KU LEUVEN

CENTER FOR ECONOMIC STUDIES

DISCUSSION PAPER SERIES
DPS14.03

JANUARY 2014

Neighborhood peer effects in the use of preventive health care

Nicolas BOUCKAERT

Public Economics

Faculty of Economics
And Business

Neighborhood peer effects in the use of preventive health care.

Nicolas Bouckaert*

January 15, 2014

Abstract

Individual participation in preventive care may depend on preventive health behavior in an individual's peer group. This paper analyzes the importance of social interactions in the context of new social policies (PROGRESA) in Mexico that aim to increase the participation in different types of preventive care. We follow the promising approach of analyzing social interactions in real world peer groups. Identification of social interactions is based on a partial-population design.

Results indicate that PROGRESA succeeded in increasing preventive care usage among program eligible households. In addition, endogenous social interactions increase preventive care usage both among eligibles and non-eligibles for various types of prevention. The overall treatment effect of PROGRESA on prevention can be decomposed in a direct effect related to financial incentives and an indirect effect related to social interactions. The indirect effect accounts for 10% up to 58% of the total treatment effect.

Keywords: preventive care; non-participation; social interactions; PROGRESA; partial-population design; treatment effects.

JEL Classification: C31, C93, I12, I18, I38, H51.

*Center for Economic Studies, KULeuven (nicolas.bouckaert@kuleuven.be).

1 Introduction

Health and income are two major constituents of individual well-being. The first foundations for both are laid during pregnancy and childhood. A vast literature describes the impact of good nutrition and health in utero and in childhood on, amongst others, life expectancy, physical and cognitive development, schooling outcomes, labour market opportunities, and income (see e.g. Behrman, 1996; Case *et al.*, 2002; Case *et al.* 2005; Currie, 2009; Currie & Madrian, 1999; Cutler *et al.*, 2006; Van den Berg *et al.*, 2006). Children born in poor households are more likely to have worse health and begin life at a distinct disadvantage in these different domains.[1]

Poverty was widespread in Mexico around 1997. Extreme poverty is concentrated in rural areas accommodating about a quarter of the Mexican population, but 60% of the extreme poor (World Bank, 2004, 2005). In 1997, the Mexican government set up a new nationwide anti-poverty program, baptized PROGRESA.[2] The program is targeted at the extreme poor in rural areas and is designed as a conditional cash transfer program, meaning that families receive social transfers conditional on the household engaging in a set of behaviors. Program requirements include participation in perinatal care, child health care and immunization, growth and weight monitoring of children, primary and secondary schooling, adult preventive check-ups and nutrition monitoring and supplementation, and finally participation in informational meetings where health and nutrition topics are discussed (pláticas). In this way, the program tries to break the feedback mechanisms that lead to an intergenerational transmission of poverty. By focussing on perinatal care, children's health, nutrition, and schooling, the objective is to enhance poor children's human capital accumulation, and hence future opportunities. By providing monetary resources to families in need and adult preventive care, current poverty is also alleviated.[3]

In this paper, we analyze the impact of PROGRESA on the participation in and usage of different types of preventive care. We look at the use of deworming drugs, participation of females in cervical cancer screening, take-up of blood sugar and blood pressure tests by adults, the weight and growth monitoring of children, and child immunization. Despite a high burden of these diseases in Mexico compared to other countries, participation in prevention was low or modest around the start of PROGRESA. Vaccination

[1] It is not entirely clear whether the correlation between low parental socioeconomic status (SES) and the lower health status of their children implies a causal relation or that a third factor causes both effects. However evidence increasingly indicates that low parental SES causes poor child health (Currie, 2009). Causality is important if one wants to create or adjust policies to improve individual opportunities.

[2] PROGRESA is an acronym for Programa de Educacion, Salud y Alimentacion (the Education, Health and Nutrition Program). The program was renamed Oportunidades in 2002, but since we use data from the period 1997-1999, we will refer to PROGRESA.

[3] The monetary transfers are generally given to the mother of the family, under the implicit assumption that resources managed by women are more likely to be used for schooling, nutrition and other family necessities than money controlled by men.

rates among children were an exception with over 90% vaccination coverage. Section 2 enters more into details on health care usage in Mexico and shows that there was a need for improvement in the different health domains. PROGRESA was an instrument of the Mexican government to increase preventive care participation, and health care participation in general, and to change misperceptions on prevention among the rural poor. An analysis of the program effects on health indicates that PROGRESA had a significant positive effect on both adult's and children's health (Barham, 2005, 2011; Gertler, 2000, 2004; Lagarde *et al.*, 2007; Ranganathan & Lagarde, 2012).

The primary focus in this paper is the role of social interactions[4] on the individual or household decision to participate in preventive care. Understanding how social interactions influence behavior is important for policymaking since they could reinforce or offset the direct (financial) incentives given by a social program to influence the individual participation decision. Social interaction effects might therefore lead to higher or lower participation rates than otherwise expected and a social program might reach non-targeted individuals and households through social spillovers. In combination with (temporary) direct incentives for behavioral change, social interactions can move a society from a low adoption equilibrium into a high adoption equilibrium (Kremer & Miguel, 2007). Once direct incentives are reduced, important social interaction effects can support the high participation equilibrium. This is especially important for a country like Mexico – characterized by low participation rates in different types of preventive care – that aims at durably increasing participation rates.

Peer effects might work through a variety of channels (see e.g. Oster & Thornton, 2012; Noguera *et al.*, 2013; Young, 2009). First, social interactions can be the result of informational conformity through signaling[5] or (implicit or explicit) information sharing on benefits, costs or beliefs. Second, individual decisions can be reinforced through a desire to 'fit in' with others in the reference group or a pressure to follow prevailing social practices. This has been named pure conformity or imitation.[6] Individuals might also learn how to use a drugs or product from their peers. Given that the preventive care that is analyzed in this paper is either easy to apply (taking deworming drugs) or administered by a health professional (vaccination, cancer screening, blood tests), learning is not expected to play an important role. Informational and pure conformity on the other hand are likely to play a role in the decision making process.

[4] A peer effect or social interaction effect occurs when the action or belief of one individual affects the actions or beliefs of other individuals belonging to the same social group.

[5] (Non-)Participation in prevention by a peer might send a signal that prevention yields a higher (lower) level of utility. This might encourage (discourage) participation.

[6] Puur conformity or imitation reflects the idea that the best life is attained if one behaves as others in one's surrounding and stays away from acting out of the ordinary. As Patacchini & Zenou (2009, pp. 2-3) note, it may well be best expressed in the old saying: "When in Rome, do as the Romans do."

Estimating peer effects has proven to be challenging because of problems of reflection, correlated unobservables and endogenous group membership (Manski, 1993). It is difficult to disentangle whether an individual decision is influenced by the decisions of her peers or vice versa, or that both the decisions of the peers and the individual are driven by e.g. shared common individual characteristics, such as income or education levels, or changes in the environment. A variety of techniques have been used to refine estimates of how peer decisions influence individual decisions. Early research estimated peer effects as the link between the propensity of the peer group to engage in a certain behavior and individual behavior, while controlling for as many group characteristics as possible. Deri (2005) is an example of this approach for health service utilization in Canada, Aizer & Currie (2004) analyze social network effects for participation in publicly funded prenatal care and delivery services. More recently, researchers use explicit randomization, where a random subset of individuals is 'treated' differently, and this random variation is used as additional information to identify social interactions more accurately. This line of research exists both for exogenously assigned peer groups and for existing peer groups. An example in health economics of the former is Carrell *et al.* (2011) who analyze fitness outcomes among students at the US Air Force academy who are randomly assigned to squadrons. The problem with this type of study is that peer groups are sometimes created artificially and it is difficult to establish whether estimates are specific to the created situation or are informative for social interactions in the real world. Estimates of peer effects in naturally occurring peer groups are therefore potentially more convincing. Kremer & Miguel (2007), for example, analyze peer effects in the usage of deworming drugs in Kenya using information on household social links; Rao *et al.* (2007) estimate peer effects in vaccination decisions among students using Facebook to derive information on their social network; and Oster & Thornton (2012) look at the role of social interactions in the usage of menstrual cups in Nepal in a school environment.[7]

We follow the promising approach of analyzing social interactions in real world peer groups. We exploit random variation in the eligibility status of individuals and treatment status of localities in PROGRESA as identifying elements in a partial-population setting. As will be discussed below, treatment and control villages are randomly chosen and eligibility status is exogenously determined by the government. This random variation is unrelated to other elements that determine participation and allow us to deal with Manki's identification issues. Methodologically, our approach follows the framework proposed by Lalive & Cattaneo (2009) and Bobonis & Finan (2009), who analyze the role of peer effects in school enrollment using PROGRESA data. An individual difference in difference approach is used in which behavioral changes related to the implementation of PROGRESA are analyzed. The difference in difference approach

[7]For an overview of research on social interactions in different economic research fields, see e.g. Dahl et al. (2012).

makes it possible to control for general trends and time invariant heterogeneity. Avitabile (2011) and Barzallo (2011) have done analyses that look at indirect treatment effects of PROGRESA on health care utilization and health.[8] They find positive spillover effects for participation in cervical cancer screening (Avitabile, 2011) and medical check-ups, and for child and adult health (Barzallo, 2011), while no indirect effect is found for blood pressure and blood sugar tests (Avitabile, 2011). Our approach surpasses their analyses, since we disentangle the indirect treatment effect in contextual, correlated and endogenous social interactions. In addition to the identification of endogenous social interactions, we also assess the relative importance of social interaction effects compared to direct financial incentives in changing preventive care participation.

Evidence of the role of social interactions on participation in or usage of preventive care is mixed. Most papers find positive peer effects (e.g. Aizer & Currie, 2004; Deri, 2005; Godlonton & Thornton, 2012; Oster & Thornton, 2012; Munshi & Myaux, 2006; Rao *et al.*, 2007), others find no effect (e.g. Meredith *et al.*, 2013), and even negative effects are found (e.g. Kremer & Miguel, 2007). The divergence in the results can be explained by the relative importance of the different channels through which social interactions play. Our results indicate that PROGRESA was successful in increasing preventive care usage both among eligible and non-eligible households in treatment villages relative to households in control villages. We were able to isolate endogenous social interactions and showed that significant positive interaction effects are present for deworming drugs usage, cervical cancer screening, blood pressure tests, and child growth and weight monitoring. The magnitude of the peer effects is, however, different depending on the type of preventive care. Social interaction effects are especially high for participation in annual growth and weight monitoring of children.[9] Using the information on social interactions, the total treatment effect can be decomposed in a direct effect, related to the financial incentive given to eligible households for complying with PROGRESA requirements, and an indirect social interaction effect. The indirect effect accounts for 10% up to 58% of the total treatment effect for the eligibles, a non-negligible element in explaining the change in preventive health behavior.

The remainder of this paper is structured as follows. Section 2 presents a brief discussion of prevention in Mexico, the PROGRESA program, and the data used in the analysis. We lay out our research question in section 3 and provide descriptive evidence. In section 4, we discuss our research design and identification strategy. The main results are presented in section 5, followed by a robustness analysis and a conclusion

[8] With indirect treatment effects, we mean behavioural changes of the non-eligible population in treatment villages.

[9] Annual participation in growth and weight monitoring was already high before PROGRESA was introduced and was further increased among the eligibles through the financial incentives. It is possible that non-participation became socially disapproved and the desire to conform higher than for other types of preventive care.

Table 1: OECD data (year 1997) on health indicators from Mexico, Chile and the US

	Mexico	**Chile**	**US**
Doctor consultations per capita	2.3	8.2	3.7
Cervical cancer screening rate (% of females aged 20-69 screened) (data from 2000)	9.7%	64.5%	–
Cervical cancer mortality (deaths per 100.000 females, age standardized)	20.4	15	3.5
Diabetes mortality (deaths per 100.000 individuals, age standardized)	103	30.7	27.7
Circulatory disease mortality (deaths per 100.000 individuals,age standardized)	341.6	322.4	424.7
Infectious disease mortality (deaths per 100.000 individuals, age standardized)	34.3	30	21.5
Neonatal mortality (deaths per 1000 live births)	14	5.7	4.8
Infant mortality (deaths per 1000 live births)	23.8	10	7.2
Low birthweight infants (% of live births)	9.2%	4.8%	7.5%
Immunization rate: measles (% of children immunised)	91.0%	96.0%	91.0%

Note: Unless otherwise stated, the presented data is OECD Health data from 1997.

in sections 6 and 7.

2 PROGRESA program and evaluation data

2.1 Prevention in Mexico

In this paper, we look at the use of deworming drugs, participation of females in cervical cancer screening, take-up of blood sugar and blood pressure tests by adults, the weight and growth monitoring of children, and child immunization. Sánchez-Castillo *et al.* (2004) state that traditionally, Mexico's health concerns have been childhood malnutrition and infectious diseases, although the latter has been overtaken by cardiovascular diseases, cancers, and diabetes as the principal causes of death. The health care indicators chosen in this paper are at the core of the challenges faced by the Mexican health care system in 1997.

In the late nineties, we can state that – except for immunization – Mexico was underperforming in different main aspects of health care. Table 1 provides a comparison of some key indicators with respect to the chosen health variables based on OECD and WHO data. Mexico is compared to two OECD countries, its neighboring country, the US, and Chile, which has a similar GDP per capita. Rather than providing a detailed overview of the Mexican health care system in comparison to other countries, the purpose of the provided information is to show that Mexico in 1997 underperformed with respect to the health variables chosen in this paper and that action was needed.

Cervical cancer was the first cause of death due to neoplasms among Mexican women (Agurto *et al.*, 2004). In fact, mortality rates were among the highest in the Americas (Lewis, 2004). Even though a screening program existed since 1974[10], and cervical cancer is fully treatable when discovered early, cancer screening among Mexican females was very low with, in the year 2000, a participation rate of 10% among females aged 20 to 69 compared to 65% in Chile. Cited reasons for non-participation were low quality of screening, a perceived breach of privacy when the pap smear is taken by male doctors, a lack of knowledge, a preference for ignorance since cancer is perceived as deadly, and seeking of medical assistance when the cancer has already entered its late stages rather than screening when feeling healthy (Agurto *et al.*, 2004; Watkins *et al.*, 2002).

