Investing in Human Capital to Escape Poverty: Evidence from Latin America

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I, Vincenzo Di Maro, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

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- Chapter 1 draws on joint work with Ciro Avitabile
- Chapter 2 draws on joint work with Emmanuel Skoufias
- Chapter 4 draws on joint work with Orazio Attanasio and Marcos Vera-Hernandez
- Chapter 5 draws on joint work with Emmanuel Skoufias, Teresa González-Cossío, and Sonia Rodríguez Ramírez

Abstract

This thesis studies the impact of different types of policy interventions on demand for human capital in Latin America. Chapter 1 focuses on the unintended consequence (spillover effects) of the *Oportunidades* program in rural Mexico. We show that the program has an indirect effect on cervical cancer screening rates of women who were not eligible for the program but lived in areas where the program was in operation. These effects – health externalities – can dramatically change the assessment of the impacts of a program as well as considerations about its design. In addition to this, we show evidence of the mechanism through which the program operates being the weakening of the social norm of husbands' opposition to their spouses being screened by male doctors. In Chapter 2 we show that *Oportunidades* is bringing families out of poverty, which is considered here as a necessary condition to allow them to invest in human capital. We also discuss why CCT programs can have perverse incentives on the labor supply of eligible individuals and show that the program is not having this effect. In chapter 3 and 4 we contribute to the evidence on the impact of Early Childhood interventions. In chapter 3 we discuss how conditional cash transfers can increase the caloric intake of very young children and young mothers. This chapter also has some methodological content, in that it shows how to apply a technique for estimating individual caloric intake when only household aggregate data is available to a program evaluation setting. Results show that *Oportunidades* is successful at increasing the caloric intake of young children and young mothers, while it does not seem to have an effect at other age ranges. Chapter 4 focuses on the evaluation of the impact of a preschool nursery program in Colombia: *Hogares Comunitarios*. When compared to a CCT program, this program can be thought as a direct attack to children development, as participants (kids age 0 to 6) in the *Hogares Comunitarios* receive daycare services and food at the house of a community mother. Our evidence shows that this program can have a positive and sizeable effect on child growth, with this result being robust to different instruments for participation into the program and different samples. In chapter 5 we deal with the long-standing debate about in-kind transfers vs. cash transfers and with how this relates to child nutrition. In particular, we study how nutrient intake responds to changes in income in a sample of rural Mexican households. This increase in income can be thought as an unconditional cash transfer to households. Our evidence is mixed: while consumption of some key nutrients (vitamins A and C, heme iron, calcium and fats) responds positively to an increase in income, other nutrients (energy, zinc and protein) seem not to be affected by a change in income, with this supporting the case for conditionalities and/or in-kind transfers.

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Contents

List of Tables

List of Figures

Introduction

Juana Maria is a mother of 4: Jaime was just born (he is 4 months), Yadira is 6 years old, Estela is 11 and Lester is 12. Her husband, José, works as a farmer in the small plot they have; they grow some fruit and vegetables for their own consumption and mainly maize and coffee for sale. In the coffee harvest season, José will typically leave the household for a couple of months and work as a coffee picker in a *finca* (farm) which is around 100 miles away. During this period, the rest of the family takes care of the small plot they farm and of the few animals they have (mainly chickens and dogs for the moment, but one day they want to buy a cow and a few pigs). Juana and José feel every single day that their budget constraint binds: in good years (good harvest, high wages in the coffee region), they earn an income which is enough to feed the kids and themselves and maybe save a little. In bad years, they have to rely on their small savings, friends and relatives to get the minimum needed to feed the family.

Jaime, the newborn, has a long way before his family needs to start thinking about investments in his human capital. Yadira is a very successful student in the local primary school, which is not a surprise for people in the community: everyone knows that Yadira *tiene mucha chispa*[1], very much like her mother. Estela and Lester, the older kids, should be attending secondary school at their age, but they are not. They actually enrolled at the beginning of the academic year, but then, as José explained in very simple words, they dropped out as their work was very much needed in the field with their father. In addition, Estela has always struggled with keeping up with her peers in primary school. She is a very well-mannered kid, but she does not speak much and she is very quiet. Juana and José know very well the importance of education as they remember when their teachers would tell them to continue studying. However, they continued only until primary school. They would like to send their kids to school, but they simply cannot afford it.

In the same community of Juana and José's, there is also Don Julio. He is the undisputed nonelected leader of the locality and he is definitely the wealthiest person around. He owns a large *finca* extending more than 400 hectares, which in a normal period would employ around 25/30 people from his and neighbor communities. His sons work with him. Aurelio is 25 and Nestor is 30 and they are very well respected by the employees, partly because they are the owner's sons and partly because they

[1]This is the Spanish for "She is a very bright kid".

know their job very well. Last year Nestor bought a brand new moped, and this year Aurelio decided to buy one as well. Money has never been a problem for Don Julio and his family. They have always worked hard to a very profitable use of the land they inherited and purchased from neighbor farmers over the years. Aurelio and Nestor have completed primary school, but no more. They could have easily afforded the fees and the trip to one of the best secondary schools around, which is located in the municipality's capital, but Don Julio thought the best future for his kids was with him in the *finca*. Don Julio has always loved his kids, as Dona Rosa does; she is his wife and she would have liked her kids to continue in school, but she never really had a say in decisions about her kids' education[2].

Investments in health and education are a key factor for successful and productive lives. In developing countries, many families do not invest in human capital, or not invest enough, for several reasons. For instance, families might simply not have enough resources to send their kids to school or to buy nutrient-rich foods to feed their children, or they might not have good information on the returns to investments in human capital. Specifically for Latin America and Caribbean (LAC), macroeconomic data reveal that countries in this region tend to invest less as percentage of GDP compared to OECD countries[3].

A long established literature has shown that investing in people is a good investment. Early studies in the mid-twentieth century compared investment in human capital to those in physical capital and concluded that investing in people—that is, in their skills and abilities—could be just as profitable as investing in physical capital. In particular, Schultz (1961, 1971) empirically showed the importance of education for productivity growth in the United States. At the individual level, starting with Mincer (1958), who was the first to empirically demonstrate that education differentials are related to differences in wages at later stages in life, a voluminous set of wage function estimates has grown and now it provides the basis for calculating market returns to education for virtually every country in the world. Becker (1964) organized this work and that of others into what we now refer to as "human capital theory," which is the foundation of much recent work on the relationship between investments in people and the benefits (both for individuals and the society as a whole). Recent reviews of studies that report positive associations between school attainment and experience and wages or other indicators of productivity are in Psacharopoulos (1985, 1994) and Psacharopoulos and Patrinos (2004).

This early seminal work focused only on education as a form of human capital. Another form of human capital, which originally received less much attention, is health. Conceptually, investments in health are the foundation of any other type of investment, as health and nutrition are intimately related to the very capacity of the body of performing tasks through which an individual can get his schooling and generate income.[4] In their review of the interrelationships

[2]While the people mentioned in the paragraphs above are fictional, the stories reported are based on very real experiences we have heard in our fieldwork in rural Nicaragua.

[3]Investments in preschool are not more than 0.6% of GDP in LAC, while are typically more than 1% of GDP in OECD countries. See Vegas and Santibáñez (2010).

[4]See Chapter 8.4 in Ray (1998) for a discussion of the relationship between nutrition (health status), work capacity and outcomes in the labor market.

between health, nutrition and economic development, Strauss and Thomas (1998) stress how the link between health and economic outcomes has received serious attention only recently. Interestingly, they spell-out the main reasons why the marginal productivity of health is likely to be higher in a developing context relative to higher-income industrialized economies. These include the fact that levels of health are much lower in developing countries as well as that incidence and nature of disease tends to be different, typically because of higher prevalence of malnutrition and infectious diseases. The age distribution of ill health is thus more tilted towards infants and preschool children: this has the one important implication that adults in poorer economies are more likely to be afflicted with health problems and to feel the consequences of ill health throughout the life cycle (and not only at older ages as in advanced societies). In addition, the structure of employment in lower income economies is such that work often relies more heavily on strength and endurance and, therefore, on good health. They conclude that a substantial progress has been made in documenting the existence of a causal impact of health on wages and productivity in low-income settings.

However, despite this evidence on positive return to investments in human capital, investments are still dramatically low in developing countries. Being perfect credit markets and complete information[5] not more than a textbook curiosity in these settings, low level of investments does not come as a surprise.

When thinking about investments in human capital in this thesis, we have in mind a simple dynamic forward-looking model of human capital demand behaviors[6] in which human capital demands are determined by a series of family or individual decisions given past, current and expected future resources, markets, policies and other institutions. The context in which these demands are determined, in turn, reflects decisions of suppliers of education and health services. In terms of timing of the decisions, it is useful to think about three main stages: preschool (from conception through about age 5 or 6), school ages and postschool. In particular, household demands are modeled within this framework as if households are maximizing an objective function subject to budget constraints and production functions which leads to a set of reduced-form demand relations. In terms of policy impacts, estimates of the reduced-form demands typically give the total policy impact, but with no indications of the channels or mechanisms through which the policy is exerting its effects and with less potential for exploring counterfactual policies. On the other hand, estimates of structural parameters[7] of objective and production functions can give insights on the underlying mechanisms and can be combined in structural models which, typically under strong assumptions, will allow simulating counterfactual policies.

This thesis presents evidence of the impact of different types of policy interventions on demand for human capital in Latin America, specifically rural Mexico and Colombia. The first three chapters study the same intervention, *Oportunidades,* which is a conditional cash

[5] Credit constraints and information failures are commonly thought as the main explanations behind the low level of investments. However, several other factors might be relevant including characteristics of education and health suppliers, gender preferences and social norms.

[6] The characterization of the model of human capital investments we are considering here is as in Behrman (2010).

[7] See Rosenzweig and Wolpin (2000); Keane (2010); and Heckman and Urzua (2009).

transfer (CCT) program targeting poor households in rural Mexico. The general objective of the program is to break the intergenerational transmission of poverty. To achieve this objective, the program's main focus is on fostering human capital accumulation of poor kids. Given its ideal evaluation design, the program has been rigorously evaluated and it has been shown that it has a substantial positive impact on several relevant dimensions, such as consumption, income, and health and education outcomes.[8]

In particular, in chapter 1 we focus on the unintended consequence (spillover effects) of the *Oportunidades* program. This chapter offers interesting contributions on two dimensions: first, we show that the program has an indirect effect on cervical cancer screening rates of women who were not eligible for the program but lived in areas where the program was in operation. This is evidence of substantial spillover effects of the program, in particular of health externalities, which is in line with results in Angelucci and De Giorgi (2009). These effects can dramatically change the assessment of the impacts of a program as well as considerations about its design. The other contribution of chapter 1 is about underlying mechanisms through which programs work. In general, study of spillover effects naturally leads to attempts to unveil program channels as the researcher typically might have only a limited idea *a priori* of how the program works in terms of its unintended effects. Here, we show evidence of the mechanism through which the program operates being the weakening of the social norm of husbands' opposition to their spouses being screened by male doctors.

In Chapter 2 we do two things: first, we estimate the impact of *Oportunidades* on the poverty status of treated households. This a total policy impact and no attempt is made to investigate underlying channels; however bringing families out of poverty is considered here as a necessary condition to allow them to invest in human capital. This chapter also discusses why CCT programs can have perverse incentives on the labor supply of eligible individuals and shows that the program is not having this effect.

Chapter 3 and 4 focus on the impact of interventions targeted at a specific age group: early childhood. A key issue in the literature on human capital investments has been about the best timing when families or individuals should invest in their human skills formation. Cunha *et al.* (2006) systematize the arguments which make the theoretical case for investments in early childhood. They develop a model of skill formation that builds on Becker (1964), Becker and Tomes (1979, 1986) and Ben-Porath (1967) which offers a number of important insights. First, ability formation is governed by a multistage technology: there are certain given periods in one's life (*sensitive periods*) in which some abilities can be produced most effectively; other abilities can *only* be produced at a particular period, which can be called *critical periods*. The existence of sensitive and critical periods means that the remediation of some abilities not acquired in early childhood is impossible or prohibitively costly. Another characteristic of skill formation is self-productivity, as skills acquired in one period persist into the next period, and those skills acquired in one dimension (for example, self-control) can make it easier to acquire skills in another dimension (for example, cognitive learning). Finally, skills are complementary, in

[8] See Skoufias (2005) for a review of the impacts of the Oportunidades program. See also Levy (2005).

that the productivity of later investments is increased by investments made in previous periods. All these arguments make a strong case for investments at early ages: investments in school or postschool period might not be productive at all were they not preceded by sufficiently high levels of investment earlier on. In addition, investments at early ages can have important multiplier effects later on as a result of self-productivity and complementarity. Carneiro and Heckman (2003) calculate the rates of return to human capital investments at different stages of life and show that, under standard assumptions, the rate of return if the highest for the preschool period.[9]

Even if the theoretical case for investments in early childhood is sound, an open question is the one about which type of intervention is the most effective. This question has to be answered empirically. Schady (2006) reviews the evidence of the impact early childhood development (ECD) interventions in the United States and Latin America and the Caribbean. He reports that, if carefully administered, intensive preschool programs (such as the Perry Preschool Project and the Carolina Abecedarian Project) can have very high returns in the US[10], and that evidence of large-scale interventions like Head Start is more mixed, although generally positive[11]. Interestingly, he argues that evidence from the US cannot be easily generalized to contexts such as countries in LAC, with this implying that specific evidence from these countries should be sought. However, the current status of the knowledge base on the effectiveness of ECD interventions in LAC is still thin, though recent research has been trying to fill this gap, with a promising switch towards the use of experimental evaluations.[12]

This thesis' contribution to the evidence on the impact of ECD interventions is twofold: in chapter 3 we discuss how conditional cash transfers can increase the caloric intake of very young children and young mothers. This chapter also has some methodological content, in that it shows how to apply a technique for estimating individual caloric intake when only household aggregate data[13] is available to a program evaluation setting. We show that *Oportunidades* is successful at increasing the caloric intake of young children and young mothers, while it does not seem to have an effect at other age ranges.

Chapter 4 focuses on the evaluation of the impact of a preschool nursery program in Colombia: *Hogares Comunitarios*. When compared to a CCT program, this program can be thought as a direct attack to children development, as participants (kids age 0 to 6) in the *Hogares Comunitarios* receive daycare services and food at the house of a community mother. Our evidence shows that this program can have a positive and sizeable effect on child growth, with this result being robust to different instruments for participation into the program and different samples.

Finally, in chapter 5, we study the relationship between household income and nutrient intake in a sample of households in rural Mexico. Studying how nutrient intake responds to

[9]See also the discussion of the model of Cunha and others in Schady (2006)

[10]See Scheweinhart (2005), Currie (2001) and Carneiro and Heckman (2003).

[11]See among others, Garces, Thomas and Currie (2002), Carneiro and Heckman (2003), Heckman and Rubinstein (2001), Currie (2001), and Blau and Currie (2004).

[12]See Gertler and Fernald (2004); Berhman, Parker and Todd (2004); Behrman, Cheng, and Todd (2004); Attanasio and Vera-Hernandez (2004); Berlinski and Galiani (2005); Berlinski, Galiani and Gertler (2009); Grantham-McGregor et al. (1997); Walker et al (2000); Powell et al. (2004).

[13]This methodology is as in Chesher (1997 and 1998).

changes in income has relevant policy content, in that increases in income can be thought as an unconditional cash transfer to households. The debate about whether to condition to some behaviors the recipients of cash transfers is a long standing one, as it is the one about in-kind transfers vs. cash transfers. Advocates of conditions attached to cash transfers and of in-kind transfers typically invoke the argument that availability of more resources for the household does not necessarily mean that this additional money will be spent on more and better nutrients. On the other extreme, arguments are that choice sets are larger and no extra costs (to comply with the conditionalities) are incurred by households when given unconditional cash transfers. Our chapter shows that increases in income are associated with significant and sizeable increases in the consumption of vital nutrients among poor households in rural Mexico, namely vitamins A and C, heme iron, calcium and fats, thus supporting transfers with no strings attached. However, intake of other nutrients seems not to respond to income or show a negative elasticity. In particular, we find that increase in income does not seem to be a policy tool that can remedy the deficiency in energy, zinc and protein for poorer households. This could indicate a limit of interventions that only focus on transfer of money.

CHAPTER 1

Spillover Effects in Healthcare Programmes: Evidence on Social Norms and Information Sharing

Screening programmes are a key healthcare priority. These programmes may have unintended effects on groups other than those the programme targets. While potential spillovers are not well understood or evaluated, their systematic assessment might change the evaluation or design of screening programmes and shed light on the determinants of screening behaviour. This paper exploits the randomized research design of a large welfare programme - PROGRESA - to study the size and determinants of spillover effects in cervical cancer screening in rural Mexico. Cervical cancer is considered to be one of the most preventable types of cancer, but cervical cancer mortality rates in Mexico have been dramatically high by international standards for many years. We find significant evidence of spillover effects in demand for Papanicolaou cervical cancer screening, yet there is no evidence of similar spillovers in non-gender specific tests, such as blood pressure and blood sugar. When we study the mechanisms that drive spillover effects we are able to distinguish between the roles of social norms and information sharing. For women living in male headed households there is increased demand for screening as PROGRESA undermines the social norm associated with husbands' opposition. Among women in female headed households screening is more frequent as a result of information sharing between those eligible and those not eligible for the programme. Importantly, these results are confirmed when a more recent, but non experimental, evaluation sample is used.

§1.1. Introduction

Cervical cancer is the second most common cancer in women, and was responsible for 250,000 deaths in 2005, approximately 80% of them in developing countries.[1] Mexico for many years has had the highest cervical cancer mortality rates in the world (Lazcano-Ponce (1997)) and cervical cancer is still the prime type of cancer mortality for women aged 25 and above.[2] Unlike the case with other types of cancer, early detection can virtually eliminate the mortality risk associated with cervical cancer. In this paper we exploit the randomized research design of PROGRESA, a large conditional cash transfer programme, to study the determinants of the decision to screen for cervical cancer in rural Mexico.

[1] See *Comprehensive cervical cancer control: a guide to essential practice*, World Health Organization.
[2] See Hidalgo-Martinez (2006).

While in many developed countries there is ongoing debate on the benefits and costs of breast cancer screening programmes, programmes aimed at detecting cervical cancer have been unanimously hailed as successful. Cervical cancer has recognized and well-described risk factors; there is an identifiable precancerous condition;[3] and a safe and cheap test (the Papanicolaou (PAP) smear test) for detecting precancer and cancer exists. As result, systematic use of PAP test has led to a huge drop in cervical cancer mortality in developed countries. However, this has not happened in developing countries.[4] Mexico is one of the most striking examples here. Despite a national screening programme being in place since 1974, the percentage of Mexican women who regularly screen for cervical cancer is well below the average of other OECD countries. Lack of compliance with cervical cancer screening advice is dramatically higher in rural areas where the percentage of women who have never been screened is more than double that in the Mexico City district (Lazcano-Ponce (1997)). While absence of screening is strongly correlated with low socioeconomic status, the mechanisms behind this correlation are largely unexplored.

The main question we address in this paper is why, despite availability of a free service and such a high expected payoff, women in rural Mexico do not screen for cervical cancer. In order to answer this question we exploit the randomized research design of PROGRESA, which is a large social welfare programme in rural Mexico. The sample consists of 506 villages: 320 were randomly assigned to be a treatment group within the PROGRESA programme, starting in May 1998, and 186 acted as controls, with the programme starting after November 1999. Data are available for all households in every village, both poor and non-poor, although only poor households are eligible for PROGRESA. The programme has two main components in the form of incentives. The first provides cash transfers to households conditional on their children's school attendance. The second provides a financial reward to households whose members, both adults and children, undertake regular health checks and attend health-related courses. In particular, eligible adults have to undergo full preventive screening: both male and female household members have their blood pressure and blood sugar tested, but the PAP smear test is female specific.

In this paper we study whether and how PROGRESA affects the demand for cervical cancer screening from women living in non-eligible households. Exploring the indirect effects of PROGRESA,[5] the so called Indirect Treatment Effect (ITE), is important for a number of reasons. First, there is limited evidence on the existence and magnitude of health externalities. Christakis (2004) stresses that from a social perspective the cost effectiveness of a medical intervention might change substantially if the evaluation of benefits and costs takes account of externalities.[6] So far there is no large scale evidence on spillover effects across individuals in active health-seeking behaviour.[7] Second, identifying how PROGRESA affects the decision of

[3]The transition to cancer occurs over a period of 10 years on average (see Blumenthal and Gaffykin (2005)).

[4]The World Health Organization estimates that 95% of women in developing countries have never been screened for cervical cancer.

[5]Programmes with similar characteristics have either been or are in the process of being adopted in Brazil, Colombia, Ecuador, Honduras, Jamaica, New York city and Nicaragua.

[6]A related strand of literature (see Dow et al. (1999)) argues that, as implied by the competing risk model, complementarities between diseases might alter the evaluation of cause-specific health programmes.

[7]Miguel and Kremer (2004) using evidence from a randomized experiment show that a deworming programme significantly reduced infection rates among children not treated by the programme.

non-eligibles to screen can shed light on the determinants of the demand for screening. As Luke and Munshi (2007) suggests, most of the existing literature fails to identify how cultural factors and social norms affect the demand for health services. Social norms might be particularly important to explain the health related decisions of the female population.[8] Others have studied the indirect effects of PROGRESA, focusing mainly on the role of income spillovers from eligible to non-eligible households and changes in informal credit markets.[9] This paper contributes to this literature by providing evidence that PROGRESA affects the behaviour of non-eligibles through non-market mechanisms, such as social norms and information sharing.

We begin by studying the effect of the programme on the demand for cervical cancer screening by non-eligible households compared to the demand for non-gender specific screening tests. In order to disentangle the effect of PROGRESA on the demand for screening from the supply, we exploit variations in health centre waiting times across villages. Our results show that the indirect treatment effect of PROGRESA on the demand for cervical cancer screening is positive, non trivial and significantly different from zero. We do not find any significant indirect effect on the probability of screening for diabetes and high blood pressure, or attending a health centre. We provide evidence that rules out the possibility that the indirect effect of PROGRESA on cervical cancer screening is due to income spillovers from eligible to non-eligible households.

We also study to what extent gender related social norms and information sharing can explain the indirect effect of the programme. While the literature that studies peer effects suggests that both mechanisms might be important (see Sacerdote (2000)), there is no empirical evidence that distinguishes one from the other. This is mainly because first, it is necessary to define the exact nature of the social norm, and second, social norms and lack of information might be strongly correlated. There is considerable qualitative evidence collected during the evaluation of PROGRESA (see Adato et al. (2000)) and in the course of epidemiological studies (see Watkins et al. (2002) and Lazcano-Ponce (1997)), to show that male opposition to wives being checked by male doctors is one of the most common reasons women give for not taking the test. In order to disentangle the social norm effect from the lack of information we examine the substantial heterogeneity of male and female headed households in terms of female status and cervical cancer risk factors. We show that while the degree of emancipation of female respondents in male headed households is lower than in female headed households, female respondents in the latter display lower levels of education and have higher risks of contracting cervical cancer. We sketch a simple model of social norm diffusion that shows how PROGRESA might affect the social norm. We test the theoretical predictions of the model exploiting across-village variations in the fraction of households eligible for the programme, and within-village variations in household composition.

Finally, we study whether PROGRESA generates information spillovers from eligible to non-

[8]Boulis (2000) argues that social norms might be an important factor to explain the low rate of breast and cervical cancer screening among ethnic and religious minorities in the US.

[9]Angelucci and De Giorgi (2009) find a significant effect of the programme on consumption of non-eligible households operating through insurance and credit mechanisms. Bobonis and Finan (2006) and Cattaneo and Lalive (2006) find that the programme significantly increases school enrollment among non-eligible families through a peer effect.

eligible households. Exploiting different definitions of peer groups, we test a prediction of learning models that is not supported by alternative explanations. Models that study the adoption of new agricultural technologies (see Bandiera and Rasul (2006) and Conley and Udry (2005)) predict that the learning externality should be bigger for farmers with lower initial information. We use information on whether the respondent has ever used any method of contraception to proxy for the initial level of information about sex related diseases.

We find significant evidence of both social norm and information sharing mechanisms. Our results show that while the social norm mechanism mainly drives the indirect effect of the programme on women living in male headed households, information sharing explains the effect on female headed households.

Even though the specific focus of this paper is on rural Mexico and the impact of a conditional cash transfer programme, our findings have much wider applicability for developing and developed countries. There is persistent controversy on the design of national screening programmes and on target groups. Traditional approaches to evaluating the costs-benefits of screening programmes, such as the *health cost effectiveness* principle, fail to take account of the externalities from eligible to non-eligible individuals. Our results suggest that spillovers might significantly increase the benefits related to a screening programme. Although many European countries have breast cancer screening programmes in place for women age 50-70, the percentage of the female population that regularly screens for breast cancer varies dramatically across countries (see Avitabile et al. (2008)). Our findings show that the design of screening programmes should explicitly address cultural beliefs if the programme is to be effective. In the specific case of rural Mexico an increase in the number of female health professionals would improve screening rates and similar interventions should be considered in areas with large presence of ethnic and religious minorities. Recent literature (see Munshi and Myaux (2006)) has stressed the role of traditional institutions in shaping the effect of external interventions to increase growth. Our results can be seen as evidence that large scale interventions can have indirect effects on social norms that have negative consequences for social well being.

The paper is organized as follows. In Section 1.2 we provide background information on cervical cancer and its prevalence in Mexico, and on other chronic diseases. Section 1.3 describes the PROGRESA programme and the evaluation data. Section 1.4 presents the empirical method and the baseline estimation results. Section 1.5 provides evidence on the specific mechanisms through which the indirect effect of PROGRESA occurs. Section 1.6 concludes. Additional robustness checks are provided in appendix A.

§1.2. Background

1.2.1 — Cervical Cancer in Mexico

The Human Papillomavirus (HPV) has been identified as the major cause of cervical cancer and is found in 99.7% of cases. Out of the 100 different HPV types that have been characterized so far, there is a group of 30 that are transmitted through sexual contact. HPVs are believed to be the most common of all sexually transmitted infections and most sexually active people get at least one HPV infection in their lifetime, usually without knowing it. Persistent infection with a subset of about a dozen so called 'high risk' sexually transmitted HPVs, including types 16, 18, 30 and 33 can lead to the development of cervical cell change (*dyskaryosis*), which in turn may lead to cancer of the cervix. The main risk factors are related to sex behaviour: early age of first intercourse, multiple sexual partners, early age of first pregnancy, multiparity,[10] and previous sexually transmitted infections. Additional risk factors include smoking and malnutrition. Since precancerous cells can be identified with the standard screening procedure, never being screened increases the risk of contracting cancer. The most common screening procedure is the PAP smear test. Although it is gradually being replaced by Liquid Based Cytology (LBC), the PAP smear test has proved very successful in reducing cervical cancer mortality. Between 1950 and 1998 in the US there was a 79% reduction in the incidence of cervical cancer and a 75% decrease in mortality; there is a unanimous consensus among specialists that most of these improvements are due to the systematic use of the PAP test (see among others Montz (2001)). When detected at an early stage, safe, effective and relatively inexpensive outpatient treatments are available (see Blumenthal and Gaffykin (2005)).

Following the example of developed countries, in 1974 the Mexican government started the Cervical Cytology Screening programme (CCSP). The programme's measures[11] allow for: i) all women to be screened free of charge regardless of their age; ii) health professionals to invite women in the age group 25-64 for screening, with particular attention to those with high risk factors; iii) written or verbal invitations to screen for all rural households with at least one woman aged 25 or over. Women who present two normal cytologies for two consecutive years are invited to screen every three years.[12] Despite this programme being in place, the adjusted mortality rate gap between Mexico and other developed and developing countries has been increasing over time (see Figure 1.1). According to the statistics provided by the National Health programme in 2000 the average cervical cancer mortality for women aged 25 or above was 20 over l00,000. Four states covered by the PROGRESA programme (Veracruz, Michoacán, Guerrero and San Luís Potosi) display an average mortality above the national average. While this high mortality might be due in part to the poor quality of health provision,[13] the main cause is the low take up of screening. OECD data for 2002 show that only 38.9% of Mexico's female population in the age group 20-69, were screened for cervical cancer in the previous three years, compared to 84.8% in the US (see Table 1.1). The National Health Survey (*ENSA*) 2000, which was designed to identify the health status of the entire Mexican population, reports that in 2000 only 27.4% of the female population aged 20 or over was screened for cervical cancer in the previous 12 months. 67.3% of the women who were screened received the results of the screening, and 9.3% were diagnosed as either a carcinoma or a dysplasia.

[10] Controversy exists over the causal explanation for this correlation. While some studies suggest that the physiological process in the last two trimesters of pregnancy modifies the host-immune response, others focus on the trauma of the cervix during delivery.

[11] The programme has been constantly modified and improved and the last change was passed by law on 31/05/2007.

[12] This is the recommended screening frequency in the UK and US for women in the age group 25-49.

[13] Flisser et al. (2002) finds that inadequate supply of reagents and inadequate laboratory facilities increases the failure rate of the PAP test.

1.2.2 — Previous studies

As cervical cancer has for many years been considered a major health issue, there is an extensive epidemiological literature on the determinants of cervical cancer mortality in Mexico. Lazcano-Ponce (1997) carried out a cross-sectional study in two geographic regions of Mexico, Oaxaca (rural area) and Mexico City (urban area) to study the determinants of participation in the CCSP. This study found that non-compliance is strongly associated with low socioeconomic status and high reproductive risk. In Mexico City 35.8% of women (compared to 70% in Oaxaca) reported that they had never had a PAP smear test. A high proportion of women (20.3% in Mexico City vs 59.5 % in Oaxaca) were not correctly informed about what the test detects. In rural areas, many women reported they did not seek testing because their partners would not allow it. Watkins et al. (2002) conducted a pilot study using direct interviews to learn about factors that may influence cervical cancer screening among rural Mexican women. In a sample of 97 rural women between the ages of 16 and 66 the most frequent reason for not seeking a PAP smear was anxiety regarding physical privacy (50%). Less frequent reasons were lack of knowledge (18%) and difficulty accessing healthcare (14%). Other studies stress that while most women know what the PAP test is for, they do not perceive themselves to be at risk (see Lazcano-Ponce (2001)).

Cultural barriers to cervical cancer screening were also documented in the PROGRESA evaluation. Adato et al. (2000), in their study of the operational performance of PROGRESA, report that when asked about the main difficulties related to the programme's health component, doctors often mentioned problems encountered by male colleagues over family planning advice and the preventive PAP smear test. Most doctors agreed that the PAP smear was the most difficult for women because many men were opposed to their wives having the test, and especially if it were done by a male doctor. However, discussions with beneficiaries and non-beneficiaries[14] suggest that PROGRESA fostered better acceptance of the PAP test.

1.2.3 — Prevalence of and screening for other chronic diseases

This section provides information on the prevalence of and screening rates for diabetes mellitus and high blood pressure using ENSA data. Among men (women) 8.5% (12.1%) of those aged 20 or above have been screened for diabetes and had a prevalence of 7.2% (7.8%). Among the states covered by PROGRESA, Guerrero, Hidalgo and Veracruz have an average prevalence higher than the national average. Prevalence among the non educated is 15.1% versus 4.8% for graduates. 10.9% (15.7%) of men (women) aged 20 or over had their blood pressure checked in 2000. The prevalence of hypertension is 30.7% for the total sample, with 32.6% for men and 29.0% for women. Prevalence of hypertension was highest among women aged 60-69 (48.1%). 22.9% of women graduates are affected by hypertension as opposed to 44% among non educated women.

Men and women in the age range 30-45, with no significant risk factors, are invited for diabetes screening every three years. Men and women aged 45 and over are recommended to have

[14]See p. 135 in Adato et al. (2000).

an annual diabetes test, and men and women aged 30 and over are advised to have their blood pressure checked annually. Women receive free screening under the Woman's Health programme (*Programma de Salud de la Mujer*); to our knowledge, there are no special arrangements for men.

§1.3. The PROGRESA programme

1.3.1 — The food and health component

The PROGRESA programme[15] was launched by the Mexican government in 1997 to alleviate poverty by fostering human capital accumulation. The programme offers two benefits: it provides cash transfers to households conditional on their children's attendance of primary and secondary school. It also provides transfer and nutritional supplements conditional on attendance in health programmes offered at local facilities.[16] The basic requirement for eligible households to receive the health and nutrition component is regular visits to local health centres. Children less than 24 months old and pregnant women are required to have screening on a continuous basis throughout the year, lactating women and children age 2-4 years old are required to have two health checks per year, and all individuals aged 17 or over are required to have an annual check up. The health centre visits include family planning advice, prenatal, childbirth and puerperal care, vaccinations, prevention and control of high blood pressure and diabetes mellitus, preventive treatment and screening for cervical cancer. In addition, beneficiaries are asked to attend health and nutrition talks (known as *platicas*).[17] In principle the cash transfer is conditional on all members of the household having met these criteria before payment is received. For the period July to December 1999 the value of the cash grant for food consumption was 125 pesos[18] per month (on average this corresponds to a quarter of the household's monthly income).

While mainly aimed at increasing the demand for health services, the food and health component also includes four actions to improve the supply of healthcare: (i) ensuring adequate supply of equipment to health centres; (ii) encouraging staff working in remote rural areas to continue there on a long-term basis; (iii) ensuring that health-care units have the necessary medicines and material (including educational health materials to distribute to families); (iv) providing training to improve the quality of medical care and the operational dimensions of the health service.

[15]Under the Fox administration the programme was renamed *Oportunidades*.

[16]The programme was initially offered to 140,544 households in 1997, and was expanded to more than 2.6 million recipient households by the end of 1999. This represents approximately 40% of all rural families (Skoufias (2005)).

[17]*Platicas* are directed mainly to mothers, but other family members as well as non-beneficiaries are invited to attend. Various aspects of health and nutrition are discussed, with special emphasis on preventive healthcare. This includes ways of reducing health risks (e.g. prenatal care, early detection of malnutrition), how to recognize signs and symptoms of sickness, how to follow appropriate primary-care procedures.

[18]10 pesos is approximately US 1$.

1.3.2 — Data and eligibility criteria

The experimental data contain information on households from a sub-sample of 506 poor rural villages in seven states: 320 villages were randomly assigned to the treatment group and started receiving the benefits of the programme from May 1998, while 186 villages were randomized out and did not receive treatment until the end of 1999. The sample initially included 24,077 households. Eligibility for the programme was based on poverty level, as defined by a measure of permanent income based on the information collected in the September 1997 census of villages. There were two rounds of selection for eligible households in PROGRESA: 52% of households were classified as poor in 1997 and were therefore eligible for cash transfers. In October 1998, 54% of households initially classified as non-poor, were added to the beneficiary group.[19] Households were informed that, once classified either as poor or non-poor, their status and thus eligibility would not change until November 1999, irrespective of any income variation. Two features of PROGRESA are particularly interesting for our analysis. First, households were clearly informed about their eligibility status and the conditionalities, mainly through village assemblies. Hence take-up rates among eligibles were over 90%. Second, within the household, it was the women who were the recipients of the cash transfers. All residents of both control and treatment villages were interviewed at roughly six month intervals: twice before the programme started (the October 1997 wave and the March 1998 wave) and again in October 1998, May 1999 and November 1999.

Households fall into four groups of eligible, and non-eligible households, in treatment and control villages. Only the eligible households in treatment villages received the PROGRESA transfer.

In the March 1998, October 1998 and May 1999 waves every respondent (usually female) was asked about three different types of screening: cervical cancer (via the PAP smear test), diabetes (blood sugar test) and hypertension (blood pressure testing). In the March 1998 wave respondents were asked whether any household member had been screened in the previous 12 months; in the following two waves this question referred to the previous six months. In addition, in March 1998 respondents were asked about sex related behaviour, including whether contraceptive methods were used, total number of pregnancies and whether or not they had a PAP smear test done.

PROGRESA also collected information about different aspects of health provision at both the village and individual level. The October 1997 and October 1998 locality questionnaires included detailed questions about the type of health infrastructures and services available in the village. The socio-economic questionnaires administered to the March 1998 and October 1998 waves elicited specific information about the main characteristics of any health centres attended by any of the household members in the previous six months . Questions enquired about centre opening times, cost of visits, waiting times before being seen, length of visits and whether or

[19] These are usually referred to as *densificados*. A non-random subset of these households began receiving PROGRESA transfers in treatment villages prior to November 1999. As no precise algorithm exists to determine which *densificados* received transfers in treatment villages, there is no counterfactual set of households in control villages.

not they had received medicines from the doctor.[20]

In 2003, a new follow up round of data and a new control group, consisting of communities where *Progresa* had yet to be received and chosen through propensity score matching, was brought into the evaluation. The 2007 Rural Evaluation Survey (ENCEL) collected data on the original evaluation sample[21] and the 2003 control localities.

Unlike the evaluation sample, information on screening decisions are at individual level. All women younger than 50 living in the household were asked if they screened for cervical cancer. Additional questions ask about screening for breast cancer, hypertension, diabetes and cholesterol. Among others, the 2007 survey contains two additional modules that are particularly relevant for our purposes. The health center questionnaire asks the center administrators an exhaustive set of questions about characteristics of the center, number and type of services offered, technical equipment, number, tenure and working hours of both doctors and nurses. There is also a doctors questionnaire that collects information on socio-demographic characteristics, specializations, training and current practices. In particular doctors are asked the frequency at which they either perform or advice gender specific screenings, i.e. PAP smear test and mammogram.

In the matched sample of localities belonging to the evaluation sample and those that acted as controls in 2003, there are 165 localities with at least one health center. Among them, at least one doctor operates regularly in 133 localities and for 128 of them we have information directly elicited from doctors.

§1.4. Empirical Analysis

1.4.1 — Preliminary descriptives

This section provides descriptive evidence on the pre-programme levels of screening and how they vary over time, by eligibility status in the treatment and control villages. It also provides a description of how healthcare supply changes over time in treatment and control villages.

In order to compare screening levels during the operation of the programme with those before, we calculate the cumulative probability that any household member is screened either in the six months before October 1998 or in the six months before May 1999. This measure can be directly compared with the March 1998 information. While pre-programme screening rates for high blood pressure and blood sugar show small and insignificant differences between treatment and control villages for both eligibles and non-eligibles, the difference for PAP tests at March 1998 is bigger and especially for non-eligible households, but not significant (see Table 1.2).[22] Our results are consistent with those of Berhman and Todd (1999), who studied the quality of the randomization comparing the equality of the distributions for many characteristics both at village

[20] Additional information about the competence and availability of doctors and nurses and how easy it was to understand what they were told, is also available.

[21] Communities with very small populations (less than 20 households) were not resurveyed in 2007

[22] In the group of non-eligibles the screening rates of the *densificados* households in March 1998 are significantly lower than those of households whose eligibility status was not revised. For example, the average screening rate for cervical cancer among *densificados* is 29.7% compared to 39.7% for *non-densificados*.

and household (only for the group of eligibles) level. Screening rates show a sharply increasing trend over time for eligibles and non-eligibles in both the treatment and control villages: this result is consistent with the increase in the health supply coverage for treatment and control villages, which we refer to later in this section. In order to measure how screening rates change after programme implementation, we estimate an unconditional Differences in Differences (DD) linear model, with standard errors clustered at village level. As expected, the screening rates for eligibles show a remarkable increase for all the tests (on average above 20%). Among non-eligibles, blood pressure and blood sugar screening rates do not change significantly between the treatment and control groups. In contrast, the DD response for cervical cancer screening is strong and significantly different from zero: there is a 6.3% increase in the PAP test take up rate for non-eligibles in treatment villages (see Table 1.2).

One of the distinctive criteria for a village to be included in the PROGRESA programme was the presence of basic health services. The upper panel of Table 1.3 provides evidence on the health providers coverage in the PROGRESA villages and how the composition between treatment and control villages varies over time. Consistent with previous work that studied the quality of the random assignment,[23] we find that, at the baseline, the proportion of villages with at least one major health provider does not differ significantly between treatment and control villages. In Mexico there are two main public providers for households not covered by insurance: Health Secretary (*SSA*) and *IMSS Solidaridad*. In October 1997, 13% of the control villages had *SSA* clinics, compared to 8% of treatment villages with a difference significant at 10%. By October 1998 the proportion of villages with at least one *SSA* hospital does not differ significantly between treatment and control villages. No significant changes are observed in the fraction of villages covered by *IMSS Solidaridad* clinics.[24]

Only 4% of the PROGRESA sample is covered by the *IMSS* insurance.[25] This explains why the presence of *IMSS* hospitals is fairly small in the PROGRESA villages and does not vary significantly between treatment and control villages either at the baseline or at October 1998. The auxiliary health units are usually in rather inaccessible rural locations, where populations vary between 500 and 1,000 inhabitants. They can usually rely on the presence of one general practitioner. The mobile health units are composed of both medical and paramedic staff who offer a full set of ambulatory services. Auxiliary health units and mobile units are the most common providers in PROGRESA villages. At the baseline there is a bigger proportion of villages with at least one auxiliary health unit in control villages, while in October 1998 coverage does not differ significantly between treatment and control villages. By contrast, the proportion

[23] See Berhman and Todd (1999).

[24] At national level 42% of all Mexican hospitals are run by *SSA*. IMSS Solidaridad is a programme launched by the Mexican Government in cooperation with Mexican Institute of Social Security (*IMSS*) to reach rural populations in marginal areas. The programme currently provides healthcare for 10.6 million Mexicans, 1.2 million of whom benefit from the PROGRESA-Oportunidades programme. In July 2000 the programme has been renamed IMSS Oportunidades.

[25] Participation is compulsory for workers employed in the formal sector. The self-employed can join on a voluntary basis. Public employees are covered by the Institute of Social Security for Public Employees (*ISSSTE*) but they represent a negligible fraction of the PROGRESA sample. At national level, *IMSS* and *ISSSTE* clinics make up approximately 33% of all hospitals and 12% of the ambulatory care facilities.

of villages served by mobile units is higher for treatment than control villages in October 1997 but becomes significantly not different once the PROGRESA programme is in place. While on average *SSA* and *IMSS Solidaridad* hospitals are bigger, and better equipped than health aides centres and mobile units, all offer the three types of screening tests.

The lower panel of Table 1.3 provides evidence on some health supply outcomes. The average number of health services available in the village[26] increases sharply in October 1998, but does not differ significantly between treatment and control villages. Taking account of the village averages of individual responses, we can provide evidence on average waiting times for individuals to be seen, opening times of centres, average duration of visits and consultation fees. Baseline differences between treatment and control villages are not significant, except for duration of visits, which is slightly longer in non-treatment than in treatment villages. PROGRESA does not result in significant changes in waiting times, opening times or visit duration.

While cervical cancer screening is provided free of charge for both eligibles and non-eligibles under the CCSP, women could decide to undertake this screening within a more general medical consultation. In this case the fee charged by the doctor for the visit would represent the real cost of the screening. For the October 1998 wave the average consultation fee for treatment and control villages dropped dramatically, but the reduction is significantly bigger for the treatment villages. This is due to the eligibles accessing health centres free of charge as part of the programme conditionalities.[27] These results, along with the screening coverage described above, suggest that health services were strengthened equally in treatment and control villages, producing an increase in the number of services available and a reduction in prices for both groups. Improvements in health facilities in the control villages might have been carried out ahead of the programme implementation at the end of 1999. This is consistent with the observed upward trend in screening rates for non-eligibles in the treatment and control villages. However, as noted above, only cervical cancer screening differs significantly between treatment and control villages (see Table 1.2).

1.4.2 — Model of demand and supply of screening

In this section we propose a framework to identify how PROGRESA can affect the demand for screening from non-eligible households. The model draws on the literature that relates waiting times to service demand and supply (see Lindsay and Feigenbaum (1984), Gravelle (1990), Blundell and Windmeijer (2000)). In this framework the village average waiting time acts as the price of the health services for households in the community. There are two main reasons why we chose waiting time rather than a more 'standard' price. First, screening tests are free for women independent of the status of the village and the health provider. Second, the average consultation fee would not represent the true cost sustained by non-eligibles in treatment villages, as eligibles access health facilities for free as part of the conditionalities.

[26] These are taken from the 7 services listed in the locality questionnaire: prenatal care, delivery care, infant care, vaccination, diarrhoea treatment, family planning and hospitalization.

[27] Our estimates show a small and insignificant indirect treatment effect of PROGRESA on the consultation fee, as opposed to a negative, sizeable and significant treatment effect for eligibles.

Formally, in each village j there are N_j^{NE} non-eligible households and $N_j^E \equiv N_j - N_j^{NE}$ eligible households. Within non-eligible households, each individual will be assumed to undertake screening at any point in time if it yields a greater expected utility than non-screening, where the uncertainty is due to the probabilistic nature of the disease being screened.[28] For each member of household i the net benefit of screening is assumed to be positively correlated with the expected payoff of the test, and negatively correlated with the average waiting time to access health services in the village, W_j. Since the data only provide information on whether at least one household member was screened, we model the demand for screening at the household level. Let q_{ij}^{NE} be a binary variable that takes the value 1 if at least one member of household i in village j is screened, and 0 otherwise.

While in the next sections we will provide evidence on the mechanisms through which PROGRESA affects the expected benefit from screening, in this section we want to test whether PROGRESA has a significant effect on the demand for screening. The reduced form demand equation for screening of household i in village j can be written as:

$$q_{ij}^{NE} = 1(X_{ij}, T_j, W_j, v_{ij}) \tag{1.1}$$

where $1(\cdot)$ is an indicator function. X_{ij} is a set of socio-demographic characteristics of household i in village j, T_j takes the value 1 if village j is covered by PROGRESA, and 0 otherwise; W_j is the average waiting time to access a generic health centre in village j. v_{ij} represents the unobserved characteristics correlated with the decision to screen. The aggregate demand for preventive screening in village j is given by:

$$D_j = \sum_{i=1}^{N_j^{NE}} q_{ij}^{NE} + \sum_{k=1}^{N_j - N_j^{NE}} q_{kj}^{E} \tag{1.2}$$

where D_j represents the proportion of both eligible and non-eligible households that demand for screening and is negatively correlated with W_j. We assume that in each period the supply of health facilities in the village, S_j, is given and is inelastic with respect to W_j. The market for screening services is in equilibrium if the observed waiting time, W_j, is equal to the waiting time W_j^* at which demand and supply of screening intersect:

$$D_j = S_j \Leftrightarrow W_j = W_j^* \tag{1.3}$$

While we want to test whether the programme affects the demand for screening from non-eligible households, q^{NE}, eq. 1.2 and eq. 1.3 show that the programme might affect the screening rate of non-eligibles through two different mechanisms. First, the higher demand of eligible households might crowd out the demand for screening by non-eligible households. Second, health supply, S_j, might improve in PROGRESA villages, benefiting both eligibles and non-eligibles. The underlying assumption of the model is that these two mechanisms affect q_{ij}^{NE} through the waiting time, W_j.

[28] Another potential source of uncertainty that we do not consider in this work is related to the effectiveness of the treatment once the disease has been diagnosed (see Picone et al. (2004)).

In order to estimate the effect of PROGRESA on the demand for screening from non-eligible households we estimate the following demand equation with a probit model:

$$q_{ij}^{NE} = \phi(\beta' X_{ij} + \gamma T_j + \delta_1 W_j + \delta_2' H_j + v_{ij}) \tag{1.4}$$

X_{ij} includes household head's characteristics,[29] the proportion of eligibles in the village, the total number of households in the village and state fixed effects. H_j is a vector of the dummy variables that control for the type of provider in the village.

We are interested in estimating the indirect treatment effect of PROGRESA (ITE) on the demand for screening, namely the parameter γ[30] in eq. 1.4. Since waiting time affects the screening decisions of non-eligibles, and waiting time can be affected by the screening decisions of non-eligibles (as well as of eligibles), positive demand shocks will lead to longer waiting times, so that waiting time and the error term are positively correlated. Although the elasticity of demand for screening with respect to the waiting time is not relevant for our purposes, the endogeneity of W_j would threaten identification of the ITE if W_j and T_j are correlated. In our empirical analysis we account for this possibility by instrumenting the waiting time with variables that can affect it only through the supply side.

Three basic assumptions are needed to identify the ITE of PROGRESA on the demand for screening. First, we assume there are no spillover effects from treatment to control villages, so that the demand for health services is driven by whether they live in a treatment village or not and not by the status of other villages. Second, we assume a random assignment of villages into treatment and control groups. This is equivalent to assuming that whether a household is in a treatment or a control village is independent of unobservables that might affect the demand for health services. These two assumptions of no cross village spillovers and random assignment are standard requirements for the identification of the ITE[31] and are equivalent to assuming that non-eligibles in control villages provide a valid counterfactual for non-eligibles in treatment villages in terms of health service utilization. To provide support for the first assumption we note that villages were included in the evaluation data because they were geographically distant. With respect to the second assumption, it has already largely documented (Schultz (2004) and Berhman and Todd (1999)) that household and village characteristics do not significantly differ across treatment and control villages, which is consistent with the random assignment. Third, we assume that PROGRESA affects the health supply only through waiting time and composition of health providers. While this assumption might at first seem overly strong, in the next section we provide evidence to support it.

Since we cannot completely rule out the presence of pre-programme differences in the preva-

[29]These controls are household head's gender, age and its square, literacy status, whether (s)he speaks the indigenous language, the household poverty index, the household's size, number of children, whether there are females in the age group 25-64, and the proportion of women over 18 with a secondary school degree.

[30]In a probit model the marginal effect is a function of the coefficient as well as of the derivative of the conditional density function. For notational simplicity, in this work we will use the coefficient when referring to the marginal effect.

[31]See Angelucci and De Giorgi (2009) and Angelucci et al. (2007).

lence of a certain disease and/or the possibility to screen for it,[32] we estimate eq. 1.4 using a DD strategy.

1.4.3 — Baseline results

We first estimate the ITE of PROGRESA, as described in eq. 1.4, on four different outcomes: testing for cervical cancer, diabetes and hypertension and the probability of at least one visit to health centres. The marginal effect of the interaction between the treatment status of the village (T) and the survey round when PROGRESA was first in place (*98o*) presented in Table 1.4 measures the effect of the programme on the demand for screening of non-eligibles. PROGRESA leads to a 5.5% increase in the demand for cervical cancer screening among non-eligibles. Comparing this effect with the overall increase due to the programme shown in Table 1.2, suggests that the variation in health supply plays a fairly limited role in explaining the indirect effect of the programme on cervical cancer screening. Later in this section we provide a more accurate benchmark to assess the size of the demand effect with respect to the supply effect. We do not find any significant evidence of ITE on either demand for other types of screening or health centre attendance.

We now compare the ITE with the average treatment effect on eligibles, the so called treatment on the treated effect (TTE). Table 1.5 shows that there is a significant increase of over 20% in the probability of an individual undertaking the screening tests. The treatment effect for the eligibles on the probability of accessing a health centre is around 16%.[33] While there is no evidence of the effect of PROGRESA on the screening behaviour of eligibles, the TTE on health centre visits is consistent with previously documented effects of PROGRESA on health centre attendance.[34] The presence of a *SSA*, an *IMSS Solidaridad* clinic and a medical aid centre significantly increases the level of screening for both eligibles and non-eligibles. Due to the limited *IMSS* social insurance coverage, the presence of an *IMSS* clinic has either a negative or a not significant effect on the access to health services. We would expect the waiting time coefficient to be upwardly biased because of the reverse causality problem and this could account for the positive coefficient in Table 1.4.

We address possible endogeneity by instrumenting the waiting time with the average weekly opening days of health providers in the village, and its square. The validity of the instrument relies on two assumptions. First, that the opening days of a health service affect the screening probability only through the waiting time. Second, that the opening times do not change in response to the increased demand for health services driven by the programme. Potential violation of the first assumption would arise if health providers that are better equipped for the performance of screening tests are open on more days, but we found no evidence of this.[35] In

[32] This might be related to the distance from a bigger hospital where screening tests can be performed more accurately and more quickly.

[33] In March 1998 and October 1998 respondents were asked about health centre attendance in the previous 6 months.

[34] Gertler (2004) using hospital data found that in 1998 the number of visits was about 8% higher in treatment than in control villages.

[35] When we control for the type of providers that operate in the village we do not find any significant correlation between the number of services available in the village and the opening times either at the baseline of October

support of the second assumption, in the previous section we showed that there is no significant change in opening times once PROGRESA is implemented. Table 1.6 shows that when we control for the possible endogeneity of waiting time both the ITE and the TTE are in line with those discussed above.[36] These results can be read as evidence that the programme had a non-trivial indirect effect on the demand for female specific screening, compared with a small and insignificant effect on non-gender specific health outcomes. Since our ultimate goal is to understand how the indirect effect on the demand for cervical cancer screening is related to its gender specific nature, we need to explore a variety of mechanisms.

First, because of the income spillover from eligible to non-eligible households already documented in previous works (Angelucci and De Giorgi (2009)), the programme might have shifted upward the demand for health services from non-eligible households. Hence, women are being screened for cervical cancer more often just as result of the higher propensity to use health services among non-eligibles. While the lack of a significant effect on other health outcomes seems to exclude this explanation, we provide further evidence by testing whether the programme increased non-eligibles' expenditure on medicines and consultations with a doctor. The results in Table 1.7 do not show any significant evidence of ITE on health related expenditure. This finding is consistent with the results in Angelucci and De Giorgi (2009), which did not find any indirect effect on the consumption of durable goods. We also do not find any significant evidence of a treatment effect on the eligibles. This might be related to the fact that poor households receive medicines and treatment under the programme. Taken together, these results suggest that the significant response for non-eligibles in terms of cervical cancer screening is not due to an increased demand for health services generally, from non-eligible households.

So far we have assumed that PROGRESA would affect the supply of health services by increasing the amount of health provision. This is a restrictive assumption. As emerged from the description of the food and health component, PROGRESA might have improved the 'quality' of the health supply in treatment villages. In particular, since the programme is mainly targeted at pregnant and lactating women, doctors operating in treatment villages might have gained a better knowledge of female specific conditions either by attending training courses or receiving specific guidelines. This might explain the significant indirect effect on cervical cancer screening but not on other screening procedures. In order to investigate this opportunity, we estimate the ITE on two prenatal care outcomes: the number of checks during pregnancy and whether the pregnant woman was administered a tetanus vaccination. The rationale behind this is straightforward: if the programme has improved the ability of the doctors to deal with female specific issues, we should observe a change in pregnancy related outcomes as well. Results in Table 1.8 display a negative and insignificant indirect effect of the programme on pregnancy related outcomes, as opposed to a positive average treatment effect for eligibles. Another potential issue related to the quality of health providers might be related to the substitution of public care with

1998.

[36]In order to check the validity of the village waiting time as a measure of the health services, we allow the treatment effect to interact with the waiting time. The sign of the interaction is negative and significantly different from zero.

private care. Consistent with the findings of Gertler (2000), we do not find any evidence of a change of healthcare provider for non-eligible households.

We have shown that PROGRESA has a large and significant effect on the demand for cervical cancer screening from non-eligibles. We now try to assess the size of this demand effect compared to the supply effect. Table 1.3 shows that in control villages between October 1997 and October 1998 there was an increase in the number of villages with at least one major health provider and a reduction in the proportion of villages covered by a *SSA* hospital. In the same time interval, there has been a significant increase in the number of available services and a reduction in the average consultation fee. Since households living in control villages would receive the cash transfer only at the end of 1999, we study how screening behaviour varies over time in control villages in order to isolate the effect of changes in health supply due to the programme. In Table 1.9 we present the results of two specifications, the second of which includes a full set of supply controls at village level, including the distance from a *SSA* hospital and the number of services available.[37] In the first specification the time dummy captures variations related and unrelated to the programme. When we include the supply-side characteristics the estimated coefficient on the time dummy shrinks for all three screening tests. Reassuringly, the effect of supply is relatively small. For the group of non-eligibles changes in supply account for an approximate 1% increase in the probability of cervical cancer screening over time. This result allows us to conclude that the indirect effect of PROGRESA on the demand for cervical cancer screening is about five times greater than the effect of changes in supply. The coefficients of the distance from a *SSA* hospital and the number of available services are significant and have the expected sign.

Our results are robust to a variety of checks. In appendix A we present some results for specific checks. The lack of a significant effect on non-gender specific health outcomes might hide responses to the programme that vary differentially with the age and gender composition of the household. While systematic screening for cervical cancer is recommended mainly for women in the 18-30 age group, it is recommended that blood pressure and blood sugar levels are checked annually only after the age 45. In order to test whether the effect of PROGRESA on screening for high blood pressure and high blood sugar is greater for households that include at least one woman aged 45 or over, we split the sample of non-eligibles and estimate the model in eq. 1.4 for each separately. Table A1 shows that for both samples the ITE on blood pressure and blood sugar screening and health centre visits is not significantly different from zero. The ITE on cervical cancer screening is above 5% and strongly significant for both samples.

We also test the validity of the waiting time as the "price" of the screening services in the village. While the consultation fee calculated as the average of the prices paid by the entire village population would be a lower biased measure of the monetary cost paid by non-eligible households for health checks, we estimate eq. 1.4 taking village price as the average fee paid by the non-eligibles. Even though the marginal effect of the price should be interpreted with care,

[37] Villages with a *SSA* hospital not included in the evaluation sample might be closer to the control villages. We assume that the over time variation in the distance as measured in our sample is a good proxy for changes in the availability of *SSA* hospitals.

estimates of the ITE confirm our earlier conclusions (see Table A2).

To summarize, the evidence suggests that the magnitude of the ITE on the demand for cervical cancer screening is non-trivial. Also, the response to PROGRESA from non-eligible households is not driven by changes in the "quantity" or the "quality" of supply. Unlike existing studies on the indirect effect of PROGRESA on consumption, we found no evidence that the ITE is due to income spillovers from eligibles to non-eligibles. Understanding how this effect is related to the gender specific nature of the test is the objective of the next section.

