Health Spillover Effects of a Conditional Cash Transfer Program*

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October 2013

Abstract

While the primary aim of most Conditional Cash Transfer (or CCT) programs is to increase the level of human capital of children in poor households, there are also potentially large spillover effects associated with such programs. We illustrate this using data from the *Familias en Acción* (FA) program that has been in operation in Colombia for almost a decade. The health and nutrition component of the program was targeted at households with at least one child aged between 0 and 5. We find significant improvements in the health of non-targeted adults in treatment households, both in terms of incidence of illness (in the short and the medium run) and on the severity of illness (over the medium run). The main mechanism behind this effect appears to be the availability of better information and creation of health public goods within the household as a result of this program.

Key Words: Conditional Cash Transfer, Health Spillovers, Colombia, *Familias en Acción*

JEL Classification Codes: I15, I38, O1, C33

*We would like to thank Sarah Baird, seminar participants at the Centre for Health Economics at Monash University, participants at the Australasian Econometric Society Meetings and at the Australian Health Economic Society Meetings for their comments and suggestions.

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1 Introduction

Policy makers in developing countries around the world are increasingly using conditional cash transfers (henceforth CCTs) to improve the health, nutritional and educational outcomes of children in poor households. Indeed the Economist terms CCTs as the *world's favourite new anti-poverty device*. See WorldBank (2009), DFID (2011), Baird, McIntosh, and Özler (2011) among others for more on CCT programs. These schemes give stipends and food to the poorest if they meet specific conditions (for example their children attend school, or their babies are vaccinated). The main idea behind such CCTs is to target children living in these poor households by investing in the human capital of children early enough in order to facilitate proper mental and physical development of these children; healthier and better educated children are likely to be more productive adults, thereby breaking the vicious cycle that perpetuates poverty over generations.

In this paper we examine the indirect effects or spillovers associated with CCT programs. While it is now accepted that CCT programs have significant direct effects[1] on child nutrition[2], child health[3], school participation[4] and consumption[5], indirect or spillover effects associated in such programs have been less studied. Ignoring these effects may in fact lead to an underestimation of the total effect of the program. We examine whether CCT programs affect the health outcomes of others in the household who are not the direct beneficiaries of the program (for example parents, grandparents and other family members/friends residing in the household).

We provide evidence of this kind of spillover using data from the *Familias en Acción* (henceforth FA) program, a CCT program that has been in operation in Colombia since 2002. The overall aim of the program is to increase the level of human capital (health, nutrition and educational attainment) of children in the poorest households of the country, by providing monetary transfers to primary caregivers (or *titulars)* in beneficiary families,

[1] See Lagarde, Haynes, and Palmer (2007) for a survey.

[2] Behrman and Hoddinott (2005); Attanasio and Mesnard (2006).

[3] Gertler and Boyce (2001); Gertler (2004); Attanasio, Gomez, Gomez Rojas, and Vera-Hernandez (2004); Attanasio, Gomez, Heredia, and Vera-Hernandez (2005b).

[4] Baez and Camacho (2011); Attanasio, Fitzsimons, and Gomez (2005a); Fitzsimons and Mesnard (2008); Attanasio, Fitzsimons, Gomez, Gutierrez, Meghir, and Mesnard (2010).

[5] Maluccio and Flores (2005); Attanasio, Battistin, Fitzsimons, and Vera-Hernandez (2005c).

conditional on having completed specific requirements: (a) children under 6 should be taken to health centres for health and development check-ups, and *titulars* had to attend sessions on nutrition, hygiene and contraception; and (b) children between 6 and 17 years old should regularly attend school.[6] The health and nutrition component of the program was targeted at households with at least one child aged 0 and 5. Each eligible household that attended a health check-up every two months received a flat-rate monthly monetary supplement of 46,500 pesos (approximately US $20.45 at the 2002 exchange rate) irrespective of the number of children aged $0-5$. We focus on the spillover effects associated with the health and nutrition component of the program. There is evidence that the program directly affected the health and nutrition status of young children. Attanasio, Gomez, Heredia, and Vera-Hernandez (2005b) find that the program reduced the occurrence of diarrhoea from 32.6 to 22 percent for children less than 24 months and from 21.3 to 10.4 percent for children aged $24-48$ months, living in rural areas. Additionally they argue that 12-month old boys grew 0.44 centimetres more than if the program had not been operational.

We take advantage of the design of the FA program to identify its effects on those members of the household who are not the direct beneficiaries. Specifically we examine the effects of the program on the health of adults (individuals aged 18 and higher at the baseline). In the case of the FA program, there are unlikely to be any direct effects on the health of adults. Adults, other than the *titulars*, were not required to attend health information sessions and only the *titulars* received useful advice about nutrition and the prevention of common diseases. Any treatment effect on the health of non-targeted members could therefore be viewed as pure externality arising from the program and it is this kind of within household spillovers that is the focus of this paper.

There can of course be a number of different pathways through which this within household spillovers can arise: *income effects*, *public good effects* and *contagion effects* (to name three). The cash transfer component of the FA program frees up resources for other

[6]The *titular* is the actual recipient of the subsidy. In most cases it is the mother of the child, though there are cases when the actual recipient is the father or the grandmother. This is particularly true if the mother does not reside in the household. Specifically around 82% of the *titulars* are the mother of the child and 95% of the *titulars* are females.

members creating an income effect; makes information about healthy practices available within the household that can be used by all members creating a household public good effect; and generates a positive contagion as a result of healthier behaviours and more hygienic surroundings. All three effects are expected to be positive and mutually reinforcing, resulting in a positive spillover effect.

We find that there are indeed strong spillover effects within households. In the short run, the strongest effects are on incidence of illness.[7] Non-targeted individuals (adults) in treatment households were significantly less likely to be ill in the 15-days prior to the survey compared to adults in control households. The effects persist over a longer period of time and indeed over time it leads to better long term health and a reduction in the severity of illness, captured by lower rates of hospitalization. Additionally we find that the effects are quite heterogeneous. Our results suggest that it is household level public goods and contagion and not a relaxation of the household budget constraint as a result of the cash transfer that is driving the results. From the policy point of view therefore simply looking at the direct effects results in significant underestimation of the effect of such CCT programs.

2 Relation to the Existing Literature

It is not that the literature has not recognized the possibility of spillover effects arising from CCT programs. The focus however has been primarily on spillovers across households. The literature has identified several possible reasons for spillovers between treated (eligible) and non-treated (ineligible) households living in the same community. This includes direct transfers from treated to non-treated households in the form of gifts or other transfers (Angelucci and De Giorgi, 2009), an increase in overall incomes (Angelucci and De Giorgi, 2009), learning from peer interaction (Bobonis and Finan, 2009; Lalive and Cattaneo, 2009), the desire to behave like the eligible population in the hope that they would become eligible (particularly true when the eligibility criteria are not particularly well defined

[7] Unfortunately we are somewhat restricted in terms of available health measures in the survey. We discuss the advantages and disadvantages of using the self-reported measures that we do in this paper in section 4.

within the treated community). In the context of health, the effect is expected to happen through changes in behavior pertaining to sanitation, hygiene, health practices and lifestyle or through the reduction in epidemics in the whole population (see Miguel and Kremer, 2004). While Gertler (2004) and Attanasio, Gomez, Heredia, and Vera-Hernandez (2005b) find evidence of significant impact of CCT programs on targeted children, spillover health effects on neigbouring, ineligible households in the same community have not been adequately explored. One exception is Avitabile (2011) who finds some evidence of an increase in preventive cervical cancer screening among women in ineligible households.

Evidence on within household spillovers is however more scarce. Gertler and Boyce (2001) examine the impact of *PROGRESA* on adults' health and find that there are significant positive short run effects on self-reported health status, specifically the number of kilometers the adult is able to walk without getting tired, reduction in the number of days of difficulty with daily activities due to illness and the number of days in bed because of illness. Behrman and Parker (2013) examine the long run effects of *PROGRESA*, specifically on the self-reported health status and demand for medical services by adults, and find a significant positive effect, particularly for women. The program conditions (regular checkups for adults and the attendance at health information sessions, particularly for women who are much more likely to attend these sessions than men) are some of the potential channels through which the program would affect adults' health.[8] However the indirect effects that we are interested here are different from those discussed by Gertler and Boyce (2001) and Behrman and Parker (2013). In *PROGRESA* all adults in the treatment households were required to attend health information sessions and get regular medical checkups as a part of the program requirements. Any effect on the health of adults could therefore be thought of as a direct program effect. So to look at true spillovers, we need to look beyond the literature that uses data from the *PROGRESA* program.

Chaudhuri (2009) using data from Bangladesh, finds a significantly positive spillover impact of a particular reproductive health program that targeted only mothers and children

[8]Income effect as a result of the CCT increasing the household income, increased flow of resources and increased bargaining power at the margin to women tending to lead to more emphasis on using a given level of resources for health and nutrition than for other uses; and the changed incentives for time use for school-aged children result in reallocation of time uses for adults are some of the other possible channels through which the effect could potentially operate.

in randomly selected treatment areas, on the health of the never-targeted elderly women. Ploeg (2009) finds that in the US children who are age-ineligible for WIC (Women Income Children) program but live in WIC-participating families have healthier diets than similar children in nonparticipating families. Bustelo (2010) examines the spillover effects associated with Nicaragua's *Red de Proteccion* Social CCT program and finds that while the program targets specifically children aged $7-13$ who have not completed 4^{th} grade, there are positive schooling effects within the households for older, non-targeted siblings, with higher impacts for boys than girls. Kazianga, de Walque, and Alderman (2013) evaluate the impact of two different school feeding programs (school lunches and take home rations) on the health outcomes of pre-school children in Burkina Faso. They find that take home rations have a significant impact on the health of younger siblings within the household. The pre-school children were not directly eligible and therefore any impact on the health of these children could be viewed as a spillover, attained through intra-household reallocation of food. Indeed as in our case, Kazianga, de Walque, and Alderman (2013) argue that ignoring such spillovers under-estimates the overall effect of the intervention.[9]

3 Theoretical Framework

To explain the mechanisms by which a spillover effect might result, we use a standard unitary model developed by Behrman and Deolalikar (1988) and used by Chaudhuri (2009). Consider a household with n members. The utility function that defines the preferences of the household is well behaved and can be written as:

$$U_j = U_j(H_{ij}, X_{ij}, Z_{ij}) \tag{1}$$

where U_j is the utility of the j^{th} household, H_{ij} represents the vector of the health of individuals $i = 1, 2, \ldots, n$ in household j and Z_{ij} represents the vector of health inputs and X_{ij} represents the vector of all other consumption goods of household members. Utility maximization is subject to the household budget constraint and the health production functions of all the individuals in the household.

[9]It is not the case that there is *always* evidence of such spillovers within household. Using data from Cambodia, Ferreira, Filmer, and Schady (2009) find that while the CSP program resulted in a significant increase in the likelihood of scholarship recipients being enrolled in school, the school enrolment and work of ineligible siblings was largely unaffected by the program.