The death burden caused by diabetes – 103 deaths per 100.000 individuals in 1997 – was very high and over three times as large in Mexico than in Chile or the US. Together with hypertension, diabetes increases the risk for heart failure. The prevalence of hypertension was 33.3% in men and 25.6% in women (Sánchez-Castillo *et al.*, 2004). In 1997, diseases of the circulatory system were as common in Mexico as in Chile, and 25% less common than in the US.

Infectious and parasitic diseases accounted for 34 deaths per 100.000 individuals (age standardized rates) in 1997 and the death rate was 10% higher than in Chile and 50% higher than in the US.

Neonatal and infant mortality were double as high in Mexico in 1997 as in Chile and three times as high as in the US. Moreover, the percentage of children born with low birthweight was 9% in Mexico, or twice as high as in Chile. WHO figures, with respect to height and weight profiles of Mexican children under 5 years, indicate that they were on average smaller and weighed less than children in the US and in Chile. Nonetheless, important improvements have been made with respect to child mortality in the period 1980-1997 with a halving of the mortality rate among children under 5 years old (Sepúlveda *et al.*, 2006). Moreover, anaemia and micronutrient deficiencies were highly prevalent in Mexico. These conditions can be improved by providing iron and zinc supplements, among others (Sepúlveda *et al.*, 2006).

Finally, vaccination rates among children were high, over 90% and comparable to those in the US and Chile. After a deadly measles epidemic in 1989-1990, the Mexican government established the successful Mexican universal vaccination program in 1991 (Barham, 2005; Sepúlveda *et al.*, 2006). By October 1992, coverage rates for tuberculosis and measles were 95% and 91%, respectively.

[10] The national cervical cancer screening program in Mexico offers free screening regardless of age and income and tries to raise awareness among women aged 25 and over.

2.2 Program background

In 1997, the Mexican government initiated a large-scale social program aimed at complementing the income of marginalized households in the poorest rural communities and fostering human capital accumulation among children. Monetary transfers were handed out as of 1998 and are conditional on compliance of behavior in two distinct components: 'education' and 'nutrition and health'.

The educational channel consists of bimonthly grants for children aged less than 17 years that regularly attend grades 3 to 9. Program transfers do not cover all costs, they are differentiated according to grade and gender[11] and are capped at a maximum of three enrolled children.

In exchange for cash transfer and nutritional supplements, PROGRESA's health and nutrition component requires regular free medical check-ups, growth and weight monitoring and vaccination of young children and perinatal care for pregnant women. Other family members have to visit a local health center at least yearly for a free check-up and preventive care. Program beneficiaries are also required to participate in pláticas, i.e. informational meetings where issues on health, hygiene and nutrition are discussed. It is possible to participate in the health and nutrition component without claiming educational grants, but not vice versa (Bobba, 2012).

PROGRESA is a targeted program. Beneficiaries were identified in two steps (see INSP, 2005). First, highly marginalized rural villages with between 50 and 2.500 inhabitants were selected for sequential entry into the PROGRESA program using a deprivation index. The villages needed to have access to schooling and health care. Next, within the selected villages, poor families were identified. A poverty index score was attributed to all households based on an assessment of their permanent income and household composition. Households with an index score below a certain region-specific threshold were considered poor and could qualify for PROGRESA transfers. Eligibility status and the corresponding rights and benefits were clearly communicated through village-wide assembly meetings. Eligibility status (and non-eligibility status) was awarded for three years and only eligible families that lived in villages where PROGRESA was implemented became potential program beneficiaries. In 1998, PROGRESA was available in 34.400 localities (1.6 million households), and coverage reached as many as 48.700 localities (2.3 million households) in 1999 and 67.500 localities (3.1 million households) in 2001.

An important feature of PROGRESA is that it included an evaluation component. The evaluation

[11] Grants increase as children reach higher grades and they are higher for girls than for boys. The latter is to enhance the educational level of girls, which is below that of boys.

design allows the analysis of PROGRESA as a partial-population intervention[12] that is phased in at random. For the evaluation, a subset of 506 localities were selected from across seven states clustered around Mexico city. In October 1997, an initial survey collected socioeconomic information to determine eligibility status of households in all 506 localities.[13] On average, 52% of the households were eligible for PROGRESA, but the percentages vary substantially across localities. Finally, a set of 320 localities were randomly selected as treatment group where PROGRESA was implemented as of April 1998. The remaining 186 communities acted as a control group and were phased in at the start of 2000. The randomization of treatment and control groups has the advantage that it should ensure that both groups are balanced in terms of observable and unobservable characteristics. Using appropriate techniques, the effects of PROGRESA can therefore be reliably identified. Behrman & Todd (1999), as well as many authors that used PROGRESA data in the past, have checked whether pre-program behavior and observable background characteristics are similar in control and treatment groups. They conclude that the randomization procedure worked effectively.

The PROGRESA interventions are designed to improve health and development from the very start of life. In a first step, PROGRESA aims to decrease the number of low birth weight babies. Low birth weight babies are more susceptible to deficiencies and diseases and run a higher risk of neonatal and infant mortality (Currie, 2009; Gertler, 2000). As discussed in the previous subsection, low birth weight, neonatal and infant mortality are more common problems in Mexico than they are in e.g. Chile or the US. While some low birth weight babies are able to catch up with their contemporaries, most of them tend to suffer a development disadvantage throughout childhood with potential consequences on future opportunities (Gertler, 2000). PROGRESA imposes pregnant women to have at least 5 prenatal care visits and offers nutritional supplements when needed.

In a second step, young children as well as their lactating mothers are required to attend medical check-ups for growth and weight monitoring and immunization. Children below 24 months are required to attend a check-up at least every two months, while children between 24 and 60 months have an appointment scheduled every four months (Gertler, 2004). Children who lag behind in physical development or are found to be malnourished receive protein and micronutrient supplement, either directly or via their

[12] A partial-population intervention refers to a design with treated and non-treated (control) clusters. Within the treated clusters only a subset of units are offered the treatment (Baird *et al.*, 2012; Moffitt, 2001).

[13] By July 1999, PROGRESA reclassified a large number of non-eligible households as eligible for the program benefits after complaints that the initial procedure discriminated against the elderly poor who no longer live with their children. The revised households (26% of the evaluation sample) are called the densificado group. However, by August 2000, PROGRESA staff found that many of the newly admitted households had not collected any benefit. Apparently, few densificado households had been notified of their revised eligibility status for the program (Buddelmeyer and Skoufias, 2004). Given that we limit our analyses to the first year of the program (March 1998 to March 1999), we consider these households as non-eligible.

lactating mother. From the pioneering work of Robert W. Fogel, we know that there is a robust relationship between height and economic well-being, and economists have found, for example, that adult height is related to earnings (Currie, 2009). Nutrition and development during childhood is likely to play an important role in this relation. Case & Paxson (2006) argue that poor nutrition during childhood likely affects both future cognitive performance and adult height, explaining the observed correlation. The obligation of growth and weight monitoring for infants combined with the distribution of nutritional supplements potentially has a high pay-off in terms of future human capital accumulation. Immunization policies aim to avoid the occurrence of serious and/or contagious diseases, such as the measles, mumps, tetanus, polio, hepatitis A and B, etc. The Mexican government has a vaccination scheme for children that determines which vaccinations are required at what age; the details of which are elaborated in official norms.[14]

In a third step, attention is paid to the health of adolescents and adults. In order to receive transfers, every family member has to attend a yearly medical check-up. Special attention is paid to reproductive health, family planning, the detection and (preventive) treatment of parasites, of arterial hypertension, of diabetes mellitus, and of cervical cancer (Gertler, 2000). The dangers of these disorders, and the benefits of early detection and treatment, are discussed as well as health and hygiene habits. Participation in cervical cancer screening (pap smear test), usage of deworming drugs, and take-up of blood sugar and blood pressure tests are not obligatory in order to receive PROGRESA transfers, but are encouraged in the obligatory pláticas and medical check-ups.

2.3 Data and sample selection

In the evaluation sample, extensive surveys have been carried out to document the effects of PROGRESA. There are two baseline surveys (October 1997 and March 1998) and three post-program surveys (October 1998, March 1999 and November 1999) on all 24.000 households in the 506 localities. At the start of 2000, the control group was phased in into the program and additional surveys were conducted. In our analysis, we primarily use the two baseline surveys and the first two post-program surveys.

Each survey contains detailed information on household demographics, socioeconomic status, education, income, expenditures, consumption and health. Not every survey asks the same questions. Questions on the use of health care services and usage are asked in March 1998, October 1998 and March 1999, while many pre-program background characteristics are observed in October 1997. Next to household

[14]Examples of such norms around the time PROGRESA was implemented are the Norma oficial Mexicana 031-SSA2-1999 on children's health or the Norma oficial Mexicana 036-SSA2-2002 which brings together prevailing norms and rules on prevention, vaccination, toxic substances etc.

or individual specific information, there are also locality surveys with information on local prices, wages and health service availability.

Individual level data is available for growth and weight monitoring of children below the age of 5. Prior to program initiation (March 1998 survey), it was asked whether a child had attended a growth and weight check-up in the past year and if so, how many times. After PROGRESA had started in the treatment villages, the same questions were asked, but for the past six months (October 1998 and March 1999 survey). Two participation variables are constructed: one variable that indicates whether a child had attended at least one check-up in the past year (evaluated in March 1998 and March 1999), and another variable that indicates whether a child had attended the required number of growth and weight check-ups as imposed by PROGRESA, evaluated for the past year in March 1998 and for the past six months in March 1999. For the latter participation variable, we choose to focus on the post-program period October 1998 to March 1999, rather than the period April 1998 to March 1999, since PROGRESA was only introduced in April 1998 and it is likely that a switch in monitoring frequency takes at least some transition time.

With respect to vaccination, data are available on vaccination of measles, tuberculosis, tetanus and polio. In March 1998 and October 1998, vaccination history is recorded for children below the age of 5, while in March 1999, the information is available only for children below the age of 2. We will focus on take-up of the vaccinations of tuberculosis and measles, since these are infrequent and therefore easily observed. There is one shot at birth for tuberculosis and one shot before age 1 for measles with a renewal around age 6. For tetanus and polio, there are at least four shots before the age of 5 and the data is not recorded accurately enough to follow the vaccination history unambiguously (Barham, 2005). When possible, we evaluate vaccination status in March 1999 and compare it with vaccination status in March 1998, however, for older children who are unobserved in March 1999, we derive post-program vaccination from the October 1998 survey.

For the usage of deworming drugs and the check-ups for blood sugar and blood pressure, we have household level data on whether someone in the household has taken these drugs or tests in the past year (March 1998 survey) or in the past six months (October 1998 and March 1999 survey). In the latter case, a yearly equivalent take-up variable is generated in order to analyze changes in yearly participation before and after program implementation. With respect to cervical cancer screening, the data are also at the household level, but more information is available. In March 1998, participants were asked if someone in the household had ever participated in screening and if so, in which year. After program implementation, participants were asked whether someone in the household took a screening test in the last six months. In 1997, the official Mexican norm for cervical cancer screening prescribed a test every three years (after normal test results for two consecutive years).[15] We create a variable that checks compliance with this norm both before (evaluated in March 1998) and after the implementation of PROGRESA (evaluated in March 1999) and analyze the changes in compliance.

3 Research question and descriptive evidence

The basic idea of this paper is that social interactions might play a role in the decision to participate or use preventive care. We assume that the social interactions occur at the locality level, since we lack information on the actual social network of an individual. Thus, the peer group of a child or a household are all other sampled children[16] or households in the locality. This choice is justifiable, since rural localities are quite small, with 47 households per village on average. Moreover, Adato (2000, p. vi) documents "a common identity in poverty" within the localities. Despite the division created by PROGRESA, there is a perception that everyone is poor, and that "beneficiaries and non-beneficiaries continue to get along with each other fine and 'the same' as before" (Adato, 2000, p. vi). This suggest that social relationships go beyond program eligibility status.

Tables 10 to 16 in appendix 1 present descriptive statistics on individual and household characteristics of the entire sample as well as the subsamples used in our empirical analyses. A distinction is made between eligibles and non-eligibles in control and treatment villages. In Table 10, we observe that literacy of the household heads and their partners in the rural villages is around 65% to 70% and is somewhat higher among the non-poor than the poor. A similar fraction has at least started primary school, but only a minor group has moved on to secondary school or beyond (5% among the poor and 8% among the non-poor). Among the group of non-eligible households, household heads and their partner in control villages are more likely to have started primary education and be able to read and write. Among the group of poor households, the partners of the household heads in control villages are more likely to have started secondary education. Aside from an educational imbalance in favor of control villages, differences between control and treatment villages are minor or non-existent, as one would expect from the random assignment of villages. Among the poor, we find a statistically significant difference in civil status. In treatment villages, couples tend to be married more frequently than in control villages, whereas in control

[15] The recommended screening frequency is laid down by the official Mexican screening norm NOM-014-SSA2-1994 and its modifications.

[16] We exclude other children living in the same household from the reference group of a child.

villages couples are more likely to live together outside marriage. Considering couples irrespective of mode of cohabitation, the differences cancel out.

A major difference between poor and non-poor households can be observed in the marginality index (the criterion for the distinction between both) and other wealth variables. The non-poor have better dwellings (more likely with a cement floor and firm roof) in which they live with fewer household members. They are also more likely to have a car and agricultural assets. Their schooling is better and they are less likely to be from indigenous origin. Finally, the non-poor household heads are more likely female, and they and their partners are on average older than their poor counterparts.

Since we limit our sample to households with pre- and post-baseline answers, there is a risk of sample selection and attrition. If we look at the subsamples in Tables 11 to 14, we observe, in general, similar trends as for the entire sample. The subsamples are, however, better educated, more literate and they have younger and fewer female household heads both for eligibles and non-eligibles in control and treatment villages. The educational and literacy imbalance in favor of control villages remains in the subsamples as well as the difference in mode of cohabitation among the eligibles. In addition, inhabitants of treatment villages are more likely to have tile roofs. The deviations from the complete sample are limited, which gives us confidence that our estimation results are applicable to the population.