§1.5. Transmission mechanisms

We formally test for two mechanisms (not necessarily exclusive) that might explain how PROGRESA has increased the demand for cervical cancer screening from non-eligibles, i.e. social norms and information sharing.

By social norm we mean that the individual decision to seek screening for cervical cancer is socially regulated. Descriptive evidence discussed in section 1.2.2 suggests that one of the most common reasons for women not having the PAP test is fear of their partners' reaction, especially if the test is performed by a male doctor.

By information sharing we mean that women who have been screened for cervical cancer might share information with their own peers about the existence and the nature of the PAP technology and also other information received from the doctors. This latter might include information about potential risk factors involved in lack of systematic screening and sexual behaviour.

Since both female status and information about cervical cancer risk factors are strongly correlated with socio-economic status, it is *a priori* difficult to distinguish between them. The strategy we adopt in this paper to separate the two exploits the substantial heterogeneity between male and female headed households in terms of female status and cervical cancer risk factors. In this section we show that while female respondents in male headed households display higher levels of formal education and lower risks of contracting cervical cancer than those in female headed households, the latter are more emancipated and less likely to be affected by the gender bias. Therefore, if PROGRESA affects the screening decisions of non-eligibles by reducing the cost of the social norm, we would expect this effect to be stronger in the sample of male headed households. On the other hand, if the programme generates information spillovers about different aspects of cervical cancer screening, the indirect effect should be stronger in the sample of female headed households.

At the baseline 14% of the 11,558 non-eligible households are female headed of which 71% are widows. First, we test whether the screening behaviour of male and female headed households systematically differs and whether the ITE effect of PROGRESA on screening behaviour varies with the gender of the household head. The results in Table 1.10 show that the dummy that controls for the gender of the household head has a large and significant negative effect on the probability of screening for cervical cancer. While living in a female headed households increases the probability of screening for high blood pressure, it is negatively correlated with the probability of screening for diabetes. In both cases the estimated marginal effects are much smaller than for cervical cancer screening. This result can be interpreted as evidence that there are no systematic differences in the screening for non-gender specific diseases between male and female headed households.

The ITE effect of PROGRESA on the probability of screening for cervical cancer does not vary significantly with the gender of the household head and is large and significant for both male and female headed households. Results for blood pressure and blood sugar tests confirm that the programme has no indirect effect on the probability of screening for non-female specific screening diseases, irrespective of the gender of the head of household.

We also examined how observable characteristics correlated with the risk of contracting the cervical cancer are distributed in the sample of male and female headed households. The upper panel of Table 1.11 shows that female heads are on average older and less educated. Male and female headed households are not significantly different when measured on the poverty index and in terms of the proportion of households where the household head speaks the indigenous language.[38] The proportion of women in the age group more at risk of contracting cervical cancer is significantly higher in female than in male headed households. In female headed households the percentage of women age 18 or over who have completed secondary education is 7% versus 13% in male headed households. The results in the lower panel of Table 1.11 show that female heads are more likely to have never used any form of contraception and to have had more pregnancies. There is no significant difference in the probability of having been screened for cervical cancer in the past.

While so far we have assumed that women in female headed households are not affected by the gender bias in the decision to screen for cervical cancer, the PROGRESA dataset contains measures of female emancipation and allows us to measure how they correlate with sex related behaviour. In the March 1998 wave all female respondents were given a set of questions about women's status. In particular, they were asked if they agreed or disagreed with the following statements: i) Woman‘s place is in the house; ii) Women have to obey men; iii) Women have their say in community issues; iv) Women should have a job outside the house; v) Women should have same rights as men; vi) Women should have their own opinions. We converted the answers to these questions into dummy variables and derived a Female Status (FS) index ranging between 0 and 6, where 6 represents the lowest degree of female emancipation. Figure 1.2 shows the distribution of this index by gender of the household head for the group of non-eligible households. As expected, female heads display, on average, higher levels of emancipation than females in male headed households.[39] Table 1.12 shows that the FS index is significantly correlated with PAP testing and use of contraceptive methods (as reported in March 1998) only

[38] As a further check on whether male and female headed households differ in the overall distribution of observable characteristics potentially correlated with the decision to screen, we estimate a propensity score based on observable characteristics elicited either on October 1997 or March 1998. Figure A1 reports the results for the group of non-eligibles. Overall female and male headed households seem quite balanced in terms of household and village characteristics.

[39] Both a Pearson chi square test and a Kolmogorov Smirnov test strongly reject the hypothesis that the two samples are drawn from the same distribution.

for the sub sample of male headed households.

On the assumption that the level of emancipation of the female respondent is a good proxy for the average level of independence of the women in the household, these results suggest that women in female headed households, while less educated (potentially less informed) and more at risk of contracting cervical cancer are, on average, not affected by the gender bias towards screening for cervical cancer. In the next section we propose a simple model of social norm diffusion. We derive and test implications for how PROGRESA might have affected the social acceptability of the PAP test. In section 1.5.3 we test for the presence of knowledge spillovers.

1.5.1 — Social Norm

We first propose a simple model of screening behaviour with social regulation. Our characterization of social norm is close to those proposed by Munshi and Myaux (2006) and Kandori (1992). While the long run equilibrium of social norm diffusion is characterized in detail in these models, the objective of our framework is to show how PROGRESA affects the strength of the social norm that regulates cervical cancer screening, in the short run.

Consider a village consisting of a continuum of women. At the beginning of each period a woman can choose between two actions: screening for cervical cancer (s) and not screening (ns). When screening behaviour is socially regulated, its payoff depends not only on the intrinsic utility that she derives from screening but also on the social pressures or sanctions that accompany it. Within each community the cost associated with the social norm, l_i, varies across women and is assumed to be normally distributed with $l_i \sim N(\bar{l}, \sigma^2)$. In our framework the individual's payoff depends on her action, as well as on her peer action.

We assume that in each period each woman can only be matched with one other woman in the village. Before implementation of the programme households that are potentially eligible and those that are not eligible for a cash transfer have the same payoffs, which correspond to four combinations of actions:

$$V_i(s, s) = w \tag{1.5}$$

$$V_i(s, ns) = w - l_i \tag{1.6}$$

$$V_i(ns, ns) = 0 \tag{1.7}$$

$$V_i(ns, s) = 0 \tag{1.8}$$

V_i is the payoff for woman i, where the first term in parentheses refers to the woman's own action and the second term refers to her peer's action (for simplicity we assume that the payoff for cervical cancer is constant); and l_i is either the husband's reaction or simply her fear of his reaction. The underlying intuition is that husbands would punish their wives if their behaviour does not conform to the behaviour of most of the community. We assume that the expected loss of utility from the decision not to screen is equal to 0, independent of peer action.[40] In each

[40] Alternatively we could assume that there is social reward for the woman who decides not to screen and is

village there is a fraction P of women who undergo screening for cervical cancer, where P is given by:

$$P = \mu p^E + (1-\mu)p^{NE} \tag{1.9}$$

μ is the exogenous fraction of village households potentially eligible for the PROGRESA transfer, p^E is the average screening probability of women eligible for the cash transfer; p^{NE} is the average screening probability of women not eligible for the cash transfer. Every woman will opt for screening if

$$Pw + (1-P)(w - l_i) \geq 0 \tag{1.10}$$

When PROGRESA is introduced, the expected payoff for women eligible for the PROGRESA transfer increases ($w^{'} > w$) in treatment villages, but not in control ones. As a result, p^E only increases in treatment villages. Among non-eligible women those with $l_i \leq l^{*NE}$ will screen for cervical cancer, with l^{*NE} given by:

$$l^{*NE} = \frac{w}{(1-P^*)} \tag{1.11}$$

and P^* given by:

$$P^* = \mu p^E + (1-\mu)\int_{-\infty}^{\frac{w}{(1-P^*)}} \phi(l)dl \tag{1.12}$$

Taking the derivative of l^{*NE} with respect to P^* in eq. 1.11 and applying the implicit function theorem to eq. 1.12, we can derive how the equilibrium social norm of non-eligibles varies in response to an increase in the screening probability of eligibles:

$$g \equiv \frac{\partial l^{*NE}}{\partial p^E} = \frac{w}{(1-P^*)^2} * h(P^*, w, \mu, \bar{l}, \sigma^2) \tag{1.13}$$

where $h(\cdot)$ is a positive function with the following properties: 1) $\dfrac{\partial h}{\partial \mu} > 0$; 2) $\dfrac{\partial h}{\partial \mu \partial \bar{l}} > 0$. Function $h(\cdot)$ describes how the fraction of women in the village that go for screening, P, changes as result of changes in the screening probability of eligible women, p^E. Previous work on social norms (see Munshi and Myaux (2006)) has modeled social norm diffusion as a learning process over time: people gradually learn about P, constantly updating their priors. In our case, even though women living in treatment villages had no information about the pre-programme screening rate in the village, they know how it varies because of the programme. Between October 1997 and August 1998, PROGRESA held public meetings where the eligibility of each household and the conditionalities were spelt out. Moreover, after the programme started a community outreach worker, known as the *promotora*, chosen from among the eligibles, was responsible for providing information about the programme throughout its duration.[41] Therefore, all the

matched with a woman who does have the test (Munshi and Myaux (2006)).

[41] Even though the promotora was mainly meant to be contacting beneficiaries, Adato et al. (2000) reports

non-eligible women in the treatment villages were informed about who was required to undergo PAP testing as part of the conditionalities of the cash transfer.

The model has two testable implications. PROGRESA increases the screening probability of non-eligibles by shifting upward the threshold value of the cost of the social norm, below which non-eligible women screen for cervical cancer. The bigger the fraction of households eligible for the programme, the greater the size of the shift. The variation in the indirect effect driven by the proportion of eligibles should be stronger in those communities where the social norm cost is higher.

In order to test these implications we estimate the model in eq. 1.4 allowing the ITE to vary with the proportion of eligibles in the village:

$$q_{ij}^{NE} = \phi(\beta' X_{ij} + \gamma_1 T_j + \gamma_2 rp_j + \gamma_3 T_j * rp_j + \delta_1 W_j + \delta_2' H_j + v_{ij}) \tag{1.14}$$

where rp_j is the ratio of eligibles in the village j.[42] The evidence provided in the previous section suggests that before the programme women's emancipation levels in male headed households are significantly correlated with the decision to have screening, but not in female headed ones. Therefore, we expect γ_3 to be higher for male than for female headed households. Further, we exploit variations in the *Female Status* index at village level to test whether γ_3 is higher in those communities where the average social norm is stronger.[43]

So far we have exploited variations in the gender of the household head, but the cost of the social norm might vary across male headed households. In particular, enforcement of the social norm might be stronger in those households with more conservative male heads. If so, the ITE should vary with the characteristics of the household head in the group of male headed households, but not in the female headed ones. An enforcement mechanism could be based on internalization of the norms of proper conduct (see Young (2007)). Although female widows need no longer fear their partners' reactions, they might have internalized their dead husbands' opposition to cervical cancer screening. Therefore the indirect effect should be stronger among widowed female heads than others.

Table 1.13 show how the ITE varies with the proportion of eligibles in the village. Consistent with our theoretical predictions, the ITE increases significantly with the proportion of eligibles in the village. On average, a 10% increase in the proportion of eligible households determines a 1.4% increase in the ITE. When we estimate our model separately for the sample of male and the sample of female headed households, we find that the interaction is strong and significant only in the sub-sample of male headed households.[44]

In order to test whether the response to the programme varies across villages according to level of female emancipation, we split the sample of villages according to the average value of the

show that there were also frequent interactions with non-beneficiaries.

[42]The ratio varies between 0.19 and 1 with the average being 0.59.

[43]While the value of the *Female Status* might not necessarily reflect the severity of the penalties inflicted by husbands, we assume that the average level of female emancipation is a good proxy for the strength of the social norm in the community.

[44]We rule out the possibility that the differential effect between male and female headed households is driven by an age effect. Estimates on a sample of household heads aged 60 or under show similar results.

Female Status index and estimate the model in eq. 1.14 for each tercile. In the first column of Table 1.14 (*High*) the analysis is restricted to those villages where women are on average more emancipated. While the ITE is strong and significant, its interaction with the proportion of eligibles in the village is not significantly different from zero. In the second column, (*Medium*), where the analysis includes only those villages with an average level of female emancipation, neither the ITE nor its interaction with the proportion of eligibles, is significant. In the last column of Table 1.14 (*Low*) only women who live in the least emancipated villages are included. We find that the interaction between the ITE and the proportion of eligibles is strong and significant. This result could be taken as evidence that the effect of the programme on the social norm is convex with respect to its strength. The presence of an ITE that does vary significantly with the proportion of eligibles in the villages characterized by high levels of female emancipation suggests that mechanisms other than the social norm might explain the ITE on cervical cancer screening.

Next we look at how the size of the ITE varies with the characteristics of the household head. As mentioned earlier, these characteristics could proxy for the strength of the social norm in male headed households, but not in female headed ones. There is a large body of medical literature (see Stephens (2005) for a summary of the existing evidence) documenting how difficult it is for the indigenous female population to access health services. In order to test whether these difficulties are related to restrictions imposed by the husband, in the first specification we interact the treatment effect of the non-eligibles with the dummy for the household head speaking the indigenous language or not.[45] We find that the interaction of the treatment effect with the indigenous status dummy has positive, non-trivial and significant marginal effects for the sample of male headed households, as opposite to negative and not significant effects for the sample of female headed households. In the second specification we interact the treatment effect with a dummy variable that takes the value 1 if the household head is over 60 and 0 otherwise. Again, the marginal effect on the interaction is positive and significant only for the sub sample of male headed households. Finally, we allow the treatment effect to vary with a dummy variable for whether the head of household is illiterate or not and, consistent with the previous results, we find that the interaction is strongly significant only for the sub sample of male headed households. In principle, the variables we interacted with the treatment effect might not necessarily proxy for the strength of the social norm within the household. However, explanations other than the social norm-based one would have shown the interactions to be significant also for the sub sample of female headed households.

Last, we study whether in the group of female headed households, the indirect effect of the programme varies with the household head being a widow or not. The results in Table 1.16 show there is a sizeable and significant effect for widows, and a smaller and insignificant effect for non-widows.[46] Taken together, these results suggest that PROGRESA weakens the social

[45] 9 million Mexicans have indigenous origins and live mainly in dispersed rural areas. Among the indigenous population, the rate of illiteracy among females is almost double that for males.

[46] Consistent with our social norm based explanation when we allow the treatment effect to interact with the *Female Status* index the interactions is positive and significant only for the sample of widows.

norm related to male opposition to women being screened for cervical cancer. The effect is particularly strong for women in male headed households, but women living in less emancipated female headed households also benefit from the increased social acceptability of the PAP smear test.

1.5.2 — Long Run Evidence on Social Norm

The objective of this section is to provide long run evidence on the effect of *Progresa* on the social norm that regulates the decision to screen for cervical cancer. While the model presented in section 1.5.1 is completely static, it is pretty straightforward to derive its dynamic implications. In localities that have been exposed longer to the program there is a higher fraction of women screening for gender specific conditions and, therefore, a lower probability of matching with peers who do no screen. In other words, the cost of the social norm should be lower for women who live in localities that received the program earlier.

Throughout the paper we have suggested that husbands' opposition to their women being screened for cervical cancer might be related with the gender of the doctor. In line with a norm based explanation, we expect the program to have a stronger effect in those localities where there is a higher fraction of male doctors. The evaluation data do not contain any information on the gender of the doctors. This information is available in the 2007 survey.

In order to evaluate the effect of exposure on female screening decisions, we restrict the sample to the localities included in the original evaluation sample and those that were selected to act as control group in the 2003 survey. The latter group of localities was chosen in such a way to match the observable characteristics of the localities included in the original evaluation sample. Although not experimental, this sample represents the best possible choice to evaluate the long run effects of program on individual behavior. Between the group of localities included in the evaluation sample and those added in 2003 there is a considerable variation in terms of when the program was implemented: while in the first one, the program was implemented at the latest in November 1999, the second started receiving it only in 2004 or afterwards. We create a dummy variable that takes value 1 if the locality belonged to the original evaluation sample, 0 if it was among those that were chosen as controls in 2003. Preliminarily, we check how observable characteristics, as elicited in 2007, are correlated with the exposure dummy. Since questions about cervical cancer are only addressed to women younger than 50, the top panel of table 1.17 reports the mean and the standard deviations of demographic characteristics of women aged 18-50. Characteristics are not perfectly balanced across the two groups. Among those reported,[47] literacy displays a difference that is statistically at 5% significance level: almost 87% of the women living in later exposure localities are literate, as opposed to 80% in those living in earlier exposure.

When we look at household assets, in localities belonging to the original evaluation sample there is a significantly higher fractions of households with radio (19% versus 11%). Quality of health centers seems to be higher in those localities that received the program after 2004. Table

[47] Similar patterns hold for those not reported.

1.17 shows that localities where the program started later display, on average, a higher number of doctors and nurses, and a longer doctors' tenure. In 77% of the localities that received *Progresa* in 2004 or afterwards there is at least one health center that offer the cervical cancer screening service, as opposed to 69% in early exposure ones.[48] However, for none of the variables the difference is statistically significant from zero. The same pattern holds when we look at the characteristics of doctors (bottom panel of table 1.17). In the group of localities added in 2003 there is a higher fraction of doctors who completed postgraduate studies (36% versus 20%) and a higher fraction of doctors who advise their patients to screen for cervical cancer at least once every two years (82% versus 74%). The fraction of doctors who advise a mammogram on biannual basis is practically the same for the two groups.[49] Also in this case the differences between the two groups of localities are not statistically different from zero. In summary, the differences, if any, in health supply characteristics between early exposure localities and later exposure ones should determine higher screening rates in the latter ones.

In order to test whether longer exposure to *Progresa* affects the propensity to screen among women younger than 50 and whether the effect varies according to the proportion of male doctors operating in the locality we estimate two specifications. In the first one, presented in the odd columns of table 1.18, we regress the decision to screen on the dummy for whether the locality belongs to the original evaluation sample or not. In the second specification, presented in the even columns of table 1.18, we add a control for the proportion of male doctors in the locality and we allow for this variable to interact with the exposure dummy. All the specifications control for the following socioeconomic variables: dummies for being literate, indigenous, head of household, for completing primary, and secondary or higher school, number of kids alive, a dummy for working the weak before the interview, whether the woman was sick in the last four weeks, whether in the house there is a television and a radio. The regressions also control for state fixed effects and for a set of health supply characteristics in the locality: the type of health provider, number of doctors, number of nurses, and the total number of families that have registered with at least one health provider in the locality. The latter variable allows to control for possible congestion effects. In localities with more than one health center we might potentially match each individual with the center they attended. However, the decision to attend a specific center might be driven by characteristics that are correlated with the strength of the social norm. Nevertheless, it is unlikely that women would travel to other localities when at least one health center operates where they live.

In table 1.18 we present results for the female propensity to undertake 5 screening exams: PAP test, mammogram, and tests for hypertension, diabetes and cholesterol. For the two female specific screenings we find very similar patterns. A longer exposure to *Progresa* increases the probability of screening for cervical (breast) cancer by 0.14 (0.06). A higher fraction of male doctors is associated with a significantly lower probability to screen for female specific conditions. Most important for us, the effect of the exposure dummy tends to be significantly

[48] We do not consider mobile health units in table 1.17.

[49] According to the latest guidelines, Mexican women aged 40-49 should be screened once every two years, and once a year if they are 50 or older.

stronger in those localities where there is a higher fraction of male doctors. When we look how *Progresa* affects non-gender specific screenings, the coefficients are very small and not significantly different from zero. The effect does not seem to vary with the proportion of male doctors, as the coefficients on the interaction term are negative, very small and never statistically different from zero.[50]

The results presented in this section, although not experimental, provide strong support the hypothesis that *Progresa* weakens the social norm that regulates the decision to screen for female specific conditions.

1.5.3 — Information sharing

Our final analysis tests whether learning contributes to explaining the indirect effect of PROGRESA in the decision to screen for cervical cancer. As already mentioned, a social norm based explanation does not exclude the fact that there are other influences. Women who take the PAP test to comply with the programme conditionalities might potentially share information about different aspects of cervical cancer screening. Eligible women might inform their peers about the existence of PAP technology and the experience of the actual test. They might pass on information provided by the doctor about risk factors and recommended frequency of screening. In the absence of direct questions about cervical cancer screening we are not able to separate these two channels.

In order to test whether eligible women share information with non-eligible women we rely on a prediction common to many social learning models in the literature. If there is learning through peers the learning externality should be bigger for those individuals who have less accurate initial information.[51] Testing this prediction poses many difficulties. First, the set of neighbors from whom a woman might learn about cervical cancer is difficult to define. Then, once a meaningful group has been chosen, we have to deal with the problems described in the literature that tries to identify peer effects,[52] namely the presence of so called *correlated effects* (e.g. shocks that affect the network as a whole[53] and creates spurious correlation between individual decisions and peer actions) and produces the *reflection* problem (within a peer group every individual‘s behaviour affects the behaviour of the others and it is impossible to distinguish whether one’s action is the cause or the outcome of peer influence).[54] Finally, we need to find a suitable proxy for the initial level of information about cervical cancer. How we deal with these problems and how we specify and test our model is described below.

We define our main network grouping as all those households in the village whose eldest

[50]In alternative specifications we add controls for experience, age, additional qualifications and working hours of the doctors, but the results are almost identical to those presented.

[51]Bandiera and Rasul (2006) and Conley and Udry (2005) test this implication in a study of whether farmers' decisions about whether or not to adopt a new technology are affected by their neighbors' decisions.

[52]See among others Manski (1993) and Sacerdote (2000).

[53]For example, all households in a particular network might be under the care of a not very well informed doctor. This is comparable to the teacher effect in schooling decisions (see De Giorgi et al. (2007), Hoxby (2000)).

[54]See Manski (1993).

child is in the same school grade, as recorded in the October 1997 wave.[55] While this might be only one of several possible network groupings,[56] there are good reasons for believing it to be appropriate for studying cervical cancer screening decisions. First, Cattaneo and Lalive (2006), studying the indirect effect of PROGRESA on schooling decisions, argue that there might be substantial exchange of information among parents of children in the same grade. Second, given the small number of households in each village (the average is 67), mothers with children in the same grade are not only likely to be of similar age, but since they obviously were pregnant at the same time, they may have exchanged information on sex related issues. The average size of peer groups defined in this way is relatively small (5.42 households), and the average age of the household head is younger than in the full sample (43 versus 47 years). Since it is beyond the scope of this work to determine the most relevant network for the sharing of cervical cancer information, in the appendix we test the robustness of our results with alternative definitions of peer groups.

In order to identify the *endogenous* peer effect in cervical cancer screening decisions we rely on the so called *partial population experiment* introduced by Moffitt (2001): a policy intervention targeted at a subgroup of the population allows identification of the endogenous peer effect in the form of an exclusion restriction.[57] Because of the PROGRESA conditions, the proportion of women who go for screening in each group exogenously increases in the treatment, but not in the control villages.

We use information about whether the female respondent has ever used contraception methods (as recorded in March 1998), as a proxy for initial information about cervical cancer. According to the medical literature the only contraception method that reduces the risk of contracting the HPV virus through sexual contact is the condom. However, in our sample only 2% of non-eligible women professing to use contraception relied on this method. For the purposes of this analysis, we interpret use of contraception as an indicator of knowledge about sex related issues.[58]

We estimate the following equation using a probit model:

$$q_{igj}^{NE} = \phi(\alpha + \beta x_{igj} + \gamma_1 \overline{Q}_{gj} + \gamma_2 c_i + \gamma_3 \overline{Q}_{gj} * c_i + \delta_1 W_j + \delta_2' H_j + u_{igj}) \tag{1.15}$$

In our framework, q_{igj}^{NE} takes the value 1 if a woman living in a non-eligible household i belonging to peer group g in village j, undergoes screening for cervical cancer,[59] x_{igj} is a set of household characteristics, $\overline{Q}_{gj}$ is the average screening rate for group g in village j, c_i takes the value 1 if the female respondent in household i has never used any method of contraception. W_j

[55]This rules out the possibility that the size of the group is affected by the increased incentives to enroll children in school.

[56]Direct data on information sharing are typically unavailable. Most of the literature makes assumptions based on different proximity criteria. Exceptions include Conley and Udry (2005).

[57]Bobonis and Finan (2006) and Cattaneo and Lalive (2006) follow this identification strategy to estimate the impact of peer effects on the schooling decisions of children living in non-eligible households.

[58]Among the women who answered no to the question about whether or not they wanted another child, 69% stated they were not practising any form of contraception.

[59]For simplicity, we suppress the index t.

and H_j are controls for health supply, described earlier in the paper. In this specification, γ_3 shows how the peer effect varies with the proxy for initial information about cervical cancer. A learning based explanation would imply that the interaction is positive and significantly different from zero.

So far we have argued that use of a contraceptive method proxies for the quality of women's initial information on cervical cancer. We might also expect use of contraception to be correlated with female emancipation. The results in the previous section show that the use of contraception is strongly correlated with the level of female emancipation in male headed, but not in female headed households. The latter display lower levels of formal schooling. Overall, this evidence suggests that if our model is effectively capturing a learning externality, we would expect γ_3 to be significantly bigger for the sample of female headed households.

In order to control for the possible endogeneity of $\overline{Q}_{gj}$, we use an Instrumental Variables approach. Our strategy recognizes that within each peer group there might be variability in the number of payments received by the eligibles. Since payments are correlated with the number of medical check-ups attended by the eligible household member(s), a higher average number of payments determines higher exposure of the peer group to medical treatment. Therefore, we use as instruments the treatment effect ($T_j * 98o$), the average number of payments received by the eligibles in the peer group at October 98 ($\overline{NP}_{98o}$), and their interaction ($T_j * 98o * \overline{NP}_{98o}$). This strategy allows identification of endogenous social interactions based on within-village variation in the number of payments across groups. Results are reported in Table 1.19.

The first column of Table 1.19 presents the estimates for the full sample and columns 2 and 3 report the results for the male and the female headed samples respectively. A 10% increase in the proportion of women undergoing screening in the peer group increases the probability of screening for cervical cancer by 4.9%.[60] The magnitude of the marginal effect is similar to that estimated by Cattaneo and Lalive (2006) when measuring peer effects in schooling decisions. In households where the female respondent has never used contraceptive methods the screening probability is significantly lower and, more important for our purposes, the peer effects are stronger. Consistent with our learning based explanation, the interaction between average screening rate and contraceptive use is sizeable and significant only for the sub sample of female headed households. We do not put too much emphasis on the magnitude of the learning externality as this might be downward biased. In fact households with older heads are excluded from the sample because of our peer group definition. While we cannot assess the size of the learning externality, these results show that there is a significant knowledge spillover providing greater benefit to those women who are more at risk.

The validity of our identification strategies relies on the assumption that the only way that PROGRESA affects the response in terms of screening of non-eligibles is by exogenously increasing the screening rate of eligibles. The results in section 1.4.3 rule out the possibility of income spillovers from eligibles to non-eligibles, but there may be other mechanisms threatening our identification strategy. First, in the presence of complementarities between consumption and

[60]Consistent with the baseline results, we do not find evidence of peer effects on screening for high blood pressure and diabetes and health centre visits.

health status, rates of cervical cancer screening might increase as a result of the positive indirect effect of the programme on consumption documented by Angelucci and De Giorgi (2009) and Angelucci et al. (2007). The lack of a significant ITE on health outcomes other than cervical cancer screening, provides enough elements to discard this explanation. Second, Grossman (1972) predicts that the investment in health might increase as a response to an increase in wages. Since one objective of the programme is to reduce child labor, a general equilibrium effect might, in principle, determine an increase in the demand for female work, with a consequential increase in wages. Neither Angelucci and De Giorgi (2009) nor Skoufias and Di Maro (2006) find any evidence in this direction. Last, non-eligible women might learn about cervical cancer not from their peers but by attending the health talks we discussed earlier in the paper. Lack of information about attendance at these events does not allow us to quantify this potential bias.

Our results are robust to a variety of robustness checks. First, we use the variation in the screening rate for high blood pressure among the eligibles as an alternative instrument for $\overline{Q}_{gj}$. This is a direct measure of screening service utilization and has the advantage that is not affected by the social norm. The results are perfectly in line with those described above. Second, we address the concern that our results might be driven by how our peer group is defined. Table A3 in appendix A presents estimates of the eq. 1.15 using as the relevant peer group all households where the eldest child is within a 2 year range difference. While marginal effects are estimated less precisely, the main conclusions do not change.

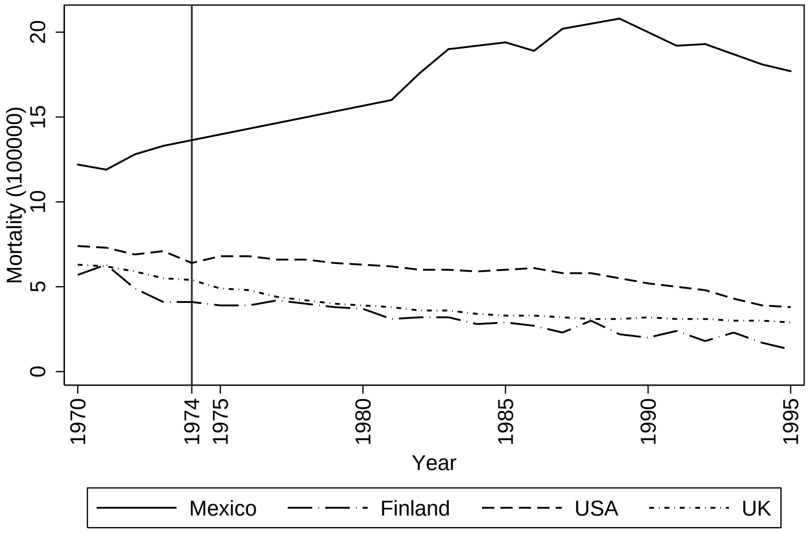
§1.6. Conclusions

In this paper we presented evidence from the PROGRESA social assistance programme on whether including cervical cancer screening among the conditions for receipt of cash transfers affects the screening decisions of women living in non-eligible households. Our main finding is that PROGRESA has a positive indirect effect on the demand for cervical cancer screening, but not on non-female specific health outcomes. Our results suggest that PROGRESA affects the screening decision of non-eligible women through two channels. First, PROGRESA weakens the social norm related to the opposition of household males to women being screened by male doctors. By exogenously increasing the proportion of eligible women who are required to be screened in order to meet the programme's requirements, PROGRESA increases the social acceptability of the PAP screening test. Our results suggest that this channel is quantitatively important for male headed households and households headed by more conservative females. Second, the programme generates an information spillover from eligible to non-eligible households, which mainly benefits women living in female headed households who are less well-informed and more at risk of contracting the disease.

There are three policy implications from our findings that could apply both to developing and developed countries. First, the design and evaluation of national screening programme should explicitly take account of potential externalities from eligible to non-eligible households. Evaluation of a programme's benefits might change substantially if externalities are considered.

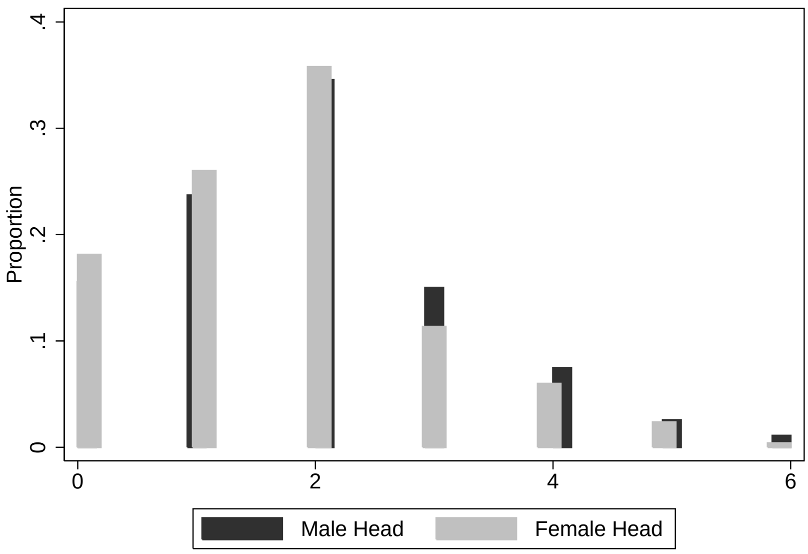
Second, cultural barriers should be addressed explicitly if the programme is to be effective. Increasing the proportion of female health professionals in areas with a high proportion of ethnic and religious minorities for many women might act as a strong incentive for systematic screening. Third, information plays a crucial role in cancer prevention. Our results show that information received from peers might be as important as the information received from health professionals.

Figure 1.1: Cervical cancer mortality in selected OECD countries



The vertical line shows the start of the CCSP in Mexico. Age adjusted standardized rate
Source: OECD Health Data 2006

Figure 1.2: Proportion of Male and Female headed households by *FS* Index



The Female Status index ranges between 0 and 6 where 6 denotes least emancipated women

Table 1.1: Cervical Cancer Screening in selected OECD countries in Year 2002

Country	Participation (%)
Belgium	61.0
Canada	74.9*
Finland	71.8
France	74.9*
Germany	55.9
Iceland	74.0
Mexico	38.9
Netherlands	66.3
New Zealand	72.7
Sweden	72.0
United States	84.8**

Note: * refers to year 2003, ** refers to year 2001. The table reports the percentage of female population age 20-69 that got screened in the last 3 years.

Table 1.2: Descriptive evidence on screening rates

Mean, standard error in parenthesis clustered by village

	Eligible			Non-eligible		
Village Status	**Treatment**	**Control**	**DIFF**	**Treatment**	**Control**	**DIFF**
Cervical Cancer						
Mar 98 (1)	0.256	0.275	-0.018	0.319	0.354	-0.035
	(0.437)	(0.446)	(0.021)	(0.466)	(0.478)	(0.021)
Oct 98	0.361	0.219	0.142***	0.263	0.246	0.017
	(0.480)	(0.414)	(0.021)	(0.440)	(0.431)	(0.016)
May 99	0.391	0.246	0.144***	0.288	0.310	-0.022
	(0.488)	(0.431)	(0.018)	(0.453)	(0.463)	(0.022)
Oct 98-May 99 (2)	0.577	0.382	0.195***	0.462	0.434	0.028
	(0.494)	(0.486)	(0.024)	(0.499)	(0.496)	(0.020)
Difference (2)-(1)	0.320***	0.108***	0.213***	0.144***	0.080***	0.063**
	(0.008)	(0.010)	(0.022)	(0.009)	(0.011)	(0.020)
Sugar Blood						
Mar 98 (1)	0.251	0.251	-0.000	0.307	0.299	0.008
	(0.434)	(0.434)	(0.019)	(0.461)	(0.458)	(0.020)
Oct 98	0.402	0.251	0.150***	0.337	0.319	0.018
	(0.490)	(0.434)	(0.021)	(0.473)	(0.466)	(0.019)
May 99	0.410	0.275	0.134***	0.335	0.353	-0.018
	(0.492)	(0.447)	(0.020)	(0.472)	(0.478)	(0.022)
Oct 98-May 99 (2)	0.620	0.416	0.204***	0.540	0.522	0.018
	(0.485)	(0.493)	(0.024)	(0.498)	(0.500)	(0.020)
Difference (2)-(1)	0.369***	0.165***	0.204***	0.233***	0.223***	0.010
	(0.008)	(0.010)	(0.023)	(0.009)	(0.011)	(0.018)
Blood Pressure						
Mar 98 (1)	0.392	0.382	0.010	0.456	0.456	0.001
	(0.488)	(0.486)	(0.022)	(0.498)	(0.498)	(0.020)
Oct 98	0.521	0.334	0.187***	0.449	0.428	0.021
	(0.500)	(0.472)	(0.023)	(0.497)	(0.495)	(0.020)
May 99	0.526	0.368	0.158***	0.455	0.450	0.005
	(0.499)	(0.482)	(0.021)	(0.498)	(0.498)	(0.023)
Oct 98-May 99 (2)	0.747	0.541	0.206***	0.673	0.645	0.028
	(0.435)	(0.498)	(0.024)	(0.469)	(0.479)	(0.020)
Difference (2)-(1)	0.355***	0.159***	0.196***	0.217***	0.189***	0.028
	(0.008)	(0.011)	(0.022)	(0.009)	(0.011)	(0.018)

Note: *** denotes significance at 1%, ** at 5% and * at 10%. The sample includes both male and female headed households. The screening indicator takes value 1 if at least one household member has been screened. In March 1998 the question refers to the previous 12 months. In October 1998 and May 1999 to the previous six months. Oct 98-May 99 is the cumulative probability. Standard errors on the differences are derived from an OLS regression, estimated on eligible and non-eligible separately.

Table 1.3: Descriptive evidence on health supply

Proportion of villages covered by type of providers

	October 97			October 98		
Village Status	**Treatment**	**Control**	**DIFF**	**Treatment**	**Control**	**DIFF**
SSA clinic	0.079	0.130	-0.051*	0.097	0.108	-0.010
	(0.271)	(0.338)	(0.028)	(0.297)	(0.311)	(0.028)
IMSS Solid.	0.038	0.043	-0.006	0.028	0.022	0.007
	(0.191)	(0.204)	(0.018)	(0.166)	(0.145)	(0.015)
IMSS	0.003	0.000	0.003	0.003	0.011	-0.008
	(0.056)	(0.000)	(0.004)	(0.056)	(0.103)	(0.007)
Private Doctor	0.000	0.000	-	0.006	0.022	-0.015
	(0.000)	(0.000)	-	(0.079)	(0.145)	(0.010)
Health Aid	0.571	0.641	-0.070	0.633	0.602	0.031
	(0.496)	(0.481)	(0.045)	(0.483)	(0.491)	(0.045)
Mobile Unit	0.769	0.712	0.057	0.809	0.801	0.008
	(0.422)	(0.454)	(0.040)	(0.394)	(0.400)	(0.037)
Any of the providers	0.915	0.914	0.001	0.944	0.941	0.003
	(0.279)	(0.281)	(0.026)	(0.231)	(0.237)	(0.021)

Health indicators by village

	March 98			October 98		
	Treatment	**Control**	**DIFF**	**Treatment**	**Control**	**DIFF**
Services available	2.358	2.454	-0.096	3.131	3.065	0.067
	(1.964)	(2.043)	(0.184)	(2.273)	(2.241)	(0.209)
Opening days	5.567	5.512	0.055	5.285	5.349	-0.064
	(0.783)	(0.705)	(0.070)	(0.832)	(0.784)	(0.075)
Opening hours	10.403	10.119	0.284	9.225	9.232	-0.006
	(3.019)	(2.829)	(0.272)	(2.144)	(2.493)	(0.210)
Waiting time	55.871	58.139	-2.268	56.048	58.477	-2.429
	(23.494)	(24.230)	(2.195)	(19.813)	(19.090)	(1.804)
Visit duration	19.151	19.775	-0.623**	19.134	19.157	-0.022
	(3.169)	(3.067)	(0.289)	(3.304)	(3.357)	(0.307)
Visit fee	11.057	11.988	-0.930	5.475	9.769	-4.294***
	(10.021)	(10.166)	(0.931)	(7.035)	(10.730)	(0.792)

Note: *** denotes significance at 1%, ** at 5% and * at 10%. The number of main services available is obtained from a list of 7 services in the locality questionnaire. The remaining indicators are averages of the individual responses. Visit durations and waiting times are expressed in minutes. Consultation fees are expressed in pesos at October 1997 values. Standard errors on the differences are derived by an OLS regression.

Table 1.4: PROGRESA and the demand for health, DD for non-eligibles

Marginal effects from probit estimations, standard errors are clustered by village

	Cerv. Cancer screening	Blood Sugar screening	Blood Pressure screening	Health Centre visit
98o	0.125***	0.210***	0.188***	-0.051***
	(0.011)	(0.010)	(0.010)	(0.010)
T	0.001	0.028	0.028	0.008
	(0.015)	(0.015)	(0.014)	(0.011)
T*98o	0.055***	0.000	0.010	-0.007
	(0.021)	(0.019)	(0.019)	(0.019)
log wait. time	0.079***	0.052**	0.028	-0.016
	(0.023)	(0.023)	(0.022)	(0.016)
SSA clinic	0.045**	0.039*	0.019	-0.006
	(0.024)	(0.022)	(0.021)	(0.014)
IMSS Sol. clinic	0.065	0.159***	0.138***	0.093***
	(0.043)	(0.038)	(0.035)	(0.030)
IMSS clinic	-0.099**	-0.007	-0.059	0.006
	(0.051)	(0.058)	(0.050)	(0.025)
Private doctor	-0.043	-0.009	-0.071***	0.001
	(0.050)	(0.027)	(0.027)	(0.017)
Medical aid	0.033**	0.031**	0.048***	0.016
	(0.015)	(0.015)	(0.014)	(0.011)
Mobile unit	0.009	0.011	0.001	-0.009
	(0.017)	(0.016)	(0.016)	(0.012)
Observations	17615	19056	19204	19998

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects are calculated as average partial effects. Standard errors are derived using the delta method. The health centre visit takes the value 1 if at least one household member visited a health centre in the previous six months. The screening tests refer to the previous 12 months. All specifications control for the gender, age and its square, and literacy status of the household head, and whether (s)he speaks the indigenous language. They also include controls for the household poverty index, the household size, number of children, whether there is a woman in the age group 25-64, percentage of women above 18 with secondary school education, percentage of eligibles in the village, total number of households in the village and state fixed effects. The excluded category among the health providers is either no doctor or a "traditional" doctor (herbalist).

Table 1.5: PROGRESA and the demand for health, DD for eligibles

Marginal effects from probit estimations, standard errors are clustered by village

	Cerv. Cancer screening	Blood Sugar screening	Blood Pressure screening	Health Centre visit
98o	0.266***	0.326***	0.314***	0.022***
	(0.012)	(0.013)	(0.012)	(0.010)
T	0.082***	0.111***	0.116***	0.106***
	(0.016)	(0.016)	(0.015)	(0.012)
T*98o	0.216***	0.206***	0.203***	0.163***
	(0.023)	(0.023)	(0.023)	(0.020)
log wait. time	0.070***	0.082***	0.053**	0.006
	(0.025)	(0.026)	(0.025)	(0.018)
SSA clinic	0.092***	0.065***	0.024	0.043***
	(0.030)	(0.027)	(0.025)	(0.016)
IMSS Sol. clinic	0.173***	0.200***	0.144***	0.087***
	(0.045)	(0.048)	(0.047)	(0.022)
IMSS clinic	-0.060	0.044	0.050	0.101***
	(0.106)	(0.124)	(0.058)	(0.018)
Private doctor	0.016	-0.007	-0.038	-0.050
	(0.088)	(0.047)	(0.063)	(0.066)
Medical aid	0.032**	0.042***	0.054***	0.025**
	(0.017)	(0.016)	(0.016)	(0.013)
Mobile unit	0.017	-0.003	-0.010	-0.028**
	(0.019)	(0.021)	(0.019)	(0.012)
Observations	20124	21130	21216	21955

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects are calculated as average partial effects. Standard errors are derived using the delta method. The health centre visit takes the value 1 if at least one household member visited a health centre in the previous six months. The screening tests refer to the previous 12 months. All specifications control for the gender, age and its square, literacy status, whether (s)he speaks the indigenous language of the head of household, the household poverty index, household size, number of children, whether there is any woman in the age group 25-64 and the percentage of women above 18 with secondary school education, the percentage of eligibles in the village, the total number of households in the village and state fixed effects. The excluded category among the health providers is either no doctor or a "traditional" doctor (herbalist).

Table 1.6: IV Estimates, DD

Marginal effects from ivprobit estimations,
bootstrapped standard errors are clustered by village

Non-Eligibles

	Cerv. Cancer screening	Blood Sugar screening	Blood Pressure screening	Health Centre visit
98o	-0.017	0.028	0.023	0.022
	(0.029)	(0.037)	(0.035)	(0.022)
T	0.136***	0.211***	0.191***	-0.060***
	(0.019)	(0.030)	(0.023)	(0.016)
T*98o	0.056**	0.001	0.012	-0.009
	(0.028)	(0.034)	(0.032)	(0.030)
log wait time	-0.263	0.029	-0.078	0.268
	(0.437)	(0.828)	(0.584)	(0.319)
Observations	17615	19056	19204	19998

Eligibles

	Cerv. Cancer screening	Blood Sugar screening	Blood Pressure screening	Health Centre visit
98o	0.083***	0.114***	0.116***	0.106***
	(0.019)	(0.018)	(0.016)	(0.014)
T	0.267***	0.327***	0.314***	0.022**
	(0.015)	(0.015)	(0.014)	(0.011)
T*98o	0.218***	0.212***	0.202***	0.162***
	(0.028)	(0.027)	(0.024)	(0.022)
log wait time	-0.016	-0.137	0.105	0.043
	(0.258)	(0.222)	(0.210)	(0.197)
Observations	20124	21130	21216	21955

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects are calculated as average partial effects. The log waiting time is instrumented using the average opening days per week of the health centres in the village. The IV probit is calculated using a two stage procedures based on the control function approach. Marginal effects are calculated as average partial effects. Standard errors are calculated with 200 bootstrap repetitions and are adjusted for clustering at village level. All specifications also control for all the variables included in the regressions presented in table 1.4.

Table 1.7: The effect PROGRESA on health expenditure, DD estimator

OLS regresions, standard errors are clustered by village

Non-eligibles

	Drugs Expenditure	Visits Expenditure
98o	-25.899***	-8.156***
	(2.645)	(1.162)
T	0.356	2.144
	(3.052)	(1.396)
T*98o	-0.323	-0.964
	(3.339)	(1.611)
Observations	20015	20044

Eligibles

	Drugs Expenditure	Visits Expenditure
98o	-20.947***	-5.640***
	(2.362)	(0.971)
T	-1.451	-0.070
	(2.650)	(1.108)
T*98o	-1.267	-2.060*
	(2.850)	(1.183)
Observations	22097	22119

Note: *** denotes significance at 1%, ** at 5% and * at 10%. The amounts are expressed in *pesos* at October 1997 values. All specifications also control for the variables included in the regressions presented in table 1.4.

Table 1.8: Prenatal care outcomes, DD

First column: OLS
Second column: Marginal effects from probit estimations

Non-eligibles

	Pregnancy checks	Tetanus vaccination
98o	-0.462*	0.021
	(0.241)	(0.055)
T	0.015	0.078
	(0.218)	(0.051)
T*98o	-0.057	-0.097
	(0.291)	(0.073)
Observations	709	706

Eligibles

	Pregnancy checks	Tetanus vaccination
98o	-0.729***	-0.031
	(0.253)	(0.052)
T	-0.228	0.015
	(0.214)	(0.043)
T*98o	0.016	0.072
	(0.312)	(0.057)
Observations	1121	1108

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects of probit estimates are calculated as average partial effects. Standard errors are derived using the delta method and are adjusted for clustering at village level. All specifications also control for the variables included in the regressions presented in table 1.4

Table 1.9: Screening behaviour in control villages

Marginal effects from probit estimations, standard errors are clustered by village

	All		Eligibles		Non-Eligibles	
Cervical cancer						
98o	0.119***	0.105***	0.133***	0.118***	0.094***	0.083***
	(0.013)	(0.014)	(0.016)	(0.017)	(0.016)	(0.016)
Distance from SSA (km)		-0.001**		-0.001**		-0.001**
		(0.000)		(0.000)		(0.000)
Number of services		0.011**		0.012**		0.008
		(0.005)		(0.005)		(0.006)
Observations	14505	14485	7493	7474	7012	7011
Blood sugar						
98o	0.218***	0.208***	0.206***	0.196***	0.220***	0.211***
	(0.013)	(0.013)	(0.018)	(0.019)	(0.015)	(0.015)
Distance from SSA (km)		-0.000		-0.000		-0.000
		(0.000)		(0.000)		(0.000)
Number of services		0.011***		0.014***		0.008*
		(0.004)		(0.004)		(0.004)
Observations	15515	15494	7906	7886	7609	7608
Blood pressure						
98o	0.201***	0.196***	0.198***	0.197***	0.186***	0.179***
	(0.013)	(0.014)	(0.018)	(0.019)	(0.014)	(0.015)
Distance from SSA (km)		-0.000		-0.000		-0.000*
		(0.000)		(0.000)		(0.000)
Number of services		0.010**		0.010**		0.008*
		(0.004)		(0.005)		(0.005)
Observations	15591	15570	7929	7909	7662	7661

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects are calculated as average partial effects. Distance from SSA is defined as the distance from the closest village with a SSA hospital. Number of services ranges between 0 and 7 as described in the locality questionnaire. All specifications also control for all the variables included in the regressions presented in table 1.4

Table 1.10: Screening behaviour by gender of the head of household, DD for non-eligibles

Marginal effects from probit estimations, standard errors are clustered by village

		Cerv. Cancer screening			Blood Pressure screening			Blood Sugar screening	
	Full Sample	Male Head	Female Head	Full Sample	Male Head	Female Head	Full Sample	Male Head	Female Head
98o	0.120***	0.118***	0.143***	0.184***	0.184***	0.183***	0.207***	0.210***	0.191***
	(0.011)	(0.011)	(0.020)	(0.009)	(0.010)	(0.020)	(0.010)	(0.010)	(0.020)
T	0.002	0.004	-0.017	0.030**	0.027*	0.045*	0.030**	0.028*	0.042*
	(0.015)	(0.016)	(0.025)	(0.014)	(0.014)	(0.025)	(0.015)	(0.015)	(0.024)
T*98o	0.054**	0.050**	0.088**	0.010	0.011	0.003	-0.000	-0.000	-0.001
	(0.021)	(0.021)	(0.037)	(0.019)	(0.020)	(0.038)	(0.019)	(0.020)	(0.037)
Female head	-0.052***			0.012***			-0.015***		
	(0.000)			(0.000)			(0.000)		
T*98o*Female head	0.016			0.011			-0.009		
	(0.029)			(0.025)			(0.028)		
Observations	17615	15446	2169	19204	16690	2514	19056	16571	2485

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects are calculated as average partial effects. Standard errors are derived using the delta method. All specifications also control for all the variables included in the regressions presented in table 1.4

Table 1.11: Cancer related characteristics by gender of the head of household

Household Characteristics

	Female Head	Male Head	DIFF
Age head	59.310	50.300	9.010***
	(14.772)	(15.900)	(0.424)
Literate head	0.444	0.738	-0.294***
	(0.497)	(0.440)	(0.012)
Indigenous head	0.241	0.255	-0.013
	(0.428)	(0.436)	(0.012)
Marginality Index	844.514	843.618	0.895
	(100.359)	(110.248)	(2.928)
Proportion women age 25-64	0.342	0.238	0.104***
	(0.315)	(0.158)	(0.005)
Proportion women above 18 with sec. school	0.072	0.131	-0.059***
	(0.210)	(0.297)	(0.008)
Observations	1588	9142	

Sex related behaviour of female respondent

	Female Head	Male Head	DIFF
Pregnancies	5.484	5.006	0.478***
	(4.070)	(3.467)	(0.121)
Never used contraception	0.595	0.508	0.086***
	(0.491)	(0.500)	(0.015)
Never PAP test	0.562	0.556	0.006
	(0.496)	(0.497)	(0.015)
Observations	1053	7331	

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Differences are computed by running an OLS regression. Statistics are calculated only on the sample of non-eligible households. Household characteristics were collected in October 1997. The information about the sexual behaviour of female respondent was collected in March 1998

Table 1.12: Sex behaviour and female status in the sample of non-eligibles

Linear probability model

	Female Head		Male Head	
	Never PAP test	Never contraception	Never PAP test	Never contraception
Age head	-0.005***	-0.005***	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Literate head	-0.045	-0.018	-0.010	-0.033*
	(0.029)	(0.029)	(0.014)	(0.014)
Poverty Index	-0.001***	-0.000*	-0.000***	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
FS index	0.010	0.002	0.021***	0.015***
	(0.011)	(0.011)	(0.004)	(0.004)
Age wife			-0.005***	0.001
			(0.001)	(0.001)
Literate wife			-0.100***	-0.085***
			(0.013)	(0.013)
Constant	1.323***	1.117***	1.163***	0.599***
	(0.103)	(0.103)	(0.036)	(0.037)
Observations	1261	1261	8539	8539

Note: *** denotes significance at 1%, ** at 5% and * at 10%. The variable Never PAP test takes the value 1 if the female respondent has never had a PAP test. The variable Never contraception takes the value 1 if she has never used any form of contraception . The *FS* index ranges between 0 and 6 where 6 denotes least emancipated women. All the variables above were created using information elicited in March 1998

Table 1.13: Intensity of the indirect treatment effect on cervical cancer screening, DD

Marginal effects from probit estimations,
standard errors are clustered by village

	Full sample	Male Head	Female Head
98o	0.121***	0.119***	0.143***
	(0.011)	(0.011)	(0.020)
T	0.002	0.004	-0.017**
	(0.015)	(0.016)	(0.025)
T*98o	0.053***	0.048**	0.088***
	(0.021)	(0.021)	(0.037)
Ratio Eligibles	-0.150***	-0.153***	-0.112***
	(0.010)	(0.011)	(0.013)
T*98o*Ratio Eligibles	0.140***	0.160***	0.005
	(0.050)	(0.057)	(0.075)
Observations	17615	15446	2169

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects are calculated as average partial effects. Standard errors are derived using the delta method. *Ratio Eligibles* is the proportion of eligibles in each village. All specifications control for all the variables included in the regressions presented in table 1.4

Table 1.14: Intensity of the ITE on cervical cancer screening by level of female emancipation, DD

Marginal effects from probit estimations,
standard errors are clustered by village

	High	*Medium*	*Low*
98o	0.088***	0.125***	0.178***
	(0.018)	(0.016)	(0.020)
T	-0.026**	0.047*	-0.025
	(0.023)	(0.025)	(0.031)
T*98o	0.080**	0.037	0.029
	(0.034)	(0.034)	(0.037)
Ratio Eligibles	-0.128***	-0.031***	-0.244***
	(0.012)	(0.006)	(0.031)
T*98o*Ratio Eligibles	0.090	0.107	0.142**
	(0.091)	(0.094)	(0.067)
Observations	7318	6013	4284

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects are calculated as average partial effects. Standard errors are derived using the delta method. The sample is split according to the terciles of the village average of the *FS* index as measured in March 1998. All specifications also control for the variables included in the regressions presented in table 1.4

Table 1.15: Indirect treatment effect on cancer screening by characteristics of the head of household

Marginal effects from probit estimations, standard errors are clustered by village

	Male Head	Female Head	Male Head	Female Head	Male Head	Female Head
98o	0.118***	0.142***	0.118***	0.143***	0.118***	0.142***
	(0.011)	(0.020)	(0.011)	(0.020)	(0.011)	(0.020)
T	0.004	-0.018	0.004	-0.016	0.004	-0.017
	(0.016)	(0.025)	(0.016)	(0.025)	(0.016)	(0.025)
T*98o	0.048**	0.089***	0.048**	0.088***	0.049**	0.086***
	(0.021)	(0.037)	(0.021)	(0.037)	(0.021)	(0.037)
Indigenous Head	-0.064***	-0.027***				
	(0.001)	(0.001)				
T*98o*Indigenous Head	0.078**	-0.023				
	(0.031)	(0.059)				
Head Above 60			-0.048***	-0.051***		
			(0.000)	(0.001)		
T*98o*Head Above 60			0.041*	-0.005		
			(0.026)	(0.044)		
Illiterate Head					-0.034***	-0.037**
					(0.000)	(0.001)
T*98o*Illiterate Head					0.063***	0.059*
					(0.026)	(0.042)
Observations	15446	2169	15446	2169	15447	2171

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects are calculated as average partial effects. Standard errors are derived using the delta method. Indigenous head takes the value1 if the head speaks the indigenous language. Head above 60 takes the value 1 if the household head is aged 60 or above. All specifications also control for the variables included in the regressions presented in table 1.4.

Table 1.16: Indirect treatment effect on cancer screening and status of the female head

Marginal effects from probit estimations, standard errors are clustered by village

	Widow	Non-widow
98o	0.149**	0.129
	(0.024)	(0.035)
T	-0.026**	-0.007
	(0.026)	(0.046)
T*98o	0.094**	0.074
	(0.045)	(0.065)
Observations	1447	702

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects are calculated as average partial effects. Standard errors are derived using the delta method. All specifications also control for all the variables included in the regressions presented in table 1.4.