The health production function of the household members can be written as:

$$H_{ij} = H(X_{ij}, Z_{ij}, W_j(F), H_{-ij}; \mu) \tag{2}$$

Health production within the household depends on the use of health inputs (Z_{ij}), consumption of all other goods (X_{ij}), household public goods (W_j), health of all other members in the household excluding oneself (H_{-ij}) and all the observed and unobserved endowments of the household (μ). We subdivide the household into two groups: the targeted or T members (for example children aged $0-5$ who are the direct beneficiaries of the program) and the other or O members of the household who are not the targeted beneficiaries. Also $H_{-ij} = \{H_{1j}, \ldots, H_{i-1,j}, H_{i+1,j}, \ldots, H_{nj}\}$ and $H_{ij} \in [H_{ij}^T, H_{ij}^O]$. Health inputs ($Z_{ij}$) depends on health inputs provided by the FA program (z^{FA}) and private health inputs (z^P), so that we can write

$$Z_{ij} = Z(z_{ij}^{FA}(F), z_{ij}^P) \tag{3}$$

Since z^{FA} is only available to targeted individuals residing in the treatment municipalities, it is a function of the health program (F). Likewise household public good (W) is also a function of F, generated when the program is present in the household. Define $F = 1$ when the program is available (for targeted individuals in the treatment municipalities) and $F = 0$ when the program is not available (either for individuals in the control municipalities or for the non-targeted individuals in the treatment municipalities). Then $z_{ij}^{FA}(F) = 0$ if $F = 0$ and $z_{ij}^{FA}(F) > 0$ if $F = 1$. Likewise $W_j(F) = 0$ if $F = 0$ and $W_j(F) > 0$ if $F = 1$.

The household budget constraint when Y is the pooled household income, p_{z^P} and p_x are prices of the private health inputs and consumption goods respectively can be written as:

$$\sum_i p_x X_{ij} + \sum_i p_{z^P} z_{ij}^P = Y + z_{ij}^{FA}(F) \tag{4}$$

Maximizing utility (given by equation (1)) subject to the production constraints (given by equations (2 and 3)) and the budget constraint (given by equation (4)), the reduced form demand functions for health inputs, consumption and outcome variables can be written as:

$$\{H_{ij}^T, H_{ij}^O, Z_{ij}, W_j, X_{ij}\} = f(p_x, p_{z^P}, Y_j; F, \mu_j) \tag{5}$$

Program intervention (through F) that changes any of the right-hand side variables will change the allocation of resources and outcomes within the households to conform to the optimizing allocation. The impact of the program on the targeted and non-targeted population can therefore be written as:

$$\begin{aligned}\frac{\partial H_{ij}^{T}}{\partial F} &= (\frac{\partial H^{T}}{\partial X_{ij}})(\frac{\partial X_{ij}}{\partial F}) + (\frac{\partial H^{T}}{\partial z_{ij}^{FA}})(\frac{\partial z_{ij}^{FA}}{\partial F}) + (\frac{\partial H^{T}}{\partial z_{ij}^{P}})(\frac{\partial z_{ij}^{P}}{\partial F}) \\ &\quad + (\frac{\partial H^{T}}{\partial W_{j}})(\frac{\partial W_{j}}{\partial F}) + (\frac{\partial H^{T}}{\partial H_{-ij}})(\frac{\partial H_{-ij}}{\partial F}) \end{aligned} \tag{6}$$

$$\begin{aligned}\frac{\partial H_{ij}^{O}}{\partial F} &= \underbrace{(\frac{\partial H^{O}}{\partial X_{ij}})(\frac{\partial X_{ij}}{\partial F}) + (\frac{\partial H^{O}}{\partial z_{ij}^{P}})(\frac{\partial z_{ij}^{P}}{\partial F})}_{\text{Income effect}} + \underbrace{(\frac{\partial H^{O}}{\partial W_{j}})(\frac{\partial W_{j}}{\partial F})}_{\text{Household public good effect}} \\ &\quad + \underbrace{(\frac{\partial H^{O}}{\partial H_{-ij}})(\frac{\partial H_{-ij}}{\partial F})}_{\text{Contagion effect}} \end{aligned} \tag{7}$$

The focus of this paper is on spillovers, and therefore we are interested in the effects captured through equation (7). The first two terms $[(\frac{\partial H^{O}}{\partial X_{ij}})(\frac{\partial X_{ij}}{\partial F}) + (\frac{\partial H^{O}}{\partial z_{ij}^{P}})(\frac{\partial z_{ij}^{P}}{\partial F})]$ denote the *income effect*, the third term $[(\frac{\partial H^{O}}{\partial W_{j}})(\frac{\partial W_{j}}{\partial F})]$ denotes the *household public good effect* and the last term $[(\frac{\partial H^{O}}{\partial H_{-ij}})(\frac{\partial H_{-ij}}{\partial F})]$ denotes the *contagion effect*. Health inputs are assumed to be normal goods and under the assumption that private and publicly provided health inputs (z^{P} and z^{FA} respectively) are perfect substitutes, by providing some of the necessary health inputs to the targeted members for free, the program would reduce the expenditure on privately provided health inputs. This would make more household resources available for the non-targeted members to increase consumption of their private heath inputs. This additional income can also be used to purchase or produce more of the composite goods for all household members; this is, in-turn, likely to result in a positive income effect on the health of the non-targeted individuals within the household. The FA program provides information about health, nutrition and hygiene practices in the household, thereby enhancing the basket of household public goods. The third component of the spillover effect is the positive biological contagion, which is generated by the reduction of diseases transmition as a result of healthier individuals within the household. This positive contagion has a multiplier effect for all household members since better health of an individual

would affect the health of other members, which in turn would affect the individual and so on.

Typically such a reduced form framework does not permit separate measurement of each component of the transmission mechanism. It can clearly predict the overall spillover effect of the program on the non-targeted individuals. Since all the components are positive and mutually reinforcing, the total spillover effect is expected to be positive. Note that neither Chaudhuri (2009) nor Ploeg (2009) can identify the channels. Specifically Ploeg (2009) writes that *it is not possible to tell whether this is due to increased food benefits that are then shared with the non participating children in the family or whether the income offset by the WIC benefits is used to improve the diets of nonparticipating members with other foods* (page 425). However our data enables us to go one step forward. While we cannot separate out the effects of the household public goods and the contagion effects, we can identify whether the effects are driven by the income effect or by a combination of the public good and contagion effect.

4 The Program

The overall aim of the FA program is to increase the level of human capital in the poorest households (those in the first quintile of the income distribution), by providing monetary transfers to *titulars* in beneficiary families, conditional on having completed specific requirements. The program was first targeted geographically. Of the 900 odd municipalities in Colombia, 622 were chosen by *Fondo de Inversiones para la Paz* (FIP), as targets. The targeted municipalities were required to meet all of the following requirements: (i) have less than 100,000 individuals, should not be the capital of a regional department and should not be in the coffee growing region that received special help following the 1995 earthquake; (ii) have at least one bank; (iii) have a minimum level of health and education infrastructure; and (iv) the local authorities must have shown interest in participating in the FA program and have complied with the administrative tasks necessary to participate in the program, which included providing a list of the SISBEN 1 beneficiaries.[10] In the

[10]The SISBEN is an indicator of economic well-being that is used throughout Colombia for targeting welfare programs as well as for the pricing of utilities. In theory, each Colombian household is classified

case of FA, only households belonging to SISBEN 1 and having children aged $0-17$ as of December 1999 were eligible and these households constitute approximately the bottom twenty percent of Colombian households (see Velez, Castano, and Deutsch, 1998). The program started, with some exceptions, in the second half of 2002 and the take up among eligible households was over 90 percent.

The evaluation survey was conducted by first choosing a stratified random sample of targeted communities. The stratification was done on the basis of geographic areas and the level of health and education infrastructure, for a total of 25 strata. Within each of these strata, the evaluation team chose control municipalities that were as similar as possible (in terms of size, population, an index of quality of life as well as health and education infrastructure) to the municipalities included in the set of treatment municipalities.[11] In each of the municipalities in the sample, 10 geographic clusters were randomly drawn, with weights proportional to the population, of which three clusters were urban and seven rural. Finally, in each of the clusters, about 20 households were randomly drawn from the SISBEN 1 lists. See IFS (2004) for more on the survey methodology. Given non response rates and household mobility, about 10 households per cluster was included in the final evaluation sample, which was, in the end, made up of about 11,500 households living in 122 municipalities, of which 57 were treatment municipalities and 65 were control municipalities. A household was eligible (for the health component of the program) if there is at least one child aged $0-5$ within the household at the baseline. The sample is restricted to eligible households in the treatment and control municipalities.

We define a household to be a *treatment* household if it is eligible and resides in a treatment

into one of six levels, on the basis of an indicator determined by the value of several variables periodically measured. SISBEN stands for (in Spanish) Identification and Classification System for social programs potential Beneficiaries. In Colombia, Families were surveyed by the municipal authorities and classified into one of the six categories according to their level of measured poverty. The poorest families were classified in level 1, and the richest in level 6. The category of each household was changed periodically to reflect changes in economic status of the household.

[11] Since the assignment of municipalities to treatment and control groups is not strictly random, the treatment and control samples could end up being different in a number of different dimensions. In the main regression results we compute the difference-in-difference estimates, controlling for baseline observables, bearing in mind that there might be some bias in the estimates. To analyze the extent of this bias, we examine the robustness of the results using propensity score matching in the comparison of treatment and control households. These results (presented in Tables 10 and 11) are very similar to the standard difference-in-difference regression results - the effects are very similar both in terms of direction and magnitude. This gives us confidence in the difference-in-difference estimates.

municipality and a household to be a *control* household if it is eligible but resides in a control municipality. This gives us a final estimating sample of 6648 households 3993 in treatment and 2655 in control municipalities. We are specifically interested in the effects of the program on individuals aged 18 or higher (at the baseline) who would not have been exposed to either the health or the education components of the FA program.

Available measures of health of other members in the household (individuals not in the health and nutrition component of the program) are quite restricted. This was done mainly to reduce the costs associated with data collection. As the primary aim of the program was the child's health, the surveys were designed to evaluate the effectiveness of the program and did not include extensive measures of health of others members of the household. We are therefore restricted in terms of what variables we can use to measure health impacts of the program. Specifically we use the following three variables:

1. Self reported illness in the last 15 days (was the individual ill during the 15 days prior to the survey?)
2. Was the individual in bed as a result of this illness?
3. Was the individual hospitalized in the last year because of any illness?

Several studies have used self reported illness as a measure of health status arguing that self-reported health reliably predicts actual morbidity and mortality even when other risk factors are controlled for (see Idler and Benyamini, 1997; Haddock, Poston, Pyle, Klesges, Weg, Peterson, and Debon, 2006; Brook, Ware, Rogers, Keeler, Davies, Sherbourne, Goldberg, Lohr, Camp, and Newhouse, 1984). Having said this, the binary nature of self reported illness makes it less informative. Additionally, an individual's self-reported health status is subjectively affected by his/her social and cultural background, given their subjective health. Schultz and Tansel (1997) argue that this is because of *cultural conditioning*: The threshold of what is considered good health varies systematically across a society, controlling for their objective health status. For example, individuals who are more educated, wealthier and from socially advantaged groups, are typically more aware of the limitations imposed on them by their health status and are more likely to report

themselves (and their family) as being of poor health. However, in the context of this paper, this is not a major problem as all households in the sample (both in the treatment and control municipalities) are drawn from the poorest income quantile. The survey also asks whether the individual was in bed as a result of the illness; this is likely to be more informative because in this case illness is considered severe enough to affect the individual's regular activities (including earnings) and is less likely to suffer from the cultural conditioning problem. This measure could also be viewed as an increase in preventative care; for example, when individuals take time off to reduce the intensity of the effect of sickness. It is difficult, if not impossible, to determine which of the two effects is operating here. The third measure (whether the individual had been hospitalized in the one year prior to the follow-up survey) is a longer-term measure of health and is based on an objective assessment by a health care professional. This variable is less likely to suffer from the cultural conditioning problem that we have discussed above. Additionally hospitalization could be regarded as a measure of the severity of the illness.