The subsamples for growth and weight monitoring and vaccination, i.e. Tables 15 and 16, contain younger and better educated households than the entire sample. As could be expected, the households in this subsample consist of more couples and have more household members. The differences between control and treatment villages show the same trend as those for the entire sample.

Table 2 provides descriptive evidence on the effect of PROGRESA on different types of preventive care. Pre- and post program values are reported both for eligibles and non-eligibles averaged over control and treatment villages. Households in control villages give information on the counterfactual situation without PROGRESA, under the assumptions that randomization at village level was successful and that control villages are not indirectly affected by the program. Several conclusions can be drawn from Table 2.

First, pre-program differences between control and treatment villages exist, but are very small and, in general, statistically not significant. One exception is growth and weight monitoring of children at a yearly frequency in non-eligible households. Participation in monitoring was 5% higher in control villages before PROGRESA was implemented. The lack of significant differences is again an indication of successful randomization.

Table 2: Descriptive evidence on the effect of PROGRESA on participation in prevention

	Eligible				Non-eligible			
	Program	Control	Difference (SD)		Program	Control	Difference (SD)	
Deworming drugs usage								
Drugs usage pre-program	0.511	0.507	0.003	(0.022)	0.439	0.463	-0.024	(0.017)
Drugs usage post-program	0.831	0.719	0.113	(0.018)***	0.636	0.633	0.003	(0.016)
Change in drugs usage	0.321	0.211	0.109	(0.018)***	0.197	0.170	0.027	(0.017)†
Observations	6616	3808			5280	3463		
Cervical cancer screening								
In accordance with screening norm pre-program	0.220	0.247	-0.028	(0.021)	0.270	0.283	-0.014	(0.019)
In accordance with screening norm post-program	0.641	0.474	0.167	(0.026)***	0.542	0.526	0.016	(0.021)
Change in accordance screening norm	0.422	0.227	0.195	(0.019)***	0.272	0.242	0.030	(0.016)*
Observations	6403	3676			5001	3331		
Blood sugar test								
Blood sugar test pre-program	0.232	0.220	0.013	(0.020)	0.314	0.315	0.000	(0.020)
Blood sugar test post-program	0.642	0.420	0.222	(0.024)***	0.539	0.522	0.017	(0.021)
Change in blood sugar test participation	0.409	0.200	0.209	(0.023)***	0.225	0.208	0.017	(0.018)
Observations	6441	3685			5198	3386		
Blood pressure test								
Blood pressure test pre-program	0.355	0.339	0.016	(0.023)	0.459	0.469	-0.010	(0.022)
Blood pressure test post-program	0.769	0.539	0.230	(0.024)***	0.675	0.646	0.029	(0.020)†
Change in blood pressure test participation	0.414	0.200	0.214	(0.022)***	0.216	0.177	0.039	(0.019)**
Observations	6530	3717			5297	3446		
Growth and weight monitoring (yearly)								
Monitoring (at least yearly) pre-program	0.811	0.831	-0.021	(0.024)	0.824	0.873	-0.049	(0.023)**
Monitoring (at least yearly) post-program	0.988	0.946	0.042	(0.010)***	0.962	0.965	-0.003	(0.010)
Change in monitoring (at least yearly)	0.177	0.115	0.062	(0.021)***	0.138	0.093	0.046	(0.020)**
Observations	6518	3773			2148	1554		
Growth and weight monitoring (PROGRESA frequency)								
Monitoring (PROGRESA frequency) pre-program	0.244	0.254	-0.009	(0.010)	0.261	0.263	-0.002	(0.017)
Monitoring (PROGRESA frequency) post-program	0.788	0.674	0.113	(0.010)***	0.659	0.629	0.030	(0.018)*
Change in monitoring (PROGRESA frequency)	0.544	0.421	0.123	(0.014)***	0.398	0.366	0.032	(0.024)
Observations	5194	3056			1651	1208		
Compliance with vaccination scheme								
Vaccination compliance pre-program	0.925	0.929	-0.004	(0.008)	0.927	0.931	-0.004	(0.010)
Vaccination compliance post-program	0.989	0.991	-0.002	(0.002)	0.985	0.987	-0.002	(0.004)
Change in vaccination compliance	0.064	0.062	0.002	(0.008)	0.058	0.056	0.002	(0.010)
Observations	7088	4187			2451	1661		

Note: Mean pre-program values are reported as measured in March 1998. Mean post-program values are reported as measured in October 1998 and/or March 1999. Differences are estimated using OLS regression with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of differences: † p<0.15, * p<0.10, ** p<0.05, *** p<0.01

Source: PROGRESA evaluation data

Second, the pre-program participation rates for blood sugar test, blood pressure test, and cervical cancer screening are systematically higher among non-eligibles than among eligibles. For the use of deworming drugs, the opposite is true. For prevention among children, pre-program participation rates are comparable among eligibles and non-eligibles.

Third, the changes in preventive behavior between pre- and post-program levels suggest an increasing participation pattern in preventive care. The trend is especially pronounced for the types of preventive care aimed at adolescents and adults and for monitoring at PROGRESA frequency. The fraction of households that is in accordance with the screening norm or took a blood sugar test almost tripled in one year among eligible households in treatment villages, going from 22% to 64%, and it almost doubled in the remainder of the population. Similar effects are observed for child weight and growth monitoring at PROGRESA frequency. There are also substantial increases in preventive behavior for deworming drugs usage and blood pressure tests. Participation, or usage, increased by 60% to 120% for eligibles in treatment villages and between 40% and 60% in other parts of the population. Annual growth monitoring and child vaccination have high pre-program participation rates, over 80% and over 90%, respectively. Hence, the change in behavior is much less pronounced. After program implementation, full participation is almost attained. The increase is fairly equal for vaccination, while for growth monitoring, the change in participation is more pronounced in treatment villages.

Fourth, in the post-program period, we observe that differences between treatment and control villages turn positive and significant for eligibles, except for child vaccination. Also, differences in pre-post changes of preventive behavior show significance for the eligibles. This is a first indication of the total treatment effects of PROGRESA on the beneficiary population and suggests a positive contribution of PROGRESA to health prevention. We can infer from Table 2, for example, that the program increased compliance with the cervical cancer screening norm by 19,5 percentage points more among eligibles and by 21 percentage points for blood sugar and blood pressure tests. The program effects implied by the difference in pre-post changes between control and treatment villages are made even more explicit in Table 3. Panel A.1 reproduces the findings of Table 2 for eligibles and panel B.1 for non-eligibles. Panels A.2 and B.2 show that the magnitude of the PROGRESA effects are smaller once individual and household characteristics are controlled for. Significance remains high for eligibles, but decreases for non-eligibles due to the smaller effects.

Fifth, the pre-post changes of preventive behavior among the non-eligibles are indicators for the indirect spillover effects. A much smaller difference in changes in preventive behavior is observed between non-eligibles in control and treatment villages. With respect to cervical cancer screening for example, the difference in the increase in compliance was 19,5 percentage points among eligibles in treatment and control villages, whereas it is only 3 percentage points among non-eligibles. The differences remain, however, significant for deworming drugs usage, cervical cancer screening, blood pressure tests and annual child monitoring. This suggests the existence of indirect spillover effects and potentially of endogenous social interaction. The effects are small and non-significant for monitoring at PROGRESA frequency and vaccination. This implies weak or no spillover effects. In the next section, we discuss our identification strategy in order to more exactly measure the social interaction effects.

Finally, a high participation rate is recorded for annual growth and weight monitoring of young children. After program implementation, it becomes almost universal. However, compliance with the frequency of visits imposed by PROGRESA is not common practice before program implementation. In March 1998, only a fifth of all children were monitored according to PROGRESA requirements in the past year. This, however, drastically increased in the year following implementation and in the period October 1998 to March 1999, compliance according to PROGRESA requirements increased to 80% among eligibles in treatment localities and around 65% for the other groups.

Table 3: Participation in prevention: treatment and spillover effects among eligibles and non-eligibles

Dependent variable: Changes in	Deworming drugs usage	Cancer screening	Blood sugar test	Blood pressure test	Monitoring (yearly)	Monitoring (Progresa)	Vaccination compliance
A. Eligibles							
A.1 OLS regression without controls							
PROGRESA treatment	0.109***	0.195***	0.209***	0.214***	0.062***	0.123***	0.002
(Standard error)	(0.018)	(0.019)	(0.023)	(0.022)	(0.021)	(0.024)	(0.008)
A.2 OLS regression with individual and household controls							
PROGRESA treatment	0.112***	0.188***	0.204***	0.210***	0.062***	0.132***	0.004
(Standard error)	(0.017)	(0.018)	(0.022)	(0.021)	(0.020)	(0.024)	(0.008)
Observations	10235	9892	9942	10057	10059	8105	10737
B. Non-eligibles							
B.1 OLS regression without controls							
PROGRESA treatment	0.027†	0.030*	0.017	0.039**	0.046**	0.032	0.003
(Standard error)	(0.017)	(0.016)	(0.018)	(0.019)	(0.020)	(0.031)	(0.010)
B.2 OLS regression with individual and household controls							
PROGRESA treatment	0.022	0.028*	0.014	0.033*	0.045**	0.039	0.007
(Standard error)	(0.016)	(0.015)	(0.018)	(0.018)	(0.020)	(0.031)	(0.010)
Observations	8538	8141	8382	8537	3608	2791	3897

Note: Coefficient estimates from OLS regressions are reported with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of coefficients: † $p<0.15$, * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Individual and household controls are those listed in the Tables with descriptive evidence, i.e. Tables 10 to 16. For cervical cancer screening, the regression controls for the number of females aged 16 or more in the household.
Source: PROGRESA evaluation data

4 Research design and identification strategy

We use a linear-in-means model to estimate social interactions. Variations of a general linear-in-means model are popular specifications that are used frequently in empirical work on social interaction effects in crime, schooling, fertility, labor market decisions, participation in welfare programs, etc. The linear-in-means specification can be derived as the unique Bayes-Nash equilibrium of a complete information game in which each individual's expected utility consists in a private benefit and a conformity benefit (Blume *et al.*, 2010).[17] This means that social interactions in this setting stem from conformity behavior. It captures both informational conformity and pure conformity elements. For our purpose, the model's foundations seems to fit our analysis since informational and pure conformity are valid choices as drivers of social interactions when analyzing changes in usage and participation in prevention. For immunization decisions, other strategic social behavior is possible, since, for contagious diseases, individuals could free ride on the preventive effort of others. However, since vaccination participation rates were high even before PROGRESA was initiated, it is more likely that conformity with prevailing social practice will dominate the social interaction effects.

4.1 Linear-in-means model

Let H_{igv} denote the change[18] in preventive care participation and usage between March 1998 and March 1999 of child/family i in peer group g in locality v. Since we do not have better information on the social connections of individuals or households within a locality, we assume that g and v coincide.[19] Therefore, we drop the subscript v. A value $P_{ig} = 1$ indicates that the family is eligible for PROGRESA, while $P_{ig} = 0$ corresponds to non-eligibility status. An indicator variable T indicates the treatment status of a locality. A value $T = 1$ denotes a PROGRESA treatment village, while control villages have an indicator value $T = 0$.

Equation (1) looks at the effect of PROGRESA on changes in preventive behavior.

$$H_{ig} = \theta_0 + \theta_1 T + \mu_{ig} \tag{1}$$

The randomized implementation of PROGRESA implies $E(\mu_{ig}|T) = 0$ when estimating eq. (1) in the

[17] The benefits are specified using a quadratic function, which is not unusual when modelling conformity (Akerlof, 1997). Two other assumptions in the model are that the size of the peer group is finite and group membership is exogenous.

[18] Our analysis focuses on changes in behaviour rather than the actual behaviour at a moment in time. This difference-in-difference approach allows us to control for time-invariant (un)observed individual, village-level and other heterogeneity.

[19] The use of village-wide peer groups has the disadvantage that village-specific shocks to the studied beliefs and behaviour cannot be controlled for. Some papers infer family connections using information on surnames, see e.g. Angelucci *et al.* (2010). This information is however not publically available.

eligible and non-eligible subsamples. Table 3 shows the results of the regression in both subsamples. The total effect of the PROGRESA program is captured by the parameter value θ_1^E if eq. (1) is estimated among the eligibles (superscript E).[20] This includes both a direct effect as a result of the cash transfers and subsequent change in behavior and a feedback effect due to social interactions. At this stage, it is not possible to disentangle both effects. As noted above, this effect is significant and positive for all preventive care variables, except child immunization. If the subgroup of non-eligibles (superscript NE) is considered, θ_1^{NE} gives an estimate of the indirect treatment effect of PROGRESA that spilled over from the eligibles to the non-eligibles.[21] Table 3 shows that these effects are in general smaller and less significant. Social interaction effects are part of the spillover effects.

In order to estimate social interactions, we use the following specification of the linear-in-means model:

$$H_{ig} = \alpha + \beta X_{ig} + \gamma X_g + \rho P_{ig} + \xi P_g + \zeta H_g + \eta S_g + \varepsilon_{ig} \tag{2}$$

where X_{ig} are exogenous characteristics of the individual (that are not time invariant) and X_g, P_g and H_g are the peer group - excluding individual i - averaged counterparts of X_{ig}, P_{ig} and H_{ig}, respectively. Changes in the shared environment of peer group g are captured by S_g. Identifying social interactions based on equation (2) is challenging, if not impossible.

First of all, group-level effects on preventive behavior can be different in nature. There are correlated effects, which means that individuals in the same peer group tend to behave similarly simply because they have similar individual characteristics or face similar economic/institutional/natural environments. In our approach, they are captured by parameter β, η and the difference in difference approach. Contextual peer effects arise when exogenous group characteristics influence individual behavior. This is represented in eq. (2) by γ and also captured by the difference in difference specification. Parameter ζ measures endogenous peer effects, which means that individual behavioral changes relate to changes in behavior of others in the peer group. This is the parameter of interest if we want to determine the presence of social interactions.