Table 1.17: Descriptives by exposure to *Progresa*

	Before 2000		After 2003			
	Mean	SD	Mean	SD	**DIFF**	SE
			Characteristics Women 18-50			
Age	36.259	(6.782)	36.646	(6.721)	-0.387	(0.406)
Literacy (Y/N)	0.795	(0.404)	0.867	(0.340)	-0.072**	(0.035)
Indigenous (Y/N)	0.259	(0.438)	0.160	(0.367)	0.099	(0.116)
Head HH Male (Y/N)	0.879	(0.327)	0.919	(0.274)	-0.040*	(0.022)
Primary School (Y/N)	0.665	(0.472)	0.696	(0.460)	-0.032	(0.044)
Secondary School or Above (Y/N)	0.183	(0.387)	0.207	(0.406)	-0.024	(0.049)
Children	4.108	(2.204)	4.018	(2.098)	0.090	(0.179)
Last Week Worked (Y/N)	0.230	(0.421)	0.160	(0.367)	0.070*	(0.038)
Sick Last Month (Y/N)	0.209	(0.407)	0.231	(0.422)	-0.022	(0.029)
			Household Characteristics			
Television (Y/N)	0.833	(0.373)	0.911	(0.285)	-0.078	(0.053)
Radio (Y/N)	0.189	(0.392)	0.111	(0.315)	0.078***	(0.022)
PC (Y/N)	0.010	(0.100)	0.018	(0.132)	-0.008	(0.007)
Refrigerator (Y/N)	0.526	(0.499)	0.644	(0.479)	-0.119	(0.079)
Wash Mach. (Y/N)	0.176	(0.381)	0.228	(0.420)	-0.053	(0.041)
Horses	1.557	(0.780)	1.519	(0.666)	0.038	(0.155)
Pigs	4.645	(8.318)	3.122	(3.074)	1.524	(0.964)
Cows	3.063	(3.654)	6.036	(7.047)	-2.973	(2.739)
			Health Center Characteristics			
Number of Doctors	1.257	(1.518)	1.615	(1.387)	-0.359	(0.414)
Doctors Tenure (Months)	33.355	(46.672)	39.037	(40.604)	-5.683	(13.097)
Doctors Working Days	5.144	(1.297)	5.033	(0.552)	0.110	(0.237)
Number of Nurses	0.932	(0.816)	1.615	(1.850)	-0.683	(0.508)
Nurses Working Days	4.473	(1.339)	4.650	(0.580)	-0.177	(0.260)
PAP Test Available (Y/N)	0.685	(0.468)	0.769	(0.439)	-0.084	(0.130)
Diabetes Test Available (Y/N)	0.716	(0.454)	0.846	(0.376)	-0.130	(0.114)
			Doctor Characteristics			
Fraction Males	0.568	(0.491)	0.636	(0.505)	-0.068	(0.164)
Doctors Age	33.056	(10.372)	33.545	(9.627)	-0.490	(3.194)
Fraction with Postgrad. Studies	0.204	(0.407)	0.364	(0.505)	-0.160	(0.159)
Fraction Advised PAP Test	0.741	(0.437)	0.818	(0.405)	-0.077	(0.134)
Fraction Advised Mammogram	0.741	(0.437)	0.727	(0.467)	0.014	(0.150)

Notes: *** denotes significance at 1%, ** at 5% and * at 10%. The table reports characteristics taken from the 2007 ENCEL survey for those localities surveyed in 2003. Localities that received *Progresa* before 2000 are those belonging to the original evaluation sample. Localities that received it after 2003 are those added as control ones in the 2003 survey. The sample is restricted to all women aged 18-50.

Table 1.18: Exposure to *Progresa* and Female Screening Decisions

	PAP Test (Y/N)		Mammogram Y/N		Blood Press. Screen. (Y/N)		Blood Sugar Screen. (Y/N)		Cholesterol Screen. (Y/N)	
Prog. Before 2000 (Y/N)	0.139**	-0.028	0.056***	-0.009	-0.023	0.031	-0.012	-0.043	0.003	0.004
	(0.057)	(0.065)	(0.020)	(0.045)	(0.027)	(0.040)	(0.023)	(0.034)	(0.008)	(0.014)
Prop. Male Doctors		-0.187***		-0.100**		0.089*		0.001		0.025
		(0.066)		(0.049)		(0.045)		(0.042)		(0.023)
Before 2000*Prop. Male Doctors		0.195**		0.125*		-0.080		-0.011		-0.011
		(0.076)		(0.066)		(0.052)		(0.023)		(0.023)
Socioeconomic Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Health Supply Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2264	1849	2267	1851	2267	1851	2267	1851	2267	1851

Notes: *** denotes significance at 1%, ** at 5% and * at 10%. The sample includes women 18-50 surveyed in 2007. The dummy *Prog. Before 2000* takes value 1 for localities belonging to the original evaluation sample, and 0 for those added as control ones in the 2003 survey. Socioeconomic variables include dummies for being literate, indigenous, head of household, for completing primary, and secondary or higher school, number of kids alive, a dummy for working the weak before the interview, whether the woman was sick in the last four weeks, whether in the house there is a television and a radio. Health supply are defined at locality level and include dummies for the type of health provider, the number of doctors, the number of nurses, and the total number of families that have registered with at least one health provider in the locality.

Table 1.19: Learning in cervical cancer screening: Effect of initial information

Marginal effects from ivprobit estimations, bootstrapped standard errors are clustered by village

	Full Sample	Male Head	Female Head
Peer group screening rate	0.492***	0.514***	0.447
	(0.144)	(0.157)	(0.398)
No contraception	-0.108***	-0.111***	-0.077
	(0.014)	(0.014)	(0.056)
Peer group screening rate*no contraception	0.153*	0.104	0.570**
	(0.093)	(0.104)	(0.285)
Observations	6931	6313	618
Cragg Donald Test	30.163	25.763	4.232
Cragg Donald χ^2	121.213	103.560	17.821

Note: *** denotes significance at 1%, ** at 5% and * at 10%. The peer group is defined by all the village households whose eldest children are in the same school grade. The level of initial information is proxied by the dummy for having never used any method of contraception, as recorded in March 1998. The IV strategy exploits the the treatment effect, the average number of payments received by the eligibles in the peer group and their interaction as instrument. The IV probit is calculated using a two stage procedure based on the control function approach. Marginal effects are calculated as average partial effects. Standard errors are calculated with 200 bootstrap repetitions and are adjusted for clustering at village level. The Cragg Donald test for the validity of the rank condition is reported. All specifications also control for the variables included in the regressions presented in table 1.4.

CHAPTER 2

Conditional Cash Transfers, Adult Work Incentives, and Poverty

Conditional cash transfer (CCT) programs aim to alleviate poverty through monetary and in-kind benefits, as well as reduce future incidence of poverty by encouraging investments in education, health and nutrition. The success of CCT programs at reducing poverty depends on whether, and the extent to which, cash transfers affect adult work incentives. In this paper we examine whether the PROGRESA program of Mexico affects adult participation in the labor market and overall adult leisure time, and we link these effects to the impact of the program on poverty. Utilizing the experimental design of PROGRESA's evaluation sample, we find that the program does not have any significant effect on adult labor force participation and leisure time. Our findings on adult work incentives are reinforced further by the result that PROGRESA leads to a substantial reduction in poverty. The poverty reduction effects are stronger for the poverty gap and severity of poverty measures.

§2.1. Introduction

Mean-tested conditional cash transfer (CCT) programs are increasingly popular in developing countries as a useful tool for poverty alleviation. Examples of such programs include PROGRESA/*Oportunidades* in Mexico, Bolsa Familia in Brazil, Bono de Desarrollo Humano in Ecuador, Familias en Accion in Colombia, PRAF in Honduras, PATH in Jamaica, and Red de Proteccion Social (RPS) in Nicaragua, among others. Targeting their benefits directly to populations in extreme poverty, primarily in rural areas, such programs aim to alleviate current poverty through monetary and in-kind benefits, as well as reduce future levels of poverty by encouraging investments in education, health and nutrition.

The success of CCT programs at reducing current poverty depends on whether, and the extent to which, cash transfers affect adult work incentives. In all of the programs mentioned above, once a household is selected as eligible for the program, usually through geographic targeting or household level means-testing, or both, the level of benefits is not affected by the work decisions of the household members or the income level of the household. Thus, the main effect of CCT on the labor supply of adults may be a pure income effect. [1]This is in contrast

[1]In most CCT programs the eligibility status of beneficiary households is, in theory, re-examined every few years. For example, in the PROGRESA program of Mexico, the eligibility status of households was supposed to

to most welfare programs in the US and Canada, that have explicit disincentives to work. For example, in the Aid to Families with Dependent Children (AFDC) in the US, the level of benefits is affected by work decisions as work income is effectively taxed by reducing the level of benefits provided.

The extent to which the transfers of CCT programs result to significant income effects on adult leisure and consumption can only be determined empirically. The incentive effects of welfare programs on labor supply have been the subject of intense scrutiny primarily in the U.S., the U.K. and Canada (e.g., Stafford (1985); Moffitt (1992); Blundell and MaCurdy (1999); and Widerquist (2005). In developing countries, however, the evidence regarding the labor supply responses of adults to transfer programs is quite scarce. The study of Sahn and Alderman (1996), one of the rare studies in this topic, suggests that the labor supply effect of a rice subsidy program in Sri Lanka is significantly large. Yet, in a recently published ex-ante microsimulation study of the impact of the Bolsa Escola program on poverty in Brazil, the income effect of the transfers on adult labor supply is assumed to be negligible Bourguignon et al. (2003). In addition to this, León et al. (2001) find a negative labor supply effect of an unconditional transfer program.

Our paper sheds light on these issues using data from a large conditional cash transfer program in the poor rural areas of Mexico called PROGRESA (Health, Education and Nutrition Program). With a cash transfer of 20% of pre-program consumption, PROGRESA has the potential of affecting adult work incentives of both program participants as well as non-participants. For eligible households, the income effect of the cash transfer may be weakened by the direct and indirect time costs associated with adhering to the requirements of the program. In addition, the means testing associated with CCT programs may also affect the incentives of both eligible and non-eligible households. On the one hand, individuals who are not eligible for the program's benefits may also have the incentive to work less or earn (or report) a lower income hoping to become eligible for the program in future rounds of expansion of the program. On the other hand, the possibility of future means-tests may impact on the labor supply and investment choices of currently eligible households.

The empirical analysis uses panel data from households surveyed for the purpose of evaluating the impact of PROGRESA on basic indicators of household investment in human capital. [2] A distinguishing feature of the PROGRESA data is that they are based on an experimental design, with randomization of the coverage of the program at the locality level. The empirical methodology consists of comparing conditional means of key outcome variables (such as labor market participation, hours of leisure, and poverty rates) between households living in villages covered by the PROGRESA program (treatment villages) and households living in comparable villages that are out of the program (controls). An additional advantage offered by the design of the sample, is that we can also examine the potential effects of the program on the labor supply of the non-eligible households living in the treated communities (i.e. the communities covered

be reviewed within three years after a household's entry into the program. In fact, more than five years elapsed before any effort was made to revise the list of beneficiaries.

[2] Skoufias (2005) provides a detailed discussion of PROGRESA, the evaluation design and a summary of the impacts of the program estimated by a large team of researchers.

by PROGRESA).

We investigate three main questions. First, we examine whether being eligible or ineligible for PROGRESA benefits affects adult labor force participation. Second, we study the effects of PROGRESA on adult leisure hours. Leisure may be an important determinant of welfare and beneficiary households may choose to increase their welfare by using the cash transfers of the program to "buy" more leisure. Finally, we analyze the impact of PROGRESA on poverty measures based on household income. Poverty measures offer the advantage of being simple, albeit imperfect, summary measures of the effects of the program on the communities with both eligible and ineligible households exposed to the program. Significant labor supply response among households in communities covered by the program may result in small impacts of the program on poverty.

The organization of the chapter is as follows. Section 2.2 describes briefly the PROGRESA program and the data used. Section 2.3 illustrates the econometric specification and estimation approach behind our results. Section 2.4 shows and discusses the results regarding the impact of PROGRESA on adult labor force participation and leisure. The impact of PROGRESA on poverty is studied in section 2.5, while section 2.6 concludes.

§2.2. A Brief Description of PROGRESA and of the Data Used

PROGRESA, initially implemented by the Mexican Federal government in 1997, adopts an integrated approach to combating the different causes of poverty by intervening simultaneously in the areas of health, education and nutrition. By the year 2004, the program which was renamed *Oportunidades,* included nearly 5 million families in 72,345 localities in all 31 states. The total annual budget of the program in 2004 was around US$2.5 billion, or 0.3% of the gross domestic product.

The education component of PROGRESA is designed to increase school enrollment among youth in Mexico's poor rural communities by making education grants available to pupils' mothers, who are then required to have their children attend school regularly. In localities where PROGRESA currently operates, households that have been characterized as poor, and have children enrolled in grades 3-9, are eligible to receive these educational grants every two months. The levels of these grants were determined taking into account, among other factors, what a child would earn in the labor force or contribute to family production. The educational grants are slightly higher at the secondary level for girls, given their propensity to drop out at earlier ages. Every two months, confirmation of whether children of beneficiary families attend school more than 85% of the time is submitted to PROGRESA by school teachers and directors, and this triggers the receipt of a bi-monthly cash transfer for school attendance.

In the area of health and nutrition, PROGRESA brings basic attention to health issues and promotes health care through free preventative interventions, such as nutritional supplements, and education on hygiene and nutrition as well as monetary transfers for the purchase of food. Receipt of monetary transfers and nutritional supplements are tied to mandatory health care visits to public clinics. This aspect of the program emphasizes targeting its benefits to children under five, and pregnant and lactating women, and is administered by the Ministry of Health and by IMSS-*Solidaridad*, a branch of the Mexican Social Security Institute, which provides benefits to uninsured individuals in rural areas.

Nutritional supplements are given to children between the ages of four months and two years, and to pregnant and breast-feeding women. If signs of malnutrition are detected in children between the ages of 2 and 5, nutritional supplements will also be administered. The nutritional status of beneficiaries is monitored by mandatory visits to the clinic and is more frequently monitored for children five years of age and under, pregnant women and lactating women. Upon each visit, young children and lactating women are measured for wasting (weight-for-height), stunting (height-for-age), and weight-for-age. An appointment monitoring system is set up and a nurse or doctor verifies adherence. The health care professionals submit every two months certification of beneficiary visits to PROGRESA, which triggers the receipt of the cash transfer for food support.

The average monthly payment, distributed every two months to the mothers in beneficiary families amounts to 20% of the value of monthly consumption expenditures prior to the initiation of the program.[3] Working counter to this transfer are the direct and indirect costs associated with participation in the program. For example, one of the conditions of the PROGRESA program is that households eligible for the program were required to stop receiving benefits from other pre-existing programs such as *Niños de Solidaridad, Abasto Social de Leche*, *de Tortilla* and the National Institute of Indigenous people (*INI*).[4] In addition to these direct costs, there are some indirect costs associated with complying with the program's requirements. Such costs include the time costs of taking children to school and to health center, waiting in line at the health center, attending educational seminars on nutrition and hygiene, and traveling to the localities where payments are being distributed.

The data used in this paper consist of the sample of communities and households surveyed between November 1997 and November 1999, for the purposes of the evaluation of the PROGRESA program. In order to obtain a credible evaluation of the potential impact of the program the PROGRESA administration decided to adopt an experimental design that allows one to compare households before and after the initiation of PROGRESA with similar households that were not yet covered by the program. Specifically, the full sample used in the evaluation of PROGRESA consists of repeated observations (panel data) collected for 24,000 households from 506 localities in the seven states of Guerrero, Hidalgo, Michoacan, Puebla, Queretaro, San Luis Potosi and Veracruz. The opportunity of having a randomized design came from the fact the

[3]The average monthly transfers during the twelve-month period from November 1998 to October 1999 are around 197 pesos per beneficiary household per month (expressed in November 1998 pesos). The calculation of this average includes households that did not receive any benefits due to non-adherence to the conditions of the program, or delays in the verification of the requirements of the program or in the delivery of the monetary benefits. On average, households receive 99 pesos for food support, and 91 pesos for the educational grant. For more details, see ?)

[4]Before the establishment of PROGRESA, previous government interventions in the areas of education, health and nutrition in the rural sector of the country consisted of many programs each intervening separately in health, education or nutrition with little prior coordination or consideration of the potential synergies that could result from a better coordinated and simultaneous intervention.

program was introduced in phases, due to budgetary constraints. In fact, of the 506 localities, 320 localities were assigned to the treatment group (where PROGRESA was in operation) and 186 localities were assigned as controls.[5] As originally planned the localities serving the role of a control group started receiving PROGRESA benefits by December 1999.

The selection of beneficiaries into PROGRESA was a two stage process (**?**). In the first stage, using national census data, poor communities with schooling and health infrastructure were identified. In the second stage, households within the selected poor communities were classified as eligible or ineligible based on the socioeconomic data collected by a census of all the households residing in the communities to be covered by the program. On average, in the evaluation sample, 78% of the households were classified as eligible for program benefits. However, the fraction of households that actually ended up receiving the PROGRESA cash transfers during the two-year interval covered by the evaluation sample is just under 65%, due to administrative errors and delays in the final registration of beneficiary households.[6]

The 1997 baseline household census called *ENCASEH* (*Encuesta de Caracteristicas Socioeconomicas de los Hogares*) was followed by a number of socio-economic household surveys (*Encuesta de Evaluación de los Hogares or ENCEL)* designed to collect information for the evaluation of PROGRESA. The first evaluation survey took place in March 1998 before the initiation of benefits in May 1998. The remaining surveys were conducted after beneficiary households in treatment villages started receiving benefits from PROGRESA. One round of surveys took place in November 1998, which was well after most households received some benefits as part of their participation in the program. The next two waves took place in June 1999 and November 1999. A number of core questions about the demographic composition of households and their socioeconomic status were applied in each round of the survey.

Data used in this paper are drawn from four rounds. The first round (R1) that took place in November 1997, the second round (R2) in November 1998 survey, the third round (R3) in June 1999, and fourth round (R4) that took place in November 1999.

The November 1997 ENCASEH survey as well as the November 1998, June 1999, and November 1999 ENCEL surveys collected detailed information on income earned or received from a variety of sources for each individual in the household. [7] The survey instrument used to collect individual and household income for these various sources changed significantly beginning with the November 1998 survey. With this caveat in mind it should be noted that a serious effort was made to maintain comparability of income by source across the survey rounds. The various sources of income (excluding the PROGRESA cash transfers) were transformed into monthly income and then aggregated into 4 main sources of income: labor income; income from self employment (such as income from sewing, food preparation, construction or carpentry, commerce, produce transportation, repairs and laundry or cooking); other income (such

[5]Behrman and Todd (1999) conducted a careful investigation of the extent to which the selection of localities into treatment and control groups can be considered as random. Their analysis did not reveal any significant differences between village means for more than 300 variables in treatment and controls.

[6]For more details see Skoufias (2005).

[7]The March 1998 baseline survey was not used because it did not include household income and labor supply information for adults.

as pensions, interest income, rents and community profits); and government transfers (such as educational scholarships from *Niños de Solidaridad,* benefits from *Instituto Nacional Indigenista* (INI), PROBECAT, *Empleo Temporal* and Procampo).

The income data by source are useful at quantifying the direct costs associated with participation in the PROGRESA program. Children can contribute income to families by working for wages or by being recipients of cash transfers from other government transfer programs excluding PROGRESA. Table 2.5 presents how the income contributed to families by children between ages 8-17 evolved across different survey rounds in the treatment and control villages (using only eligible households E=1). Table 2.5 reveals that the total (labor + other) income (excluding PROGRESA cash transfers) beneficiary families received from children declined in both treatment and control localities since the initiation of PROGRESA in November 1998. The mean total income contributed by children in November 1998 is slightly lower among treatment villages compared to the control villages and the gap gets even bigger by the June 1999 round. By November 1999, this gap is completely eliminated as control households are already incorporated into PROGRESA.[8] Other columns in the same table break down total income from children into its two components, i.e., income from labor and other income that consists mainly of government transfers. These columns reveal that the differences in mean total income from children in beneficiary households and eligible households in control localities are primarily due to drops in the child-related income beneficiary families received from other government programs. It also appears that there are no significant differences in the labor income of children from beneficiary households in treatment localities and the labor income contribution of children in eligible households in control villages.

In addition to the potential income losses from children's work and benefits, households receiving PROGRESA benefits are also required to give up benefits from programs like *Abasto Social de Leche*, *de Tortilla* and the National Institute of Indigenous people (INI). Figure 2.1 makes clear that among beneficiary households (i.e. those that received any PROGRESA benefits between May 1998 and November 1999 in the treatment villages) the incidence of benefits received from DIF, Ninos de Solidaridad and *Abasto Social de Leche* decreased dramatically. In combination the preceding discussion suggests that the effect of PROGRESA on household income and poverty may not be adequately summarized by the size and incidence of the cash transfers.

§2.3. Econometric specification and estimation

We begin with a brief discussion of the estimation approach that underlies our estimates of the impact of PROGRESA on labor force participation, and leisure time.

The estimation of the impact of PROGRESA (for labor force participation and poverty rates) is based on the *difference-in-differences* estimator. This estimator is based on comparing differences between the treatment and control groups before and after the start of the PROGRESA

[8] Note that control households started receiving cash benefits in December 1999. Households are first incorporated into PROGRESA, meaning that they are given all the necessary forms and informed of all the program requirements. A few months later, the cash benefits are sent out by the PROGRESA administration headquarters.

program[9]. It offers the advantage that any pre-program differences between the treatment and control group are eliminated in the estimation of impacts. Under the assumption that any unobserved heterogeneity between the treatment and control groups is fixed over time, the difference-in -differences estimator (2DIF, discussed in detail below) eliminates this heterogeneity. Specialized empirical specifications are implemented for the labor force participation and poverty estimation and are discussed below. We use also a number of control variables which may be useful for reducing any remaining statistical bias.

To begin with, consider the case where there are data available for treatment and control households before and after the start of the program. Restricting the sample to eligible households only (E=1), the following regression equation defines a model that can nest various "difference" estimators allowing for controlling for individual, household and locality observed characteristics:

$$Y(i,t) = \alpha + \beta_T T(i) + \beta_{R2}(R2) + \beta_{R3}(R3) + \beta_{R4}(R4) + \beta_{TR2}(T(i) * R2) +$$

$$+\beta_{TR3}(T(i) * R3) + \beta_{TR4}(T(i) * R4) + \sum_{j=1}^{J} \theta_j X_j(i,t) + \eta(i,t) \tag{2.1}$$

$Y(i,t)$denotes the value of the outcome indicator of interest for household, individual or population i in period/round t, α, β, and θ are fixed parameters to be estimated, $T(i)$is an binary variable taking the value of 1 if the household resides in a treatment community and 0 otherwise (i.e., for control communities), R2, R3, and R4 are a binary variables equal to 1 for the second, third and fourth rounds of the survey respectively, and equal to zero otherwise. Note that the first round is the baseline round prior to the initiation of the program. The vector $\boldsymbol{X}$ summarizes household (and possibly village) characteristics and η is an error term summarizing the influence random disturbances.

To better understand the preceding specification it is best to consider the case of only two rounds: one before the initiation of the program and the other after the start of the program (e.g. round 2 or R2 = 1, 0 otherwise). One may then divide the parameters into two groups: one group summarizing differences in the conditional mean of the outcome indicator before the start of the program (i.e., α, β_T,) and another group summarizing differences after the start of the program (i.e., β_R, and β_{TR}). Specifically, the coefficient β_T allows the conditional mean of the outcome indicator to differ between eligible households in treatment and control localities before the initiation of the program whereas the rest of the parameters allow the passage of time to have a different effect on households in treatment and control localities. For example, the combination of parameters β_R andβ_{TR} allow the differences between eligible households in

[9]Behrman and Todd (1999)) state in their investigation of whether assignment to treatment and control groups can be considered random that formal tests of equality between the distributions of various characteristics generally do not reject the hypothesis when the test is performed on locality means. However, when the test is performed on household level data, they find many more rejections of the null than would be expected by chance given standard significance level. This motivates the choice of including controls in our regressions and employ a 2DIF approach.

treatment and control localities to be different after the start of the program.

Specifically, given the preceding specification, the conditional mean values of the outcome indicator for treatment and control groups before and after the start of the program are as follows:

$$[E(Y|T=1,R2=1,X)] = \alpha + \beta_T + \beta_R + \beta_{TR} + \sum_j \theta_j X_j \tag{2.2}$$

$$[E(Y|T=1,R2=0,X)] = \alpha + \beta_T + \sum_j \theta_j X_j \tag{2.3}$$

$$[E(Y|T=0,R2=1,X)] = \alpha + \beta_R + \sum_j \theta_j X_j \tag{2.4}$$

$$[E(Y|T=0,R2=0,X)] = \alpha + \sum_j \theta_j X_j \tag{2.5}$$

The advantage offered by the 2DIF estimator is that it provides an estimate of the impact of the program that is net of any pre-program differences between treatment and control households and/or any time trends or aggregate effects in changes of the values of the outcome indicator. By comparing before and after differences between treatment and control households (or differences between treatment and control households after and before the program) one is able to get an estimate of the impact of the program (summarized by the single parameter β_{TR}).

$$2DIF = [E(Y|T=1,R2=1,X) - E(Y|T=1,R2=0,X)] -$$

$$[E(Y|T=0,R2=1,X) - E(Y|T=0,R2=0,X)] \tag{2.6}$$

Using the terminology of Heckman et al. (1999), the parameter β_{TR} provides an estimate of the "intent to treat effect," which is inclusive of the operational efficiency or inefficiency of the program implementation. Thus, β_{TR} provides a lower bound estimate of the impact of the program on those who actually receive the treatment (or of "the effect of the treatment on those who actually received the treatment").

The availability of repeated observations on non-eligible households in treatment areas before and after the start of the program also offers the opportunity to examine the potential effects of the program on the non-eligible households residing in the treatment communities. For example, non-eligible households in treatment localities may alter their behavior (e.g., work less or withdraw their children from school) in anticipation that such actions may qualify them for the program. An evaluation of the extent to which the program has had some indirect effects on the outcome indicator among non-eligible households in treatment areas can also be conducted by estimating a regression similar to (2.1) but restricted to the sample of non-eligible households (E=0).

Note that $\beta_T$is expected to be insignificantly different from zero (that is, pre-program differences prior to program implementation are expected to be zero) and the interaction terms represent the impact of being in a treatment community on work participation after program implementation. The different intercept α terms capture the point that participation in work may vary (for reasons unrelated to PROGRESA) during the first round of the sample.

In estimating the impact of the program on adult leisure time, we are limited by the fact that there is only one round of data on time allocation (June 99). Given this constraint we estimate the model:

$$L(i) = \alpha_0 + \beta_T T(i) + \sum_{j=1}^{J} \beta_j X_j(i) + \varepsilon_i \tag{2.7}$$

where $L(i)$measure leisure time of individual i, $T(i)$represents a binary variable equal to 1 if individual i lives in a treatment community and 0 otherwise, and $X_j(i)$ represents the vector of j control variables for individual i (described in section 4). In this model the estimate of the coefficient β_T is the "cross-sectional difference" (CSDIF) estimate of program impact.

§2.4. The impact of PROGRESA on adult labor force participation and leisure

2.4.1 — Adult labor force participation

To estimate the impact on labor force participation, we use the data as our baseline round and three post-program rounds of November 1998, June 1999, and November 1999.

The dependent variable $Y(i,t)$ in equation (2.1) is specified by a binary variable indicating whether an individual i works in the labor market in period t. Specifically, a person is classified as working in the labor market ($Y(i,t) = 1$) if he/she reported that having worked over the previous week (whether paid or unpaid). In addition, we take into account a follow-up question to capture individuals who may engage in informal activities, which the respondent may not have initially considered as work. This question collects information about participation in a) selling a product; b) helping in family business; c) making products to sell; d) washing, cooking or ironing; and e) working in agriculture activities or caring for animals. We also include as working those individuals who respond that they engage in any of these activities. It is important to keep in mind that domestic activities are not included in this definition of work.

We also consider two other outcomes variables, salaried work and non-salaried work and estimate the impact of PROGRESA on each category. The distinction between salaried and non-salaried[10] work is made through what a worker reports as his/her occupational position. Workers who report that they were daily agricultural workers or non-agricultural employees are considered as salaried workers. All other workers are classified separately and include self-employed workers, business owners, unpaid workers and *ejidatarios*.

Equation (2.1) is estimated separately for males and females. In addition, we conduct the

[10] In preliminary analysis, we considered separating non-salaried workers between self-employed workers and unpaid family workers. Nevertheless, the proportions of individuals participating in each of these activities are quite small for all age groups, and the distinction between these activities is often blurred so that we prefer to aggregate these groups in the impact analysis.

empirical analysis separately for 5 age group (ages 18 to 24, 25 to 34, 35 to 44, 45 to 54 and 55 and over) and for the group of all adults. In general the labor force participation of women of all ages is quite low (for no age group do overall labor force participation rates exceed 18%). In addition, the majority of women who do work tend to participate in unsalaried activities: this is particularly true of women over the age of 35. It is interesting to note the decreasing relative participation in salaried work versus unsalaried activities with age of women. Men, on the other hand, show a very high labor force participation rate, over 90% for men between the ages of 24 and 55. The majority of men are salaried workers, although the percentage in salaried work tends to decrease with age. For male workers over the age of 55, almost half participate in non-salaried activities.

In particular, our sample includes 183,646 individual observations (89,207 observations on adult males and 94,439 observations on adult females) for 4 rounds of the PROGRESA evaluation sample (Nov 97, Oct 98, Jun 99 and Nov 99) that are divided in treatment and control localities. The analysis is first carried on the sample of those who are classified as "poor"[11], and thus eligible to receive the program if they live in treatment localities (E=1). We then re-do the same estimation on the sample of those who are classified as non-eligible (E=0).

The vector $\boldsymbol{X}$ in equation (2.1) consists of individual and demographic composition variables. In particular, we include as individual characteristics his/her age, age squared, marital status, whether he/she is head of the household, speaks an indigenous language, and his/her level of education. The demographic composition variables include the number of children aged 0 to 2 and aged 3 to 5, boys and girls between 6 and 7 years old , 8 to 12, and 13 to 18, men and women aged 19 to 54 and men and women over the age of 55. At the community level, the model also contains a variable measuring distance to the *"cabecera municipal"* which is an indicator of distance to the governing center of the municipality (and likely the largest locality of the municipality).

Given that dependent variable is binary, equation (2.1) is estimated with a probit model. Table 2.1 presents the 2DIF estimates (summarized by the parameterβ_{TR} in equation 1) of the impact of PROGRESA on the probability of working of male and female adults for the sample of individuals form eligible households (E=1).[12] The clustering of the households within villages implies that the household-specific error terms $\eta(i,t)$ are likely to be correlated within each village (as well as across time). Thus standard errors were estimated taking into account of the clustered nature of the sample. The results are presented showing the initial level of participation in work activities (that is prior to program implementation) and the impact estimates for each round of the survey carried out after program implementation. The impact from each round should be interpreted as the percentage point difference from the pre-program level (not from the previous round). In other words, the estimates reported represent the marginal effects of being in a household eligible for PROGRESA benefits on the probability of being in the labor

[11]This is the poor status after the densifcacion, the revision of the eligibility that raised the number of households eligible for the program from 52% to 78%. It has to be noticed that the fraction of households that actually ended up receiving the PROGRESA cash transfers during the two-year interval covered by the evaluation sample is just under 65%, due to administrative errors and delays in the final registration of beneficiary households.

[12]The complete set of parameter estimates is available directly from the authors upon request.

force.[13]

Beginning with men, the results of the impact of PROGRESA on overall participation levels show little impact. Moreover, irrespective of the age group examined, participation in PROGRESA seems to have no impact on labor market participation.

Looking at the decomposition between salaried work and other types of works, there are some impacts, particularly in the November 1998 round the first round after PROGRESA was implemented in these communities. In this round, there is a universal, for all age groups, increase in the probability of working in salaried work and a corresponding decrease in the probability of working in non-salaried work. These effects remain present in the next round of the data (June 1999) only for men aged 25 to 34 and disappear by the fourth round of the survey. The results seem to suggest that, at least initially, families may have used some part of the grants to seek work in salaried activities and to reduce their participation, perhaps, in less profitable family enterprises. This impact, however, appears to disappear over time.

For women, the results show few overall impacts of PROGRESA on participation in the labor market. For women in the age group 45-54, there is a significant reduction in participation according to the first after program round of the ENCEL, although this impact does not hold up over time. As with men, there is also a significant reduction in the probability of participating in non-salaried work activities in the first after program round, but again these effects do not hold up over time. In short, these data do not show particularly significant or lasting effects of PROGRESA on labor market participation. Rather, they are consistent with a story that PROGRESA does not affect participation of men and women.

In table 2.2 we present the results for the sample of individuals who are classified as non-eligible for the program (or E=0). Even though these individuals are not in PROGRESA (that only covers the "poor" individuals in treatment localities), the program may have been generating *spillover* effects; for example, households in PROGRESA localities who are not poor could have revised their labor force participation choices in order to become eligible for the program. However, our results suggest that PROGRESA does not have any significant effect on participation on individuals who are not poor; the only coefficients that are significant (only at 10% level) are all negative, but very small in magnitude.

2.4.2 — Leisure

Our measure of leisure time is constructed as a residual, that is as the difference between 24 hours and the time spent on all reported activities. In particular, we use the time use module (present only in the June 1999 round) with information on 18 activities[14] carried out during the previous

[13]The estimates reported were obtained using the "dprobit" command in STATA v7.0. They can be easily converted into percentage changes or elasticities by dividing the marginal effect by the pre-program level, both reported in table 2.1.

[14]Activities are: Working: for salary or wage, in own business or family land. Attending school, Doing homework after school, Community work, Voluntary work for neighbors or other relatives, Purchasing food or other products for the household (HH), Sewing, making clothes for HH members, Taking HH members to school, clinic, or work, Cleaning house, Washing and ironing clothes for HH members, Preparing food, Fetching water, firewood or throwing out trash, Taking care of animals, Taking care of small children, elderly and sick, Making HH repairs, Transportation time to work, school, market etc., Other activities.

day; the previous day as reference period is not particularly ideal, as for some individuals, the survey may refer to a day which was not "typical" of normal activities. Additionally, many activities may be activities which are done infrequently (i.e. not daily). The survey was carried out this way as it was thought that a "previous day" reference period would reduce recall bias, given the large number of activities included in the questionnaire. The control variables included are the same of those in the labor force participation estimation (discussed above).

Table 2.3 presents the CSDIF estimates of the effect of PROGRESA on adult leisure (summarized by parameter β_T in equation (2.7)). Note that one hypothesis of the impact of PROGRESA on leisure is that, if the PROGRESA transfers are perceived as strict income transfers, and leisure is a normal good, then one might expect leisure to increase with PROGRESA. Nevertheless, the structure of the grants which reduce the price of schooling of children and thus may reduce the work of children may imply that overall hours dedicated to household production work (previously done by children) might increase. This would then imply that the program would have an ambiguous effect on the leisure time of adults and especially women. For example, it is conceivable that complying with the program's requirements might decrease the leisure time of women, as they attempt to substitute for the time children used to allocate in home production activities.[15]

Overall, the results do not show significant impacts of PROGRESA on the leisure time of men or women. There are some small negative impacts of PROGRESA on leisure for men for one age group, namely men aged 18 to 24, which corresponds to increases in work for this group of men of about 0.3 hours daily, or about 2 hours weekly. Nevertheless, there are no significant impacts on any other age groups for males. The results for women are insignificant in all specifications and for all groups. Accordingly, we can say that there is not much evidence to support the hypothesis that PROGRESA has reduced the leisure time of men and women. There is certainly no evidence to support that leisure time has increased under PROGRESA[16].

§2.5. The impact of PROGRESA on poverty

The results presented so far on the impact of the program on adult labor market participation and leisure suggest that the program has no adverse effects on labor income. Thus, *ceteris paribus*, the cash transfers received by program beneficiaries are likely to increase household income at least among beneficiary families. However, the extent to which this occurs depends on the size of the direct and indirect costs associated with participation in the program. In addition, the income of non-eligible households may be adversely affected to the extent that non-eligible households believe that a lower household income increases their chances of becoming

[15] Skoufias and Parker (2001) and Schultz (2004) study the impact of PROGRESA on the work time and school attendance of school-age children. In particular, Skoufias and Parker (2001) find significant increases in the school attendance of boys and girls that are accompanied by significant reductions in the participation of boys and girls in work activities.

[16] Note, however, that the results on leisure do not necessarily suggest that there has been no reallocation of time between work activities for adults. For instance, there may have been a substitution towards more time in domestic work and less time in market work. (for results on this issue see Parker and Skoufias (2000)).

eligible for the program[17]. Estimating the impact of the program on the poverty rate provides a useful summary measure of how the program affects the income of both eligible and non-eligible households in the treatment localities relative to those in the control localities.

The measure of welfare used in our analysis is income per capita. [18][19] For households in treatment villages receiving PROGRESA cash transfers, total income per month was adjusted upwards by the cash transfer per month received by the household[20]. The actual amount of cash transfers received per month was obtained from the records of payments sent out each month since May 1998 by the PROGRESA administration headquarters in Mexico City. The monthly income measure calculated for each round of the survey was then converted in November 1998 pesos by dividing by the corresponding adjustment ratio of the national consumer price index. We calculate different poverty measures using two different poverty lines: the value of the basic food basket (*canasta basica*) and the median of the per capita value of consumption in November 1998. The first poverty line (basic food basket) yields a baseline poverty rate of 82.16% in the treatment localities (and 81.99% in the control localities). The median of per capita consumption in November 1998 yields a baseline headcount poverty rate P(0) of 55.44% in the treatment localities (53.16% in the control localities) which is slightly below the fraction of the population in the treatment villages that actually received the benefits of the program (see section 2.2 above).

Poverty is measured along the lines suggested by Foster et al. (1984), henceforth FGT. The FGT poverty measures are summarized by the formula:

[17]Our measure of income is based on reported income, with this bringing into the picture all the possible biases due to misreporting (especially underreporting) of income. As stated above eligibility for the program was decided based on socioeconomic data collected in the 1997 baseline household census (ENCASEH) before the start of the program. As by our knowledge households were not told that the ENCASEH information was going to be used to discriminate eligibility. In addition to this, household did not have to apply for the program but this was universally given to all households identified as eligible in the treatment localities. This suggests that there might not be a clear and strong incentive to misreport income. In general, evidence on this issue is scarce due to the severity of data requirements. Martinelli and Parker (2009) study this issues with data from the urban evaluation sample of Progresa (now Oportunidades; it has to be noticed that the rural and urban component of Progresa have very different characteristics: for example, households need to apply for the program in the urban version) and find that while underreporting is widespread also over reporting is common in goods that may have a “status” value.

[18]The absence of reliable information on household consumption prior to the start of the program precluded the use of consumption as a measure of the poverty impacts of the program. For more details on the consumption impacts of PROGRESA see Hoddinott and Skoufias (2004).

[19]We do not consider value of leisure in our definition of income (i.e.our income is not full income). Our definition of income is consistent with that used for deciding eligibility for the program. It is important to stress that the effect of the program on poverty may be depending upon the definition of income; for example, PROGRESA would have a priori a positive effect on poverty in case we employed a full income approach (and so we considered the value of leisure).

[20]Many studies have considered whether the introduction of public transfers affects private transfers among the households targeted by the public scheme. For example, Cox and Jimenez (1992) argue that public cash transfers may reduce the amount of private transfers to low-income households so that the net-income effect may be significantly less that the value of the public transfer. Albarran and Attanasio (2003) study this issue with Progresa data and find that the program does crowd our private transfers: both the likelihood to receiving a transfer and the amount received conditional on receiving private transfers are significantly and negatively affected by the programme.

$$P(\alpha) = \left(1/N\right) \sum_{i=1}^{q} \left(\frac{z - y_i}{z}\right)^{\alpha}, \tag{2.8}$$

where N is the total number of households, y_i is the per capita income of the ith household, z is the poverty line, q is the number of poor individuals, and α is the weight attached to the severity of household poverty (or the distance from the poverty line). When $\alpha = 0$, the FGT measure collapses to the Headcount Index, or the percentage of the population that is below the poverty line. When α= 1 the FGT measure gives the *poverty gap* P(1), a measure of the average depth of poverty. When $\alpha = 2$, the FGT index becomes the *severity of poverty* index. The P(2) measure assigns more weight to individuals that are further away from the poverty line[21].

As described in section 3, relying on reported household income allows one to obtain the difference-in-differences (2DIF) estimate of the impact of the program on poverty which compares the change in a poverty measure in treatment villages to the changes in the corresponding poverty measure in control villages. In addition to controlling for macroeconomic shocks common to both treatment and control localities, this estimate allows one to account for any pre-existing differences in poverty between control and treatment localities and thus yield "cleaner" estimate of the impact of the program on poverty.

The regression equation behind the estimation of PROGRESA's impact on poverty is:

$$\begin{aligned} P(i,t,\alpha) &= \beta_0 + \beta_T T(i) + \beta_{R2}(R2) + \beta_{TR2}(T(i) * R2) \\ &+ \beta_{R3}(R3) + \beta_{TR3}(T(i) * R3) + \beta_{R4}(R4) + \beta_{TR4}(T(i) * R4) + \eta(i,t) \end{aligned} \tag{2.9}$$

where the left hand side variable $P(i,t,\alpha)$ is defined as

$$P(i,t,\alpha) = \left(\frac{z - y(i,t)}{z}\right)^{\alpha} * Poor(i,t), \tag{2.10}$$

where $y(i,t)$denotes the income of household i in period/round t, z is the poverty line used, α takes on the values 0, 1, and 2, and $Poor(i,t)$ is a binary variable taking the value of 1 if $y(i,t) \leq z$, and equal to 0 otherwise. Based on the specification of the regression equation (2.9), the intercept term β_0 is the estimate of the poverty rate (headcount ratio, poverty gap, or the severity of poverty) in the control localities in the baseline round, while $\beta_0 + \beta_T$ is the corresponding estimate of poverty in the treatment localities (in the baseline round).[22] As discussed above, the estimates of the parameters β_{TR2}, β_{TR3}, and β_{TR4} are the 2DIF estimates of the impact of the program in rounds 2, 3, and 4 of the survey.

In Table 2.4 we present the estimates of the parameters of equation (2.9) along with standard errors adjusted for the clustering of households within villages. [23] The negative and strongly

[21]FGT poverty measures are related to stochastic dominance. In particular, first-order stochastic dominance (SD) implies that all P(a) for a >0 are robust to the choice of the poverty line; the same applies for all a >1 for second-order SD and for all a >2 for third-order SD.

[22]Along similar lines, $\beta_0 + \beta_{R2}$ is the poverty rate in control localities in round 2 and $\beta_0 + \beta_T + \beta_{R2} + \beta_{TR2}$ is the poverty rate in treatment localities in the same round.

[23]We have also estimated the impact of the program by symmetrically trimming the top and bottom five percent of the sample of observations in each round so as to eliminate extreme outliers from the sample. Using the trimmed sample resulted in slightly lower impacts of the program on poverty. Overall, however, the estimates

significant estimates of β_{TR2}, β_{TR3}, and β_{TR4} imply that PROGRESA had a significant impact in reducing poverty between November 1997 and November 1999. For example, using the 50th percentile of the value of consumption per capita as a poverty line, suggests that the headcount poverty rate declined by around 4.88% between November 1997 and November 1998 and by 18.11% in the November 1999 in treatment areas (using as base the 55.44% headcount poverty rate in treatment localities in November 1997). Over the same period, and using as base the corresponding value of the poverty gap and squared poverty gap indices in treatment areas in November 1997, the poverty gap measure declined by 37.40%, and the severity of poverty measure (squared poverty gap) declined by 47.42%. The higher impacts of the program in reducing poverty over time are consistent with the findings of Gertler et al. (2006), who demonstrated that rural households increased their investments in micro enterprises and agricultural activities which, in turn, improved the households' ability to generate income.

In general, these estimates are remarkably in line with the estimates obtained using *ex-ante* simulations. These simulations are based on the predicted consumption of each household in the evaluation sample in November 1997 and adding the maximum amount of PROGRESA cash transfers an eligible household could receive assuming full compliance with the program's requirements (see Skoufias et al. (2001)). In particular, the results obtained from the simulated impact of PROGRESA's cash transfers show that the headcount ratio is reduced by about 10% through the supports of PROGRESA. The poverty gap and severity of poverty measures, that place greater weight on the poorest households within the population in poverty, show that the level of poverty according to the poverty gap is reduced by 30% whereas the severity of the poverty index (squared poverty gap) is reduced by 45%.

The choice of a poverty line is a major concern when poverty measures are estimated. In addition to estimating program impact on poverty based on two alternative poverty lines, we also conduct tests of stochastic dominance, up to order three, between the distribution of monthly income in treatment and control areas for each round. As far as the program impact on poverty is concerned, stochastic dominance of one distribution on the other can have important implications. The order of stochastic dominance achieved leads to different conclusions as regards poverty measures considered, with first-order stochastic dominance being the most stringent criteria. For instance, if the distribution of income in treatment areas first-order stochastically dominates that in control areas, this implies that all the poverty measures we are considering [headcount ratio P(0), poverty gap P(1), and squared poverty gap P(2)] will always show less poverty in treatment areas no matter which poverty line is chosen. Higher orders of stochastic dominance mean that poverty will be less in treatment areas regardless of the poverty line only according to a smaller set of poverty measures; for example, second-order stochastic dominance implies only P(1) and P(2) showing less poverty in treatment areas regardless of the poverty line. In brief, stochastic dominance is explored here in order to understand whether there is evidence of a program impact on poverty that is robust to choice of a poverty line.

Results from our test of stochastic dominance (for details see Davidson and Duclos (2000);

of the impact of the program on poverty did not change the results presented above in any substantial manner, which implies that the role of outlier observations in income is trivial.

test results are in 2.5.1) add some robustness to the evidence above. In particular, we find that the distribution of income for population of treated households first-order stochastically dominates that of control group in round June 1999 and November 1999 (while for round November 1998 we find third-order stochastic dominance; see also figure 2.2 where we report the estimated CDF's and poverty deficit curves[24]). An implication of these results is that in June and November 1999 P(0) (and so P(1) and P(2)) will be always smaller in treatment than in control group regardless of the poverty line chosen.

In conclusion, since we find the strongest evidence of poverty reduction when we analyze poverty gap and severity of the poverty index (poverty gap squared), which put greater weight on the poorest of the poor, both the simulation and the actual ex-post results suggest that the largest reductions in poverty of PROGRESA are being achieved in the poorest of the poor population.

2.5.1 — Test of Stochastic dominance and poverty

Test results

Z = grid point for income

D1 = statistics for distribution if households in treatment areas and poor

D2 = statistics for distribution if households in non treatment areas and poor

Nov 1999

Minimum test point is 22.631578

Maximum test point is 143.33333

Order 1

Z D1 D2 t-statistic

22.63 0.08 0.12 3.21

28.98 0.10 0.13 2.40

35.34 0.11 0.13 1.56

41.69 0.13 0.14 0.94

48.04 0.14 0.15 0.82

54.40 0.16 0.17 0.82

60.75 0.17 0.19 1.14

67.10 0.19 0.21 1.29

73.45 0.21 0.23 0.87

79.81 0.23 0.25 0.71

86.16 0.26 0.28 1.30

92.51 0.28 0.30 1.03

98.86 0.30 0.32 0.55

105.22 0.34 0.37 1.78

111.57 0.36 0.39 1.33

[24]The estimated CDF gives the P(0) for any level of income. Poverty deficit curve is defined as the area under the CDF up to some poverty line. If the poverty deficit curve of one distribution lies above the poverty deficit curve of another, the first distribution will always have more poverty according to the poverty-gap measure, P(1).

117.92 0.38 0.41 1.05

124.28 0.41 0.44 1.46

130.63 0.44 0.47 1.70

136.98 0.46 0.50 1.72

143.33 0.49 0.52 1.48

Order 2

Z D1 D2 t-statistic

22.63 1.15 2.30 5.05

28.98 1.71 3.08 4.52

35.34 2.37 3.91 4.01

41.69 3.15 4.79 3.50

48.04 4.01 5.73 3.10

54.40 4.96 6.75 2.80

60.75 6.00 7.88 2.58

67.10 7.13 9.13 2.43

73.45 8.41 10.52 2.29

79.81 9.83 12.04 2.16

86.16 11.40 13.73 2.08

92.51 13.13 15.60 2.01

98.86 15.00 17.57 1.93

105.22 17.05 19.84 1.92

111.57 19.26 22.25 1.91

117.92 21.62 24.78 1.88

124.28 24.15 27.51 1.86

130.63 26.85 30.47 1.88

136.98 29.71 33.58 1.88

143.33 32.72 36.82 1.88

Order 3

Z D1 D2 t-statistic

22.63 10.89 24.48 5.52

28.98 19.91 41.56 5.23

35.34 32.82 63.76 4.91

41.69 50.33 91.36 4.57

48.04 73.05 124.71 4.24

54.40 101.51 164.29 3.94

60.75 136.29 210.72 3.68

67.10 177.93 264.70 3.45

73.45 227.21 327.05 3.26

79.81 285.06 398.63 3.10

86.16 352.41 480.38 2.95

92.51 430.24 573.48 2.83

98.86 519.53 678.79 2.72
105.22 621.25 797.41 2.62
111.57 736.53 931.05 2.55
117.92 866.31 1080.39 2.48
124.28 1011.61 1246.36 2.42
130.63 1173.52 1430.43 2.37
136.98 1353.06 1633.74 2.32
143.33 1551.26 1857.31 2.29
Dominance achieved at order 3
June 1999
Minimum test point is 20.678703
Maximum test point is 130.96512
Order 1
Z D1 D2 t-statistic
20.68 0.11 0.20 8.30
26.48 0.13 0.21 6.52
32.29 0.16 0.21 4.55
38.09 0.17 0.22 3.72
43.90 0.19 0.23 2.61
49.70 0.21 0.24 2.41
55.51 0.22 0.25 2.27
61.31 0.24 0.27 2.52
67.12 0.25 0.29 2.51
72.92 0.26 0.31 2.91
78.72 0.28 0.34 3.64
84.53 0.30 0.36 3.49
90.33 0.33 0.41 4.42
96.14 0.35 0.42 3.91
101.94 0.36 0.43 3.43
107.75 0.39 0.46 3.97
113.55 0.42 0.49 3.74
119.36 0.44 0.51 3.67
125.16 0.46 0.52 3.00
130.97 0.48 0.54 2.97
Dominance achieved at order 1
Nov 1999
Minimum test point is 25.789475
Maximum test point is 163.33334
Order 1
Z D1 D2 t-statistic
25.79 0.04 0.10 6.72

33.03 0.05 0.11 6.16
40.27 0.06 0.11 5.82
47.51 0.07 0.12 5.66
54.75 0.08 0.14 6.83
61.99 0.09 0.17 7.27
69.22 0.10 0.19 7.16
76.46 0.12 0.22 7.83
83.70 0.14 0.24 7.37
90.94 0.16 0.29 8.24
98.18 0.18 0.31 7.23
105.42 0.21 0.35 8.05
112.66 0.24 0.38 7.59
119.90 0.27 0.41 7.23
127.14 0.30 0.44 6.63
134.38 0.34 0.48 6.73
141.62 0.37 0.50 6.41
148.86 0.40 0.54 6.71
156.09 0.42 0.56 6.45
163.33 0.45 0.57 5.66
Dominance achieved at order 1

§2.6. Conclusions

Conditional cash transfer (CCT) programs aim to alleviate current poverty through monetary and in-kind benefits, as well as reduce future levels of poverty by encouraging investments in education, health and nutrition. The success of CCT programs at reducing current poverty depends on whether, and the extent to which, cash transfers affect adult work incentives.

Based on the experimental design of PROGRESA's evaluation sample, our findings yield a very consistent answer. PROGRESA does not have any significant effect on adult labor supply choices. In particular, the results show that there has been no particular reduction in labor market participation rates, as may have been predicted by some economic models of behavior. There is some evidence that individuals, at least right after they started to receive the transfers, may have used part of the grants to seek work in salaried activities and to reduce their participation in perhaps less profitable family enterprises. This impact, however, appears to disappear over time (see the results for the last round, November 1999). In addition, there is not much evidence to support the hypothesis that PROGRESA beneficiaries use their transfers to "buy" more leisure.

The success of CCT programs at reducing current poverty depends on whether, and the extent to which, cash transfers affect adult work incentives. In particular, policy-makers have concerns about possible adverse effects on labor supply, such as disincentive to work for eligible households or even for non-eligible households (that might hope to become eligible for the program in future rounds of expansion of the program). It is then a welcome result finding that PROGRESA is not having a significant effect on adult labour supply (and so any adverse effect). We also stress that the program does not have the objective of affecting adult labor supply, with this suggesting that its design is working properly.

Our findings on adult work incentives are reinforced further by the result that PROGRESA leads to a substantial reduction in current poverty. The poverty reduction effects are stronger for the poverty gap and severity of poverty measures, which put greater weight on the poorest of the poor, and our evidence suggests that these estimated poverty measures are robust to the choice of different poverty lines. As an additional piece of evidence, we notice that our results on the PROGRESA impact on poverty rates are remarkably in line with the *ex-ante* simulated impact of PROGRESA's cash transfers (as in Skoufias et al. (2001)). Thus, ex-ante simulations that ignore or assume away behavioral responses to the transfer (as in Bourguignon et al. (2003)) are likely to provide good estimates of the *ex-post* impact on poverty.