5 Estimation Methodology, Data and Descriptive Statistics

The data used in this paper come from panel data collected for the evaluation of the impact of the FA program. The data collection was done in three rounds. The first one in 2002, in order to establish the baseline before the start of the program; the first follow up conducted one year later with the primary aim of obtaining the short run impact of the program. The second follow-up survey was conducted in 2006, with the aim of assessing the medium term impact of the program.[12] Recall the baseline sample consisted of 6648 households; the attrition rate was approximately 6 percent in the first follow up (conducted in 2002), with 6255 households being re-interviewed. The attrition rate was slightly higher for the treatment households (6.5 percent) compared to the control households (5.1 percent). The overall attrition rate is higher in the second follow up (conducted in 2006): 5609 households were re-interviewed, which translates to an attrition rate of 15.6 percent relative to the baseline. The attrition rate was similar for treatment and control households (15.6 percent and 15.7 percent, respectively).

[12] See http://www.dnp.gov.co for more details.

Table 1 presents the differences between treatment and control households at the baseline. Of particular interest are household income, access to services that can potentially affect the health of members and educational attainment within the household, which could affect how information is used within the household. It has been argued (see for example Wolfe and Behrman, 1982; Jalan and Ravallion, 2003; Attanasio, Gomez, Gomez Rojas, and Vera-Hernandez, 2004) that households where the members are more educated are able to use information more efficiently and productively. We see that households in control municipalities are richer, are more likely to have access to piped water, access to waste collection services and are more likely to use piped water for cooking. The household head is more likely to be a single parent in a treatment household. There is very little difference in the educational attainment of the household head or the spouse of the household head between the treatment and the control households. There are some significant differences at the baseline in terms of asset ownership, and the means suggest that the control households were relatively better off.

Table 1 also presents the unconditional means for the three outcome variables of interest. Overall, at the baseline individuals in the treatment households are worse off compared to individuals in control households - both the intensity and severity of illness is significantly higher for individuals in treatment households. This implies that the difference-in-difference (program) effects that we present below give us the lower bounds of the program impacts, even after controlling for a full set of observable characteristics.

While the baseline survey was designed to obtain pre-program information about the households, for political reasons the program actually started in 26 of the 57 treatment municipalities prior to the baseline survey. These were the *early treatment municipalities* and households in these municipalities were already receiving the cash transfers at the time the baseline survey was conducted. We examined the robustness of our results by excluding the early treatment sample and restricting the sample to those households residing in the 31 treatment municipalities where the baseline survey was conducted prior to the program becoming operational. The corresponding regression results, presented in columns 1 – 3 of Table 9, show that the spillover effects are unchanged when we exclude the *early treatment municipalities*.

As a part of government policy the program was expanded in 2005 and a second follow up survey was conducted in 2005-2006. But the program was extended to 13 of the previous control municipalities (in the second follow up these 13 municipalities can be thought of as being treatment municipalities). We call these the *converted municipalities*. In the results that we present below, we continue to include these *converted municipalities* as control municipalities − the argument being that the change happened not long before the second follow up survey. However we examine the robustness of our results by excluding these *converted municipalities* from the estimation sample. The results are presented in columns 4 – 6 of Table 9 and show that including these *converted municipalities* does not make any difference to our results.

5.1 Estimation Methodology

We use a difference-in-difference model to estimate the intent-to-treat (ITT) estimates of the program on non-targeted individuals in treatment households. The panel dimension of the data for the health outcome of interest allows us to control for any initial differences across groups. Our primary estimating equation takes the following form:

$$H_{ict} = \beta_0 + \beta_1 Treatment_c + \beta_2 Year_t + \beta_3 Program_{ct} + \mathbf{X}_{ic}^{'}\gamma + \varepsilon_{ict} \tag{8}$$

Where H_{ict} is an outcome of interest (for example health of an adult in household i residing in municipality c at time t); $Treatment_c$ is a dummy variable for the treatment group or community; $Year_t$ is an indicator variable for the post-intervention period; $Program_{ct}$ is an indicator variable for assignment into the program (this variable takes the value of 1 for treatment municipalities in the post intervention period); $\mathbf{X}_{ic}$ is a set of baseline individual, household and cluster (or municipality) characteristics to control for any remaining pre-treatment differences and ε_{ict} is a random disturbance term. The causal estimate of assignment to the program on the health of individuals in the household is given by β_3, which gives us the ITT estimates of the program. Remember that we are not talking about direct program effects here: rather we are focusing on indirect effects on non-treated members arising because mothers or *titulars* have exposure to health training/information as a result of having to accompany children aged $0-5$ to the health centre. We think of these as spillover effects within the household.

5.2 Attrition

The attrition rate in the first follow up was around 5.9 percent but increases to 15.6 percent in the second follow up (relative to the baseline). Attrition, if non-random (particularly if the likelihood of attrition is correlated with the baseline variable of interest) could result in biased estimates. To examine the issue of attrition in more detail, in unreported regressions we first (following the methodology proposed by Fitzgerald, Gottschalk, and Moffitt, 1998) estimate an attrition probit model on a set of baseline observables and including a set of quality of fieldwork at the baseline as additional explanatory variables.[13] The dependent variable in this regression is $ATTRITE_s$, a dummy variable that takes the value of 1 if the household is not surveyed in the first ($s = 1$) or the second ($s = 2$) follow up survey. While a number of observable (household, geographical and interview) characteristics significantly affect the likelihood of attrition, the $Treatment$ dummy is not statistically significant, indicating that there is no evidence of differential attrition across the treatment and control households.

Is the initial health status of attriting households different from non-attriting households? To examine this, we regress the different outcome variables of interest for the baseline sample, on the baseline observables, the attrition dummy ($ATTRITE$) and a set of interaction terms between the attrition dummy and each of the explanatory variables. The non-interacted coefficients give us the effects for the (eventually) non-attriting households while the interacted coefficients give us the difference between the attriters and non-attriters at the baseline. A test of the joint significance of the $ATTRITE$ dummy and the interaction terms tells us whether the attriting households are different from the non-attriting households. The results (not presented in the paper but are available on request) show that the null hypothesis that the attriting households are not different from the non-attriting households can never be rejected. There is therefore no evidence to suggest that attrition is non random.

[13] We include the number of visits to complete the interview, the number of enumerators to complete the interview, the number of supervisors of the enumerators and if the interview was incomplete as measures of the quality of the interview. We also include fixed effects for the percentage of attrition in the municipality and the supervisor code.

6 Results

Tables 2 – 4 present the ITT estimates for the program effect denoted by *Program*. In each case Panel A presents the short run results (comparing the baseline to the first follow-up) and Panel B present the medium term results (comparing the baseline to the second follow-up). We present estimates for the full sample and for different sub-samples: young adults aged $18-25$, working age adults aged $26-59$ and elderly aged 60 and higher. This is done to examine whether the program effects are different across the different sub-samples. The stratification is done on the basis of age at the baseline. We present the marginal effects as they are interpreted more easily. Table 2 presents the ITT estimates for illness (*was the individual ill during the 15 days prior to the survey?*); Table 3 presents the corresponding estimates for being in bed as a result of this illness (*was the individual in bed as a result of this illness?*); and finally Table 4 presents the ITT estimates for hospitalization (*was the individual hospitalized in the last year because of any illness?*). In all regression results that are presented we include a full set of controls (individual, household and municipality level controls) to capture pre-treatment differences and standard errors are clustered at the municipality level. Note that the full set of results (for all controls) are presented in Tables A.1 and A.2.

6.1 Short Run Results

We start with a discussion of the short run effects. The program effect is given by the coefficient estimate associated with *Program*. Individuals (adults aged 18 and higher) in treatment households are almost 3 percentage points less likely to be ill in the 15 days prior to the survey (post intervention), compared to individuals in control households (Table 2, Panel A, Column 1). Given the mean incidence of illness of 20 percent for adults in the comparison group, this translates to a 15 percent drop in the incidence of illness at the mean. In the short run there is no statistically significant program effect on the likelihood of being in bed as a result of this illness (Table 3, Panel A, Column 1) and on the likelihood of being hospitalized in the one-year prior to the survey (Table 4, Panel A, Column 1).

The estimation results presented in column 1 in Tables 2 – 4 essentially give us the overall spillover effects. But it is quite possible that the spillover effects vary across the different age groups (i.e., the effects are heterogenous). We therefore conduct and present in columns $2-4$ of each table the corresponding difference-in-difference estimates for the different age specific sub-samples: $18-25$ or young adults including young parents (column 2), $26-59$ or working age adults (column 3) and 60 and higher or the elderly (column 4).

We start with the sub-sample estimates for being ill in the 15 days prior to the survey (Table 2). While the coefficient estimate associated with *Program* is always negative, it is statistically significant only in the case of working age adults and the elderly (columns 4 and 5). Adults aged $26-59$ belonging to treatment households are more than 3 percentage points less likely to be ill in the 15 days prior to the survey compared to a similar aged adult in a control household. The magnitude of the effect is even larger in the case of the elderly: the results in column 5 show that individuals aged 60 or higher in treatment households are almost 9 percentage points less likely to be ill in the 15 days prior to the follow-up survey relative to those in control households. The subsample estimation results for being in bed (conditional on being ill) in the 15 days prior to the survey and being in a hospital in the year preceding the survey (Panel A in Tables 3 and 4 respectively) show that there are no program effects, irrespective of the age group. The entire spillover effect therefore operates through the short run effect on the likelihood of being ill in the 15 days prior to the survey.

Next we seek to examine is whether there are any gender effects of the program. The *titular*, the person who accompanies the child to the health centre and attends the conferences and workshops on health, hygiene and nutrition, is typically a woman (recall from footnote 6 that 95% of the *titulars* are females) and it is worth examining whether the spillover effects are concentrated along gender lines. If, for example, women have more information on the behavior of other women and of children and can advice them, the peer effects could be stronger for women. On the other hand, if men are better able to internalize the information about health improvements, the effects could be stronger for men.[14] Table 5 presents the gender specific regressions (columns $1-3$ in women and columns $4-6$

[14]We find the level of education is, in general, higher for men than for women; and this pattern is stronger for the elderly.

for men). Panel A presents the results for all individuals aged 18 and higher; Panel B restricts the sample to young and working age adults (males and females aged $18-59$) and finally Panel C examines the effects for the elderly (males and females aged 60 or higher). Females aged $18-59$ are more likely to have children enrolled in the program and any effects on the elderly are important from a public health point of view.

Women in treatment households are 3 percentage points less likely to be ill compared to women in control households (Panel A, column 1). The effect is similar for men (at 2.9 percentage points) in treatment households (Panel A, column 4). Also, while there is a 2 percentage point reduction on the likelihood of being hospitalized for women in treatment households, there is no corresponding treatment effect on men. There is no treatment effect on the likelihood of being in bed as a result of illness for either men or women.

The results presented in Panel B show that irrespective of the gender of the adult under consideration, there is no program effect on the $18-59$ year olds and this result holds for all three outcome measures. On the other hand, the program effects are very strong on the incidence of illness for both elderly males and females and the program effects are similar for elderly males and females. The fact that there is such a strong effect on the health of the elderly can have substantial implications on public health, given that in most countries, expenditure on health of the elderly is a substantial component of the health budget, both at the macro and the micro level. Finally, in general there is no short run program effect on the likelihood of being in bed as a result of illness or on hospitalization.

Are the effects driven by the *titulars*?