Secondly, while researchers are especially interested in the endogenous social interactions, Manski (1993) showed that the different effects cannot be separately identified in the typical linear-in-means model as in eq. (2), due to self-selection into similar groups and simultaneity of individual behavior. However, the specific design of PROGRESA's evaluation component allows us to address these two identification problems under fairly weak assumptions. The fact that individuals tend to self-select

[20] This is sometimes denoted the average treatment effect of the treated (ATT).
[21] This is sometimes denoted the average treatment effect of the non-treated (ATNT) or indirect treatment effect (ITE).

into similar groups is an omitted variable problem (Lalive and Cattaneo, 2009). Randomization of the PROGRESA treatment implies that whether or not a household resides in a treatment or control village is independent from unobservables that might affect our dependent variables, thereby addressing the omitted variable problem. The simultaneity of individual behavior relates to the fact that each member in a social group affects every other member. Behavioral changes are jointly observed, and it is unclear who affected who. Moffitt (2001) showed that this problem can be overcome in a partial-population setting, whereby the outcome of a randomly chosen subgroup is exogenously altered by some treatment. This is exactly what happens in PROGRESA. PROGRESA has a treatment selection at the locality level (random division between control and treatment villages) and a poverty eligibility threshold at the household level. As shown below, identification relies on the fact that an exogenously determined subset of households within a treated village remains untreated, and on the crucial assumption that non-eligibles in control villages provide a valid counterfactual. We have shown evidence of the successful randomization in the evaluation component of PROGRESA. We can rewrite equation (2) with an additional treatment variable for eligibles in treated villages ($P_{ig}T$):

$$H_{ig} = \alpha + \beta X_{ig} + \gamma X_g + \rho P_{ig} + \xi P_g + \zeta H_g + \eta S_g + \delta P_{ig} T + \varepsilon_{ig} \tag{3}$$

The direct effect of the program is now captured by δ, while the peer group effect is identified by ζ times the change in average peer group preventive behavior. Beneficiaries in treated villages are influenced by both, while non-eligibles are not subject to the direct effect. If we look at the subgroups of eligibles and non-eligibles, the equations are:

$$H^E_{ig} = \alpha + \beta X_{ig} + \gamma X_g + \rho + \xi P_g + \zeta H_g + \eta S_g + \delta T + \varepsilon_{ig} \tag{3'}$$

$$H^{NE}_{ig} = \alpha + \beta X_{ig} + \gamma X_g + \xi P_g + \zeta H_g + \eta S_g + \varepsilon_{ig} \tag{3''}$$

The peer group averaged outcome can be decomposed in the underlying subgroup averages:

$$H_g = P_g H^E_g + (1 - P_g) H^{NE}_g \tag{4}$$

Taking the expectations of eqs. (3') and (3") and inserting them in eq. (4), gives the following expression:

$$H_g = \frac{\alpha}{1-\zeta} + \frac{\beta+\gamma}{1-\zeta}X_g + \frac{\rho+\xi}{1-\zeta}P_g + \frac{\eta}{1-\zeta}S_g + \frac{\delta}{1-\zeta}P_gT \tag{5}$$

Equation (5) does not allow us to directly estimate ζ, but it suggests an identification strategy. Since P_gT is not included in eq. (3"), inserting eq. (5) in eq. (3") provides an identification method:

$$H_{ig}^{NE} = \frac{\alpha}{1-\zeta} + \beta X_{ig} + \frac{\beta\zeta+\gamma}{1-\zeta}X_g + \frac{\zeta\rho+\xi}{1-\zeta}P_g + \frac{\eta}{1-\zeta}S_g + \zeta\frac{\delta}{1-\zeta}P_gT + \varepsilon_{ig} \tag{6}$$

Exploiting the treatment effect in PROGRESA's partial-population design results in two reduced-form equations, eqs. (5) and (6). The endogenous social interactions, represented by ζ, can be identified as the ratio of the treatment effect of PROGRESA on H_{ig}^{NE} to the treatment effect of PROGRESA on H_g. More specifically, the two reduced-form equations can be estimated with the "eligible share in PROGRESA treatment villages" as an instrument for changes in average preventive behavior in the peer group of non-eligible families.[22]

The IV identification strategy relies on the fact that the "eligible share in PROGRESA treatment villages" is correlated with average peer group outcomes and uncorrelated with the error term in eq. (6). The correlation between the instrument and changes in peer group outcomes can be estimated (see Table 4), the lack of correlation with the error term is, however, not directly testable. We can provide evidence to support this assumption. The random assignment of localities to the control and treatment group is a first indication (see above). Secondly, we can estimate the correlation between our dependent variables and the PROGRESA locality treatment status before the program was introduced. The results are shown above in Table 2 and indicate no significant difference in the pre-program values of the different types of prevention, except for a difference of 5 percentage points in participation rates for annual growth monitoring between non-eligible children in control and treatment villages. Thirdly, the IV strategy is based on the idea that changes in preventive behavior among non-eligibles in treatment villages result from the PROGRESA induced changes in preventive behavior among the eligibles within the locality. They do not come from changes in contextual variables and non-eligibles are not affected through other channels. We condition our estimations on a large number of peer group contextual variables and as a robustness check (see Sections 6 and 7), we introduce a variety of features that might affect individual

[22] Bobonis and Finan (2009) use PROGRESA treatment as instrument (T), rather than the interaction of PROGRESA treatment and the fraction of eligibles (P_gT). They do this, because the share of eligibles in a village may not be exogenous if there is any sorting of families in and out of the village based on unobservable characteristics of the households or villages. However, since villages are randomly assigned to treatment status and eligibility is fixed for three years, we keep the theoretically proposed instrument, the method also chosen by Lalive and Cattaneo (2009). Our results however do not quantitatively change when using T as an instrument, but the precision of the estimates decreases. Adding T as a second instrument, does not lead to different or more precisely estimated coefficients.

preventive behavior, e.g. geographic variation, changes in waiting time, supply, and quality of health care facilities that might explain changes in the take-up of prevention.

4.2 Direct versus indirect effect

We have now identified the endogenous social interaction effect ζ. As can be seen in eqs. (5) and (6), the social interaction effect gives leverage to changes in average group characteristics. The leverage factor $(1-\zeta)^{-1}$ is called the social multiplier.

From a policy point of view, we are not only interested in the presence and magnitude of endogenous social interaction effects, we are equally interested in the program effects on individual behavior. In the end, we want estimates for the direct effect, δ, and the indirect effect, i.e. ζ times the change in peer group preventive care usage. This would allow us to decompose the total program effect in its constituting parts for eligibles in treated villages. In the previous subsection, the identification of ζ was discussed. The change in peer group preventive behavior can be measured, therefore the remaining challenge is to identify δ. As shown by Lalive and Cattaneo (2009) it is possible to identify the direct effect by subtracting changes in peer group average preventive behavior H_g from changes in individual values H_{ig}.

$$H_{ig} - H_g = \beta(X_{ig} - X_g) + \rho(P_{ig} - P_g) + \delta T(P_{ig} - P_g) + \varepsilon_{ig} \tag{7}$$

How should we think about the different effects? Consider a treated village with one beneficiary household and many non-eligible households. The beneficiary household will get a direct effect of δ, but no indirect effect, since no other households are affected. If, on the other hand, all households would be eligible, the program would generate an effect δ among all eligibles, but would in addition also generate a social effect, because behavior in the peer group changes. Because all households in the peer group are now more likely to participate in prevention, this creates an additional effect on preventive behavior of $\zeta\delta$, this indirect effect creates a second order effect of $\zeta^2\delta$, and so on. If all indirect effects are added, the resulting effect is $\zeta\delta(1-\zeta)^{-1}$, or the coefficient of the indirect program effect on individual behavior in eq. (6).

5 Results

5.1 Estimation of neighborhood peer effects

Table 4: IV and OLS estimates of endogenous peer effects among non-eligibles

Dependent variable:	**Deworming drugs usage**		**Cancer Screening**		**Sugar test**		**Pressure test**		**Monitoring (yearly)**		**Monitoring (Progresa)**		**Vaccination compliance**	
A. IV estimates (eq. 3") (Dependent variable: Changes in individual values H_{ig}^{NE}; Coefficient of interest: ζ)														
Changes in peer group mean values (H_g)	0.411**	0.368*	0.327***	0.247*	0.286**	0.145	0.381***	0.288**	0.422	0.637***	0.403*	0.530**	-0.352	1.262
(Standard error)	(0.160)	(0.218)	(0.098)	(0.131)	(0.117)	(0.154)	(0.101)	(0.127)	(0.333)	(0.214)	(0.241)	(0.227)	(17.508)	(2.214)
Individual and household controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Peer group contextual controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8743	8538	8332	8141	8584	8382	8743	8537	3694	3608	2859	2791	3979	3897
B. OLS Peer group regression (eq. 5) (Dependent Variable: Changes in peer group mean values H_g; Coefficient of interest: $\delta(1-\zeta)^{-1}$)														
PROGRESA treatment x eligible fraction (P_gT)	0.145***	0.130***	0.216***	0.198***	0.242***	0.223***	0.273***	0.252***	0.060**	0.088***	0.152***	0.167***	0.001	0.005
(Standard error)	(0.025)	(0.026)	(0.027)	(0.029)	(0.033)	(0.034)	(0.033)	(0.033)	(0.024)	(0.025)	(0.031)	(0.032)	(0.009)	(0.010)
F-statistic	34.03***	3.82***	65.22***	3.18***	52.91***	3.77***	70.04***	4.03***	6.42**	3.05***	24.73***	2.08**	0.01	1.61***
Kleibergen-Paap statistic	34.03	24.14	65.22	47.36	52.91	42.61	70.04	59.80	6.42	12.29	24.73	27.38	0.01	0.29
C. OLS Individual regression (eq. 6) (Dependent Variable: Changes in individual values H_{ig}^{NE}; Coefficient of interest: $\zeta\delta(1-\zeta)^{-1}$)														
PROGRESA treatment x eligible fraction (P_gT)	0.060**	0.048	0.071**	0.049†	0.069*	0.032	0.104***	0.073*	0.025	0.056**	0.061	0.089*	0.000	0.007
(Standard error)	(0.030)	(0.035)	(0.027)	(0.031)	(0.036)	(0.038)	(0.036)	(0.039)	(0.026)	(0.028)	(0.044)	(0.047)	(0.014)	(0.016)
F-statistic	3.90**	3.04***	6.76***	1.98***	3.76**	2.52***	8.17***	1.86***	0.92	2.85***	1.99	2.50***	0.00	2.88***

Note: Coefficient estimates from OLS and 2SLS IV regressions are reported with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of coefficients: † p<0.15, * p<0.10, ** p<0.05, *** p<0.01. Individual and household controls are those listed in the Tables with descriptive evidence, i.e. Tables 10 to 16. For cervical cancer screening, the regression controls for the number of females aged 16 or more in the household. The Stock-Yogo (2005) critical value for the Kleibergen-Paap weak identification statistic at 10% maximal IV size is 16.38 and 8.96 at the 15% maximal IV size.
Source: PROGRESA evaluation data

Table 4 reports the main results of the neighborhood peer effects estimates. Panel A provides the IV estimates of the endogenous social interaction effect, ζ, from eq. (3"). It results from the estimates of the two reduced-form equations, eqs. (5) and (6). Panel B reports the effects of the former, while the latter is presented in panel C. Estimates are reported both with and without controlling for individual and household characteristics and contextual effects. For adolescent and adult preventive care, the magnitude of the spillover effect estimates decreases once control variables are included. For child preventive care, the opposite is found.[23] Taking deworming drugs usage as an example, the results should be read as follows: when the eligible fraction in the peer group of a non-eligible household living in a PROGRESA treated village increases from zero to one, the average usage rate in the peer group increases by 13 percentage points. This increase in peer group usage leads to a 4.8 percentage point increase in the usage of the non-eligible household. The peer group responsiveness is generally stronger than the behavioral change of non-eligibles, because the peer group partly consist of eligibles, whose behavioral change is financially stimulated. The relation between the peer group responsiveness and the individual responsiveness gives the social interaction estimator. As the individual responsiveness increases relative to the peer group responsiveness, this translates into a higher social interaction parameter.

The first row in Table 4 indicates that social interaction effects are present and significant for four types of preventive care, i.e. deworming drugs usage, blood pressure test, cervical cancer screening, and growth monitoring. The magnitude of the social interaction effect varies across the different types of prevention. They are especially important for annual weight and growth monitoring of children. For vaccination compliance, no effects are found. For participation in blood sugar tests, minor positive effects are found, but estimated imprecisely.

We test for potential weakness of the instrumental variable using the Kleibergen-Paap Wald statistic. Contrary to the Cragg-Donald Wald statistic, the Kleibergen-Paap Wald statistic is robust to clustered standard errors. The test shows that the instrument is, in general, not weak when controls are added, except for vaccination compliance. This indicates an unreliable estimation of social interactions with respect to vaccination compliance. The rejection of weak instruments for the other types of preventive care supports the reliability of our baseline estimates.

What can we learn from the results in Tables 2 to 4? Immunization of children below 5 years old against tuberculosis and measles has been generally adopted before PROGRESA was set up and

[23] Especially the inclusion of contextual effects X_g and of the fraction of poor in the community P_g have an important influence on the changes in coefficient point estimates. Their omission creates some bias at first sight. However, the point estimates are statistically not significantly different from each other.

compliance among this group of children increased further as they aged. Vaccination compliance was not different between eligibles and non-eligibles in control and treatment villages. Table 3 shows no PROGRESA effect among eligibles or non-eligibles. The lack of direct impact of PROGRESA explains the absence of indirect social interaction effects.

Participation in annual growth and weight monitoring of children was high before PROGRESA started and increased to almost full participation one year later. Pre-program monitoring according to PROGRESA's guidelines was much lower. Less than a third of all children below 5 years were monitored regularly, but compliance more than tripled among treated eligibles after one program year. It increased slightly less among the other groups. The increase in child monitoring on an annual basis and according to PROGRESA frequency is 6.2 and 12.3 percentage higher among the eligibles in treatment villages than in control villages, respectively, providing evidence of a PROGRESA treatment effect. The results in Table 4 show the presence of endogenous social interactions for annual monitoring, as well as for monitoring at PROGRESA frequency.