Table 2.1: The impact of PROGRESA (2DIF estimates) on the probability of working among eligible (E=1) adults

Age group	Males: pre-prog labor force	Males Oct. 98 coef.	se	t-stat	Males Jun-99 coef.	se	t-stat	Males Nov. 99 coef.	Se	t-stat	Females: pre-prog labor force	Females Oct. 98 coef.	se	t-stat	Females Jun-99 coef.	se	t-stat	Females Nov. 99 coef.	se	t-stat
All Work																				
18-24	0.86	-0.005	0.018	-0.32	-0.011	0.019	-0.58	0	0.019	0.04	0.18	-0.022	0.016	-1.3	-0.037**	0.015	-2.23	-0.02	0.017	-1.13
25-34	0.94	-0.003	0.01	-0.32	-0.014	0.01	-1.45	0.001	0.009	0.2	0.16	-0.012	0.015	-0.77	-0.019	0.013	-1.33	-0.012	0.015	-0.78
35-44	0.95	0.005	0.009	0.54	0.009	0.009	0.95	0.013	0.008	1.39	0.18	-0.014	0.013	-0.99	0.011	0.018	0.61	-0.017	0.016	-0.99
45-54	0.94	0	0.013	0.01	-0.003	0.014	-0.26	0.014	0.011	1.18	0.18	-0.038**	0.014	-2.36	-0.03*	0.014	-1.91	-0.012	0.02	-0.61
55 +	0.78	-0.008	0.019	-0.44	-0.02	0.023	-0.89	0	0.019	0	0.15	0.002	0.014	0.21	0.011	0.013	0.83	0.032**	0.017	1.96
All adults	0.89	-0.003	0.009	-0.36	-0.007	0.009	-0.81	0.006	0.008	0.7	0.17	-0.014	0.011	-1.24	-0.013	0.01	-1.18	-0.006	0.012	-0.5
Salaried work																				
18-24	0.64	0.04*	0.025	1.78	0.021	0.026	0.8	0.023	0.03	0.76	0.1	-0.003	0.012	-0.29	-0.026**	0.011	-2.08	-0.016	0.011	-1.34
25-34	0.7	0.034	0.025	1.27	0.037*	0.021	1.65	0.017	0.026	0.67	0.07	0.005	0.008	0.6	-0.003	0.008	-0.33	0.006	0.009	0.76
35-44	0.66	0.044*	0.025	1.65	0.034	0.025	1.3	0.014	0.029	0.49	0.07	0.002	0.009	0.31	0.006	0.01	0.63	-0.003	0.009	-0.37
45-54	0.62	0.054*	0.031	1.66	0.038	0.031	1.19	0.043	0.031	1.36	0.05	-0.001	0.01	-0.16	-0.005	0.009	-0.56	0.008	0.012	0.68
55 +	0.43	0.04	0.034	1.18	0.025	0.031	0.79	0.019	0.033	0.58	0.04	0.005	0.007	0.77	0.006	0.008	0.85	0.013*	0.008	1.74
All adults	0.61	0.045	0.023	1.87	0.034	0.021	1.56	0.025	0.023	1.06	0.07	0.002	0.005	0.36	-0.004	0.005	-0.74	0.001	0.006	0.2
Self-employed/family business																				
18-24	0.21	-0.035*	0.018	-1.8	-0.017	0.02	-0.85	0	0.021	0.01	0.07	-0.013	0.009	-1.33	-0.004	0.01	-0.37	0.008	0.014	0.64
25-34	0.24	-0.03	0.022	-1.27	-0.048	0.017	-2.57	-0.01	0.023	-0.46	0.09	-0.008	0.01	-0.76	-0.01	0.008	-1.18	-0.012	0.011	-1.05
35-44	0.29	-0.036	0.024	-1.43	-0.02	0.023	-0.82	0.02	0.028	0.07	0.11	-0.013	0.008	-1.48	0.02	0.016	1.35	-0.008	0.012	-0.67
45-54	0.31	-0.047	0.03	-1.46	-0.037	0.029	-1.2	-0.023	0.029	-0.79	0.13	-0.022*	0.01	-2.22	-0.018	0.01	-1.53	-0.004	0.016	-0.29
55 +	0.35	-0.031	0.027	-1.13	-0.029	0.024	-1.18	-0.002	0.027	-0.1	0.1	-0.002	0.01	-0.2	0.007	0.01	0.72	0.018	0.016	1.22
All adults	0.28	-0.035	0.019	-1.75	-0.031	0.017	-1.78	-0.007	0.02	-0.38	0.1	-0.011	0.007	-1.46	-0.002	0.008	-0.27	0	0.01	-0.07

****=significant at 1% level, **=significant at 5% level, *=significant at 10% level*
See text for more details

Table 2.2: The impact of PROGRESA (2DIF estimates) on the probability of working among non-eligible (E=0) adults

Age group	Pre-prog labor force participation	Impact on Males									Pre-prog labor force participation	Impact on Females								
		Oct. 98			Jun-99			Nov. 99				Oct. 98			Jun-99			Nov. 99		
		coef.	se	t-stat	coef.	se	t-stat	coef.		t-stat		coef.	se	t-stat	coef.	se	t-stat	coef.	se	t-stat
All Work																				
18-24	0.88	0.003	0.026	0.15	-0.029	0.029	-1.03	-0.016	0.029	-0.57	0.29	0.001	0.03	0.05	-0.006	0.03	-0.22	-0.003	0.034	-0.1
25-34	0.95	-0.021	0.021	-1.03	-0.036*	0.023	-1.74	0.002	0.02	0.11	0.32	0.015	0.033	0.47	0.039	0.037	1.08	-0.01	0.034	-0.3
35-44	0.96	-0.022	0.023	-1.1	-0.008	0.02	-0.43	0.004	0.02	0.21	0.26	-0.018	0.031	-0.57	0.012	0.031	0.39	-0.061**	0.028	-1.96
45-54	0.96	-0.032	0.026	-1.34	-0.016	0.024	-0.85	0.015	0.019	0.71	0.21	0.018	0.028	0.69	0.007	0.028	0.25	-0.032	0.021	-1.42
55 +	0.88	-0.06	0.024	-0.26	-0.026	0.028	-0.97	-0.017	0.027	-0.65	0.18	-0.034*	0.016	-1.9	-0.031*	0.017	-1.67	-0.035*	0.019	-1.62
All adults	0.92	-0.014	0.011	-1.23	-0.024	0.012	-2.03	-0.007	0.011	-0.63	0.27	-0.01	0.017	-0.59	-0.002	0.017	-0.14	-0.03	0.017	-1.71
Salaried work																				
18-24	0.57	0.014	0.037	0.39	-0.015	0.047	-0.32	-0.029	0.04	-0.73	0.17	0.003	0.024	0.16	0.021	0.029	0.74	0.024	0.029	0.87
25-34	0.58	0.028	0.034	0.83	0.023	0.038	0.61	0.027	0.04	0.68	0.15	0.029	0.025	1.23	0.015	0.027	0.57	0.007	0.024	0.31
35-44	0.54	0.042	0.04	1.02	0.046	0.04	1.12	0.05	0.048	1.01	0.11	0.01	0.02	0.5	-0.01	0.019	-0.5	-0.007	0.019	-0.35
45-54	0.47	-0.035	0.051	-0.7	-0.032	0.0511	-0.63	0.028	0.511	0.55	0.08	0.001	0.015	0.08	0.004	0.016	0.25	-0.02*	0.009	-1.79
55 +	0.37	0.013	0.044	0.3	-0.01	0.041	-0.24	-0.032	0.041	-0.77	0.04	0.001	0.009	0.14	-0.005	0.009	-0.55	-0.003	0.008	-0.43
All adults	0.5	0.016	0.028	0.59	0.002	0.03	0.1	0.001	0.029	0.04	0.11	0.005	0.009	0.57	0.002	0.011	0.24	-0.002	0.009	-0.3
Self-employed/family business																				
18-24	0.31	-0.02	0.03	-0.64	-0.01	0.033	-0.3	0.009	0.035	0.29	0.11	0.013	0.021	-0.7	-0.016	0.014	-1.01	-0.019	0.016	-1.08
25-34	0.38	-0.043	0.029	-1.41	-0.052	0.031	-1.57	-0.019	0.036	-0.52	0.16	0	0.021	-0.04	0.025	0.027	1.01	-0.015	0.02	-0.71
35-44	0.42	-0.054	0.037	-1.42	-0.046	0.037	-1.2	-0.037	0.045	-0.8	0.18	-0.015	0.023	-0.62	0.021	0.025	0.81	-0.044*	0.02	-1.85
45-54	0.49	0.01	0.046	0.22	0.02	0.047	0.43	-0.009	0.048	-0.18	0.17	0.011	0.023	0.49	0.006	0.021	0.3	-0.002	0.02	-0.14
55 +	0.51	-0.017	0.038	-0.45	-0.01	0.038	-0.27	0.021	0.039	0.54	0.18	-0.026*	0.012	-1.89	-0.02	0.014	-1.28	-0.026	0.015	-1.47
All adults	0.42	-0.026	0.024	-1.06	-0.02	0.025	-0.79	-0.002	0.027	-0.1	0.16	-0.005	0.012	-0.45	-0.001	0.012	-0.14	-0.022	0.011	-1.85

****=significant at 1% level, **=significant at 5% level, *=significant at 10% level*
See text for more details

Table 2.3: The impact of PROGRESA on leisure time of program eligible (E=1) adults

Age group	Pre-prog daily hours	Men Impact Jun-99			Pre-prog daily hours	Women Impact Jun-99		
		coef.	se	t-stat		coef.	se	t-stat
18-24	16.24	-0.321*	0.169	-1.9	17.18	0.026	0.087	0.3
25-34	14.69	0.122	0.122	1	16.17	-0.236	0.148	-1.6
35-44	14.64	-0.061	0.087	-0.7	16.65	-0.016	0.160	-0.1
45-54	14.72	0.06	0.200	0.3	17.44	0.023	-0.230	-0.1
55 +	16.63	-0.144	0.206	-0.7	19.21	0.09	0.150	0.6

**=significant at 10% level*

Figure 2.1: Percentage of Households in Treatment Localities that Receive the Transfers from Other Programs and PROGRESA

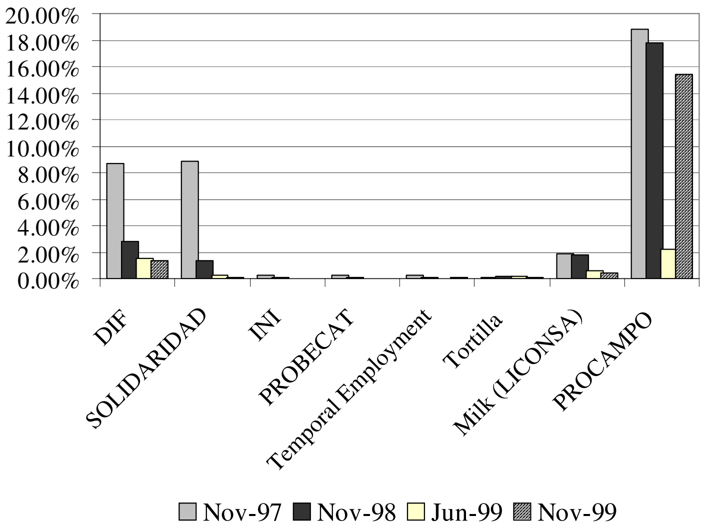


Table 2.4: The impact of PROGRESA on poverty using two different poverty lines

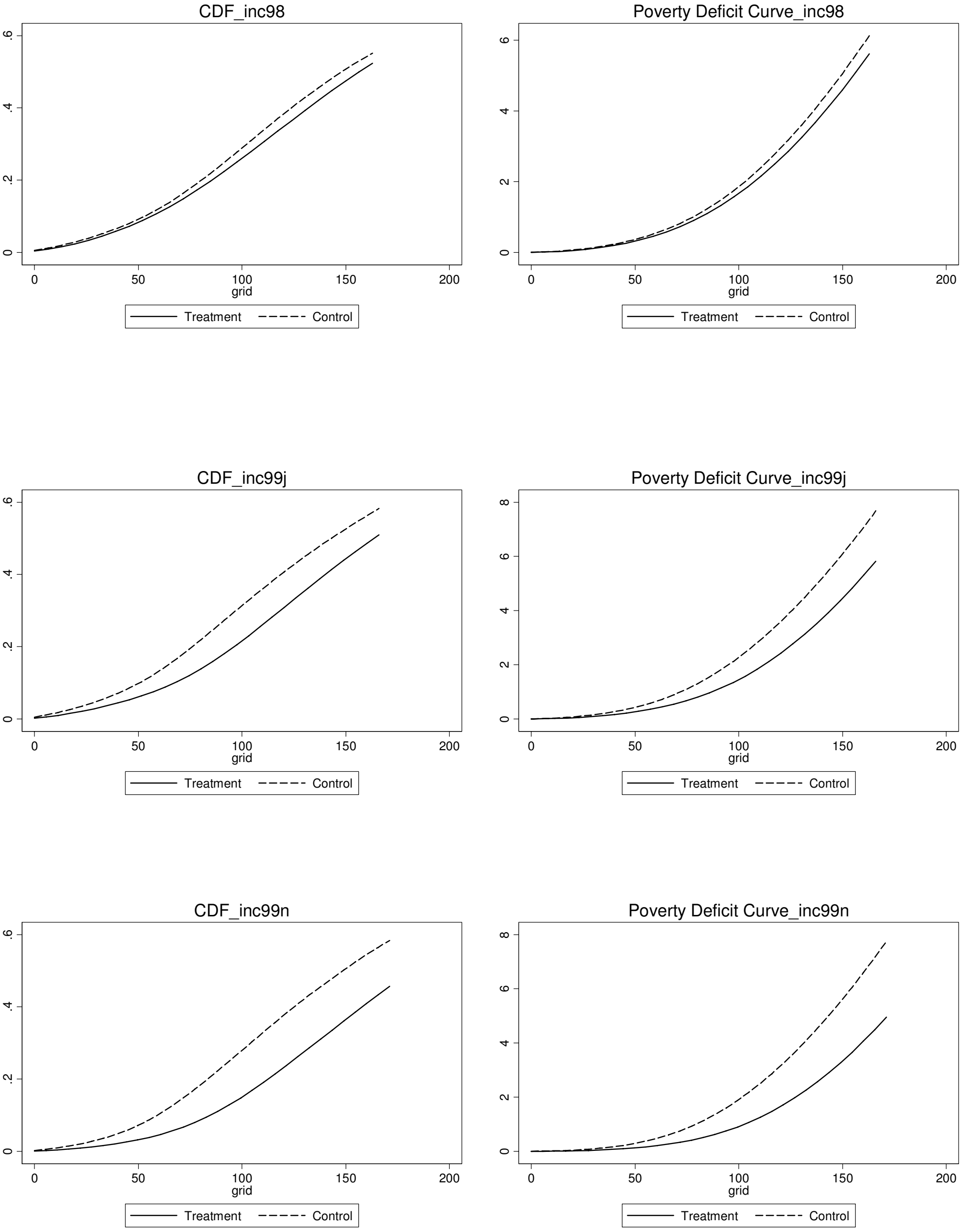
Poverty line: Canasta basica in Nov 97						Poverty line: Median of Nov 98 Consumption p.c.					
(a) Headcount Ratio P(0)											
Variable	**Coeff.**	**st.error**	**t-val.**	**p-val.**	**Impact***	**Variable**	**Coeff.**	**st.error**	**t-val.**	**p-val.**	**Impact***
T	0.0017	0.012	0.13	0.893		T	0.0228	0.019	1.23	0.219	
R2	0.0183	0.007	2.49	0.013		R2	0.0379	0.012	3.28	0.001	
R2xT	0.0007	0.009	0.08	0.938	0.09	R2xT	-0.0271	0.014	-1.89	0.059	-4.88
R3	0.0429	0.009	4.91	0		R3	0.1120	0.011	10.46	0	
R3xT	-0.0060	0.011	-0.57	0.568	-0.73	R3xT	-0.0545	0.014	-4.03	0	-9.83
R4	0.0392	0.006	6.29	0		R4	0.0533	0.012	4.39	0	
R4xT	-0.0207	0.009	-2.26	0.024	-2.52	R4xT	-0.1004	0.016	-6.3	0	-18.11
_cons	0.8199	0.010	78.2	0		_cons	0.5316	0.015	36.44	0	
(b) Poverty Gap P(1)											
Variable	**Coeff.**	**st.error**	**t-val.**	**p-val.**	**Impact***	**Variable**	**Coeff.**	**st.error**	**t-val.**	**p-val.**	**Impact***
T	0.0215	0.014	1.53	0.126		T	0.0333	0.014	2.32	0.021	
R2	0.0399	0.008	4.7	0		R2	0.0425	0.010	4.36	0	
R2xT	-0.0284	0.011	-2.61	0.009	-5.70	R2xT	-0.0457	0.013	-3.6	0	-15.92
R3	0.0992	0.008	11.97	0		R3	0.1248	0.009	14.19	0	
R3xT	-0.0445	0.011	-4.2	0	-8.95	R3xT	-0.0717	0.012	-6.08	0	-24.99
R4	0.0375	0.008	4.59	0		R4	0.0306	0.009	3.33	0.001	
R4xT	-0.0794	0.012	-6.74	0	-15.94	R4xT	-0.1074	0.014	-7.71	0	-37.40
_cons	0.4763	0.011	45	0		_cons	0.2538	0.010	26.6	0	
(c) Severity of Poverty P(2)											
Variable	**Coeff.**	**st.error**	**t-val.**	**p-val.**	**Impact***	**Variable**	**Coeff.**	**st.error**	**t-val.**	**p-val.**	**Impact***
T	0.0287	0.014	2.1	0.037		T	0.0346	0.013	2.65	0.008	
R2	0.0412	0.009	4.58	0		R2	0.0382	0.009	4.18	0	
R2xT	-0.0393	0.012	-3.37	0.001	-10.96	R2xT	-0.0488	0.012	-4	0	-24.21
R3	0.1151	0.008	13.93	0		R3	0.1257	0.008	15.03	0	
R3xT	-0.0616	0.011	-5.61	0	-17.15	R3xT	-0.0774	0.011	-6.73	0	-38.42
R4	0.0325	0.008	3.83	0		R4	0.0223	0.008	2.7	0.007	
R4xT	-0.0938	0.013	-7.36	0	-26.13	R4xT	-0.0955	0.014	-7.03	0	-47.42
_cons	0.3301	0.010	34.68	0		_cons	0.1669	0.008	21.11	0	

**The columns labeled "impact" contain the 2DIF estimate of the program's impact in each round as a percent of the respective poverty measure in the treatment localities in the baseline round.*

Table 2.5: Mean Household Income from Children

		Nov-97		Nov-98		Jun-99		Nov-99	
		Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Total	**Mean**	**150**	**147**	**132**	**145**	**93**	**116**	**111**	**119**
	SD	137	135	112	198	99	115	147	138
Labor	Mean	113	114	117	105	86	85	102	108
	SD	140	140	130	201	112	117	151	137
Other	**Mean**	**37**	**34**	**15**	**40**	**7**	**31**	**9**	**11**
	SD	12	11	23	7	22	5	9	6

Figure 2.2: Estimated Distribution Function and Poverty Deficit Curve



CHAPTER 3

Looking within the household: Impacts for individual outcomes using aggregate household data

While most development policies target individuals, the estimation of the program impact on individual outcomes might be *a priori* impossible due to lack of individual level data on key variables, such as food consumption. This paper applies an approach to infer individual outcomes impacts when only aggregate household data is available to estimate the impact of a program. In particular, we estimate the impact of PROGRESA-*Oportunidades* on individual caloric intake. Our main results show that the program is having a stronger impact at younger ages (both for males and females) and for females up to age 30 (mothers). These findings are remarkably in line with the program's design. When we allow for household composition in the model, we show that number of children living in the household can make a difference in terms of response to the program. Another issue explored here is whether there is asymmetric information within the household as regards food consumption. Preliminary evidence show that women might not have a complete information on food intakes of adult male members within the household.

§3.1. Introduction

Most development objectives focus on the well-being of individuals. The welfare of an individual is largely based on the set of economic and social interactions in which he/she is involved. These interactions can affect, and be affected by, the creation, existence, and dissolution of institutions within which the individual is situated, of which family and households are the most important ones. Within family and households we commonly refer to the processes of allocation of resources among individuals and the outcomes of those processes as *intrahousehold resource allocation.*

Development policies, while commonly targeting individuals, might be missing two important points as far as the individual dimension is concerned. At the design stage policies do not always acknowledge the intrahousehold resource allocation. One of the main results of the vast literature[1] on resource allocation models predicts that neglecting patterns of intrahousehold inequalities can lead to policy failure[2].

[1]See the review in Haddad et al. (1997)

[2]Examples are in Haddad and Kanbur (1992), Pelletier et al. (1991), Senauer and Garcia (1992), Beaton and Ghassemi (1982), Kennedy and Alderman (1987), Apps and Savage (1989) and **?**)

Another problem arises at the interventions' evaluation stage: even if the program is targeting individuals, it might be impossible to estimate the impact of the program on individual outcomes simply because of lack of data at the individual level, with this being especially true for nutritional outcomes. Obviously, the choice of collecting household level data (and not individual data) is commonly dictated by reasons that are not directly related to the program's evaluation process: household budget surveys are less expensive and require less time for the interviews; individual surveys might be too intrusive, for example as regards eating habits; information collected in household budget surveys might be more comparable between countries.

This paper focuses on this second issue since it shows how it is possible to estimate the program impact on individual outcomes when only aggregate household data is available. In particular, a methodology for inferring individual outcomes from aggregate household data, introduced by Chesher (1997), is used to estimate the impact of PROGRESA[3] on individual caloric intake. Key features of the PROGRESA's design and evaluation sample make it very suitable for the purposes of this paper: first, while all the members of the household can potentially benefit from the program's benefits, PROGRESA has a particular focus on improving educational, health and nutritional status of specific members within the household: children and pregnant and lactating mothers. In addition to this and as regards nutrition outcomes, consumptions of food are only observed at the household level in the evaluation sample. Another key aspect refers to the assignment of the program that is completely exogenous being based on randomization between treatment and control localities.

The positive and significant impact of PROGRESA on average household caloric availability is an established result in the literature: **?**) find that by November 1999, beneficiary households in treatment localities obtained around 7% more calories than did comparable households in control localities. Here we try to shed some light on how this increased caloric availability is shared within the household. Our results show that the program is having a stronger impact on caloric availability for younger ages (both male and females) and for females up to age 30 (mothers). This result is remarkably in line with the very program design, which focuses particularly on welfare of children and their mothers.

A strong assumption of the simple food consumption model used to derive individual consumption from aggregate data is separability between individual and household characteristics. For instance, this assumption implies that the consumption levels of one child do not depend on age and/or presence of other children in the household, which is undoubtedly a quite strong assumption. We relax this assumption in a very simple way including dummies for some age groups in our model. When we allow for household composition, we show that the different age categories can make a difference in terms of impact confirming the problems in maintaining the assumption above.

One of the findings motivated the analysis of another issue explored here: we find that while the estimated calorie-age profile for females displays reasonable values in terms of per capita daily calorie, the profile for the sample of males shows unreasonably low values (especially for

[3]PROGRESA, now known as *Oportunidades*, is an intervention targeting poor households in rural Mexico

adults; see results in section 3.5). We argue that this can be the result of asymmetric information within the family regarding food consumptions. In particular, when the respondent to the survey questions is female (in our sample 85% of respondents) she might not hold a complete information of intakes of adult male members of the household and therefore she might be understating their food consumption. Some preliminary evidence confirms this pattern.

The rest of the paper is organized as follows. Section 3.2 describes PROGRESA's program design and evaluation sample. The methodology for inferring individual outcomes when only household level data is available is described in section 3.3. In section 3.4 we present the estimated nutrient-age/sex profile. Section 3.5 present and discusses our results on individual caloric consumption. The results after allowing for household composition are discussed in section 3.6. In section 3.7 we present some evidence of the possible presence of asymmetric information regarding food consumptions. Finally, section 3.8 draws some concluding remarks.

§3.2. Progresa (now *Oportunidades*): details of the program and evaluation sample

Program and data are already described at length in chapter 1 and 2 (see section 1.3 and section 2.2). Here, we only add some details on some of the components of the program.

The health and nutrition component of the program aims to improve the health and nutritional status of all household members with a special attention for children and mothers' welfare. PROGRESA pursues its ends by the following interrelated sub-components: a basic package of primary health care services, nutrition and health education for families and communities, improved supply of health services and nutritional supplement for pregnant and lactating mothers and young children. The nutritional supplement has the objective of prevent malnutrition in infants and small children. In order to achieve this end the program provide a food supplement to pregnant and lactating women and to children between 4 months and 2 years old. However, the food supplement is provided anyway if signs of malnutrition are still noticed in children between age 2 and 5 and in non-PROGRESA children. There are 2 type of food supplement, one specific for pregnant and lactating women and one for young children. They are distributed in packages of grams each and are ready-to-eat after hydration. A 40 grams daily ration of the package (of dry product) supplies approximately one recommended daily allowance of selected micronutrients. The packages are distributed to health centres trough DICONSA, an operational arm of the Ministry of Social Development (SEDESOL). About 18000 DICONSA stores are spread in rural areas. In particular, mothers pick up a one-month supply of the supplements for each targeted household member during one of their visits to the clinic (they have to visit the clinic at least once a month). *Plàticas* provided in the clinics address the issues of appropriate use of food supplements and optimal child feeding.

Community participation and promotion of a culture of preventive care are the underlying assumptions that characterize PROGRESA health and nutrition component. Beneficiaries are asked to visit health centres on a scheduled timetable basis and to attend health and nutritional talks, *plàticas*. Only those households who comply for these requirements are then eligible to receive the cash grant for food consumption (125 pesos per month between July and December

1999).

As in previous chapters, our estimates will be exploiting the unique feature of PROGRESA evaluation sample: randomization of localities between treatment and control group: 506 localities selected 320 were assigned to the treatment group and 186 to the control group, this assignment corresponding to a probability of being assigned to the treatment (control) group of 60% (40%).

§3.3. From household level data to individual outcomes.

In order to estimate consumptions for individuals using the household level consumption we observe we use a method developed in Chesher (1997). The description of this method, which follows, will largely draw on section 3 and 4 of Chesher (1997). Its building block is a simple model of household food intakes. Consider a household with P people each with a consumption of a given nutrient cp, p=1,. . . ,P. The characteristic of person p (for example, age and sex) are denoted by xp, household composition is measured by a vector $x = (x1,\dots,xp)$ which lists the characteristics of all household members and z denotes household characteristics such as income, living in urban or rural areas, or being a farm household and, in principle, could contain functions of household composition x. The average rate of consumption by person p conditional on household composition and household characteristics can be then written as a function of individual and household characteristics:

$$E\left[c_p|x,z\right] = f(x_p,z) \tag{3.1}$$

In an intake survey of individuals we would observe c_p and we could estimate these functions directly. Having only a survey of household intakes, the estimation of these functions requires further steps. We first notice that during a given recording period household nutrient consumption c is just the sum of nutrient consumption by individuals, in expectation we have:

$$E\left[c|x,z\right] = \sum_{p=1}^{P} f(x_p,z) \tag{3.2}$$

With data on household consumption the function $f(x_g,z)$ could be estimated by exploiting the moment conditions

$$E\left[\left\{c - \sum_{p=1}^{P} f(x_p,z)\right\} g(x,z)|x,z\right] = 0 \tag{3.3}$$

which hold for arbitrary functions $g(x,z)$. Chesher (1997) stresses that when only household totals are available all that is available for estimating individual specific rates of consumptions is a method such as the one above.

Some more structure is needed as regards the nutrient consumption functions $f\left(x_p,z\right)$. The simplifying assumption we make is that these functions can be written as multiplicatively separable functions of individual and household characteristics: $f\left(x_p,z\right) = f\left(x_p\right)u\left(z\right)$. This means that ratios of consumptions by identical individuals (that is, having the same x_p) in households with different characteristics are the same for all types of individuals. In other terms, a change in household characteristics will have the same relative effect at each individual type. For instance, if we have only two types of households, "rich" and "poor", and x_p= age, then this assumption is implying that the consumption of a given individual type in a "rich" household is going to be always the same proportion of the consumption of the same individual type (that is, of the same age) in a "poor" household at any age (that is, at any x_p). Undoubtedly this is a strong assumption, which is required to identify the individual consumptions when only household level intakes are observed.

The individual characteristics we consider are age (a_p) and sex (s_p) with s_p=1 if person is male and 0 otherwise) and we allow for different functions for males and females:

$$f(x_p) = s_p f_M(a_p) + (1 - s_p) f_F(a_p)$$

where $f_M(\cdot)$ and $f_F(\cdot)$are age-intake functions for respectively males and females.

Household characteristics are modeled with a parametric form, $u\left(z\right) = \exp\left(z'\gamma\right)$.

A parametric specification would be difficult to achieve for the age-intake functions, as the underlying relationship between intake and age is complex and high non-linear: demand for nutrients varies substantially over the life-cycle and is affected by tastes which also vary through the life-cycle. The same nonparametric approach as in Chesher (1997) is followed here. In particular, a roughness penalty approach[4] is pursed, which amounts to add terms

$$Ps = \lambda_S^2 \int \left\{ \frac{d^2 f_s\left(x\right)}{dx^2} \right\}^2 dx \tag{3.4}$$

to the objective function whose minimization determines the estimator on interest. A further simplification assures that the optimization of the resulting roughness penalized objective function does not become intractable. In particular, we write the $f_M(\cdot)$ and $f_F(\cdot)$ as step functions with points of increase at integer years of age:

$$f_M(a_p) = w_p^{'}\beta^M f_F(a_p) = w_p^{'}\beta^F$$

where $w_p = (w_{p,0}, ..., w_{p,97})$ is a vector of binary indicators with $w_{p,a} = 1_{[a \leq a_p < a=1]}$ and $\beta^M = \left(\beta_0^M, ..., \beta_{97}^M\right)$ and $\beta^F = \left(\beta_0^F, ..., \beta_{97}^F\right)$ Ages go from 0 to 97 because this is the age span recorded in our sample. The model for expected household nutrient intake can then be written as:

$$\begin{aligned} E\left[c|x,z\right] &= \left[\beta_0 + \sum_{p=1}^{P} \left\{ S_p w_p^{'}\left(\beta^M\right) + (1 - S_p) w_p^{'}\left(\beta^F\right) \right\} \right] \exp(z'\gamma) \\ &= \left(\beta_0 + n_M^{'}\beta^M + n_F^{'}\beta^F\right) \exp(z'\gamma) \end{aligned} \tag{3.5}$$

where $n_M^{'}$ and $n_F^{'}$ containing counts of household members at each integer year of age.

[4]See Green and Silverman (1994) for an exposition of this approach.

The constant $\beta_0$is included to capture intakes of nutrients that are unrelated to numbers of household members, for example food bought for human consumption but then fed to pets. In addition to this, also food given to visitors or any other person not residing permanently with the households (say a daily labourer) should be captured by the constant. This issue is potentially particularly relevant in our sample of poor rural areas in Mexico.

Finally, estimators for parameters γ, β^M and β^Fcan be defined as:

$$\underset{\gamma,\beta^M,\beta^F}{\arg\min}\left[\sum_{h=1}^{H}\left\{c_h-\left(\beta_0+n_{hM}^{'}\beta^M+n_{hF}^{'}\beta^F\right)\exp(z_h^{'}\gamma)\right\}^2\right] \tag{3.6}$$

where h identified households in our sample.

In order to add the roughness penalty to the objective function we consider the discrete analogue of the roughness penalty (3.4), which is the sum of squared second differences of the elements of β^M and β^F, or $\lambda^2\left(A\beta^M\right)^{'}\left(A\beta^M\right)$ and $\lambda^2\left(A\beta^F\right)^{'}\left(A\beta^F\right)$ where λ controls the amount of smoothing , with $\lambda=0$ meaning no smoothing and the matrix A is matrix which produces second differences of the vector to which it is applied (notice that we use the same λ for females and males.

It can be shown that for this particular problem, the addition of the roughness penalties to the objective function is equivalent to append a set of additional observations to the original survey data [refer to Chesher (1997) for more details].

In principle, one can relax the strong identification assumption above and write a model in which some household characteristics enter in a non separable way in the nutrient consumption functions. This also constitutes a simple way of assessing the original identification assumption. In particular, we allow a household composition variable, the number of household members of age less than 15, to enter the $f\left(x_p,z\right)$ in a non separable way. We do not impose any particular structure of the relationship between individual intake and household composition, but we allow vectors β^M and β^F to differ at each age according to a categorical variable that divides the households in 4 groups: "no children age less than 15", "1 child"; "2 or 3 children", "4 or more".

The model for nutrient consumption becomes:

$$\begin{aligned} E\left[c|x,z\right] &= \left[\beta_0+\sum_{p=1}^{P}\left\{S_p w_p^{'}\left(\beta_c^M\right)+(1-S_p)w_p^{'}\left(\beta_c^F\right)\right\}\right]\exp(z'\gamma) \\ &= \left(\beta_0+n_M^{'}\beta_c^M+n_F^{'}\beta_c^F\right)\exp(z'\gamma) \end{aligned} \tag{3.7}$$

where c takes values from 1 to 4. Under this specification, the vectors β_c^M and β_c^F include 4 parameters at each age.

Estimators of parameters in (3.7) can be obtained following the same steps described above. It has to be stressed that the possibility of estimating such a model depends on the availability of a large enough sample[5].

[5]Allowing for our household composition categorical variable raised the number of parameters to estimated from 98 (ages from 0 to 97) * 2 (male and female vectors) = 196 to 98 * 4 (categories of household members age

As our objective here is to estimate the program impact on the nutrient intake at each age and sex, we extend the nutrient consumption model in such a way to be able to estimate the nutrient intake parameters and the treatment effects. Let $T = 1$ if household are in a treatment locality (in which the program is operating) and 0 if in a control locality, then (3.7) can be extended as follows:

$$E\left[c|x,z\right] = \left(\beta_0 + \delta_0 T + \delta_c^{T,M} n_M^{'} T + \delta_c^{T,F} n_F^{'} T + n_M^{'} \beta_c^M + n_F^{'} \beta_c^F\right) \exp(z'\gamma) \tag{3.8}$$

Under this specification vectors $\beta^M$and β^Fwould estimate the average nutrient consumptions of individual types living in control localities, $\delta^M$and $\delta^F$the impact of the program on these consumption, and the sums $\beta^M + \delta^M$and $\beta^F + \delta^F$ would give the average nutrient consumption of individuals in treatment localities.

§3.4. Nutrient intake-age/sex profiles

A convenient way of summarizing the results of the estimation is to plot nutrient intakes against age for each sex, this gives a nutrient-age profile. Profiles with age on the x-axis and daily caloric intake on the y-axis are reported in figure 3.1 for females and males. These are estimated with a model that includes all the households with available data in our sample, this means that we pool waves Oct 98, May 99 and Nov 99. As household characteristics we included household poverty score and adult-equivalent (male, age 25) household size in addition to a set of variables included as dummies for each category: spouse education level, number of children age 0-15, head speaking indigenous language, living in a treatment locality, being eligible for the program (poor), change in poverty status wave (*densificacion*), wave and state.

It not clear *a priori* how to assess whether the estimated profile gives a meaningful representation of the underlying relationship between calories and age. One obvious question is whether the estimated caloric intakes are in line with biological requirements and with the main patterns to be expected from a mere nutritional point of view. Naska, Vasdekis and Trichopoulou (2001) have compared age-gender specific food availability based on data collected at the household level with individual nutrition surveys for the same population finding that the individualization procedure seems to work quite well in practice[6].

Patterns that are well documented in the nutritional literature are the peak of energy intake at puberty, the rise of the intake into middle age and a fall in old age. Our profiles seem to be generally quite in line with these patterns.

Another possible way to validating the estimated profiles is to compare them with existing individual consumption data from similar settings. In table 3.1 we compare our estimated caloric intake with individual data from sample of individuals in Mexico and Colombia. In these samples not all the age ranges are surveyed and/or reported, however it seems that our estimated profiles fare reasonably well compared to these benchmarks.

less than 15) * 2 = 784.

[6]The study uses household budget surveys and individual nutrition surveys of four European countries: Belgium, Greece, Norway and UK.

§3.5. Impact of PROGRESA on caloric consumption

At the household level, previous studies (see Hoddinott and Skoufias, 2004) show that households in PROGRESA localities consume around 200 kcal per capita (or 7%) more than poor people in control localities.

Even if this is a substantial positive impact and it gives a first indication that the program is successful in improving nutritional status of eligible families, we stress that this first result does not tell anything about program impact at individual level. In particular, program might have an heterogenous impact across different member/groups within the household; for example, by its very design PROGRESA has a special focus on children and mothers. We cannot estimate directly outcomes at individual level due to unavailability of data at this level of aggregation.

Estimation is carried both on the treated group (*pobre*=1 and in PROGRESA communities) and on the control group (*pobre*=1 and not in PROGRESA communities). Figure 3.1 and 3.2 reports the predicted per capita caloric consumption-age relationship both for the treated and control group respectively for females and males. One main pattern that emerges is that for younger ages the estimated calorie intake for treated group is higher than control group's. Another interesting finding is that while the estimated values for caloric intake for females (see figure 3.1) are not far from meaningful values (for example, daily recommended intake for girls age 0-5 is around 890 kcal and 2000 kcal for adults), the values for males (see figure 3.2) are not in line with benchmark intakes (recommended intake for a male age 15-19 is 3000 kcal). This issue is explored further below.

The program impact (in our case treatment effect on the treated, TT) is the difference between the estimated caloric intake for treatment and control group. We compute this difference both for females (see figure 3.3) and males (see figure 3.4) together with 2 standard error pointwise confidence bands.

Our main findings are that program seems to have a positive and sizeable impact only for younger ages, both for males and females. Particularly for females positive impact lasts till age 30. These results are remarkably in line with the very program design: PROGRESA wants to have an impact on the nutritional status of poor families, particularly of children and their mother.

As robustness check we also re-estimate the calorie-age profile not allowing any longer for a different function between males and females; the estimated calorie intake profile is in figure 3.5 and the impact in figure 3.6. These pooled results are consistent with findings above: impact is positive and significant only for younger ages.

§3.6. Allowing for household composition

A strong assumption of the simple food consumption model used to derive individual consumption from aggregate data is separability between individual and household characteristics. For instance, this assumption implies that the consumption levels of one child do not depend on age and/or presence of other children in the household, which is undoubtedly a quite strong assumption.

Here, we relax this assumption in a simple way: we allow nutrient consumption at each age to vary with number of children in the household. In particular, we consider 4 categories: "living in a household with no children 0-15", "1 children", "2 or 3 children", "4 or more" and extend the model as explained above in section 3.3. We do not separate between males and females here to have enough observations in each cell (age-age category group).

As said this is a very simple way of allowing for household composition, however some insights can be gained from figure 3.7 and 3.8 where we report the results of this exercise. Figure 3.7 reports the impact for the four age categories at all ages: it seems that the different age categories can make a difference in terms of impact. As impacts at older ages are only imprecisely estimated, we can focus on ages under 40, as we do in figure 3.8, to have a clearer picture.

One interesting finding is that there is a positive impact of the program for individuals below age 15 only if they live in household with "2 or 3 children" or with "4 or more", while there is no impact for one child households. This seem to suggest that the impact of the program is manifest only when its intensity is above a certain level: program's operation imply that households with more children would receive a bigger total transfer. Obviously, household in these different categories have different characteristics and some of these might be related to the way they respond to the program: for example, households with only 1 child are expected to consist of younger parents which may still have a very high daily caloric requirement (which will be financed with the program's transfer, possibly leaving little to spend on the only child), compared to households with older parents whose caloric requirement are expected to be lower.

§3.7. Measurement error in sample of males?

One issue left unaddressed above is the fact that for the male calorie-age estimated profile we find values that are not close to reasonable ones, for example to recommended daily intakes. One possibility is that some type of measurement issue is biasing estimates for our sample of males. Due to randomization we can safely assume that this possible bias is affecting treated and control group in the same way , with this meaning that estimates of program impact are still unbiased. However, it is interesting to explore further the issue. One possible explanation is that there is asymmetric information regarding food consumptions; respondents (to food consumption questions) might not have a good information of household activities made by specific age-sex groups within the family. Our sample is very "asymmetric" in terms of who responds since respondent is a woman in 85% of questionnaires. Accordingly, it might be that respondent women do not hold complete information on intakes of adult male members within the household and therefore they are understating their food consumption. A previous study (Boozer and Goldstein, 2003) explores a similar issue with a dataset from Ghana where husbands and wives were interviewed separately and each respondent was asked to report its own expenditure, the expenditure of their spouse (cross-reporting), and the expenditure of any other person in the household that was used for household consumption. A major finding is that some components of consumption are "private" in nature, and thus essentially unobserved in the cross reports.

We try to test whether this measurement issue is in operation in our sample with a simple strategy: estimation is repeated in the samples "only female respondents" (see figure 3.9) and "only male respondents" (see figure 3.10).

One interesting finding is that when respondent is male the shape is different (increasing for adults) and caloric intake is higher with respect to samples with different respondents (somewhat closer to reasonable values) for adults. In addition to this, shapes for younger ages do not seem substantially different.

In conclusion, we found some evidence of under-reporting of food intake (caloric intake) of other-sex adult members in the household. In particular, since most of the respondents are females in our sample, women seem to have distorted information on food intake of male adults, with this explaining the unreasonable low values we find for the estimated calorie-age profile for men.

We are aware that this explanation is tentative and preliminary, however it seems to make a case for further research on this issue.

§3.8. Concluding remarks

This paper has shown how to estimate the program impact on individual outcomes when only aggregate household data is available. This can be useful because, while most development policies target individuals, the estimation of the program impact might be *a priori* impossible due to lack of individual level data on key variables. In particular, we estimate the impact of PROGRESA on individual caloric intake. This impact complements the results of a positive and significant impact of PROGRESA on average household caloric availability which is well established result in the literature. Our results show that the program is having a stronger impact on caloric availability for younger ages (both male and females) and for females up to age 30 (mothers). This result is remarkably in line with the very program design, which focuses particularly on welfare of children and their mothers.

A strong assumption of the simple food consumption model used to derive individual consumption from aggregate data is separability between individual and household characteristics. For instance, this assumption implies that the consumption levels of one child do not depend on age and/or presence of other children in the household, which is undoubtedly a quite strong assumption. We relax this assumption in a very simple way including dummies for some age groups in our model. When we allow for household composition, we show that the different age categories can make a difference in terms of impact confirming the problems in maintaining the assumption above.

One of the findings motivated the analysis of another issue explored here: we find that while the estimated calorie-age profile for females displays reasonable values in terms of per capita daily calorie, the profile for the sample of males shows unreasonably low values (especially for adults; see results in section 3.5). We argue that this can be the result of asymmetric information within the family regarding food consumptions. In particular, when the respondent to the survey questions is female (in our sample 85% of respondents) she might not hold a complete information of intakes of adult male members of the household and therefore she might be understating their food consumption. Some preliminary evidence confirms this pattern.

Table 3.1: Estimated intakes vs. other sources

Age groups	Progresa		INN99		PAL		ENSIN 2005	
	Females	Males	Females	Males	Females	Males	Females	Males
0-4	706	823	820		736	791	896	941
5-9	1218	1203	1214		…	…	1223	1310
10-14	1459	1538	1492	…	…	…	1478	1586
15-19	1479	1618		…	1536	…	1492	1841
20-24	1543	1618		…	1561	…	1444	1874
25-29	1577	1780		…	1503	…	1458	2123
30-34	1757	1846		…	1521	…	1291	2124
35-39	1696	1804		…	1489	…	1271	2111
40-44	1551	1858		…	1467	…	1578	1767
45-49	1641	1937		…	1332	…	1290	2058
50-54	1639	1893	…	…	1157	…	1214	1698
55-59	1540	1784	…	…	1363	…	925	1693
60-64	1446	1804	…	…	…	…	1113	2161
65-69	1299	1712	…	…	…	…	…	…
70-74	1147	1694	…	…	…	…	…	…
75 and older	903	1298	…	…	…	…	…	…
All	1325	1583	…	…	…	…	1299	1537

Notes: columns labeled "Progresa" refer to our estimated intakes; "ENN99" refers to the Mexican Encuesta Nacional de Nutricion 1999 (in particular, we are reporting the values for rural areas which are the most comparable to Progresa's villages); "PAL" refers to the baseline wave (2003) of the evaluation sample of Programa Apoyo Alimentario. Localities included in PAL are expected to be more marginalized than those in Progresa.; "ENSIN 2005" refers to the Colombian Encuesta Nacional de la Situacion Nutricional. In particular, we report the value for areas with dispersed population (rural) which are the most comparable to Progresa's villages).

Figure 3.1: Nutrient-Age Profile Females

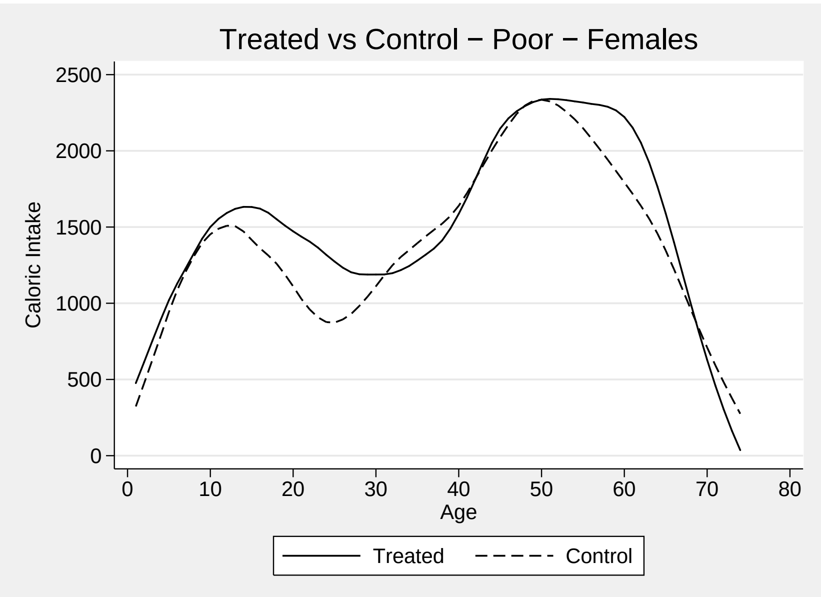


Figure 3.2: Nutrient-Age Profile Males

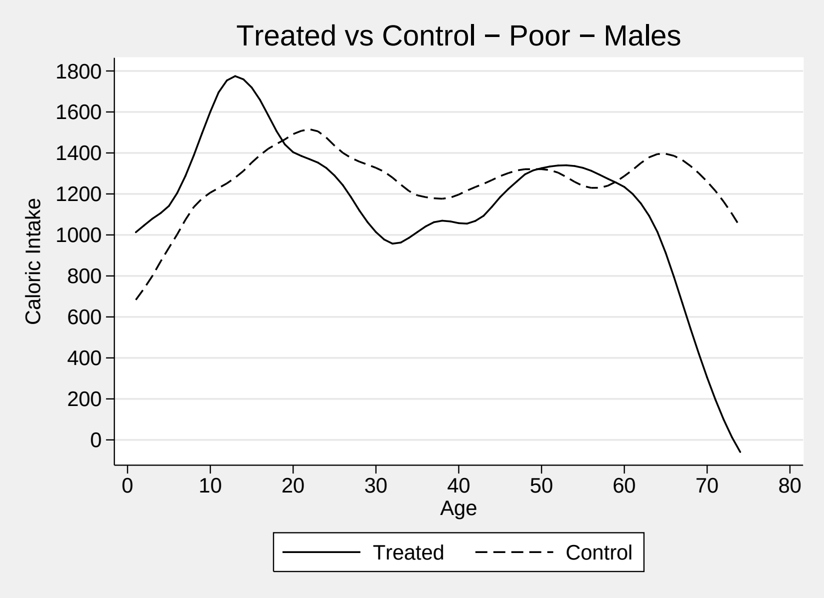


Figure 3.3: Impact Females

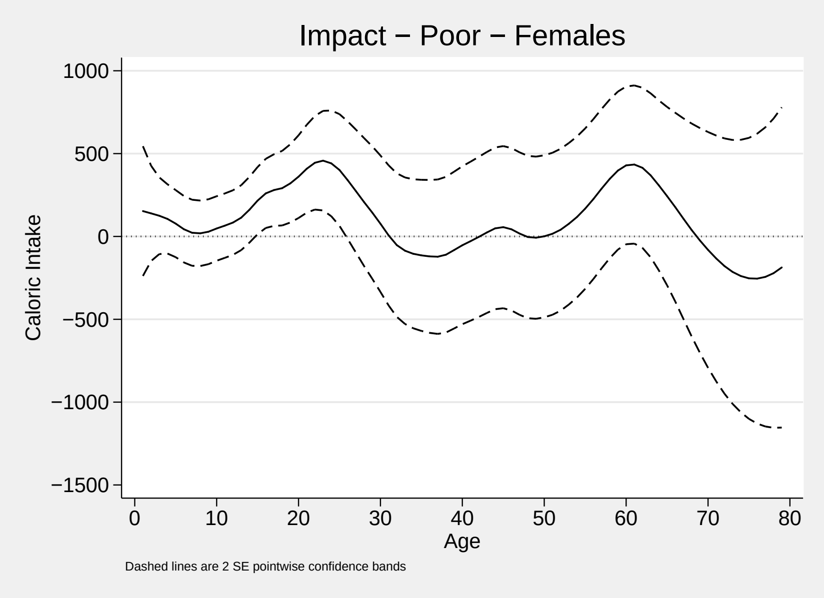


Figure 3.4: Impact Males

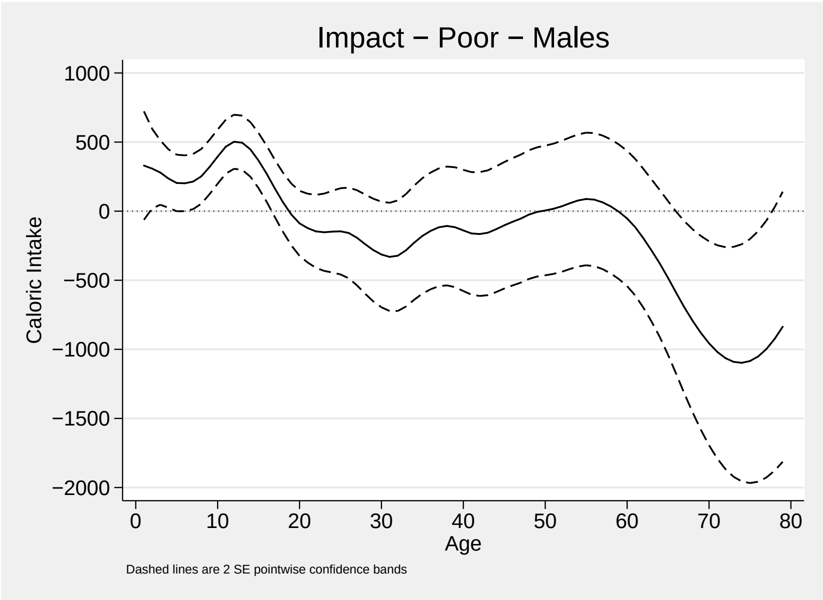


Figure 3.5: Nutrient-Age Profile Pooled

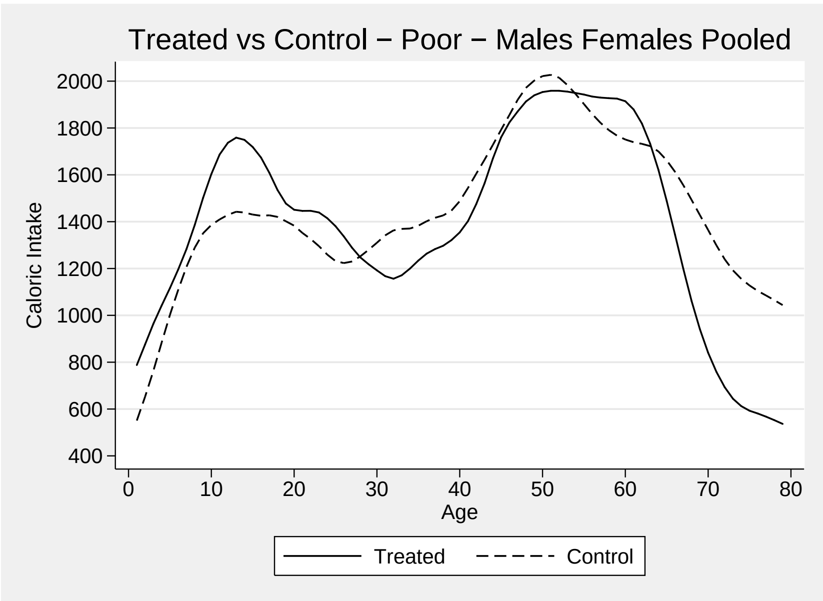


Figure 3.6: Impact Pooled

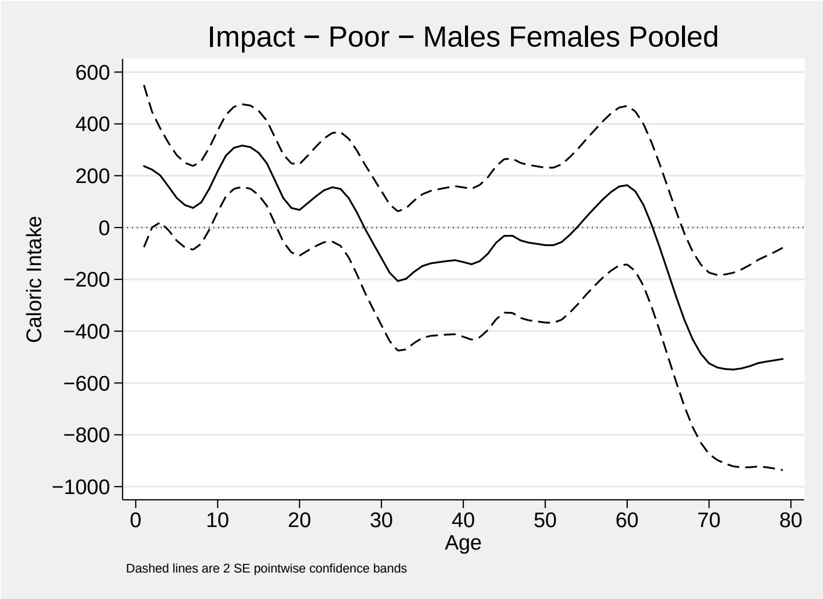


Figure 3.7: Allowing for Household Composition - All ages

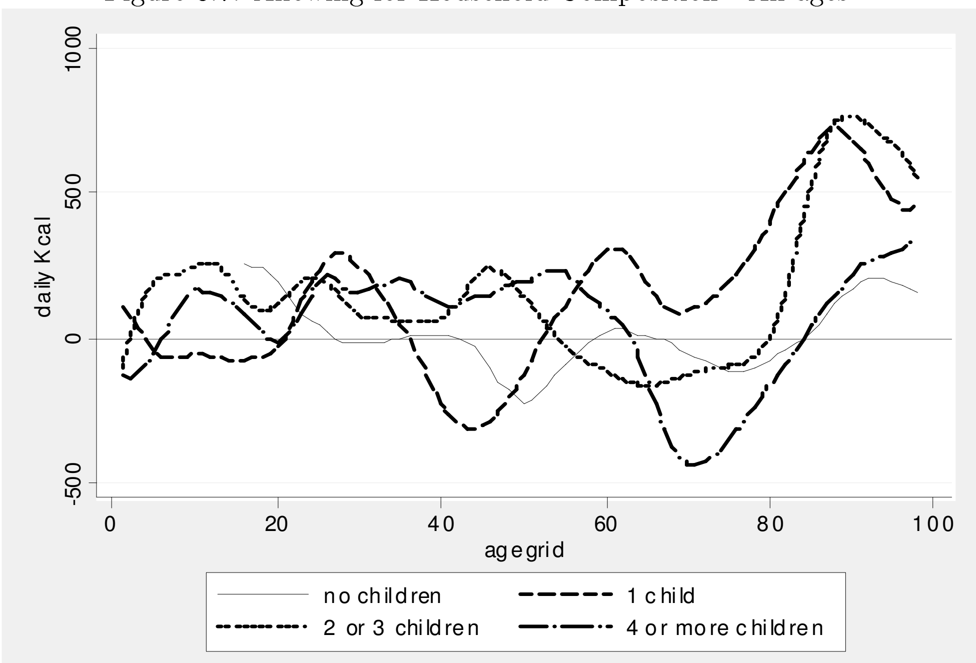


Figure 3.8: Allowing for Household Composition - under 40

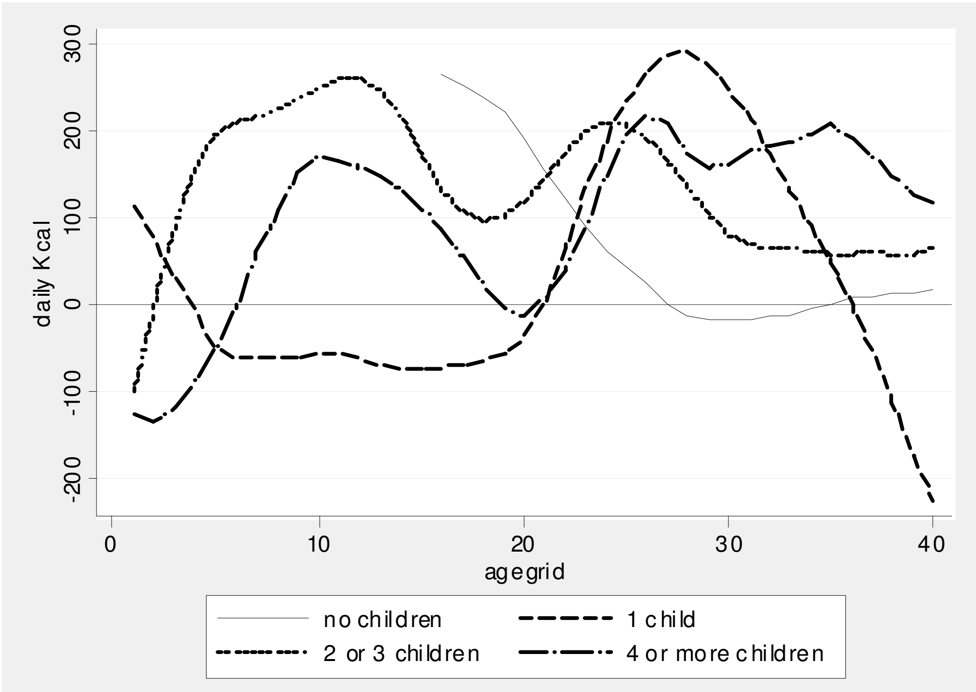


Figure 3.9: Female Profiles and Respondents

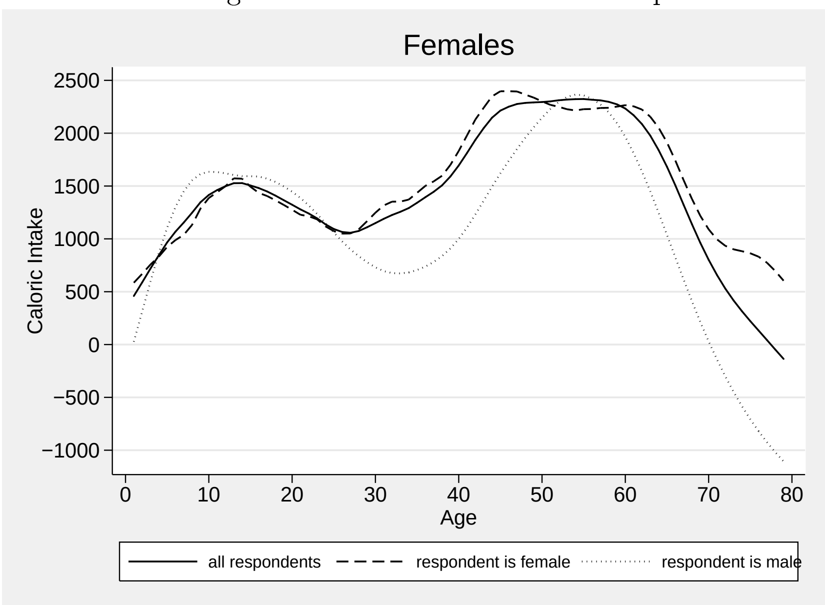
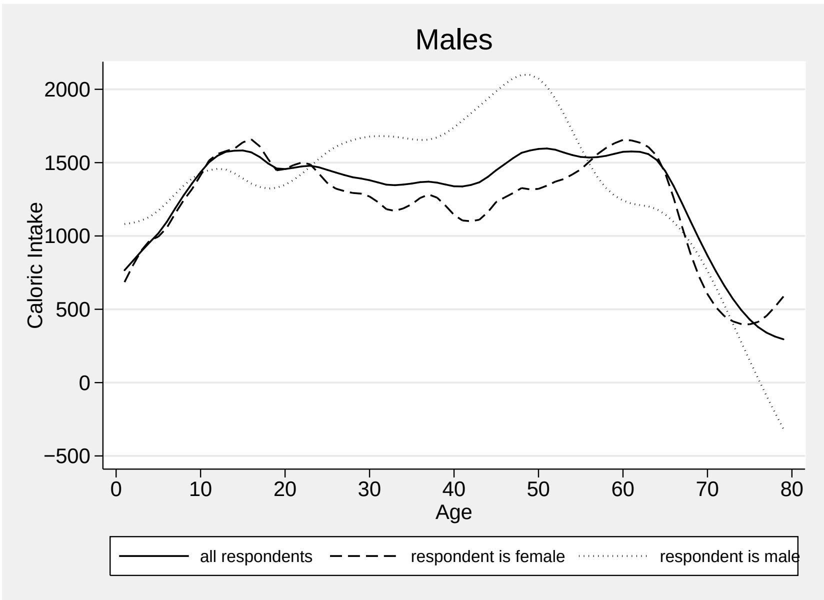


Figure 3.10: Male Profiles and Respondents



CHAPTER 4

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

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.

§4.1. 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 4.3), 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.

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

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

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 4.6.3. 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 4.2, we describe the operation of the programme. In section 4.3, we provide a theoretical framework that helps us in choosing an empirical strategy and in interpreting the results we obtain. We stress in particular that the parameter we estimate identifies the overall effect of the program, including both direct and indirect effects. In section 4.4, we briefly describe the two data sets we use to estimate the impact of the programme. In Section 4.5, we present some evidence in support of our identification strategy. Section 4.6 presents the main empirical results of the paper, section 4.7 provides support to the credibility of our identification strategy. Section 4.8 concludes.

§4.2. 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

[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.

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.

§4.3. 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.

4.3.1 — 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:

$$MaxU\left(X, H,\ L\right) \tag{4.1}$$

subject to the following restrictions:

$$H = H\left(A, F, L,\ z, \varepsilon\right) \tag{4.2}$$

$$X = Y - pF - qA + w(L - DA) \tag{4.3}$$

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 (4.3) is the budget constraint that reflects the importance of cost variables (the distance to a HC and fee). Equation (4.2) 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 (4.2) 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 (4.2) can be approximated by a linear function:

$$H_i = \vartheta + \alpha_i A_i + \beta F_i + \gamma L_i + \theta z_i + \varepsilon_i \tag{4.4}$$

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 (4.4). 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 (4.4).

