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

Ciro Avitabile†

Abstract

Although cervical cancer is considered one of the most preventable types of cancer, mortality rates in many developing countries are extremely high. This paper exploits the randomized research design of a large welfare program - *PROGRESA* - to study the existence of spillover effects in cervical cancer screening in rural Mexico. I find significant evidence of increased demand for Papanicolaou cervical cancer screening among women ineligible for the transfer, yet no evidence of similar externalities in non-gender specific tests, such as blood pressure and blood sugar checks. Different pieces of evidence from the evaluation sample and the nationwide rollout are consistent with the hypothesis that the *PROGRESA* program has weakened the social norm related to husbands' opposition to screening of their wives by male doctors. I find less evidence to support the hypothesis that the spillover effect is driven by higher levels of health information.

Keywords: Cervical cancer, Social norm, Information sharing, *PROGRESA*

JEL Classification: D83, I12, J16

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1 Introduction

Cervical cancer is the second most common cancer in women. In 2005, it was responsible for 250,000 deaths, approximately 80% of these in developing countries (World Health Organization, 2006). In contrast to other types of cancer, cervical cancer has very well-defined risk factors, mostly related to sexual activity, and early detection can virtually eliminate the mortality risk. This paper studies how a program that provides financial incentives to conduct systematic health checks can affect the propensity to screen for cervical cancer of women who are not eligible for the program. For this purpose, I use data from the evaluation sample and the nationwide rollout of *PROGRESA* (later renamed *Oportunidades*), a Conditional Cash Transfer (CCT) program that targets poor households in rural Mexico.

The use of a safe and cheap test, the Papanicolaou (PAP) smear test, has led to a huge drop in cervical cancer mortality in developed countries.[1] However, this is not the case in developing countries, Mexico being one of the most striking examples. The country has displayed for many years one of the highest cervical cancer mortality rates in the world (World Health Organization, 2008). Despite the existence of a national cervical cancer screening program (CCSP) since 1974, the percentage of Mexican women who regularly get screened for cervical cancer is well below the OECD country average, and lack of compliance with cervical cancer screening advice is dramatically high in rural areas (Lazcano-Ponce, 1997; Watkins et al., 2002).

Compliance with health screening services is one of the requirements that the beneficiaries of *PROGRESA* have to satisfy in order to receive the transfer.[2] Started in 1997 and still ongoing, *PROGRESA* is the ideal context to study the presence of spillover effects in the demand for medical screening among individuals who are not eligible for the transfer. The evaluation of the program is based on a village-level randomized design. From a group of 506 villages, 320 were randomly assigned to be in the treatment group for the *PROGRESA* program starting in May 1998, and 186 were assigned to a control group for the program phase starting in November 1999. Data are available for all households in every village, both poor and non-poor, although only poor households are *eligible* for the transfer.[3] Only the adult members of eligible households have to undergo full preventive screenings: while both male and female household members have their blood pressure and blood sugar levels tested, the PAP smear test is female specific.

There is limited evidence on the existence and magnitude of spillover effects across indi-

[1] For instance, between 1950 and 2000 in the US there was a 79% reduction in the incidence of cervical cancer and a 75% decrease in mortality.

[2] Most CCT programs require the attendance of health checks and screening exams. See, among others, *Familias en Accion* in Colombia, *Red de Proteccion Social* in Nicaragua, *Programa de Asignacion Familiar* in Honduras.

[3] From now onwards I will use the terms non-poor/ineligible and poor/eligible interchangeably, as each pair identifies the same group of households.

viduals in active health-seeking behavior.[4] From a social perspective, the cost effectiveness of a medical screening program might change substantially in the presence of externalities (Christakis, 2004).[5] In the first part of the paper I study the effect of *PROGRESA* on demand for cervical cancer screening by ineligible households compared to demand for non-gender-specific screening tests. In order to disentangle the effect of the program on demand for and supply of screening, I exploit the variation across villages in health center waiting time, which acts as the price of the health services. My results show that the indirect treatment effect (ITE) of *PROGRESA* on the propensity to screen for cervical cancer is positive, non-trivial and significantly different from zero. I do not find any significant indirect effect on the probability of screening for diabetes and high blood pressure (hypertension), or attending a health center. Different empirical tests do not provide support for the hypotheses that the indirect effect of *PROGRESA* on cervical cancer screening is due to income spillovers from eligible to ineligible households, or to changes in the supply of health provisions and in female bargaining power.

In the second part of the paper, I study whether gender-related social norms and information sharing can explain the indirect effect of the program on the propensity to screen for a female-specific condition. Qualitative evidence on Mexico, collected via the evaluation of *PROGRESA* (Adato et al., 2000) and by various epidemiological studies (Lazcano-Ponce, 1997; Watkins et al., 2002), shows that one of the most common reasons why women do not attend PAP smear testing is male opposition to wives being checked by male doctors. I investigate whether, by increasing the fraction of women who screen in order to meet the conditionalities, *PROGRESA* increased the social acceptability of the smear test. *PROGRESA* might have also increased the availability of information about the risk factors associated with cervical cancer and the benefits of screening.[6] These mechanisms are hard to separate empirically. The data from the *PROGRESA* evaluation sample do not allow one to disentangle the importance of gender-related social norms and information sharing unless additional assumptions are introduced.

I propose a model of social norm diffusion in which the individual utility from screening depends on the action of other individuals in the locality, and women differ in the cost associated with the social norm that regulates screening for gender-specific diseases. *PROGRESA*,

[4] Miguel and Kremer (2004) using evidence from a randomized experiment show that a deworming program in Kenya significantly reduced infection rates among children not receiving the treatment.

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

[6] Lange (2011) uses data from the National Health Interview Survey (NHIS) on real and perceived cancer risks and cancer screening behavior to provide evidence for the US on the role of health information as one of the mechanisms behind the correlation between education and the propensity to screen. Dupas (2011) exploits a randomized experiment in Kenya to show that providing information on the relative risk of HIV infection by partner's age led to a large and significant decrease in unprotected sex among teenage women.

by providing poor households with economic incentives to screen, leads to an increase in the fraction of people in the locality who attend for screening. The model has three main empirical predictions: 1) for socially regulated screening tests the effect of *PROGRESA* on both ineligible and eligible households should vary with the fraction of poor households in the locality; 2) there should be no significant interaction between the effect of *PROGRESA* and the fraction of poor households for non-gender-specific screening tests, such as high blood pressure and diabetes; 3) for gender-specific tests, the intensity of the effect of *PROGRESA* with respect to the fraction of poor households should be stronger among women who are potentially more affected by the social norm.

A learning-based explanation implies that women only care about the screening behavior of other women in the locality to the extent that it conveys useful information, but their payoff from screening is not directly affected by others' actions. Drawing on recent frameworks used to model learning in the adoption of agricultural technology (Bandiera and Rasul, 2006) and consumption decisions (Moretti, 2011), I propose a model where women update their *prior* on preventive screening benefits using information obtained through alternative sources - e.g. their peers and health information sessions. I obtain three additional implications. First, if ineligible households learn from eligible ones about health risk factors and the benefits of screening, I would expect the propensity to screen among ineligibles to increase with the fraction of eligibles, irrespective of whether the health condition is gender specific or not. Second, the importance of social learning should be systematically different for eligible and ineligible households. In order to qualify for the transfer, eligible members have to attend health and hygiene related courses where they are given information on various health issues including cervical cancer, high blood pressure and diabetes. Since eligible households can rely on this additional source of information, information received from their peers should matter less than for ineligible household members. Third, the learning externality should be bigger among individuals whose initial level of information about cervical cancer risk factors is less precise.

I exploit the features of the *PROGRESA* evaluation sample and the variation in female emancipation between male-headed and female-headed (widowed) households to test the empirical predictions of the social norm and the social learning models. Overall, the three empirical predictions of the social norm model are consistent with the data. I find less evidence to support a social learning explanation.

The *PROGRESA* program has been extended gradually to the entire country. The 2007 survey differs from previous ones in collecting detailed information on health centers and the characteristics of doctors including age, qualifications and, most important for this study, their sex. It also provides information on an additional female specific screening, namely mammogram. I am able to test directly whether the effect of *PROGRESA* on gender specific screening tests is related to the presence of male doctors. I find that only the propensity to screen for cervical and breast cancer is positively correlated with the exposure to *PROGRESA*, and negatively correlated with the fraction of male doctors in the locality. The effect of the exposure to the program on female-specific screening is significantly stronger in those localities where there is a higher proportion of male doctors. There is no evidence that male doctors are more likely to advise the systematic use of gender-specific screening than female doctors.

The different pieces of evidence, taken all together, support the hypothesis that *PROGRESA* increased the social acceptability of female-specific screening tests. Nevertheless, the data limitations do not allow me to rule out the possibility that the indirect effect of the program on cervical cancer screening might be partly explained by other mechanisms, with information sharing being the most prominent one.

This paper contributes to different strands of the literature. Much of the work on the importance of cultural background for health outcomes focuses on fertility and compares outcomes for individuals from different countries of origin (see Fernández and Fogli (2006) for the US and Almond et al. (2009) for Canada).[7] This work contributes by providing a specific example of a gender-related cultural norm that affects the demand for medical screening. More important, this is the first work to provide suggestive evidence that large scale policy interventions, such as the *PROGRESA* program, can have significant effects on the social norms that regulate individual behavior.[8]

A recent body of work studies the indirect effects of welfare programs. Angelucci and De Giorgi (2009) provide evidence that *PROGRESA* increased the consumption of ineligible households operating through insurance and credit-market mechanisms. Angelucci et al. (2010b) use information on the surnames of household partners to study the role of the extended family in shaping the indirect effect of *PROGRESA* on consumption and investment.[9] This paper contributes by providing evidence that *PROGRESA* also affects the behavior of ineligible households through non-market mechanisms.

Another strand of literature studies the mechanisms through which peers affect *experience goods* consumption. Among others, Cai et al. (2009) exploits a randomized natural field experiment to study the presence of observational learning on menu items in restaurants. Moretti (2011), using box-office data, provides empirical evidence of the effect of social learning on movie choice. The findings in Oster and Thornton (2011) show that social learning

[7] An exception is represented by Luke and Munshi (2007), which studies how caste affiliation affects investment in children's health in India.

[8] Di Tella et al. (2007) exploit a natural experiment that induced exogenous allocation of property rights to study the formation of pro-market beliefs among squatters.

[9] Bobonis and Finan (2009) find that *PROGRESA* significantly increased school enrollment among ineligible families through a peer effect. Lalive and Cattaneo (2009) find evidence of a social interaction effect in school attendance among ineligible children and argue that it might be driven by a change in parents' perceptions of children's ability. Angelucci et al. (2010a) do not find robust evidence that the program has a higher than average effect on secondary school enrolment among children living in ineligible households.

plays an important role in the propensity to adopt menstrual cups in Nepal. This study provides evidence of the effect of social pressure on the decision to consume a particular type of experience good, i.e. preventive screening, in a developing country context.

The paper is organized as follows. In Section 2 I provide background information on female-specific conditions in Mexico, and I describe the *PROGRESA* program and my data. Section 3 presents descriptive evidence of how the program has affected the supply of health provision and screening rates for gender- and non-gender-specific conditions in the evaluation sample. Section 4 proposes a simple model to disentangle the program's demand and supply effects and presents some baseline evidence. Section 5 provides and discusses empirical evidence for two mechanisms that might explain the indirect effect, namely social norm and information sharing. In Section 6 I test whether the long-run evidence of the expansion of the program is consistent with the conclusions based on the randomized evaluation sample. Section 7 concludes.

