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Policy-induced Social Interactions
and Schooling Decisions

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Policy-induced Social Interactions and Schooling Decisions

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Abstract

This paper considers a conditional cash transfer program targeting poor households in small rural villages and studies the effects of the geographic proximity between villages on individual enrollment decisions. Exploiting variations in the treatment status across contiguous villages generated by the randomized evaluation design, the paper finds that the additional effect stemming from the local density of neighboring recipients amounts to roughly one third of the direct effect of program receipt. Importantly, these spatial externalities are concentrated among children from beneficiary households. This suggests that the intervention has enhanced educational aspirations by triggering social interactions among the targeted population.

Keywords: spatial externalities; social interactions; peer effects; conditional cash transfers.

JEL Codes: C93, D62, I20, O15.

1 Introduction

Demand-side schooling interventions are now widespread in developing countries, and the available empirical evidence suggests that such interventions can have large effects (e.g. Glewwe and Kremer [2006]). Cash subsidies, in particular, have been found to be effective devices for encouraging the human capital investments of poor households (e.g. Parker et al. [2008]; Fiszbein and Schady [2009]). Recent studies have also documented that such programs may positively affect the schooling behaviors of non-participants through non-market, or social, interactions (Bobonis and Finan [2009]; Kremer et al. [2009]; Lalive and Cattaneo [2009]). However, there is still much to learn about those externalities and the related social dynamics prompted by these policies.

The social networks which underlie policy spillovers are notably assumed to be pre-existing, or exogenous, to those interventions. Yet, cash or in-kind transfer programs create many opportunities for information sharing and interactions between beneficiaries, notably women who are the primary recipients of the transfers and regularly encounter during program operations. Moreover the targeting of those interventions implies that participants often have similar socio-economic backgrounds and are thus likely to identify the ones to the others [Akerlof, 1997]. Hence, human capital interventions of this sort are likely to activate social interactions among groups of program beneficiaries and produce externalities that would not occur were individuals treated in isolation.

Assessing and quantifying the implications of these interactions is important from both a policy and an analysis perspective. First, monetary incentives alone may not suffice to overcome the main constraints on schooling decisions. Interventions that spur interactions and in turn affect beneficiaries' preferences and aspirations for their children's education may have larger effects. Second, the strength of those interactions should depend on the density of beneficiaries in a given area. The targeting of a program is thus likely to affect their extent and be crucial for its effectiveness. Third, treatment density will invariably change with the scaling-up of the intervention. Interferences between beneficiaries would thus make it difficult to extrapolate the policy impacts from pilot evaluation studies, which usually investigate small subsets of the eligible population in specific geographic areas.

In this paper, we consider a cash transfer program which targets poor house-

holds in small villages located in rural areas of Mexico and evaluate the effects of the geographic proximity between villages on individual school enrollment decisions. More specifically, we construct a simple empirical framework which allows us to disentangle the direct effects of the incentives produced by receipt of the program from the indirect effects which originate from the density of neighboring participants. We next investigate whether externalities arise in this setting because of program-related social interactions or due to other changes associated with variations in the local density of the program across village neighborhoods.

The policy we consider in this study is the *Progresa* program. Initiated in 1997 and still ongoing, *Progresa* is a large-scale social program that aims to foster the accumulation of human capital in the poorest communities of Mexico by providing cash transfers, which are conditional on specific family members' behavior in the key areas of health and education. Beyond direct economic incentives, two features of this intervention are propitious to enhance beneficiaries' demand for schooling through social interactions. First, the multiple and unbundled components of the program, each of which provides cash transfers conditional on a different household behavior, make partial take-up possible, whereby some households with children not currently attending school take part to the program. Second, high program coverage coupled with the clustering of localities implied by the geographic targeting entails a very high density of beneficiary villages within treated regions. In this setting, the periodic encounters of beneficiaries who live in neighboring villages induced by the program's operations may enhance views and aspirations regarding education among the targeted population and persuade some initially reluctant parents to enroll their children in school.

Exploiting the randomized evaluation design and the clustered spatial distribution of the villages in our sample, we causally identify program externalities across neighboring villages. In each village neighborhood, the allocation of evaluation localities between the treatment and control group is random. These exogenous variations enable us to identify the spillovers induced by the density of program delivery at any distance from the villages in our sample.