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:

$$F_i = f\left(A_i, p, w, q, D, Y\right) \tag{4.5}$$

$$L_i = l\left(A_i, p, w, q, D, Y\right) \tag{4.6}$$

and let define $f_A = \frac{\partial f}{\partial A}$ and $l_A = \frac{\partial l}{\partial A}$. 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 Ai in the following regression:

$$H_i = \widetilde{\vartheta} + \quad \widetilde{\alpha} A_i + \widetilde{\theta} z_i + \widetilde{\varepsilon_{i.}} \tag{4.7}$$

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

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.

4.3.2 — 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 4.6.3.

§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.

4.4.1 — 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]

[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).

[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

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 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.

4.4.2 — 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).

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

4.4.3 — 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 75th% 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.

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.

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 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.

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

[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%..

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

In Table 4.4, we report the mean and three percentiles (25th, 50th and 75th) for our three instruments. In the left-hand panel we consider the statistics computed over the whole sample, while on the right hand panel, we restrict our attention to the sample of participants to HC. As expected, participants tend to live close to HC centres

§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 4.3, 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 (4.7). 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 4.7, we provide more evidence to support our empirical strategy and our identifying assumptions.

4.5.1 — First Stage Regressions.

As discussed in section 4.3.2, 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 4.5. 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 A4 in the Appendix for details.

4.5.2 — 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 (4.7) 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 A6 in the appendix), we do not find any statistically significant difference.

[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%).

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 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 4.2.

4.5.3 — 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 4.5.2 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 4.6 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 4.6, 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 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

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

in towns with higher proportion of households with piped water and health insurance.

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).

4.5.4 — 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 4.1, 4.2, and 4.3 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 4.1). Figure 4.2 and Figure 4.3 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.

§4.6. 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.

4.6.1 — Average Treatment Effects

In columns 1 to 5 of Table 4.8 (both top and bottom panel), we present our IV estimates of equation (4.7), 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 (4.1) 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 A7, A9 and A11 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 4.5.3, 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 4.7.

The top panel of Table 4.8 (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.

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 4.8 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 4.5.4 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 4.8 (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 (although note that, given the size of the standard errors, the confidence intervals overlap).[16] Interestingly, we obtain very sizeable effects of the programme

[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 4.8 (see Table A4 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, 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 4.4, (2) households have aged since the first wave and they have fewer children between 0 and 6 years old.

[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

in both datasets.

The bottom panel of Table 4.8 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 4.7, 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 4.8. 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 4.5.1).

4.6.2 — 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 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 4.9 (full results are in table A12 to A14 in the Appendix) shows the estimates for selected quantiles.

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.

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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

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.

4.6.3 — 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 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 4.8, 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

[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.

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).

§4.7. 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 (4.1). 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).

4.7.1 — 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 4.8, 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 4.10 replicates Table 4.8 but with birth weight as dependent variable (full results are found in table A18, A20 and A22 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 4.8. Tables 4.8 and 4.10 taken together seem to indicate little scope for bias.

Column 5 of Table 4.10 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 4.8 constitute a lower bound. This corroborates our results in section 4.5.3 that poorer households were associated with higher capacity levels.

4.7.2 — 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 (4.7). In this regard, we follow the approach of Conley, Hansen and Rossi (2008) which consists on estimating α in the following regression:

$$H_i = \alpha A_i + \theta Z_i + gI_i + \varepsilon_i, \tag{4.8}$$

where Ii 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 4.5.3, 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 4.5.3, 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 (4.8).

Figure 4.4 and 4.5 shows the confidence interval for α 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.

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

[21] This is the reason why our results in Table 4.8 differ slightly from those shown in the Figures 4.4 to 4.9 when g=0.

[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.

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.

Figures 4.6 and 4.7 (4.8 and 4.9) scrutinize the robustness of our conclusions for the capacity instrument in the FeA (ENDS) dataset. We concentrate on negative values of k because subsection 4.5.3 concluded that capacity was positively correlated with observed indicators of poverty (the results in Table 4.10 -birth weight regression- also seem to support this). Hence, we will expect children living in localities with higher capacity to be shorter. Figures 4.6 to 4.9 show that, if k is negative, our results in Table 4.8 underestimate the effect of the HC program as the point estimate of α increases as k becomes more negative.

§4.8. 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

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

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.

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**
25^{th}Perc	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%
75^{th}Perc	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 4.5: 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 4.6: Compatibility regression

	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]

Table 4.7: Compatibility regression: continued

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 4.8: Effect of HC Participation on Child's Height (z-score)

	1	2	3	4	5	6	7
						Ordinary Least Squares	
			Top Panel: IV - non linear+				
	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 4.9: 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 4.10: Effect of HC Participation on Child's and Mother's Birthweight Using Predicted Instrument

	1	2	3	4	5	6	7
			FeA				ENDS
	OLS		IV - non linear+			OLS	IV - non linear+
		All	Capacity	Fee	Distance		
Child's Birthweight							
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]
Attendance to HC	-0.0136	0.384	0.0415	-0.59	0.539	-0.0101	-0.0553
	[0.0627]	[0.359]	[0.808]	[1.643]	[0.384]	[0.0287]	[0.169]
			IV - linear+				IV - linear+
Exposure to HC		0.178	-0.196	-0.0156	0.439		-1.558*
		[0.495]	[0.644]	[0.747]	[0.590]		[0.872]
Attendance to HC		0.175	0.235	0.345	0.223		-0.875*
		[0.230]	[0.301]	[0.299]	[0.239]		[0.515]
Mother's Birthweight	OLS		IV - non linear+			OLS	IV - non linear+
Exposure to HC	-0.0133	-0.0205	-0.0129	-0.0463	-0.0354	-0.00433	0.0052
	[0.00805]	[0.0230]	[0.0320]	[0.0543]	[0.0300]	[0.00368]	[0.0216]
Attendance to HC	-0.00573	-0.00366	-0.000368	-0.0122	-0.00541	9.94E-05	0.00253
	[0.00478]	[0.0151]	[0.0197]	[0.0320]	[0.0193]	[0.00154]	[0.00989]
			IV - linear+				IV - linear+
Exposure to HC		-0.0292	-0.0265	-0.0633	-0.0253		-0.0536
		[0.0257]	[0.0371]	[0.0781]	[0.0350]		[0.0461]
Attendance to HC		-0.0188	-0.0173	-0.0436	-0.0175		-0.0303
		[0.0174]	[0.0320]	[0.0580]	[0.0199]		[0.0252]

Standard errors in parentheses are clustered at the town level. The regressions include a number of other controls.
Complete results are reported in the Appendix. Sample sizes: (children) FeA 1371, ENDS 2093 (Exposure) 2097 (Attendance);
(mothers) FeA 1848, ENDS 6325 (Exposure) 6334 (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

Figure 4.1: Relation between Capacity and Fees

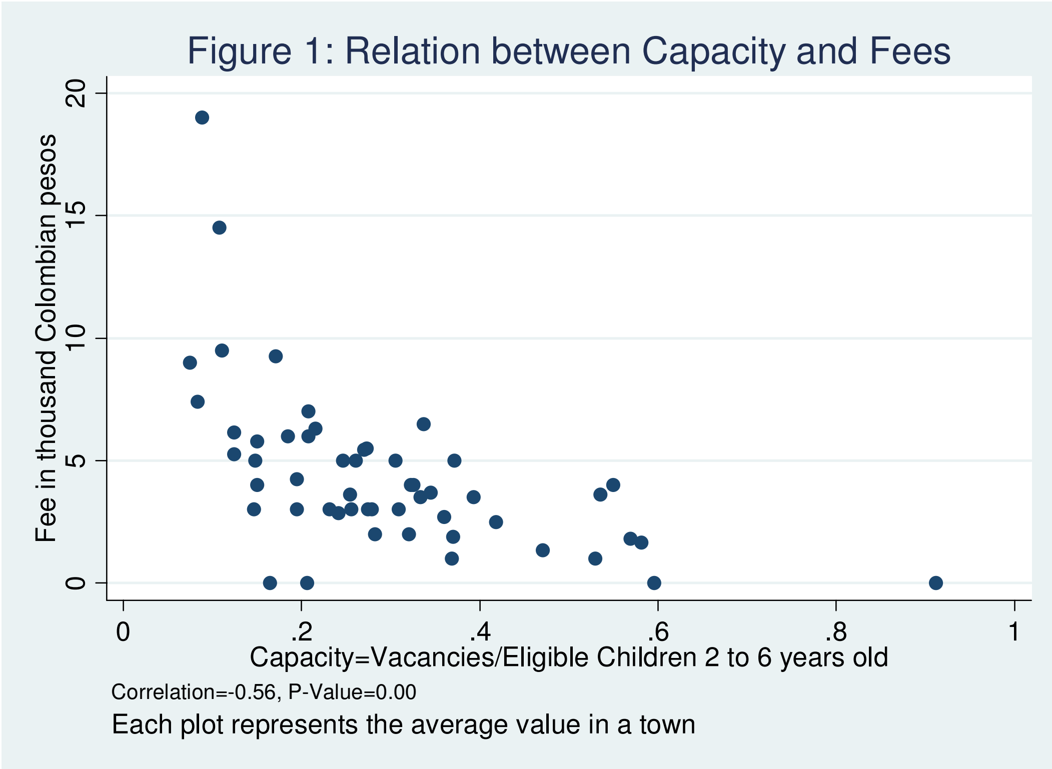


Figure 4.2: Relation between Distance and Fee

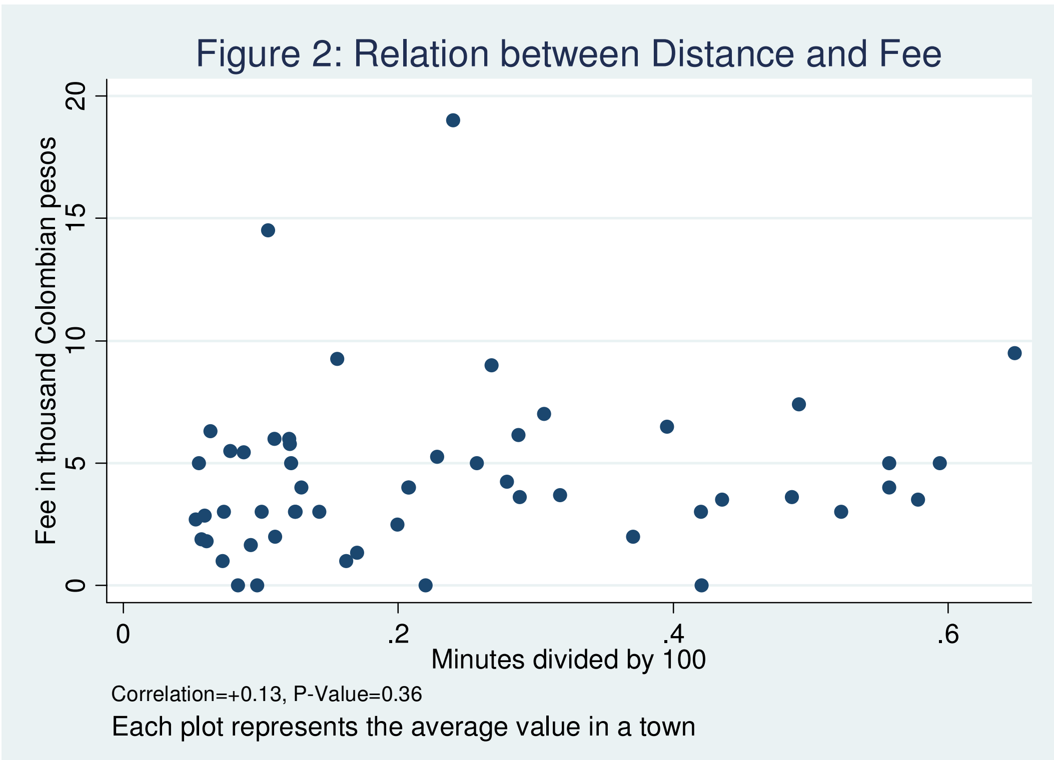


Figure 4.3: Relation between Capacity and Distance

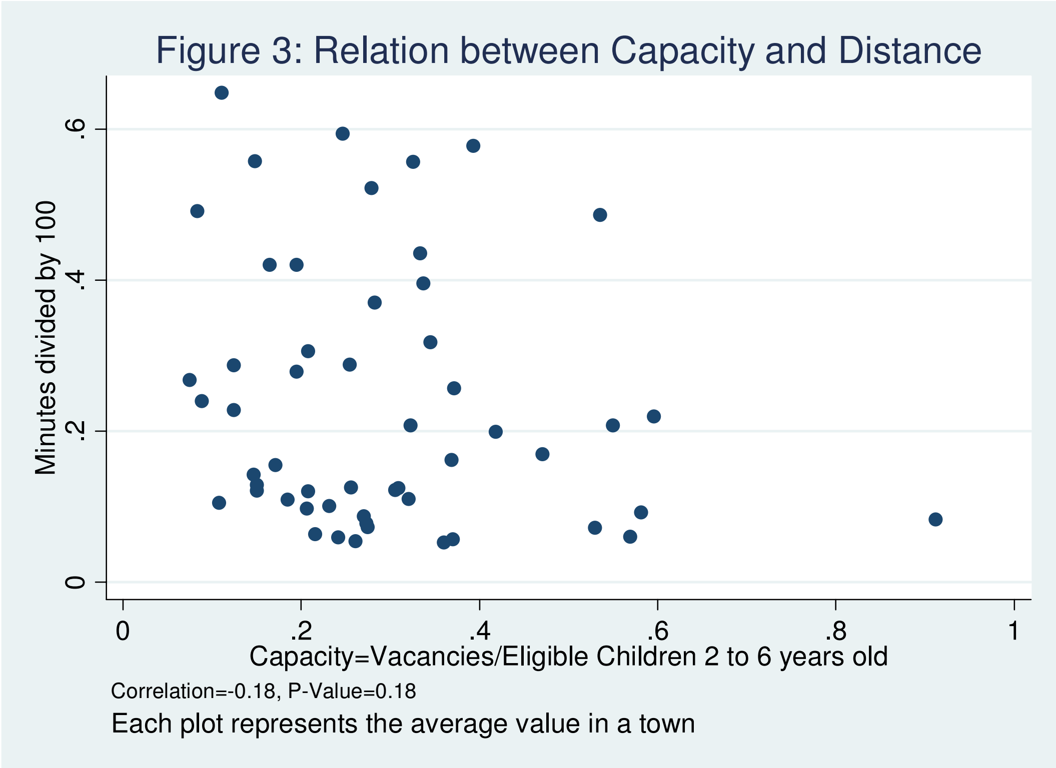


Figure 4.4: 90% confidence intervals for Attendance

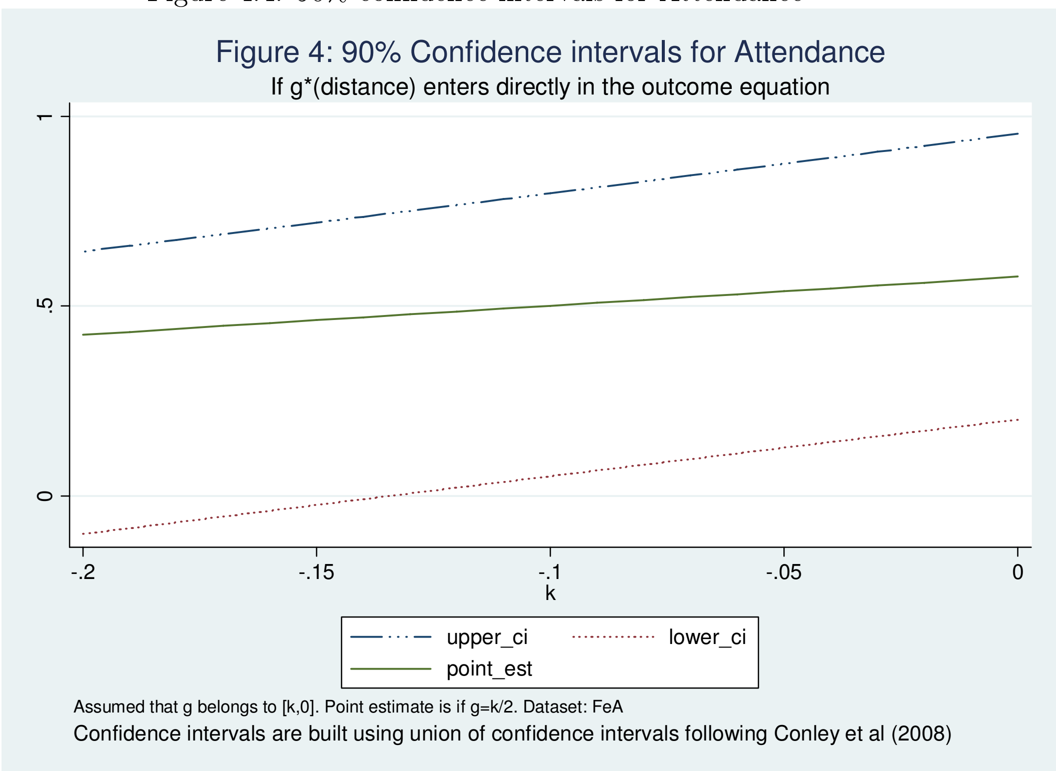


Figure 4.5: 90% confidence intervals for Exposure

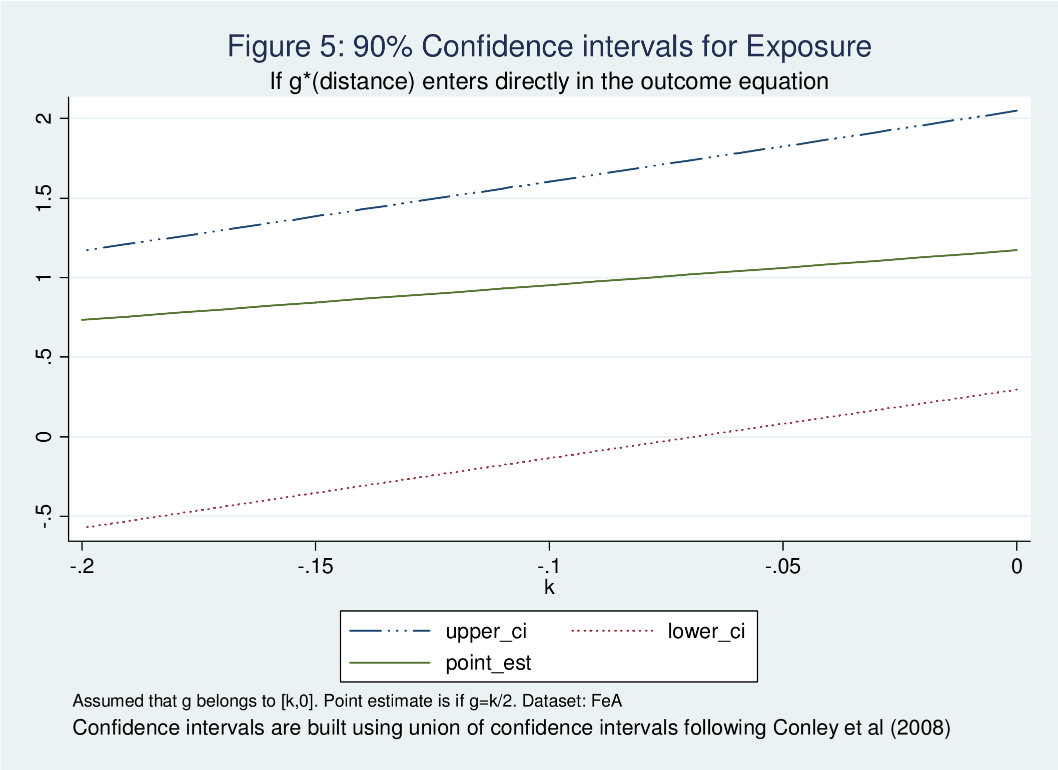


Figure 4.6: 90% confidence intervals for Attendance

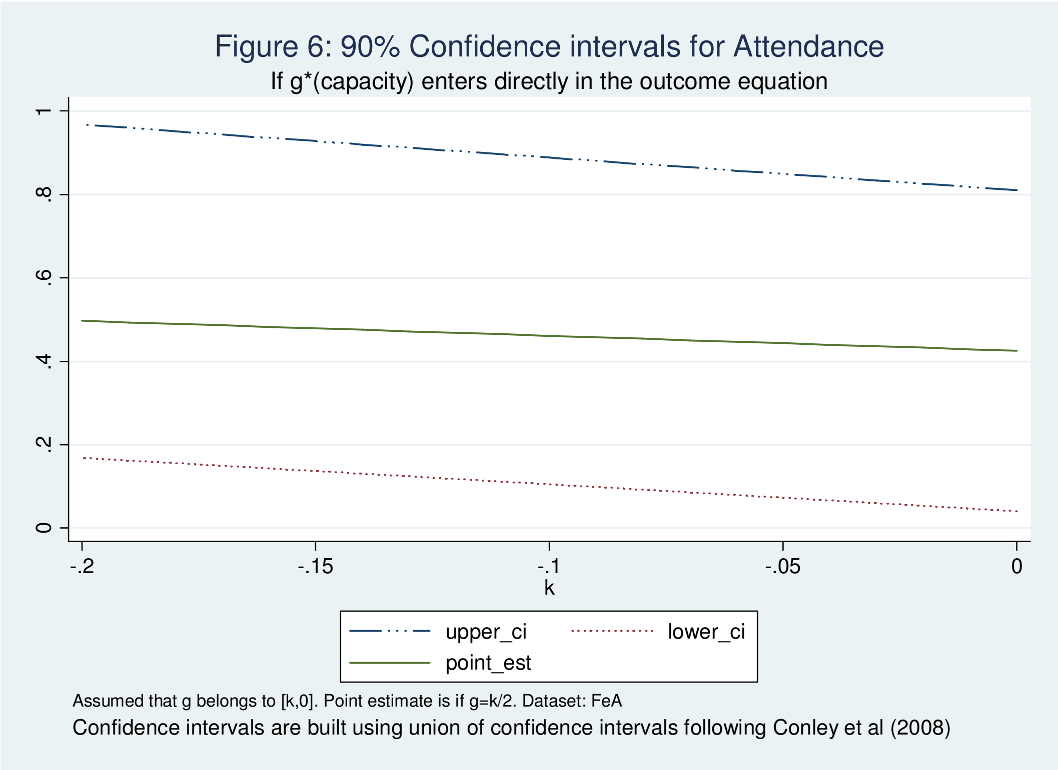


Figure 4.7: 90% confidence intervals for Exposure

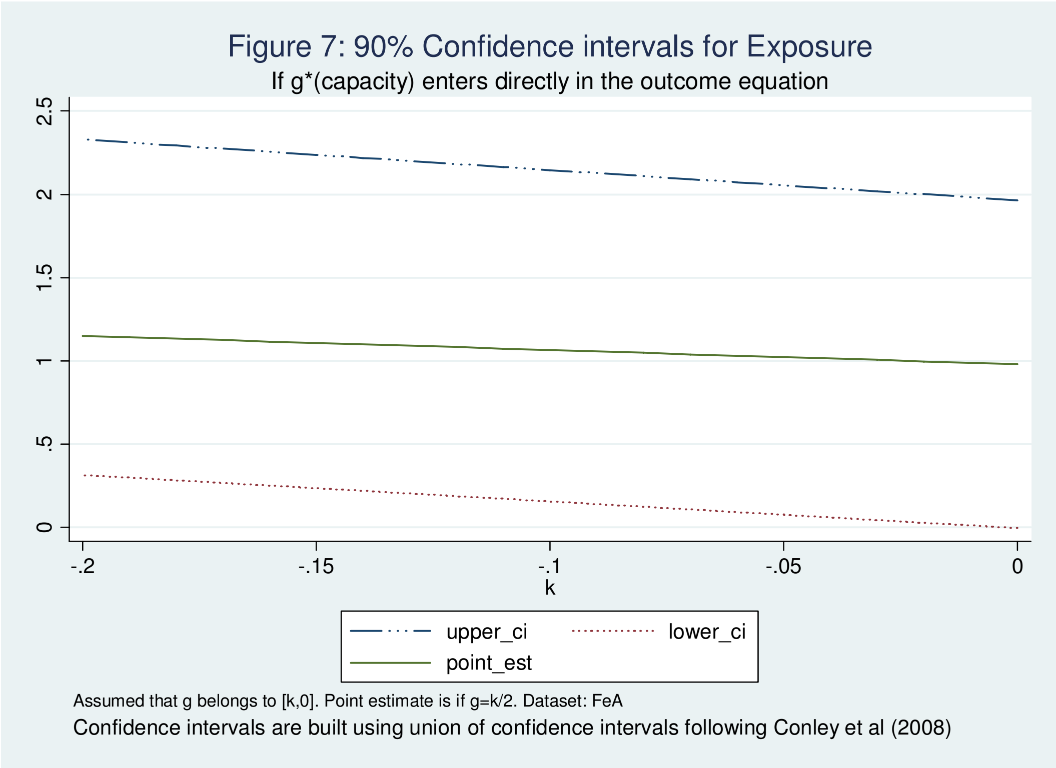


Figure 4.8: 90% confidence intervals for Attendance

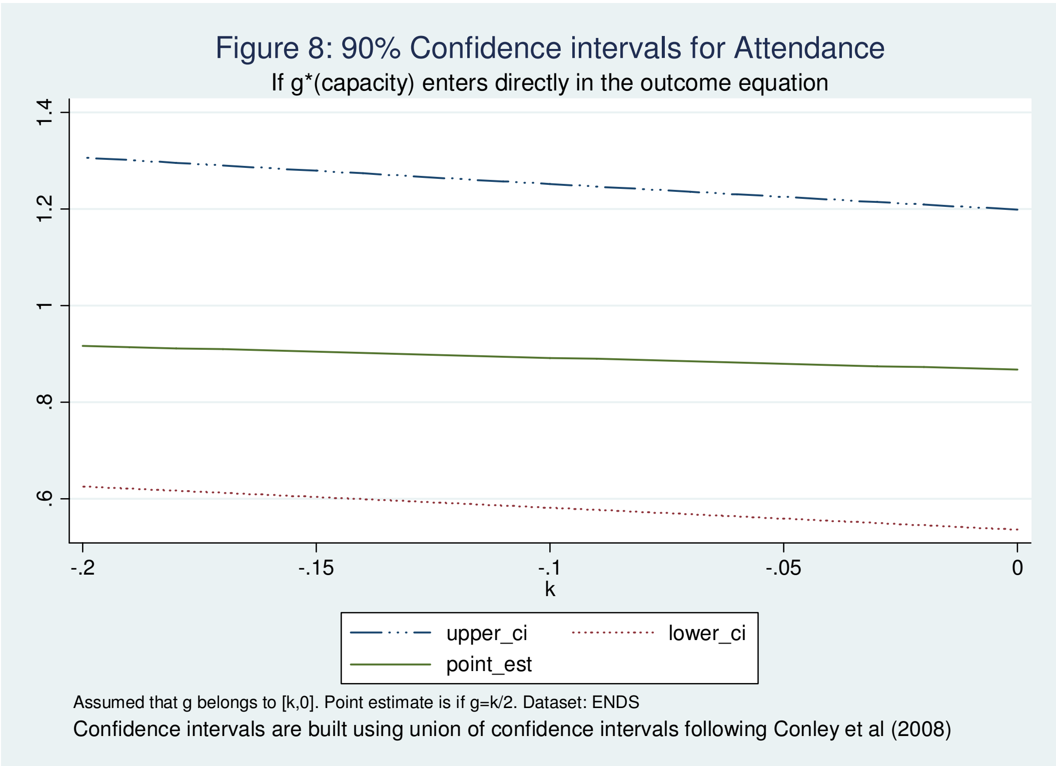
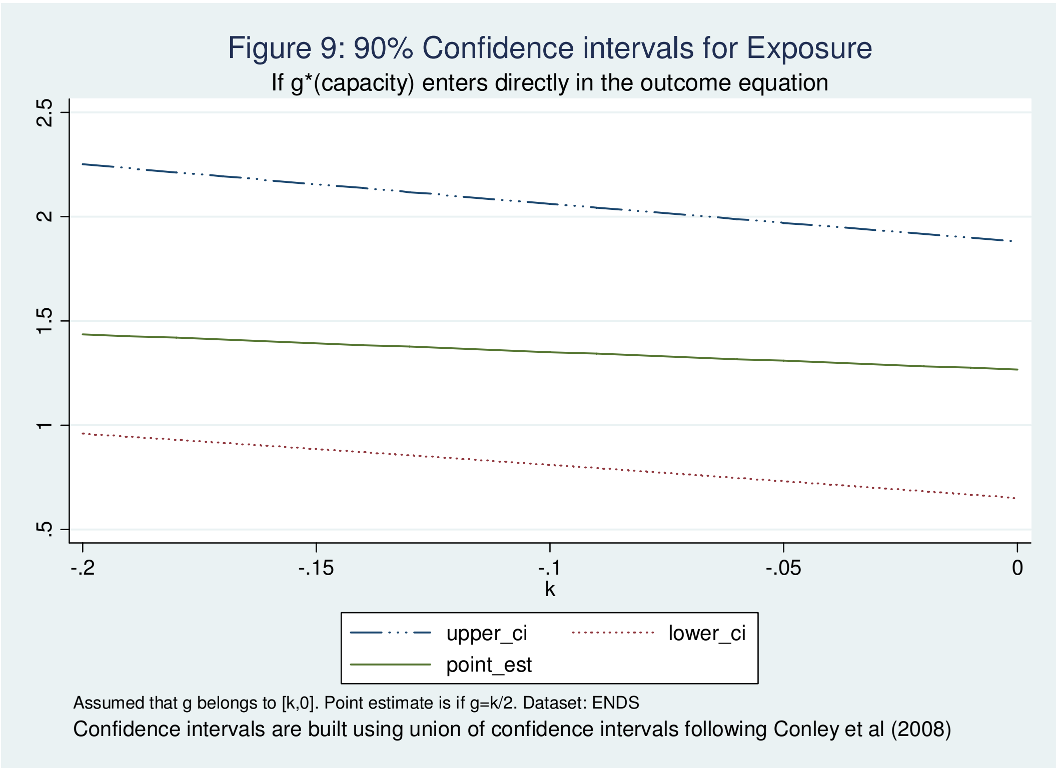


Figure 4.9: 90% confidence intervals for Exposure



CHAPTER 5

Nutrient consumption and household income in rural Mexico

We estimate income elasticities for a variety of macro- and micro-nutrients using a sample of poor rural households in Mexico. The nutrient-income elasticity is estimated using both parametric methods and a semi-parametric approach. A special focus is placed on the non linearity of the relationship between nutrient intake and income and on measurement error and endogeneity issues. One major finding is that income elasticity for calories is close to zero when we control for measurement error issues. For some nutrients, namely fats, vitamin A and C, calcium and heme iron we find a sizeable positive income elasticity robust to the choice of the estimator and percentiles at which is evaluated. Interestingly, these nutrients are also those for which we find the largest deficiency in our sample. In addition to this, we find that for a particularly vulnerable group in our sample (poorer households, at the bottom 25% of per capita expenditure) a deficiency of total energy, protein and zinc is not accompanied with estimated positive income elasticity.

§5.1. Introduction

Deficiencies in nutrients–such as iron, zinc, vitamins A and C, and iodine–are increasingly recognized as an important nutrition problem that affects millions of children and adults in the developing world. The consequences of child malnutrition during the preschool period have been studied extensively (Beaton, et al., 1993, Bhutta, et al., 1999, Bleichrodt and Born 1994, Lozoff and Wachs 2000, Pelletier, Frongillo and Habicht 1993, Pelletier, et al., 1995, Rose, Martorell and Rivera 1992, Wachs 1995). It is estimated that about half of all deaths in developing countries in children less than five years of age are due to the interaction between malnutrition and common infections such as diarrheal diseases, respiratory infections and measles. These infections kill children easily only in the presence of malnutrition, which impairs immune function and lowers resistance to infections. Two nutrient deficiencies, iodine deficiency and anemia, have been shown to be important causes of poor cognitive development, particularly when they affect children under two years of age (e.g. Horton and Ross, 2003).

In view of the negative consequences of a diet poor in nutrients, the potential of social programs to improve the nutrition of vulnerable populations is of particular concern to policy makers. The interventions available for resolving nutrient deficiencies range from multiple nutrient supplementation in young children, which are more useful in the short-run, to food fortification and diet diversification that are more effective in the long-run. This paper contributes in this area by providing estimates of the extent to which nutrient consumption at the household level responds to increases in household income. Cash transfer programs, frequently combined with conditions on some specific behavior such as attending nutrition workshops and regular visits to health centers, provide an increasingly popular approach towards alleviating poverty and malnutrition.[1] The income elasticity for a specific nutrient, the parameter that summarizes the percentage change in the consumption of a specific nutrient corresponding to a one percent change in household income, is critical to understanding one of the key determinants of consumption of nutrients. As household income increases, households may change the composition of their food consumption, and thus their nutrient intake. If increases in income result in changes in the diet of households, towards foods with higher nutrient content (for example, eating more vegetables/fruits and meat), then nutrient deficiencies may fall.

In much of the economic development literature nutrition problems are practically synonymous to the inadequacy of energy as measured by the availability or consumption of calories (Subramanian and Deaton, 1996; Strauss and Thomas, 1995, 1998). Unfortunately, irrespective of the size of the estimated income elasticity for calories, there is nothing that can be inferred about the consumption of essential nutrients. A significantly positive relationship between calories and income does not necessarily imply a higher consumption of nutrients since a higher income may simply result in households buying food items with a higher caloric content, but not higher nutrient content. A similar argument applies when the income elasticity for calories is very small or zero. When household income decreases, household calories may be maintained more or less constant through substitutions within and between food groups while the consumption of essential nutrients may decrease dramatically as households consume less meat, vegetables, eggs and milk.

Thus, even though there is an abundance of estimates on the income elasticity for calories, empirical evidence on the nutrient income elasticity is relatively scarce (Behrman, 1995). In addition, the evidence that does exist suggests substantial differences in nutrient-income elasticities (e.g. Behrman and Deolalikar, 1987; Bouis, 1991). In Indonesia, for example, Pitt and Rosenzweig (1985), using data from farm households, report very low nutrient-income elasticities (below 0.03) for many of the same nutrients considered in the present study (i.e., calories, protein, fat, carbohydrates, calcium, iron, vitamin A, and vitamin C). Another study using data from rural and urban areas in Indonesia reports much higher nutrient income elasticities (for example, from 0.70 to 1.20 for the lower 40 percent of the population by expenditure on Java, see Chernichovsky and Meesook, 1984). Similarly diverse estimates are reported for other countries. Behrman and Deolalikar, (1987), for example, using data from ICRISAT report income elasticity estimates of 0.06 to 0.19 for protein (depending on whether level estimates or differences over time are used), 0.30 to –0.22 for calcium, -0.11 to 0.30 for iron, 0.19 to 2.01 for carotene, -0.08 to 0.18 for thiamine, 0.69 to 0.01 for riboflavin, -0.15 to 0.21 for niacin, and 0.15

[1] The *Oportunidades* program of the Mexican government is one such program aimed at increasing the investments of poor households in human capital.

to 1.25 for ascorbic acid. The Nicaraguan study (Behrman and Wolfe, 1987) reports significant income elasticity estimates in the range of 0.04 to 0.11 for calories, protein, iron, and vitamin A (with statistically significant, but quantitatively small, nonlinearities). The Philippine study (Bouis, 1991) reports an iron-income elasticity of 0.44, a calorie income elasticity of 0.16, and insignificant income elasticities for vitamin A and vitamin C. To date, to our knowledge, there are no estimates of the income elasticity for nutrients in Mexico.

The objective of this paper is to provide some of the first estimates of the income elasticity for key micronutrients in Mexico, such as vitamin A and C, folate, iron, zinc and calcium as well as for energy (kcal), and all the macronutrients (protein, saturated, monounsaturated and polyunsaturated fat, and carbohydrates). Given that the consumption of fiber can inhibit the absorption of some essential nutrients, such as zinc and iron, we also examine the income elasticity for dietary fiber. Reliable elasticity estimates can help policy makers determine *ex-ante* whether a cash transfer program, and/or economic growth per se can be at all effective at increasing nutrient consumption among poor households or whether different interventions altogether may be needed. Considering the frequency at which poor rural areas in Mexico are affected by natural disasters such as floods, it is also useful to know how effective cash transfers could be as an instrument for maintaining (if not improving) the nutritional status of affected households.

The study of the nutrient consumption patterns and the relationship between nutrients and income are particularly important for Mexico. On the one hand, the 1999 National Nutrition Survey of Mexico identifies zinc and iron deficiency as a major nutritional problem in Mexican children (Barquera et al., 2003a). On the other hand, Mexico, like a number of developing countries during the last fifteen years, appears to be experiencing important reductions in the prevalence of infections and undernourishment, accompanied by large increases in the incidence of chronic diseases and overnourishment (Rivera et al. 2002, Bobadilla et al. 1993; Frenk et al. 1991, Popkin, 1994;, Drewnoski and Popkin, 1997; Murphy et al., 1992; and Zeitlin, Ghassemi and Mansour, 1990). In such a context, it is critical to have a better understanding of the effects of increases in household income on the composition of household diet, in general, and the consumption of nutrients in particular.

In line with the recent trend in the literature[2] on the calorie income elasticity, our study places particular emphasis on the heterogeneity and sensitivity of the elasticity estimates. In particular, we are interested in whether, and to what extent, income elasticity varies across relevant groups of the population under study. Our econometric methodology consists of both parametric methods and a semi-parametric approach. Parametric methods will allows us to control properly for biases due to measurement error in consumption and endogeneity issues. In particular, we use a standard linear regression approach that imposes a linear relationship between nutrient consumption and income, which in turn results in a nutrient income elasticity that is constant and independent of the level of income. Then, we adopt two different flexible specifications, still linear in parameters, but that allow elasticity to change with income. Finally, we explore heterogeneity

[2]Recent published studies include Gibson and Rozelle (2002), Abdulai and Aubert (2004), and Skoufias (2003).

of the response of nutrient to income with a quantile regression approach. In addition to this, we try to explore whether the presence of zero consumption for specific nutrients in our sample can be a source of bias for the estimates. On the other hand, the semi-parametric approach will allow the income elasticity for nutrients to vary in the most flexible manner possible with the level of household income but it only allows a restricted set of controls to be included in a tractable way in the estimation. An alternative approach would be to estimate a fully specified food demand system which would allow us to impose all the relevant restrictions imposed by economic theory and to study the degree of substitution between nutrients as prices of foods change. However, given our focus is on the income elasticity we do not pursue this approach.

The rest of the paper is organized as follows. Section 5.2 describes in more detail the data used and discusses some descriptive evidence on the nutrient consumption in the sample. Section 5.3 presents and discusses the results from linear regression approach that imposes constant elasticity. Section 5.4 illustrates the parametric and semi-parametric approaches based on non linear specifications. In section 5.4, we sum up our results and put forward some policy implications.

§5.2. Data and Macro and Micro-Nutrient consumption patterns

The data we use is based on a sample of 7553 households in 240 poor rural localities from eight Mexican states (Campeche, Chiapas, Guerrero, Oaxaca, Quintana Roo, Tabasco, Veracruz and Yucatan), surveyed between October 2003 and April 2004 . This sample has been collected for the purposes of evaluating the *Programa de Apoyo Alimentario* (PAL). [3] This program has as its major objective the improvement of the nutritional status of poor households living in rural localities of Mexico and it is targeted to localities that are not covered by other food programs, or programs with a substantial nutrition component, such as *Oportunidades* and *Abasto Social de Leche*. [4] In order to be incorporated into the program the localities have to meet some requirements such as having a population of less than 2500, having at least one household with a poor nutritional status (according to the criterions established by SEDESOL, *Secretaria de Desarrollo Social*, that is the social development arm of the Mexican government), being accessible (not more than 2.5km from a road), and close enough (not more than metricconverterProductID2.5 km2.5 km) to a DICONSA[5] store.

The support provided is either in-kind transfers (value of food provided is of 150 pesos) or cash transfer of 150 pesos according to what the program administrators think it is the more appropriate in the specific case. While the major component of the program is food support, other complementary operations are being provided, such as health assistance, nutritional education classes and support to build floors and latrines.

[3] Since one of the purposes of the evaluation of PAL is studying is impact on the nutrition of children of age less than 5, it was decided from the beginning that 40% of households interviewed in each locality had to have children less than 5.

[4] For instance, the localities that do not fulfill the requirements in terms of education and health infrastructures in order to be included in Oportunidades can be included in PAL.

[5] DICONSA is the Mexican government' agency that manages the supply of food (through its stores) to rural and marginalized localities.

The analysis in this paper is based on the baseline survey round that took place before the start of the program. The nutrient income elasticity estimates derived here can thus serve as benchmark estimates for the impact of the cash component of the program on nutrient consumption at the household level. The survey collects extensive socioeconomic information, as well as information about food and non food expenditures. Specifically, the consumption module collects information on the quantity consumed (including that out of own production) in the last seven days for sixty one food items.[6]

In much of the development literature estimates of the demand for nutrients are typically derived through an indirect approach. Since consumption of nutrients is determined by what foods and how much of those foods are consumed, good estimates of the demand system parameters for food can be used, by applying nutrient-to-food conversion factors (Pitt, 1983, Strauss, 1984). However, for such an indirect procedure to lead to good estimates of the demand for nutrients, the estimates of the food demand system must be good in a variety of respects. Similarly, deriving direct estimates of nutrient demand, it is important to use food groups that are not aggregated "too much" or else important within food group substitutions may be missed. For example, Behrman and Deolalikar (1987) suggest that the indirect approach of estimating systems of demand for food groups with food groups being fairly broad aggregates of individual foods, may lead to nutrient income elasticities that are considerably biased (Subramanian and Deaton, 1996).

Mindful of these considerations we adopt a flexible approach that simply examines the total consumption of major nutrients in rural poor households in Mexico by aggregating the nutrient contents of the sixty one food items contained in the PAL survey. We use a food composition database compiled by the National Institute of Public Health of Mexico (INSP) that contains information on the nutrient content per metricconverterProductID100 grams100 grams of all the major food items in Mexico to convert the quantity consumed of each of the sixty one food items by each household into its equivalent content of calories, protein, fat, carbohydrates, and micronutrients. The quantity of each nutrient consumed is then aggregated at the household level.

In order to shed some light on the nutrient consumption patterns in our sample, it is useful to conduct a descriptive analysis of nutrient consumption. This offers a *status quo* picture of macro and nutrient consumption. The behavior-related issues, such as the response of nutrient consumption to changes in income are discussed in the next section.

Table 5.1 reports some descriptive statistics (mean, median and interquartile range) for a list of macronutrients and major micronutrients (fiber, protein, fat, cholesterol, saturated, monounsaturated and polyunsaturated fat, carbohydrates, vitamin A and C, folate, iron and heme iron, zinc and calcium). These descriptive statistics and all the estimations below are computed on a sample that excludes households with a value of per capita caloric consumption

[6]We did not use the information collected on purchases of food, as this would provide information on nutrient availability instead of consumption. The PAL questionnaire also contains a module based on the alternative approach of measuring food consumption through a 24 hour recall survey, whereby respondents are asked to recall all the foods consumed by each household member during the previous day.

that is extremely low (<500 kcal) or extremely high (>4500 kcal). Iron is of particular interest since iron deficiency determines to a large degree the prevalence of anemia, so widely present in Mexico. (see Barquera et al., 2003a). Inorganic (or nonheme) iron is a mineral widely present in relatively inexpensive foods such as beans and spinach. Mexican diets are very rich in nonheme iron, but the quality of it is poor, so nutrient absorption is poor. Heme iron, on the other hand, has a much better absorption rate, but its sources are animal-products, which are expensive. More importantly, it is the lack of heme iron that determines the prevalence of anemia. We present separate results for heme iron since findings for this nutrient are much better understood and applied (than those for total iron).

Since one of our main purposes is to study whether nutrient consumption changes between poorer and richer households we present the statistics for three groups of households: all households, households at the bottom 25% of the distribution of per capita expenditure (PCE), and households at the top 25% of PCE. We use PCE, and not current income as a measure of household welfare and income because, in general, current expenditure and consumption tend to be a more reliable estimate of a household's permanent income than current income[7]. PCE is derived by dividing total food and nonfood expenditures by household size. Total household expenditure (per month) is defined as the sum of value of food consumption, value of meals consumed away from home and total expenditure for goods other than food (excluding expenditures on health services). Deaton and Zaidi (2002) stress that in cases in which the amount of food consumed can be distinguished from food purchased (as is the case with our data), it is the value of food consumed that should go into the consumption aggregate. The value of food consumed at home is constructed, following the guidelines above, using the quantity of food consumed at home and expressing it in monthly value using as prices the median unit value for each food at the locality level[8].

One general pattern that is obvious in table 5.1 is that the value for the mean is bigger than the median value for all the nutrients considered here. This means that the distribution of each nutrient is positively skewed rather than symmetric and that considering only the mean would lead to an overestimation of nutrient consumption. For this reason we also present the interquartile difference (IQ=Q75-Q25) as a measure of the standard deviation in the consumption of nutrients. Both the median and the interquartile range are better summaries of a distribution when the data are skewed or contain outliers. Another remarkable finding is the difference between the consumption for the top 25% PCE and the bottom 25% PCE population (for example, the top 25% PCE households displays a calorie consumption that is 64% bigger than the bottom 25% PCE group; the comparison for calcium is even more striking since the top 25% PCE population's consumption is around four times bigger than the bottom 25% PCE's). Iron and heme (blood) iron present a contrasting pattern: while top 25% PCE's consumption for total iron is only around 1.2 times bigger than bottom 25%'s, the proportion is around 6.5

[7]It can be argued that for very poor households' differences in current expenditure and current income might be less pronounced (see De la Torre, 2005).

[8]We also have the information of the market price for the food items at the locality level. However, we do not have the market price for all the food items that are included in the list of foods consumed (either some items are not included in the market price list or the definition of the food item is different).

times when it comes to heme iron. These descriptive results seem to suggest that changes in income can affect nutrient consumption, even in a sample of households which overall is quite poor and by first appearances not very heterogeneous.

One implication of this result is that on average households in the top 25% group could have a nutrient consumption adequacy[9] above 100% while those in the bottom 25%'s could be far below 100% with this completely differentiating the possible interventions to be designed for the two groups. We study the nutrient consumption adequacy in table 5.2, which reports for some selected nutrients the ratio between the median intake (as shown in table 5.1) and the recommended daily intake (RDI; constructed from nutritional tables[10]).

For the median person in our sample, the biggest nutrient deficiencies are about vitamin A, iron, and calcium. When we consider the sample of those at the bottom 25% of PCE this list has to be enlarged so as to include fat and vitamin C: intakes are well below the benchmark values. Richer households continue to have insufficient intake of vitamin A, iron and calcium while displaying values above 100% for the other nutrients. On average calorie intake is close to the reference level (94.9%), though poorer households are only consuming 70.8% of the recommended intake. Some nutrients display remarkably high adequacy ratios across all the samples, these are carbohydrates and folate. Intake of protein seem not to be the biggest nutritional problem in this sample, but poorer households are below the benchmark level (adequacy ratio is 78.7%).

Overall figures in table 5.2 are suggesting that while calorie intake is a problematic issue only for very poor households, there are major deficiencies of key nutrients, which both poorer and richer households are facing (especially for vitamin A, iron, and calcium).

While our focus here is on the consumption of nutrients, it is worth giving a brief descriptive picture of the food consumption patterns in our sample. Changes in nutrient intakes can be ultimately linked to changes in food consumptions, therefore it will guide the reader having a grasp of which foods are consumed in our sample and how this consumption changes with income. In table 5.5 we first report the percentage of calories that households are getting from a specific food group[11] (see column labeled "average percentage of calories from:") and we notice that on average almost half of the calories are coming from maize (42.4%). Other food groups that provide a substantial percentage of the total calories are oils, meat, fish and dairy, sugars, and beans. Another interesting exercise is to assess which foods households are using to get their caloric intake from, one first way to do that is to study the percentage of households that are consuming a positive amount of a specific food group (that implies they are using it as a source of calorie) and how this changes with deciles of PCE. In table 5.5 we see that on average (see column labeled "all sample") almost all households (98.9%) get part of their calories from maize, but only around 59% use wheat as source. More interesting patterns arise when we consider different samples according to deciles of PCE: groups such as wheat and fruit are consumed by, respectively, only 22% and 56% of the households in the first decile of PCE sample.

[9] The nutrient intake adequacy is typically expressed as the ratio between the household's nutrient consumption and an appropriate reference intake.

[10] The recommended intake used in our calculation takes into account the different age and gender composition of the 3 samples we use: all households, bottom 25% of PCE, top 25% of PCE.

[11] See notes in table 5.5 for the list of foods included in each food group.

However, as we consider higher income deciles these percentages rise dramatically and for the top income deciles sample we have around 79% and 94% of the households using wheat and fruit, respectively, as a source of calories.

Another interesting indicator is the number of different foods households use to get their calories from (we call it "variety index"; see last row in table 5.5): households add almost 2 food groups to their diet passing from the lowest decile of PCE (they consume 7.43 foods on average) to the highest (9.29).

We then study how calories taken from each food change as income rises: in figure 5.1 we plot the percentage of calories coming from the different food groups at each decile of PCE. One clear pattern arises: richer households tend to substitute maize and beans with meat, fish and dairy, wheat and fruit as their source of calorie. Interestingly, the contribution of vegetables to the total caloric intake remains very low also for rich households.

Finally, we give some descriptive statistics of PCE, which we use here as our measure of income. In table 5.4 we report statistics for the sample of all households we have available (see row "all sample") and for several sub-samples, defined according to PCE, which are relevant for the analysis below. In addition to this, we also report for each of these sub-samples the proportion of poor households (the headcount ratio, P0, is reported in the table) according to both an absolute and a relative poverty line (see the notes in table 5.4 for details on how the poverty line is defined). For example, households that have a PCE between the 25th and the 75th percentile of PCE have a median (average) PCE of 430 (435) monthly pesos and 14% of households in the same group are poor according to an absolute poverty line (22% according to a relative poverty line).

§5.3. Nutrient – Income Elasticity: linear specification

We estimate here the nutrient-income elasticity with a linear regression approach. Some of the benefits of the linear regression model include the ability to control for a large set of control variables, including village or municipality specific fixed effects, and to take into account with standard econometric methods the possible biases due to measurement error and endogeneity issues. The cost, on the other hand, is that the conditional relationship between nutrient and income is assumed to be linear, or in other terms, the elasticity nutrient-income is constant. This is a quite restrictive assumption, whose validity will be tested in next section in which we explore the non linearity of the relationship between nutrient consumption and income with several approaches.

Here, for each nutrient, we estimate a linear regression of the form:

$$\ln NUT_{i,v} = \alpha_0 + \alpha_1 FE_v + \beta Z_{i,v} + \gamma \ln PCE_{i,v} + \varepsilon_{i,v} \tag{5.1}$$

where NUT is per capita nutrient consumption in household i in locality/municipality v, FE is a vector of binary variables summarizing village or municipality-specific fixed effects, Z is a vector of household characteristics and ε is an error term.

The inclusion of area-specific fixed effects, FE, is intended to control for village or municipality-specific characteristics that may have also a direct impact on nutrient consumption[12]. The vector Z includes age-sex household composition ratios, age and educational level of the household head and of his/her spouse, count of how many assets (of a list including radio, television, VCR, phone, computer, washing machine, heater, motorcycle and car) the household owns (included as binary variables for each category from 1 to 5 assets and one binary variable for "owns 6 or more assets"), binary variables for "dirt floor", "wall material is cardboard, palm, reed or bamboo" and "roof material is cardboard, palm or word tiles" and binary variables indicating whether the household head and his/her spouse speak an indigenous language. Moreover, we include the locality price for 40 food items as recorded in the locality module of the PAL survey.

The results of the OLS estimation of (5.1) are presented in table 5.5 (in the first column). One clear pattern that emerges is that the estimated elasticities for the sample of all households are all positive, quite high and significant for all macro and nutrients. The calorie income elasticity is 0.44, remarkably similar to 0.35 calorie income elasticity estimate of Subramanian and Deaton (1996) for India, and the 0.43 estimate of Skoufias (2003) for Indonesia. The nutrients with the highest income elasticity are vitamin A (1.26) and vitamin C (1.11). Thus a 1 % increase in income is likely to result in an increase of more than 1% in the consumption of vitamins A and C. These estimates are much higher than the income elasticities reported by Pitt and Rosenzweig (1985) for the same nutrients using a sample of farm households.[13] One possible explanation for the lower elasticity estimates obtained by these authors is the fact that nutrient conversion factors were applied at twelve aggregated food groups rather at the individual food item level as in this study. As Pitt and Rosenzweig (1985) acknowledge, this approach may be responsible for their low elasticity estimates since it ignores possible substitutions within food groups.

Among the nutrients examined, remarkably high values are also displayed by calcium, cholesterol, and iron heme. The income elasticity for iron, folate, and zinc ranges from 0.315 (iron) to 0.415 (zinc). [14]

In this study the measurement of nutrient consumption is done by converting food quantities into nutrient availability using food composition tables. While this method has the advantage of being easily implemented, it suffers from several potentially important sources of systematic error. Major drawbacks are that this method assumes that no food is wasted (and this will cause problems in case very low-income households waste less than those that are better off), does not take into account explicitly meals given to guests or employees and meals received in-kind (however, this issue can be addressed if the survey collects information about it) and meals taken

[12]In the OLS we include locality dummies (there are 235 localities in our sample). In the IV approach we include municipality dummies (there are 108 municipalities in our sample and each of them includes on average 2.16 localities) as we use instruments that vary at the locality level.

[13]Their profit elasticity estimates are 0.0245 and 0.0274 for vitamins A and C, respectively.

[14]In the literature several estimates have been provided: among others, Behrman and Deolalikar (1987) that using data from ICRISAT report elasticity estimates of 0.06 to 0.19 for protein, 0.30 to -0.22 for calcium, -0.11 to 0.30 for iron (depending on whether level estimates or differences over time are used); Behrman and Wolfe (1987) study Nicaragua and find elasticities in the range 0.04 to 0.11 for calories, protein, iron and vitamin A; Bouis (1991) reports an iron-income elasticity of 0.44, a calorie-income's of 0.16 and insignificant elasticities for vitamin A and C.

away from home (this can be a source of bias since, for example, it is not necessarily true that meals taken away from home have the same caloric consumption of meals eaten at home).

Accordingly, it is likely that measurement error in nutrient consumption will be correlated with PCE, with this being a source of bias in estimates of nutrient-PCE elasticities. In particular, as first noted by Bouis and Haddad (1992), the possibility that measurement errors in nutrient consumption are likely to be positively correlated with measurement errors in household consumption implies that this type of measurement error is not the classical errors-in-variables problem where coefficients are likely to be biased towards zero (attenuation bias). In the context of correlated measurement errors in the dependent and independent variables of a regression, the upward bias from the correlated errors will typically outweigh the standard downward attenuation bias from the measurement error in total consumption, leaving a net upward bias in income elasticity estimates obtained using OLS methods. However, even assuming that consumption is perfectly measured several other factors can potentially bias our estimates: unobserved variables could be correlated both with nutrient intake and income. In addition to this, a reverse causality argument applies here as high nutrient consumption might bring better health that itself causes higher income. With this in mind we estimate specification (5.1) also with an instrumental variable (IV) approach. Choice of a valid instrument is always a quite complicated task. Ideally, we would like to have a variable that is correlated with PCE but not with unobservables that drive nutrition. We try four different instruments: locality median non food expenditure, locality mean non food expenditure (we assign a value to each household that is the locality mean calculated not considering the same household), count of how many assets the household owns (as a binary variable for each category) and locality median of PCE. Each of them has its own strengths and weaknesses, *a priori* we prefer the first 2 as they are not affected by the measurement error issue of using the same quantities for constructing the nutrient intakes and the expenditure measure. Following this same argument, locality median (or mean) PCE is a less valid instrument. In this and in the following section we will always present both the OLS and the IV results, even if we will mostly focus on the IV results when we discuss and interpret our findings.

In table 5.5, we report the results with these different variables as instrument for ln PCE[15]. All the instruments seem to have enough power as the F-test reported at the bottom of table 5.5 suggests. One general pattern from the IV results is that most of the estimated elasticities are lower than those from OLS (and often not significant), with this supporting the story of upward biased OLS estimates. If we focus on the IV with locality median non food expenditure as an instruments, a notable result is that the elasticity calorie-income is very small and not significant (and this is robust to the choice of the instruments; only when locality median PCE is used the elasticity is positive, but much smaller than OLS, and significant). The same pattern arises for key macro and nutrients such as fiber, protein, carbohydrates, folate and zinc. On the contrary, remarkably high elasticities are displayed by the IV estimate for cholesterol and fats in general, vitamin A and C, and calcium. Interestingly, these results are very consistent with

[15]When the instrument is the locality mean or median, also its square is included in the first stage regression.

the adequacy ratios (see table 5.2): bigger IV elasticities are associated with nutrients with low adequacy ratios. In other terms, increase in income seems to be the most effective exactly for those nutrients for which the average person in our sample is the most deficient.