Recall that the *titulars* accompany their children to the health centres. The *titulars* are directly exposed to the program by accompanying their children to the health centres, attending sessions on health, nutrition, and hygiene. So it is worth examining whether the *titulars* are affected differently compared to the other similar aged women in the household. To examine this we restrict the sample to treatment households and conduct the following regression:

$$H_{ict} = \alpha_0 + \alpha_1 Titular_i + \alpha_2 Year_t + \alpha_3 Program_{it} + \mathbf{X}_{ict}^{'}\gamma + \epsilon_{ict} \tag{9}$$

Here $Titular$ is a dummy variable that takes the value of 1 if the woman is the *titular*, 0 otherwise. The estimated coefficient $alpha_3$ gives us the differential effect of being in the treatment group on *titulars*.[15] The regression results presented in Table 7, Panel A show that *titulars* are not benefitting any more compared to the non-titulars within the treatment households: the estimated coefficient α_3 is never statistically significant. The program effects are therefore unlikely to be driven by attendance at the clinics (i.e., by the potentially improved health of the *titulars*).

Further as the results presented in Panel B of Table 7 show, even when we exclude the *titulars* from the treatment households, the treatment effect continues to be statistically significant. The results therefore are not driven by improvements in the health of the titulars (through the information they gain by visiting the clinics), rather through what they bring back to the household.

To summarize our results: in the short run, there is evidence of spillover effects within the household, occurring through a significantly reduced likelihood of illness in the 15 days prior to the date of the survey. The overall effects are driven by the improvements in the health of the elderly (males and females). Additionally the effects are not driven by improvements in the health of the *titulars*.

6.2 Medium Run Effects

The medium run effects are however quite different from the short run effects. Panel B in Tables 2 – 4 present the medium run ITT estimates of being ill in the 15 days prior to the survey, being in bed because of this illness and being hospitalized in the one year prior to the survey respectively. These effects are weaker but still significant over the longer run, when we consider the effects on the incidence of illness. The medium run effects are stronger when we consider the severity of illness (measured by a reduction in hospitalization). Individuals in treatment households are 1.8 percentage points less likely to be ill in the 15 days prior to the survey compared to individuals in control households

[15] We consider different definitions of *titular*. In the results presented in Table 7 *titulars* are defined as those who answered the *titular* specific Module (Module 2) in the questionnaire in the treatment municipalities at the baseline. The results are however consistent across the different definitions.

(see Table 2, Panel B, Column 1), down from 3 percentage in the short run, and no longer statistically significant. Conditional on being ill, individuals in treatment households are 4 percentage points more likely to be in bed compared to individuals in control households (see Table 3, Panel B, Column 1), though again not statistically significant. Finally, unlike in the short run case, we find a statistically significant treatment effect on the likelihood of hospitalization − individuals in treatment households are 1.6 percentage point less likely to have been hospitalized compared to individuals in control households (see Table 4, Panel B, Column 1). The effect is statistically significant at the 5 percent level of significance and given the control mean of 7 percent, this corresponds to a 23 percent drop in the likelihood of hospitalization at the mean.

The impacts are again quite heterogeneous across the different age subsamples. There is a strong and statistically significant effect for individuals aged $18 - 25$. Individuals in this age group are 3.4 percentage points less likely to be ill in the 15 days prior to the survey and 5.2 percentage points less likely to be in a hospital in the one year prior to the survey. Additionally the elderly (individuals aged 60 and higher) are 4.5 percentage points less likely to have been hospitalized in the one year prior to the survey, indicative of a significant improvement in the long term health (measured by a reduction in the intensity of illness) of the elderly in the treatment households. This is important because in the control households, the severity of illness of both young adults and the elderly has actually increased over the period.

The gender specific medium run effects for the different sub-samples are presented in Table 6. The impact is surprisingly almost always stronger on the health of men and while this holds across all age groups, the largest impact is on the health of the elderly men. This result can possibly be related to differences in self-perception of health across genders. There are biological differences in interpreting health conditions and symptom recognition across gender. Women typically report rates of illness higher than for men: this is true for both the FA sample (used in our analysis) and the overall Colombian context.[16] However as women are more exposed to information relating to health, they perceive themselves as

[16] At the national level in 2000, 14.5 percent of women reported to be sick while only 11.7 percent of men did. The rate of hospitalization in the same year was reported as 5.6 percent and 3.8 percent for women and men, respectively (Guarnizo-Herreno and Agudelo, 2008).

being of poorer health (compared to men) and this can partly explain why the impacts on women are smaller. The results presented in Table 8 suggest that *titulars* are not differently affected and surprisingly *non-titulars* appear to have benefitted more (compared to women in control households) in the form of a reduction in the severity of illness.

6.3 Robustness

We conduct a number of different robustness checks. First, we examine the robustness of our results by

(1) excluding the *early treatment municipalities*, i.e. the set of municipalities that received the payment before to the baseline survey was conducted; and

(2) by excluding the *converted municipalities*, i.e., the control municipalities that actually got converted to treatment municipalities in 2005.

The corresponding regression results for the first and second follow up presented in Table 9 are very similar to those presented in Tables 2 – 4.

Difference-in-Difference Propensity Score Matching

The evaluation followed a quasi-experimental methodology, which meant that the assignment of municipalities into treatment and control groups was not fully random (the treatment municipalities were randomly chosen from the list provided by the government and the control municipalities were matched). Therefore the treatment and control municipalities could be different along a number of different dimensions. In the difference-in-difference regressions (above) we control for the baseline characteristics, to account for possible pre-treatment differences in observables. To ensure that the results presented thus far are not biased, we also compute the average treatment effect on ineligible members of the household using a difference-in-difference propensity score (DID-PSM) estimator. We can obtain the indirect effect of the program on adults' health by comparing the average of the health indicators after matching households using their propensity score. This proce-

dure follows the Rosenbaum and Rubin (1983) methodology, which suggests that finding and comparing *similar* households in control and treatment municipalities, based on their propensity scores (rather than using a full set of observable variables), will generate consistent estimators.[17] In this case, we use two sets of observable characteristics to find the household propensity score. The first one (scenario A) gives us a balanced distribution of the propensity score between households in control and treatment municipalities. As the estimated bias can be affected by the set of variables used to estimate the propensity score (Smith and Todd, 2005), we include a second set of variables (scenario B) based on the variables used by Attanasio, Fitzsimons, Gomez, Gutierrez, Meghir, and Mesnard (2010) in their propensity score matching estimation and used this as an additional sensitivity test. The variables used in the two scenarios are listed in section A.3 in the Appendix.

The propensity score matching method allows us to find comparable households in treatment and control municipalities, using information on the observed baseline characteristics. This is defined as the region of support. Figure 1 shows the distribution of the propensity score of all households on the common support for the short run estimations, while Figure 2 shows the corresponding distribution for the medium run. We argue that using a PSM estimator can help us to reduce the bias based on observables, given that the information comes from similar economic setting and the measurement of the outcomes are done in the same way. The use of the difference-in-difference matching estimators also allows us to control for time invariant characteristics.

We use a probit model to estimate the propensity score. Then we use the kernel non-parametric matching estimator to estimate the average indirect effect on health. The advantage of the kernel estimator is that it does not impose any structure on the functional form of the propensity score distribution. However, for purposes of sensitivity analysis we also estimate parametrically the indirect impact on health using the nearest neighbour and the caliper matching estimators.[18] We also take advantage of the variation on the common support definition, modifying the matching estimations using a trimming specification to

[17] Of course this methodology relies on the assumption of no significant differences between treatment and control municipalities in terms of the unobservables.

[18] We also estimate the average treatment effect using the local linear estimator and variations of the nearest neighbour estimator and we find very similar results. These are available on request.

determine the support region.[19]

Tables 10 and 11 present the results of the DID-PSM estimation (for the short run and medium run respectively). The estimates are very similar to those presented in Tables 2 - 4, not only in terms of the magnitude but also in in terms of the significance of the impact. We can therefore conclude that in the short run the program impacts non-targeted individuals by reducing the incidence of illness while in the medium run it reduces both the incidence and severity of illness. Alternative specifications of the matching technique do not change the results. The results presented in Tables 10 and 11 therefore makes us more confident of the DID regression results that we have discussed so far.

7 Mechanism

Recall that there can be a number of different pathways through which the within household spillovers can arise: the cash transfer component of the FA program frees up resources for other members creating an *income effect*, produces public goods such as health information that creates a household *public good effect* and generates a positive *contagion effect* as a result of healthier and more hygienic surroundings within the household. The three components are mutually reinforcing but from a policy point of view it is important to know which effect is the strongest.

While we do not have data that will allow us to separate the public good effect from the contagion effect, the eligibility condition of the conditional cash transfer components can potentially allow us to partially identify the income effect. To do this, we conduct a falsification test. We utilize the education component of the FA program, which was targeted at households with children aged 6 to 17. Eligible households satisfying the attendance requirement received a per-child monthly subsidy of 14,000 pesos (US $6.15) and 28,000 pesos (US $12.30) for each child attending primary and secondary school respectively.

Specifically, we restrict the sample to households with no children aged $0 - 5$. In this

[19]Using this variation is important because the non-parametric regression estimators of the counter factual mean outcome are unreliable when evaluated at points where the estimated density is close to 0.

sample the only way in which exposure to the program can affect adult health is through the income effect.[20] We estimate a specification where we include a set of interaction effects interacting *Program* with dummies for the number of children aged $6-17$ in the household. The variation in the number of children aged $6-17$ in the household allows us to identify the income effect arising from the FA program. The results for the non-interaction term and the difference estimates are presented in Table 12; so ξ_i gives the additional effect of having i children in the household, $i = 2, 3, 4, 5, 6$ and higher. The non-interacted term *Program* gives us the effect for households with one child aged $6-17$. Neither in the short run, nor in the medium run is the joint test $\beta_3 + \xi_i = 0$ rejected, indicating that additional income (through the FA program) does not have any effect on any of the three health measures that we consider. Recall that for this sample, the only way exposure to the FA program can affect adult health is through the income effect. There is no evidence of an income effect. The treatment effect therefore is driven by a combination of the public goods effect and the contagion effect. Lack of data prevents us from further decomposition.

8 Conclusion

Conditional Cash Transfer programs are increasingly becoming the policy makers' vehicle of choice to provide benefits to poor households that can potentially break the vicious inter-generational cycle of poverty. While the stated aims of most CCT programs is to improve the health and nutritional status and educational attainment of children in poor households, we argue that there are strong within household spillovers that can arise as a result of the introduction of such programs. The total program effects go beyond the direct effects on the health of children and it would be incorrect to evaluate the program solely on the basis of the direct effect. We illustrate this using data from the *Familias en Acción* program that has been in operation in Colombia for almost a decade now.

Our results show that there are indeed strong spillover effects within households. In

[20] In all regressions we control for labor supply including the number of hours worked. This helps us to isolate the program income effect from any labor supply income effect. The IFS-Econometria-SEI (2006) program report shows an increase in the job market participation by adults but no program effect on the number of hours worked.

the short run, the strongest effects are on self-reported illness. Non-targeted individuals (adults) in treatment households were significantly less likely to be ill in the 15-days prior to the survey compared to adults in control households. The effects persist over a longer period of time and indeed over time it leads to better long term health and a reduction in the severity of illness, captured by lower rates of hospitalization. Given that the health of individuals in treatment households was actually poorer at the baseline, these are possibly the lower bounds of the true program effects. Additionally we find that the effects are quite heterogeneous and the effects are stronger for men, over the medium run. Our results suggest that it is household level public goods and contagion, happening through changes in behavior and not a relaxation of the household budget constraint as a result of the cash transfer that is driving the results. From the policy point of view therefore simply looking at the direct effects results in significant underestimation of the effect of such CCT programs.

All of this has significant effects on the inter-generational poverty cycle. Healthier adults are more productive and this increase in productivity of adults is likely to positively affect the human capital of the next generation. None of this is captured by examining only the effects on the targeted group. This program has had significant within household spillovers and simply by looking at the direct effects, one would significantly underestimate the effects of the program. Proper cost-benefit analysis of such CCT programs needs to take into account the improved health of the non-targeted individuals and the consequent reduction in both the incidence and severity of illness, resulting in improvements in long term health.