We find evidence of a positive and robust effect of the local density of the program on secondary school participation decisions. The magnification effect of the intervention is large, amounting to roughly one third of the direct program

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impact, and the effect is stronger for girls than for boys. Crucially, these spatial externalities appear to be concentrated among children from beneficiary households; there is no evidence of such effects for children in the control group and for those in treated villages who are not eligible to receive the program's benefits. This remarkable heterogeneity sheds some light on the mechanisms behind program externalities. We argue that, while interactions through pre-existing social networks should affect all households that share local resources, social interactions that are restricted to program beneficiaries are likely to be directly spurred by the intervention. To further corroborate this hypothesis, we check that our results are not driven by any other heterogeneity associated with local treatment density, such as variations in program implementation or in the supply of education.

Those findings suggest that the program may have served as a vehicle to spread positive attitudes toward schooling through social interactions, thereby increasing recipients' demand for education. Accordingly, we find that subjective measures of parents' aspirations for their daughters' schooling are positively related to the local density of neighboring participants. This effect also only accrues to eligible households who reside in treated villages.

This paper builds on the empirical literature that seeks to quantify the presence and magnitude of externalities due to social interactions within policy interventions. Manski [2000] and Moffitt [2001] advocate the use of experimental data in order to resolve the identification issues that stem from the endogenous formation of groups of peers and the simultaneous determination of outcomes for individuals and their peers (the "reflection problem"). Accordingly, recent studies have employed random variations in the composition of groups of peers to estimate the influence of group behaviors on individual responses to policies. For instance, Duflo and Saez [2003] study the transmission of information in retirement plan decisions, Miguel and Kremer [2004] consider cross-school externalities of a deworming program on school participation, and Kling et al. [2007] investigate residential neighborhood peer effects within a housing voucher program.

Some authors have studied the effects of externalities on schooling responses to the *Progresa* program by examining the roles of pre-existing social networks. In particular, Bobonis and Finan [2009] and Lalive and Cattaneo [2009] exploit the randomized design to identify spillover effects from eligible to ineligible

children within beneficiary villages. While Angelucci et al. [2010] identify family networks and find that the program raises secondary enrollment only among beneficiary households that are embedded in such a network.

Other recent research puts forward the role of interactions between beneficiaries and program staff as an important channel driving the effects of conditional schooling interventions. More specifically, Macours and Vakis [2009] report that interactions with program leaders increased the aspirations and human capital investments of beneficiaries of a cash transfer program in Nicaragua. Similarly Chiapa et al. [2010] find that the mandated exposure to doctors and nurses increases parental aspirations about children's academic achievements among Progresa beneficiaries.

This paper also relates to studies seeking to understand what features of the design of cash transfer interventions account for the observed outcomes. For instance, Filmer and Schady [2011] find that, in a Cambodian program, modest transfers had a substantial impact on school attendance, while somewhat larger amounts did not raise attendance rates above this level. There seems to be amounting evidence that schooling interventions can have effects through other channels than relaxed liquidity constraints or reduced costs of schooling.

The paper is organized as follows. Section 2 describes the setting of the program, notably the factors of interactions between beneficiaries, and the features of the data which allow us to empirically investigate the presence and effects of those interactions. Section 3 presents the empirical framework we employ to disentangle the impacts which are due to the direct effects of program benefit incentives from the indirect effects which arise from the density of neighboring recipients. Section 4 reports the main findings of program externalities on school participation decisions. In Section 5, we provide some additional evidence that corroborates our interpretation of social interactions between beneficiaries and rules out alternative channels. Section 6 concludes.

2 Non-Market Interactions between Program Beneficiaries

In this section, we first present the program features which are propitious for social interactions between beneficiary households living in neighboring villages.

We then describe our sample and the characteristics of the data which allow us to evaluate empirically how those social interactions affect the schooling responses to the program.

2.1   Program Features

Initiated in 1997 and still ongoing, *Progresa* is a large-scale social program that aims to foster the accumulation of human capital in the poorest communities of Mexico by providing cash transfers, which are conditional on specific family members' behavior in the key areas of nutrition, health, and education.[1] Monetary benefits are channeled through two distinct components. First, the scholarships and school supplies, for children aged less than 17 years, are conditional on regular attendance of one of the four last grades of primary schooling (grades 3 to 6) or one of the three grades of junior secondary schooling (grades 7 to 9). These transfers increase with school grade and are larger for girls than for boys for grades 7 to 9. Second, the fixed-value food stipends are conditional on all family members making regular visits to local health centers for checkups and preventive care. Both transfers are delivered to the female head of the household (usually the mother) on a bimonthly basis after verification of each family member's attendance in the relevant facility (school or health clinic).

