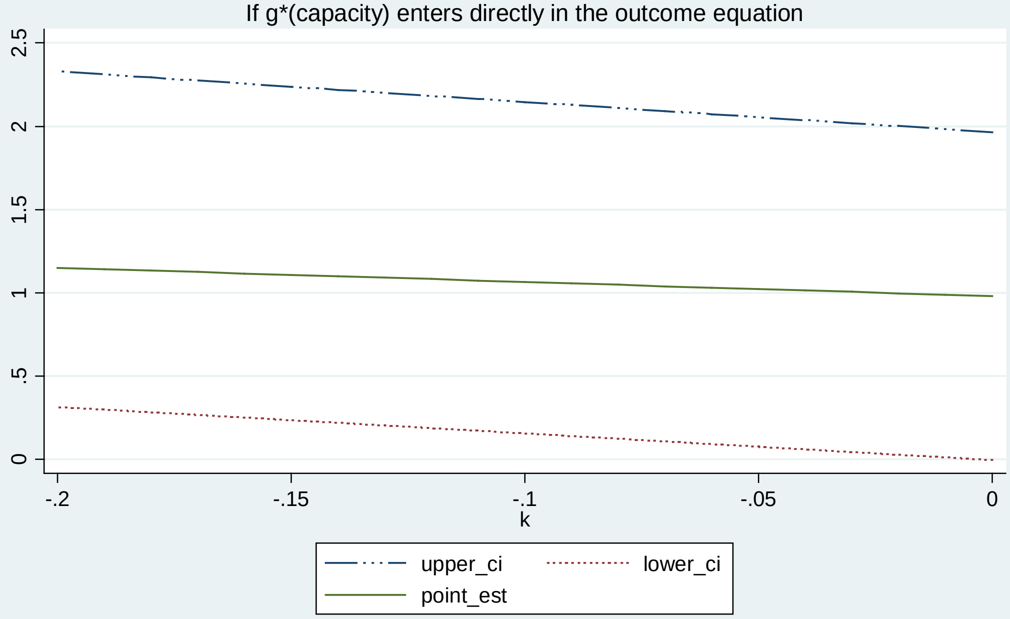


Assumed that g belongs to [k,0]. Point estimate is if g=k/2. Dataset: FeA

Confidence intervals are built using union of confidence intervals following Conley et al (2008)

Figure 7: 90% Confidence intervals for Exposure

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

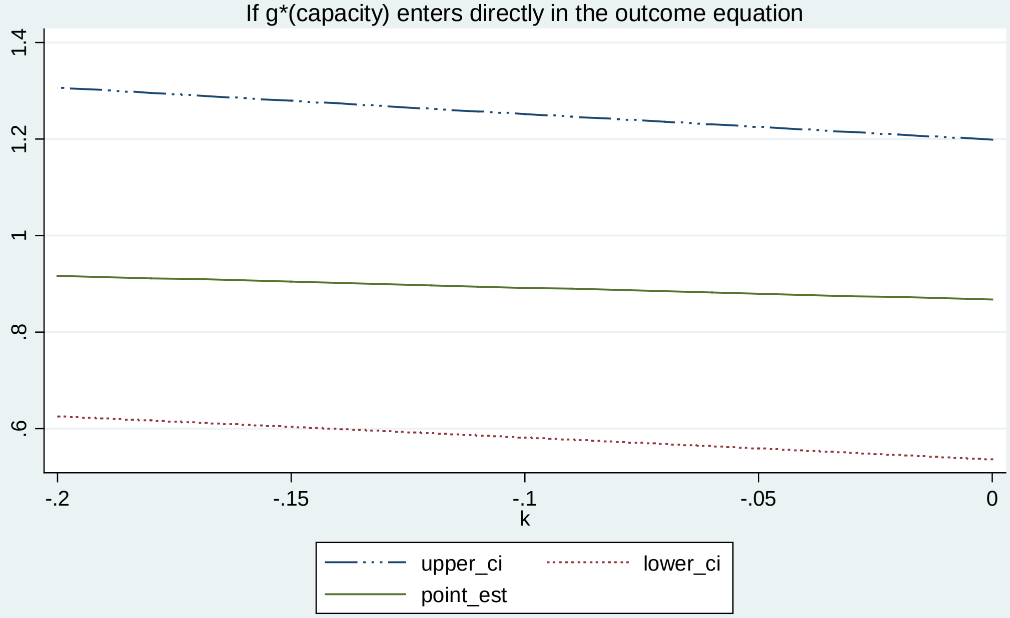


Assumed that g belongs to [k,0]. Point estimate is if g=k/2. Dataset: FeA

Confidence intervals are built using union of confidence intervals following Conley et al (2008)

Figure 8: 90% Confidence intervals for Attendance

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

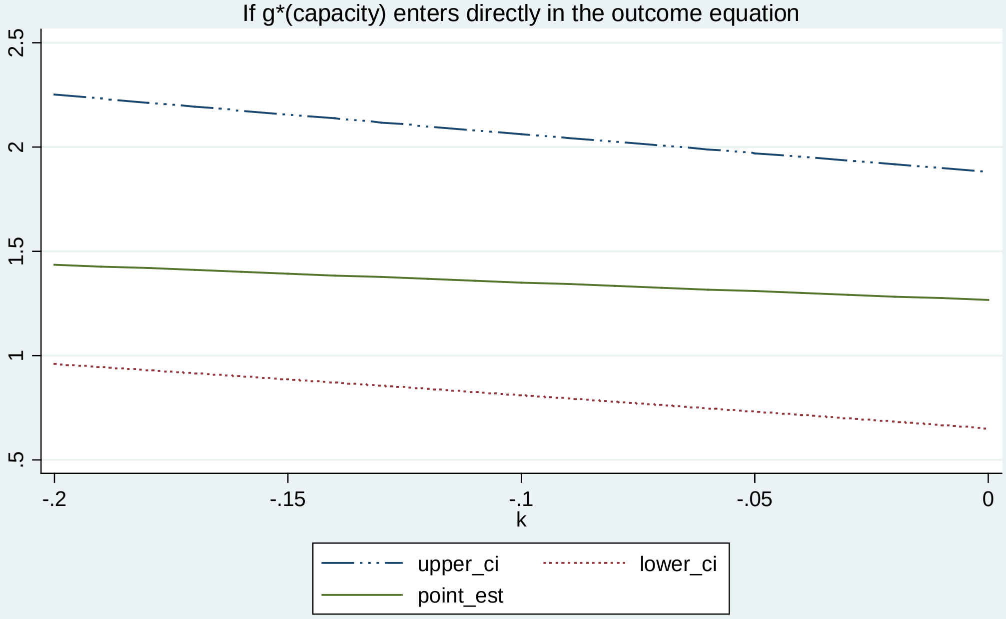


Assumed that g belongs to [k,0]. Point estimate is if g=k/2. Dataset: ENDS

Confidence intervals are built using union of confidence intervals following Conley et al (2008)

Figure 9: 90% Confidence intervals for Exposure

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



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

Confidence intervals are built using union of confidence intervals following Conley et al (2008)