2 Background

2.1 Gender specific diseases in Mexico

Cervical cancer has a precancerous condition, the Human Papilloma virus (HPV), which is present in 99.7% of the cervical cancer cases and can be detected through a standard PAP smear test. The main risk factors for HPV are related to sexual behavior: early age at first intercourse, multiple sexual partners, early age at first pregnancy, multiparity, and previous sexually transmitted infections.[10]

Following the example of many developed countries, in 1974 the Mexican government launched its Cervical Cytology Screening Program (CCSP). This program has been constantly improved by the Mexican government, and includes measures that: i) allow all women to be screened free of charge regardless of their age; ii) require health professionals to offer screening to women in the 25-64 age group, with particular attention to those with high risk factors; iii) include written or verbal invitations for screening to all rural households with at least one woman aged 25 or over. Women who present normal cytologies for two consecutive years are invited to screen only every three years. However, despite this program, the adjusted mortality rate gap between Mexico and the other OECD countries continued to increase until the late 1990s. It was not until the first decade of the 2000s that a significant reduction occurred, although mortality rates are still high compared to the

[10] Additional risk factors include smoking and malnutrition. Since precancerous cells can be identified in a standard screening procedure, never being screened increases the risk of contracting cancer. The evolution from precancerous to cancerous cells can take many years, thus increasing the benefits from screening (Blumenthal and Gaffykin, 2005; World Health Organization, 2006).

other OECD countries and to other Latin American countries.[11] According to the Mexican Statistical Office, in 2007 cervical cancer mortality accounted for 12.1% of all cancer deaths in the female population, with breast cancer accounting for 13.8% (the highest percentage).

While the high mortality might be due in part to the poor quality of health provisions (Flisser et al., 2002), a key determinant is the low uptake of screening. Despite the increase in recent years, the percentage of women who screen regularly is very low. According to the 2006 National Health and Nutrition Survey (*ENSANUT*), 36.1% of women aged 20 or over had submitted to a PAP test in the 12 months before the survey, up from 27.4% in 2000 based on data from the National Health Survey (*ENSA*). In the same year, an average of 64% of women aged 20-69 in OECD countries screened for cervical cancer.

There is a breast cancer screening program that targets Mexican women aged 40-69. Also in this case, in recent years there has been an increase in the percentage of women who screen regularly, but the uptake is still low.[12]

2.2 The *PROGRESA* program: features

PROGRESA is a cash-transfer, anti-poverty program that targets poor households. The average monthly grant up to November 1999 was 200 pesos per household, or 32.5 pesos per adult equivalent.[13] This is equivalent to about 23 percent and 16 percent of average food consumption per adult equivalent for poor and non-poor in the control villages, respectively (Angelucci and De Giorgi, 2009). Eligibility for the program is based on poverty level as defined by a measure of permanent income based on the information collected in the September 1997 census of villages. Women within the household are the transfer recipients.

The program offers two benefits: i) it provides cash transfers to households conditional on their children's attendance at primary and secondary school; ii) it provides transfer and nutritional supplements conditional on regular health checks and attendance at health courses offered at local facilities. Children under 24 months and pregnant women are required to undertake screenings throughout the year; lactating women and children aged 2-4 years are required to have two health checks per year; all individuals aged 17 or over must have an annual check up. The health center visits include advice on family planning, prenatal, childbirth, and puerperal care; vaccinations; prevention and control of high blood pressure and diabetes mellitus; and preventive treatment and screening for cervical cancer. In addition, beneficiaries are asked to attend health and nutrition classes (known as *platicas*). While

[11]According to the WHO, in 2002 the cervical cancer standardized mortality rate in Mexico was 14.1 per 100,000 inhabitants compared to 10.2 in Brazil and 7.8 in Argentina. In the same year mortality rates in the US and Canada were respectively 2.3 and 2.5 per 100,000 inhabitants.

[12]According to *ENSA*, in 2000 only 12% of women in the target group had a mammogram in the 12 months before the interview. In 2006 about 22% of women aged 40-69 reported being screened in the previous year (*ENSANUT*).

[13]In the late 1990s 10 pesos was approximately US 1$.

classes are mainly aimed at mothers, any member of the beneficiary households can attend. Non-beneficiaries in principle are allowed to attend educational classes. However, although there is some variation across villages, Adato et al. (2000) report that there is a consistent lack of participation in health and nutrition talks among those not entitled to the transfer. The classes cover various health and nutrition aspects with special emphasis on preventive health care.

Although *PROGRESA* is focused mainly on increasing demand for health services, it promotes actions to improve the supply of healthcare, including ensuring adequate supplies of equipment and medicines at health centers, and training of health professionals to improve the quality of medical care.

2.3 Data

The experimental data contain information on households from a sub-sample of 506 poor rural villages in seven states: 320 villages were randomly assigned to the treatment group and started receiving benefits in May 1998; 186 villages were randomized out and did not receive treatment until November 1999. The sample initially included 24,077 households. Households were informed that once they were classified as poor or non-poor this status (and thus eligibility) would remain unchanged through November 1999 regardless of any income variation. Only the poor households in treatment villages were eligible for the *PROGRESA* transfer (see Fig. 1). Two selection rounds were held: in 1997 52% of households were classified as poor and therefore eligible for the cash transfers. However, this allocation between eligible and ineligible households was revised before the program was rolled out and 54% of households usually referred to as *densificados* and initially classified as non-poor were reclassified as being in the eligible group.[14]

The main results presented in this paper are based on a sample that includes the *densificados*. For each specification I conduct sensitivity analysis excluding them and, while some of the tests have lower statistical power, the main conclusions are virtually the same. Since most of the explanations for which I test are strongly correlated with the socioeconomic status, by excluding the *densificados* I lose the group of ineligible households that are most likely to be affected by these mechanisms. Two important points should be stressed. First, most of the *densificados* did not receive any benefit between May 1998 and May 1999, which is the time interval between the introduction of the program and the last survey of the evaluation sample for which I have information on screening behavior. Second, and most important, if the *densificados* are screening more in order to comply with conditionalities of the program, I should observe an increase in the screening rates for all health conditions,

[14] A non-random subset of these households began receiving *PROGRESA* transfers in treatment villages before November 1999.

both gender and non-gender specific.

Table 1 shows the means and the standard deviations of socioeconomic variables, measured mostly at the baseline, for poor and non-poor households included in the evaluation sample. Socio-demographic characteristics are well balanced between the treatment and the control group for both eligible and ineligible households. The March 1998 survey asks female respondents for information on present and past sexual activity, thus allowing the construction of proxies for some of the risk factors associated with cervical cancer, namely the number of pregnancies, the probability of having never used contraception, and the probability of having never submitted to a PAP test. The *PROGRESA* dataset also contains measures of female emancipation. Before the program was implemented (March 1998) all female respondents were asked 6 questions about women's status.[15] I converted the answers to these questions into dummy variables and derived a Female Status (*FS*) index ranging between 0 and 6, where 6 represents the lowest degree of female emancipation. Irrespective of whether I consider poor or non-poor households, the proxies for both cervical cancer risk factors and female emancipation are on average not statistically different in treatment and control villages.

In 2003, a new follow-up round of data and a new control group, consisting of communities not yet covered by *PROGRESA* and chosen through propensity score matching, was included in the evaluation. This group began participating in the program only in 2004 or afterwards. The 2007 Rural Evaluation Survey (ENCEL) collected data on the original evaluation sample[16] and the 2003 control localities.

In 2007, the information on screening decisions is at the individual level (which contrasts with the evaluation sample). All women in the household aged less than 50 were asked whether they had been screened for cervical cancer and, unlike previous surveys, breast cancer. They were also asked about hypertension, diabetes and cholesterol screening. The survey also included three modules particularly relevant for my purposes. There is a health center questionnaire directed to center administrators that includes an exhaustive set of questions on center characteristics, number and type of services offered, technical equipment, and numbers and working hours of doctors and nurses. A second module is a doctors' questionnaire to collect information on socio-demographic characteristics, specializations, training and current practices. It asks specifically about the frequency of advice on and performance of gender-specific screenings, i.e. PAP smear test and mammogram. Finally, there is a module addressed to young people in the age group 14-24 that includes questions designed to assess their knowledge of health risk factors, including sex-related conditions.

[15] In particular, they were asked if they agreed or disagreed with the following statements: i) a woman's place is in the house; ii) women have to obey men; iii) women have their say in community issues; iv) women should have a job outside the house; v) women should have same rights as men; vi) women should have their own opinions.

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

Table 2 presents the characteristics of the sample of women aged 18-50 interviewed in 2007 by the time of inclusion in the *PROGRESA* program. The top panel shows that the two groups display significant differences in terms of individual and household characteristics. For instance, women who live in localities that received *PROGRESA* after 2003 display significantly higher levels of literacy and education than those living in localities that received the program before 2000. The bottom panel displays doctor and health center characteristics. Localities where the program started later have on average a higher number of doctors and nurses and more experienced (longer tenure) doctors. In 79% of the localities that received *PROGRESA* in 2004 or after there is at least one permanent health center that offers the cervical cancer screening service, as opposed to 69% in early exposure localities.[17]

3 Descriptive Analysis

3.1 Health Supply

The first source of information on the provision of health services in the *PROGRESA* localities are the October 1997 and October 1998 locality questionnaires, which included detailed questions about the type of health providers and services available in the village. The upper panel in Table 3 provides evidence of health providers' coverage in treatment and control villages before and after the program was in place. One of the distinctive criteria for a village to be included in the *PROGRESA* evaluation sample was the presence of basic health services. At the baseline, approximately 90% of the *PROGRESA* localities have at least one health provider and the percentage is practically the same for treatment and control localities.

While the Health Secretary (*SSA*) and *IMSS Solidaridad* hospitals are, on average, bigger and better equipped than health aid centers and mobile units, all offer basic screening services.[18] When I look at the composition of the health providers at the baseline, the only significant difference between treatment and control localities is in the fraction of localities covered by the Health Secretary (*SSA*) clinics, with 13% of control villages covered by *SSA* clinics, compared to 8% of treatment villages (the difference is statistically significant at 10%). At the baseline, treatment and control localities have on average the same number of health services.[19] After the program started, I observe an increase in the percentage of

[17] The fraction of localities where it is possible to screen should be higher since Table 2 does not take account of mobile units, for which I do not have information.

[18] *IMSS Solidaridad* is a program launched by the Mexican Government in cooperation with the Mexican Institute of Social Security. The auxiliary health units are usually in rather inaccessible rural locations (with populations of between 500 and 1,000 inhabitants) and they can usually rely on the services of one general practitioner. The mobile health units are staffed by medical practitioners and paramedics who offer a full set of outpatient services. Auxiliary health units and mobile units are the most common providers in *PROGRESA* villages.

[19] Based on the 7 services listed in the locality questionnaire: prenatal care, delivery care, infant

localities covered by at least one health provider (approximately 94%), both in the treatment and the control groups. The composition of the health providers is basically the same for the two groups. Both treatment and control localities display an increase in the average number of services available (approximately 3).

The socio-economic questionnaires administered in the March 1998 and October 1998 waves asked for specific information on the main characteristics of health centers attended by any of the household members in the previous six months, including center opening times, cost of visits, waiting times, length of consultation, and reception of medicines from the doctor. In the lower panel of Table 3 I consider the averages of individual responses at village level and provide evidence for a number of health supply characteristics. Baseline differences between treatment and control villages are not significant except for duration of consultations, which is slightly longer in control villages. *PROGRESA* does not result in significant changes in waiting times, opening times or visit duration. The average consultation fee for treatment and control villages dropped dramatically in October 1998, but the reduction is significantly bigger for the treatment villages. This is entirely due to eligibles accessing health centers free of charge as part of the program conditionalities.[20]

Overall, the results presented in this section suggest that health services were strengthened equally in treatment and control villages. This is most likely due to the fact that improvements in health facilities in the control villages were carried out ahead of program implementation at the end of 1999.

3.2 Screening Behavior

One pre-program survey (March 1998) and two surveys after the program started (October 1998 and May 1999) contain household-level information on the uptake of three screening tests: cervical cancer (via the PAP smear test), diabetes (blood sugar test) or hypertension (blood pressure test). In the March 1998 wave the household respondents (usually female) were asked whether any household member had been screened for these conditions in the previous 12 months; in the following two waves the question referred to the previous six months. In order to compare program and pre-program screening levels, I calculate the cumulative probability that any household member is screened either in the six months before October 1998 or in the six months before May 1999. This measure can be compared directly with the March 1998 information.

Table 4 displays the screening rates by poverty status before and after the program was in place. The propensity to screen shows an increasing trend over time for eligibles and ineligibles in both treatment and control villages. This result is consistent with the

care, vaccination, diarrhea treatment, family planning, and hospitalization.