The *Progresa* program is targeted both at the village and household levels. During the first years of the program, poor rural households were selected through a centralized process which encompasses three main steps. First, villages are ranked by a composite index of marginality, which is computed using information on socio-economic characteristics and access to the program infrastructures from the censuses of 1990 and 1995.[2] Second, potentially eligible localities were grouped based on geographical proximity, and relatively isolated communities were excluded from the selection process. Third, eligible households were selected using information on covariates of poverty obtained from a field census conducted in each locality before its incorporation into the program.

The program began in 1997 in 6,300 localities with about 300,000 beneficiary households, and expanded rapidly during the following years. In 1998, it was delivered to 34,400 localities (1.6 million households), and in 1999, the

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[1]For more details on *Progresa*, see Skoufias [2001].

[2]Localities with fewer than 50 or more than 2,500 inhabitants were excluded during the first years of the program.

number increased to 48,700 localities (2.3 million households). Geographical expansion in rural areas continued in subsequent years, and by 2001 coverage reached 67,500 localities (3.1 million households). Urban areas were included after that year, and the program has come to cover more than 5 millions households in the following years.

An experimental evaluation of the program was conducted during its phase of geographical expansion in rural areas from 1997 until late 1999. A random sample of 506 villages was drawn from a set of program-eligible localities situated in seven central states of Mexico. Among those villages, 320 localities were randomly assigned to the treatment group and started receiving the program's benefits in March–April 1998. The remaining 186 localities formed the control group and were thus prevented from receiving the program's benefits until November 1999.

2.1.1 Partial Take-Up

The two transfer components are unbundled. Households declared eligible to receive benefits can take up food stipends, scholarships, or both. They can also chose to receive the scholarships for some but not all of their eligible children. Beyond transfer amounts, take-up decisions largely depend on the tightness of the conditions attached to each grant component. While nominally conditional, a substantial fraction of the transfers is *de facto* unconditional. In particular, the conditions attached to the food stipends and scholarships for primary school-level children do not seem to incur a high cost to households.

At the opposite, the transfers conditionality is actually binding for many households whose eligible school-age children would have not gone to school in the absence of the program. The poor might have low educational aspirations, in part because their own experiences and those of their peers can suggest that escaping poverty through the acquisition of education is not a feasible option [Ray, 2006]. Relatedly, parents (and children) might underestimate the actual returns to education since they mainly rely on information on the returns gathered within their own community [Jensen, 2010]. Finally, the opportunity costs of schooling may be too high and the financial incentives provided by the secondary school transfer judged insufficient to modify enrollment decisions. For boys, who are more likely to work for a wage, the secondary school trans-

fer amounts to only around two-thirds of full-time child wage [Schultz, 2004], whereas adolescent girls may be difficult to replace in performing household chores such as the care of younger siblings [Dubois and Rubio-Codina, 2010].

Partial take-up of program benefits is thus likely in this setting, whereby some eligible households comply with the food stipend conditions but do not enroll some or all their children in school. However, once they are incorporated into the program, recipients can further adjust their behaviors by enrolling some of their program-eligible children.

2.1.2 Village Neighborhoods

The dramatic expansion of the program during its first years, coupled with the clustering of localities as implied by the targeting mechanism, means that villages belonging to the evaluation sample (see below) were literally surrounded by other beneficiary localities. In late 1997, there was on average less than one beneficiary locality within walking distance (5 kilometers) from each evaluation village. Due to the scaling-up of the program during that period, this figure had increased to 8.6 by 1998 and to 10.6 by 1999.

These figures suggest that the topography of the area covered by the program consists of village clusters with a quasi-continuum of dwellings, rather than isolated villages. In this context, non-market interactions among neighbors are likely to occur within but also across villages.

The involvement of beneficiaries residing in the same geographic cluster into joint program operations reinforces the likelihood of interactions among beneficiaries of different nearby villages. Two sorts of such operations are noteworthy. First, basic infrastructures are shared by several villages belonging to the same program-incorporated clusters. For instance, only 13 percent of the villages in the evaluation sample have a health clinic. Yet, 68 percent of these localities have access to such a facility within 5 kilometers. Similarly, most localities do not have a junior secondary school (only 17 percent have one in the sample), while 93 percent of them have access to one or more of those schools situated in other villages within 5 kilometers.