With respect to adolescent and adult preventive health care, the patterns are similar, but the actual magnitude of the effects differ. Despite a relatively high prevalence of diabetes and cervical cancer in Mexico (see Section 2.1), participation rates for cervical cancer screening and blood sugar test were low. The pre-program participation for households in our sample was below 25% for eligibles and a little above 25% for non-eligibles. Take-up of blood pressure tests and the usage of deworming drugs was higher and fluctuated between 35% and 50%. In Tables 2 and 3, we observe a large increase in preventive take-up in all layers of the population for all four types of prevention. The increase among eligibles in treatment villages is, however, much more pronounced, allowing us to conclude that there was an important direct effect of the stimuli to attend preventive check-ups. The change in behavior among non-eligibles was also systematically higher in treatment villages than in control villages, the difference is, however, small, and not significant for blood sugar tests. Our social interaction estimates in Table 4 suggest that the spillover effects from eligibles to non-eligibles are (partly) the result of social interactions, with significant effects for deworming drugs usage, participation in cervical cancer screening and take-up of blood pressure tests. For blood sugar tests, the social interaction effect is not significantly different from zero once control variables and contextual effects are added.

We conclude that PROGRESA had a direct effect on preventive behavior, especially among the treated households. The results show that non-eligible families in treated villages also changed their preventive behavior, albeit to a lesser extent. Social interactions play a positive reinforcing role in the transmission. It is not entirely clear what is driving the social interaction effect, pure conformity or informational conformity or a combination of both.

5.2 Direct versus indirect effect

In the PROGRESA program, policymakers give financial stimuli to poor households in order to change their preventive health behavior. In the previous subsection, we have shown that the effect of PROGRESA among eligibles in treated villages is important. We have equally shown that a social interaction effect exists that reinforces the behavioral change related to the direct financial incentive and triggers behavioral changes among non-eligibles. For policymaking, it is important to understand what part of the change in behavior can be attributed to the financial stimulus and what part is related to social interactions.

Table 5 presents the decomposition of the total treatment effect of PROGRESA on eligibles and non-eligibles in a direct and an indirect effect. The analysis is performed for all types of preventive care except vaccination, since in the previous sections, we have found no indication of a direct or indirect effect for child immunization. Panel A shows the results for the eligibles and panel B for the non-eligibles. Only the indirect effect plays for the latter. Row 1 in panel A shows the estimation of the direct effect as laid down in eq. (7), while row 4 shows the indirect effect. The direct effect is always significant and it varies from a 3 percentage point increase in growth monitoring on an annual base to a 18.5 percentage point increase in the take-up of blood sugar tests. The indirect effect is smaller and increases participation rates in prevention by 1.5 to 6.5 percentage points. It is, in general, smaller among the non-eligibles than among the eligibles. This is a result of a different composition of the peer groups of eligibles and non-eligibles living in treatment villages. The fraction of eligible households in the peer group of an eligible household is higher than in the peer group of a non-eligible household.[24] As the fraction of eligibles increases, a larger share of households are affected by the direct effect, which leads to a stronger change in average peer group behavior, consequently leading to more important indirect effects as well.

The total treatment effect (row 5) is the combination of the direct and indirect effect. If we calculate the share of the indirect effect in the total treatment effect, we find that social interactions amount to 16% of the total change in cervical cancer prevention. It increases up to around 20% for deworming drugs usage and blood pressure test and 46% and 58% for child growth and weight monitoring, respectively at PROGRESA frequency and on an annual base. At least for these types of preventive care, it appears that social interactions explain a non-negligible part of the change in preventive behavior that is observed after the introduction of PROGRESA.

[24] For example, in the subsample of cervical cancer screening, the fraction of other eligible households in the peer group of a (treated) eligible household is 60%, whereas it is only 44% in the peer group of a non-eligible household.

Table 5: Decomposition of total treatment effect of PROGRESA

Dependent variable: Changes in	Deworming drugs usage	Cancer screening	Blood sugar test	Blood pressure test	Monitoring (yearly)	Monitoring (Progresa)
A. Eligibles						
1. Direct treatment effect	0.088***	0.150***	0.185***	0.171***	0.030†	0.079**
(Standard error)	(0.020)	(0.018)	(0.021)	(0.021)	(0.020)	(0.037)
2. Social interaction parameter	0.386*	0.247*	0.145	0.288**	0.637***	0.530**
(Standard error)	(0.218)	(0.131)	(0.154)	(0.127)	(0.214)	(0.227)
3. PROGRESA effect on peer group	0.069***	0.118***	0.119***	0.142***	0.065***	0.124***
(Standard error)	(0.014)	(0.015)	(0.019)	(0.019)	(0.018)	(0.021)
4. Indirect effect (2 x 3)	0.026†	0.029*	0.017	0.041**	0.041**	0.066**
(Standard error)	(0.016)	(0.016)	(0.018)	(0.019)	(0.018)	(0.030)
5. Total treatment effect (1 + 4)	0.114***	0.179***	0.202***	0.212***	0.072***	0.145***
(Standard error)	(0.026)	(0.024)	(0.028)	(0.028)	(0.027)	(0.048)
Indirect effect as % of total effect	22.5%	16.3%	8.5%	19.3%	57.7%	45.5%
B. Non-eligibles						
1. Direct treatment effect	0.000	0.000	0.000	0.000	0.000	0.000
(Standard error)	–	–	–	–	–	–
2. Social interaction parameter	0.386*	0.247**	0.145	0.288**	0.637***	0.530**
(Standard error)	(0.218)	(0.131)	(0.154)	(0.127)	(0.214)	(0.227)
3. PROGRESA effect on peer group	0.056***	0.089***	0.094***	0.110***	0.067***	0.100***
(Standard error)	(0.013)	(0.014)	(0.016)	(0.016)	(0.016)	(0.022)
4. Indirect effect (2 x 3)	0.021†	0.022*	0.014	0.032**	0.043**	0.053**
(Standard error)	(0.013)	(0.012)	(0.015)	(0.015)	(0.018)	(0.026)
5. Total treatment effect (1 + 4)	0.021†	0.022*	0.014	0.032**	0.043**	0.053**
(Standard error)	(0.013)	(0.012)	(0.015)	(0.015)	(0.018)	(0.026)

Note: Coefficients in rows 1 are the result of estimating eq. 7 on the sample of eligibles and non-eligibles combined. Coefficients in rows 2 come from Table 4. Coefficients in rows 3 are the result of an OLS regression of change in average peer group value on "treatment village" and control variables. All coefficients in rows 1 to 3 have robust standard errors that allow for correlation of disturbance terms within localities. Standard errors in rows 4 and 5 are computed using the Delta method. Significance levels of coefficients: † p<0.15, * p<0.10, ** p<0.05, *** p<0.01.
Source: PROGRESA evaluation data

Table 6: Descriptive evidence on health supply, quality and price changes

	Pre-program				Post-program			
	Treatment	Control	Difference	(SD)	Treatment	Control	Difference	(SD)
Health care provider present (0=No, 1=Yes)	0.959	0.945	0.014	(0.022)	0.960	0.968	-0.007	(0.017)
Services available (0 to 7)	2.727	2.958	-0.232	(0.217)	3.556	3.368	0.188	(0.235)
Opening time (hours per week)	10.443	10.233	0.210	(0.267)	9.233	9.192	0.041	(0.226)
Availability staff and equipment (0 to 1)	0.577	0.561	0.015	(0.017)	0.575	0.586	-0.012	(0.016)
Quality of doctors (0 to 1)	0.972	0.976	-0.004	(0.003)	0.960	0.957	0.002	(0.008)
Quality of nurses (0 to 1)	0.959	0.960	-0.001	(0.005)	0.952	0.956	-0.003	(0.007)
Clear explanation given (0=No, 1=Yes)	0.986	0.978	0.007	(0.003)**	0.979	0.985	-0.006	(0.003)*
Waiting time (minutes per visit)	55.418	59.054	-3.636	(2.416)†	55.769	58.958	-3.189	(1.871)*
Visit time (minutes per visit)	41.107	37.019	4.088	(3.857)	28.156	34.077	-5.920	(2.362)**

Note: Mean pre-program values are reported as measured in March 1998. Mean post-program values are reported as measured in October 1998 and/or March 1999. Differences are estimated using OLS regression with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of differences: † p<0.15, * p<0.10, ** p<0.05, *** p<0.01
Source: PROGRESA evaluation data

6 Health care supply and quality

An important element that was neglected until now are changes in health supply that might stem from the implementation of PROGRESA. Agurto *et al.* (2004), for example, conclude from focus groups and interviews that, amongst others, time costs, courtesy of providers, inadequacy of counseling, and poor quality material and instruments are important barriers to participation in cervical cancer screening in Mexico and other Latin American countries. Similar effects could play for other types of preventive care. Changes in offered services, quality, or prices could thus perfectly lead to the observed changes in health demand and behavior. The differential increase in preventive health behavior between treatment and control villages might be the result of improvements in health supply or quality in treatment villages relative to control villages or to a change in (time) costs to attend medical services for non-eligibles.

The survey data contain information that makes it possible to test this mechanism. Pre- and post-program information is available on the type of health care providers that are located in or around the locality, the type of services offered, the opening time, the perceived quality (sufficient staff and material, clear explanation of problem, quality of doctors and quality of nurses), and the waiting and visit time.[25] Information on health care providers and services are at the locality level, the other data are recorded at the household level. However, we average the household level information at the locality level, since only a limited number of households have provided the information and restricting our subsamples further to this group would eliminate many observations and potentially create bias. In Table 6, the pre- and post-program values are indicated for treatment and control villages as well as the differences. Table 6 shows that almost all localities have at least one health care provider (which was a program requirement). The number of services is similar in treatment and control villages and has increased slightly after PROGRESA started. Only 60% of the households agree that there is sufficient staff and equipment in the medical centres, a percentage that is stable over time. The quality of the staff is perceived as high, households are very satisfied with doctors, nurses and the explanation they are given. Waiting time is shorter in treatment villages, but does not change after program implementation. Finally, visit time in treatment villages reduces by more than 25%, while for control villages, the difference between pre- and post-program visit time is not significant. Shortening the visit time might be the chosen solution to deal with

[25] We construct the following variables: a dummy variable that indicates whether any provider (hospital, doctor, health aid or midwife) was available in the locality. A variable, ranging from 0 to 7, indicating the number of services available in the locality (pre-natal care, delivery care, baby care, immunization, family planning service, hospitalization, diarrhea care). Opening time in hours per week. An index score, ranging from 0 to 1 for the availability of staff and equipment, based on 4 yes/no questions (has medical centre sufficient doctors?, nurses?, medication?, material?). An index score, ranging from 0 to 1 for the quality of doctors, based on 4 yes/no questions (is doctor respectful?, prepared?, responsible? and trustable?). A similar quality index variable for nurses. A dummy indicating whether doctors provide clear information. Waiting time and visit time are indicated in minutes per visit. We limit the boundaries for visit time. Visit time below 5 minutes is changed to 5 minutes and visit time is capped at 1 hour.

Table 7: Social interaction estimates when controlling for changes in health care supply and quality (ζ)

	1. Baseline	**2. Health supply**	**3. Health supply and quality**	**4. Health supply, quality and time**
Deworming drugs usage	0.368*	0.426**	0.511***	0.489**
(Standard error)	(0.218)	(0.200)	(0.183)	(0.192)
Cervical cancer screening	0.247*	0.267**	0.273**	0.268*
(Standard error)	(0.131)	(0.129)	(0.134)	(0.142)
Blood sugar test	0.145	0.162	0.255**	0.263**
(Standard error)	(0.154)	(0.150)	(0.124)	(0.126)
Blood pressure test	0.288**	0.328***	0.372***	0.375***
(Standard error)	(0.127)	(0.119)	(0.113)	(0.116)
Monitoring (yearly)	0.637***	0.662***	0.640***	0.713***
(Standard error)	(0.214)	(0.197)	(0.208)	(0.191)
Monitoring (PROGRESA)	0.530**	0.508**	0.519**	0.550***
(Standard error)	(0.227)	(0.229)	(0.228)	(0.207)

Note: Social interaction estimates from IV regressions are reported with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of coefficients: † $p<0.15$, * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Individual and household controls are those listed in the Tables with descriptive evidence, i.e. Tables 10 to 16.
For cervical cancer screening, the regression controls for the number of females aged 16 or more in the household.
Source: PROGRESA evaluation data

the influx of eligibles in the health care system of treatment villages.

In Table 7, the regression results are reported when controls for health supply, quality and waiting and visit time are subsequently added. Column 1 reproduces the baseline results, for all prevention types except vaccination where social interaction effects are non-existent. Controlling for health provision characteristics, the social interaction estimates are in line with or higher than the baseline results.[26] If anything, our results get more significant and convincing.

7 Robustness and alternative hypotheses

In order to support the validity of the results shown in Tables 4 and 5, it is important to discuss alternative channels that might have generated the observed changes in preventive health behavior of non-eligible households in treatment villages. In a series of robustness tests, we will address four mechanisms that provide alternative explanations for the social interaction effects that we have identified. The first column reproduces the baseline estimates of Table 4.

[26] Especially the effects of doctors who provide a clear explanation and doctor quality play an important role. However, none of the new point estimates is significantly different from the baseline estimates.