One issue we did not address explicitly in the estimation above is that of zero intakes. Some nutrients may be only present in some particular foods, and in case these foods are not consumed by the households, consumption of these nutrients will be zero. Households may be facing non-negativity constraints which at current income and prices make it optimal to consume only some foods, i.e. zero expenditures reflect corner solutions. Estimation of the income elasticity of nutrient consumption on a sample that does not include zero consumption (as in our case since we use variables in logs) might lead to biased estimates, especially for the sample of households at the bottom part of the distribution of PCE. While the bias might depend upon different factors other than zero consumption (such as endogeneity of PCE), simple intuition suggests the presence of a downward bias of the estimated elasticity: provided that households with zero intakes are those at a corner solution (i.e., poorer households) the selected sample of those with positive intake will consist of better-off families for which we expect the nutrient-income elasticity to be lower. In our sample, only two nutrients show a relevant fraction of zero values: cholesterol and heme iron[16]. Hence, we estimate the income elasticity of these two nutrients with 4 estimators that take into account the presence of zero values: censored least absolute deviation estimator (CLAD), CLAD-IV, TOBIT and TOBIT-IV. The CLAD relies on much weaker distributional assumptions than the TOBIT method, but it cannot easily control for endogenous regressors.[17] Our findings suggest that the presence of zero intakes is a very mild source of bias in the estimation of the elasticity cholesterol-income (estimated coefficients with CLAD and TOBIT are quite close to those from OLS and IV. On the contrary, not taking into account zero intakes seems to bring to a sizeable downward bias of the OLS and IV estimates for heme iron: for example, the TOBIT-IV estimator yields an income elasticity of 1.353 as compared to an elasticity of 0.411 with the linear IV. This result is going to be taken into account when interpreting findings about heme iron in what follows.

§5.4. Nutrient-income elasticity: non linear specification

We explore potential nonlinearities in the relationship between nutrients and income in four different ways. First, we use two flexible specifications, still linear in parameters, but that allow elasticity to change with income. In particular, we estimate specification (5.2) ,"log PCE + inverse PCE", and (5.3) "log PCE + log PCE squared".

$$\ln NUT_{i,v} = \alpha_0 + \alpha_1 FE_v + \beta Z_{i,v} + \lambda_0 \ln PCE_{i,v} + \lambda_1 \frac{1}{PCE_{i,v}} + \varepsilon_{i,v} \tag{5.2}$$

[16] This finding is consistent with the 1999 National Nutrition Survey of Mexico identifies zinc and iron deficiency as a major nutritional problem in Mexican children (Barquera et al., 2003a).

[17] Our IV approach is to use the residuals (third order polynomial) from a first stage (OLS of ln PCE on our instruments) in the CLAD and TOBIT regression in the second stage. For a more intuitive explanation of the CLAD see Deaton (1997). Blundell and Powell (2004) provide a description of more recent developments.

$$\ln NUT_{i,v} = \alpha_0 + \alpha_1 FE_v + \beta Z_{i,v} + \gamma_0 \ln PCE_{i,v} + \gamma_1 \left(\ln PCE_{i,v}\right)^2 + \varepsilon_{i,v} \tag{5.3}$$

Nutrient-income elasticity will be as in (5.4) and (5.6) for specification (5.2) and (5.3) respectively.

$$\frac{d \ln NUT}{d \ln PCE} = \lambda_0 - \lambda_1 \frac{1}{PCE} \tag{5.4}$$

$$\frac{d \ln NUT}{d \ln PCE} = \gamma_0 + 2\gamma_1 \ln PCE \tag{5.5}$$

Obviously this represents only a basic way to deal with nonlinearities, but it allows estimation of (5.2) and (5.3) with standard linear econometric tools.

Results are in table 5.6 and 5.7, for specification (5.2) and (5.3), respectively. We report the elasticity for each nutrient computed at three percentiles of the PCE distribution: 25th, median and 75th. Both the OLS and IV (using as instrument locality median non food expenditure and its square) results are shown.

If we focus on the IV results for specification (5.2) (see table 5.6; these are very similar to those for specification (5.2)) some interesting patterns arise: for some nutrients (namely calcium, vitamins A and C) income elasticity is positive, very high and significant across the entire PCE distribution; in particular it remains very high also at the 75th PCE percentiles. For fats (fat, cholesterol, saturated, monounsaturated and polyunsaturated fats) the pattern is a sizeable elasticity at the 25th PCE percentile that is decreasing as PCE increases and it is never significantly different from zero at the 75th percentile (see the results for IV).

One important finding is that even at the bottom of PCE distribution energy does not seem to increase with income (IV estimate for 25th percentile is 0.041 and not significant) and elasticity is negative at median and 75th percentile. Intake of carbohydrates would never go up as PCE increases according to our IV results. Estimated negative elasticity increases in magnitude at higher PCE percentiles, though it is never significant. Protein and Zinc show a similar pattern as income elasticity is increasing with PCE, but the estimated elasticity is not significantly different from zero. Finally, the behavior of iron heme is quite peculiar as the elasticity goes from a being negative for the 25th PCE percentile to a very high positive value for the 75th percentile. Obviously, the zero intake issue (potentially biasing downwards the IV estimate) is particularly relevant for the 25th percentile sample.

A more general approach to capture non linearities of the nutrient-income relationship is to estimate an income spline specification, which is a specification that includes intercept and interaction dummies for relevant segments of the PCE distribution. In particular, we include dummies and their interactions with ln PCE for the 4 population quartiles based on the distribution of PCE. In this exercise we try to deal with endogeneity of PCE with a control function approach in which the residuals from the first stage regression (as a 6th order polynomial; estimated from a regression of ln PCE on controls and locality median non food expenditure and its square) are included in the regression of ln nutrient intake on intercept dummies, interactions with ln PCE and other covariates.[18] Results are in table 5.8, where the estimated coefficients for each quartile are reported both for OLS and the IV approach[19]. Findings are remarkably in line with those from the flexible specification exercise above. Income elasticity for calories is higher for poorer households (those in quartile 1) but not significant in the IV approach. Fats, vitamins and calcium display a sizeable, positive and significant elasticity no matter which segment of the PCE distribution is considered, that is decreasing at higher quartiles. Elasticity for protein is never significant, even if the point estimate is generally quite large (0.139 for quartile 1). Carbohydrates and zinc seem not to respond to income increases at any quartile and the point estimates are negative at higher quartiles for carbohydrates. Finally, iron heme show a large and significant elasticity at all quartiles (that is not significant for the first quartile, but once again we stress that the downward bias from the zero intake issue is expected to be much more relevant for poorer households).

Next, we address non linearity in the relationship between nutrients and income by means of a quantile regression approach. Besides providing a richer characterization of the data, quantile regression is more robust to outliers than least-squares regression and quantile regression estimators can be consistent under weaker stochastic assumptions than with least-squares estimation (see Koenker and Bassett, 1978). We estimate a conditional quantile linear model for percentiles 10, 25, 50, 75 and 90. This means that the estimator will fit a line through the observations that are at the same percentile of the nutrient intake distribution conditional to the regressors. The difference of the estimated income elasticity for the different percentiles will give us a sense of the heterogeneity of the response of nutrition to income among groups that are at different parts on the distribution of nutrient intake. We deal with the endogeneity of PCE with a control function approach, which follows Lee (2007). In particular, we use a two-step estimator in which the first step is the estimation of residuals from a quantile regression of ln PCE on a set of covariates[20] and the ln locality median non food expenditure and its square (our instruments for PCE) and the second step consist of a quantile regression of the ln nutrient on covariates and a 3rd order polynomial of the residuals estimated in the first step. Standard errors are obtained bootstrapping this procedure with 300 replications.

In table 5.9 we report the results of the quantile regression linear approach (that is, not taking into account endogeneity of PCE) in the first set of columns and of our IV approach in the last set of columns for 5 selected percentiles. The IV approach results offer several interesting findings: energy intake (kcal) is unaffected by increase in income for households at low percentiles of the conditional distribution (below the median) with this confirming the results above. However, energy intake will decrease as PCE increases at the top percentiles (75th and

[18]For more details on the control function approach see, among others, Florens et al. (2007) and Wooldridge (1997, 2003).

[19]As they are more easily interpreted, we report the estimated coefficient for each quartile and not the difference with the omitted category. For example, the 0.368 for energy, quartile 2 in the OLS estimation in table 5.8 refers to the slope of the income spline for quartile 2 (and not to the difference between the slope of quartile 2 and the omitted quartile, quartile 1).

[20]In particular, we include age and gender composition dummies (number of males and females in age groups 0-4, 5-9, 10-14, 15-54 and above 55), age of head and spouse, education of head and spouse, dummy for head and spouse speaking indigenous language, and locality prices 40 food items.

90th). As regards another macronutrient, proteins, intake would decrease with PCE at the very top of the conditional distribution (90th percentile) and would not increase at low percentiles (point estimates are sizeable but not significant). A completely different pattern is displayed by the fat group (see rows in table 5.9 from "Fat" to "Polyunsaturated fat") as no matter which percentile we consider the effect of PCE is always significant and very high. In addition to this, the coefficient of ln PCE is quite similar across the percentiles considered, with the exception of cholesterol for which the effect of PCE is much larger at the 10th percentile. A similar pattern applies for vitamins A and C and calcium as they show extremely high elasticities at low percentiles which tend to become smaller at higher percentiles, but still quite high and significant. One important result is the one about carbohydrates: the intake will decrease no matter which percentile is considered and the effect tends to be larger at higher percentiles. Finally, intake of iron heme would always increase with PCE across the percentiles of the conditional distribution.

Lastly we deal with non linearity in a semi-parametric way. This approach gives full flexibility as regards the function linking PCE to nutrient intake, but it comes with the cost of allowing only a restricted set of controls to be included in a tractable way in the estimation. As we do not explicitly take into account the endogeneity of PCE in this exercise, the findings here should just be taken as giving a very general picture of the relationship between nutrient intake and PCE.

The model we estimate below is a partially linear model:

$$y_i = z_i\beta + m\left(x_i\right) + \varepsilon_i \tag{5.6}$$

where y_i denotes the *ln* of the nutrient intake, $z_i$is a vector of the variables that we would like to control for in a linear fashion, β is a vector of parameters and *m(x)* is a nonlinear function of x, here ln PCE.

This model has been traditionally estimated with the Robinson (1988) estimator, which is especially suitable for the estimation of the vector β in (5.6). Since we are primarily interested in the estimation of *m(x)* we implement an estimator based on a differencing approach (first suggested by Yatchew, 1997, and discussed by DiNardo and Tobias, 2001). The procedure for estimating (5.6) consists of the following steps: first, the data are sorted by ascending values of the x variable (in our case ln PCE) and the m-th order[21] differences are calculated on the sorted data. The idea here is that if $x_i$and x_{i-1} are close enough in the sorted data, then so will $m\left(x_i\right)$and $m\left(x_{i-1}\right)$. Accordingly, the differenced version of the model (5.2) on the sorted data will remove the nonparametric component $m\left(x_i\right)$. Then the vector β can be estimated with a regression of the differenced y's on the differenced z's. With the estimated vector $\hat{\beta}$ in hand it is then possible to derive a new "adjusted" dependent variable net of the linear effect of the z variables, i.e.,

[21] As noted in Yatchew (1997) the differencing order is important as far as the efficiency of the estimator is concerned. In order to maximize the efficiency of the estimator, we use the optimal differencing weights, as tabulated in Yatchew (1997), to compute differences of the sorted data. We set the differencing order to 3 to compute differences in the semi-parametric estimation. We also tried other differencing orders and the results did not change substantially.

$$y_{adjusted} = y_i - z_i\hat{\beta} \tag{5.7}$$

The final step is to perform a local linear regression using the variable defined in (5.7) as dependent variable. In particular, we use a smooth local regression technique similar to that used by Subramianan and Deaton (1996). Their procedure works as follows. At any given point of x, we run a weighted linear regression of the logarithm of the dependent variable $y_{adjusted}$ on ln *PCE*. The weights are chosen to be largest for sample points close to x and to diminish with distance from x; they are also set so that, as the sample size increases, the weight given to the immediate neighborhood of x is increased so that, in the limit, only x is represented. In our case, for the local regression at x, observation i gets the (quartic kernel) weight

$$w_i\left(x\right) = \frac{15}{16}\left[1 - \left(\frac{x - x_i}{h}\right)^2\right]^2 \tag{5.8}$$

if $-h \leq x - x_i \leq h$ and zero otherwise. The quantity h is a bandwidth that is set so as to trade off bias and variance (in general a small bandwidth brings smaller variance but higher bias while a large h determines a small bias but higher variance). Our main objective is to plot the regression function and its slope so that, instead of calculating local regressions for each point in the sample, we use an evenly spaced grid of 60 points in the distribution of ln *PCE* and calculate a local regression for each grid. The estimate of *m(x)* is the predicted value from the local regression at x, while the local estimated slope coefficient provides an estimate of the slope *m'(x)*. Given that both y and x are expressed in log form, the derivative of the regression function, *m'(x)*, is an estimate of the elasticity of the demand for nutrients with respect to income. Then a graph of the nutrient-income elasticity estimate against the level of (log) income allows one to determine easily the extent to which the elasticity varies with income. The bandwidth h for the quartic kernel weight is set to 0.5 after inspection of alternative plots. This value for h seems to be appropriate with respect to the trade off between bias and variance of the estimated regression function. The vector z in eq. (5.6) includes the age and gender composition of the household expressed as ratios of the total family size. Specifically, the age and gender groups are males and females between age 0 to 4, 5 to 9, 10 to 14, 15 to 54 and more than 55.

Figure 5.2, 5.3 and 5.4 show the slope of the estimated functions that link the nutrients and ln *PCE* (we do not show the estimate functions themselves for the sake of brevity). In general the relationship between nutrient intake and PCE seems not to far from being linear around the median of ln *PCE* (indicated by the vertical line in the plots) but not at the extremes of PCE.

Two main patterns seem to be prevalent in the income elasticity of nutrients. For some nutrients (namely fiber, protein, folate, zinc, calcium) the elasticity is either gently decreasing or constant at low values of ln PCE, then it becomes constant around the median of ln PCE for starting to decrease only a quite high value of ln PCE. Most of the other nutrients (particularly fats and vitamins) show a much more steady decrease of the estimated elasticity as ln PCE increases. Not a completely clear pattern arises when it comes to elasticity of energy to ln PCE as the slope of the estimated regression function is fluctuating quite a lot. However, elasticity of energy is spanning a much smaller range than in the case of the other nutrients. From figures 5.2, 5.3 and 5.4 it can be gained that the average elasticity below the median of ln PCE (the horizontal line in the plot) is larger than the average elasticity above the median. Heme iron shows a remarkably ample fluctuation at low levels of ln PCE, then it stays constant around the median and there is only a quite small drop at very high values of ln PCE.

§5.5. Concluding remarks: putting together the results and policy implications

This paper provides estimates of the extent to which nutrient consumption at the household level responds to increases in household income. The income elasticity for a specific nutrient, the parameter that summarizes the percentage change in the consumption of a specific nutrient corresponding to a one percent change in household income, is critical to understanding one of the key determinants of consumption of nutrients. As household income increases, households may change the composition of their food consumption and thus affecting both the intake of total energy (kcal) and of specific nutrients. If increases in income result in changes in the diet of households towards foods with higher nutrient content (for example, eating more vegetables/fruits and meat), then nutrient deficiencies may fall.

An obvious starting point in interpreting our results is the discussion of the size of the income elasticity for calories. As discussed above estimated elasticity for calories in previous literature span a very large range, going from zero to a quite sizeable positive number, with these differences depending on the estimation method, type of food survey and geographic area of interest. As such, these estimates have very little policy content as the implications of a small vs. a large income elasticity are completely different. Here, we find a calorie-income elasticity close to zero: our preferred estimates (IV with locality median non food expenditure as instrument) gives a not statistically significant elasticity of 0.029 (see table 5.5). When we use a flexible parametric specification we find that the elasticity is bigger for households at the 25th percentile of PCE (between 0.04 and 0.07; see IV results in table 5.6 and 5.7) but still not significantly different from zero. In addition to this, the quantile regression (see IV approach in table 5.9) results show a zero elasticity at low percentiles and a negative and significant elasticity at the 75th and 90th percentile. A zero elasticity for calories is quite consistent with the adequacy ratios in table 5.2, which show that on average households do not have a deficiency of energy intake (that is around 95% of the recommended daily intake). However, we would expect poorer population in this sample to have a deficient intake of calories (and this is the case: household in the bottom 25% of PCE have an adequacy ratio of only around 71%) and our results are suggesting that elasticity is not different from zero also for this group of more vulnerable households. A zero calorie-income elasticity suggests that households are relatively successful in maintaining energy levels constant as their income varies. This result is consistent with the findings of a recent paper that studies the consequences of the world food price crisis on nutrition in China (see Jensen and Miller, 2008). They find that the food price increase did not have an effect on the calorie intake of poor households in two Chinese provinces mainly because these households were able to substitute to cheaper foods and because the domestic prices of staple foods remained relatively insulated from the world price increase due to government intervention in grain markets. From a policy perspective, this would require policies to focus more on the way (which macro and micronutrients) households obtain their calories. In terms of research focus, this supports the idea that the behavior of specific macro and micronutrients when income or price changes is probably a more interesting research topic than what happens to aggregate calorie intake.

For a group of nutrients – namely fats (fat, cholesterol, saturated, monounsaturated, and polyunsaturated fat), vitamin A and C, heme iron and calcium – our findings are remarkably consistent across different specifications and estimation methods: elasticity is positive, very high and statically significant. In addition to this, it is generally extremely large for lower percentiles and it tends to decrease at higher percentiles but still remaining quite sizeable in magnitude. Interestingly, these nutrients are also those for which we find the largest deficiency in our sample; for example, on average the intake of vitamin A is only 23.5% of the recommended intake and that of calcium only 48.1% and even households in the top 25% of PCE do not afford an adequacy ratio of 100%. As regards heme iron, it has to be noticed that the zero intake issue is particularly relevant for this nutrient with this meaning that the estimates presented might be affected by a downward bias, especially for the poorer population. Overall, increases in household income seems to translate into greater intake of these nutrients, which is probably capturing the effect of changes in the diet from one made mostly of cereals to one with more meats, vegetables and fruits.

Elasticity for carbohydrates and fiber are either zero or negative but never significant in the IV and flexible specification exercise. In the quantile regression a neater pattern arises as elasticity is negative and significant for all the percentiles considered and the magnitude tends to increase at higher percentiles. As the adequacy ratio for these two nutrients is well above 100%, also for the poorer population, this result seems to give additional evidence to the results above about fats, vitamins and calcium: households might be substituting away from cereals.

Findings are more mixed for protein and zinc: in the IV estimation elasticity is not significantly different from zero. Results from the flexible specification exercise show an elasticity that is close to zero or even negative at the 25th percentile of PCE and then it tends to increase at higher percentiles, but still it is not significant. The quantile regression's results suggest that elasticity is significantly negative at higher percentiles (90th for protein, above median for zinc). As we would expect the poorer population to face deficiency of protein and zinc (as it is the case: for households at the bottom 25% of PCE the adequacy ratio for protein and zinc is below 80%), this results is suggesting that increase in income might not be enough to foster an increase in intake of these nutrients for the most vulnerable groups.

Some policy implications can be put forward on the basis of the results above. Overall, our estimates establish that increases in income are associated with significant and sizeable increases in the consumption of vital nutrients among poor households in rural Mexico, namely vitamins A and C, heme iron, calcium and fats.

Thus, increases in household income resulting from participation in poverty alleviation programs that provide direct (and unconditional) cash transfers, or economic policies that result in higher rural wages, and increased profitability of agricultural production might be particularly successful in achieving increases in the consumption of key nutrients at the household level.

On the other hand, intake of other nutrients seem not to respond to income or show a negative elasticity. This can be exactly in line with policymakers' objectives in case of nutrients for which there is an over-intake behavior (as it is the case for carbohydrates and fiber in our sample), even among poorer population. However, in some other cases a zero response of nutrient intake to income could be more generally indicating a limit of interventions that only focus on transfer of money. For instance, we find that for a particularly vulnerable group in our sample (poorer households, at the bottom 25% of PCE) a deficiency of total energy, protein and zinc is not accompanied with an estimated positive income elasticity. In other terms, increase in income does not seem to be a policy tool that can remedy the deficiency in energy, zinc and protein for poorer households.

Finally, our study focused on the estimation of income elasticity at the household level. As such, it is leaving unanswered the critical question of whether increases in nutrient consumption at the household level translate to increases in the intake of key nutrients by infants and other vulnerable children to nutrient deficiencies. Perhaps alternative approaches that are more direct may be more effective. For example, in-kind transfers of key food items that provide the essential nutrients may be more effective than direct cash transfers to their parents at decreasing malnutrition among infants and young children. It is hoped that future research as well as the data collected over the next rounds for the evaluation of the PAL program will be able to shed more light on this issue.

Table 5.1: Per capita daily nutrient consumption

Nutrient	**All**			**bottom 25% of PCE**			**top 25% of PCE**		
	mean	median	IQ range	mean	median	IQ range	mean	median	IQ range
Energy (kcal)	2203	2086	1162	1657	1549	853	2700	2653	1231
Fiber (g)	35.7	32.9	21.9	31.7	28.3	20.6	39.3	37.3	22
Protein (g)	56.1	52.5	31.3	40.63	37.2	23.1	72.6	70.7	32.8
Fat (g)	59.4	54.3	38.4	33	21.9	10.4	84.7	81	44.2
Cholesterol (mg)	147.2	117.3	121.4	63.1	50.5	69.9	231.5	185.5	151.8
Saturated fat (g)	15.6	13.6	11.8	7.2	6.6	5	23.9	22.4	12.9
Monounsaturated fat (g)	20	18.1	13.5	10.6	9.9	7	29	27.7	15.8
Polyunsaturated fat (g)	14	12.3	12	7.4	6.5	6.7	20	18.3	14
Carbohydrates (g)	364.4	338.9	204	303.4	278.4	180.7	413.9	395.2	206.5
Vitamin A (mcg ER)	158.6	122	155.2	53	41.2	51	283.5	250.8	200.7
Vitamin C (mcg)	73.6	50.2	69.3	28.5	18.9	26.61	128.7	96.6	95.3
Folate (mcg)	417.6	373.8	258.4	334.4	278.2	218	507.7	474.4	275.9
Iron (mg)	15.6	14.1	10	14.1	12.7	9.46	16.9	15.6	9.4
Heme iron (mg)	0.155	0.109	0.187	0.044	0	0.068	0.288	0.236	0.272
Zinc (mg)	8.71	8.06	2.84	7.1	6.42	4.64	10.3	9.8	5.1
Calcium (mg)	609.9	495.5	628.1	258	182.7	185.7	1004.4	942.2	677.4

Notes: Sample is restricted to households with a kcal intake >=500 and <=4500.

Table 5.2: Adequacy ratios

Nutrient	All	bottom 25% of PCE	top 25% of PCE
Energy (kcal)	94.9	70.8	120.4
Fiber (g)	118.0	101.8	133.7
Protein (g)	110.3	78.7	147.9
Fat (g)	89.8	36.4	133.5
Carbohydrates (g)	257.7	211.3	300.6
Vitamin A (mcg ER)	23.5	8.0	48.3
Vitamin C (mcg)	94.2	35.9	179.8
Folate (mcg)	243.1	182.7	306.4
Iron (mg)	63.6	57.5	70.1
Zinc (mg)	96.7	77.5	117.2
Calcium (mg)	48.1	17.8	91.4

Adequacy ratio= ratio of median intake (as in table 1) to recommended daily intake (from nutritional tables); Recommended daily intake was calculated taking into account the different age and gender composition of the 3 samples: all, bottom 25% of PCE, top 25% of PCE.

Table 5.3: Food consumption patterns

	Average Percentage of calories from:	Percentage of households getting calories from the food group:									
		All sample	p10	p20	p30	p40	p50	p60	p70	p80	p90
Maize	42.4	98.9	99.5	99.7	98.5	98.6	98.9	99.2	99.2	98.5	98.0
Oil	12.5	90.4	83.6	90.6	87.6	91.0	91.9	90.9	93.4	92.6	91.9
Meat Fish and Dairy	10.4	95.7	78.2	95.2	96.4	97.4	99.1	98.6	98.6	98.9	98.8
Sugars	10.4	96.3	88.0	96.4	97.4	97.4	98.0	98.6	97.4	97.7	95.8
Beans	8.0	93.5	89.2	93.4	93.4	93.5	96.8	93.7	92.8	94.3	94.1
Other cereals	3.3	77.0	55.2	73.1	74.5	75.2	82.2	81.9	82.5	82.4	85.8
Wheat	2.8	58.8	22.0	43.0	50.8	58.5	62.6	66.6	69.9	76.6	78.8
Fruit	2.4	82.0	56.2	76.0	81.3	80.7	87.0	86.4	87.2	89.1	93.4
Vegetables	1.0	97.5	90.4	97.4	97.4	97.9	99.1	99.1	98.9	99.2	98.2
Other foods	6.8	92.3	80.3	90.4	93.7	91.6	93.8	95.2	94.7	96.2	94.1
Variety index		8.83	7.43	8:55	8.71	8.82	9.09	9.10	9.15	9.26	9.29

Foods included in the groups: Maize (maize tortilla, maize in grains, maize flour); Oil (vegetable oil); Meat, Fish and Dairy (chicken, beef, pork, goat, seafood, sardines, tuna, eggs, milk, yogurt, cheeses, lard, cold meats); Sugars (sugar); Beans (kidney beans); Other cereals (pasta soup, biscuits, breakfast cereals); Wheat (white bread, sweet bread, loaf of bread, wheat flour, wheat tortilla); Fruits (guava, mandarins, papaya, oranges, bananas, apples, lemons, watermelon); Vegetables (tomatoes, onions, potatoes, carrots, leafy vegetables, pumpkin, chayote, chili, edible cactus); Other foods (rice, sweets, carbonated beverages, coffee)

Table 5.4: Per capita expenditure and poverty

Samples of PCE	Per capita expenditure		Absolute poverty*	Relative poverty**	
	Median	Mean	P0 (pov line=375.94)	pov line	P0
All sample	360	403	0.53	375.94	0.53
Quantile 1: bottom 25%	176	171	1	312	1
Quantile 2: between 25% and 50%	298	298	1	351	0.93
Quantile 3: between 50% and 75%	430	435	0.14	387	0.22
Quantile 4: top 25%	663	696	0	442	0
between 25% and 75%	357	367	0.56	377	0.57
bottom 10%	126	124	1	299	1
below median	240	236	1	326	0.85
around the median (between 40% and 60%)	357	360	0.65	362	0.54
above median	526	566	0.074	407	0.18
top 10%	807	829	0	450	0

* P0 (head-count ratio) is the proportion of household that have a PCE smaller than the 375.94 (which is the median PCE of households that have per capita caloric intake around the median (in particular between the median-100 kcals and the median+100 kcals); ** the poverty line is different in each sample (the median PCE in each sample of households that have per capita caloric intake around the median of the same sample (in particular between the median-100 kcals and the median+100 kcals)).

Table 5.5: Income elasticity of nutrients: linear specification

Nutrient	OLS	IV Loc median non food	IV Loc mean nonfood+	IV Count of assets	IV Loc median PCE
Energy	0.440*** (0.013)	0.029 (0.061)	-0.104 (0.102)	0.101 (0.120)	0.174*** (0.055)
Fiber	0.304*** (0.020)	-0.166 (0.143)	-0.446* (0.258)	-0.184 (0.309)	0.238 (0.188)
Protein	0.495*** (0.017)	0.098 (0.113)	-0.050 (0.209)	-0.058 (0.240)	0.446*** (0.159)
Fat	0.522*** (0.019)	0.514*** (0.115)	0.656*** (0.218)	-0.032 (0.251)	0.789*** (0.185)
Cholesterol	0.923*** (0.031)	1.004*** (0.141)	1.294*** (0.240)	0.706*** (0.164)	0.878*** (0.110)
Saturated fat	0.665*** (0.021)	0.778*** (0.125)	1.006*** (0.232)	0.078 (0.249)	1.015*** (0.180)
Monounsaturated fat	0.565*** (0.019)	0.491*** (0.070)	0.641*** (0.125)	0.358*** (0.118)	0.541*** (0.061)
Polyunsaturated fat	0.517*** (0.032)	0.727*** (0.160)	1.158*** (0.280)	0.076 (0.272)	0.912*** (0.222)
Carbohydrates	0.338*** (0.016)	-0.060 (0.094)	-0.221 (0.170)	-0.289 (0.208)	0.214* (0.122)
Vitamin A	1.259*** (0.036)	1.470*** (0.163)	1.741*** (0.281)	1.202*** (0.233)	1.444*** (0.147)
Vitamin C	1.109*** (0.036)	1.549*** (0.152)	1.874*** (0.274)	0.451* (0.242)	1.711*** (0.178)
Folate	0.381*** (0.020)	-0.022 (0.163)	-0.272 (0.284)	0.236 (0.273)	0.316* (0.174)
Iron	0.319*** (0.019)	-0.186* (0.112)	-0.426** (0.201)	-0.350 (0.247)	0.142 (0.136)
Iron heme	0.852*** (0.028)	0.411*** (0.137)	0.321 (0.248)	0.367* (0.217)	0.494*** (0.107)
Zinc	0.427*** (0.019)	-0.037 (0.114)	-0.234 (0.208)	-0.190 (0.249)	0.316** (0.151)
Calcium	0.780*** (0.022)	0.675*** (0.169)	0.823*** (0.292)	0.961*** (0.259)	0.991*** (0.195)
F-test instrument p value		87.21 0.0000	20.65 0.0000	3.08 0.0067	340.27 0.0000
Obs	5343	5343	5321	5343	5343

* significant at 10%, ** significant at 5%,, *** significant at 1%; Standard errors (in brackets) are clustered at the locality level; + for each household this is the locality average of all the other households in the same locality.

Table 5.6: Income elasticity of nutrients: flexible specification log inverse

Nutrient	p25		Median		p75	
	OLS	IV	OLS	IV	OLS	IV
Energy	0.432***	0.041	0.412***	-0.040	0.400***	-0.091
	(0.014)	(0.069)	(0.013)	(0.134)	(0.016)	(0.207)
Fiber	0.303***	-0.205	0.269***	0.018	0.249***	0.155
	(0.022)	(0.165)	(0.022)	(0.254)	(0.027)	(0.388)
Protein	0.487***	0.056	0.470***	0.293	0.459***	0.439
	(0.019)	(0.129)	(0.019)	(0.197)	(0.024)	(0.297)
Fat	0.551***	0.533***	0.494***	0.385*	0.459***	0.293
	(0.021)	(0.111)	(0.020)	(0.208)	(0.025)	(0.313)
Cholesterol	1.025***	1.137***	0.847***	0.647***	0.738***	0.345
	(0.043)	(0.215)	(0.026)	(0.210)	(0.035)	(0.365)
Saturated fat	0.708***	0.803***	0.637***	0.613***	0.593***	0.496
	(0.023)	(0.120)	(0.022)	(0.217)	(0.028)	(0.329)
Monounsaturated fat	0.600***	0.551***	0.528***	0.142	0.484***	-0.111
	(0.021)	(0.068)	(0.019)	(0.134)	(0.024)	(0.202)
Polyunsaturated fat	0.593***	0.786***	0.456***	0.342	0.372***	0.069
	(0.042)	(0.153)	(0.028)	(0.270)	(0.036)	(0.430)
Carbohydrates	0.342***	-0.050	0.297***	-0.122	0.270***	-0.167
	(0.018)	(0.097)	(0.017)	(0.174)	(0.021)	(0.261)
Vitamin A	1.389***	1.555***	1.162***	1.012***	1.023***	0.677*
	(0.046)	(0.187)	(0.031)	(0.251)	(0.041)	(0.397)
Vitamin C	1.228***	1.538***	1.060***	1.611***	0.957***	1.656***
	(0.041)	(0.163)	(0.033)	(0.258)	(0.042)	(0.395)
Folate	0.383***	-0.122	0.358***	0.466*	0.343***	0.829**
	(0.026)	(0.224)	(0.021)	(0.250)	(0.026)	(0.404)
Iron	0.315***	-0.217*	0.280***	-0.044	0.259***	0.062
	(0.021)	(0.128)	(0.021)	(0.214)	(0.025)	(0.331)
Iron heme	0.803***	-0.214	0.837***	0.631***	0.858***	1.151***
	(0.029)	(0.303)	(0.028)	(0.181)	(0.037)	(0.350)
Zinc	0.415***	-0.073	0.397***	0.134	0.386***	0.262
	(0.021)	(0.124)	(0.020)	(0.209)	(0.025)	(0.316)
Calcium	0.806***	0.642***	0.764***	0.859***	0.739***	0.992**
	(0.027)	(0.196)	(0.025)	(0.266)	(0.031)	(0.411)
F-test log PCE			21.56			
p-value			0.0000			
F-test (1/PCE)			21.02			
p-value			0.0000			
Obs			5343			

* significant at 10%, ** significant at 5%,, *** significant at 1%; Standard errors (in brackets) are clustered at the locality level; Instrument for ln PCE is ln locality median non food expenditure and its square.

Table 5.7: Income elasticity of nutrients: flexible specification log squared

Nutrient	p25		Median		p75	
	OLS	IV	OLS	IV	OLS	IV
Energy	0.435***	0.067	0.418***	-0.020	0.402***	-0.101
	(0.016)	(0.087)	(0.012)	(0.108)	(0.016)	(0.224)
Fiber	0.311***	-0.276	0.278***	-0.038	0.247***	0.185
	(0.025)	(0.204)	(0.021)	(0.207)	(0.030)	(0.418)
Protein	0.492***	-0.019	0.474***	0.234	0.458***	0.470
	(0.021)	(0.155)	(0.018)	(0.163)	(0.027)	(0.319)
Fat	0.569***	0.580***	0.508***	0.422**	0.450***	0.273
	(0.023)	(0.133)	(0.020)	(0.170)	(0.029)	(0.336)
Cholesterol	1.068***	1.265***	0.891***	0.765***	0.726***	0.297
	(0.049)	(0.273)	(0.027)	(0.166)	(0.036)	(0.388)
Saturated fat	0.732***	0.864***	0.654***	0.660***	0.581***	0.471
	(0.026)	(0.146)	(0.022)	(0.178)	(0.032)	(0.354)
Monounsaturated fat	0.623***	0.681***	0.546***	0.243**	0.473***	-0.166
	(0.023)	(0.080)	(0.018)	(0.109)	(0.027)	(0.217)
Polyunsaturated fat	0.631***	0.927***	0.491***	0.453**	0.359***	0.010
	(0.049)	(0.206)	(0.029)	(0.212)	(0.039)	(0.467)
Carbohydrates	0.354***	-0.027	0.309***	-0.104	0.266***	-0.177
	(0.020)	(0.115)	(0.016)	(0.143)	(0.023)	(0.281)
Vitamin A	1.447***	1.724***	1.220***	1.146***	1.009***	0.606
	(0.053)	(0.239)	(0.032)	(0.202)	(0.042)	(0.426)
Vitamin C	1.271***	1.515***	1.103***	1.593***	0.947***	1.666***
	(0.048)	(0.202)	(0.032)	(0.211)	(0.044)	(0.426)
Folate	0.390***	-0.309	0.364***	0.320	0.341***	0.907**
	(0.030)	(0.289)	(0.021)	(0.203)	(0.028)	(0.437)
Iron	0.322***	-0.273*	0.289***	-0.087	0.259***	0.085
	(0.023)	(0.161)	(0.020)	(0.173)	(0.028)	(0.356)
Iron heme	0.809***	-0.327	0.830***	0.447***	0.849***	1.171***
	(0.034)	(0.352)	(0.026)	(0.155)	(0.039)	(0.362)
Zinc	0.419***	-0.139	0.402***	0.082	0.387***	0.289
	(0.023)	(0.149)	(0.020)	(0.172)	(0.028)	(0.339)
Calcium	0.823***	0.574**	0.773***	0.805***	0.727***	1.021**
	(0.031)	(0.246)	(0.024)	(0.218)	(0.034)	(0.443)
F-test log PCE			21.56			
p-value			0.0000			
F-test (log PCE)^2			21.01			
p-value			0.0000			
Obs			5343			

* significant at 10%, ** significant at 5%,, *** significant at 1%; Standard errors (in brackets) are clustered at the locality level; Instrument for ln PCE is ln locality median non food expenditure and its square.

Table 5.8: Income elasticity of nutrients: income spline specification

Nutrient	OLS				IV approach			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
Energy	0,481***	0,368***	0,456***	0,459***	0,097	-0,054	0,045	0,074
	(0,035)	(0,075)	(0,076)	(0,047)	(0,063)	(0,097)	(0,097)	(0,081)
Fiber	0,385***	0,286**	0,256*	0,236**	-0,079	-0,202	-0,204	-0,196
	(0,051)	(0,124)	(0,146)	(0,099)	(0,147)	(0,189)	(0,179)	(0,185)
Protein	0,543***	0,432***	0,534***	0,478***	0,139	0,026	0,156	0,145
	(0,041)	(0,094)	(0,114)	(0,090)	(0,115)	(0,144)	(0,136)	(0,151)
Fat	0,652***	0,548***	0,553***	0,420***	0,603***	0,518***	0,536***	0,424***
	(0,049)	(0,104)	(0,145)	(0,104)	(0,116)	(0,159)	(0,165)	(0,156)
Cholesterol	1,322***	1,142***	0,550***	0,891***	1,247***	1,202***	0,627***	0,972***
	(0,118)	(0,199)	(0,163)	(0,094)	(0,180)	(0,246)	(0,231)	(0,174)
Saturated fat	0,838***	0,768***	0,720***	0,569***	0,899***	0,852***	0,798***	0,683***
	(0,056)	(0,115)	(0,146)	(0,111)	(0,126)	(0,173)	(0,172)	(0,167)
Monounsaturated fat	0,711***	0,632***	0,625***	0,486***	0,591***	0,528***	0,531***	0,440***
	(0,052)	(0,096)	(0,095)	(0,084)	(0,078)	(0,120)	(0,119)	(0,100)
Polyunsaturated fat	0,885***	0,649***	0,674***	0,228*	0,960***	0,831***	0,891***	0,420**
	(0,108)	(0,166)	(0,188)	(0,121)	(0,173)	(0,234)	(0,239)	(0,188)
Carbohydrates	0,426***	0,271***	0,312***	0,265***	0,038	-0,147	-0,086	-0,105
	(0,043)	(0,099)	(0,117)	(0,074)	(0,097)	(0,137)	(0,140)	(0,126)
Vitamin A	1,838***	1,670***	1,161***	1,151***	1,922***	1,793***	1,290***	1,347***
	(0,129)	(0,188)	(0,175)	(0,097)	(0,207)	(0,246)	(0,240)	(0,203)
Vitamin C	1,559***	1,305***	0,971***	0,986***	1,912***	1,659***	1,317***	1,358***
	(0,111)	(0,195)	(0,187)	(0,129)	(0,183)	(0,246)	(0,237)	(0,216)
Folate	0,475***	0,484***	0,464***	0,285***	0,069	0,065	0,069	-0,096
	(0,062)	(0,125)	(0,128)	(0,093)	(0,179)	(0,201)	(0,179)	(0,189)
Iron	0,400***	0,263**	0,320**	0,280***	-0,083	-0,257*	-0,188	-0,201
	(0,047)	(0,114)	(0,126)	(0,093)	(0,114)	(0,156)	(0,154)	(0,152)
Iron heme	0,653***	0,915***	0,789***	0,828***	0,240	0,478**	0,364*	0,467***
	(0,084)	(0,177)	(0,146)	(0,118)	(0,163)	(0,222)	(0,193)	(0,177)
Zinc	0,467***	0,371***	0,460***	0,399***	0,015	-0,103	0,008	-0,009
	(0,046)	(0,108)	(0,123)	(0,089)	(0,116)	(0,155)	(0,149)	(0,152)
Calcium	0,876***	1,114***	1,023***	0,614***	0,718***	0,993***	0,925***	0,556***
	(0,060)	(0,128)	(0,139)	(0,096)	(0,183)	(0,207)	(0,197)	(0,190)
F-test instrument						21.56		
p-value						0.0000		
Obs				5343				

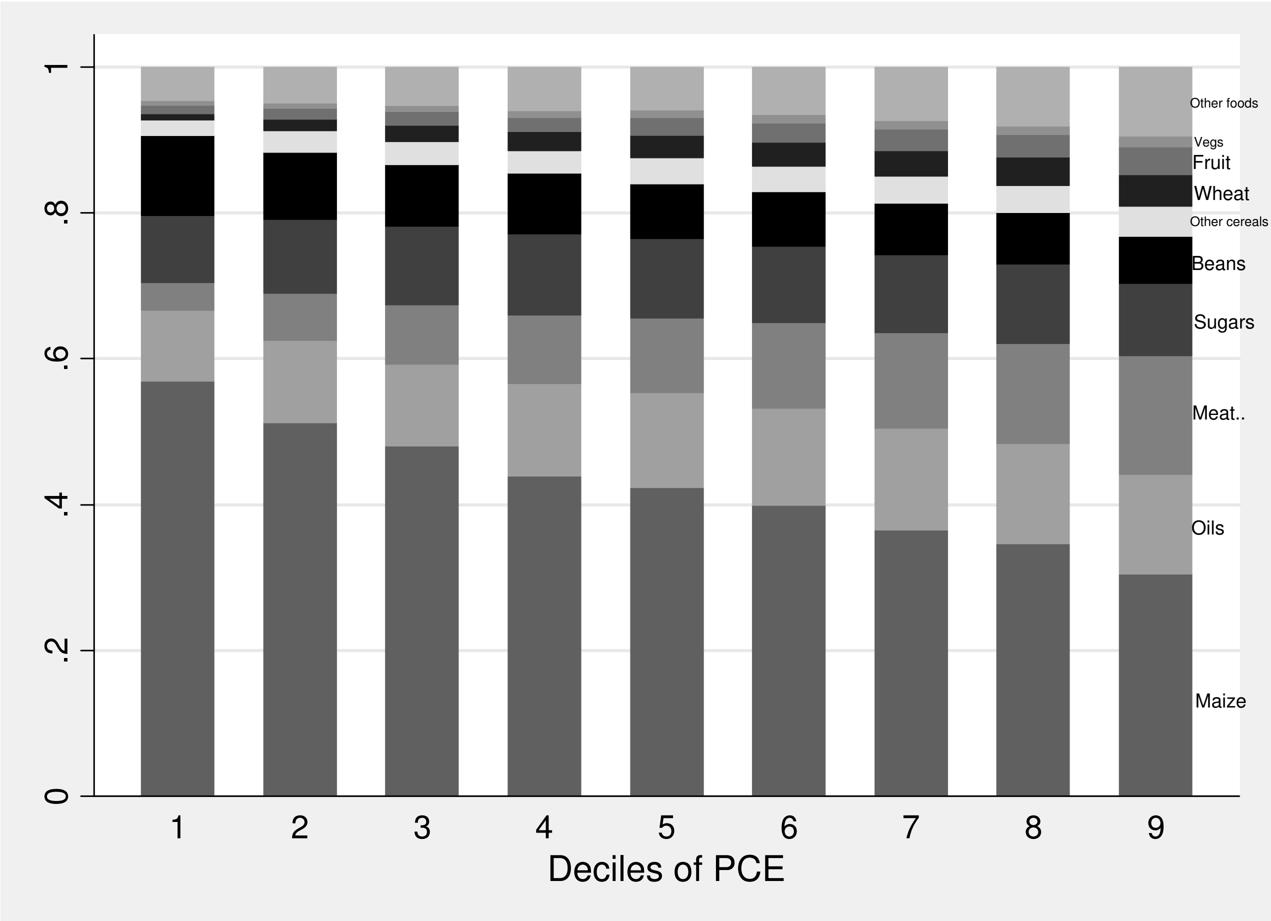
* significant at 10%, ** significant at 5%,, *** significant at 1%; Standard errors (in brackets) are clustered at the locality level; Instrument for ln PCE is ln locality median non food expenditure and its square.

Table 5.9: Income elasticity of nutrients: quantile regression

Nutrient	Linear approach					IV approach				
	p10	**p25**	**p50**	**p75**	**p90**	**p10**	**p25**	**p50**	**p75**	**p90**
Energy	0.413***	0.403***	0.371***	0.345***	0.321***	-0.015	0.007	-0.043	-0.114**	-0.216***
	(0.020)	(0.017)	(0.019)	(0.016)	(0.019)	(0.069)	(0.072)	(0.050)	(0.056)	(0.075)
Fiber	0.284***	0.233***	0.211***	0.208***	0.205***	-0.292**	-0.296***	-0.317***	-0.429***	-0.527***
	(0.030)	(0.023)	(0.022)	(0.024)	(0.027)	(0.116)	(0.085)	(0.086)	(0.088)	(0.119)
Protein	0.463***	0.456***	0.438***	0.433***	0.410***	0.112	0.059	-0.007	-0.054	-0.199**
	(0.022)	(0.018)	(0.017)	(0.018)	(0.021)	(0.082)	(0.059)	(0.051)	(0.066)	(0.084)
Fat	0.511***	0.507***	0.509***	0.510***	0.492***	0.409***	0,408***	0.397***	0.429***	0.453***
	(0.025)	(0.021)	(0.017)	(0.016)	(0.019)	(0.097)	(0.078)	(0.055)	(0.063)	(0.085)
Cholesterol	1.318***	0.941***	0.776***	0.713***	0.688***	1.234***	0.855***	0.716***	0.713***	0.657***
	(0.052)	(0.036)	(0.031)	(0.024)	(0.035)	(0.188)	(0.100)	(0.092)	(0.101)	(0.183)
Saturated fat	0.672***	0.675***	0.673***	0.672***	0.640***	0.718***	0.676***	0.689***	0.670***	0.716***
	(0.028)	(0.022)	(0.019)	(0.018)	(0.021)	(0.115)	(0.081)	(0.067)	(0.077)	(0.117)
Monounsaturated fat	0.571***	0.550***	0.540***	0.533***	0.512***	0.541***	0.484***	0.472***	0.492***	0.471***
	(0.032)	(0.022)	(0.018)	(0.018)	(0.020)	(0.111)	(0.069)	(0.059)	(0.066)	(0.088)
Polyunsaturated fat	0.735***	0.503***	0.432***	0.397***	0.419***	0.695***	0.453***	0.469***	0.435***	0.563***
	(0.073)	(0.042)	(0.024)	(0.022)	(0.029)	(0.258)	(0.174)	(0.083)	(0.087)	(0.130)
Carbohydrates	0.302***	0.291***	0.263***	0.248***	0.230***	-0.170*	-0.160**	-0.217***	-0.316***	-0.371***
	(0.023)	(0.022)	(0.020)	(0.022)	(0.021)	(0.087)	(0.071)	(0.060)	(0.065)	(0.096)
Vitamin A	1.695***	1.299***	1.159***	1.041***	0.939***	2.108***	1.504***	1.244***	1.119***	0.930***
	(0.082)	(0.050)	(0.031)	(0.030)	(0.032)	(0.268)	(0.143)	(0.131)	(0.103)	(0.144)
Vitamin C	1.186***	1.184***	1.129***	1.050***	0.945***	1.415***	1.236***	1.040***	0.844***	0.362*
	(0.057)	(0.045)	(0.035)	(0.036)	(0.040)	(0.198)	(0.155)	(0.126)	(0.156)	(0.214)
Folate	0.378***	0.334***	0.335***	0.331***	0.308***	0.069	0.013	-0.055	-0.141	-0.546***
	(0.038)	(0.026)	(0.020)	(0.024)	(0.031)	(0.143)	(0.100)	(0.096)	(0.131)	(0.156)
Iron	0.285***	0.248***	0.219***	0.203***	0.198***	-0.302***	-0.325***	-0.378***	-0.476***	-0.516***
	(0.028)	(0.027)	(0.026)	(0.026)	(0.026)	(0.102)	(0.083)	(0.073)	(0.089)	(0.125)
Iron heme	0.633***	0.758***	0.870***	0.876***	0.846***	0.305*	0.473***	0.652***	0.658***	0.615***
	(0.051)	(0.041)	(0.032)	(0.033)	(0.037)	(0.166)	(0.125)	(0.112)	(0.135)	(0.192)
Zinc	0.441***	0.378***	0.338***	0.316***	0.287***	0.003	-0.102	-0.184***	-0.295***	-0.430***
	(0.025)	(0.023)	(0.022)	(0.021)	(0.024)	(0.090)	(0.068)	(0.064)	(0.079)	(0.112)
Calcium	0.869***	0.929***	0.835***	0.735***	0.662***	1.073***	1.078***	0.931***	0.736***	0.598***
	(0.046)	(0.035)	(0.034)	(0.034)	(0.034)	(0.169)	(0.147)	(0.132)	(0.156)	(0.207)
F-test instruments						77.77	252.90	315.04	221.09	187.00
p-vslue						0.0000	0.0000	0.0000	0.0000	0.0000
Obs	5352									

* significant at 10%, ** significant at 5%,, *** significant at 1%; Standard errors (in brackets) are clustered at the locality level; Instrument for ln PCE is ln locality median non food expenditure and its square.

Figure 5.1: Proportion of calories from food groups and per capita expenditure



Foods included in the groups: Maize (maize tortilla, maize in grains, maize flour); Oil (vegetable oil); Meat, Fish and Dairy (chicken, beef, pork, goat, seafood, sardines, tuna, eggs, milk, yogurt, cheeses, lard, cold meats); Sugars (sugar); Beans (kidney beans); Other cereals (pasta soup, biscuits, breakfast cereals); Wheat (white bread, sweet bread, loaf of bread, wheat flour, wheat tortilla); Fruits (guava, mandarins, papaya, oranges, bananas, apples, lemons, watermelon); Vegetables (tomatoes, onions, potatoes, carrots, leafy vegetables, pumpkin, chayote, chili, edible cactus); Other foods (rice, sweets, carbonated beverages, coffee)

Figure 5.2: Semiparametric estimation of the relationship between log daily per capita nutrient and log per capita expenditure

The horizontal line in the right panel is the mean slope below the median; all values in logarithmic scale

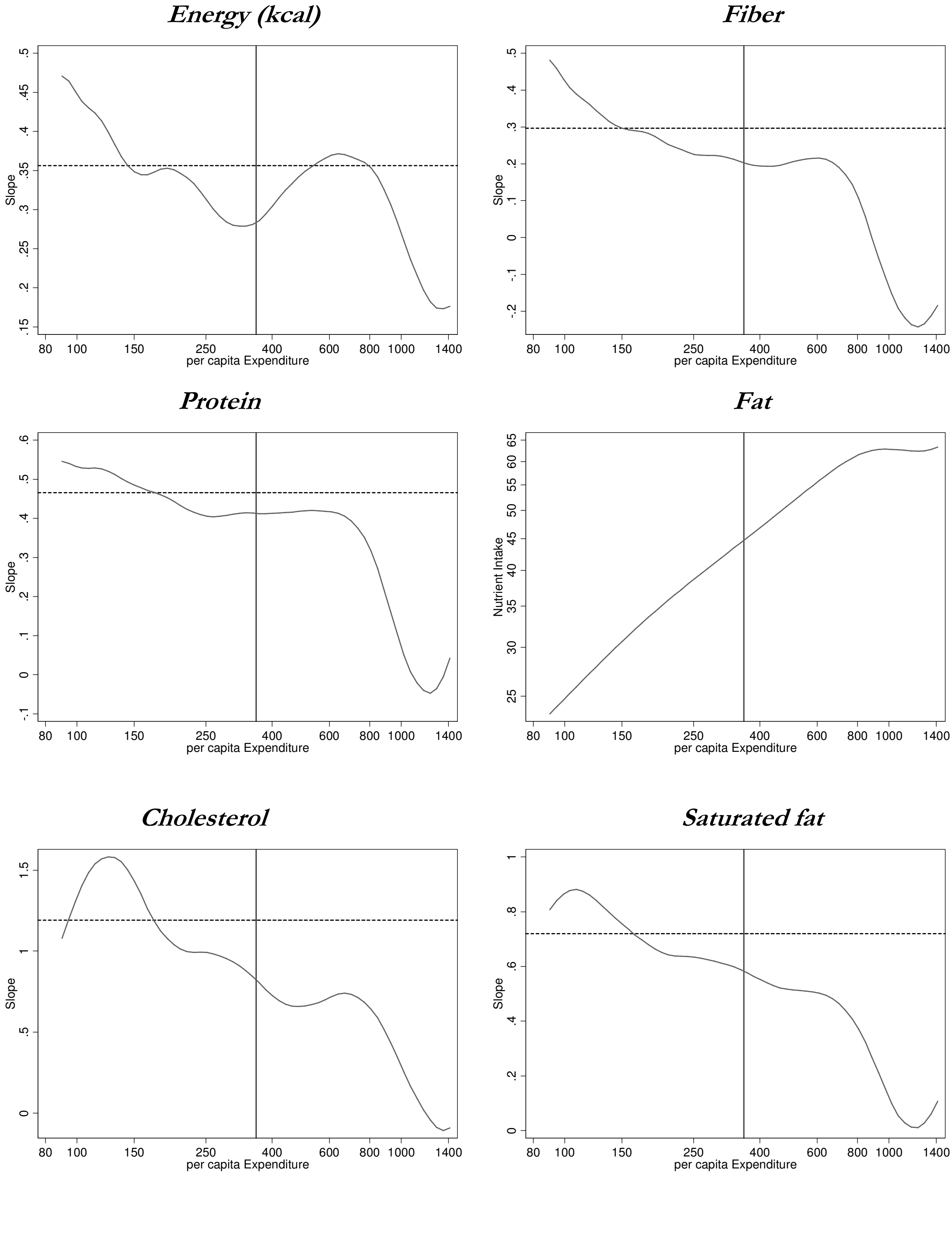


Figure 5.3: Semiparametric estimation of the relationship between log daily per capita nutrient and log per capita expenditure: continued

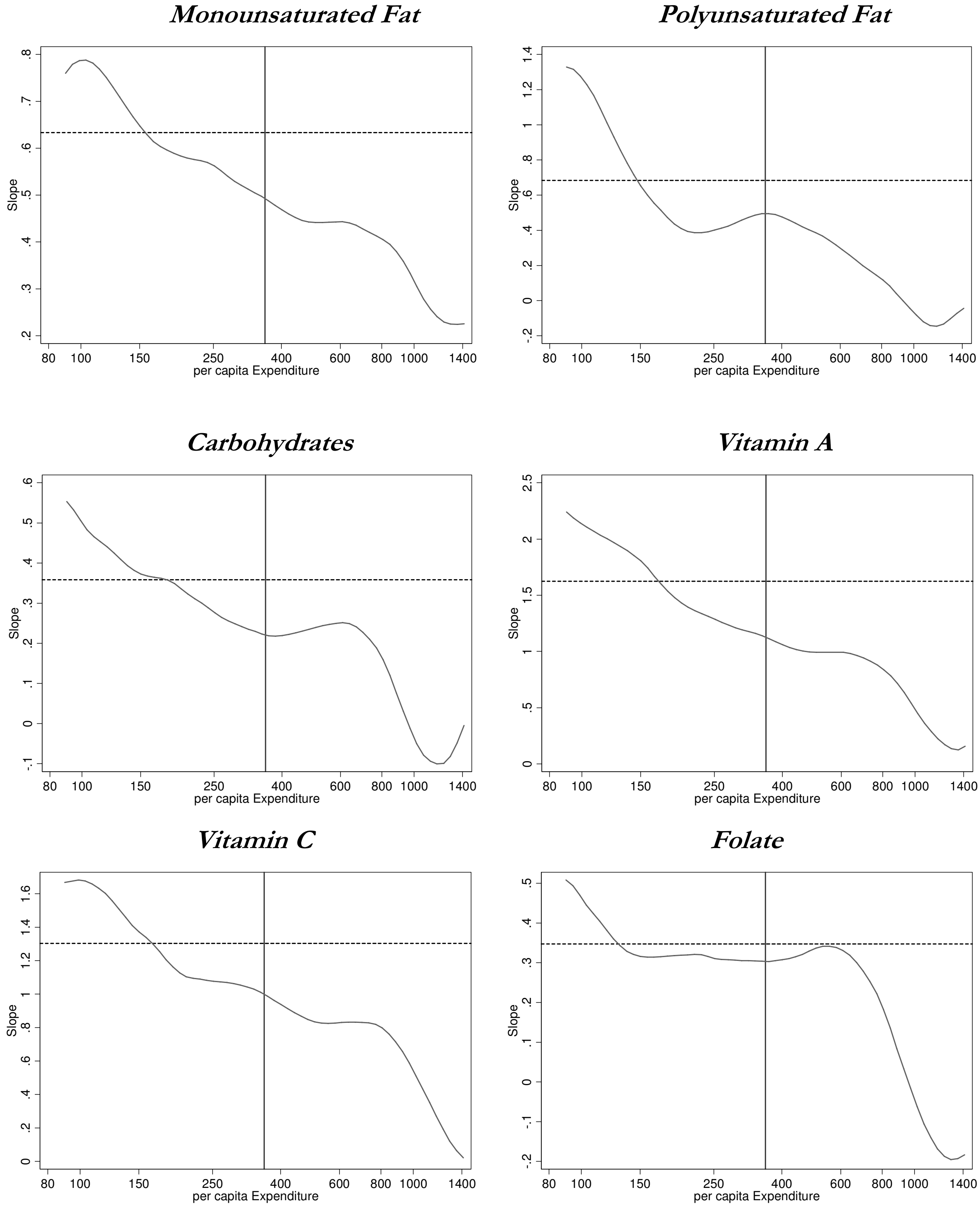
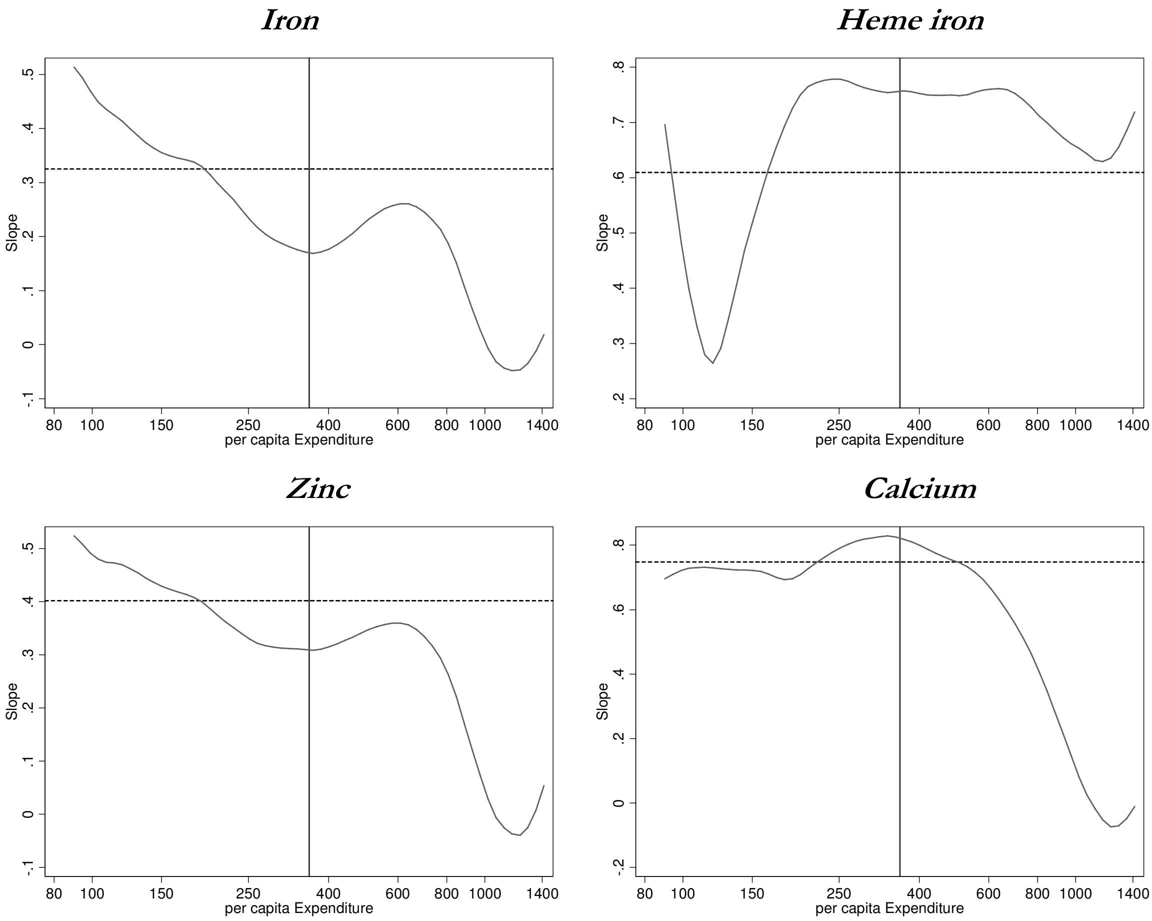


Figure 5.4: Semiparametric estimation of the relationship between log daily per capita nutrient and log per capita expenditure:continued



Conclusions

Despite a strong theoretical case supporting investments in human capital and the evidence on positive return to these investments, investments in human capital are still dramatically low in developing countries. In addition, the debate about the best timing of these investments is still not closed. A well established theoretical case supports investments at early stages of one's life. However, it is still an open question whether this case is confirmed empirically in developing contexts and which type of intervention is the most effective. These are questions which have to be answered empirically.