Second, transfers are delivered through temporary and mobile outposts located in junction beneficiary localities that serve a number of neighboring communities and further assist beneficiaries by conveying information about the pro-

gram.

In this setting, program beneficiaries are likely to interact in health facilities, transfer collection points, and common meetings, and those non-market interactions can modify the decisions regarding secondary schooling of some households that are already in the program but have not yet taken scholarships and enrolled all their eligible children.

2.2 Data and Sample Description

We employ three of the five rounds of the subsequent evaluation surveys, collected respectively in October 1997 (baseline and first round), October 1998 (third round), and November 1999 (fifth round).[3] The resulting dataset contains detailed information on the socioeconomic characteristics of a panel of households who reside in the evaluation localities. To investigate the effects of the geographic proximity between villages on schooling decisions, we complement the evaluation dataset with information from a census of localities with the exact latitudes and longitudes of rural localities in Mexico, which allows us to identify the geographic location of the evaluation localities.

The evaluation surveys were intended to cover all the inhabitants of the localities under study. However, a small share of the population was not interviewed at baseline and there were some changes in the village populations, so the total number of households observed in the data is 24,077 in October 1997, 25,846 in October 1998, and 26,972 in November 1999. There is also some attrition, as 8.4 percent of the 1997 households cannot be followed and matched in all three rounds of the survey.[4] Because of the non-negligible attrition rate, we do not match individuals in all three rounds of the survey; instead, we consider an unrestricted pooled sample of all valid child observations and use the panel sample only for robustness checks.[5]

At baseline (1997), 60 percent of the households in evaluation localities were

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[3]We have discarded the March 1998 and June 1999 rounds of the surveys in order to avoid seasonal variations in enrollment rates.

[4]Attrition is undoubtedly due in part to migration out of the villages, but it mainly reflects errors in identification codes that occurred for a few enumerators in the second round.

[5]Age limitations on the children reporting in the subsequent surveys, which may make the oldest and youngest groups in the matched panel sample unrepresentative, also contribute to attrition in the sample of children. Our main estimates are nevertheless very similar when we consider the panel sample; see Section 3.

classified as eligible to receive program benefits.[6] We consider the sample of children who live in eligible households, who are less than 16 years old in 1998 and less than 18 years old in 1999 and have completed at least the second and no more than the eighth grade, and are thus eligible to the program. Our main sample contains 23,841 primary school children and 13,992 secondary school children (6,784 girls and 7,198 boys) observed in one or both of the two post-implementation periods (October 1998 or November 1999).

Pre-program school enrollment is high at the primary level. It is 91 percent for both boys and girls, but drops sharply at junior secondary secondary level to 61 percent for boys and 50 percent for girls. Among those who have completed primary school, 33 percent of eligible boys are reported to be working for a wage or in the family business, and 22 percent of girls perform domestic work.[7] Enrollment in secondary school is thus the most problematic decision and, not surprisingly, corresponds to the grade levels at which the program has its greatest impact, especially for girls [Schultz, 2004].

Additional data from the program administrative monitoring allows documenting the take-up of the different components. The take-up of the food stipend is almost complete: 97 percent and 98 percent of eligible households did take those transfers in 1998 and 1999, respectively. Among those beneficiaries, take-up of scholarships amounts to 88 percent for households with only primary school children, but drops to 74 percent for those with only secondary school children.[8] Interestingly, 26 percent of households take up the educational component of the program for some but not all of their program-eligible children.

Those figures suggest that cash grants do not overcome all constraints on school enrollment decisions. Indeed, parents' aspirations toward their children's education might be too low for them to take up the scholarships. Before the intervention, 8 percent of the households expect their daughters to terminate school after completing the primary level and 38 percent after junior secondary.

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[6]About 12 percent of the households were classified as "non-poor" at baseline but were later reclassified as eligible. To avoid arbitrary classifications, we exclude those households from our analysis.

[7]Unfortunately, the sequence of questions on child work is not identical over the three rounds of the survey. Information on domestic work activities was not collected before October 1998.

[8]Logistic and administrative inefficiencies might also have caused some delays in the delivery of scholarships in some areas. See Section 5.