Table 8: Robustness results of social interaction estimates (ζ)

	1. Baseline	**2. Gifts**	**3. Geographic controls**	**4. Environment shocks**	**5. Anticipation effects**	**6. Specific information**	**7. Combination (2, 3, 4, 6, health supply and quality)**
Deworming drugs usage	0.368*	0.333†	0.353†	0.375*	0.399*	0.382*	0.486**
Standard error	(0.218)	(0.224)	(0.237)	(0.219)	(0.224)	(0.220)	(0.199)
Cervical cancer screening	0.247*	0.223*	0.168	0.262**	0.228*	0.256*	0.205
Standard error	(0.131)	(0.135)	(0.158)	(0.132)	(0.132)	(0.140)	(0.176)
Blood sugar test	0.145	0.125	0.073	0.179	0.190	–	0.169
Standard error	(0.154)	(0.156)	(0.168)	(0.146)	(0.161)	–	(0.142)
Blood pressure test	0.288***	0.288**	0.255*	0.328***	0.227†	–	0.357***
Standard error	(0.127)	(0.126)	(0.137)	(0.121)	(0.149)	–	(0.121)
Monitoring (yearly)	0.637***	0.640***	0.627***	0.633***	0.645**	–	0.676***
Standard error	(0.214)	(0.215)	(0.210)	(0.207)	(0.265)	–	(0.201)
Monitoring (PROGRESA)	0.530**	0.511**	0.541**	0.583***	0.375	–	0.542**
Standard error	(0.227)	(0.233)	(0.220)	(0.216)	(0.295)	–	(0.215)

Note: Social interaction estimates from IV regressions are reported with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of coefficients: † $p<0.15$, * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Individual and household controls are those listed in the Tables with descriptive evidence, i.e. Tables 10 to 16. For cervical cancer screening, the regression controls for the number of females aged 16 or more in the household.

Source: PROGRESA evaluation data

First, it might be that eligible households share the PROGRESA benefits they receive with non-eligible households in their locality and that part of the shared resources are used to increase medical consumption. Adato (2000) concludes from focus group research that sharing of benefits is rare, since benefits are perceived as small and used primarily by eligible households to finance schooling costs (Lalive & Cattaneo, 2009), increase food consumption and food quality, and buy clothing (Bobonis, 2004; Hoddinott & Skoufias, 2004). The increase in expenditures by eligible households might indirectly benefit non-eligible households if the additional expenditures are realized in shops owned by non-eligibles. This is, however, not the case since only 20 out of the 506 villages have a local supermarket or street market (Lalive & Cattaneo, 2009). Nonetheless, Angelucci & De Giorgi (2009) present evidence that non-eligible households in treatment villages have received more gifts and loans since PROGRESA was rolled out. The additional resources are used to increase food consumption levels but are not sufficient to cover the increase in food expenditures. It appears that little additional money is available for increased medical consumption. Moreover, it seems that medical expenses are not prioritized. In the March 1998 baseline survey, it is asked what the top priority would be to spend additional monthly household resources. Medication was among the possible answers, and was only prioritized by 2% of the households (see Table 9). Food is pre-eminently given the highest priority. Other researchers have found no clear evidence of consumption or income externalities that might provide an alternative explanation for social spillovers (e.g. Bobonis & Finan, 2009; Lalive & Cattaneo, 2009). Given the limited evidence of income spillovers in treatment villages and the fact that increases in financial resources are primarily used to finance food consumption, we argue that this channel provides no good alternative explanation for the results shown in Tables 4 and 5. However, since the PROGRESA survey from October 1998 contains information on received gifts, we include the available information as a robustness check for income spillovers. In column 2 in Table 8, the social interaction effects are estimated while controling for the amount of monetary gifts a household has received and dummies for receiving food and clothes through in kind gifts. As expected, the results indicate that the inclusion of these controls has no effect on the social interaction estimates.

Table 9: First priority in spending additional monthly household resources (% of households)

	All households	Non-eligible households
Food consumption	77.0%	74.4%
Debt payment and saving	6.6%	7.8%
Housing expenses	5.4%	6.1%
Clothing and shoes	4.9%	4.7%
Investments in agriculture (seeds, animals, tools)	3.2%	3.9%
Medication	1.6%	1.9%
School supplies	1.0%	0.9%
Other expenditures (alcohol, toys, entertainment)	0.2%	0.3%

Source: PROGRESA evaluation data

Second, it might be the case that institutional, local or environmental factors have played an important role in the increase in preventive care behavior among the non-eligibles in treatment villages. In column 3, state dummies are added. Our difference in difference approach already captured time invariant geographic effects. Including dummies in the regressions controls for changes at the state level that occurred at the same time as the implementation of PROGRESA. The introduction of geographic variation has little effect on the social interaction estimates. Estimates for cervical cancer screening lose their significance. The new point estimate is, however, not significantly different from the baseline estimate. In column 4, we add dummies for natural disasters (drought, flood, earthquake, frost, pest and a residual category) that occurred between April 1998 and March 1999. The information is household specific, but we also add peer group averages. The results are in line with the baseline.

A third mechanism is that non-eligible households misunderstood their eligibility status or anticipated future eligibility and changed their preventive health behavior. This is unlikely to be the case, since households were notified clearly about their eligibility status. Moreover, eligibility was awarded until at least November 1999 and during this period non-eligible or new households were not able to attain eligibility status, irrespective of income or behavior (Angelucci & De Giorgi, 2009). Nevertheless, we test this possibility by removing the 25% non-eligible households that are closest to the poverty cut-off point. The idea is that these households are most likely to be in doubt with their eligibility status or influenced by anticipation effects. If anticipation effects would drive our results, the estimates in column 5 would show a reduced social interaction coefficient. The results correspond to the baseline, except for growth monitoring at PROGRESA frequency, where social interactions get less important and lose significance. The coefficient of the latter is also less precisely estimated. If the baseline standard errors would apply on the new point estimate, it would still be significant at the 10% level.

A final possibility is that disease-specific elements explain the observed effects. For cervical cancer, it might be that some individuals have better information on risk factors than others or engage in more risky sexual behavior, which might explain participation differences. In the pre-implementation data, information on contraceptive use and knowledge is present. In the cervical cancer screening regression, we add dummies for having ever used contraception, not being familiar with contraception, and having a partner or other relative that is against contraception. Results are shown in column 6. Infections by parasitic diseases are linked to public hygiene and sanitation (Meredith *et al.*, 2013). Households living in localities with sewer systems and public water networks may feel less inclined to use deworming drugs, because parasitic infections are less common. If sewer systems or water networks are more common in control villages, this can explain lower usage rates. In the regression for deworming drugs usage, we add dummies for a public sewer system and a public water network in the locality. We do not expect to see any effects if the randomization of villages was successful, which is confirmed by the results in column 6. Moreover the regression results show that households living in localities with a sewer system use fewer deworming drugs and women whose partners are opposed to contraception use are more inclined to screen for cancer. Although it is not entirely clear what explains the latter effect, it might be the case that these women are aware of the risk factors for HPV infection, i.e. more skin-to-skin contact through sexual intercourse, earlier pregnancies (women who were younger than 17 when having their first full-term pregnancy are more likely to develop cervical cancer), and more pregnancies (women who have more than 3 full-term pregancies have increased risk for developing cervical cancer).

In the last column, different channels are combined. We control at the same time for income spillovers, geographic effects, environmental disasters, disease specific information and health supply, quality and time effects. The combined robustness results are again in line with the baseline results.

8 Conclusion

Individual participation in preventive health care may depend on preventive health behavior in the peer group of the individual. This paper analyzes the importance of social interactions in the context of new social policies in Mexico that aim to increase health care usage among a targeted subgroup of the population. We followed the promising approach of analyzing social interactions in real world peer groups. We exploited the partial-population design with random variation in eligibility status of households and in treatment status of localities in PROGRESA for the identification of social interactions.

Results indicate that PROGRESA was successful in increasing preventive care usage among the eligible households. Non-eligible households in treatment villages have also changed their preventive health behavior more than their counterparts in control villages, providing evidence of spillover effects. We were able to isolate endogenous social interactions from contextual and correlated effects - under weak assumptions - and showed that social interaction effects are present for deworming drugs usage, cervical cancer screening, blood pressure tests and child growth and weight monitoring. No social interactions are found for immunization of children and for blood sugar tests. The results are robust to the inclusion of health supply, quality and waiting time controls as well as income spillover through gifts, geographic variation, environmental shocks, anticipation effects and specific disease related information. The magnitude of the social interaction effects differs between the different types of prevention.

Using the information on social interactions, the total treatment effect can be decomposed in a direct effect, related to the financial incentive given to eligible households for complying with PROGRESA requirements, and an indirect effect related to informational and pure conformity effects. The total treatment effect indicates that participation in prevention among eligibles increased as much as 20 percentage points for blood sugar and blood pressure tests, around 15 percentage points for cervical cancer screening and growth monitoring at PROGRESA frequency, 11 percentage points for deworming drugs usage and 7 percentage points for annual monitoring. The latter started with a pre-program participation rate well above 80%. The indirect effect due to social interactions accounts for 10% up to 58% of the total treatment effect for the eligibles, i.e. a non-negligible share.

Evidence of the presence and magnitude of social interactions is important, regardless of the underlying mechanisms that cause them. It is important for policymaking since social interactions could reinforce or offset the direct incentives given by a social program that aims to influence the individual participation decision. Important (positive) social interaction effects might also support a high adoption equilibrium after direct financial incentives are cut back or removed. In our case, positive, reinforcing effects are found, that amplify participation both of eligibles and non-eligibles. Thus, by targeting the extreme poor, PROGRESA has succeeded to improve not only their health care usage, but also that of their non-eligible neighbors. As Barham (2005), Gertler (2000, 2004) and Skoufias (2005) have shown, this is translated in health improvements for children and adults in PROGRESA localities and is a potential gamechanger in the human capital accumulation of these children and households.

9 References

Adato, M. (2000). *The impact of PROGRESA on community social relationships.* Final Report PROGRESA, IFPRI, pp. 36.

Adato, M., B. de la Brière, D. Mindek, and A. Quisumbing (2000). *The impact of PROGRESA on women's status and intrahousehold relations.* Final Report PROGRESA, IFPRI, pp. 94.

Agurto, I., A. Bishop, G. Sanchez, Z. Betancourt, and S. Robles. (2004). Perceived barriers and benefits to cervical cancer screening in Latin America. *Preventive Medicine*, 39, nr. 1, pp. 91-98.

Aizer, A., and J. Currie (2004). Networks or neighborhoods? Correlations in the use of publicly-funded maternity care in California. *Journal of Public Economics*, 88, nr. 12, 2573-2585.

Akerlof, G. (1997). Social distance and Social Decisions. *Econometrica*, 65, nr. 5, pp. 1005-1027.

Angelucci, M., and G. De Giorgi (2009). Indirect Effects of an Aid Program: How Do Cash Transfers Affect Ineligibles' Consumption?. *American Economic Review*, 99, nr. 1, pp. 486-508.

Angelucci, M., G. De Giorgi, M. A. Rangel, and I. Rasul (2010). Family networks and school enrolment: Evidence from a randomized social experiment. *Journal of Public Economics*, 94, nr. 3, pp. 197-221.

Armendáriz de Aghion, B. and J. Morduch (2005). *The economics of microfinance.* The MIT Press, Cambridge.

Avitabile, C. (2011) *Spillover effects in healthcare programs: Evidence on social norms and information sharing.* CSEF working paper nr. 271, pp. 55.

Baird, S., A. Bohren, C. McIntosh, and B. Özler (2012). *Designing experiments to measure spillover and threshold effects*, mimeo, pp. 61.

Barham, T. (2005). *The impact of the Mexican conditional cash transfer on immunization rates.* Mimeo, Department of Agriculture and Resource Economics, University of California at Berkeley.

Barham, T. (2011). A healthier start: the effect of conditional cash transfers on neonatal and infant mortality in rural Mexico. *Journal of Development Economics*, 94, nr. 1, pp. 74-85.

Barzallo, D.P. (2011). *Health conditions and social interactions.* IRENE working paper 11-03, pp. 33.

Behrman, J.R., and P. Todd (1999). *Randomness in the Experimental Samples of PROGRESA.* Mimeo, IFPRI, pp. 36.

Behrman, J.R. (1996). The Impact of Health and Nutrition on Education. *World Rank Research Observer*, 11, nr. 1, pp. 23-37.

Bobba M. and J. Gignoux (2012). *Policy-induced social interactions.* Discussion papers, pp. 30.

Bobonis, G. J. (2004). *Income transfers, marital dissolution, and intra-household resource allocation: Evidence from rural Mexico.* University of California at Berkeley working paper, pp. 52.

Bobonis, G.J. and F. Finan (2009). Neighborhood peer effects in secondary school enrolment decisions. *The Review of Economics and Statistics*, 91, nr. 4, pp. 695-716.

Blume, L.E., W.A. Brock, S.N. Durlauf, and Y.M. Ioannides (2010). Identification of Social Interactions (chapter 18, pp. 853-964). In: Benhabib, J., A. Bisin, and M. Jackson. *Handbook of Social Economics.* North-Holland.

Buddelmeyer, H. and E. Skoufias (2004). *An Evaluation of the Performance of Regression Discontinuity Design on PROGRESA.* World Bank Policy Research Working Paper 3386.

Carrell, S. E., M. Hoekstra, and J. E. West (2011). Is poor fitness contagious?: Evidence from randomly assigned friends. *Journal of Public Economics*, 95, nr. 7, pp. 657-663.

Case, A., A. Fertig, and C. Paxson (2005). The Lasting Impact of Childhood Health and Circumstance. *Journal of Health Economics*, 24, nr. 2, pp. 365-389.

Case, A., D. Lubotsky, and C. Paxson (2002). Economic Status and Health in Childhood: The Origins of the Gradient. *American Economic Review*, 92, nr. 5, pp. 1308-1334.

Case, A., and C. Paxson (2006). Stature and Status: Height, Ability, and Labor Market Out comes. NBER working paper, nr. w12466.

Currie, J. (2009). Healthy, Wealthy, and Wise: Socioeconomic Status, Poor Health in Childhood, and Human Capital Development. *Journal of Economic Literature*, 47, nr. 1, pp. 87-122.

Currie, J. and B. C. Madrian (1999). Health, Health Insurance and the Labor Market. (pp. 3309-3416). In: Ashenfelter, O. and D. Card. *Hand book of Labor Economics, Volume 3C.* Amsterdam; New York and Oxford: Elsevier Science, North-Holland.

Cutler, D., A. Deaton, and A. Lleras-Muney (2006). The Determinants of Mortality. *Journal of Economic Perspectives*, 20, nr. 3, pp 97-120.