[20]The average consultation fee reported by ineligible households in the October 1998 wave is not statistically different in treatment and control localities.

already discussed increase in health supply coverage for treatment and control villages. In order to measure how screening rates change after program implementation, I estimate an unconditional Differences in Differences (DD) linear model, with standard errors clustered at the village level. As expected, screening rates for eligibles show a remarkable increase for all the tests, on average above 20 percentage points (see top panel in Table 4).

Among ineligibles, those living in treatment localities are 6.3 percentage points more likely to screen for cervical cancer than those in control localities after the program was in place (see column 1 of the bottom panel in Table 4). The size of the effect corresponds to about 18% of the pre-program screening rate among ineligible households in control localities. In contrast, ineligible households in treatment localities display small and not statistically significant changes in the propensity to screen for blood pressure and blood sugar.[21] In Table AI I present results for the sample of non-poor households that excludes the *densificados*. The coefficients are smaller than those presented in the bottom panel of Table 4 but the conclusions are virtually the same.

The effects presented in this section have to be interpreted as the overall effect of *PROGRESA* on screening behavior, since they might reflect both potential demand and supply changes induced by the program.

4 The Effect of *PROGRESA* on the Demand for Screening

4.1 Identification Strategy

There are three ways by which *PROGRESA* might lead to changes in the screening rates of ineligible households. First, the program might affect the demand for screening from ineligible households. Second, the improvement of the health services in treatment localities might benefit both eligible and ineligible households (the *supply* effect). Third, the higher demand of health services by eligible households that have to comply with program conditionalities might *crowd out* the demand from ineligible ones.

In order to disentangle the first effect from the other two, I exploit variations across villages and over time in average waiting time and in the presence of at least one health provider in the locality. There is a large literature (e.g., Lindsay and Feigenbaum, 1984; Gravelle, 1990; Blundell and Windmeijer, 2000) that relates waiting times to the demand and supply of health services. In this framework, a village's average waiting time acts as the price of health services for households in the community. There are two main reasons why I choose waiting time rather than a more standard monetary price. First, because of the

[21] A standard cross-equation test rejects the hypothesis that the size of the DD estimates among ineligibles is the same for the three screening exams.

national screening program, cervical cancer screening is free of charge for women irrespective of the treatment status of the village and the health provider. Second, the locality's average consultation fee would not represent the true cost sustained by households not eligible for the transfer in treatment villages, since the eligible ones access health facilities for free according to program conditionalities.

Formally, I estimate the following equation using a linear model:[22]

$$Y_{it} = \gamma_0 + \gamma_1 P_i + \gamma_2 T_t + \gamma_3 P_i * T_t + \beta' X_i + \delta_1 W_{it} + \delta_2' H_{it} + u_{it} \tag{1}$$

Y_{it} denotes the health screening decision of household i at time t. P_i takes the value 1 if household i lives in a locality covered by *PROGRESA*, and 0 otherwise. T_t takes the value 1 for surveys after the program's implementation, 0 for those before. X_i includes age and literacy of the household head, household poverty index, household size, number of children (in dummies), and whether the household is covered by an *IMSS* insurance. I control also for the average poverty index for the locality and state-fixed effects. All these controls are measured at the baseline. Although controlling for baseline sociodemographic characteristics likely increases the precision of the estimates, it does not affect the estimation of my parameter of interest. The specification also controls for the average waiting time in the locality of household i at time t, W_{it}, and a dummy variable that controls for the presence of at least one health provider in the village at time t, H_{it}. W_{it} and H_{it} are measured both before and after implementation of *PROGRESA*. In the estimation standard errors are clustered at village level, which is the level at which *PROGRESA* operates, in order to capture common shocks that might have affected household screening behavior within the village.

If I consider the sample of non-poor households, the parameter γ_3 identifies the indirect treatment effect (ITE) of *PROGRESA* on the demand for screening. If I estimate equation (1) on the sample of poor households, γ_3 identifies the average treatment effect (ATE). These are the two parameters of interest for my analysis. By using a DD strategy, I control for the possibility that there are pre-program differences in the prevalence of a certain disease and/or the possibility to screen for it, which I cannot control for.

The inclusion of W_{it} and H_{it} to isolate the demand effect of the program from the supply and the crowding out effects might in principle impart bias in the estimate of the parameter γ_3 if *PROGRESA* has a direct effect on waiting time and health providers' coverage at the village level. To see this, assume that the equation for W_{it} is given by:

$$W_{it} = \mu_0 P_i + \mu_1 T_t + \mu_2 P_i * T_t + \epsilon_{it} \tag{2}$$

where μ_2 captures the effect of the *PROGRESA* program on the waiting time in the

[22] Results based on probit models, not reported here, are perfectly in line with the reported results.

locality where household i lives. Similarly, H_{it} can be written as follows:

$$H_{it} = \rho_0 P_i + \rho_1 T_t + \rho_2 P_i * T_t + \eta_{it} \quad (3)$$

Overall the evidence presented in Table 3 does not support the hypothesis that the implementation of *PROGRESA* led to differential changes of waiting time and health providers' composition in treatment and control localities, thus suggesting that for the case I consider the endogeneity bias is negligible. Nevertheless, it is possible to show that if the supply effect of *PROGRESA* is stronger than the crowding out effect - μ_2 and ρ_2 are negative - and, ϵ_{it} and η_{it} are negatively correlated with u_{it}, the OLS estimate of γ_3 is a downward biased estimate of the demand effect induced by the program.[23]

Three basic assumptions are needed to identify the effect of *PROGRESA* on the demand for screening of non-poor and poor households. First, I assume there are no spillover effects from treatment to control villages, so that the demand for medical screening is driven by whether households live in a treatment village or not, and not by the statuses of other villages. Second, I assume a random assignment of villages into treatment and control groups. This is equivalent to assuming that whether a household is in a treatment or a control village is independent of unobservables that might affect the demand for health services. These two assumptions of no cross-village spillovers and random assignment are standard requirements for identifying ITE and ATE (Angelucci and De Giorgi, 2009; Angelucci et al., 2010a). They are equivalent to assuming that non-poor (and poor) households in control villages provide a valid counterfactual for non-poor (and poor) households in treatment villages in terms of health service utilization. To provide support for the first assumption I note that villages were included in the evaluation data because they were geographically distant. With respect to the second assumption, it has been documented already (Schultz, 2004; Berhman and Todd, 1999) that household and village characteristics do not significantly differ across treatment and control villages, which is consistent with the random assignment. Third, I assume that changes in health supply and crowding out driven by *PROGRESA* can affect the propensity to screen only in terms of waiting time and health provider composition. While this assumption might seem overly strong, I discuss its validity in the next section.

4.2 Baseline Results

I first estimate the ITE of *PROGRESA*, as described in equation (1), for three different outcomes: testing for cervical cancer, testing for diabetes, and testing for hypertension. The results presented in column (1) in the top panel in Table 5 show that once I account for waiting time and the presence of at least one health provider *PROGRESA* led to a 6.1

[23] For simplicity, assume that $cov(\epsilon_{it}, \eta_{it}) = 0$. Then the least squares estimate of γ_3 from equation (1) given equations (2) and (3), $\hat{\gamma_3}$, is $\gamma_3 - \mu_2[\frac{cov(\epsilon_{it},u_{it})}{var(\epsilon_{it})}] - \rho_2[\frac{cov(\eta_{it},u_{it})}{var(\eta_{it})}]$.

percentage point increase in the propensity to screen for cervical cancer among women living in non-poor households. The results in columns (2) and (3) in the top panel show that there is a small and not statistically significant effect of *PROGRESA* on the demand for blood pressure and blood sugar screening among non-poor households. Comparing the results in Table 5 with the overall effects due to the program, shown in Table 4, suggests that the variation in health supply plays a fairly limited role in explaining the indirect effect of the program on the propensity to screen. This is not surprising since, irrespective of being in a treatment locality or not, most health providers can perform basic screening tests such as the PAP smear, the blood pressure, and the blood sugar screens. The bottom panel in Table 5 shows the results for eligible households: there is a significant increase of over 20 percentage points in the probability of undertaking all screening tests, irrespective of whether or not they are gender specific. In alternative specifications, whose results are not displayed for lack of space, I control for differences in health supply by including dummies for each type of health provider. The results are in line with those presented.

Next I explore some of the mechanisms by which *PROGRESA* might have increased the demand for cervical cancer screening among ineligible households.

Income Effect. Previous work (Angelucci and De Giorgi, 2009; Angelucci et al., 2010b) provides evidence of income spillovers from poor to non-poor households. This increase in available income might have shifted upward the demand for health services by non-poor households. In other words, women are being screened for cervical cancer more often just as a result of the higher propensity to use health services among ineligibles. While the lack of a significant effect on non-gender-specific screening outcomes seems to exclude this explanation, I can test whether the program increases access to clinics and health-related expenditure. The results in columns (1) to (3) in Table 6 report results for three different outcomes: probability of accessing a health center for a visit in the last 6 months; expenditure on doctor consultations; and expenditure on medicines. The upper panel of the table presents the results for the non-poor. There is no significant evidence of ITE on the probability of accessing a health center to see a doctor. While this result might seem to be inconsistent with an increased cervical cancer screening rate, it is consistent with a change in the demand for female-specific screening. The national program guidelines require health professionals in all Mexican localities to invite women aged 25-64 for regular cervical cancer screening, but the evidence in Adato et al. (2000) suggests that women frequently refuse to be tested. I also found no indirect effect on health-related expenditure (see top panel in Table 6, columns (2) and (3)).

The bottom panel in Table 6 reports the results for the group of poor households. As expected, members of poor households are significantly more likely (16.8 percentage point increase) to have accessed a clinic in the previous 6 months to visit a doctor. For this group there is a reduced expenditure on both doctor consultations and medicines (columns (2) and (3)). This is most likely due to the fact that poor households receive medicines and treatment as part of the conditionalities for receiving the transfer.

Health Supply. *PROGRESA* might have improved the "quality", rather than the "quantity", of health care in treatment villages. In particular, since the program is targeted mainly at pregnant and lactating women, doctors working in treatment villages may have more in-depth knowledge about female-specific conditions, gained through attendance at training courses or adherence to specific guidelines. This could explain the significant indirect effect on screening for cervical cancer screening but not for other conditions. In order to investigate this, I test the effect of the program on the probability of receiving an immunization that is routinely recommended during pregnancy, the tetanus one. The underlying rationale is straightforward: if the program has improved the ability of doctors to deal with female-specific issues, I should observe a change in the probability of being vaccinated during pregnancy. The results presented in column (4) in Table 6 do not provide any significant evidence of the indirect effect of the program on this pregnancy-related outcome. Another potential issue related to the quality of health providers might the substitution of public care by private care. Consistent with Gertler (2000), I found no evidence of a change between health care providers among non-poor households.

Female Empowerment. Among eligibles, women are the transfer beneficiaries. Previous studies have provided evidence of changes in household consumption patterns that are consistent with the hypothesis that *PROGRESA* led to increased female bargaining power in the household.[24] If women in eligible households transfer money to women in ineligible ones, the latter might also display an increase in bargaining power within the household. The indirect effect on cervical cancer screening might then be the effect of increased female bargaining power.

Using information on household expenditures during the week before the interview, I find no evidence of any significant ITE on the expenditure shares for boys' and girls' clothing or alcohol (top panel in Table 7).[25] This result is not consistent with an increased bargaining power for women living in ineligible households. The evidence for eligible households, however, does support the hypothesis of an increase in female bargaining power: *PROGRESA* leads to higher expenditure for boys' and girls' clothing and lower alcohol expenditure (see bottom panel in Table 7).

To summarize, the evidence presented so far shows that the magnitude of the indirect

[24] Attanasio and Lechene (2002); Rubalcava et al. (2009) exploit the exogenous variation in the female income induced by the *PROGRESA* program to provide evidence that, conditioning on household income, a higher share of income in female hands leads, among others, to a higher household expenditure in boys' and girls' clothing and a lower alcohol expenditure.

[25] Data on expenditure for these items are not available in the pre-program data collection. The estimates are based on a simple difference between treatment and control localities.

effect of *PROGRESA* on demand for cervical cancer screening from ineligible households is non-trivial and statistically significant. My findings do not support the hypothesis that the behavioral response of non-poor households in terms of screening for a female-specific condition is due to changes in either the "quantity" or the "quality" of supply. I also find no evidence that either income spillovers from eligible to ineligible households or changes in female bargaining power can explain the indirect effect on cervical cancer screening.