Only 54 percent of parents desire a high education degree (senior secondary and college) for their daughters. The corresponding figures are only slightly higher for boys.

Although this information may internalize some of the constraints on schooling choices, it should at least partly capture parental preferences with respect to education. This is confirmed by basic regressions results (available upon request) showing that parents with lower levels of education tend to place a significantly lower value on their children's academic achievements after controlling for family income and village fixed effects.

Given the proximity in the underlying population of beneficiary villages discussed above, many of the sampled evaluation villages are located very close the ones to the others. Table 1 reports the unconditional and conditional distributions of the numbers of other neighboring evaluation villages within 5, 10 and 20 kilometers.[9] Indeed, 40 percent (80 percent) of the villages in the sample have at least another evaluation locality situated within 5 kilometers (10 kilometers). Among the localities with at least one neighboring evaluation villages, there are on average 1.5 (3.0) other evaluation villages situated within 5 kilometers (10 kilometers), roughly two thirds of which are randomly assigned to the treatment group. While the mass of the distribution is concentrated at one nearby treatment village when considering a radius of 5 kilometers, it shifts to three or more nearby treatment villages when extending the neighborhood to 20 kilometers.

Due to the sampling design, the density of nearby evaluation localities should mirror to a large extent the targeting of the program. To illustrate this, Table 2 presents baseline means and standard deviations of various covariates of poverty across quartiles of the number of evaluation localities in 5-kilometer neighborhoods. The proximity between evaluation villages seems associated with poorer and less educated households as reflected by the lower household income and lower maternal education in the upper quartiles. Villages with more numerous neighboring evaluation villages also have a higher marginality index (the composite index used in the geographic targeting of the program) and are less likely

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[9]We define neighborhoods using only geodesic distances from each evaluation village and do not take into account local geography (natural obstacles or communication axes such as mountains, rivers, or valleys) or transportation networks. This restriction can potentially introduce some measurement error into neighborhood characteristics and generate some attenuation biases in our estimates.

to have a secondary school. Further, secondary schools in areas with several evaluation villages tend to be more crowded as indicated by the higher students-per-teacher and students-per-class ratios.

3 Empirical Strategy

In this section, we first discuss a simple regression framework which allows us to disentangle and quantify the relative importance of the effects stemming from the local density of neighboring recipients, with respect to the direct effects of program receipt. We then develop a test for investigating the mechanisms through which the local density of the program affects schooling responses.

3.1 Direct and Indirect Treatment Effects

Our identification strategy exploits two features of the program evaluation design: the village-level random assignment to treatment and the proximity between evaluation villages. After conditioning on the number of neighboring evaluation localities, the parceling of those assigned to the treatment and control groups is random. This enables us to identify the effects of variations in the density of the treatment on schooling decisions at any given distance from each village.[10]

More specifically, we consider the following linear regression model:

$$y_{i,l} = \alpha_1 T_l + \alpha_2 N_{d,l}^t + \alpha_3 N_{d,l}^e + \epsilon_{i,d}, \tag{1}$$

where $y_{i,l}$ is a school participation indicator variable for child i in locality l and T_l is the randomly assigned treatment indicator which denotes whether locality l receives the program or not. The variables $N_{d,l}^t$ and $N_{d,l}^e$ indicate respectively the number of treated and evaluation localities situated within distance d from locality l. Individual disturbance terms $\epsilon_{i,d}$ are likely to be correlated

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[10]Miguel and Kremer [2004] employ a similar approach to study health externalities across school districts in rural Kenya. Instead of relying on proximity between randomization units, the effects of the local density of program delivery may be identified by randomizing ex ante this density across different evaluation clusters. This area-level randomization is used, for instance, by Crépon et al. [2011] in the context of a workfare program in France. To our knowledge, there is no comparable data collected for social policies in developing countries.

across groupings of neighboring localities, hence we cluster standard errors at the neighborhood level.[11,12]

In this framework, α_1 measures the direct treatment effect of the program while α_2 captures the spillover effect, on school participation, of an additional beneficiary neighboring village. Identification of both parameters stems directly from the randomized evaluation design of the program. In particular, the variations in treatment density in the surroundings of each village l generated by the random treatment assignment assure that the estimator of the $N_{l,d}^t$ term is an unbiased estimate of program externalities across neighboring localities.