Dahl, G. B., K.V. Løken, and M. Mogstad (2012). *Peer effects in program participation.* NBER working paper, nr. w18198.

Deri, C. (2005). Social networks and health service utilization. J*ournal of Health Economics* 24, nr. 6, pp. 1076-1107.

Gertler, P. (2000). *The impact of PROGRESA on health.* Final Report PROGRESA, IFPRI, pp. 31.

Gertler, P. (2004). Do Conditional Cash Transfers Improve Child Health? Evidence from PROGRESA's Control Randomized Experiment. *American Economic Review*, 94, nr. 2, pp. 336-341.

Godlonton, S., and R. Thornton (2012). Peer effects in learning HIV results. *Journal of development* *economics*, 97, nr. 1, pp. 118-129.

Hoddinott, J., and E. Skoufias (2004). The Impact of PROGRESA on Food Consumption. *Economic development and cultural change*, 53, nr. 1, pp. 37-61.

INSP (2005). *General Rural Methodology Note.* Instituto Nacional de Salud Pública (INSP), Sedesol.

Kremer, M., and E. Miguel (2007). The illusion of sustainability. *The Quarterly Journal of Economics*, 122, nr. 3, pp. 1007-1065.

Lagarde, M., A. Haines, and N. Palmer. (2007). Conditional cash transfers for improving uptake of health interventions in low-and middle-income countries. J*AMA: the journal of the American Medical Association*, 298, nr. 16, pp. 1900-1910.

Lalive, R. and A. Cattaneo (2009) Social interactions and schooling decisions. *The Review of Economics and Statistics*, 91, nr. 3, pp. 457-477.

Lewis M. J. (2004). *A Situational Analysis of Cervical Cancer in Latin America and the Caribbean.* PAHO, Washington, DC , pp. 29.

Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *The review of economic studies*, 60, nr. 3, pp. 531-542.

Meredith, J. M., J. Robinson, S. Walker, and B. Wydick (2013). *Keeping the doctor away: experimental evidence on investment in preventative health products.* NBER working paper, nr. w19312.

Moffitt, R. A. (2001). Policy Interventions, Low Level Equilibria, and Social Interactions (chapter 3, pp. 45–82). In Durlauf, S.N. and H. P. Young. *Social Dynamics.* The MIT Press.

Munshi, K., & J. Myaux (2006). Social norms and the fertility transition. *Journal of Development Economics*, 80, nr. 1, pp. 1-38.

Noguera, J. A., F.J. Miguel, E. Tapia, and T. Llàcer (2013). *Tax compliance, rational choice and social influence: An agent based model.* Mimeo, pp. 33.

Oster, E., and R. Thornton (2012). Determinant of technology adoption: Peer effects in menstrual cup take-up. *Journal of the European Economic Association*, 10, nr. 6, pp. 1263-1293.

Patacchini, E. and Y. Zenou (2009). Juvenile Delinquency and Conformism. *The Journal of Law, Economics, & Organization*, 28, nr. 1, pp. 1-31.

Ranganathan, M., and M. Lagarde. (2012). Promoting healthy behaviours and improving health outcomes in low and middle income countries: A review of the impact of conditional cash transfer programmes. *Preventive Medicine*, 55, pp. S95-S105.

Rao, N., M. M. Mobius, and T. Rosenblat (2007). *Social networks and vaccination decisions.* Working paper 07-12, Federal Reserve Bank, Boston.

Sánchez-Castillo, C. P., O. Velásquez-Monroy, A. Lara-Esqueda, A. Berber, J. Sepúlveda, R. Tapia-Conyer, and W. P. T. James. (2005). Diabetes and hypertension increases in a society with abdominal obesity: results of the Mexican National Health Survey 2000. *Public health nutrition*, 8, nr. 1, pp. 53-60.

Sepúlveda, J., F. Bustreo, R. Tapia, J. Rivera, R. Lozano, G. Oláiz, V. Partida, L. Garciá-Garciá, and J. L.Valdespino. (2006). Improvement of child survival in Mexico: the diagonal approach. *The Lancet*, 368, pp. 2017-2027.

Skoufias, E. (2005). PROGRESA and its impacts on the welfare of rural households in Mexico. Research Report PROGRESA, nr. 139, IFPRI, pp. 84.

Stock, J. H., and M. Yogo (2005). Testing for Weak Instruments in Linear IV Regression. (pp. 109–120). In: Andrews, D., and J. H. Stock. *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg* Cambridge University Press.

Young, H. P. (2009). Innovation diffusion in heterogeneous populations: Contagion, social influence, and social learning. *American economic review*, 99, nr. 5, pp. 1899-1924.

Van den Berg, G. J., M. Lindeboom, and F. Portrait (2006). Economic Conditions Early in Life and Individual Mortality. *American Economic Review*, 96, nr. 1, pp. 290-302.

Watkins, M. M., C. Gabali, M. Winkleby, E. Gaona, and S. Lebaron. (2002). Barriers to cervical cancer screening in rural Mexico. *International journal of Gynecological cancer*, 12, nr. 5, pp. 475-479.

World Bank (2004). *Poverty in Mexico: An Assessment of Conditions, Trends and Government Strategy*. Report No. 28612-ME.

World Bank (2005). *Mexico - Income Generation and Social Protection for the Poor: Volume 4. A Study of Rural Poverty in Mexico*. Report No. 32867-MX.

10 Appendix 1: Descriptive statistics

Table 10: Descriptive statistics for the entire sample

	Eligible				Ineligible			
	Program	Control	Difference	(SD)	Program	Control	Difference	(SD)
HH: age	42.240	42.606	–0.366	(0.421)	51.504	51.661	–0.157	(0.563)
HH: literate	0.665	0.664	0.001	(0.023)	0.681	0.721	–0.040	(0.017)**
HH: female	0.083	0.086	–0.003	(0.006)	0.140	0.139	0.000	(0.009)
HH: speaks only indigenous language	0.050	0.058	–0.008	(0.014)	0.026	0.024	0.002	(0.008)
HH: speaks spanish and indigenous	0.373	0.380	–0.007	(0.049)	0.239	0.210	0.030	(0.041)
HH: degree in primary school	0.625	0.622	0.002	(0.021)	0.578	0.623	–0.450	(0.019)**
HH: degree in secondary school or beyond	0.053	0.050	0.003	(0.007)	0.089	0.083	0.006	(0.009)
HH: married	0.659	0.619	0.040	(0.023)*	0.620	0.621	–0.001	(0.020)
HH: partner but not married	0.215	0.259	–0.043	(0.021)**	0.146	0.160	–0.014	(0.016)
HH: seperated or alone	0.021	0.019	0.002	(0.003)	0.036	0.033	0.003	(0.004)
HH: widow	0.079	0.084	–0.005	(0.006)	0.141	0.132	0.009	(0.009)
P: age	31.293	31.423	–0.130	(0.421)	33.739	34.856	–1.117	(0.646)*
P: literate	0.516	0.507	0.009	(0.025)	0.472	0.498	–0.026	(0.017)
P: speaks only indigenous language	0.081	0.111	–0.030	(0.025)	0.037	0.039	–0.002	(0.011)
P: speaks spanish and indigenous	0.278	0.256	0.022	(0.037)	0.157	0.132	0.025	(0.029)
P: degree in primary school	0.507	0.497	0.011	(0.023)	0.414	0.452	–0.038	(0.016)**
P: degree in secundary school or beyond	0.033	0.043	–0.011	(0.005)**	0.068	0.062	0.006	(0.007)
household size	3.599	3.616	–0.017	(0.031)	2.904	2.965	–0.062	(0.046)
wealth: roof of tin	0.276	0.249	0.027	(0.028)	0.292	0.307	–0.015	(0.032)
wealth: roof of cement	0.074	0.082	–0.008	(0.013)	0.214	0.211	0.003	(0.024)
wealth: roof of tiles	0.115	0.084	0.031	(0.023)	0.127	0.098	0.029	(0.022)
wealth: floor of cement	0.253	0.220	0.033	(0.024)	0.547	0.555	–0.008	(0.029)
wealth: owner of agricultural land or animals	0.603	0.569	0.034	(0.029)	0.673	0.658	0.017	(0.022)
wealth: car owner	0.005	0.004	0.002	(0.002)	0.039	0.047	–0.008	(0.007)
wealth: marginality index	639.346	638.767	0.579	(4.672)	841.460	847.290	–5.829	(8.131)

Note: HH refers to household head; P refers to partner. Mean values are reported of characteristics as measured in October 1997. Differences are estimated using OLS regression with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of differences: * p<0.10, ** p<0.05, *** p<0.01

Source: PROGRESA evaluation data

Table 11: Descriptive statistics for the sample of deworming drugs usage

	Eligible				Ineligible			
	Program	Control	Difference	(SD)	Program	Control	Difference	(SD)
HH: age	40.651	40.919	-0.268	(0.416)	49.965	49.979	-0.014	(0.619)
HH: literate	0.697	0.698	-0.001	(0.022)	0.705	0.741	-0.037	(0.018)**
HH: female	0.070	0.073	-0.002	(0.006)	0.129	0.133	-0.003	(0.009)
HH: speaks only indigenouse language	0.042	0.046	-0.004	(0.013)	0.023	0.021	0.002	(0.008)
HH: speaks spanish and indigenous	0.369	0.379	-0.011	(0.050)	0.232	0.204	0.028	(0.040)
HH: degree in primary school	0.655	0.652	0.003	(0.020)	0.595	0.643	-0.048	(0.019)**
HH: degree in secundary school or beyond	0.056	0.054	0.002	(0.007)	0.098	0.089	0.009	(0.010)
HH: married	0.681	0.637	0.045	(0.024)*	0.644	0.650	-0.006	(0.020)
HH: partner but not married	0.223	0.269	-0.046	(0.022)**	0.159	0.169	-0.010	(0.017)
HH: seperated or alone	0.018	0.017	0.002	(0.003)	0.031	0.030	0.001	(0.004)
HH: widow	0.056	0.063	-0.007	(0.005)	0.119	0.109	0.010	(0.009)
P: age	31.369	31.479	-0.110	(0.422)	34.166	35.336	-1.169	(0.660)*
P: literate	0.559	0.551	0.007	(0.026)	0.518	0.549	-0.031	(0.019)
P: speaks only indigenous language	0.074	0.103	-0.029	(0.024)	0.035	0.034	0.001	(0.011)
P: speaks spanish and indigenous	0.289	0.267	0.022	(0.039)	0.160	0.138	0.022	(0.030)
P: degree in primary school	0.548	0.537	0.011	(0.023)	0.451	0.496	-0.045	(0.017)**
P: degree in secundary school or beyond	0.036	0.048	-0.012	(0.006)**	0.078	0.069	0.009	(0.007)
household size	3.614	3.622	-0.008	(0.031)	2.876	2.962	-0.086	(0.043)**
wealth: roof of tin	0.280	0.250	0.030	(0.028)	0.298	0.315	-0.017	(0.032)
wealth: roof of cement	0.075	0.088	-0.012	(0.014)	0.211	0.212	-0.001	(0.024)
wealth: roof of tiles	0.114	0.076	0.038	(0.022)*	0.121	0.089	0.032	(0.021)
wealth: floor of cement	0.262	0.226	0.036	(0.025)	0.540	0.557	-0.017	(0.030)
wealth: owner of agricultural land or animals	0.597	0.562	0.035	(0.029)	0.663	0.652	0.011	(0.024)
wealth: car owner	0.006	0.003	0.003	(0.002)	0.039	0.047	-0.009	(0.007)
wealth: marginality index	637.200	636.743	0.458	(4.811)	838.519	845.621	-7.101	(8.175)

Note: AW refers to answering woman. The values can best be compared to the values of partner of the entire sample. Mean values are reported of characteristics as measured in October 1997. Differences are estimated using OLS regression with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of differences: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Source: PROGRESA evaluation data

Table 12: Descriptive statistics for the sample of cervical cancer screening

	Eligible				Ineligible			
	Program	Control	Difference	(SD)	Program	Control	Difference	(SD)
HH: age	40.414	40.715	-0.301	(0.417)	49.539	49.620	-0.081	(0.620)
HH: literate	0.701	0.703	-0.002	(0.022)	0.712	0.746	-0.034	(0.018)*
HH: female	0.071	0.072	-0.001	(0.006)	0.135	0.138	-0.003	(0.010)
HH: speaks only indigenouse language	0.043	0.044	-0.001	(0.013)	0.023	0.022	0.001	(0.008)
HH: speaks spanish and indigenous	0.369	0.379	-0.009	(0.050)	0.236	0.201	0.034	(0.040)
HH: degree in primary school	0.660	0.657	0.003	(0.020)	0.600	0.648	-0.048	(0.019)**
HH: degree in secundary school or beyond	0.056	0.054	0.001	(0.007)	0.101	0.089	0.012	(0.010)
HH: married	0.688	0.645	0.043	(0.024)*	0.669	0.676	-0.007	(0.020)
HH: partner but not married	0.225	0.271	-0.046	(0.022)**	0.164	0.168	-0.004	(0.017)
HH: seperated or alone	0.017	0.014	0.003	(0.003)	0.024	0.026	-0.002	(0.004)
HH: widow	0.051	0.058	-0.007	(0.005)	0.107	0.101	0.006	(0.008)
P: age	31.727	31.816	-0.089	(0.417)	35.427	36.301	-0.874	(0.646)
P: literate	0.564	0.561	0.003	(0.026)	0.542	0.566	-0.025	(0.019)
P: speaks only indigenous language	0.075	0.101	-0.026	(0.024)	0.035	0.035	0.000	(0.011)
P: speaks spanish and indigenous	0.293	0.271	0.021	(0.040)	0.167	0.141	0.026	(0.031)
P: degree in primary school	0.555	0.548	0.007	(0.023)	0.471	0.510	-0.039	(0.018)**
P: degree in secundary school or beyond	0.036	0.048	-0.012	(0.006)**	0.082	0.073	0.008	(0.008)
household size	3.644	3.643	0.001	(0.030)	2.945	3.006	-0.061	(0.042)
wealth: roof of tin	0.281	0.252	0.029	(0.028)	0.293	0.312	-0.019	(0.031)
wealth: roof of cement	0.075	0.088	-0.013	(0.015)	0.223	0.214	0.009	(0.025)
wealth: roof of tiles	0.115	0.074	0.041	(0.022)*	0.120	0.090	0.030	(0.021)
wealth: floor of cement	0.264	0.225	0.039	(0.025)	0.554	0.562	-0.008	(0.030)
wealth: owner of agricultural land or animals	0.599	0.566	0.032	(0.029)	0.660	0.649	0.011	(0.024)
wealth: car owner	0.006	0.003	0.002	(0.002)	0.040	0.050	-0.010	(0.008)
wealth: marginality index	636.561	636.764	-0.203	(4.899)	838.957	845.719	-6.762	(8.234)