Table 1: Baseline Descriptive Statistics

	Control (1)	Treatment (2)	Difference‡ (3 = 2 − 1)
Outcome Variables			
Ill	0.1840	0.2230	0.0394***
	(0.0051)	(0.0046)	(0.0070)
In Bed	0.5810	0.6000	0.0196
	(0.0154)	(0.0115)	(0.0192)
Hospitalization	0.0790	0.0990	0.0200***
	(0.00361)	(0.00331)	(0.0050)
Individual, Household and Municipality level Variables			
Household income per capita	56244	53378	-2865.70**
	(951.7)	(849.9)	(1299.45)
Household has piped gas service	0.0752	0.0726	-0.0025
	(0.0051)	(0.0041)	(0.0066)
Household has piped water	0.625	0.594	-0.0315**
	(0.0094)	(0.0078)	(0.0122)
Household has sewage system	0.247	0.251	0.0041
	(0.0084)	(0.0069)	(0.0108)
Household has waste collection service	0.351	0.285	-0.0658***
	(0.0093)	(0.0072)	(0.0116)
Household has access to any telephone	0.0906	0.0877	-0.0029
	(0.0056)	(0.0045)	(0.0071)
Household use piped water for cooking	0.618	0.578	-0.0403***
	(0.0094)	(0.0078)	(0.0123)
Does the water receive any treatment before drinking	0.591	0.613	0.0220*
	(0.0096)	(0.0077)	(0.0122)
Does the household have wc connected to sewer or septic tank	0.509	0.508	-0.0015
	(0.0097)	(0.0079)	(0.0125)
What is the status of ownership of this house	1.687	1.655	-0.0327
	(0.0175)	(0.0139)	(0.0222)
Household has other assets	0.0998	0.0967	-0.0032
	(0.0058)	(0.0047)	(0.0074)
Household has fridge	0.301	0.274	-0.0278**
	(0.0089)	(0.0071)	(0.0113)
Household has sewing machine	0.0836	0.0846	0.0010
	(0.0054)	(0.0044)	(0.0070)
Household has black-white TV	0.253	0.244	-0.0083
	(0.0084)	(0.0068)	(0.0108)
Household has radio	0.457	0.404	-0.0537***
	(0.0097)	(0.0078)	(0.0124)
Household has bicycle	0.346	0.367	0.0207*
	(0.0092)	(0.0076)	(0.0120)
Household has motorcycle	0.0328	0.0501	0.0173***
	(0.0035)	(0.0035)	(0.0051)
Household has fan	0.373	0.319	-0.0542***
	(0.0094)	(0.0074)	(0.0119)
Household has blender	0.414	0.41	-0.0037
	(0.0096)	(0.0078)	(0.0123)
Household has color tv	0.363	0.342	-0.0215*
	(0.0093)	(0.0075)	(0.0119)

Continued . . .

Table 1 (continued)

	Control (1)	Treatment (2)	Difference (3 = 2 − 1)
Household has kerosene lamp	0.102	0.0624	-0.0400***
	(0.0059)	(0.0038)	(0.0067)
Household has boat	0.0508	0.0238	-0.0270***
	(0.0043)	(0.0024)	(0.0046)
Household has energy plant	0.0075	0.0088	0.0012
	(0.0017)	(0.0015)	(0.0023)
Household has livestock	0.635	0.683	0.0480***
	(0.0094)	(0.0074)	(0.0118)
Household member born in the last 12 months	0.217	0.202	-0.0153
	(0.008)	(0.0064)	(0.0102)
Household member died in the last 12 months	0.0309	0.0389	0.0080*
	(0.0034)	(0.0031)	(0.0046)
Household member is pregnant	0.0912	0.0812	-0.0100
	(0.0056)	(0.0043)	(0.0070)
Small municipality center	0.28	0.333	0.0524***
	(0.0087)	(0.0075)	(0.0116)
Medium municipality center	0.403	0.287	-0.1169***
	(0.0095)	(0.0072)	(0.0117)
Atlantic region	0.434	0.385	-0.0488***
	(0.0096)	(0.0077)	(0.0123)
Central region	0.215	0.273	0.0583***
	(0.008)	(0.0071)	(0.0108)
Pacific region	0.142	0.134	-0.0079
	(0.0068)	(0.0054)	(0.0086)
Household lives in grouped populated rural area of the municipality	0.365	0.444	0.0797***
	(0.0093)	(0.0079)	(0.0123)
Household lives in sparsely populated rural area of the municipality	0.0731	0.11	0.0369***
	(0.0051)	(0.005)	(0.0073)
Household member migrated in the last 12 months	0.106	0.11	0.0039
	(0.006)	(0.005)	(0.0078)
Walls of good-quality	0.455	0.426	-0.0289**
	(0.0097)	(0.0078)	(0.0124)
Walls of poor-quality	0.0418	0.0523	0.0105**
	(0.0039)	(0.0035)	(0.0054)
Number of children under 7	1.868	1.866	-0.0015
	(0.0188)	(0.0156)	(0.0245)
Number of children between 7-11	1.064	1.08	0.0156
	(0.0193)	(0.0155)	(0.0247)
Number of children between 12-17	0.909	0.851	-0.0580**
	(0.02)	(0.0161)	(0.0256)
Number of of household members	6.779	6.631	-0.1484**
	(0.0519)	(0.0402)	(0.0650)
Number of female adults	1.5	1.482	-0.0181
	(0.0164)	(0.0127)	(0.0205)
Access to health system by the household	0.124	0.203	0.0783***
	(0.0064)	(0.0064)	(0.0094)
Age of the household head	43.32	42.54	-0.7736**
	(0.255)	(0.207)	(0.3286)
Age of the household spouse	37.39	36.5	-0.8897***
	(0.254)	(0.196)	(0.3165)
No education of the household head	0.234	0.218	-0.0154
	(0.0082)	(0.0065)	(0.0104)

Continued . . .

Table 1 (continued)

	Control (1)	Treatment (2)	Difference (3 = 2 − 1)
Incomplete primary school of the household head	0.131	0.148	0.0163*
	(0.0066)	(0.0056)	(0.0087)
Complete primary school of the household head	0.407	0.422	0.0155
	(0.0095)	(0.0078)	(0.0123)
Incomplete secondary school of the household head	0.0331	0.0343	0.0012
	(0.0035)	(0.0029)	(0.0045)
No education of the spouse	0.188	0.176	-0.0128
	(0.0083)	(0.0067)	(0.0106)
Incomplete primary school of the spouse	0.156	0.163	0.0070
	(0.0077)	(0.0065)	(0.0101)
Complete primary school of the spouse	0.414	0.443	0.0286**
	(0.0105)	(0.0087)	(0.0136)
Incomplete secondary school of the spouse	0.0366	0.0399	0.0033
	(0.004)	(0.0034)	(0.0053)
Single parent or head	0.166	0.185	0.0181*
	(0.0072)	(0.0061)	(0.0096)
Number of observations	2,655	3,993	
Number of municipalities	65	58	

Standard deviation in parentheses.
Significance: *** : 1%;** : 5%;* : 10%
‡: Difference = Treatment − Control

Table 2: Program effects: on being ill in the 15 days prior to the follow-up survey

	All	$18-25$	$26-59$	60 or Higher
	(1)	(2)	(3)	(4)
Panel A: First Follow up				
Treatment	0.0323***	0.0204	0.0338***	0.0474
	(0.0116)	(0.0160)	(0.0109)	(0.0366)
Year	0.1790	-0.1010	0.2630**	-0.3770
	(0.1320)	(0.1470)	(0.1310)	(0.4580)
Program	-0.0296*	-0.0024	-0.0307*	-0.0898**
	(0.0153)	(0.0205)	(0.0162)	(0.0423)
Sample size	26,884	5,535	20,112	2,188
Mean Control	0.20	0.13	0.20	0.36
Panel B: Second Follow up				
Treatment	0.0327***	0.0229	0.0304***	0.0426
	(0.0115)	(0.0152)	(0.0114)	(0.0349)
Year	0.0055	-0.1250	0.0704	-0.2660
	(0.1670)	(0.1730)	(0.1720)	(0.5240)
Program	-0.0180	-0.0338*	-0.0099	-0.0439
	(0.0191)	(0.0202)	(0.0195)	(0.0481)
Sample size	23,338	4,446	17,764	1,821
Mean Control	0.18	0.14	0.18	0.28

Notes:
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. Full set of results corresponding to column (1) are presented in Tables A.1 and A.2

Table 3: Program effects: being in bed as a result of the illness

	All	$18-25$	$26-59$	60 or Higher
	(1)	(2)	(3)	(4)
Panel A: First Follow up				
Treatment	0.0259	0.0089	0.0190	0.0513
	(0.0224)	(0.0559)	(0.0257)	(0.0589)
Year	-0.0769	-0.6400	-0.0127	-0.3580
	(0.3250)	(0.4810)	(0.3590)	(0.9230)
Program	0.0170	-0.0423	0.0257	0.0495
	(0.0321)	(0.0741)	(0.0346)	(0.0766)
Sample size	5,686	775	4,162	780
Mean Control	0.55	0.57	0.55	0.51
Panel B: Second Follow up				
Treatment	0.0322	0.0069	0.0250	0.0799
	(0.0249)	(0.0589)	(0.0281)	(0.0684)
Year	-0.2310	0.7890*	-0.4260*	-0.7520
	(0.2930)	(0.4060)	(0.2400)	(0.6560)
Program	0.0417	0.0211	0.0468	0.0636
	(0.0358)	(0.0735)	(0.0392)	(0.1030)
Sample size	4,722	581	3,602	579
Mean Control	0.54	0.59	0.54	0.49

Notes:
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. Full set of results corresponding to column (1) are presented in Tables A.1 and A.2

Table 4: Program effects: on being hospitalized in the year prior to the survey

	All	$18-25$	$26-59$	60 or Higher
	(1)	(2)	(3)	(4)
Panel A: First Follow up				
Treatment	0.0184***	-0.0003	0.0153***	0.0161
	(0.00557)	(0.0124)	(0.0057)	(0.0215)
Year	0.0157	0.0154	0.0329	-0.0299
	(0.0618)	(0.1620)	(0.0606)	(0.2890)
Program	-0.0054	-0.0155	-0.0062	0.0140
	(0.0075)	(0.0129)	(0.0083)	(0.0278)
Sample size	27,884	5,522	20,081	2,183
Mean Control	0.08	0.08	0.08	0.09
Panel B: Second Follow up				
Treatment	0.0197***	0.0064	0.0160***	0.0182
	(0.0057)	(0.0130)	(0.0059)	(0.0213)
Year	-0.0523	-0.0723	-0.0825	0.4430
	(0.0619)	(0.1400)	(0.0641)	(0.3360)
Program	-0.0164**	-0.0524***	-0.0041	-0.0453*
	(0.0076)	(0.0125)	(0.0083)	(0.0265)
Sample size	23,338	4,438	17,733	1,816
Mean Control	0.07	0.09	0.06	0.12

Notes:
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. Full set of results corresponding to column (1) are presented in Tables A.1 and A.2