This thesis presents evidence of the impact of different types of policy interventions on demand for human capital in Latin America, specifically rural Mexico and Colombia. Most of the results are based on the evaluation of *Oportunidades*, which is a conditional cash transfer (CCT) program targeting poor households in rural Mexico. This type of program has proved successful in breaking the intergenerational transmission of poverty and has been extensively evaluated given its unique experimental evaluation design.

In particular, in chapter 1 we focus on the unintended consequence (spillover effects) of the *Oportunidades* program. This chapter offers interesting contributions on two dimensions: first, we show that the program has an indirect effect on cervical cancer screening rates of women who were not eligible for the program but lived in areas where the program was in operation. These effects – health externalities – can dramatically change the assessment of the impacts of a program as well as considerations about its design. The other contribution of chapter 1 is about underlying mechanisms through which programs work. In general, study of spillover effects naturally leads to attempts to unveil program channels as the researcher typically might have only a limited idea *a priori* of how the program works in terms of its unintended effects. Here, we show evidence of the mechanism through which the program operates being the weakening of the social norm of husbands' opposition to their spouses being screened by male doctors.

In Chapter 2 we show that *Oportunidades* is bringing families out of poverty, which is considered here as a necessary condition to allow them to invest in human capital. In addition to this, we discuss why CCT programs can have perverse incentives on the labor supply of eligible individuals and show that the program is not having this effect.

In chapter 3 and 4 we contribute to the evidence on the impact of ECD interventions. In chapter 3 we discuss how conditional cash transfers can increase the caloric intake of very young children and young mothers. This chapter also has some methodological content, in that it shows how to apply a technique for estimating individual caloric intake when only household aggregate data is available to a program evaluation setting. Result show that *Oportunidades* is successful at increasing the caloric intake of young children and young mothers, while it does not seem to have an effect at other age ranges.

Chapter 4 focuses on the evaluation of the impact of a preschool nursery program in Colombia: *Hogares Comunitarios.* When compared to a CCT program, this program can be thought as a direct attack to children development, as participants (kids age 0 to 6) in the *Hogares Comunitarios* receive daycare services and food at the house of a community mother. Our evidence shows that this program can have a positive and sizeable effect on child growth, with this result being robust to different instruments for participation into the program and different samples.

In chapter 5 we deal with the long-standing debate about in-kind transfers vs. cash transfers and with how this relates to child nutrition. In particular, we study how nutrient intake responds to changes in income in a sample of rural Mexican households. This increase in income can be thought as an unconditional cash transfer to households. Our evidence is mixed: while consumption of some key nutrients (vitamins A and C, heme iron, calcium and fats) responds positively to an increase in income, other nutrients (energy, zinc and protein) seem not to be affected by a change in income, with this supporting the case for conditionalities and/or in-kind transfers.

While this thesis focuses almost exclusively on interventions which try to affect the demand for human capital, several results suggest solutions which have to be sought on the supply side. For instance, the evidence on the social norm in *Oportunidades* localities seem to make a case for supplying more female doctors in rural health centers. Other results point to the quality of the supply: if availability of more resources does not necessarily translate into consumption of better nutrients, one policy option is to give a more specific training to health staff on how to advice rural households on consumption of more nutritious foods.

These issues are consistent with the literature on CCTs which shows that CCTs have been very successful at increasing demand for education and health services, but quality of services offered did not necessarily improve. Another open question in the literature is about long-term impacts of CCTs, being the evidence so far almost exclusively on short-term impacts (see, among others, Fizsbein and Schady, 2009). A related issue is that all the results in this thesis are about reduced form policy impacts, hence missing the interconnections with local markets in which individuals and households whose demand behavior is changing live. Interestingly, Levy (2009) makes the point that CCTs in Mexico can only have a lasting effect on the generation of children intervened if they are accompanied with labor market and fiscal reforms.

To continue the work developed in this thesis, we plan to focus on some these issues above in our future research. In particular, we plan to deal with sustainability of the impact of CCT programs. We are currently involved in a research project to study the long-term impact of *Red de Proteccion Social*, a CCT in rural Nicaragua. Among other things, this project will allow us to assess the impact of a typical CCT program on young adults who started to receive the programs 10 year earlier (and who were exposed to it for only 3 years). In addition, we are going to be able to differentiate the impact between kids who received the intervention when they were age 0-3 vs. kids who received it at age 4-6.

Another topic we plan to study refers to the role of information in these types of interventions.

This continues to be a black box of the evaluation of CCTs. Even with the *Oportunidades* experimental design it has not been possible to disentangle the effect of information from other program's effects.[22]

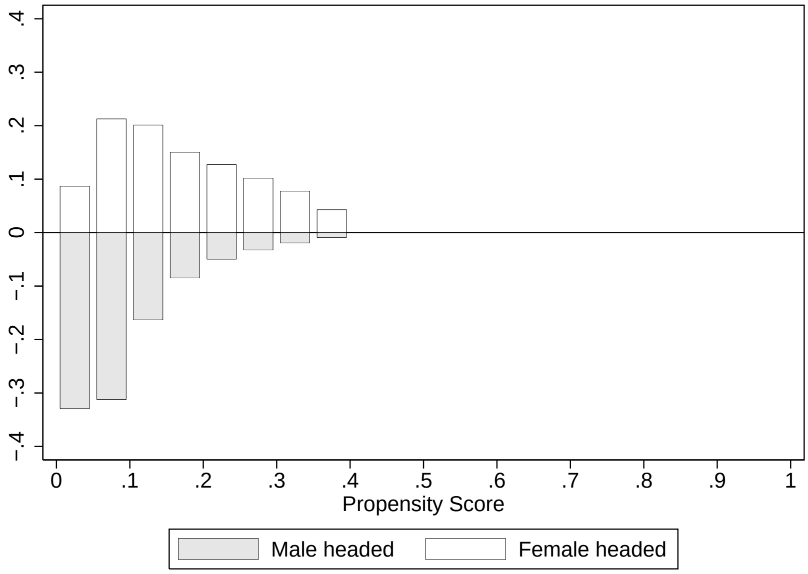
Finally, we plan to address the issues of which conditions allow investment in human capital to be productive in the long run. CCTs, and other similar interventions, are creating a generation of young adults which are substantially more productive in the labor market (thanks to investment in health and education), however this increased productivity might not have any value if it is not accompanied by incentives to create and accept higher productivity jobs.

[22]One of the component of the program is to deliver talks (*platicas*) about nutritional and health practices to which the recipient of the transfer are required to attend as a conditionality of the program. However, both beneficiaries and non-beneficiaries are free to attend the talks.

Appendix

Appendix to Chapter 1: Additional Figures and Tables

Figure A1: Appendix: Propensity scores for Female and Male headed households



Note:
The propensity score is based on the following observable characteristics - age of household head, household composition ratios (male and females for age groups 0-4, 5-10, 11-14, 15-19, 20-34, 35-54 and over 55), household poverty index and its square, distance from a SSA provider, the percentage of eligibles in the village, variables for sex related behaviour of female respondents (number of pregnancies, dummy variables for whether they ever used contraception methods and whether they ever had a PAP test), female status index, dummy variables for whether the household head is working, is literate, speaks the indigenous language, whether the household owns land, whether there is an electricity supply, and state fixed effects. The variables are elicited either in October 1997 or March 1998

Table A1: Household composition and the demand for health, DD

Marginal effects from probit estimations, standard errors are clustered by village

Non-eligibles with no women over 45

	Cerv. Cancer screening	Blood Sugar screening	Blood Pressure screening	Health Center visit
98o	0.129***	0.221***	0.203***	-0.042
	(0.014)	(0.013)	(0.012)	(0.011)
T	-0.008	0.010	0.000	0.008
	(0.019)	(0.017)	(0.016)	(0.013)
T*98o	0.054**	-0.028	0.003	-0.021
	(0.025)	(0.024)	(0.023)	(0.023)
Observations	8343	8481	8557	8893

Non-eligibles with at least one woman over 45

	Cerv. Cancer screening	Blood Sugar screening	Blood Pressure screening	Health Center visit
98o	0.124***	0.209***	0.183***	-0.057***
	(0.014)	(0.012)	(0.012)	(0.011)
T	0.009	0.043	0.051**	0.007
	(0.016)	(0.017)	(0.016)	(0.013)
T*98o	0.051**	0.019	0.009	0.004
	(0.026)	(0.023)	(0.023)	(0.022)
Observations	9272	10575	10647	11105

Note: *** denotes significance at 1%, ** at 5% and * at 10%. The amounts are expressed in *pesos* at October 1997 values. Marginal effects are calculated as average partial effects. Standard errors are derived using the delta method. All specifications also control for the variables included in the regressions presented in table 1.4.

Appendix to Chapter 4: Additional Tables

Table A2: PROGRESA and the demand for health, DD for non-eligibles

Marginal effects from probit estimations, standard errors are clustered by village

	Cerv. Cancer screening	Blood Sugar screening	Blood Pressure screening	Health Center visit
98o	0.125***	0.203***	0.180***	-0.051***
	(0.012)	(0.011)	(0.010)	(0.010)
T	-0.011**	0.022	0.023	0.003
	(0.016)	(0.015)	(0.014)	(0.011)
T*98o	0.053**	0.001	0.011	0.000
	(0.023)	(0.020)	(0.020)	(0.020)
log visit fee	-0.010	-0.033***	-0.033***	-0.020***
	(0.008)	(0.008)	(0.007)	(0.006)
SSA clinic	0.035	0.023	0.012	-0.004
	(0.025)	(0.022)	(0.022)	(0.014)
IMSS Solid. clinic	0.002	0.093**	0.075**	0.069**
	(0.045)	(0.041)	(0.037)	(0.034)
IMSS clinic	-0.145***	-0.063	-0.103***	-0.005
	(0.034)	(0.042)	(0.042)	(0.019)
Private doctor	-0.059	-0.015	-0.069***	0.009
	(0.040)	(0.024)	(0.025)	(0.016)
Medical aid	0.038**	0.026*	0.043***	0.004
	(0.016)	(0.016)	(0.015)	(0.011)
Mobile unit	0.012	0.016	0.000	-0.011
	(0.018)	(0.016)	(0.016)	(0.013)
Observations	16042	17389	17530	18270

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Marginal effects are calculated as average partial effects. Standard errors are derived using the delta method. Log visit fee is the log of the village average consultation fee paid by non-eligibles.

Table A3: Learning in cervical cancer: Effect of initial information

Marginal effects from ivprobit estimations,
bootstrapped standard errors are clustered by village

	Full Sample	Male Head	Female Head
Peer group screening rate	0.463***	0.492***	0.005
	(0.124)	(0.123)	(0.417)
No contraception	-0.112***	-0.118***	-0.050
	(0.013)	(0.012)	(0.046)
Peer group screening rate* no contraception	0.205**	0.170*	0.474*
	(0.094)	(0.099)	(0.288)
Observations	9061	8258	803
Cragg Donald Test	53.711	48.485	4.232
Cragg Donald χ^2	215.558	194.623	17.563

Note: *** denotes significance at 1%, ** at 5% and * at 10%. The peer group is defined by all the households in the village whose eldest children's age is within a 2 year difference range. The level of initial information is proxied by the dummy for having never used any method of contraception, as recorded in March 1998. The IV strategy exploits the the treatment effect, the average number of payments received by the eligibles in the peer group and their interaction as the instrument. The IV probit is calculated using a two stage procedure based on the control function approach. Marginal effects are calculated as average partial effects. Standard errors are calculated with 200 bootstrap repetitions clustered at village level. The Cragg Donald test for the validity of the rank condition is reported. All specifications also control for the variables included in the regressions presented in table 1.4.

Table A4: First Stage Regressions

First Stage Regressions

VARIABLES	1 **Linear. Exposure**	2 **Linear. Attendance**	3 **Non Linear. Exposure**	4 **Non Linear. Attendance**	5 **Linear. Exposure**	6 **Linear. Attendance**	7 **Non Linear. Exposure**	8 **Non Linear. Attendance**
	FeA	FeA	FeA	FeA	ENDS	ENDS	ENDS	ENDS
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]
capacity2	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]
hc_fee	-0.0144***	-0.00835	-0.0234**	-0.0235				
	[0.00524]	[0.00905]	[0.0118]	[0.0396]				
hc_fee2	0.000423	-2.20E-05	0.000479	-0.000754				
	[0.000304]	[0.000527]	[0.000696]	[0.00231]				
time_hc	-0.154***	-0.328***	-0.403***	-1.449***				
	[0.0338]	[0.0704]	[0.0911]	[0.426]				
time_hc2	0.0729***	0.133***	0.127*	0.304				
	[0.0170]	[0.0392]	[0.0660]	[0.345]				
time_hc_b	-0.143***	-0.165**	-0.338***	-0.747*				
	[0.0438]	[0.0783]	[0.110]	[0.395]				
time_hc_b2	0.0556**	0.0719	0.0675	0.262				
	[0.0240]	[0.0434]	[0.0671]	[0.250]				
female	4.40E-05	0.00948	0.00065	0.0252	-0.000487	-0.0113	-0.00823	-0.0379
	[0.00657]	[0.00983]	[0.0119]	[0.0375]	[0.00424]	[0.00967]	[0.0126]	[0.0333]
age_m	0.00881***	0.0233***	0.0287***	0.110***	0.00633***	0.0224***	0.0394***	0.111***
	[0.000927]	[0.00200]	[0.00149]	[0.00691]	[0.000520]	[0.00125]	[0.00177]	[0.00547]
age_m2	-0.00699***	-0.0273***	-0.0235***	-0.127***	-5.07e-05***	-0.000200***	-0.000371***	-0.00106***
	[0.000873]	[0.00228]	[0.00149]	[0.00788]	[5.96e-06]	[1.57e-05]	[2.05e-05]	[6.37e-05]
ln_age_h	-0.0313	-0.0244	-0.0705*	-0.0998	-0.0268***	-0.0827***	-0.0956***	-0.306***
	[0.0196]	[0.0301]	[0.0374]	[0.120]	[0.00710]	[0.0175]	[0.0219]	[0.0626]
ln_age_m	-0.0139	-0.0384	-0.0404	-0.128	-0.0280**	-0.0681**	-0.0935**	-0.259**
	[0.0215]	[0.0377]	[0.0415]	[0.152]	[0.0129]	[0.0325]	[0.0383]	[0.122]
height_mot	-0.0692	-0.0458	-0.139	-0.289	-0.029	0.0363	-0.045	0.071
	[0.0718]	[0.119]	[0.138]	[0.510]	[0.0376]	[0.0898]	[0.116]	[0.315]
ln_order	0.0247**	0.0186	0.0336*	0.076	0.0102**	0.0128	0.0229	0.0414
	[0.0105]	[0.0140]	[0.0189]	[0.0590]	[0.00491]	[0.0126]	[0.0143]	[0.0442]
edu_m_b	0.0141	-0.0034	0.0212	-0.0232	-0.0165	0.01	-0.017	0.037
	[0.0115]	[0.0165]	[0.0211]	[0.0677]	[0.0183]	[0.0456]	[0.0389]	[0.146]
edu_m_c	0.0159	-0.0466	0.0151	-0.224*	-0.01	0.0314	0.0105	0.114
	[0.0204]	[0.0333]	[0.0391]	[0.136]	[0.0184]	[0.0478]	[0.0396]	[0.155]
edu_h_b	0.0119	0.0212	0.0259	0.0888	-0.00529	-0.0145	-0.0176	-0.0544
	[0.0121]	[0.0163]	[0.0207]	[0.0618]	[0.00813]	[0.0166]	[0.0233]	[0.0583]
edu_h_c	-0.0332	-0.0256	-0.0669	-0.0834	-0.00159	0.00554	-0.00282	0.00584
	[0.0289]	[0.0444]	[0.0483]	[0.171]	[0.0101]	[0.0212]	[0.0282]	[0.0743]
centretown	0.0162	0.0209	0.0348	0.0696				
	[0.0162]	[0.0208]	[0.0316]	[0.0841]				
time_hea	-0.0129	0.0389	-0.0406	0.0112				
	[0.0353]	[0.0399]	[0.0620]	[0.191]				
time_hea2	0.0194**	0.0139	0.0215*	0.0326				
	[0.00749]	[0.0157]	[0.0122]	[0.0527]				
time_sch	-0.0724	-0.142	0.114	0.169				
	[0.0574]	[0.113]	[0.134]	[0.490]				
time_sch2	0.129*	0.225	0.0026	-0.184				
	[0.0689]	[0.169]	[0.171]	[0.672]				
time_hea_sch	0.0323	0.0658	0.217	1.462***				
	[0.0434]	[0.0752]	[0.144]	[0.543]				
time_alc	0.0398	0.0173	0.0886	0.135				
	[0.0431]	[0.0490]	[0.0614]	[0.199]				
time_alc2	0.00828	0.0201*	0.00831	0.0920**				
	[0.00773]	[0.0112]	[0.0107]	[0.0386]				
timealchea	-0.0264*	-0.0353*	-0.0239	-0.110**				
	[0.0144]	[0.0188]	[0.0187]	[0.0510]				
timealcsch	-0.0911	-0.142	-0.407**	-1.738***				
	[0.0579]	[0.105]	[0.164]	[0.608]				
time_sch_mun	0.417***	0.282	0.766***	1.164				
	[0.106]	[0.209]	[0.206]	[0.919]				
time_hea_mun	-0.0627**	-0.0801	-0.0818	-0.247				
	[0.0258]	[0.0502]	[0.0523]	[0.208]				
time_alc_mun	-0.0176	-0.0214	-0.0265	-0.0781				
	[0.0285]	[0.0513]	[0.0507]	[0.204]				
hosp	0.00585	0.011	0.0108	0.0284				

Table A5: First Stage Regressions: continued

	[0.0117]	[0.0225]	[0.0220]	[0.0854]				
pipe	-0.0321	-0.0638	-0.0347	-0.246	0.0156	0.00267	0.0173	-0.0279
	[0.0511]	[0.0897]	[0.109]	[0.386]	[0.0176]	[0.0497]	[0.0399]	[0.167]
sewage	0.0172	0.00491	0.0854*	0.109	-0.0982***	-0.199***	-0.237***	-0.595***
	[0.0212]	[0.0339]	[0.0513]	[0.148]	[0.0262]	[0.0678]	[0.0551]	[0.226]
insur_mun	0.164	0.335	0.528**	1.417				
	[0.123]	[0.203]	[0.246]	[0.932]				
insur_mun2	-0.0965	-0.22	-0.337	-0.883				
	[0.108]	[0.182]	[0.226]	[0.813]				
numchildren	0.0221	-0.0267	-0.00175	-0.125	-0.00101	-0.00723**	-0.00461	-0.0238**
	[0.0180]	[0.0357]	[0.0325]	[0.129]	[0.00125]	[0.00339]	[0.00413]	[0.0117]
altitud	-0.0463	-0.0448	-0.113*	-0.161	0.0208	0.0345	0.0496	0.0788
	[0.0294]	[0.0566]	[0.0671]	[0.265]	[0.0248]	[0.0602]	[0.0579]	[0.217]
altitud2	0.0133	0.0185	0.0306	0.0677	0.00101	0.00383	0.00585	0.0203
	[0.0105]	[0.0218]	[0.0254]	[0.102]	[0.0111]	[0.0245]	[0.0235]	[0.0884]
wage_fu	-0.0025	0.00282	-0.00629	-0.0143				
	[0.0306]	[0.0461]	[0.0687]	[0.201]				
wage_fr	-0.0186	-0.0629	-0.0534	-0.229				
	[0.0274]	[0.0418]	[0.0584]	[0.176]				
price_index	0.0804	0.311*	0.386**	1.558**				
	[0.0698]	[0.155]	[0.191]	[0.678]				
sisben2					-0.00561	-0.0147	-0.0223	-0.0469
					[0.00841]	[0.0202]	[0.0218]	[0.0705]
sisben3					-0.0181*	-0.0530**	-0.0660***	-0.187**
					[0.00923]	[0.0223]	[0.0249]	[0.0803]
Observations	5719	5719	5719	5719	6170	6179	6170	6179
R-squared	0.256	0.204			0.169	0.221		

Robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

Table A6: Difference in Distance

Difference in distance to the nearest HC according to whether or not the household moved

	Without additional covariates	With distance to other facilities as covariates
Moved (1 if household moved address, 0 otherwise)	-2.2	0.453
	(1.86)	(1.511)

Sample size is 3095. Standard errors shown in parenthesis are clustered at the town level.

Table A7: Effect on Child's Height Linear IV FeA

Effect of HC Participation on Child's Height Using Linear IV - FeA

	1	2	3	4	5	6	7	8	9	10
VARIABLES	OLS	IV: All Instruments	IV: Capacity	IV: Fee	IV: Distance	OLS	IV: All Instruments	IV: Capacity	IV: Fee	IV: Distance
exposure	-0.00439	0.997***	1.002*	0.722	1.001					
	[0.0895]	[0.349]	[0.528]	[0.588]	[0.619]					
asis_hc						-0.0913**	0.611**	0.709	0.621	0.533
						[0.0443]	[0.251]	[0.445]	[0.486]	[0.361]
female	0.146***	0.146***	0.146***	0.146***	0.146***	0.147***	0.141***	0.140***	0.141***	0.141***
	[0.0342]	[0.0320]	[0.0324]	[0.0328]	[0.0324]	[0.0347]	[0.0321]	[0.0320]	[0.0314]	[0.0332]
age_m	-0.0284***	-0.0373***	-0.0373***	-0.0349***	-0.0373***	-0.0263***	-0.0428***	-0.0451***	-0.0430***	-0.0410***
	[0.00330]	[0.00436]	[0.00522]	[0.00581]	[0.00667]	[0.00334]	[0.00669]	[0.0102]	[0.0113]	[0.00947]
age_m2	0.0249***	0.0319***	0.0319***	0.0300***	0.0319***	0.0225***	0.0416***	0.0443***	0.0419***	0.0395***
	[0.00312]	[0.00381]	[0.00441]	[0.00484]	[0.00551]	[0.00325]	[0.00742]	[0.0118]	[0.0130]	[0.0107]
ln_age_h	0.264***	0.293***	0.293***	0.285***	0.293***	0.262***	0.278***	0.281***	0.279***	0.277***
	[0.0799]	[0.0868]	[0.0909]	[0.0874]	[0.0869]	[0.0797]	[0.0850]	[0.0894]	[0.0871]	[0.0845]
ln_age_m	0.327***	0.344***	0.344***	0.339***	0.344***	0.323***	0.352***	0.356***	0.352***	0.349***
	[0.115]	[0.124]	[0.127]	[0.124]	[0.125]	[0.115]	[0.119]	[0.126]	[0.124]	[0.120]
height_mot	5.555***	5.645***	5.646***	5.620***	5.646***	5.548***	5.604***	5.612***	5.605***	5.598***
	[0.453]	[0.445]	[0.459]	[0.448]	[0.450]	[0.452]	[0.468]	[0.486]	[0.471]	[0.471]
ln_order	-0.277***	-0.306***	-0.306***	-0.298***	-0.306***	-0.275***	-0.293***	-0.295***	-0.293***	-0.291***
	[0.0370]	[0.0422]	[0.0455]	[0.0393]	[0.0452]	[0.0369]	[0.0406]	[0.0429]	[0.0395]	[0.0415]
edu_m_b	0.123**	0.109**	0.109**	0.113**	0.109*	0.123**	0.124**	0.124**	0.124**	0.124**
	[0.0538]	[0.0532]	[0.0535]	[0.0530]	[0.0554]	[0.0540]	[0.0526]	[0.0534]	[0.0533]	[0.0533]
edu_m_c	0.232***	0.217***	0.217***	0.221***	0.217***	0.228***	0.257***	0.261***	0.258***	0.254***
	[0.0752]	[0.0755]	[0.0772]	[0.0754]	[0.0771]	[0.0750]	[0.0768]	[0.0789]	[0.0811]	[0.0780]
edu_h_b	0.0834	0.07	0.0699	0.0737	0.07	0.0855	0.0689	0.0665	0.0686	0.0707
	[0.0582]	[0.0537]	[0.0545]	[0.0555]	[0.0548]	[0.0583]	[0.0572]	[0.0586]	[0.0593]	[0.0581]
edu_h_c	0.218**	0.246**	0.246**	0.239**	0.246**	0.217**	0.231**	0.233**	0.231**	0.230**
	[0.0913]	[0.103]	[0.102]	[0.102]	[0.107]	[0.0904]	[0.0993]	[0.102]	[0.102]	[0.1000]
centretown	0.0137	-0.0295	-0.0297	-0.0176	-0.0297	0.0193	-0.0252	-0.0314	-0.0258	-0.0203
	[0.0641]	[0.0668]	[0.0729]	[0.0739]	[0.0658]	[0.0630]	[0.0670]	[0.0769]	[0.0807]	[0.0653]
time_hea	0.0812	0.14	0.141	0.124	0.141	0.0771	0.111	0.115	0.111	0.107
	[0.105]	[0.0962]	[0.0999]	[0.109]	[0.0997]	[0.106]	[0.0977]	[0.100]	[0.107]	[0.0994]
time_hea2	0.0436	0.0218	0.0216	0.0278	0.0217	0.0456	0.0295	0.0272	0.0292	0.0313
	[0.0396]	[0.0388]	[0.0429]	[0.0408]	[0.0385]	[0.0394]	[0.0412]	[0.0453]	[0.0431]	[0.0407]
time_sch	-0.201	-0.0302	-0.0293	-0.0772	-0.0294	-0.227	-0.0215	0.00733	-0.0188	-0.0443
	[0.244]	[0.245]	[0.232]	[0.253]	[0.284]	[0.244]	[0.262]	[0.256]	[0.271]	[0.289]
time_sch2	0.508	0.323	0.322	0.374	0.322	0.535*	0.319	0.289	0.317	0.343
	[0.306]	[0.314]	[0.302]	[0.312]	[0.354]	[0.305]	[0.343]	[0.337]	[0.343]	[0.369]
time_hea_sch	0.217	0.0514	0.0505	0.097	0.0506	0.244	0.0322	0.00252	0.0294	0.0557
	[0.236]	[0.265]	[0.257]	[0.274]	[0.295]	[0.231]	[0.273]	[0.271]	[0.309]	[0.296]
time_alc	-0.183*	-0.238**	-0.239**	-0.223**	-0.239**	-0.179*	-0.210**	-0.214**	-0.210*	-0.206**
	[0.0967]	[0.0973]	[0.102]	[0.111]	[0.0969]	[0.0968]	[0.0967]	[0.102]	[0.110]	[0.0952]
time_alc2	0.0790**	0.0668**	0.0667**	0.0701**	0.0667**	0.0815***	0.0617*	0.0590*	0.0615**	0.0639*
	[0.0296]	[0.0303]	[0.0300]	[0.0288]	[0.0329]	[0.0298]	[0.0320]	[0.0316]	[0.0299]	[0.0349]
timealchea	-0.146***	-0.121***	-0.121***	-0.128***	-0.121***	-0.149***	-0.123***	-0.119**	-0.122***	-0.126***
	[0.0349]	[0.0384]	[0.0414]	[0.0373]	[0.0405]	[0.0346]	[0.0400]	[0.0447]	[0.0411]	[0.0411]
timealcsch	-0.0596	0.175	0.177	0.111	0.177	-0.0951	0.186	0.225	0.189	0.154

Table A8: Effect on Child's Height Linear IV FeA: continued

	[0.303]	[0.345]	[0.336]	[0.352]	[0.389]	[0.293]	[0.367]	[0.371]	[0.410]	[0.396]
time_sch_mun	-1.844***	-1.868***	-1.868***	-1.861***	-1.868***	-1.859***	-1.750***	-1.735***	-1.749***	-1.762***
	[0.599]	[0.541]	[0.547]	[0.557]	[0.549]	[0.605]	[0.566]	[0.579]	[0.588]	[0.572]
time_hea_mun	-0.197	-0.13	-0.13	-0.149	-0.13	-0.203	-0.156	-0.15	-0.156	-0.161
	[0.125]	[0.143]	[0.144]	[0.145]	[0.150]	[0.123]	[0.144]	[0.147]	[0.152]	[0.146]
time_alc_mun	0.208*	0.167	0.166	0.178	0.166	0.210**	0.188*	0.185	0.187	0.190*
	[0.104]	[0.108]	[0.108]	[0.110]	[0.113]	[0.104]	[0.111]	[0.113]	[0.116]	[0.112]
hosp	0.155***	0.155***	0.155***	0.155***	0.155***	0.155***	0.150***	0.149***	0.150***	0.150***
	[0.0456]	[0.0416]	[0.0422]	[0.0421]	[0.0421]	[0.0465]	[0.0430]	[0.0435]	[0.0436]	[0.0438]
pipe	0.343	0.298	0.298	0.31	0.298	0.344	0.337	0.336	0.336	0.337
	[0.244]	[0.222]	[0.226]	[0.236]	[0.224]	[0.247]	[0.225]	[0.227]	[0.229]	[0.229]
sewage	0.172	0.159	0.159	0.162	0.159	0.173	0.161	0.16	0.161	0.163
	[0.105]	[0.100]	[0.102]	[0.102]	[0.101]	[0.105]	[0.107]	[0.110]	[0.109]	[0.107]
insur_mun	-0.93	-1.145*	-1.146*	-1.086*	-1.146*	-0.901	-1.130*	-1.162*	-1.133*	-1.104
	[0.732]	[0.618]	[0.603]	[0.624]	[0.670]	[0.750]	[0.658]	[0.618]	[0.617]	[0.706]
insur_mun2	0.699	0.799	0.799	0.772	0.799	0.686	0.791	0.806	0.792	0.779
	[0.602]	[0.511]	[0.505]	[0.522]	[0.538]	[0.616]	[0.544]	[0.525]	[0.525]	[0.571]
numchildren	-0.0536	-0.0303	-0.0302	-0.0367	-0.0302	-0.0616	0.000609	0.0093	0.00143	-0.00629
	[0.0819]	[0.0801]	[0.0828]	[0.0822]	[0.0803]	[0.0821]	[0.0861]	[0.0998]	[0.103]	[0.0846]
altitud	0.157	0.277**	0.277**	0.244*	0.277**	0.148	0.222*	0.233*	0.223	0.214
	[0.138]	[0.123]	[0.135]	[0.143]	[0.136]	[0.141]	[0.128]	[0.137]	[0.137]	[0.132]
altitud2	-0.0938**	-0.129***	-0.129***	-0.119**	-0.129***	-0.0911*	-0.113***	-0.116**	-0.113**	-0.110**
	[0.0460]	[0.0404]	[0.0445]	[0.0476]	[0.0427]	[0.0470]	[0.0428]	[0.0457]	[0.0463]	[0.0437]
wage_fu	0.151	0.155	0.155	0.154	0.155	0.152	0.146	0.145	0.146	0.147
	[0.0959]	[0.102]	[0.103]	[0.100]	[0.103]	[0.0960]	[0.0984]	[0.101]	[0.0997]	[0.0988]
wage_fr	-0.16	-0.11	-0.11	-0.124	-0.11	-0.170*	-0.0934	-0.0827	-0.0924	-0.102
	[0.0985]	[0.103]	[0.103]	[0.103]	[0.110]	[0.0986]	[0.102]	[0.101]	[0.115]	[0.110]
price_index	0.338	-0.076	-0.078	0.0377	-0.0778	0.395	-0.0587	-0.122	-0.0647	-0.00835
	[0.243]	[0.240]	[0.319]	[0.326]	[0.294]	[0.243]	[0.251]	[0.374]	[0.381]	[0.289]
Observations	5717	5717	5717	5717	5717	5717	5717	5717	5717	5717
R-squared	0.191	0.154	0.154	0.171	0.154	0.192	0.13	0.112	0.128	0.143

Robust standard errors in brackets

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

Table A9: Effect on Child's Height Nonlinear IV FeA

Effect of HC Participation on Child's Height Using nonlinear IV - FeA

	1	2	3	4	5	6	7	8	9	10
VARIABLES	OLS	IV: All Instruments	IV: Capacity	IV: Fee	IV: Distance	OLS	IV: All Instruments	IV: Capacity	IV: Fee	IV: Distance
exposure	-0.00439	0.945**	0.977	1.016	1.090**					
	[0.0895]	[0.366]	[0.599]	[0.656]	[0.507]					
asis_hc						-0.0913**	0.448**	0.450*	0.496**	0.504**
						[0.0443]	[0.190]	[0.240]	[0.229]	[0.229]
female	0.146***	0.146***	0.146***	0.146***	0.146***	0.147***	0.142***	0.142***	0.142***	0.142***
	[0.0342]	[0.0325]	[0.0325]	[0.0324]	[0.0323]	[0.0347]	[0.0330]	[0.0329]	[0.0327]	[0.0329]
age_m	-0.0284***	-0.0368***	-0.0371***	-0.0375***	-0.0381***	-0.0263***	-0.0390***	-0.0390***	-0.0401***	-0.0403***
	[0.00330]	[0.00573]	[0.00737]	[0.00814]	[0.00707]	[0.00334]	[0.00675]	[0.00756]	[0.00772]	[0.00776]
age_m2	0.0249***	0.0315***	0.0317***	0.0320***	0.0325***	0.0225***	0.0372***	0.0372***	0.0385***	0.0387***
	[0.00312]	[0.00489]	[0.00612]	[0.00676]	[0.00592]	[0.00325]	[0.00726]	[0.00832]	[0.00842]	[0.00845]
ln_age_h	0.264***	0.292***	0.293***	0.294***	0.296***	0.262***	0.275***	0.275***	0.276***	0.276***
	[0.0799]	[0.0877]	[0.0918]	[0.0911]	[0.0890]	[0.0797]	[0.0839]	[0.0848]	[0.0845]	[0.0843]
ln_age_m	0.327***	0.343***	0.343***	0.344***	0.345***	0.323***	0.345***	0.345***	0.347***	0.347***
	[0.115]	[0.122]	[0.122]	[0.121]	[0.122]	[0.115]	[0.117]	[0.117]	[0.117]	[0.117]
height_mot	5.555***	5.641***	5.644***	5.647***	5.654***	5.548***	5.591***	5.591***	5.595***	5.595***
	[0.453]	[0.448]	[0.453]	[0.448]	[0.447]	[0.452]	[0.468]	[0.471]	[0.471]	[0.469]
ln_order	-0.277***	-0.304***	-0.305***	-0.306***	-0.309***	-0.275***	-0.289***	-0.289***	-0.290***	-0.290***
	[0.0370]	[0.0425]	[0.0456]	[0.0445]	[0.0441]	[0.0369]	[0.0396]	[0.0399]	[0.0398]	[0.0401]
edu_m_b	0.123**	0.109*	0.109*	0.108*	0.107*	0.123**	0.124**	0.124**	0.124**	0.124**
	[0.0538]	[0.0546]	[0.0549]	[0.0552]	[0.0558]	[0.0540]	[0.0533]	[0.0533]	[0.0533]	[0.0533]
edu_m_c	0.232***	0.217***	0.217***	0.216***	0.215***	0.228***	0.251***	0.251***	0.253***	0.253***
	[0.0752]	[0.0763]	[0.0769]	[0.0764]	[0.0772]	[0.0750]	[0.0760]	[0.0767]	[0.0777]	[0.0766]
edu_h_b	0.0834	0.0707	0.0703	0.0698	0.0688	0.0855	0.0727	0.0727	0.0716	0.0714
	[0.0582]	[0.0561]	[0.0572]	[0.0581]	[0.0566]	[0.0583]	[0.0584]	[0.0586]	[0.0590]	[0.0587]
edu_h_c	0.218**	0.245**	0.246**	0.247**	0.249**	0.217**	0.228**	0.228**	0.229**	0.229**
	[0.0913]	[0.104]	[0.104]	[0.106]	[0.108]	[0.0904]	[0.0974]	[0.0966]	[0.0976]	[0.0986]
centretown	0.0137	-0.0272	-0.0286	-0.0303	-0.0335	0.0193	-0.0149	-0.015	-0.0179	-0.0184
	[0.0641]	[0.0682]	[0.0753]	[0.0765]	[0.0694]	[0.0630]	[0.0665]	[0.0693]	[0.0699]	[0.0666]
time_hea	0.0812	0.137	0.139	0.142	0.146	0.0771	0.103	0.103	0.105	0.105
	[0.105]	[0.0967]	[0.100]	[0.106]	[0.0981]	[0.106]	[0.0996]	[0.100]	[0.102]	[0.0999]
time_hea2	0.0436	0.0229	0.0222	0.0213	0.0197	0.0456	0.0332	0.0332	0.0321	0.0319
	[0.0396]	[0.0390]	[0.0429]	[0.0429]	[0.0388]	[0.0394]	[0.0410]	[0.0424]	[0.0425]	[0.0413]
time_sch	-0.201	-0.039	-0.0335	-0.0269	-0.0143	-0.227	-0.0692	-0.0688	-0.0552	-0.053
	[0.244]	[0.259]	[0.255]	[0.272]	[0.282]	[0.244]	[0.261]	[0.251]	[0.263]	[0.273]
time_sch2	0.508	0.332	0.326	0.319	0.306	0.535*	0.37	0.369	0.355	0.353
	[0.306]	[0.331]	[0.327]	[0.343]	[0.354]	[0.305]	[0.341]	[0.327]	[0.339]	[0.354]
time_hea_sch	0.217	0.06	0.0546	0.0482	0.036	0.244	0.0813	0.0809	0.0669	0.0646
	[0.236]	[0.280]	[0.284]	[0.305]	[0.301]	[0.231]	[0.273]	[0.271]	[0.288]	[0.286]
time_alc	-0.183*	-0.235**	-0.237**	-0.239**	-0.244**	-0.179*	-0.203**	-0.203**	-0.205**	-0.205**
	[0.0967]	[0.100]	[0.107]	[0.114]	[0.103]	[0.0968]	[0.0968]	[0.0984]	[0.101]	[0.0970]
time_alc2	0.0790**	0.0674**	0.0670**	0.0665**	0.0656**	0.0815***	0.0663**	0.0663**	0.0650**	0.0648*
	[0.0296]	[0.0308]	[0.0302]	[0.0301]	[0.0319]	[0.0298]	[0.0317]	[0.0305]	[0.0305]	[0.0326]
timealchea	-0.146***	-0.122***	-0.121***	-0.120***	-0.118***	-0.149***	-0.129***	-0.129***	-0.127***	-0.127***

Table A10: Effect on Child's Height Nonlinear IV FeA: continued

	[0.0349]	[0.0386]	[0.0412]	[0.0409]	[0.0401]	[0.0346]	[0.0388]	[0.0396]	[0.0397]	[0.0397]
timealcsch	-0.0596	0.163	0.171	0.18	0.197	-0.0951	0.121	0.121	0.14	0.143
	[0.303]	[0.370]	[0.380]	[0.407]	[0.399]	[0.293]	[0.366]	[0.362]	[0.381]	[0.384]
time_sch_mun	-1.844***	-1.866***	-1.867***	-1.868***	-1.870***	-1.859***	-1.775***	-1.775***	-1.768***	-1.767***
	[0.599]	[0.552]	[0.551]	[0.551]	[0.548]	[0.605]	[0.570]	[0.570]	[0.566]	[0.565]
time_hea_mun	-0.197	-0.134	-0.132	-0.129	-0.124	-0.203	-0.167	-0.167	-0.164	-0.163
	[0.125]	[0.141]	[0.143]	[0.147]	[0.146]	[0.123]	[0.138]	[0.137]	[0.140]	[0.141]
time_alc_mun	0.208*	0.169	0.167	0.166	0.163	0.210**	0.193*	0.193*	0.191*	0.191*
	[0.104]	[0.108]	[0.108]	[0.110]	[0.110]	[0.104]	[0.109]	[0.108]	[0.109]	[0.110]
hosp	0.155***	0.155***	0.155***	0.155***	0.155***	0.155***	0.151***	0.151***	0.151***	0.151***
	[0.0456]	[0.0421]	[0.0421]	[0.0421]	[0.0423]	[0.0465]	[0.0435]	[0.0434]	[0.0436]	[0.0437]
pipe	0.343	0.3	0.299	0.297	0.294	0.344	0.338	0.338	0.338	0.338
	[0.244]	[0.224]	[0.224]	[0.225]	[0.221]	[0.247]	[0.231]	[0.231]	[0.230]	[0.229]
sewage	0.172	0.159	0.159	0.159	0.158	0.173	0.164	0.164	0.163	0.163
	[0.105]	[0.103]	[0.104]	[0.104]	[0.103]	[0.105]	[0.107]	[0.108]	[0.108]	[0.107]
insur_mun	-0.93	-1.134*	-1.141*	-1.149*	-1.165*	-0.901	-1.077	-1.077	-1.092*	-1.095
	[0.732]	[0.639]	[0.617]	[0.618]	[0.644]	[0.750]	[0.676]	[0.657]	[0.652]	[0.678]
insur_mun2	0.699	0.794	0.797	0.801	0.808	0.686	0.767	0.767	0.774	0.775
	[0.602]	[0.524]	[0.510]	[0.509]	[0.522]	[0.616]	[0.558]	[0.549]	[0.545]	[0.558]
numchildren	-0.0536	-0.0315	-0.0308	-0.0299	-0.0281	-0.0616	-0.0138	-0.0137	-0.00957	-0.00891
	[0.0819]	[0.0825]	[0.0861]	[0.0874]	[0.0848]	[0.0821]	[0.0854]	[0.0901]	[0.0911]	[0.0868]
altitud	0.157	0.270**	0.274*	0.279*	0.288**	0.148	0.205	0.205	0.21	0.211
	[0.138]	[0.125]	[0.140]	[0.142]	[0.130]	[0.141]	[0.131]	[0.135]	[0.134]	[0.132]
altitud2	-0.0938**	-0.127***	-0.128***	-0.129***	-0.132***	-0.0911*	-0.108**	-0.108**	-0.109**	-0.109**
	[0.0460]	[0.0405]	[0.0448]	[0.0453]	[0.0412]	[0.0470]	[0.0435]	[0.0447]	[0.0445]	[0.0436]
wage_fu	0.151	0.154	0.155	0.155	0.155	0.152	0.147	0.147	0.147	0.147
	[0.0959]	[0.102]	[0.103]	[0.103]	[0.104]	[0.0960]	[0.0980]	[0.0979]	[0.0984]	[0.0986]
wage_fr	-0.16	-0.113	-0.111	-0.109	-0.106	-0.170*	-0.111	-0.111	-0.106	-0.105
	[0.0985]	[0.104]	[0.102]	[0.104]	[0.108]	[0.0986]	[0.100]	[0.0963]	[0.0994]	[0.103]
price_index	0.338	-0.0545	-0.0678	-0.0839	-0.114	0.395	0.0465	0.0456	0.0156	0.0108
	[0.243]	[0.270]	[0.370]	[0.386]	[0.308]	[0.243]	[0.257]	[0.301]	[0.296]	[0.273]
asis_hc						-0.0913**	0.448**	0.450*	0.496**	0.504**
						[0.0443]	[0.190]	[0.240]	[0.229]	[0.229]
Observations	5717	5717	5717	5717	5717	5717	5717	5717	5717	5717
R-squared	0.191	0.158	0.156	0.153	0.147	0.192	0.155	0.155	0.149	0.148

Robust standard errors in brackets

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

Table A11: Effect on Child's Height ENDS

Effect of HC Participation on Child's Height Using linear and nonlinear IV - ENDS

	1	2	3	4	5	6
VARIABLES	OLS. DHS	OLS. DHS	Non-Linear IV: Capacity. DHS	Non-Linear IV: Capacity. DHS	Linear IV: Capacity. DHS	Linear IV: Capacity. DHS
exposure	-0.0694		1.227***		0.751	
	[0.0553]		[0.365]		[0.692]	
asis_hc		-0.0651***		0.826***		0.442
		[0.0250]		[0.192]		[0.381]
female	0.0379*	0.0372*	0.0389*	0.0475**	0.0385*	0.0431**
	[0.0197]	[0.0195]	[0.0206]	[0.0216]	[0.0201]	[0.0211]
age_m	-0.0358***	-0.0348***	-0.0440***	-0.0547***	-0.0410***	-0.0461***
	[0.00214]	[0.00212]	[0.00296]	[0.00458]	[0.00493]	[0.00888]
age_m2	0.000369***	0.000360***	0.000434***	0.000537***	0.000410***	0.000460***
	[2.71e-05]	[2.69e-05]	[3.19e-05]	[4.55e-05]	[4.52e-05]	[8.21e-05]
ln_age_h	0.00776	0.00451	0.0402	0.0756*	0.0283	0.0449
	[0.0404]	[0.0405]	[0.0419]	[0.0452]	[0.0456]	[0.0537]
ln_age_m	0.757***	0.755***	0.789***	0.811***	0.777***	0.787***
	[0.0752]	[0.0752]	[0.0795]	[0.0851]	[0.0793]	[0.0830]
height_mot	5.390***	5.386***	5.435***	5.362***	5.418***	5.372***
	[0.238]	[0.237]	[0.240]	[0.254]	[0.238]	[0.244]
ln_order	-0.326***	-0.326***	-0.338***	-0.336***	-0.334***	-0.332***
	[0.0268]	[0.0267]	[0.0273]	[0.0279]	[0.0282]	[0.0276]
edu_m_b	0.181**	0.183**	0.206**	0.179*	0.197**	0.180*
	[0.0841]	[0.0832]	[0.0914]	[0.104]	[0.0881]	[0.0938]
edu_m_c	0.178**	0.182**	0.194**	0.157	0.188**	0.167*
	[0.0850]	[0.0840]	[0.0921]	[0.107]	[0.0885]	[0.0963]
edu_h_b	-0.0298	-0.0325	-0.0207	-0.0162	-0.024	-0.0232
	[0.0492]	[0.0490]	[0.0485]	[0.0501]	[0.0491]	[0.0500]
edu_h_c	-0.127**	-0.131**	-0.125**	-0.135**	-0.126**	-0.133**
	[0.0624]	[0.0624]	[0.0609]	[0.0624]	[0.0612]	[0.0615]
pipe	-0.0342	-0.0285	-0.0391	-0.0119	-0.0373	-0.019
	[0.0728]	[0.0729]	[0.0793]	[0.0881]	[0.0757]	[0.0779]
sewage	-0.076	-0.0884	0.0677	0.107	0.0149	0.0226
	[0.104]	[0.103]	[0.124]	[0.138]	[0.136]	[0.144]
numchildren	0.0141**	0.0132*	0.0120*	0.0152**	0.0128*	0.0144**
	[0.00712]	[0.00709]	[0.00701]	[0.00724]	[0.00724]	[0.00691]
altitud	0.0507	0.055	0.0262	0.0224	0.0352	0.0365
	[0.111]	[0.109]	[0.115]	[0.123]	[0.114]	[0.116]
altitud2	-0.0939**	-0.0963**	-0.0968**	-0.0992**	-0.0958**	-0.0980**
	[0.0430]	[0.0422]	[0.0451]	[0.0484]	[0.0437]	[0.0446]
sisben2	0.153***	0.151***	0.164***	0.169***	0.160***	0.161***
	[0.0435]	[0.0441]	[0.0451]	[0.0441]	[0.0443]	[0.0438]
sisben3	0.286***	0.284***	0.313***	0.335***	0.303***	0.313***
	[0.0493]	[0.0498]	[0.0490]	[0.0456]	[0.0490]	[0.0488]
Observations	6170	6179	6170	6179	6170	6179
R-squared	0.215	0.216	0.165	0.075	0.195	0.17

Robust standard errors in brackets

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

Table A12: Effect Quantiles FeA Exposure

Effect of HC Participation on Child's Height at different quantiles - FeA - Exposure

	Q10			Q25			Q50			Q75			Q90		
	Coeff	SE	z	Coeff	SE	z	Coeff	SE	z	Coeff	SE	z	Coeff	SE	z
exposure	1.7377	0.8639	2.01	1.8838	0.7138	2.64	1.4562	0.6144	2.37	0.5106	0.5973	0.85	0.0507	0.7518	0.07
res_exp	-1.749	0.89	-1.97	-1.956	0.7323	-2.67	-1.688	0.6162	-2.74	-0.679	0.6084	-1.12	-0.092	0.7938	-0.12
res_exp2	-0.216	0.546	-0.39	-0.071	0.3051	-0.23	-0.423	0.3502	-1.21	-0.572	0.4633	-1.23	0.508	0.6585	0.77
female	0.0585	0.0596	0.98	0.1687	0.0442	3.82	0.1484	0.0398	3.73	0.1516	0.0365	4.15	0.1601	0.0446	3.59
age_m	-0.03	0.0106	-2.82	-0.044	0.009	-4.85	-0.042	0.0067	-6.28	-0.037	0.0064	-5.83	-0.034	0.0069	-4.98
age_m2	0.0255	0.009	2.83	0.0385	0.0077	4.98	0.0357	0.0057	6.24	0.0328	0.0057	5.73	0.0282	0.0062	4.53
ln_age_h	0.3405	0.1371	2.48	0.331	0.1102	3	0.2742	0.1097	2.5	0.2759	0.1038	2.66	0.2297	0.1344	1.71
ln_age_m	0.2225	0.2118	1.05	0.2729	0.1512	1.81	0.3293	0.1384	2.38	0.3043	0.1389	2.19	0.5022	0.154	3.26
height~t	6.0717	0.6509	9.33	6.0958	0.5171	11.79	5.2466	0.4656	11.27	5.4274	0.5382	10.09	5.3417	0.6461	8.27
ln_order	-0.3	0.061	-4.92	-0.349	0.0552	-6.33	-0.29	0.044	-6.58	-0.268	0.0476	-5.63	-0.237	0.0581	-4.08
edu_m_b	0.0912	0.0855	1.07	0.0615	0.0651	0.94	0.1198	0.0624	1.92	0.0992	0.0632	1.57	0.1105	0.0649	1.7
edu_m_c	0.077	0.1906	0.4	0.1487	0.1206	1.23	0.2366	0.0989	2.39	0.2902	0.097	2.99	0.2365	0.1122	2.11
edu_h_b	0.094	0.0732	1.28	0.1016	0.0615	1.65	0.0386	0.0582	0.66	0.0578	0.062	0.93	0.0769	0.0936	0.82
edu_h_c	0.3774	0.2314	1.63	0.2574	0.1222	2.11	0.268	0.1382	1.94	0.2743	0.0776	3.53	0.3701	0.1045	3.54
centre~n	0.0863	0.1108	0.78	0.0532	0.0919	0.58	-0.044	0.0837	-0.53	-0.077	0.0743	-1.04	-0.2	0.0956	-2.1
time_hea	0.0866	0.2046	0.42	0.0354	0.1666	0.21	0.0521	0.1588	0.33	0.1315	0.1798	0.73	0.1092	0.2187	0.5
time_h~2	0.0593	0.0991	0.6	0.0138	0.0671	0.21	0.0116	0.0823	0.14	0.0438	0.1384	0.32	0.1692	0.1835	0.92
time_sch	-0.349	0.4823	-0.72	-0.258	0.3246	-0.79	0.1822	0.3795	0.48	0.3983	0.3726	1.07	0.2779	0.475	0.58
time_s~2	0.6008	0.6699	0.9	0.4666	0.5714	0.82	0.2386	0.5479	0.44	0.0357	0.6616	0.05	-0.501	0.8065	-0.62
time_h~h	0.4546	0.6074	0.75	0.4446	0.4182	1.06	0.0402	0.4029	0.1	0.3382	0.4774	0.71	0.3337	0.558	0.6
time_alc	-0.164	0.2324	-0.71	-0.105	0.1676	-0.63	-0.201	0.1502	-1.34	-0.211	0.1678	-1.26	-0.321	0.2117	-1.52
time_a~2	0.094	0.0986	0.95	0.0691	0.0524	1.32	0.0511	0.0578	0.88	0.0817	0.0656	1.25	0.1192	0.0754	1.58
timeal~a	-0.197	0.1385	-1.42	-0.118	0.0811	-1.45	-0.077	0.0844	-0.91	-0.161	0.1303	-1.23	-0.301	0.1458	-2.06
timeal~h	-0.148	0.5897	-0.25	-0.194	0.5119	-0.38	0.0586	0.5241	0.11	-0.171	0.5934	-0.29	0.134	0.5851	0.23
time_s~n	-1.093	0.9575	-1.14	-1.737	0.8401	-2.07	-2.046	0.842	-2.43	-1.884	0.8625	-2.18	-1.552	1.0393	-1.49
time_h~n	-0.148	0.2876	-0.51	-0.174	0.2357	-0.74	-0.271	0.241	-1.13	-0.088	0.2497	-0.35	-0.187	0.2783	-0.67
time_a~n	0.1388	0.2578	0.54	0.1874	0.1733	1.08	0.3419	0.1689	2.02	0.0736	0.1845	0.4	0.1077	0.1992	0.54
hosp	0.0991	0.0794	1.25	0.1926	0.071	2.71	0.1352	0.0697	1.94	0.1117	0.0697	1.6	0.0955	0.0836	1.14
pipe	0.8658	0.4462	1.94	0.2451	0.4409	0.56	0.2054	0.4054	0.51	0.3402	0.3909	0.87	0.0661	0.4084	0.16
sewage	0.1509	0.2688	0.56	0.0857	0.2508	0.34	0.1334	0.2266	0.59	0.1472	0.2014	0.73	0.3134	0.2197	1.43
insur_mun	-1.18	1.847	-0.64	-0.973	1.3676	-0.71	-1.263	1.3578	-0.93	-0.817	1.4688	-0.56	-1.507	1.5177	-0.99
ss12_m~2	1.1029	1.4209	0.78	0.72	1.0533	0.68	0.7885	1.067	0.74	0.3189	1.1843	0.27	0.961	1.2225	0.79
numchi~n	-0.014	0.3403	-0.04	0.0024	0.2435	0.01	0.0026	0.188	0.01	-0.028	0.2114	-0.13	-0.098	0.2312	-0.42
altitud	0.2666	0.2763	0.96	0.3854	0.2083	1.85	0.2417	0.223	1.08	0.0221	0.2431	0.09	0.1443	0.28	0.52
altitud2	-0.143	0.1019	-1.4	-0.172	0.0751	-2.29	-0.106	0.0774	-1.36	-0.037	0.0843	-0.44	-0.104	0.0983	-1.06
wage_fu	-0.011	0.1718	-0.07	0.165	0.1559	1.06	0.2185	0.1611	1.36	0.2145	0.1759	1.22	0.322	0.1873	1.72
wage_fr	0.0645	0.1843	0.35	-0.085	0.1545	-0.55	-0.214	0.1687	-1.27	-0.289	0.1812	-1.59	-0.185	0.1955	-0.95
price_~x	-0.405	0.534	-0.76	-0.286	0.617	-0.46	-0.049	0.6273	-0.08	0.4218	0.4992	0.85	0.4017	0.5436	0.74
region_2	0.4208	0.2652	1.59	0.4797	0.2473	1.94	0.3631	0.2274	1.6	0.398	0.22	1.81	0.2171	0.2233	0.97
region_3	0.1577	0.2931	0.54	0.2475	0.2472	1	0.166	0.2291	0.72	0.2003	0.2214	0.9	-0.04	0.2474	-0.16
region_4	0.223	0.2562	0.87	0.1535	0.2137	0.72	0.0248	0.2041	0.12	0.0337	0.1912	0.18	-0.147	0.2319	-0.63
time2	0.0843	0.0571	1.48	0.037	0.0519	0.71	0.0238	0.0491	0.49	-0.023	0.0502	-0.46	-0.03	0.0586	-0.52
time3	0.1274	0.1096	1.16	0.0957	0.0945	1.01	0.0402	0.0984	0.41	-0.018	0.0842	-0.21	0.0347	0.1034	0.34
_cons	-11.17	1.2001	-9.31	-9.908	1.049	-9.45	-7.77	0.9466	-8.21	-7.764	0.9795	-7.93	-6.498	1.2146	-5.35