Note: HH refers to household head; P refers to partner. Mean values are reported of characteristics as measured in October 1997. Differences are estimated using OLS regression with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of differences: * p<0.10, ** p<0.05, *** p<0.01

Source: PROGRESA evaluation data

Table 13: Descriptive statistics for the sample of blood sugar test

	Eligible				Ineligible			
	Program	Control	Difference	(SD)	Program	Control	Difference	(SD)
HH: age	40.803	41.190	-0.387	(0.430)	50.410	50.460	-0.050	(0.621)
HH: literate	0.697	0.696	0.000	(0.022)	0.700	0.736	-0.036	(0.018)**
HH: female	0.070	0.072	-0.003	(0.006)	0.133	0.135	-0.002	(0.010)
HH: speaks only indigenouse language	0.043	0.047	-0.004	(0.013)	0.023	0.022	0.001	(0.008)
HH: speaks spanish and indigenous	0.372	0.383	-0.010	(0.050)	0.231	0.204	0.028	(0.040)
HH: degree in primary school	0.656	0.652	0.004	(0.020)	0.595	0.643	-0.048	(0.019)**
HH: degree in secundary school or beyond	0.055	0.052	0.002	(0.007)	0.093	0.084	0.009	(0.010)
HH: married	0.680	0.637	0.044	(0.024)*	0.642	0.652	-0.011	(0.020)
HH: partner but not married	0.224	0.270	-0.046	(0.022)**	0.156	0.166	-0.009	(0.017)
HH: seperated or alone	0.018	0.015	0.003	(0.003)	0.031	0.030	0.001	(0.004)
HH: widow	0.056	0.064	-0.008	(0.006)	0.122	0.111	0.010	(0.009)
P: age	31.516	31.704	-0.188	(0.433)	34.270	35.605	-1.335	(0.665)**
P: literate	0.557	0.550	0.007	(0.026)	0.511	0.543	-0.032	(0.019)*
P: speaks only indigenous language	0.074	0.104	-0.030	(0.024)	0.035	0.035	0.000	(0.011)
P: speaks spanish and indigenous	0.292	0.272	0.020	(0.040)	0.159	0.139	0.020	(0.030)
P: degree in primary school	0.548	0.538	0.010	(0.023)	0.447	0.491	-0.044	(0.018)**
P: degree in secundary school or beyond	0.035	0.046	-0.011	(0.006)*	0.074	0.067	0.007	(0.007)
household size	3.618	3.617	0.002	(0.031)	2.858	2.952	-0.095	(0.044)**
wealth: roof of tin	0.279	0.250	0.029	(0.028)	0.298	0.316	-0.019	(0.031)
wealth: roof of cement	0.074	0.086	-0.013	(0.014)	0.207	0.210	-0.002	(0.024)
wealth: roof of tiles	0.115	0.075	0.040	(0.022)*	0.124	0.086	0.037	(0.021)*
wealth: floor of cement	0.260	0.224	0.036	(0.025)	0.539	0.555	-0.015	(0.030)
wealth: owner of agricultural land or animals	0.600	0.570	0.031	(0.029)	0.665	0.655	0.010	(0.024)
wealth: car owner	0.006	0.003	0.002	(0.002)	0.039	0.048	-0.009	(0.007)
wealth: marginality index	637.199	637.484	-0.285	(4.856)	838.386	846.478	-8.092	(8.247)

Note: HH refers to household head; P refers to partner. Mean values are reported of characteristics as measured in October 1997. Differences are estimated using OLS regression with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of differences: * p<0.10, ** p<0.05, *** p<0.01

Source: PROGRESA evaluation data

Table 14: Descriptive statistics for the sample of blood pressure test

	Eligible				Ineligible			
	Program	Control	Difference	(SD)	Program	Control	Difference	(SD)
HH: age	40.794	41.157	-0.363	(0.428)	50.383	50.328	0.055	(0.616)
HH: literate	0.697	0.698	-0.001	(0.022)	0.700	0.739	-0.039	(0.018)**
HH: female	0.071	0.072	-0.001	(0.006)	0.133	0.136	-0.002	(0.010)
HH: speaks only indigenouse language	0.042	0.046	-0.003	(0.012)	0.024	0.021	0.003	(0.008)
HH: speaks spanish and indigenous	0.371	0.382	-0.011	(0.050)	0.232	0.203	0.029	(0.040)
HH: degree in primary school	0.656	0.654	0.002	(0.020)	0.594	0.644	-0.050	(0.019)***
HH: degree in secundary school or beyond	0.055	0.052	0.003	(0.007)	0.094	0.086	0.009	(0.010)
HH: married	0.681	0.637	0.044	(0.024)*	0.640	0.652	-0.012	(0.020)
HH: partner but not married	0.223	0.270	-0.047	(0.022)**	0.157	0.164	-0.008	(0.016)
HH: seperated or alone	0.019	0.016	0.003	(0.003)	0.032	0.030	0.001	(0.004)
HH: widow	0.056	0.064	-0.007	(0.006)	0.122	0.111	0.011	(0.009)
P: age	31.455	31.657	-0.202	(0.434)	34.200	35.479	-1.279	(0.668)*
P: literate	0.556	0.551	0.005	(0.025)	0.510	0.543	-0.033	(0.019)*
P: speaks only indigenous language	0.074	0.103	-0.029	(0.024)	0.035	0.034	0.001	(0.011)
P: speaks spanish and indigenous	0.290	0.270	0.020	(0.040)	0.159	0.136	0.022	(0.030)
P: degree in primary school	0.547	0.539	0.007	(0.023)	0.445	0.490	-0.045	(0.017)**
P: degree in secundary school or beyond	0.035	0.045	-0.010	(0.006)*	0.076	0.068	0.008	(0.007)
household size	3.618	3.617	0.001	(0.031)	2.855	2.941	-0.086	(0.044)*
wealth: roof of tin	0.279	0.252	0.027	(0.028)	0.296	0.317	-0.020	(0.032)
wealth: roof of cement	0.075	0.086	-0.011	(0.014)	0.210	0.209	0.002	(0.024)
wealth: roof of tiles	0.115	0.075	0.039	(0.022)*	0.122	0.090	0.031	(0.021)
wealth: floor of cement	0.262	0.225	0.037	(0.025)	0.538	0.555	-0.017	(0.030)
wealth: owner of agricultural land or animals	0.600	0.570	0.030	(0.029)	0.663	0.652	0.011	(0.024)
wealth: car owner	0.006	0.003	0.003	(0.002)	0.039	0.048	-0.009	(0.007)
wealth: marginality index	637.236	637.127	0.109	(4.865)	838.681	846.336	-7.655	(8.153)

Note: HH refers to household head; P refers to partner. Mean values are reported of characteristics as measured in October 1997. Differences are estimated using OLS regression with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of differences: * p<0.10, ** p<0.05, *** p<0.01

Source: PROGRESA evaluation data

Table 15: Descriptive statistics for the sample of growth and weight monitoring of children below 5 years old

	Eligible				Ineligible			
	Program	Control	Difference	(SD)	Program	Control	Difference	(SD)
HH: age	37.028	37.574	-0.546	(0.477)	42.727	43.430	-0.703	(0.803)
HH: literate	0.750	0.735	0.015	(0.021)	0.808	0.833	-0.025	(0.019)
HH: female	0.044	0.051	-0.006	(0.006)	0.072	0.066	0.006	(0.011)
HH: speaks only indigenouse language	0.033	0.038	-0.005	(0.011)	0.018	0.022	-0.004	(0.010)
HH: speaks spanish and indigenous	0.382	0.384	-0.002	(0.054)	0.229	0.199	0.030	(0.047)
HH: degree in primary school	0.698	0.678	0.020	(0.020)	0.657	0.683	-0.026	(0.026)
HH: degree in secundary school or beyond	0.072	0.070	0.002	(0.010)	0.157	0.150	0.007	(0.018)
HH: married	0.698	0.639	0.058	(0.029)**	0.704	0.708	-0.004	(0.031)
HH: partner but not married	0.238	0.299	-0.061	(0.028)**	0.186	0.199	-0.013	(0.028)
HH: seperated or alone	0.012	0.009	0.002	(0.003)	0.018	0.017	0.000	(0.005)
HH: widow	0.039	0.046	-0.007	(0.005)	0.074	0.059	0.015	(0.012)
P: age	29.313	29.749	-0.435	(0.484)	32.203	33.988	-1.784	(0.861)**
P: literate	0.611	0.610	0.001	(0.026)	0.652	0.686	-0.034	(0.025)
P: speaks only indigenous language	0.074	0.092	-0.018	(0.023)	0.043	0.047	-0.004	(0.018)
P: speaks spanish and indigenous	0.307	0.282	0.025	(0.044)	0.169	0.143	0.027	(0.036)
P: degree in primary school	0.596	0.588	0.008	(0.024)	0.526	0.584	-0.059	(0.025)**
P: degree in secundary school or beyond	0.045	0.063	-0.018	(0.008)**	0.147	0.115	0.032	(0.015)**
household size	3.938	3.971	-0.033	(0.038)	3.585	3.750	-0.165	(0.066)**
wealth: roof of tin	0.276	0.256	0.020	(0.030)	0.277	0.317	-0.040	(0.037)
wealth: roof of cement	0.085	0.090	-0.005	(0.016)	0.290	0.286	0.004	(0.034)
wealth: roof of tiles	0.106	0.072	0.034	(0.022)	0.084	0.059	0.026	(0.019)
wealth: floor of cement	0.280	0.241	0.039	(0.027)	0.625	0.652	-0.026	(0.035)
wealth: owner of agricultural land or animals	0.577	0.542	0.035	(0.033)	0.633	0.636	-0.003	(0.033)
wealth: car owner	0.005	0.005	0.000	(0.003)	0.050	0.076	-0.026	(0.013)*
wealth: marginality index	616.705	616.172	0.533	(5.733)	827.859	834.459	-6.600	(8.612)

Note: HH refers to household head; P refers to partner. Mean values are reported of characteristics as measured in October 1997. Differences are estimated using OLS regression with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of differences: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Source: PROGRESA evaluation data

Table 16: Descriptive statistics for the sample of vaccination of children below 5 years old

	Eligible				Ineligible			
	Program	Control	Difference	(SD)	Program	Control	Difference	(SD)
HH: age	37.252	37.757	-0.505	(0.441)	43.374	43.535	-0.161	(0.806)
HH: literate	0.743	0.732	0.011	(0.022)	0.803	0.827	-0.024	(0.019)
HH: female	0.046	0.053	-0.007	(0.006)	0.073	0.067	0.006	(0.011)
HH: speaks only indigenouse language	0.032	0.039	-0.007	(0.012)	0.020	0.022	-0.001	(0.010)
HH: speaks spanish and indigenous	0.380	0.374	0.006	(0.053)	0.243	0.195	0.048	(0.048)
HH: degree in primary school	0.690	0.681	0.009	(0.021)	0.658	0.682	-0.023	(0.026)
HH: degree in secundary school or beyond	0.074	0.068	0.006	(0.010)	0.153	0.144	0.008	(0.018)
HH: married	0.692	0.653	0.039	(0.029)	0.707	0.693	0.014	(0.032)
HH: partner but not married	0.241	0.285	-0.044	(0.028)	0.185	0.210	-0.024	(0.029)
HH: seperated or alone	0.012	0.011	0.001	(0.003)	0.018	0.019	0.000	(0.005)
HH: widow	0.041	0.045	-0.004	(0.005)	0.070	0.061	0.009	(0.011)
P: age	29.460	29.956	-0.496	(0.480)	32.884	33.909	-1.025	(0.861)
P: literate	0.607	0.613	-0.006	(0.027)	0.645	0.677	-0.033	(0.026)
P: speaks only indigenous language	0.074	0.094	-0.020	(0.024)	0.040	0.042	-0.001	(0.017)
P: speaks spanish and indigenous	0.306	0.274	0.032	(0.044)	0.185	0.143	0.042	(0.037)
P: degree in primary school	0.590	0.588	0.002	(0.025)	0.529	0.570	-0.041	(0.025)
P: degree in secundary school or beyond	0.047	0.065	-0.018	(0.009)**	0.132	0.115	0.017	(0.015)
household size	3.949	3.966	-0.017	(0.037)	3.633	3.751	-0.118	(0.068)*
wealth: roof of tin	0.265	0.249	0.016	(0.030)	0.258	0.309	-0.051	(0.037)
wealth: roof of cement	0.086	0.089	-0.003	(0.016)	0.295	0.288	0.006	(0.034)
wealth: roof of tiles	0.109	0.084	0.025	(0.023)	0.085	0.063	0.022	(0.021)
wealth: floor of cement	0.279	0.241	0.037	(0.027)	0.607	0.648	-0.041	(0.034)
wealth: owner of agricultural land or animals	0.585	0.542	0.043	(0.033)	0.637	0.648	-0.011	(0.032)
wealth: car owner	0.006	0.005	0.001	(0.003)	0.052	0.074	-0.022	(0.013)*
wealth: marginality index	616.048	615.021	1.027	(5.831)	828.488	832.185	-3.697	(8.388)

Note: HH refers to household head; P refers to partner. Mean values are reported of characteristics as measured in October 1997. Differences are estimated using OLS regression with robust standard errors that allow for correlation of disturbance terms within localities. Significance levels of differences: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Source: PROGRESA evaluation data