5 Potential Mechanisms

5.1 Social Norm

Results from epidemiological research (see Lazcano-Ponce, 1997; Watkins et al., 2002) show that male opposition to wives being checked and concerns about physical privacy are two of the main reasons why women do not go for screening. Adato et al. (2000), in their study of the operational performance of *PROGRESA*, report that most doctors agree that PAP smear testing was problematic because many men were opposed to their wives having the test, especially if screening was conducted by a male doctor. This evidence suggests that the individual decision to seek screening for cervical cancer might be socially regulated. In the next section I propose a model of social norm diffusion that describes how the *PROGRESA* transfer might have increased the social acceptability of the PAP smear test.

5.1.1 Model of Social Norm Diffusion

In this section I outline a simple framework that describes how the introduction of *PROGRESA* in the presence of an established social norm might have affected screening behavior. My characterization of social norms is close to those proposed by Kandori (1992) and Munshi and Myaux (2006). The overall aim is to assess whether the indirect effect of *PROGRESA* on cervical cancer screening is consistent with the hypothesis that the program weakened a gender-related social norm. The model is designed to generate transparent and testable predictions.

Consider a village consisting of a continuum of women. A woman can choose between two actions: screening for a gender-specific condition (s) and not screening (ns). When screening behavior is socially regulated, the payoff depends on both the intrinsic utility the individual woman derives from screening and also on the social pressures or sanctions that accompany it. The individual's payoff depends on her individual action and on the action of a peer. I can assume without loss of generality that in each period each woman can only be matched with one other woman in the village.

Formally, I model the payoff from screening before implementation of *PROGRESA* as follows:

$$V_i^k(s,s) = w^k \tag{4}$$
$$V_i^k(s,ns) = w^k - l_i \tag{5}$$
$$V_i^k(ns,ns) = 0 \tag{6}$$
$$V_i^k(ns,s) = 0 \tag{7}$$

where k denotes the household's poverty status and is equal to P for poor (eligible) households and NP for non-poor (ineligible) households. V_i^k is the payoff for a woman i living in a household with the poverty status k, where the first term in parentheses refers to the woman's own action and the second term refers to the action of her peer. I allow for the possibility that the payoff from screening is different for poor and non-poor households.[26] l_i, which varies across women, is the cost of the social norm for woman i and stands for either the husband's reaction or the woman's fear of his reaction. The underlying intuition is that husbands will punish their wives if their behavior does not conform to the behavior of most of the wives in the community. l_i is assumed to be normally distributed with $l_i \sim N(\bar{l}, \sigma^2)$. I assume that the expected loss of utility from the decision not to screen is equal to 0, independent of peer action.[27]

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

$$\Pi = \mu\pi^P + (1-\mu)\pi^{NP} \tag{8}$$

μ is the fraction of poor households in the village; π^P is the average screening probability for women living in poor households; and π^{NP} is the average screening probability for women living in non-poor households. Every woman will opt for screening if:

$$\Pi w^k + (1-\Pi)(w^k - l_i) \geq 0 \tag{9}$$

Women base their decision to screen or not on the overall probability of meeting other women who screen, irrespective of their poverty status.

As *PROGRESA* provides women in poor households with a financial incentive to screen for cervical cancer, the expected payoff for poor women increases by an amount τ, but does not change for women in non-poor households. In equilibrium, among poor households only

[26] This is consistent with the higher screening rates displayed by ineligible women at the baseline (see Table 4). Women living in non-poor households are likely to have a higher opportunity cost from contracting the disease since at the baseline they are more likely to work outside the house and have a higher income.

[27] Alternatively, I could assume that there is a social reward for a woman who decides not to screen and who is matched with a woman who does have the test (Luke and Munshi, 2007). My main conclusions would not change.

women with $l_i \leq l^{P*}$ screen, where l^{P*} is given by:

$$l^{P*} = \frac{w^P + \tau}{(1 - \Pi^*)} \tag{10}$$

For women living in non-poor households, only women with $l_i \leq l^{NP*}$ will screen, where l^{NP*} is given by:

$$l^{NP*} = \frac{w^{NP}}{(1 - \Pi^*)} \tag{11}$$

Π^* represents the overall fraction of women who screen in equilibrium and it is given by

$$\Pi^* = \mu \int_{-\infty}^{\frac{w^P+\tau}{(1-\Pi^*)}} \phi(l)dl + (1-\mu) \int_{-\infty}^{\frac{w^{NP}}{(1-\Pi^*)}} \phi(l)dl \tag{12}$$

Using equations (10), (11), (12), and the implicit function theorem, I can derive how the equilibrium screening rates of both poor and non-poor households change in response to the cash transfer:

$$\frac{\partial l^{P*}}{\partial \tau} = \frac{1}{(1-\Pi^*)} + \frac{w^P + \tau}{(1-\Pi^*)^2} \frac{\Delta \Pi^*}{\Delta \tau} \tag{13}$$

$$\frac{\partial l^{NP*}}{\partial \tau} = \frac{w^{NP}}{(1-\Pi^*)^2} \frac{\Delta \Pi^*}{\Delta \tau} \tag{14}$$

The function $h(\cdot) \equiv \frac{\Delta \Pi^*}{\Delta \tau}$ has the following properties:

1. $\dfrac{\partial h}{\partial \mu} > 0$;

2. $\dfrac{\partial^2 h}{\partial \mu \partial \bar{l}} > 0$

Munshi and Myaux (2006) model social norm diffusion as a learning process over time where people gradually update their priors. In my case, although women from treatment villages have no information about pre-program screening rates in their villages, they can make inferences about changes as result of the program. Between October 1997 and August 1998, *PROGRESA* convened public meetings where the eligibility and conditionalities applying to each household were spelled out.[28] Therefore, given the small size of the villages, it is reasonable to assume that all the women in the treatment villages were informed about who was required to undergo PAP testing as part of the conditionalities of the cash transfer.

The model has three testable predictions:

[28] After the program started, a community outreach worker, the *promotora*, chosen from among the eligibles, was responsible for providing information on the program for its duration. Although the promotora was meant to be in contact mainly with beneficiaries, Adato et al. (2000) reports frequent interactions with non-beneficiaries.

HP1 For socially regulated screening tests, both the effect for the non-poor (as measured by the ITE) and for the poor (as measured by the ATE) should increase significantly with the fraction of eligible households in the locality;

HP2 For non-socially regulated screening tests neither the ATE nor the ITE should vary with the fraction of eligible households;

HP3 For socially regulated tests, the size of the interaction between the treatment effects of the program, both ITE and ATE, and the fraction of eligible households in the locality should be bigger for those groups of women whose cost of violating the social norm is higher.

5.1.2 Empirical Evidence on the Social Norm Mechanism

In order to investigate the three predictions of the model presented above I estimate the following model:

$$Y_{it} = \gamma_0+\gamma_1 P_i+\gamma_2 T_t+\gamma_3 P_i*T_t+\gamma_4 FP_i+\gamma_5 P_i*FP_i+\gamma_6 P_i*T_t*FP_i+\beta' X_i+\delta_1 W_{it}+\delta_2' H_{it}+v_{it} \quad (15)$$

where FP_i denotes the fraction of poor households in the locality where household i lives. In *PROGRESA* localities the fraction of poor households represents the proportion of households required to comply with program conditionalities in order to receive the transfer.[29] The main parameter of interest is γ_6. When I estimate equation (15) for the sample of non-poor and poor households, γ_6 captures how the ITE and ATE vary respectively as the fraction of poor households in the locality increases.

My first hypothesis (HP1) implies that for the propensity to screen for cervical cancer screening, γ_6 should be positive both among eligible and ineligible households. According to the second hypothesis (HP2) γ_6 should, in contrast, be not significantly different from zero when I consider the propensity to screen for high blood pressure and diabetes, irrespective of whether I focus on eligible or ineligible households. Columns (1), (4) and (7) in Table 8 report the empirical tests of HP1 and HP2. The top panel reports the results for non-poor households. For cervical screening, the effect of the program is statistically non-significant in those villages where there is a low fraction of eligible households, but it increases as the fraction of poor households in the village rises (see column (1)). For blood pressure and

[29] The term P_i*FP_i accounts for possible anticipation effects. Because of the extensive information campaign implemented by the program organizers, non-poor households might anticipate that in localities with a higher fraction of eligible households there would be a higher demand for health services once the program was in place, and decide to screen before its start. This is relevant in my case since information on baseline screening rates comes from the March 1998 survey and the population started receiving information in October 1997.

diabetes there is no evidence that the effect of the program increases with the fraction of poor households (columns (4) and (7)).

The bottom panel in Table 8 reports the results for poor households. The ATE, not interacted, measures the direct effect of the conditionality: in order to receive the transfer, poor households are more likely to screen for cervical cancer, diabetes and hypertension. In principle, this parameter might also capture an increased level of health information due to the attendance of the health and nutrition courses. For cervical cancer (column (1)) the effect becomes stronger as the fraction of eligible households in the locality increases, while results in columns (4) and (7) show no evidence of any significant interaction of the program's effects with the fraction of eligible households for non-gender specific conditions.

In order to test the third implication of the model, HP3, I need to make additional assumptions. Male partners could be censorious (or might be perceived as such) concerning the decision of women in their household to screen for cervical cancer, especially if they knew that the test would be performed by a male doctor. This should not apply to widows.[30] Therefore, I assume that the cost of the social norm associated with cervical cancer screening is higher for married women than widows. The evidence presented in Tables AII and AIII shows that the lack of female emancipation, as measured by the *FS* index, is stronger among female respondents living in male-headed households than those living in widow-headed ones, irrespective of whether I consider poor or non-poor households. Previous evidence for Mexico (see, among others, Lazcano Ponce et al., 2001) shows that the risk of contracting cervical cancer is not statistically different for married women and widows. Women who have never been married display a significantly higher risk of developing cervical cancer, as their status might be correlated with some of the sex-related risk factors. For this reason, in the baseline results I exclude them when testing HP3. For both non-poor and poor households, I estimate the model in equation (15) separately for male-headed and widow-headed households.[31] Columns (2) and (3) in Table 8 report the results for cervical cancer screening.

The top panel displays the results for non-poor households. The coefficient on the interaction term, γ_6, is positive and statistically significant for the sample of male-headed households, while it is negative and not significantly different from zero for widow-headed households. The high standard error for the sample of widow-headed households might arguably be related to the reduced number of observations. Nevertheless, when I test whether the coefficient of the interaction term for male-headed households is statistically different from the one for widow-headed households, I can reject the null hypothesis at standard sig-

[30]In rural areas the probability that widows get remarried is extremely low: out of the 3,293 women who were widows according to the information elicited in the October 1997 survey, only 29 reported being married in the November 2000 survey.

[31]The results for the specifications where widows and women who have never been married are considered together are in line with those presented.

nificance levels (p-value=0.046).[32] Among poor households, the interaction term is positive and statistically significant at 10% level for the sample of male-headed households, while it is basically 0 for widow-headed households. In this case, however, the difference between the two coefficients is not statistically significant at conventional levels. When I consider non-gender-specific screening tests (columns (4)-(5) and (8)-(9) in Table 8), I do not find any consistent pattern for male-headed and widow-headed households.

An obvious concern is that male-headed and widow-headed households might be different along many dimensions (both observable and non-observable) that are correlated with the propensity to screen. Previous work suggests that older and poor women are least likely to be screened (e.g., Gakidou et al., 2008). On the one hand, the household head and his partner in the male-headed households are much younger than the head in widow-headed ones, irrespective of whether I consider poor or non-poor households (Tables AII and AIII). On the other hand, widow-headed households display higher levels of wealth than male-headed ones. I test for the possibility that the differential responses of male- and widow-headed households are due to differences in the age of the woman and household wealth index, but I do not find any evidence in support of this possibility.[33]

Male- and widow-headed households might also differ according to characteristics that cannot be observed - e.g. risk aversion and time discount - and my results might be explained by differences along these dimensions. While in principle I would have expected differences in time discount and risk aversion to affect also the propensity to screen for hypertension and diabetes, two advantages of my empirical strategy have to be stressed. First, since I combine a DD strategy with a randomized experiment, my results cannot be explained by unobservable characteristics that enter equation (15) additively, either time variant or time invariant. Second, since my main parameter of interest is the interaction between the treatment and the fraction of eligible households, my specification controls for unobserved characteristics that enter in a multiplicative way as long as their effect does not vary with the fraction of households that are eligible for the program in the locality.