Table 5: Gender Specific Short Run Effects

	Ill (1)	In bed (2)	Hospitalized (3)	Ill (4)	In bed (5)	Hospitalized (6)
Panel A	Female 18 and higher			Male 18 and higher		
Treatment	0.0215*	0.0248	0.0317***	0.0390***	0.0062	0.0070
	(0.0124)	(0.0292)	(0.0084)	(0.0147)	(0.0318)	(0.0061)
Year	0.1727	0.0518	0.0397	0.0813	-0.5337**	-0.0183
	(0.1566)	(0.4178)	(0.0927)	(0.1363)	(0.2448)	(0.0608)
Program	-0.0303*	0.0260	-0.0187*	-0.0291*	0.0084	0.0002
	(0.0175)	(0.0390)	(0.0105)	(0.0172)	(0.0401)	(0.0079)
Sample size	14,406	3,082	14,455	13,754	2,718	13,792
Mean Control	0.21	0.53	0.10	0.19	0.56	0.05
Panel B	Females $18-59$			Males $18-59$		
Treatment	0.0219*	0.0133	0.0306***	0.0380***	0.0212	0.0062
	(0.0120)	(0.0290)	(0.0089)	(0.0139)	(0.0347)	(0.0059)
Year	0.1814	0.1390	0.0638	0.1129	-0.5111**	-0.0259
	(0.1504)	(0.4478)	(0.0964)	(0.1348)	(0.2453)	(0.0612)
Program	-0.0267	0.0428	-0.0195*	-0.0227	-0.0224	-0.0023
	(0.0173)	(0.0399)	(0.0109)	(0.0177)	(0.0435)	(0.0075)
Sample size	13,483	2,731	13,524	12,505	2,297	12,535
Mean Control	0.20	0.53	0.10	0.17	0.58	0.05
Panel C	Females 60 and higher			Males 60 and higher		
Treatment	0.0416	0.1532*	0.0218	0.0352	-0.1310*	0.0081
	(0.0549)	(0.0810)	(0.0315)	(0.0427)	(0.0764)	(0.0246)
Year	0.3621	0.0331	-0.3302	-0.2269	-0.0672	0.1672
	(0.6461)	(1.4775)	(0.5669)	(0.6292)	(1.3019)	(0.3699)
Program	-0.1201*	-0.1196	0.0097	-0.1063**	0.1916**	0.0240
	(0.0635)	(0.1187)	(0.0452)	(0.0532)	(0.0860)	(0.0340)
Sample size	883	355	887	1,227	425	1,229
Mean Control	0.39	0.52	0.11	0.35	0.50	0.08

Notes
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics.

Table 6: Gender Specific Medium Run Effects

	Ill	In bed	Hospitalized	Ill	In bed	Hospitalized
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Female 18 and higher			Male 18 and higher		
Treatment	0.0165	0.0301	0.0304***	0.0373***	0.0250	0.0114*
	(0.0134)	(0.0316)	(0.0085)	(0.0141)	(0.0334)	(0.0063)
Year	0.0426	0.2396	-0.0290	-0.0075	-0.7099***	-0.0516
	(0.1965)	(0.3936)	(0.0895)	(0.1755)	(0.2476)	(0.0858)
Program	-0.0095	0.0460	-0.0175*	-0.0303	0.0328	-0.0198**
	(0.0227)	(0.0427)	(0.0099)	(0.0192)	(0.0471)	(0.0084)
Sample size	12,672	2,661	12,722	11,717	2,165	11,757
Mean Control	0.19	0.53	0.09	0.17	0.55	0.06
Panel B	Females $18-59$			Males $18-59$		
Treatment	0.0185	0.0149	0.0290***	0.0376***	0.0406	0.0104*
	(0.0129)	(0.0309)	(0.0087)	(0.0137)	(0.0384)	(0.0061)
Year	0.0422	0.1988	-0.0250	0.0321	-0.5418*	-0.1156
	(0.1813)	(0.4225)	(0.0942)	(0.1778)	(0.2970)	(0.1010)
Program	-0.0105	0.0603	-0.0181*	-0.0266	0.0057	-0.0145*
	(0.0210)	(0.0419)	(0.0104)	(0.0192)	(0.0485)	(0.0083)
Sample size	11,903	2,404	11,942	10,684	1,850	10,716
Mean Control	0.19	0.53	0.09	0.15	0.57	0.05
Panel C	Females 60 and higher			Males 60 and higher		
Treatment	0.0387	0.1858*	0.0280	0.0338	-0.1138	0.0093
	(0.0559)	(0.0997)	(0.0310)	(0.0401)	(0.0876)	(0.0254)
Year	0.1408	0.6068	0.0157	-0.6895*	-0.9989***	0.7110**
	(0.8454)	(1.3134)	(0.5842)	(0.4191)	(0.0075)	(0.3575)
Program	0.0265	-0.1227	0.0161	-0.0824*	0.2144*	-0.0708**
	(0.0886)	(0.1505)	(0.0537)	(0.0467)	(0.1165)	(0.0295)
Sample size	715	260	715	1,001	319	1,003
Mean Control	0.26	0.49	0.08	0.29	0.49	0.14

Notes
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics.

Table 7: Direct and Indirect Short Run Effects on Women aged $18 - 59$.

	Ill, past 15 days (1)	In bed (2)	Hospitalized (3)
Panel A: Titulars and Non-Titulars in Treatment Households			
Titular	-0.0029	-0.0048	0.0142*
	(0.0120)	(0.0359)	(0.0083)
Year	0.3290*	-0.1070	0.0184
	(0.1770)	(0.5380)	(0.1090)
Program	0.0046	0.0128	0.0065
	(0.0156)	(0.0575)	(0.0136)
Sample size	7,991	1,694	7,967
Mean Control (Non Titular)	0.20	0.57	0.10
Panel B: Non-Titulars in Treatment and All Women in Control Households			
Treatment	0.0313**	0.0201	0.0218*
	(0.0159)	(0.0367)	(0.0115)
Year	0.1200	0.6710**	-0.0331
	(0.1620)	(0.3200)	(0.1370)
Program	-0.0320*	0.0288	-0.0182
	(0.0178)	(0.0502)	(0.0122)
Sample size	8,546	1,677	8,297
Mean Control	0.20	0.53	0.10

Notes
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. *Titulars* are defined as those who answered the *titular* specific Module (Module 2) in the questionnaire in the treatment municipalities at the baseline.

Table 8: Direct and Indirect Medium Run Effects on Women aged $18 - 59$.

	Ill, past 15 days (1)	In bed (2)	Hospitalized (3)
Panel A: Titulars and Non-Titulars in Treatment Households			
Titular	0.0018	-0.0081	-0.0026
	(0.0134)	(0.0393)	(0.0083)
Year	0.0050	-0.0711	-0.1300
	(0.1700)	(0.6250)	(0.1180)
Program	-0.0093	-0.0315	0.0220
	(0.0221)	(0.0618)	(0.0142)
Sample size	7,067	1,508	7,040
Mean Control (Non Titular)	0.21	0.62	0.08
Panel B: Non-Titulars in Treatment and All Women in Control Households			
Treatment	0.0205	0.0220	0.0236**
	(0.0166)	(0.0382)	(0.0114)
Year	0.1510	0.5180	-0.1200
	(0.2270)	(0.4030)	(0.1260)
Program	-0.0148	0.0886	-0.0221*
	(0.0254)	(0.0572)	(0.0118)
Sample size	7,448	1,435	7,172
Mean Control	0.19	0.53	0.09

Notes
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. *Titulars* are defined as those who answered the *titular* specific Module (Module 2) in the questionnaire in the treatment municipalities at the baseline.

Table 9: Robustness

	Excluding *early treatment municipalities* Short run			Excluding *converted municipalities* Medium run		
	Ill	In Bed	Hospitalized	Ill	In Bed	Hospitalized
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0377**	0.0276	0.0243***	0.0245*	0.0540*	0.0213***
	(0.0155)	(0.0270)	(0.0085)	(0.0138)	(0.0276)	(0.0065)
Year	0.1947	0.2426	-0.0005	0.1439	-0.2522	-0.0339
	(0.1277)	(0.4187)	(0.0754)	(0.1540)	(0.2974)	(0.0549)
Program	-0.0338**	0.0271	-0.0106	-0.0210	0.0138	-0.0247***
	(0.0163)	(0.0371)	(0.0103)	(0.0182)	(0.0390)	(0.0073)
Sample Size	18,619	3,884	19,246	20,717	4,226	21,143

Notes:
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. In columns 1 – 3, estimating sample excludes the *early treatment municipalities* that received the payment before to the baseline survey was conducted. In columns 4 – 6 the estimating sample excludes the *converted municipalities* control municipalities that became treatment municipalities before the second follow-up survey was conducted.

Table 10: DID-PSM Estimation Results. Short Run

	Ill		In Bed		Hospitalized	
	A	B	A	B	A	B
DID Estimation by Kernel	-0.0232**	-0.0226**	-0.0195	-0.032	-0.0138**	-0.0138**
Common support‡	(0.0098)	(0.0096)	(0.0448)	(0.0566)	(0.0063)	(0.0061)
Unmatched Sample						
Mean Absolute Standardized Bias	9.4619	5.3515	10.9269	8.3081	9.4619	5.3515
Median Absolute Standardized Bias	7.4457	4.6146	11.6229	6.6793	7.4457	4.6146
Pseudo R2	0.047	0.017	0.065	0.038	0.047	0.017
LR χ^2	815.58***	297.08***	70.55***	41.09**	815.58***	297.08***
Matched Sample						
Mean Absolute Standardized Bias	1.3733	1.2828	3.0435	1.7636	1.3733	1.2828
Median Absolute Standardized Bias	1.218	0.8801	2.6228	1.1823	1.218	0.8801
Pseudo R2	0.002	0.001	0.006	0.002	0.002	0.001
LR χ^2	36.13	23.13	9.5	3.25	36.13	23.13
Sensitivity analysis						
Parametric						
Nearest Neighbour Matching	-0.0215*	-0.0244**	0.0047	-0.0265	-0.0189***	-0.0166**
($N = 5$) common support	(0.0117)	(0.0112)	(0.0576)	(0.0539)	(0.0079)	(0.0075)
Nearest Neighbour Matching	-0.0166	-0.0271**	0.0468	-0.0205	-0.013	-0.0143*
($N = 5$) trimming	(0.0124)	(0.0122)	(0.0625)	(0.0607)	(0.0084)	(0.0083)
Caliper Matching (radius=0.001)	-0.0164	-0.0244***	-0.0371	-0.0053	-0.0142*	-0.0137**
Common support	(0.0107)	(0.0102)	(0.0694)	(0.0634)	(0.0072)	(0.0069)
Caliper Matching (radius=0.001)	-0.0164	-0.0262***	-0.0116	-0.0161	-0.0126*	-0.012
trimming	(0.0113)	(0.0109)	(0.0726)	(0.0686)	(0.0076)	(0.0074)
Non-parametric						
Kernel Matching (bandwidth=0.06)	-0.0210**	-0.0275**	0.0014	-0.029	-0.0115	-0.0134*
(bandwidth=0.06) trimming‡	(0.0100)	(0.0112)	(0.0583)	(0.0602)	(0.0081)	(0.0069)

Notes:
‡: Bootstrapped Standard Errors. Significance: *** : 1%;** : 5%;* : 10%.