Formally, let $y_{i,l}^T$ denote potential outcomes by treatment status T. Consistent estimation of the α_2 parameter relies on the following spatial conditional independence property, implied by the randomized experiment:

$$\mathbb{E}[y_{i,l}^T | N_{l,d}^t, N_{l,d}^e] = \mathbb{E}[y_{i,l}^T | N_{l,d}^e], \ \forall T \in \{0, 1\}. \tag{2}$$

The conditioning term $N_{d,l}^e$ partly captures the effects of unobserved determinants of the school participation decision which are correlated with the program targeting mechanism. As this targeting is correlated with poverty, low parental education and access to more congested schools (see Section 2), we expect the estimate of α_3 to be downward-biased. However, the bias in the $N_{l,d}^e$ coefficient does not contaminate the estimate of the $N_{l,d}^t$ term. In fact, the latter is solely determined by the random selection of treatment and control villages which is, by construction, orthogonal to any observable and unobservable in equation (1).

Note that identification of the effects of program density is local in nature, as the estimate of the α_2 parameter is obtained for the villages that have other evaluation localities in their neighborhoods (see Section 2). Besides, this parameter captures the effects of neighboring evaluation villages that are randomly assigned to the program, and it does not necessarily extend to other program beneficiary localities which are located nearby the villages in our sample.

As a validation test for our identifying assumption (2), we use data from the

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[11]For ease of exposition, in this section we omit background characteristics at the household, village, and neighborhood level. In the estimation, however, we do control for a set of observed characteristics in order to improve precision.

[12]Even though our main dependent variable is dichotomous, we discuss and estimate linear forms for simplicity of interpretation. We use probit estimates as a robustness check; see Table A.2 in the Appendix.

baseline collected in October 1997 and estimate equation (1) using as dependent variables children's schooling outcomes —primary and secondary school enrollment, years of education attained and expected—and some of their determinants at the household level, such as mother's years of education and total per capita income. Table 3 gives the OLS estimation results. As expected from the random design of the evaluation, none of those variables are significantly associated with either the village treatment assignment (T_l) or with the number of nearby treated localities ($N_{d,l}^t$).[13]

3.2 Endogenous versus Contextual Interactions

The next step is to investigate whether spatial externalities arise from interactions that involve only program beneficiaries or from more general externalities of treatment density, such as social interactions within pre-existing networks (e.g., extended families), or changes in local markets (e.g. access to credit) and in the supply of public goods (e.g., learning conditions in local schools). We argue that, while such general externalities are likely to affect households and children of both treatment and control localities, indirect effects restricted to treatment villages should reveal interactions between beneficiaries.

In equation (1), local treatment density is orthogonal to village level treatment, so that the indirect effect of the program can be identified for both treatment and control group villages. This feature of our empirical framework allows us to disentangle whether spatial externalities extend to the entire population or affect exclusively the outcomes of children and families who are included in the program, and thus test, in an indirect way, whether externalities arise because of program-induced social interactions or due to other indirect effects of the local density of program delivery.

More specifically, we evaluate whether the effects on school participation of the local density of the program vary with the village-level treatment assignment. We thus consider the following variant of equation (1):

$$y_{i,l} = \beta_1 T_l + \beta_2 N_{d,l}^t + \beta_3 [N_{d,l}^t \times T_l] + \beta_4 N_{d,l}^e + \beta_5 [N_{d,l}^e \times T_l] + u_{i,d}. \qquad (3)$$

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[13]For consistency with our main estimates, we estimate those placebo regressions using a 5 kilometer radius ($d = 5$). Results (available upon request) are very similar when considering larger radiuses.

where village-level treatment (T_l) is interacted with the number of neighboring treatment localities $(N_{d,l}^t)$, but also with the number of neighboring evaluation localities $(N_{d,l}^e)$. This latter control warrants that conditional randomness holds—see expression (2)—so that the effects of spatial externalities are identified separately for the control and treatment groups. In this equation, consistent estimation of the β_3 coefficient enables us to discriminate among possible explanations underlying the indirect program effect. In particular, contextual effects such as changes in local markets or in public infrastructures due to the program are likely to affect the schooling behaviors of all children, regardless of the treatment status of the village they reside in $(\beta_3 = 0)$. On the contrary, if some of the program operations have shaped interaction networks and generated spillovers, then the local density of the treatment should mainly affect the enrollment responses of beneficiaries $(\beta_3 > 0)$.