In summary, the evidence presented in this section is remarkably consistent with the model of social norm diffusion presented in Section 5.1. Nevertheless, given the number of structural assumptions I had to impose and the fact that not all the tests have enough statistical power, I interpret these results as suggestive, rather than conclusive, that *PROGRESA* weakened the social norm that regulates cervical cancer screening. Additional evidence will

[32]When I exclude the *densificados*, the p-value of the test is 0.124.

[33]In order to account for age differences in the two groups, I estimate two alternative specifications: in the first one I control for the age of the female respondent in single-year age dummies, in the second I restrict the sample to those households where the head of household is 65 or younger. In both cases, the results (not reported here) were perfectly in line with the results presented. Similarly, when I estimate alternative specifications that include the square and the cube of the individual poverty score and the interactions with the treatment effects, the results were in line with those discussed above.

be provided in Section 6.

5.2 Social Learning

There is an alternative mechanism through which *PROGRESA* might affect the screening decision of non-poor households, namely social learning. Women who take the PAP test could share information with other women about different aspects of cervical cancer screening: risk factors, the existence of PAP technology, and their experience of the test. Women might learn from those who screen either through word of mouth or by observing their actions (observational learning). Similarly, both men and women could learn about the screening of non-gender specific conditions, such as hypertension and diabetes. A higher fraction of people in the locality who screen, driven by compliance with the *PROGRESA* conditionalities, would increase the opportunity for social learning. Therefore, a significant interaction between the ITE (or ATE) and the fraction of poor households in the locality in principle would be consistent with the presence of social learning. However, this is not the only empirical implication of the social learning mechanism. Appendix I presents a simple model that closely follows Moretti (2011) to describe how social learning affects the screening decisions of poor and non-poor households in *PROGRESA* localities. Here I provide the intuition and the main implications of the model.

Individuals have imperfect knowledge about the risk of contracting a specific health condition. Before the implementation of *PROGRESA*, individuals living in poor and non-poor households have a *prior* on the probability of contracting a disease, i.e. the utility from screening for it. This prior is updated through direct sharing of information with peers or observation of their screening behavior. Using the terminology of the social learning literature (Ellison and Fudenberg, 1995), the information received by others represents a *signal*. In the presence of social learning, an individual's expectation of utility from medical screening is the weighted average of the prior and the signal received from her peers, where the weights reflect the relative precision of prior and signal. In order to keep the model simple, I assume that before the introduction of *PROGRESA* there were no other sources of learning, such as learning by doing.[34] In my setting, additional mechanisms for acquiring information would not affect the predictions that I test empirically.

PROGRESA has two effects. First, since more individuals are screening in order to comply with the conditionalities of the program, the precision of the signal from peers' feedback is improved. In my framework, this improved precision affects individuals living in poor and non-poor households equally. Second, poor households receive an additional signal of the expected utility of screening tests, obtained through compulsory attendance at health and nutrition courses. Therefore, the expected utility from screening for poor households is now a weighted average of the prior, the peer feedback, and the information received in

[34]Individuals who have screened in the past have better knowledge of the risk factors.

classes. Although they could attend, there is no requirement for non-poor households to attend these courses. There are no individual data on class attendance in the evaluation sample but the qualitative evidence discussed above suggests that attendance among non-poor was extremely low. The model generates three empirical predictions. While these are formally derived in Appendix I, I summarize them below in an informal discussion:

1) Social learning should matter also for non-gender-specific conditions such as hypertension and diabetes. Knowledge about healthy lifestyles (risks related to smoking, drinking, and lack of physical exercise) and nutritional issues can play a key role in the prevention and treatment of diabetes and hypertension, making the benefits from increased information potentially large. Moreover, the prevalence of hypertension and diabetes in the Mexican population is higher than the prevalence of cervical cancer.[35] The probability of sharing information with someone with direct experience of the disease is higher for hypertension and diabetes than for cervical cancer.

2) The weight of social learning should be bigger for non-poor than for poor households. Non-poor households update their priors using only the information received from their peers. Since poor households have an additional source of information, namely the *platicas*, they should give less weight to the information received from their peers.

3) Social learning should be more important for those individuals whose priors are less precise. The greater the precision of the information that an individual holds about a particular health condition and the benefits associated with screening, the lower the weight given to feedback from peers.

According to the first prediction, the interaction between the treatment effect and the fraction of eligible households should be positive and statistically significant for both hypertension and diabetes screening. The results in Table 8 show that for both types of screening the signs of the interaction terms are never statistically significant, irrespective of whether I consider the sample of poor or non-poor households.

The second implication of the learning model suggests that, regardless of the gender-specific nature of the disease, the size of the interaction between the ITE and the proportion of eligible households should be bigger than the interaction between the ATE and the same proportion of households. Results in Table 8 show no clear pattern to support this prediction.

Third, the social learning model predicts that the size of the coefficient of the interaction term should reflect the precision of the prior for the risk of contracting a disease. I considered

[35] In 2000, ENSA found that in the age group 20 and above the prevalence of diabetes was 7.8% among women and 7.2% among men. 79.5% of women (and 76.4% of men) testing positive for diabetes were already aware of their condition. The prevalence of hypertension is 29% among women (and 32% among men) aged 20 and above. 48% of women (and 31% of men) diagnosed with hypertension were aware of their condition.

separately male-headed and widow-headed households. The results for hypertension and diabetes do not display a pattern consistent with the hypothesis that the two groups differ in the precision of their priors. If I look at cervical cancer screening for both non-poor and poor households, the size of the interaction between the effect of *PROGRESA* and the fraction of poor is systematically bigger for the sample of male-headed compared to widow-headed households. This result would be consistent with the learning model only under the assumption that the prior about cervical cancer risk in male-headed households is less precise than in widow-headed households. The evidence on the socio-demographic characteristics of male- and widow-headed households (Tables AII and AIII) does not support this hypothesis: among others, female respondents in male-headed households are more likely to be educated and to have used contraception than the same type of respondents in widow-headed ones. In Appendix II I provide a further test of the third prediction of the social learning model by using pre-program information on knowledge about contraceptive methods to construct a proxy for the preciseness of the knowledge about cervical cancer. There is no evidence that the size of the interaction term between treatment effects and the fraction of eligibles in the locality changes with knowledge about contraceptive methods (see Table AIV).

In summary, I find little empirical support for the three predictions from the learning model. One possible explanation for this result might be related to the fact that the information received from peers does not add extra content with respect to the information, both written and verbal, that women living in rural Mexico receive as part of the national screening program discussed in Section 2.1.

6 Long-Run Evidence

Here I assess whether the long-run evidence for the effect of *PROGRESA* is consistent with the results based on the randomized evaluation sample. I consider first the social norm mechanism. While the model presented in Section 5 is completely static, it is straightforward to derive its dynamic implications. In localities where the *PROGRESA* program has been in place for longer, there is a higher fraction of women familiar with the PAP test as a result of the program's conditionalities. In the model presented in Section 5.1.1, this corresponds to a lower probability of matching with peers who do not screen.

Throughout the paper I have suggested that husbands' opposition (or simply fear of their opposition) to cervical cancer screening might be related to the gender of the doctor. If *PROGRESA* affected the propensity to screen by weakening the norm related to the possible reaction of husbands to their wives being screened by a male doctor, I should observe the program to have a stronger effect in those localities where there is a higher fraction of male doctors. In a nutshell, the data from the 2007 survey allow me to test three additional implications of the social norm model: a) the exposure to *PROGRESA* should be positively correlated with the propensity to screen for female-specific conditions; b) the probability of being screened by a male doctor should be negatively correlated with the probability of undertaking female-specific tests; c) the effect of the exposure to *PROGRESA* should be stronger in those localities where women have a higher probability of being screened by a male doctor.

I measure the exposure to *PROGRESA* using a dummy variable that takes the value 1 for those localities belonging to the original evaluation sample and 0 for those chosen to act as controls in the 2003 evaluation.[36] The former entered the program in November 1999 or earlier, while the latter received *PROGRESA* in 2004 or later. I use the fraction of male doctors who operate in the locality as proxy for the probability of being visited by a male doctor In early exposure localities, 55% of the doctors on average are male, as opposed to 54% in late exposure localities (see Table 2). The evidence on health center and doctor characteristics presented in Table 2 is suggestive that the localities added in 2003 might be better off in terms of health supply, but for none of the characteristics is the difference between the two groups of localities statistically different from zero. The differences (if any) in health supply characteristics between early and late exposure localities should be associated with higher screening rates in the latter.

In order to test whether longer participation in *PROGRESA* affects the propensity to screen among women aged under 50 and whether the effect varies according to the proportion of male doctors operating in the locality, I estimate two specifications. In the first, presented in the odd numbered columns in Table 9, I regress the decision to screen on the dummy for whether the locality belongs to the original evaluation sample or not. In the second specification, presented in the even numbered columns in Table 9, I add a control for the proportion of male doctors in the locality and allow this variable to interact with the exposure dummy. All the specifications control for the following variables: age (in dummies), marital status, being literate, indigenous, head of household, completing primary and secondary or higher school, number of children still living, working the week before the interview, illness during the four weeks before the interview, and the presence of a television and a radio in the house. The regressions also control for state-fixed effects and for a set of health supply characteristics at the locality level: number of doctors, number of nurses, and total number of families registered with the health providers operating in the locality.[37]

[36] The survey does not report administrative information on the exact date in which each village started receiving the program, thus making it impossible to create a continuous measure of exposure to *PROGRESA*.

[37] In localities with more than one health center I could potentially match each individual with the characteristics of the center they attend. However, the decision to attend a specific center might be driven by characteristics that are correlated with the strength of the social norm. In fact, women who can choose between different providers operating in the same village might decide on the basis of attendance by a female rather than male professional. However, it is unlikely that women would travel to another locality if there is at least one health center where they live.

Table 9 presents the results for the propensity to undertake five screening tests for women aged 18-50: PAP test, mammogram, and tests for hypertension, diabetes and cholesterol. The patterns are similar for the two female-specific screenings. Living in a locality that received *PROGRESA* in 1999 or before (rather than after 2003) significantly increases screening for cervical cancer by 0.14 and for breast cancer by 0.06. I do not find any significant correlation between the exposure dummy and the propensity to screen for hypertension, diabetes and cholesterol. A higher fraction of male doctors in the locality is associated with a significantly lower probability to screen for female-specific conditions. The fraction of male doctors has no effect on the probability that women screen for non-female-specific conditions. If I allow the exposure dummy to interact with the proportion of male doctors, I find that for cervical cancer and breast cancer screening, but not for the other health conditions, the effect of the exposure dummy tends to be significantly stronger in those localities where there is a higher fraction of male doctors. Additional information elicited from the health professionals suggests that the negative association between the fraction of male doctors and the propensity to screen for female-specific conditions is not related to male and female doctors following different practices. The percentages of male doctors who advise their patients to screen for cervical cancer and breast cancer at least once every two years are 82% and 72%, respectively. The same figures for female doctors are 80% and 73%.[38]

When I look at the level of health knowledge of young people, I find that 82.6% of the women aged 14-24 living in localities that received *PROGRESA* before 2000 knew what the PAP test is for, compared to 81.9% of those in localities that received the program in 2004 or later (see Table 10). More generally, the results in Table 10 do not provide clear evidence that the level of knowledge about sex-related issues of young women living in localities that received *PROGRESA* in 1999 or earlier is better than for those living in localities that received it later. When I assess how the fraction of male doctors is correlated with young women's level of knowledge, I find a positive though not significant effect and there is no systematic interaction with exposure to the program (results are available upon request).

The results in this section, although not experimental, support the evidence based on the randomized evaluation sample. Overall, the evidence is consistent with the hypothesis that *PROGRESA* led to an increase in the social acceptability of the screening tests for female-specific conditions. I find less evidence that exposure to the program is positively correlated with better knowledge about cervical cancer.