Table 11: DID-PSM Estimation Results. Medium Run

	Ill		In Bed		Hospitalized	
	A	B	A	B	A	B
DID Estimation by Kernel	-0.0340***	-0.0214**	0.0469	0.0512	-0.0249***	-0.0224**
Common support‡	(0.0116)	(0.0105)	(0.0583)	(0.0707)	(0.0065)	(0.0077)
Unmatched Sample						
Mean Absolute Standardized Bias	9.2514	4.8855	10.4717	9.7621	9.2514	4.8855
Median Absolute Standardized Bias	7.3329	3.7688	9.399	9.0464	7.3329	3.7688
Pseudo R2	0.05	0.016	0.079	0.047	0.05	0.016
LR χ^2	723.91***	228.90***	63.63***	37.73**	723.91***	228.90***
Matched Sample						
Mean Absolute Standardized Bias	1.4493	1.3412	2.8854	2.4068	1.4493	1.3412
Median Absolute Standardized Bias	1.1248	1.0324	2.3586	2.0755	1.1248	1.0324
Pseudo R2	0.002	0.001	0.007	0.003	0.002	0.001
LR χ^2	33.01	18.53	7.23	3.4	33.01	18.53
Sensitivity analysis						
Parametric						
Nearest Neighbour Matching	-0.0463***	-0.0104	0.0647	0.061	-0.0177**	-0.0251***
($N = 5$) common support	(0.0126)	(0.0121)	(0.0677)	(0.0636)	(0.0087)	(0.0083)
Nearest Neighbour Matching	-0.0422***	-0.0105	0.0213	0.0424	-0.0201**	-0.0197**
($N = 5$) trimming	(0.0133)	(0.0133)	(0.0667)	(0.0689)	(0.0091)	(0.009)
Caliper Matching (radius=0.001)	-0.0372***	-0.0158	0.0247	0.0745	-0.0191***	-0.0248***
Common support	(0.0118)	(0.0111)	(0.0855)	(0.0849)	(0.0081)	(0.0077)
Caliper Matching (radius=0.001)	-0.0357***	-0.0124	0.0476	0.0826	-0.0208***	-0.0183**
trimming	(0.0123)	(0.0119)	(0.0888)	(0.0906)	(0.0085)	(0.0082)
Non-parametric						
Kernel Matching (bandwidth=0.06)	-0.0317***	-0.0175	-0.0008	0.027	-0.0234***	-0.0187**
(bandwidth=0.06) trimming‡	(0.0120)	(0.0120)	(0.0715)	(0.0714)	(0.0084)	(0.0086)

Notes:
‡: Bootstrapped Standard Errors. Significance: *** : 1%;** : 5%;* : 10%.

Table 12: Income Effect?

	Short Run			Medium Run		
	Ill	In Bed	Hospitalized	Ill	In Bed	Hospitalized
	(1)	(2)	(3)	(4)	(5)	6)
$Program(\beta_3)$	-0.0129	-0.0181	-0.0147*	-0.0168	-0.0720	-0.0191**
	(0.0228)	(0.0440)	(0.00849)	(0.0281)	(0.0577)	(0.00962)
$Program \times \#\text{Children}(6-17) = 2(\xi_2)$	-0.00657	0.0663	0.00190	0.0193	0.0625	0.00888
	(0.0193)	(0.0439)	(0.0118)	(0.0245)	(0.0466)	(0.0126)
$Program \times \#\text{Children}(6-17) = 3(\xi_3)$	-0.0242	0.0161	0.0168	0.00674	0.0107	0.00612
	(0.0174)	(0.0418)	(0.0109)	(0.0225)	(0.0478)	(0.0127)
$Program \times \#\text{Children}(6-17) = 4(\xi_4)$	0.0144	0.182***	0.0241*	0.0263	0.00743	-0.00601
	(0.0256)	(0.0366)	(0.0145)	(0.0295)	(0.0630)	(0.0125)
$Program \times \#\text{Children}(6-17) = 5(\xi_5)$	-0.0168	0.00794	-0.00372	-0.0509	0.145	0.0181
	(0.0309)	(0.0787)	(0.0151)	(0.0422)	(0.0998)	(0.0207)
$Program \times \#\text{Children}(6-17) \geq 6(\xi_6)$	0.0193	0.274***	-0.0543***	0.00797	-0.133	-0.0125
	(0.0502)	(0.0742)	(0.0119)	(0.0390)	(0.154)	(0.0344)
Sample Size	17,106	3,920	17,343	12,352	2,662	12,489
Joint Test						
$\beta_3 + \xi_2 = 0$	0.738	2.232	3.928	0.748	2.269	3.548
$\beta_3 + \xi_3 = 0$	3.364	0.241	3.637	0.352	1.661	3.781
$\beta_3 + \xi_4 = 0$	0.441	21.46***	4.471	0.868	1.906	5.574
$\beta_3 + \xi_5 = 0$	0.774	0.172	3.356	1.868	2.556	3.567
$\beta_3 + \xi_6 = 0$	0.465	8.156	7.938	0.373	3.246	3.563

Notes:
Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality. Regressions control for a set of individual, household and municipality characteristics. Sample restricted to households with no children aged $0-5$.

Figure 1: Distribution of Propensity Scores Short Run

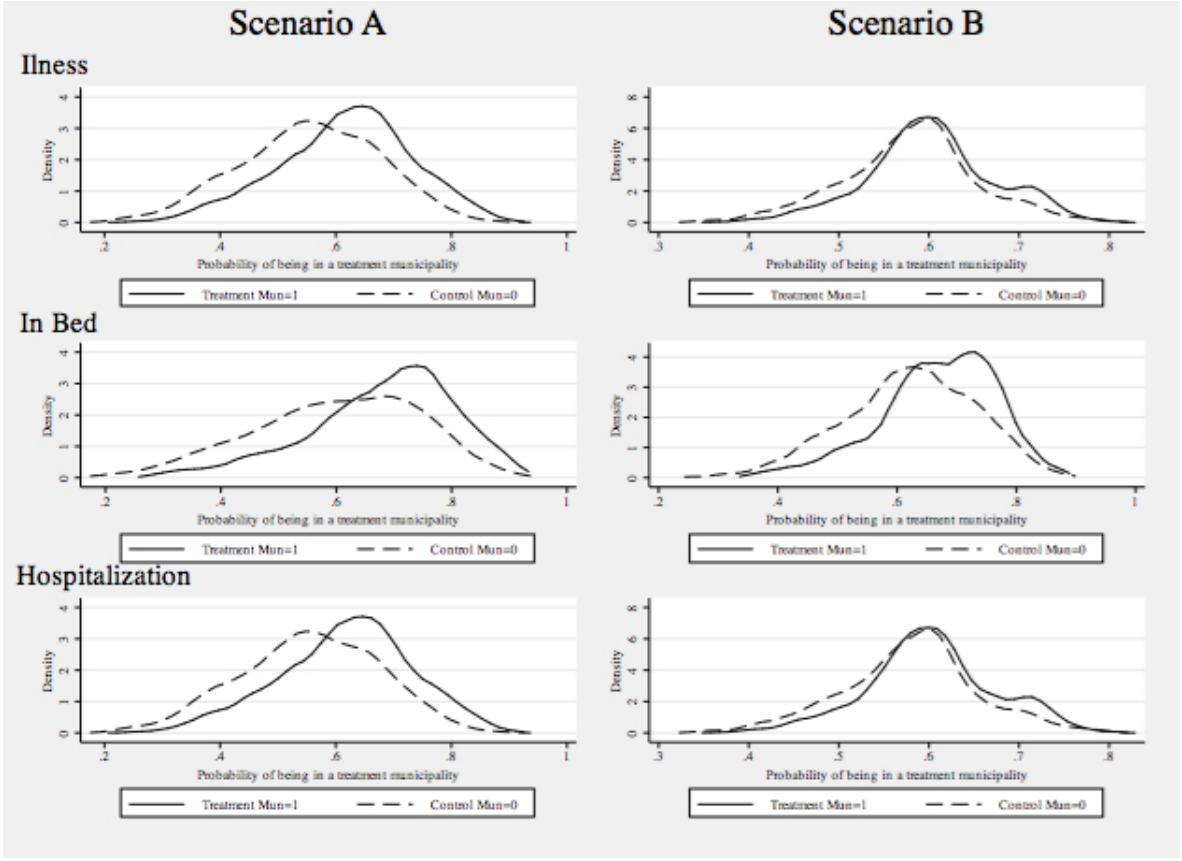
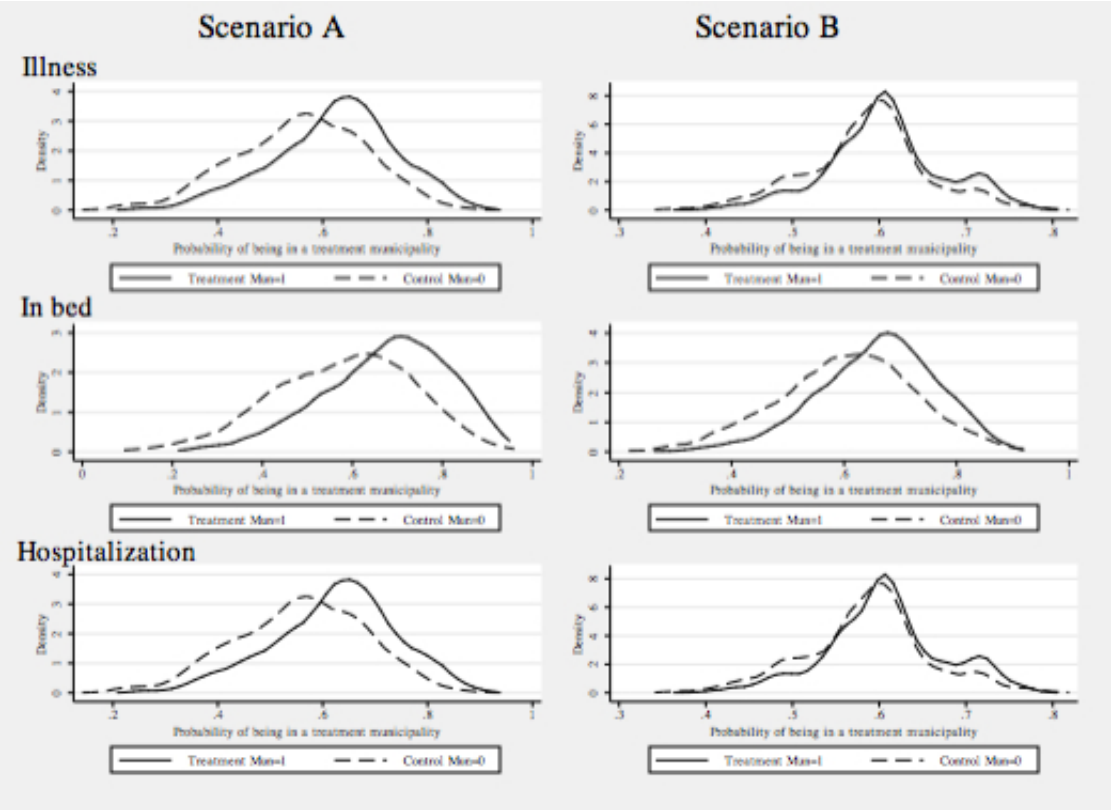