Table A13: Effect Quantiles FeA Attendance

Effect of HC Participation on Child's Height at different quantiles - FeA - Attendance

	Q10			Q25			Q50			Q75			Q90		
	Coeff	**SE**	**z**	**Coeff**	**SE**	**z**	**Coeff**	**SE**	**z**	**Coeff**	**SE**	**z**	**Coeff**	**SE**	**z**
asis_hc	0.6491	0.2899	2.24	0.6196	0.2604	2.38	0.4191	0.2307	1.82	0.1252	0.2216	0.57	0.0335	0.2772	0.12
res_asis	-0.422	0.1861	-2.27	-0.452	0.1681	-2.69	-0.35	0.1479	-2.37	-0.114	0.1394	-0.82	-0.098	0.2006	-0.49
res_as~2	-0.069	0.0631	-1.09	0.0269	0.0398	0.68	0.0119	0.033	0.36	-0.064	0.0392	-1.62	-0.003	0.0562	-0.06
female	0.0692	0.0584	1.18	0.163	0.0434	3.76	0.163	0.0417	3.91	0.144	0.0374	3.85	0.1591	0.0425	3.74
age_m	-0.033	0.0106	-3.06	-0.044	0.0092	-4.77	-0.042	0.0077	-5.46	-0.035	0.0074	-4.66	-0.032	0.0077	-4.12
age_m2	0.0335	0.0116	2.9	0.044	0.0099	4.45	0.0402	0.0084	4.79	0.0306	0.0082	3.72	0.0252	0.009	2.81
ln_age_h	0.2799	0.1256	2.23	0.328	0.1016	3.23	0.2667	0.1061	2.51	0.2598	0.1001	2.6	0.2098	0.134	1.57
ln_age_m	0.2359	0.206	1.15	0.2431	0.1524	1.6	0.349	0.132	2.64	0.2984	0.1389	2.15	0.5642	0.1592	3.54
height~t	6.1422	0.6483	9.47	6.0724	0.5303	11.45	5.0657	0.4922	10.29	5.3395	0.5422	9.85	5.2202	0.6629	7.87
ln_order	-0.312	0.0549	-5.68	-0.327	0.0523	-6.25	-0.301	0.0451	-6.67	-0.263	0.0458	-5.73	-0.241	0.054	-4.47
edu_m_b	0.1004	0.0814	1.23	0.0842	0.0623	1.35	0.1403	0.061	2.3	0.0877	0.0627	1.4	0.1123	0.0643	1.75
edu_m_c	0.0917	0.2003	0.46	0.1909	0.1176	1.62	0.3037	0.0951	3.19	0.2951	0.1038	2.84	0.2926	0.1103	2.65
edu_h_b	0.0818	0.0756	1.08	0.0966	0.0659	1.46	0.0452	0.0579	0.78	0.0678	0.0637	1.06	0.069	0.0944	0.73
edu_h_c	0.3378	0.2243	1.51	0.2073	0.1206	1.72	0.2307	0.129	1.79	0.2549	0.0751	3.39	0.3257	0.1054	3.09
centre~n	0.0794	0.1038	0.77	0.0789	0.0834	0.95	-0.002	0.078	-0.03	-0.071	0.0728	-0.97	-0.206	0.0935	-2.2
time_hea	0.0893	0.1937	0.46	-0.057	0.1653	-0.35	-0.016	0.1649	-0.09	0.1092	0.1751	0.62	0.0694	0.2081	0.33
time_h~2	0.0522	0.0957	0.55	0.0014	0.0739	0.02	0.0088	0.073	0.12	0.062	0.1408	0.44	0.1753	0.182	0.96
time_sch	-0.323	0.4416	-0.73	-0.07	0.3283	-0.21	0.2785	0.3639	0.77	0.4299	0.3576	1.2	0.2971	0.4743	0.63
time_s~2	0.5744	0.6285	0.91	0.0913	0.5539	0.16	0.1138	0.5018	0.23	-0.049	0.6406	-0.08	-0.485	0.7828	-0.62
time_h~h	0.4765	0.489	0.97	0.4985	0.356	1.4	0.2955	0.329	0.9	0.4229	0.4239	1	0.3665	0.4999	0.73
time_alc	-0.22	0.2258	-0.97	-0.08	0.1686	-0.47	-0.106	0.1511	-0.7	-0.204	0.1632	-1.25	-0.292	0.2052	-1.42
time_a~2	0.1191	0.0997	1.19	0.0575	0.0588	0.98	0.0419	0.0574	0.73	0.0889	0.0634	1.4	0.1005	0.0734	1.37
timeal~a	-0.21	0.135	-1.55	-0.091	0.0857	-1.07	-0.066	0.0819	-0.8	-0.177	0.1289	-1.37	-0.274	0.1432	-1.92
timeal~h	-0.237	0.4344	-0.55	-0.129	0.3927	-0.33	-0.231	0.3819	-0.6	-0.313	0.4982	-0.63	0.0489	0.4987	0.1
time_s~n	-1.042	0.9774	-1.07	-1.726	0.8461	-2.04	-2.144	0.8281	-2.59	-1.76	0.8611	-2.04	-1.397	1.0471	-1.33
time_h~n	-0.133	0.2611	-0.51	-0.145	0.2291	-0.63	-0.26	0.2303	-1.13	-0.138	0.2435	-0.57	-0.165	0.2705	-0.61
time_a~n	0.2215	0.2347	0.94	0.1978	0.181	1.09	0.3269	0.1644	1.99	0.0733	0.1859	0.39	0.0837	0.1993	0.42
hosp	0.1002	0.0768	1.31	0.2113	0.0721	2.93	0.1376	0.0717	1.92	0.1093	0.0688	1.59	0.0702	0.0816	0.86
Pipe	0.7856	0.4353	1.8	0.3793	0.447	0.85	0.2023	0.4196	0.48	0.3464	0.3914	0.88	0.0081	0.4173	0.02
sewage	0.2508	0.2578	0.97	0.0995	0.2447	0.41	0.1608	0.2268	0.71	0.1854	0.2054	0.9	0.2662	0.2115	1.26
insur_mun	-1.193	1.8022	-0.66	-0.799	1.3521	-0.59	-1.256	1.4305	-0.88	-0.825	1.4863	-0.55	-1.5	1.5228	-0.99
ss12_m~2	1.1399	1.394	0.82	0.5937	1.0541	0.56	0.7852	1.1244	0.7	0.3251	1.1947	0.27	0.9916	1.2277	0.81
numchi~n	-0.058	0.349	-0.17	-0.01	0.2481	-0.04	-0.012	0.1924	-0.06	-0.041	0.2096	-0.2	-0.037	0.2348	-0.16
altitud	0.2172	0.2917	0.74	0.3195	0.2095	1.53	0.1936	0.2343	0.83	-0.054	0.2484	-0.22	0.1375	0.2791	0.49
altitud2	-0.138	0.1075	-1.28	-0.161	0.0774	-2.09	-0.091	0.082	-1.11	-0.012	0.0861	-0.14	-0.102	0.0996	-1.02
wage_fu	-0.021	0.1626	-0.13	0.1031	0.1545	0.67	0.2202	0.1549	1.42	0.2165	0.1692	1.28	0.2889	0.1849	1.56
wage_fr	0.1046	0.188	0.56	-0.045	0.1514	-0.3	-0.233	0.1695	-1.38	-0.293	0.1758	-1.67	-0.189	0.1863	-1.02
price_~x	-0.247	0.5878	-0.42	-0.219	0.6409	-0.34	0.0514	0.6613	0.08	0.5211	0.4951	1.05	0.4891	0.5264	0.93
region_2	0.2887	0.2445	1.18	0.4224	0.2435	1.73	0.2875	0.2247	1.28	0.3817	0.219	1.74	0.2141	0.2158	0.99
region_3	0.034	0.2722	0.12	0.1361	0.2402	0.57	0.0902	0.22	0.41	0.1642	0.2208	0.74	-0.028	0.2356	-0.12
region_4	0.0793	0.2441	0.32	0.114	0.2151	0.53	-0.003	0.2067	-0.01	-4E-04	0.1963	0	-0.11	0.2368	-0.47
time2	0.0522	0.0549	0.95	0.0235	0.052	0.45	0.0085	0.0487	0.18	-0.028	0.0464	-0.6	-0.03	0.0561	-0.54
time3	0.1043	0.1135	0.92	0.0893	0.0979	0.91	0.0299	0.1026	0.29	-0.047	0.0835	-0.56	0.0456	0.0994	0.46
_cons	-11.12	1.1557	-9.62	-9.894	1.0158	-9.74	-7.35	0.9795	-7.5	-7.662	0.969	-7.91	-6.253	1.2357	-5.06

Table A14: Effect Quantiles ENDS Exposure

Effect of HC Participation on Child's Height at different quantiles - ENDS - Exposure

	Q10			Q25			Q50			Q75			Q90		
	Coeff	SE	z	Coeff	SE	z	Coeff	SE	z	Coeff	SE	z	Coeff	SE	z
exposure	3.4189	1.0746	3.18	3.0145	0.8444	3.57	2.2837	0.8978	2.54	1.6675	0.8051	2.07	1.6968	1.06811	1.59
res_exp	-3.489	1.087	-3.21	-3.138	0.8502	-3.69	-2.416	0.8985	-2.69	-1.781	0.8003	-2.23	-2.1	1.076014	-1.95
res_exp2	0.985	0.4554	2.16	0.612	0.3822	1.6	1.0511	0.2932	3.59	0.7171	0.366	1.96	0.1709	0.360952	0.47
age_m	-0.059	0.009	-6.57	-0.055	0.007	-7.87	-0.057	0.0068	-8.36	-0.053	0.0063	-8.5	-0.05	0.009483	-5.29
age_m2	0.0006	9E-05	6.53	0.0005	7E-05	7.51	0.0006	7E-05	8.35	0.0005	6E-05	8.35	0.0005	9.66E-05	5.01
female	0.0572	0.0422	1.36	0.0154	0.0352	0.44	0.057	0.0324	1.76	0.0611	0.0294	2.08	0.051	0.040575	1.26
ln_order	-0.356	0.0584	-6.1	-0.375	0.039	-9.62	-0.348	0.0375	-9.29	-0.315	0.0407	-7.73	-0.343	0.0458	-7.49
height~t	5.1568	0.4395	11.73	5.3531	0.3626	14.76	5.5715	0.3473	16.04	5.7511	0.2739	21	5.9225	0.392225	15.1
ln_age_h	0.1163	0.076	1.53	0.0742	0.0579	1.28	0.0912	0.0532	1.71	0.0245	0.0645	0.38	0.0999	0.082315	1.21
ln_age_m	1.0056	0.1601	6.28	0.9292	0.113	8.22	0.814	0.0992	8.2	0.6957	0.116	6	0.8207	0.117212	7
edu_m_b	0.1954	0.1588	1.23	0.2235	0.11	2.03	0.156	0.1305	1.2	0.1358	0.1238	1.1	0.1639	0.144726	1.13
edu_m_c	0.2444	0.1729	1.41	0.2006	0.1147	1.75	0.1315	0.1271	1.03	0.1085	0.1179	0.92	0.0956	0.147132	0.65
edu_h_b	-0.039	0.0806	-0.48	0.002	0.0605	0.03	0.0234	0.064	0.37	-0.005	0.0536	-0.09	-0.071	0.077537	-0.92
edu_h_c	-0.196	0.0952	-2.05	-0.156	0.0718	-2.17	-0.106	0.0785	-1.36	-0.131	0.0745	-1.75	-0.129	0.094826	-1.36
sisben2	0.1548	0.0824	1.88	0.2068	0.0651	3.18	0.148	0.0587	2.52	0.185	0.0476	3.88	0.1885	0.071244	2.65
sisben3	0.3458	0.0841	4.11	0.3771	0.0651	5.79	0.309	0.0646	4.78	0.3457	0.053	6.53	0.3349	0.081326	4.12
pipe	0.038	0.1861	0.2	-0.001	0.1578	-0.01	-0.056	0.1483	-0.37	-0.076	0.147	-0.52	-0.012	0.132505	-0.09
sewage	-0.025	0.2767	-0.09	0.1669	0.2557	0.65	0.1384	0.2152	0.64	0.1218	0.2063	0.59	0.0686	0.201959	0.34
altitud	-0.01	0.2189	-0.04	0.0861	0.2102	0.41	-0.045	0.1745	-0.26	-0.014	0.1933	-0.07	-0.003	0.215038	-0.01
altitud2	-0.084	0.0837	-1	-0.134	0.0761	-1.76	-0.083	0.0677	-1.23	-0.076	0.0759	-1.01	-0.085	0.086234	-0.98
numchi~n	0.0016	0.0224	0.07	0.0121	0.0166	0.73	0.0153	0.0133	1.15	0.0125	0.017	0.74	0.0252	0.01606	1.57
dent1	-0.115	0.568	-0.2	-0.33	0.4742	-0.7	-0.102	0.4283	-0.24	-0.06	0.4441	-0.14	0.1419	0.508549	0.28
dent2	-0.459	0.5508	-0.83	-0.755	0.4699	-1.61	-0.562	0.425	-1.32	-0.35	0.4357	-0.8	-0.242	1.52E+11	0
dent3	-0.002	0.5166	0	-0.277	0.4676	-0.59	-0.278	0.3857	-0.72	-0.182	0.3865	-0.47	-0.265	1.21E+12	0
dent4	-0.677	0.5704	-1.19	-0.739	0.4997	-1.48	-0.6	0.4417	-1.36	-0.38	0.4383	-0.87	-0.261	1.84E+11	0
dent5	0.1607	0.6261	0.26	0.0455	0.5105	0.09	0.166	0.4601	0.36	0.3503	0.4642	0.75	0.6319	3.68E+11	0
dent6	-0.082	0.6031	-0.14	-0.321	0.4781	-0.67	0.1425	0.4291	0.33	0.1366	0.4674	0.29	0.4516	2.96E+11	0
dent7	-0.255	0.5755	-0.44	-0.26	0.4849	-0.54	-0.146	0.4306	-0.34	-0.079	0.447	-0.18	0.1843	2.40E+11	0
dent8	-0.354	0.5842	-0.61	-0.482	0.4923	-0.98	-0.221	0.4447	-0.5	0.0455	0.4472	0.1	0.1952	2.40E+11	0
dent9	-0.55	0.5719	-0.96	-0.599	0.4918	-1.22	-0.394	0.429	-0.92	-0.107	0.4463	-0.24	-0.004	6.42E+10	0
dent10	-0.621	0.5613	-1.11	-0.557	0.4732	-1.18	-0.41	0.4214	-0.97	-0.258	0.435	-0.59	-0.162	1.37E+11	0
dent11	0.3655	0.5595	0.65	0.1215	0.4636	0.26	0.3896	0.4274	0.91	0.2849	0.4398	0.65	0.2966	9.10E+10	0
dent12	-0.499	0.5613	-0.89	-0.488	0.5101	-0.96	-0.208	0.4679	-0.44	-0.14	0.4549	-0.31	0.0817	2.80E+11	0
dent13	-0.171	0.5817	-0.29	-0.364	0.4912	-0.74	-0.06	0.4399	-0.14	0.2401	0.4904	0.49	0.3226	1.12E+11	0
dent14	-0.423	0.5717	-0.74	-0.552	0.4721	-1.17	-0.46	0.4259	-1.08	-0.127	0.4458	-0.29	0.0756	1.95E+11	0
dent15	-0.681	0.5596	-1.22	-0.696	0.4917	-1.42	-0.465	0.4317	-1.08	-0.316	0.4492	-0.7	-0.102	1.02E+11	0
dent16	-0.277	0.588	-0.47	-0.182	0.4777	-0.38	0.0046	0.4337	0.01	0.1936	0.4431	0.44	0.2997	2.21E+11	0
dent17	0.032	0.5725	0.06	-0.027	0.4668	-0.06	0.04	0.4344	0.09	0.1586	0.4703	0.34	0.4533	2.11E+11	0
dent18	-0.053	0.5522	-0.1	-0.263	0.4574	-0.57	-0.071	0.4194	-0.17	0.1359	0.4539	0.3	0.3481	1.89E+11	0
dent20	-0.094	0.5597	-0.17	-0.166	0.462	-0.36	0.0832	0.4304	0.19	0.1376	0.4472	0.31	0.3601	2.16E+11	0
dent21	-0.033	0.5816	-0.06	-0.298	0.4799	-0.62	-0.087	0.4319	-0.2	-0.103	0.4679	-0.22	-0.035	1.21E+11	0
dent22	-0.069	0.5743	-0.12	-0.169	0.4747	-0.36	0.0194	0.4265	0.05	0.1141	0.4423	0.26	0.3237	1.12E+11	0
dent23	-0.426	0.5594	-0.76	-0.539	0.4604	-1.17	-0.464	0.4195	-1.11	-0.304	0.4387	-0.69	-0.212	1.37E+11	0

Table A15: Effect Quantiles ENDS Exposure: continued

dent24	0.1558	0.5785	0.27	-0.211	0.4647	-0.45	-0.067	0.415	-0.16	0.1869	0.4392	0.43	0.4754	0.518293	0.92
dent25	0.0334	0.5222	0.06	-0.187	0.4223	-0.44	0.0822	0.3883	0.21	0.2552	0.4034	0.63	0.4062	5.08E+11	0
dent26	0.0891	0.5115	0.17	-0.167	0.4141	-0.4	-0.148	0.3661	-0.4	-0.071	0.4042	-0.18	0.0545	0.477197	0.11
dent27	0.1311	0.5492	0.24	0.0598	0.4574	0.13	0.1492	0.4087	0.37	0.1446	0.4314	0.34	0.1003	5.41E+11	0
dent30	0.1702	0.4292	0.4	-0.217	0.3669	-0.59	0.0016	0.3315	0	0.0592	0.3512	0.17	0.1562	8.80E+11	0
dent31	-0.123	0.5463	-0.23	-0.158	0.4265	-0.37	-0.098	0.38	-0.26	0.2748	0.3957	0.69	0.2859	6.37E+11	0
dent32	0.1699	0.4795	0.35	0.0856	0.3688	0.23	0.1227	0.3399	0.36	0.2278	0.3637	0.63	0.2554	8.79E+11	0
dent33	0.1558	0.5642	0.28	0.0497	0.4838	0.1	0.3042	0.3989	0.76	0.3943	0.4019	0.98	0.3761	4.69E+11	0
_cons	0.0727	1.106	0.07	-0.044	0.9046	-0.05	-0.046	0.7855	-0.06	0.019	0.7132	0.03	0.2345	0.881164	0.27
dent19	-0.029	0.5617	-0.05	-0.113	0.4835	-0.23	-0.002	0.4236	0	0.1647	0.4362	0.38	0.3526	2.41E+11	0
dent29	-13.27	0.4903	-27.06	-12.58	0.3436	-36.6	-11.91	0.3299	-36.11	-11.04	0.3714	-29.72	-11.64	8.83E+11	0

Table A16: Effect Quantiles ENDS Attendance

Effect of HC Participation on Child's Height at different quantiles - ENDS - Attendance

	Q10			Q25			Q50			Q75			Q90		
	Coeff	SE	z	Coeff	SE	z	Coeff	SE	z	Coeff	SE	z	Coeff	SE	z
asis_hc	1.3309	0.288185	4.62	1.0631	0.2108928	5.04	0.987	0.2261421	4.36	0.6598	0.1888501	3.49	0.4745	0.2801613	1.69
res_asis	-0.8621	0.1802515	-4.78	-0.7233	0.1317933	-5.49	-0.6856	0.1384924	-4.95	-0.4726	0.1143777	-4.13	-0.3811	0.1780049	-2.14
res_as~2	0.1083	0.0401465	2.7	0.0931	0.0309992	3	0.1197	0.0267809	4.47	0.053	0.0300422	1.77	0.0374	0.0397432	0.94
age_m	-0.0634	0.0079704	-7.95	-0.058	0.0059206	-9.8	-0.0617	0.0060938	-10.12	-0.0558	0.0050887	-10.97	-0.0487	0.0084422	-5.76
age_m2	0.0006	0.0000797	7.65	0.0005	0.0000617	8.79	0.0006	0.0000627	9.57	0.0006	0.0000542	10.39	0.0005	0.0000878	5.45
female	0.05	0.0437905	1.14	0.0151	0.0352942	0.43	0.0594	0.0330522	1.8	0.0747	0.0297002	2.52	0.0462	0.0411223	1.12
ln_order	-0.3365	0.0604048	-5.57	-0.3595	0.0403699	-8.9	-0.3383	0.0376683	-8.98	-0.3139	0.0417206	-7.52	-0.3313	0.047308	-7
height~t	5.1529	0.4408052	11.69	5.3019	0.3841333	13.8	5.5389	0.3531213	15.69	5.6779	0.2811404	20.2	5.8202	0.4137008	14.07
ln_age_h	0.1251	0.0697195	1.79	0.085	0.0578855	1.47	0.1016	0.0569556	1.78	0.0602	0.0644383	0.93	0.115	0.0854476	1.35
ln_age_m	0.9944	0.1588412	6.26	0.921	0.1129339	8.16	0.8174	0.1003565	8.15	0.694	0.1169221	5.94	0.7868	0.1190684	6.61
edu_m_b	0.2289	0.1731122	1.32	0.1744	0.118839	1.47	0.1152	0.1453083	0.79	0.1541	0.1218145	1.26	0.1336	0.144111	0.93
edu_m_c	0.2754	0.1899409	1.45	0.1437	0.1240817	1.16	0.1032	0.1414382	0.73	0.1158	0.1141267	1.01	0.0671	0.1502656	0.45
edu_h_b	-0.0305	0.0876479	-0.35	0.0247	0.0616707	0.4	0.0151	0.0638899	0.24	0.0086	0.0536464	0.16	-0.0632	0.085838	-0.74
edu_h_c	-0.1929	0.102553	-1.88	-0.1436	0.0751472	-1.91	-0.1525	0.0778407	-1.96	-0.0954	0.0737346	-1.29	-0.1049	0.100649	-1.04
sisben2	0.1476	0.0771209	1.91	0.1763	0.0686948	2.57	0.162	0.0585125	2.77	0.1719	0.0488996	3.52	0.1875	0.0722483	2.6
sisben3	0.3571	0.0806927	4.43	0.3585	0.0678202	5.29	0.3379	0.0618847	5.46	0.328	0.0520024	6.31	0.3251	0.0823306	3.95
pipe	0.0233	0.1793097	0.13	-0.0382	0.154678	-0.25	-0.0338	0.1539125	-0.22	-0.0547	0.1455292	-0.38	0.0093	0.1399204	0.07
sewage	0.0215	0.275104	0.08	0.1218	0.2360827	0.52	0.1306	0.225885	0.58	0.1532	0.2047109	0.75	-0.0152	0.2023494	-0.08
altitud	0.0475	0.227422	0.21	0.0614	0.201636	0.3	-0.0338	0.175981	-0.19	0.0795	0.1923556	0.41	0.1014	0.2240435	0.45
altitud2	-0.1028	0.087807	-1.17	-0.1188	0.0737049	-1.61	-0.0801	0.069636	-1.15	-0.1114	0.0750143	-1.49	-0.1208	0.088685	-1.36
numchi~n	0.0043	0.0208644	0.2	0.0162	0.0163124	0.99	0.0185	0.0139612	1.32	0.0068	0.0169748	0.4	0.0285	0.0175089	1.63
dent1	-0.2233	0.5696543	-0.39	-0.3842	0.5015229	-0.77	-0.1729	0.443178	-0.39	-0.1069	0.4312428	-0.25	0.0014	0.499844	0
dent2	-0.531	2.60E+10	0	-0.7526	1.89E+11	0	-0.5716	2.30E+09	0	-0.3805	0.4260083	-0.89	-0.2541	0.4936105	-0.51
dent3	-0.0858	0.5110135	-0.17	-0.4039	1.34E+13	0	-0.362	2.09E+10	0	-0.0376	0.4177048	-0.09	-0.3913	0.4844463	-0.81
dent4	-0.8487	3.91E+10	0	-0.8852	3.06E+11	0	-0.7401	5.22E+10	0	-0.4648	0.4206065	-1.11	-0.3374	0.4989149	-0.68
dent5	0.1229	0.5942594	0.21	-0.0342	4.51E+12	0	0.151	7.09E+09	0	0.3137	0.4587913	0.68	0.5909	0.5051645	1.17
dent6	-0.1615	6.06E+10	0	-0.3064	3.67E+12	0	0.0986	8.17E+10	0	0.0397	0.450606	0.09	0.412	0.5104511	0.81
dent7	-0.3597	4.74E+10	0	-0.3109	3.55E+11	0	-0.1777	6.33E+10	0	-0.1922	0.4301466	-0.45	0.1279	0.5071397	0.25
dent8	-0.5145	4.40E+10	0	-0.4945	2.68E+12	0	-0.302	5.76E+10	0	-0.0677	0.4421813	-0.15	0.1061	0.5264225	0.2
dent9	-0.6181	1.50E+10	0	-0.6326	1.12E+11	0	-0.4484	2.00E+10	0	-0.1999	0.4325563	-0.46	-0.0552	0.5123427	-0.11
dent10	-0.6991	2.60E+10	0	-0.6252	2.03E+11	0	-0.4427	3.48E+10	0	-0.3426	0.4190373	-0.82	-0.147	0.5033517	-0.29
dent11	0.3523	2.29E+10	0	0.1602	1.37E+12	0	0.4024	3.06E+10	0	0.2249	0.4368306	0.51	0.2922	0.5124098	0.57
dent12	-0.585	5.48E+10	0	-0.4993	4.26E+11	0	-0.2936	7.50E+10	0	-0.1839	0.4320011	-0.43	0.0467	0.4948322	0.09
dent13	-0.3386	2.75E+10	0	-0.3949	3.13E+11	0	-0.0983	3.67E+10	0	0.1235	0.4777679	0.26	0.269	0.5212526	0.52
dent14	-0.5513	3.91E+10	0	-0.5593	2.93E+11	0	-0.4998	5.22E+10	0	-0.2105	0.4446649	-0.47	0.0062	0.5043136	0.01
dent15	-0.8746	1.73E+10	0	-0.7698	1.46E+11	0	-0.5846	2.58E+10	0	-0.4308	0.4314245	-1	-0.1052	0.4958785	-0.21
dent16	-0.339	4.39E+10	0	-0.2124	3.39E+11	0	-0.0565	5.86E+10	0	0.103	0.4359463	0.24	0.2515	0.4978199	0.51
dent17	0.0276	3.49E+10	0	-0.015	9.95E+11	0	0.0374	4.80E+10	0	0.1837	0.4393228	0.42	0.4154	0.5234113	0.79
dent18	-0.119	2.75E+10	0	-0.3117	2.16E+11	0	-0.0832	3.67E+10	0	0.122	0.4445364	0.27	0.2918	0.5097089	0.57
dent20	-0.2204	4.01E+10	0	-0.1565	8.83E+11	0	0.0495	5.36E+10	0	0.0394	0.4318868	0.09	0.293	0.5054437	0.58
dent21	-0.2117	2.45E+10	0	-0.3574	1.47E+12	0	-0.1787	3.27E+10	0	-0.2231	0.4579376	-0.49	-0.1365	0.5313393	-0.26
dent22	-0.1738	2.12E+10	0	-0.2106	1.56E+11	0	-0.053	2.83E+10	0	-0.0755	0.4246742	-0.18	0.1692	0.4954751	0.34
dent23	-0.5272	2.46E+10	0	-0.5763	4.89E+11	0	-0.5252	3.48E+10	0	-0.4056	0.4350767	-0.93	-0.238	0.5050306	-0.47

Table A17: Effect Quantiles ENDS Attendance: continued

dent24	0.0843	0.560805	0.15	-0.2192	0.4851738	-0.45	-0.0587	0.4274893	-0.14	0.1206	0.4246788	0.28	0.3784	0.5038934	0.75
dent25	-0.0664	9.71E+10	0	-0.1688	7.20E+11	0	0.0831	1.21E+11	0	0.22	0.3875877	0.57	0.331	0.4464315	0.74
dent26	-0.0061	1.40E+11	0	-0.1505	0.4247659	-0.35	-0.163	1.73E+11	0	-0.1316	0.3865346	-0.34	0.022	0.4497306	0.05
dent27	0.0142	1.01E+11	0	-0.0133	0.4810213	-0.03	0.1311	1.35E+11	0	0.0329	0.4090277	0.08	0.0522	0.4669473	0.11
dent30	0.1132	0.4444667	0.25	-0.1867	1.31E+12	0	-0.0004	2.23E+11	0	0.0431	0.3360463	0.13	0.1032	0.3929244	0.26
dent31	-0.1587	1.20E+11	0	-0.2062	7.13E+12	0	-0.0742	1.49E+11	0	0.1969	0.3914075	0.5	0.2416	0.4625335	0.52
dent32	0.1749	1.66E+11	0	0.0298	6.26E+10	0	0.1552	0.3511204	0.44	0.2164	0.3372387	0.64	0.215	0.4000204	0.54
dent33	0.0453	8.99E+10	0	-0.0207	5.33E+12	0	0.2426	1.19E+11	0	0.3217	0.3870521	0.83	0.3097	0.4745297	0.65
_cons	-0.0002	1.043592	0	-0.1228	0.9251655	-0.13	-0.0305	0.79288	-0.04	-0.0592	0.7021394	-0.08	0.2028	0.8659093	0.23
dent19	-0.0981	4.39E+10	0	-0.1655	2.63E+12	0	0.0229	5.86E+10	0	0.1342	0.422547	0.32	0.3541	0.5052972	0.7
dent29	-12.793	1.68E+11	0	-11.949	1.24E+12	0	-11.556	2.08E+11	0	-10.813	0.36	-30.01	-11.092	0.4139089	-26.8

Table A18: Effect on Birthweight linear IV FeA

Effect of HC Participation on Child's Birthweight Using linear IV – FeA

	1	2	3	4	5	6	7	8	9	10
VARIABLES	OLS	IV: All Instruments	IV: Capacity	IV: Fee	IV: Distance	OLS	IV: All Instruments	IV: Capacity	IV: Fee	IV: Distance
exposure	0.0202	0.399	-0.0947	-0.331	1.01					
	[0.0932]	[0.566]	[0.851]	[1.211]	[0.689]					
asis_hc						-0.0136	0.384	0.0415	-0.59	0.539
						[0.0627]	[0.359]	[0.808]	[1.643]	[0.384]
female	-0.0936**	-0.0956***	-0.0929**	-0.0917**	-0.0989**	-0.0933**	-0.0989***	-0.0940**	-0.0851**	-0.101**
	[0.0361]	[0.0366]	[0.0364]	[0.0357]	[0.0394]	[0.0362]	[0.0368]	[0.0379]	[0.0399]	[0.0380]
age_m	0.000987	-0.00187	0.00185	0.00363	-0.00647	0.00144	-0.00724	0.000234	0.014	-0.0106
	[0.00398]	[0.00552]	[0.00649]	[0.00892]	[0.00711]	[0.00381]	[0.00877]	[0.0170]	[0.0348]	[0.00989]
age_m2	-0.00292	-0.000857	-0.00355	-0.00483	0.00247	-0.00337	0.00658	-0.00199	-0.0178	0.0105
	[0.00452]	[0.00521]	[0.00555]	[0.00718]	[0.00644]	[0.00449]	[0.00999]	[0.0195]	[0.0402]	[0.0112]
ln_age_h	0.00614	0.0148	0.00353	-0.00185	0.0287	0.0057	0.00509	0.00562	0.0066	0.00485
	[0.0790]	[0.0768]	[0.0759]	[0.0771]	[0.0842]	[0.0787]	[0.0785]	[0.0791]	[0.0827]	[0.0819]
ln_age_m	0.183*	0.194**	0.180*	0.174*	0.211**	0.182*	0.205**	0.185*	0.149	0.214**
	[0.0990]	[0.0978]	[0.0985]	[0.100]	[0.103]	[0.0993]	[0.0987]	[0.103]	[0.130]	[0.103]
height_mot	0.449	0.478*	0.441	0.423	0.524*	0.446	0.510*	0.454	0.353	0.535*
	[0.269]	[0.272]	[0.291]	[0.280]	[0.293]	[0.270]	[0.274]	[0.323]	[0.371]	[0.280]
ln_order	-0.0223	-0.0362	-0.0181	-0.00938	-0.0586	-0.0211	-0.0334	-0.0228	-0.00328	-0.0382
	[0.0451]	[0.0485]	[0.0518]	[0.0502]	[0.0555]	[0.0449]	[0.0461]	[0.0490]	[0.0574]	[0.0488]
edu_m_b	-0.0473	-0.0497	-0.0466	-0.0451	-0.0537	-0.0474	-0.0408	-0.0465	-0.057	-0.0382
	[0.0638]	[0.0637]	[0.0645]	[0.0626]	[0.0677]	[0.0641]	[0.0655]	[0.0641]	[0.0702]	[0.0690]
edu_m_c	-0.108	-0.0975	-0.111	-0.117	-0.0812	-0.109	-0.0795	-0.105	-0.152	-0.0679
	[0.0796]	[0.0764]	[0.0798]	[0.0955]	[0.0791]	[0.0809]	[0.0778]	[0.0915]	[0.166]	[0.0817]
edu_h_b	0.0264	0.0228	0.0275	0.0297	0.017	0.0269	0.0175	0.0256	0.0406	0.0138
	[0.0494]	[0.0478]	[0.0500]	[0.0527]	[0.0507]	[0.0494]	[0.0503]	[0.0523]	[0.0673]	[0.0537]
edu_h_c	0.0351	0.0353	0.035	0.0349	0.0357	0.0356	0.0219	0.0337	0.0554	0.0165
	[0.0961]	[0.0925]	[0.0966]	[0.0981]	[0.0957]	[0.0963]	[0.0962]	[0.103]	[0.129]	[0.0995]
centretown	0.0191	0.00717	0.0227	0.0301	-0.012	0.0203	0.00244	0.0178	0.0462	-0.00455
	[0.0569]	[0.0657]	[0.0665]	[0.0647]	[0.0742]	[0.0569]	[0.0628]	[0.0730]	[0.0968]	[0.0645]
time_hea	-0.0815	-0.016	-0.101	-0.142	0.0895	-0.0868	-0.0347	-0.0796	-0.162	-0.0143
	[0.172]	[0.196]	[0.233]	[0.264]	[0.225]	[0.173]	[0.178]	[0.209]	[0.275]	[0.185]
time_hea2	0.187	0.135	0.203	0.235	0.0519	0.192	0.13	0.183	0.282	0.106
	[0.165]	[0.181]	[0.219]	[0.238]	[0.190]	[0.167]	[0.173]	[0.224]	[0.315]	[0.174]
time_sch	0.35	0.497	0.305	0.214	0.735*	0.335	0.552	0.365	0.0199	0.637*
	[0.293]	[0.363]	[0.438]	[0.590]	[0.396]	[0.297]	[0.340]	[0.527]	[0.974]	[0.349]
time_sch2	0.0628	-0.06	0.1	0.177	-0.258	0.078	-0.174	0.0431	0.443	-0.273
	[0.384]	[0.393]	[0.456]	[0.590]	[0.392]	[0.388]	[0.403]	[0.612]	[1.153]	[0.419]
time_hea_sch	-0.408	-0.408	-0.408	-0.408	-0.408	-0.406	-0.458*	-0.413	-0.331	-0.478*
	[0.273]	[0.253]	[0.279]	[0.292]	[0.255]	[0.275]	[0.265]	[0.283]	[0.404]	[0.276]
time_alc	-0.0103	-0.047	0.000821	0.0237	-0.106	-0.00724	-0.0397	-0.0117	0.0398	-0.0524
	[0.111]	[0.126]	[0.145]	[0.134]	[0.155]	[0.110]	[0.126]	[0.139]	[0.154]	[0.136]
time_alc2	0.0279	0.0297	0.0273	0.0261	0.0328	0.0281	0.0193	0.0268	0.0408	0.0158
	[0.0375]	[0.0359]	[0.0376]	[0.0369]	[0.0377]	[0.0377]	[0.0359]	[0.0418]	[0.0632]	[0.0365]
timealchea	-0.122	-0.0918	-0.132	-0.151	-0.0424	-0.126	-0.067	-0.118	-0.212	-0.0438
	[0.141]	[0.145]	[0.172]	[0.180]	[0.143]	[0.143]	[0.147]	[0.202]	[0.300]	[0.145]
timealcsch	0.25	0.267	0.245	0.234	0.294	0.245	0.346	0.259	0.0998	0.385
	[0.327]	[0.307]	[0.333]	[0.361]	[0.314]	[0.330]	[0.330]	[0.370]	[0.593]	[0.350]
time_sch_mun	0.149	0.128	0.156	0.169	0.0943	0.147	0.236	0.16	0.0189	0.27
	[0.576]	[0.549]	[0.586]	[0.588]	[0.558]	[0.582]	[0.535]	[0.599]	[0.818]	[0.545]
time_hea_mun	-0.457**	-0.437**	-0.463**	-0.476**	-0.404**	-0.458**	-0.450***	-0.457**	-0.470**	-0.447**
	[0.181]	[0.176]	[0.196]	[0.208]	[0.173]	[0.182]	[0.168]	[0.184]	[0.213]	[0.169]
time_alc_mun	0.466***	0.465***	0.466***	0.467***	0.463***	0.466***	0.465***	0.466***	0.468***	0.464***
	[0.120]	[0.113]	[0.123]	[0.128]	[0.112]	[0.121]	[0.109]	[0.120]	[0.143]	[0.110]
hosp	0.0146	0.0219	0.0124	0.00785	0.0337	0.0147	0.00157	0.0129	0.0337	-0.00355
	[0.0636]	[0.0629]	[0.0709]	[0.0774]	[0.0623]	[0.0625]	[0.0603]	[0.0607]	[0.0669]	[0.0635]
pipe	-0.0323	-0.0407	-0.0298	-0.0245	-0.0543	-0.0306	-0.0671	-0.0357	0.0223	-0.0814

Table A19: Effect on Birthweight linear IV FeA:continued

	[0.160]	[0.155]	[0.162]	[0.167]	[0.161]	[0.160]	[0.149]	[0.178]	[0.251]	[0.148]
sewage	0.288***	0.287***	0.288***	0.288***	0.287***	0.289***	0.256***	0.284***	0.335**	0.244***
	[0.0832]	[0.0773]	[0.0846]	[0.0880]	[0.0766]	[0.0839]	[0.0806]	[0.103]	[0.155]	[0.0843]
insur_mun	-0.917**	-0.874**	-0.931**	-0.958**	-0.804*	-0.920**	-0.918**	-0.919**	-0.923*	-0.917**
	[0.404]	[0.394]	[0.434]	[0.460]	[0.421]	[0.402]	[0.376]	[0.399]	[0.473]	[0.387]
insur_mun2	0.879**	0.805**	0.901*	0.947*	0.686*	0.884**	0.851**	0.879**	0.931*	0.839**
	[0.388]	[0.388]	[0.452]	[0.506]	[0.400]	[0.384]	[0.358]	[0.397]	[0.480]	[0.364]
numchildren	-0.015	-0.0202	-0.0135	-0.0103	-0.0285	-0.0154	0.00313	-0.0128	-0.0423	0.0104
	[0.0845]	[0.0846]	[0.0912]	[0.0993]	[0.0893]	[0.0830]	[0.0771]	[0.0744]	[0.0798]	[0.0833]
altitud	-0.0426	-0.00896	-0.0528	-0.0737	0.0452	-0.0458	-0.00296	-0.0399	-0.108	0.0138
	[0.115]	[0.116]	[0.133]	[0.138]	[0.130]	[0.112]	[0.116]	[0.141]	[0.191]	[0.122]
altitud2	-0.00347	-0.0122	-0.000837	0.00458	-0.0262	-0.00266	-0.0128	-0.00406	0.012	-0.0167
	[0.0381]	[0.0370]	[0.0408]	[0.0387]	[0.0410]	[0.0373]	[0.0371]	[0.0414]	[0.0455]	[0.0388]
wage_fu	0.175*	0.189**	0.171*	0.163*	0.210**	0.175*	0.170*	0.174*	0.182*	0.168
	[0.0951]	[0.0933]	[0.100]	[0.0906]	[0.101]	[0.0963]	[0.0984]	[0.0971]	[0.108]	[0.104]
wage_fr	-0.0764	-0.0751	-0.0768	-0.0776	-0.073	-0.0773	-0.0528	-0.0739	-0.113	-0.0433
	[0.104]	[0.102]	[0.105]	[0.107]	[0.107]	[0.106]	[0.106]	[0.120]	[0.165]	[0.109]
price_index	0.0877	-0.0517	0.13	0.217	-0.276	0.102	-0.0923	0.0749	0.383	-0.168
	[0.191]	[0.278]	[0.386]	[0.476]	[0.312]	[0.187]	[0.244]	[0.455]	[0.815]	[0.247]
Observations	1371	1371	1371	1371	1371	1371	1371	1371	1371	1371
R-squared	0.092	0.079	0.09	0.081	0.009	0.092	0.058	0.091	0.022	0.027

Robust standard errors in brackets

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

Table A20: Effect on Birthweight Nonlinear IV FeA

Effect of HC Participation on Child's Birthweight Using nonlinear IV - FeA

	1	2	3	4	5	6	7	8	9	10
VARIABLES	OLS	IV: All Instruments	IV: Capacity	IV: Fee	IV: Distance	OLS	IV: All Instruments	IV: Capacity	IV: Fee	IV: Distance
exposure	0.0202	0.178	-0.196	-0.0156	0.439					
	[0.0932]	[0.495]	[0.644]	[0.747]	[0.590]					
asis_hc						-0.0136	0.175	0.235	0.345	0.223
						[0.0627]	[0.230]	[0.301]	[0.299]	[0.239]
female	-0.0936**	-0.0944**	-0.0924**	-0.0934**	-0.0958**	-0.0933**	-0.0959**	-0.0968**	-0.0983***	-0.0966**
	[0.0361]	[0.0368]	[0.0360]	[0.0359]	[0.0373]	[0.0362]	[0.0370]	[0.0367]	[0.0364]	[0.0371]
age_m	0.000987	-0.000202	0.00261	0.00126	-0.00217	0.00144	-0.00269	-0.00399	-0.0064	-0.00373
	[0.00398]	[0.00494]	[0.00495]	[0.00561]	[0.00567]	[0.00381]	[0.00611]	[0.00651]	[0.00660]	[0.00668]
age_m2	-0.00292	-0.00206	-0.0041	-0.00312	-0.00064	-0.00337	0.00136	0.00285	0.0056	0.00255
	[0.00452]	[0.00487]	[0.00463]	[0.00501]	[0.00527]	[0.00449]	[0.00703]	[0.00770]	[0.00781]	[0.00766]
ln_age_h	0.00614	0.00974	0.00123	0.00533	0.0157	0.0057	0.00541	0.00532	0.00515	0.00534
	[0.0790]	[0.0766]	[0.0753]	[0.0761]	[0.0787]	[0.0787]	[0.0792]	[0.0795]	[0.0802]	[0.0794]
ln_age_m	0.183*	0.188*	0.177*	0.182*	0.195*	0.182*	0.193*	0.196*	0.202*	0.196*
	[0.0990]	[0.100]	[0.0999]	[0.102]	[0.103]	[0.0993]	[0.100]	[0.101]	[0.103]	[0.102]
height_mot	0.449	0.461	0.433	0.447	0.481*	0.446	0.476*	0.486*	0.503*	0.484*
	[0.269]	[0.276]	[0.285]	[0.277]	[0.279]	[0.270]	[0.276]	[0.285]	[0.282]	[0.274]
ln_order	-0.0223	-0.0281	-0.0143	-0.021	-0.0377	-0.0211	-0.027	-0.0288	-0.0322	-0.0284
	[0.0451]	[0.0463]	[0.0467]	[0.0457]	[0.0499]	[0.0449]	[0.0454]	[0.0440]	[0.0442]	[0.0461]
edu_m_b	-0.0473	-0.0483	-0.0459	-0.0471	-0.05	-0.0474	-0.0442	-0.0433	-0.0414	-0.0435
	[0.0638]	[0.0646]	[0.0642]	[0.0641]	[0.0654]	[0.0641]	[0.0652]	[0.0654]	[0.0667]	[0.0658]
edu_m_c	-0.108	-0.103	-0.113	-0.109	-0.0964	-0.109	-0.0951	-0.0906	-0.0824	-0.0915
	[0.0796]	[0.0800]	[0.0808]	[0.0841]	[0.0803]	[0.0809]	[0.0802]	[0.0790]	[0.0840]	[0.0818]
edu_h_b	0.0264	0.0249	0.0285	0.0268	0.0225	0.0269	0.0225	0.021	0.0184	0.0213
	[0.0494]	[0.0492]	[0.0496]	[0.0494]	[0.0494]	[0.0494]	[0.0502]	[0.0498]	[0.0506]	[0.0505]
edu_h_c	0.0351	0.0352	0.035	0.0351	0.0354	0.0356	0.0291	0.027	0.0232	0.0274
	[0.0961]	[0.0954]	[0.0973]	[0.0962]	[0.0949]	[0.0963]	[0.0969]	[0.0969]	[0.0991]	[0.0965]
centretown	0.0191	0.0141	0.0258	0.0202	0.00591	0.0203	0.0118	0.00914	0.00419	0.00968
	[0.0569]	[0.0635]	[0.0621]	[0.0611]	[0.0657]	[0.0569]	[0.0615]	[0.0621]	[0.0601]	[0.0609]
time_hea	-0.0815	-0.0542	-0.119	-0.0877	-0.00911	-0.0868	-0.062	-0.0542	-0.0398	-0.0558
	[0.172]	[0.181]	[0.201]	[0.193]	[0.178]	[0.173]	[0.176]	[0.180]	[0.176]	[0.174]
time_hea2	0.187	0.165	0.216	0.192	0.13	0.192	0.162	0.153	0.136	0.155
	[0.165]	[0.163]	[0.180]	[0.161]	[0.153]	[0.167]	[0.168]	[0.166]	[0.159]	[0.163]
time_sch	0.35	0.412	0.266	0.336	0.513	0.335	0.438	0.471	0.531	0.464
	[0.293]	[0.362]	[0.394]	[0.446]	[0.389]	[0.297]	[0.320]	[0.344]	[0.353]	[0.324]
time_sch2	0.0628	0.0117	0.133	0.0744	-0.073	0.078	-0.0418	-0.0795	-0.149	-0.0719
	[0.384]	[0.432]	[0.469]	[0.520]	[0.446]	[0.388]	[0.416]	[0.438]	[0.464]	[0.430]
time_hea_sch	-0.408	-0.408	-0.408	-0.408	-0.408	-0.406	-0.431	-0.438	-0.453	-0.437
	[0.273]	[0.267]	[0.284]	[0.275]	[0.259]	[0.275]	[0.276]	[0.278]	[0.283]	[0.278]
time_alc	-0.0103	-0.0256	0.0106	-0.00684	-0.0509	-0.00724	-0.0227	-0.0275	-0.0365	-0.0265
	[0.111]	[0.113]	[0.127]	[0.112]	[0.115]	[0.110]	[0.118]	[0.123]	[0.121]	[0.117]
time_alc2	0.0279	0.0286	0.0268	0.0277	0.0299	0.0281	0.0239	0.0226	0.0201	0.0228
	[0.0375]	[0.0370]	[0.0379]	[0.0368]	[0.0369]	[0.0377]	[0.0371]	[0.0373]	[0.0385]	[0.0373]
timealchea	-0.122	-0.11	-0.14	-0.125	-0.0886	-0.126	-0.098	-0.0892	-0.0727	-0.0909
	[0.141]	[0.136]	[0.149]	[0.134]	[0.125]	[0.143]	[0.141]	[0.139]	[0.132]	[0.137]
timealcsch	0.25	0.257	0.24	0.248	0.268	0.245	0.293	0.308	0.336	0.305
	[0.327]	[0.326]	[0.346]	[0.343]	[0.320]	[0.330]	[0.342]	[0.350]	[0.366]	[0.350]
time_sch_mun	0.149	0.14	0.161	0.151	0.126	0.147	0.189	0.203	0.227	0.2
	[0.576]	[0.570]	[0.592]	[0.579]	[0.563]	[0.582]	[0.558]	[0.545]	[0.539]	[0.549]
time_hea_mun	-0.457**	-0.448**	-0.468**	-0.459**	-0.435**	-0.458**	-0.454**	-0.453**	-0.451**	-0.453**
	[0.181]	[0.183]	[0.193]	[0.192]	[0.179]	[0.182]	[0.177]	[0.176]	[0.174]	[0.175]
time_alc_mun	0.466***	0.465***	0.467***	0.466***	0.465***	0.466***	0.465***	0.465***	0.465***	0.465***

Table A21: Effect on Birthweight Nonlinear IV FeA:continued

	[0.120]	[0.118]	[0.125]	[0.122]	[0.115]	[0.121]	[0.116]	[0.115]	[0.113]	[0.115]
hosp	0.0146	0.0176	0.0104	0.0139	0.0227	0.0147	0.00844	0.00648	0.00285	0.00687
	[0.0636]	[0.0644]	[0.0682]	[0.0677]	[0.0620]	[0.0625]	[0.0615]	[0.0605]	[0.0598]	[0.0628]
pipe	-0.0323	-0.0358	-0.0275	-0.0315	-0.0416	-0.0306	-0.048	-0.0534	-0.0635	-0.0523
	[0.160]	[0.160]	[0.162]	[0.161]	[0.159]	[0.160]	[0.153]	[0.151]	[0.147]	[0.150]
sewage	0.288***	0.287***	0.288***	0.288***	0.287***	0.289***	0.273***	0.269***	0.260***	0.270***
	[0.0832]	[0.0814]	[0.0861]	[0.0837]	[0.0790]	[0.0839]	[0.0824]	[0.0865]	[0.0877]	[0.0835]
insur_mun	-0.917**	-0.899**	-0.942**	-0.921**	-0.869**	-0.920**	-0.919**	-0.918**	-0.918**	-0.918**
	[0.404]	[0.406]	[0.433]	[0.430]	[0.401]	[0.402]	[0.390]	[0.388]	[0.386]	[0.388]
insur_mun2	0.879**	0.848**	0.921**	0.886*	0.797**	0.884**	0.868**	0.864**	0.855**	0.865**
	[0.388]	[0.400]	[0.441]	[0.444]	[0.391]	[0.384]	[0.372]	[0.372]	[0.370]	[0.367]
numchildren	-0.015	-0.0172	-0.0121	-0.0145	-0.0208	-0.0154	-0.00658	-0.00381	0.00132	-0.00437
	[0.0845]	[0.0864]	[0.0899]	[0.0886]	[0.0845]	[0.0830]	[0.0789]	[0.0788]	[0.0803]	[0.0818]
altitud	-0.0426	-0.0286	-0.0617	-0.0458	-0.00542	-0.0458	-0.0255	-0.019	-0.00715	-0.0203
	[0.115]	[0.115]	[0.121]	[0.122]	[0.121]	[0.112]	[0.111]	[0.115]	[0.114]	[0.113]
altitud2	-0.00347	-0.0071	0.00148	-0.00265	-0.0131	-0.00266	-0.00746	-0.00897	-0.0118	-0.00867
	[0.0381]	[0.0371]	[0.0384]	[0.0379]	[0.0388]	[0.0373]	[0.0364]	[0.0373]	[0.0369]	[0.0370]
wage_fu	0.175*	0.181*	0.168*	0.174*	0.190*	0.175*	0.173*	0.172*	0.171*	0.172*
	[0.0951]	[0.0936]	[0.0977]	[0.0925]	[0.0950]	[0.0963]	[0.0981]	[0.0987]	[0.101]	[0.0985]
wage_fr	-0.0764	-0.0758	-0.0771	-0.0765	-0.0749	-0.0773	-0.0657	-0.062	-0.0552	-0.0627
	[0.104]	[0.104]	[0.106]	[0.105]	[0.104]	[0.106]	[0.107]	[0.109]	[0.109]	[0.106]
price_index	0.0877	0.0296	0.167	0.101	-0.0664	0.102	0.00947	-0.0195	-0.0734	-0.0137
	[0.191]	[0.258]	[0.323]	[0.344]	[0.272]	[0.187]	[0.218]	[0.255]	[0.242]	[0.216]
Observations	1371	1371	1371	1371	1371	1371	1371	1371	1371	1371
R-squared	0.092	0.079	0.09	0.081	0.009	0.092	0.058	0.091	0.022	0.027

Robust standard errors in brackets

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

Table A22: Effect on Birthweight ENDS

Effect of HC Participation on Child's birthweight Using linear and nonlinear IV - ENDS

	1	2	3	4	5	6
VARIABLES	OLS. DHS	OLS. DHS	Non-Linear IV: Capacity. DHS	Non-Linear IV: Capacity. DHS	Linear IV: Capacity. DHS	Linear IV: Capacity. DHS
exposure	0.00695		-0.249		-1.558*	
	[0.0812]		[0.402]		[0.872]	
asis_hc		-0.0101		-0.0553		-0.875*
		[0.0287]		[0.169]		[0.515]
female	-0.127***	-0.128***	-0.128***	-0.128***	-0.132***	-0.136***
	[0.0225]	[0.0224]	[0.0226]	[0.0223]	[0.0252]	[0.0267]
age_m	0.00116	0.0014	0.00217	0.00225	0.00731	0.0176*
	[0.00287]	[0.00289]	[0.00338]	[0.00456]	[0.00452]	[0.0103]
age_m2	-1.10E-05	-1.28E-05	-1.47E-05	-1.95E-05	-3.39E-05	-0.000143
	[4.50e-05]	[4.49e-05]	[4.57e-05]	[5.51e-05]	[4.78e-05]	[9.52e-05]
ln_age_h	-0.0451	-0.0461	-0.0495	-0.0494	-0.0718	-0.110*
	[0.0431]	[0.0433]	[0.0430]	[0.0434]	[0.0484]	[0.0605]
ln_age_m	0.0303	0.0298	0.0216	0.0278	-0.0228	-0.00947
	[0.0853]	[0.0848]	[0.0876]	[0.0865]	[0.0903]	[0.0955]
height_mot	1.226***	1.227***	1.237***	1.234***	1.290***	1.366***
	[0.216]	[0.215]	[0.216]	[0.216]	[0.241]	[0.266]
ln_order	0.104***	0.104***	0.107***	0.104***	0.120***	0.101***
	[0.0330]	[0.0328]	[0.0333]	[0.0328]	[0.0344]	[0.0374]
edu_m_b	0.252**	0.252**	0.259**	0.254**	0.299**	0.288**
	[0.0978]	[0.0982]	[0.101]	[0.0995]	[0.128]	[0.131]
edu_m_c	0.290***	0.291***	0.297***	0.294***	0.332**	0.346**
	[0.0998]	[0.100]	[0.102]	[0.101]	[0.130]	[0.142]
edu_h_b	-0.078	-0.0784	-0.0791	-0.0792	-0.0845	-0.0931
	[0.0519]	[0.0519]	[0.0516]	[0.0526]	[0.0516]	[0.0595]
edu_h_c	-0.0806	-0.0816	-0.0819	-0.0824	-0.0884	-0.0967
	[0.0666]	[0.0666]	[0.0671]	[0.0675]	[0.0708]	[0.0778]
pipe	0.0799	0.0796	0.0727	0.0786	0.0361	0.0598
	[0.111]	[0.111]	[0.113]	[0.111]	[0.125]	[0.124]
sewage	-0.0748	-0.0737	-0.0693	-0.0747	-0.0414	-0.0934
	[0.132]	[0.132]	[0.135]	[0.132]	[0.156]	[0.164]
numchildren	-0.00213	-0.0024	-0.00158	-0.00253	0.00124	-0.0049
	[0.0108]	[0.0108]	[0.0112]	[0.0108]	[0.0133]	[0.0117]
altitud	0.205*	0.204*	0.203*	0.203*	0.19	0.196
	[0.120]	[0.120]	[0.121]	[0.120]	[0.136]	[0.140]
altitud2	-0.0763	-0.0759	-0.0758	-0.0761	-0.0731	-0.0794
	[0.0513]	[0.0512]	[0.0513]	[0.0510]	[0.0576]	[0.0596]
sisben2	0.0261	0.026	0.0236	0.0248	0.0108	0.0024
	[0.0526]	[0.0524]	[0.0518]	[0.0518]	[0.0514]	[0.0529]
sisben3	0.0232	0.0234	0.0208	0.0212	0.00864	-0.0182
	[0.0573]	[0.0573]	[0.0564]	[0.0562]	[0.0583]	[0.0616]
Observations	2093	2097	2093	2097	2093	2097
R-squared	0.076	0.076	0.071	0.075		

Robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

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