[38]According to the latest guidelines, Mexican women aged 40-49 should be screened for breast cancer once every two years and once a year after the age of 50.

7 Conclusions

Health interventions can generate spillover effects on the demand for healthcare among individuals who do not belong to the original target group. The failure to take these externalities into account can lead to biased estimates of the benefits and costs associated with health programs. I present evidence from the *PROGRESA* social assistance program that addresses the question of whether including cervical cancer screening among the conditions for the receipt of cash transfers affects the screening decisions of women living in ineligible households. I find that *PROGRESA* has a positive indirect effect on the demand for cervical cancer screening, but not on non-female-specific health outcomes.

I investigated different potential channels through which *PROGRESA* might affect the propensity to screen for female-specific conditions. My results do not seem to be driven by changes in health supply, increased female bargaining power, and income spillovers from eligible to ineligible households. I focused on the role of social norms and lack of information as potential explanations. Male opposition to women being screened by male doctors is often mentioned as one of the reasons for the low take-up of cervical cancer screening among women living in rural Mexico. While I can not completely rule out the alternative explanation of social learning, the weight of the evidence strongly supports the hypothesis that *PROGRESA* has increased the social acceptability of female-specific screening tests.

The findings in this paper have two important policy implications that could affect the design of health programs in both developing and developed countries. First, the design and evaluation of screening programs should take explicit account of potential externalities from eligible to ineligible individuals. Second, cultural barriers need to be addressed explicitly if a program is to be effective. Increasing the proportion of female health professionals in areas with a high proportion of ethnic and religious minorities might increase the incentive for systematic screening for many women. A third policy implication relates to the design of conditional cash transfer programs in poor countries. While health and nutrition courses are mainly addressed to mothers, my results suggest that improving men's awareness about female-specific conditions is essential for facilitating women's access to health services.

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Appendix I: Model of Social Learning

The utility that individual i obtains from screening for a disease j is

$$U_{ij} = g_j + u_{ij} \tag{16}$$

where g_j represents the prevalence of condition j in the population. A higher risk of contracting a certain disease increases the utility from screening for it. u_{ij} is normally distributed - $\sim N(0, \frac{1}{b_j})$ - and represents how individual i differs from the average in terms of the risk of contracting condition j. I assume that g_j and u_{ij} are unobserved and individuals have a prior for the average risk of contracting condition j. I assume that

$$g_j \sim N(\mu_j, \frac{1}{d_j}) \tag{17}$$

where μ_j represents an individual's prior for the prevalence of condition j. d_j is the precision of the prior, which I assume is different across health conditions since the amount of information available to individuals may vary depending on the condition. All the individuals in the village, irrespective of their poverty status, update their prior on the utility from screening for condition j based on feedback from peers. I assume that each individual i has N_i peers. Of these N_i peers, n_{ij} screen for condition j and individual i aggregates these feedbacks to obtain an unbiased estimate of the average risk of contracting condition j. I call this estimate s_{ij} and, following Moretti (2011), it is possible to show:

$$s_{ij} \sim N(g_j, \frac{1}{\gamma^0_{ij}}) \tag{18}$$

where γ^0_{ij} is the precision of the signal that individual i receives from his or her peers before *PROGRESA*. γ^0_{ij} increases as the fraction of peers who screen ($\frac{n_{ij}}{N_i}$) increases.[39] The expected utility from screening for condition j of the representative individual is a weighted average of the prior (μ_j) and the peers feedback (s_{ij}), with the weights reflecting the relative precision of the prior and the signal:

$$E(U_{ij}|\mu_j, s_{ij}) = \omega_{ij}\mu_j + (1 - \omega_{ij})s_{ij} \tag{19}$$

with $\omega_j = \frac{k_j}{(k_j+\gamma^0_{ij})}$ and $k_j = \frac{d_j * b_j}{b_j + d_j}$. Individual i, irrespective of whether she belongs to a poor or a non-poor household, screens for condition j if

$$E(U_{ij}|\mu_j, s_{ij}) \geq q_0 \tag{20}$$

where q_0 represents the cost (both monetary and non-monetary) of screening for condition j. There are two channels through which compliance with *PROGRESA* conditionalities

[39] This property holds under very general assumptions about the model parameters.

affects the expected utility from screening for condition j. First, individuals in poor households have to screen for condition j in order to receive the transfer, which can be modeled as a reduction in the cost of screening ($q_1 < q_0$). As result, a higher fraction of poor women will screen for condition j. Each individual i, irrespective of poverty status, will observe an increase in the fraction of peers who screen. The precision of the signal received through peers' feedback increases ($\gamma_{ij}^1 > \gamma_{ij}^0$) and I assume that the increase in precision on average is the same for poor and non-poor households. This assumption is supported by the evidence provided by Angelucci et al. (2010b) for an important type of network, the family network: on average about 80% of both poor and non-poor households, irrespective of whether they are in treatment or control villages, belong to an extended family network within the same village.

Second, individuals in poor households have to attend health and nutrition classes where they learn about condition j. I assume that each poor individual who attends the classes receives a noisy, idiosyncratic signal about her utility from screening:

$$c_{ij}^P = U_{ij} + \epsilon_{ij} \tag{21}$$

I assume that the signal related to health condition j that individuals in poor household receive from the attendance at classes is unbiased and normally distributed with precision v_{ij}:

$$\epsilon_{ij} \sim N(0, \frac{1}{v_{ij}}) \tag{22}$$

Following the introduction of *PROGRESA*, the expected utility from screening an individual from a poor household is a weighted average of the prior, the signal she receives from her peers and the signal received through attendance at health courses. For non-poor households the expected utility is the weighted average of the prior and the peers' signal. Formally I can write the expected utility from screening for condition j for the representative individual in the group of poor households as follows:

$$E^P(U_{ij}|\mu_j, s_{ij}, c_{ij}^P) = \frac{k_j}{(k_j + \gamma_{ij}^1 + h_{ij})}\mu_j + \frac{\gamma_{ij}^1}{(k_j + \gamma_{ij}^1 + h_{ij})}s_{ij} + \frac{h_{ij}}{(k_j + \gamma_{ij}^1 + h_{ij})}c_{ij}^P \tag{23}$$

where $h_{ij} = \frac{d * v_{ij}}{d + v_{ij}}$.

For the representative individual in the group of non-poor households the average utility can be written as:

$$E^{NP}(U_{ij}|\mu_j, s_{ij}) = \frac{k_j}{(k_j + \gamma_{ij}^1)}\mu_j + \frac{\gamma_{ij}^1}{(k_j + \gamma_{ij}^1)}s_{ij} \tag{24}$$

From equations (23) and (24) it is immediate to derive 3 implications:

1 For any health condition j the weight of social learning, s_{ij}, can be equal to zero only if γ_{ij}^1=0;

2 For each condition j, the weight of social learning for individuals in poor households, $\frac{\gamma_{ij}^1}{(k_j+\gamma_{ij}^1+h_{ij})}$, is smaller than its weight for individuals in non-poor households, $\frac{\gamma_{ij}^1}{(k_j+\gamma_{ij}^1)}$;

3 For each condition j, the weight of social learning should decrease as the precision of the prior (k_j) increases.

Appendix II: Further Test of the Learning Model

I provide further evidence on the third prediction of the social learning model presented in Appendix I using direct questions on knowledge about contraceptive methods to construct a proxy for the preciseness of the knowledge on cervical cancer before the introduction of *PROGRESA*. The March 1998 survey asked female respondents why they were not doing/had never done anything to avoid pregnancies. They were given a list of reasons to choose from:[40] approximately 9% of the female respondents chose "I do not know about contraceptive methods: either how to use or where to obtain them". I construct a dummy variable that takes the value 1 if the respondent has no knowledge about contraceptive methods, and 0 if they used contraception or did not mention lack of information as reason for not using it. This is an imperfect proxy for the level of precision, since women who mentioned reasons other than lack of information for not using contraception might not necessarily be informed. However, among those who indicated lack of knowledge as the explicit reason for not using contraception there would potentially be greater benefit from information received from peers. I estimate equation (15) separately for those households where the female respondent had no knowledge and those where she has at least a little. The top and bottom panels in Table AIV report the results for non-poor and poor households respectively. According to my model, I should expect the coefficient of the interaction term to be significantly bigger for the groups with no knowledge about contraception. Among non-poor households, the coefficient of the interaction term is smaller and not statistically significant for the group with no knowledge than for the group with at least some knowledge. Among poor households, the coefficient of the interaction term is bigger for those with no knowledge, but is statistically not significant. In neither case is the difference between the coefficient for those with no knowledge and those with at least some, statistically significant.

[40] Other choices included: a) partner's or family's opposition; b) having passed the menopause; c) not needed because partner is absent; d) sterility; e) lack of sexual relationship; f) willingness to become pregnant; g) fear of collateral effects; h) breastfeeding; i) other.

Figure 1: The Experimental Design

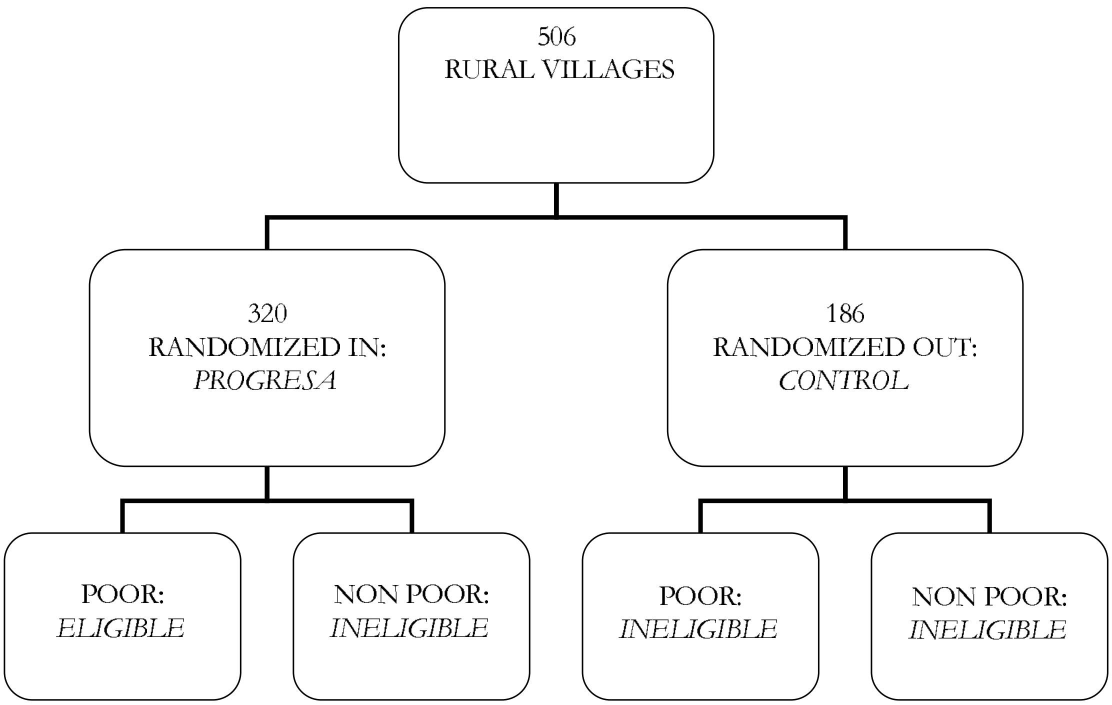

Table 1: Sociodemographic Characteristics by Poverty Status

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Poor Households			Non-Poor Households		
	Mean	SD	**T-C** p-value	Mean	SD	**T-C** p-value
Age Head of Household	44.941	(15.349)	0.849	48.943	(16.147)	0.252
Literacy Head (Y/N)	0.664	(0.473)	0.428	0.721	(0.449)	0.119
Indigenous Head (Y/N)	0.415	(0.493)	0.891	0.253	(0.434)	0.377
Household Wealth Index	6.84	(1.217)	0.498	7.955	(1.374)	0.576
Household Size	5.619	(2.561)	0.505	4.931	(2.545)	0.354
Number of children	2.753	(2.026)	0.825	1.955	(1.848)	0.786
IMSS coverage (Y/N)	0.028	(0.165)	0.206	0.055	(0.227)	0.199
Frac. Educated Women	0.064	(0.219)	0.627	0.112	(0.277)	0.977
Fridge (Y/N)	0.09	(0.286)	0.538	0.227	(0.419)	0.181
Heating (Y/N)	0.207	(0.405)	0.418	0.43	(0.495)	0.439
Land (Y/N)	0.623	(0.485)	0.319	0.639	(0.480)	0.356
Horses	0.321	(0.832)	0.653	0.45	(1.176)	0.566
Donkeys	0.353	(0.852)	0.363	0.388	(1.085)	0.395
Pigs	1.134	(2.813)	0.375	1.183	(3.134)	0.827
Cows	0.842	(3.051)	0.316	1.378	(4.330)	0.937
Number of Pregnancies	5.26	(3.321)	0.65	5.059	(3.541)	0.505
Never Contraception (Y/N)	0.574	(0.494)	0.278	0.518	(0.500)	0.551
Never PAP Test (Y/N)	0.652	(0.476)	0.37	0.556	(0.497)	0.089
FS Index	2.015	(1.296)	0.315	1.852	(1.297)	0.623
Locality Wealth Index	7.133	(0.772)	0.305	7.648	(0.781)	0.636
Locality Population	355.999	(251.863)	0.366	364.488	(259.299)	0.527

Note: The p-values on the differences between Treatment (T) and Control (C) villages are reported from the corresponding OLS regressions allowing standard errors to be clustered by village. All data is taken from October 1997 except for the sexual and female status related information for the female respondent, which are recorded in March 1998. The Female Status (*FS*) index is defined over the range 0-6, where 0 denotes the highest and 6 the lowest level of female emancipation. Village characteristics statistics use one observation per village.