Figure 2: Distribution of Propensity Scores Medium Run



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Appendix

Table A.1: Program effects in the First Follow up: Full results for All adults

	Ill	In Bed	Hospitalization
Treatment	0.0323***	0.0259	0.0184***
	(0.00771)	(0.0206)	(0.00479)
Year	0.179*	-0.0769	0.0157
	(0.105)	(0.293)	(0.0618)
Program	-0.0296***	0.0170	-0.00547
	(0.00993)	(0.0278)	(0.00622)
Municipality literacy	-0.000254	-5.15e-05	-0.000537
	(0.000759)	(0.00207)	(0.000507)
Municipality poverty indicator	0.000816*	0.000524	5.15e-05
	(0.000427)	(0.00107)	(0.000276)
Percentage Piped water	0.0550**	0.0161	0.0179
	(0.0263)	(0.0748)	(0.0172)
Percentage piped sewer	0.00682	0.0159	0.0157
	(0.0224)	(0.0617)	(0.0151)
Number health centres	0.00330	-0.00307	0.000290
	(0.00259)	(0.00644)	(0.00157)
Number hall candidates	-0.00424**	-0.00547	-0.000287
	(0.00211)	(0.00529)	(0.00113)
Distance to the main city of the state	8.56e-05***	7.45e-05	7.48e-05***
	(2.81e-05)	(7.36e-05)	(1.80e-05)
Extension of the department	1.35e-07	-6.34e-06	1.17e-06
	(2.76e-06)	(6.40e-06)	(1.85e-06)
Municipality literacy × Time	-0.000476	-0.000384	0.000349
	(0.00100)	(0.00278)	(0.000601)
Municipality poverty indicator × Time	-0.00164***	0.000477	-0.000423
	(0.000551)	(0.00153)	(0.000312)
Percentage Piped water × Time	-0.000993	-0.105	-0.0562***
	(0.0342)	(0.0957)	(0.0209)
Percentage piped sewer × Time	-0.0738***	0.253***	0.0212
	(0.0283)	(0.0789)	(0.0176)
Number health centres × Time	-0.00274	0.00380	-0.00268
	(0.00326)	(0.00816)	(0.00212)
Number hall candidates × Time	-0.00892***	0.0199**	0.00222
	(0.00258)	(0.00777)	(0.00138)
Number of children 0 to 5	-0.00311	0.00799	0.000615
	(0.00345)	(0.00842)	(0.00215)
Number of children 6 to 17	-0.00106	0.0145***	-0.00175
	(0.00189)	(0.00461)	(0.00121)
Household income per capita	-1.61e-07**		
	(6.26e-08)		
The Household has sewage system		0.0406*	0.000353
		(0.0223)	(0.00545)
Does the water receive any treatment before be drinked		0.0302**	
		(0.0152)	
Does the Household have wc connected to sewer or septic tank		-0.0177	
		(0.0168)	
The Household has blender	-0.00874		
	(0.00636)		
The Household has color TV	-0.00990		
	(0.00652)		
The Household has sewing machine		-0.0284	

Continued . . .

Table A.1 (continued)

	Ill	In Bed	Hospitalization
		(0.0232)	
Someone in the Household has other assets besides this house		-0.0271	
		(0.0238)	
Has someone born in the Household		0.0251	0.0351***
		(0.0179)	(0.00522)
Is someone pregnant in the Household			0.0301***
			(0.00699)
Small municipality center	-0.0275***		
	(0.00921)		
Medium municipality center	0.000249		0.00523
	(0.00790)		(0.00431)
Atlantic Region		0.0801***	
		(0.0280)	
Central region		0.0611***	
		(0.0228)	
Pacific region		0.0252	
		(0.0292)	
The Household lives in grouped populated rural area of the municipality		0.0325*	-0.0189***
		(0.0180)	(0.00459)
The Household lives in sparsely populated rural area of the municipality			-0.0220***
			(0.00616)
Has someone migrated from the Household	0.0139		0.0118*
	(0.00956)		(0.00631)
Good quality floor		0.0997***	
		(0.0276)	
Good quality walls			0.00535
			(0.00412)
fuel for cooking: gas in cylinder			0.00594
			(0.00454)
fuel for cooking: wood	0.0214***		
	(0.00699)		
Any Income last month		-0.0183	
		(0.0136)	
Gender	0.0180***		0.0337***
	(0.00698)		(0.00440)
Age	0.00338***	-0.00109**	0.000393***
	(0.000220)	(0.000464)	(0.000149)
Marital Status (Married or Partnership)			0.00255
			(0.00416)
No access to health system	-0.0247***	0.0204	
	(0.00828)	(0.0231)	
Access to Private health system			0.0201*
			(0.0109)
Can Write	-0.0173**		
	(0.00674)		
Education Level: None			-0.0200***
			(0.00531)
Education Level: Incomplete Primary	-0.0109		-0.0114**
	(0.00739)		(0.00531)
Education Level: Complete Primary			-0.0139***
			(0.00461)
Working last week	-0.0464***		-0.0239***
	(0.00685)		(0.00462)

Continued . . .

Table A.1 (continued)

	Ill	In Bed	Hospitalization
Not working last week but Job	0.0690***	0.163***	0.0267*
	(0.0215)	(0.0351)	(0.0160)
Disable to work			0.136***
			(0.0281)
Study last week		-0.0580	-0.0536***
		(0.0787)	(0.00783)
Head of the household	0.0366***		
	(0.00730)		
Sample Size	26,884	5,686	27,688
Mean Control	0.20	0.55	0.08

Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality.
Regressions control for a set of individual, household and municipality characteristics.

Table A.2: Program effects in the Second Follow up: Full results for All adults

	Ill	In Bed	Hospitalization
Treatment	0.0327***	0.0322	0.0197***
	(0.00794)	(0.0216)	(0.00478)
Year	0.00549	-0.231	-0.0523
	(0.113)	(0.273)	(0.0700)
Program	-0.0180*	0.0417	-0.0164**
	(0.0109)	(0.0299)	(0.00659)
Municipality literacy	-0.000143	-0.000245	-0.000308
	(0.000790)	(0.00217)	(0.000501)
Municipality poverty indicator	0.000650	0.00115	0.000251
	(0.000451)	(0.00117)	(0.000286)
Percentage Piped water	0.0608**	-0.0405	0.0162
	(0.0270)	(0.0797)	(0.0173)
Percentage piped sewer	0.00168	0.0467	0.0128
	(0.0232)	(0.0648)	(0.0151)
Number health centres	0.00424	-0.00357	0.000297
	(0.00268)	(0.00661)	(0.00154)
Number hall candidates	-0.00610***	-0.00565	-0.00113
	(0.00217)	(0.00568)	(0.00115)
Distance to the main city of the state	6.40e-05**	-7.28e-05	5.02e-05***
	(2.92e-05)	(7.92e-05)	(1.76e-05)
Extension of the department	1.94e-06	-1.04e-05	-7.42e-07
	(2.95e-06)	(7.10e-06)	(1.69e-06)
Municipality literacy × Time	0.00167	0.00309	0.000918
	(0.00107)	(0.00280)	(0.000658)
Municipality poverty indicator × Time	-0.00126**	-0.00111	-3.55e-05
	(0.000594)	(0.00142)	(0.000360)
Percentage Piped water × Time	-0.218***	0.00606	-0.0586**
	(0.0374)	(0.100)	(0.0228)
Percentage piped sewer × Time	0.108***	-0.0127	0.0349*
	(0.0311)	(0.0847)	(0.0196)
Number health centres × Time	-0.00570	-0.00431	-0.00486*
	(0.00348)	(0.0102)	(0.00251)
Number hall candidates × Time	0.0102***	0.00380	0.00148
	(0.00253)	(0.00740)	(0.00149)
Number of children 0 to 5	-0.00119	0.00628	0.00275
	(0.00364)	(0.00895)	(0.00216)
Number of children 6 to 17	-0.000875	0.0112**	-0.00298**
	(0.00197)	(0.00508)	(0.00118)
Household income per capita	-7.03e-08		
	(6.44e-08)		
The Household has sewage system		0.0189	0.00289
		(0.0233)	(0.00542)
Does the water receive any treatment before be drinked		0.0146	
		(0.0161)	
Does the Household have wc connected to sewer or septic tank		-0.0334*	
		(0.0179)	
The Household has blender	-0.0113*		
	(0.00662)		
The Household has color TV	-0.0123*		
	(0.00692)		
The Household has sewing machine		-0.0135	
		(0.0263)	
Someone in the Household has other assets besides this house		-0.00577	
		(0.0262)	

Continued . . .

Table A.2 (continued)

	Ill	In Bed	Hospitalization
Has someone born in the Household		0.0154	0.0353***
		(0.0200)	(0.00521)
Is someone pregnant in the Household			-0.00942
			(0.00634)
Small municipality center	-0.0120		
	(0.00985)		
Medium municipality center	0.0157*		0.00132
	(0.00866)		(0.00422)
Atlantic Region		0.0221	
		(0.0303)	
Central region		0.0300	
		(0.0246)	
Pacific region		0.0605*	
		(0.0313)	
The Household lives in grouped populated rural area of the municipality		-0.00709	-0.0105**
		(0.0185)	(0.00470)
The Household lives in sparsely populated rural area of the municipality			-0.0216***
			(0.00562)
Has someone migrated from the Household	0.0257**		0.0110*
	(0.0102)		(0.00611)
Good quality floor		0.0469	
		(0.0310)	
Good quality walls			0.00790*
			(0.00415)
fuel for cooking: gas in cylinder			0.00748
			(0.00462)
fuel for cooking: wood	0.0174**		
	(0.00739)		
Any Income last month		-0.00488	
		(0.0149)	
Gender	0.0287***		0.0331***
	(0.00751)		(0.00454)
Age	0.00314***	-0.00153***	0.000401***
	(0.000234)	(0.000525)	(0.000150)
Marital Status (Married or Partnership)			0.00646
			(0.00416)
No access to health system	-0.0134	-0.00146	
	(0.00902)	(0.0253)	
Access to Private health system			0.0166*
			(0.00997)
Can Write	-0.0152**		
	(0.00719)		
Education Level: None			-0.0109**
			(0.00544)
Education Level: Incomplete Primary	-0.00884		-0.0101*
	(0.00777)		(0.00531)
Education Level: Complete Primary			-0.0118**
			(0.00466)
Working last week	-0.0429***		-0.0160***
	(0.00729)		(0.00466)
Not working last week but Job	0.0762***	0.129***	0.0276*
	(0.0216)	(0.0400)	(0.0157)
Disable to work			0.131***

Continued . . .

Table A.2 (continued)

	Ill	In Bed	Hospitalization
			(0.0293)
Study last week		0.113	-0.0307**
		(0.0837)	(0.0124)
Head of the household	0.0316***		
	(0.00792)		
Sample Size	23,338	4,722	23,862
Mean Control	0.18	0.54	0.07

Significance: *** : 1%;** : 5%;* : 10%. Standard errors clustered by municipality.
Regressions control for a set of individual, household and municipality characteristics.

Table A.3: Observables used in DID-PSM Estimations

Observable Characteristics Scenario A	Observable Characteristics Scenario B‡
Household income per capita	Health Insurance of head:
The Household has piped water	Access to Private health system
The Household has waste collection service	Covered by the health system
The water facility is into the house	Access to Public health system
Does the water receive any treatment before be drinked	Age of the head
The Household has fridge	Age of the spouse
The Household has radio	Single parent
The Household has bicycle	*Education Level of the head:*
The Household has motorcycle	No education
The Household has fan	Incomplete primary
The Household has color TV	Complete primary
The Household has kerosene lamp	Incomplete secondary
The Household has livestock	*Education level spouse:*
Has someone died in the Household in the last year	No education
Small municipality center	Incomplete primary
Medium municipality center	Complete primary
Atlantic Region	Incomplete secondary
Central region	*House walls:*
The Household lives in grouped populated rural area of the municipality	Good quality wood
The Household lives in sparsely populated rural area of the municipality	Poor quality wood
Good quality walls	Mud, Cardboard/none
Poor quality walls	The Household has piped gas service
Number of children between 12 and 17	The Household has piped water
Number of household members	The Household has sewage system
Age of the head of the Household	The Household has waste collection service
Age of the spouce of the head of the Household	The Household has telephone access
Head with no partner	Does the Household have toilet connected to sewer or septic tank
Education level of the spouse: Complete primary	Ownership of this house
No access to health system by the Household	Household suffered from violence 2000-2002

Notes:
‡: See Attanasio, Fitzsimons, Gomez, Gutierrez, Meghir, and Mesnard (2010)