Table 2: The 2007 Characteristics of Women Aged 18-50 by Time of Inclusion in *PROGRESA*

	(1)		(2)		(3)
	PROGRESA Before 2000		*PROGRESA* After 2003		**Difference**
	Mean	SD	Mean	SD	**P-Value**
Age	30.312	(8.503)	30.756	(8.759)	0.020
Literacy (Y/N)	0.775	(0.417)	0.855	(0.352)	0.032
Indigenous (Y/N)	0.294	(0.456)	0.189	(0.392)	0.349
Married (Y/N)	0.528	(0.499)	0.568	(0.495)	0.318
Primary School (Y/N)	0.304	(0.460)	0.355	(0.479)	0.024
Sec. School or Above (Y/N)	0.089	(0.285)	0.140	(0.347)	0.083
Children	3.902	(2.221)	3.799	(2.103)	0.574
Last Week Worked (Y/N)	0.225	(0.417)	0.246	(0.431)	0.543
Sick Last Month (Y/N)	0.168	(0.374)	0.188	(0.391)	0.385
Television (Y/N)	0.791	(0.407)	0.876	(0.330)	0.074
Radio (Y/N)	0.187	(0.390)	0.115	(0.319)	0.002
PC (Y/N)	0.010	(0.099)	0.018	(0.133)	0.181
Refrigerator (Y/N)	0.491	(0.500)	0.640	(0.480)	0.011
Wash Mach. (Y/N)	0.137	(0.344)	0.194	(0.395)	0.119
Horses	1.612	(0.721)	1.721	(1.026)	0.488
Pigs	5.049	(8.096)	3.568	(3.387)	0.041
Cows	3.853	(3.856)	6.336	(7.778)	0.161
Chickens	1.001	(0.039)	1.002	(0.044)	0.824
Number of Doctors	1.214	(1.457)	1.714	(1.383)	0.207
Doctors Tenure (Months)	31.226	(44.867)	41.265	(39.696)	0.414
Doctors Working Days	5.163	(1.239)	5.031	(0.528)	0.542
Number of Nurses	0.929	(0.818)	1.857	(1.994)	0.081
Nurses Working Days	4.494	(1.300)	4.682	(0.560)	0.435
PAP Test Available	0.687	(0.467)	0.786	(0.426)	0.420
Diabetes Test Available	0.702	(0.460)	0.857	(0.363)	0.152
Fraction Males	0.551	(0.493)	0.538	(0.519)	0.938
Doctors Age	33.821	(10.822)	34.692	(10.086)	0.779
Fraction with Postgrad. Studies	0.196	(0.401)	0.385	(0.506)	0.205
Fraction Advised PAP Test	0.711	(0.448)	0.846	(0.376)	0.257
Fraction Advised Mammogram	0.729	(0.439)	0.769	(0.439)	0.763

Note: The sample is restricted to localities with at least one health facility, belonging either to the original evaluation sample or to the sample of those that acted as control group in the 2003 survey. The p-values on the difference are obtained from an OLS regression that allows for standard errors clustered by village. It includes all women in the age group 18-50. Health center and doctor characteristics use one observation per village.

Table 3: Descriptive Evidence on Health Supply

	(1)	(2)	(3)	(4)	(5)	(6)
	October 1997			October 1998		
	Treatment	Control	**Diff**	Treatment	Control	**Diff**
SSA clinic	0.079	0.130	-0.051*	0.097	0.108	-0.010
	(0.271)	(0.338)	(0.028)	(0.297)	(0.311)	(0.028)
IMSS Solid.	0.038	0.043	-0.006	0.028	0.022	0.007
	(0.191)	(0.204)	(0.018)	(0.166)	(0.145)	(0.015)
IMSS	0.003	0.000	0.003	0.003	0.011	-0.008
	(0.056)	(0.000)	(0.004)	(0.056)	(0.103)	(0.007)
Private Doctor	0.000	0.000	-	0.006	0.022	-0.015
	(0.000)	(0.000)	-	(0.079)	(0.145)	(0.010)
Health Aid	0.571	0.641	-0.070	0.633	0.602	0.031
	(0.496)	(0.481)	(0.045)	(0.483)	(0.491)	(0.045)
Mobile Unit	0.769	0.712	0.057	0.809	0.801	0.008
	(0.422)	(0.454)	(0.040)	(0.394)	(0.400)	(0.037)
Any of the providers	0.915	0.914	0.001	0.944	0.941	0.003
	(0.279)	(0.281)	(0.026)	(0.231)	(0.237)	(0.021)
Services available	2.358	2.454	-0.096	3.131	3.065	0.067
	(1.964)	(2.043)	(0.184)	(2.273)	(2.241)	(0.209)

Additional Measures of Health Supply

	March 1998			October 1998		
	Treatment	Control	**Diff**	Treatment	Control	**Diff**
Opening days	5.567	5.512	0.055	5.285	5.349	-0.064
	(0.783)	(0.705)	(0.070)	(0.832)	(0.784)	(0.075)
Opening hours	10.403	10.119	0.284	9.225	9.232	-0.006
	(3.019)	(2.829)	(0.272)	(2.144)	(2.493)	(0.210)
Waiting time	55.871	58.139	-2.268	56.048	58.477	-2.429
	(23.494)	(24.230)	(2.195)	(19.813)	(19.090)	(1.804)
Visit duration	19.151	19.775	-0.623**	19.134	19.157	-0.022
	(3.169)	(3.067)	(0.289)	(3.304)	(3.357)	(0.307)
Visit fee	11.057	11.988	-0.930	5.475	9.769	-4.294***
	(10.021)	(10.166)	(0.931)	(7.035)	(10.730)	(0.792)

Note: *** denotes significance at 1%, ** at 5% and * at 10%. The reported differences are the coefficients from the corresponding OLS regressions that allow standard errors to be clustered by village. Standard deviations are reported in parenthesis. The number of main services available is obtained from a list of 7 services in the locality questionnaire. Measures reported in the bottom panel are averages of the individual responses. Visit durations and waiting times are expressed in minutes. Consultation fees are expressed in pesos at October 1997 values.

Table 4: Descriptive Evidence on Screening Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Cervical Cancer Screening			Diabetes Screening			Hypertension Screening		
	Treatment	Control	**Diff**	Treatment	Control	**Diff**	Treatment	Control	**Diff**
				Poor Households					
Mar-98	0.256	0.274		0.251	0.251		0.392	0.382	
	(0.437)	(0.446)		(0.434)	(0.434)		(0.488)	(0.486)	
May-99	0.598	0.387		0.637	0.421		0.765	0.542	
	(0.490)	(0.487)		(0.481)	(0.494)		(0.424)	(0.498)	
Diff	0.342***	0.112***	0.230***	0.386***	0.170***	0.216***	0.373***	0.160***	0.213***
	(0.017)	(0.015)	(0.023)	(0.017)	(0.016)	(0.023)	(0.015)	(0.017)	(0.023)
Observations		20871			22254			22361	
				Non-Poor Households					
Mar-98	0.319	0.354		0.307	0.299		0.456	0.456	
	(0.466)	(0.478)		(0.461)	(0.458)		(0.498)	(0.498)	
May-99	0.465	0.438		0.545	0.526		0.677	0.649	
	(0.499)	(0.496)		(0.498)	(0.499)		(0.468)	(0.477)	
Diff	0.147***	0.084***	0.063***	0.238***	0.227***	0.011	0.220***	0.193***	0.027
	(0.013)	(0.015)	(0.020)	(0.011)	(0.014)	(0.018)	(0.012)	(0.014)	(0.018)
Observations		18378			20557			20743	

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Standard errors on the differences are derived from an OLS regression and are clustered by village. In March 1998 the questions about screening refer to the previous 12 months. Since in October 1998 and May 1999 they refer to the previous 6 months, the cumulative probabilities in May 1999 are reported. The screening indicator takes the value 1 if at least one household member has been screened.

Table 5: *PROGRESA* and the demand for screening

	(1)	(2)	(3)
	Cervical Cancer Screening	Blood Sugar Screening	Blood Press. Screening
ITE	0.061***	0.010	0.025
	(0.020)	(0.018)	(0.018)
Observations	18291	20459	20645
ATE	0.222***	0.213***	0.211***
	(0.022)	(0.023)	(0.022)
Observations	20726	22093	22198

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Standard errors are clustered by village. All the specifications control for the following sociodemographic characteristics as elicited in the baseline survey: age and literacy of the household head, household poverty index, household size, number of children (in dummies), whether the household is covered by an *IMSS* insurance, the average poverty index for the locality and state fixed effects. Health supply variables are measured both in the baseline and follow-up survey and include the locality average waiting time for being seen by a doctor and a dummy for the presence of at least one health provider in the locality.

Table 6: *PROGRESA* and alternative health outcomes

	(1)	(2)	(3)	(4)
	Health Center Visit	Health Expenditure	Drug Expenditure	Pregnancy Tetanus Vaccination
ITE	-0.006	-0.591	-0.819	-0.066
	(0.019)	(3.199)	(1.558)	(0.069)
Observations	21400	21290	21323	713
ATE	0.168***	-1.187	-2.128*	0.091
	(0.021)	(2.815)	(1.169)	(0.062)
Observations	22883	22791	22813	1148

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Standard errors are clustered by village. The health center visit takes the value 1 if at least one household member visited a health center in the previous six months. The health and drug expenditures are expressed in pesos at October 1997 values. The tetanus vaccination takes the value 1 if the woman received vaccination against tetanus during pregnancy. All the specifications control for the sociodemographic and health supply characteristics described in Table 5.

Table 7: Female Empowerment

	(1)	(2)	(3)
	Expenditure Share Boys' Clothing	Expenditure Share Girls' Clothing	Expenditure Share Alcohol
ITE	0.001	0.000	0.000
	(0.000)	(0.000)	(0.000)
Observations	10678	10678	10618
ATE	0.003***	0.002***	-0.001***
	(0.000)	(0.001)	(0.000)
Observations	11688	11688	11637

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Standard errors are clustered by village. Data on consumption are available only for the follow-up survey. Estimates of ITE and ATE are based on a simple cross-sectional difference. The expenditure shares are defined over the range between 0 and 1. All the specifications control for the sociodemographic and health supply characteristics described in Table 5.

Table 8: Social Norm Test

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
	Cervical Cancer Screening			Blood Sugar Screening			Blood Pressure Screening		
	Full Sample	Male Head	Widow Head	Full Sample	Male Head	Widow Head	Full Sample	Male Head	Widow Head
ITE	-0.008	-0.022	0.114	-0.008	0.004	-0.115	0.001	0.016	-0.045
	(0.040)	(0.041)	(0.079)	(0.030)	(0.030)	(0.076)	(0.034)	(0.035)	(0.077)
Ratio Eligibles	-0.163	-0.193*	-0.094	-0.189**	-0.186**	-0.359*	-0.230***	-0.222***	-0.521**
	(0.099)	(0.106)	(0.166)	(0.076)	(0.076)	(0.201)	(0.075)	(0.075)	(0.238)
ITE*Ratio Eligibles	0.160**	0.184**	-0.103	0.041	0.018	0.151	0.055	0.030	0.117
	(0.072)	(0.075)	(0.145)	(0.060)	(0.060)	(0.148)	(0.068)	(0.071)	(0.154)
Observations	18291	16050	1522	20459	17880	1798	20645	18036	1815
ATE	0.136***	0.129***	0.231*	0.194***	0.185***	0.353***	0.170***	0.159***	0.351***
	(0.048)	(0.048)	(0.117)	(0.046)	(0.046)	(0.114)	(0.042)	(0.042)	(0.105)
Ratio Eligibles	-0.088	-0.093	0.123	-0.099	-0.111	0.164	-0.164**	-0.163**	-0.165
	(0.106)	(0.106)	(0.185)	(0.078)	(0.080)	(0.166)	(0.069)	(0.070)	(0.167)
ATE*Ratio Eligibles	0.142*	0.154*	0.001	0.027	0.045	-0.261	0.062	0.082	-0.191
	(0.082)	(0.083)	(0.168)	(0.080)	(0.078)	(0.176)	(0.072)	(0.071)	(0.149)
Observations	20726	19021	1207	22093	20155	1404	22198	20248	1408

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Standard errors are clustered by village. Ratio elig. represents the fraction of poor households in the locality and is defined over the range between 0 and 1. The full sample includes households headed by women who have never been married as well as male and widow headed ones. All the specifications control for the sociodemographic and health supply characteristics described in Table 5.

Table 9: Female Screening Behavior and Exposure to *PROGRESA*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	PAP Screening		Breast Screening		Blood Pressure Screening		Blood Sugar Screening		Cholesterol Screening	
PROGRESA Before 2000	0.139**	-0.028	0.056***	-0.009	-0.023	0.031	-0.012	-0.043	0.003	0.004
	(0.057)	(0.065)	(0.020)	(0.045)	(0.027)	(0.040)	(0.023)	(0.034)	(0.008)	(0.014)
Frac. Male Doctors		-0.187***		-0.100**		0.089*		0.001		0.025
		(0.066)		(0.049)		(0.045)		(0.042)		(0.023)
Before 2000*Frac. Male Doctors		0.195**		0.125*		-0.080		0.065		-0.011
		(0.076)		(0.066)		(0.052)		(0.047)		(0.023)
Socioeconomic Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Health Supply	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2264	1849	2267	1851	2267	1851	2267	1851	2267	1851

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Standard errors are clustered by village. The sample includes women aged 18-50. The dummy *PROGRESA* Before 2000 takes value 1 for those localities that belonged to the original evaluation sample, 0 for those that acted as control in the 2003 survey. The fraction of male doctors is defined on the range between 0 and 1. The socioeconomic characteristics include dummies for age, marital status, being literate, indigenous, head of household, for completing primary, secondary or higher school, number of kids alive, a dummy for working the weak before the interview, a dummy whether the woman was sick in the last four weeks, whether in the house there is a television and a radio. The health supply characteristics are measured at the locality level and include the number of doctors, the number of nurses, and the total number of families that have registered with the health providers operating in the locality. All the regressions control for state fixed effects.

Table 10: Sex Related Knowledge of Young Women and Exposure to *PROGRESA*

	(1)		(2)		(3)	(4)
	PROGRESA Before 2000		*PROGRESA* After 2003		**Difference**	Observations
	Mean	SD	Mean	SD	**P-Value**	
Do you know what PAP Test is for? (Y/N)	0.826	(0.379)	0.819	(0.385)	0.879	1351
Can a woman get pregnant at the first intercourse? (Y/N)	0.704	(0.457)	0.683	(0.466)	0.599	1456
Is condom an anti-contraceptive method? (Y/N)	0.815	(0.388)	0.793	(0.405)	0.510	1689
Can genital herpes be prevented? (Y/N)	0.889	(0.315)	0.937	(0.245)	0.294	419
Can HIV be transmitted through sexual relations? (Y/N)	0.855	(0.352)	0.819	(0.386)	0.391	1639
Can condom reduce the risk of STDs? (Y/N)	0.810	(0.392)	0.798	(0.402)	0.822	1647

Note: The sample includes women in the age group 14-24. The p-values on the difference are obtained from an OLS regression that allows for standard errors clustered by village.

Table AI: Descriptive Evidence on Screening Rates excluding the *densificados*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Cervical Cancer Screening			Diabetes Screening			Hypertension Screening		
	Treatment	Control	**Diff**	Treatment	Control	**Diff**	Treatment	Control	**Diff**
				Non-Poor Households					
Mar 98	0.327	0.354		0.314	0.299		0.463	0.456	
	(0.469)	(0.478)		(0.464)	(0.458)		(0.499)	(0.498)	
May 99	0.453	0.438		0.534	0.526		0.665	0.649	
	(0.498)	(0.496)		(0.499)	(0.499)		(0.472)	(0.477)	
	0.126***	0.084***	0.041**	0.220***	0.227***	-0.007	0.202***	0.193***	0.009
	(0.013)	(0.015)	(0.020)	(0.012)	(0.014)	(0.018)	(0.013)	(0.014)	(0.019)
Observations	16270			18160			18323		

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Standard errors on the differences are derived from an OLS regression and are clustered by village. In March 1998 the questions about screening refer to the previous 12 months. Since in October 1998 and May 1999 they refer to the previous 6 months, the cumulative probabilities in May 1999 are reported. The screening indicators take the value 1 if at least one household member has been screened.

Table AII: Sociodemographic Characteristics of Poor Households by Gender of the Head of Household

	(1)	(2)	(3)	(4)	(5)	(6)
	Male Head			Widow Head		
	Mean	SD	**T-C** p-value	Mean	SD	**T-C** p-value
Age Head of Household	43.845	(14.930)	0.938	58.732	(14.317)	0.851
Literacy Head (Y/N)	0.697	(0.460)	0.320	0.278	(0.448)	0.903
Indigenous Head (Y/N)	0.418	(0.493)	0.850	0.423	(0.494)	0.572
Household Wealth Index	6.804	(1.210)	0.508	7.249	(1.213)	0.824
Household Size	5.773	(2.522)	0.456	4.131	(2.573)	0.492
Number of children	2.856	(2.018)	0.709	1.661	(1.838)	0.858
IMSS coverage (Y/N)	0.030	(0.171)	0.201	0.005	(0.071)	0.521
Frac. Educated Women	0.066	(0.223)	0.683	0.036	(0.137)	0.406
Fridge (Y/N)	0.091	(0.288)	0.614	0.071	(0.257)	0.162
Heating (Y/N)	0.208	(0.406)	0.596	0.194	(0.396)	0.057
Land (Y/N)	0.633	(0.482)	0.341	0.573	(0.495)	0.368
Horses	0.337	(0.850)	0.614	0.186	(0.615)	0.887
Donkeys	0.364	(0.864)	0.402	0.261	(0.669)	0.991
Pigs	1.144	(2.772)	0.334	1.090	(3.534)	0.999
Cows	0.872	(3.042)	0.437	0.557	(2.656)	0.117
Number of Pregnancies	5.221	(3.294)	0.929	5.769	(3.520)	0.113
Never Contraception (Y/N)	0.569	(0.495)	0.296	0.618	(0.486)	0.672
Never PAP Test (Y/N)	0.656	(0.475)	0.453	0.604	(0.489)	0.624
FS Index	2.039	(1.299)	0.322	1.829	(1.238)	0.868
Locality Wealth Index	7.126	(0.771)	0.345	7.194	(0.769)	0.120
Locality Population	357.832	(253.273)	0.379	344.442	(243.340)	0.354

Note: The p-values on the differences between Treatment and Control localities are reported from the corresponding OLS regressions allowing standard errors to be clustered by village. The samples of Male and Widow headeded households include 10,172 and 785 observations, respectively. All data is taken from October 1997 except for the sexual and female status related information for the female respondent, which are recorded in March 1998. The Female Status (*FS*) index is defined over the range 0-6, where 0 denotes the highest and 6 the lowest level of female emancipation. Village characteristics statistics use one observation per village.

Table AIII: Sociodemographic Characteristics of Non-Poor Households by Gender of the Head of Household

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Male Head			Widow Head		
	Mean	SD	**T-C** p-value	Mean	SD	**T-C** p-value
Age Head of Household	47.806	(15.883)	0.136	62.140	(13.512)	0.330
Literacy Head (Y/N)	0.753	(0.432)	0.295	0.388	(0.488)	0.010
Indigenous Head (Y/N)	0.256	(0.437)	0.384	0.243	(0.429)	0.410
Household Wealth Index	7.924	(1.391)	0.531	8.166	(1.139)	0.928
Household Size	5.123	(2.507)	0.405	3.322	(2.385)	0.107
Number of children	2.055	(1.858)	0.866	1.027	(1.504)	0.105
IMSS coverage (Y/N)	0.059	(0.236)	0.195	0.014	(0.117)	0.431
Frac. Educated Women	0.118	(0.285)	0.864	0.047	(0.158)	0.350
Fridge (Y/N)	0.228	(0.419)	0.173	0.199	(0.400)	0.552
Heating (Y/N)	0.429	(0.495)	0.450	0.398	(0.490)	0.969
Land (Y/N)	0.651	(0.477)	0.385	0.587	(0.493)	0.176
Horses	0.477	(1.204)	0.515	0.272	(1.020)	0.833
Donkeys	0.410	(1.134)	0.368	0.244	(0.618)	0.763
Pigs	1.188	(3.065)	0.869	1.170	(3.719)	0.634
Cows	1.450	(4.455)	0.980	0.863	(3.250)	0.717
Number of Pregnancies	5.006	(3.467)	0.502	6.007	(4.120)	0.608
Never Contraception (Y/N)	0.508	(0.500)	0.458	0.588	(0.492)	0.816
Never PAP Test (Y/N)	0.556	(0.497)	0.103	0.553	(0.497)	0.367
FS Index	1.873	(1.304)	0.683	1.790	(1.243)	0.327
Locality Wealth Index	7.641	(0.780)	0.630	7.673	(0.771)	0.500
Locality Population	366.696	(260.171)	0.559	349.091	(262.885)	0.357

Note: The p-values on the differences between Treatment and Control localities are reported from the corresponding OLS regressions allowing standard errors to be clustered by village. The samples of Male and Widow headeded households include 9,487 and 864 observations, respectively. All data is taken from October 1997 except for the sexual and female status related information for the female respondent, which are recorded in March 1998. The Female Status (*FS*) index is defined over the range 0-6, where 0 denotes the highest and 6 the lowest level of female emancipation. Village characteristics statistics use one observation per village.

Table AIV: Heterogeneity by Knowledge of Contraceptive Methods

	(1)	(2)
	Cervical Cancer Screening	
	Some Knowledge	No Knowledge
ITE	-0.001	-0.045
	(0.042)	(0.129)
Ratio Eligibles	-0.136	-0.318
	(0.101)	(0.249)
ITE*Ratio Eligibles	0.142*	0.236
	(0.077)	(0.214)
Observations	15634	913
ATE	0.120**	0.175*
	(0.050)	(0.097)
Ratio Eligibles	-0.055	0.049
	(0.110)	(0.228)
ATE*Ratio Eligibles	0.145	0.134
	(0.088)	(0.126)
Observations	16722	1945

Note: *** denotes significance at 1%, ** at 5% and * at 10%. Standard errors are clustered by village. Female respondents are classified as having some knowledge of contraceptive methods if they report having used contraceptive methods or they report reasons for not using other than the lack of knowledge. They are classified as having no knowledge if they reported not using contraceptive methods because they did not know either how to use them or where to obtain them. All the specifications control for the sociodemographic and health supply characteristics described in Table 5.