A complementary test of the hypothesis that program externalities are restricted to program beneficiaries consists in estimating equation (1) separately for the two groups of children that are respectively eligible and non-eligible to receive the program. Ineligible households and children may differ in other ways from the population targeted by the program, hence the resulting evidence should be interpreted as suggestive. However, the finding of spillover effects restricted to eligible children would corroborate the notion that interactions exclusively involve program beneficiaries.

4   Results

In this section, we present the main findings obtained using the above empirical strategy. We first document the estimates of both the direct and indirect impacts of the program on schooling outcomes. We then report some evidence suggesting that the externalities of the local density of the program are heterogenous and affect exclusively program beneficiaries. Finally, we discuss a series of specification checks.

4.1   Main Estimates

Table 4 reports OLS estimates, for the post-intervention period (1998–1999), of the coefficients of equation (1). In column 1 we consider the sample of primary

school children. We find that living in a treated community increases enrollment rates by 2.6 percentage points. However, there seems to be no additional effects of having an additional program beneficiary village in a 5 kilometer radius. When we focus on the secondary school sample (column 2), consistently with previous evaluation studies (e.g. Schultz [2004]), we find much larger effects of the direct exposure to the program, with a 9.6 percentage points difference between treated and control children. Moreover, we find that a marginal increase in the local density of the program increases secondary school enrollment by 4.6 percentage points. This is a large effect. When normalized in standard deviations of the number of treatment localities in a 5 kilometer radius (0.72 for this sample), this indirect effect accounts for roughly one third (0.34 percent exactly) of the average treatment effect of the program.

In order to shed light on the geographic scope of those spatial externalities, we introduce explanatory variables for the numbers of evaluation and treatment group localities located at a distance between 5 and 10 kilometers in addition to the corresponding variables within a 5-km radius. The results, reported in column 3, reveal no evidence of neighborhood effects over those larger distances. The estimated parameter for the number of treatment group localities situated at 5–10 kilometers is negative and statistically insignificant. This suggests that interactions among program beneficiaries operate within very small areas surrounding households' place of residence.

Cross-village externalities are likely to increase with the share of the local population that receives the program, but this relationship does not need to be linear. To inspect this, we introduce quadratic terms of local treatment densities in our specification. The results, reported in column 4, do not provide support for the presence of non-linearities in our data. The estimated coefficient of the quadratic term for the number of neighboring villages that are randomly assigned to the program is close to zero and not statistically different from zero.[14]

Given the marked pre-program differences in secondary school enrollment rates between boys and girls (see Section 2), we further split the sample by gender. The results, reported in columns 4 and 5, show that the local density of the program boosts the secondary school enrollment of girls, while the effect

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[14]This may be partly due to the small variation in the data beyond one neighboring village (see Table 1).

for boys is positive but not statistically significant.[15]

The estimates of the parameters for the density of nearby evaluation villages are negative and significant for the sample of secondary school children. This provides some evidence of the abovementioned downward bias, due to the geographic targeting of the program, in the estimates of treatment externalities that would be obtained in the absence of an experimental design.

We next examine whether neighborhood externalities take place exclusively among beneficiaries or instead affect the schooling outcomes of both beneficiary and non beneficiary children. Table 5 reports the results for heterogeneity in the effects of the density of the program by village-level treatment status (see equation (3)). We find that program externalities matter only for children who live in treatment group localities (column 1), with a point estimate for children in control group localities that is statistically insignificant and close to zero (column 2). The relative test in column 3 confirms that the effects for the two samples are significantly different from each other at the 10 percent confidence level. The point estimate in column 1 implies a substantial magnification effect of the program. An increase of one standard deviation in the local density of the treatment raises the secondary school enrollment rate of children who live in program villages by 5.4 (0.72×0.075) percentage points.

We also investigate whether externalities affect the schooling behaviors of all children in a treated village, whether eligible to the program or not. The results provided in column 4 do not support this hypothesis: there are no effects of the density of neighboring beneficiaries on the enrollment of non-eligible children. It thus appears that being entitled to receive the treatment, as opposed to simply living in a treated village, is a key factor for the exposure to program externalities in this setting.

4.2   Sensitivity Analysis

The estimates of program externalities we have discussed so far do not take into account the relative population sizes of the neighboring villages. Table 6 reports estimation results for the coefficients of equations (1) and (3) when using the number of program eligible households in neighboring villages as an

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[15]The direct effect of the program is also higher for girls, which is consistent with previous findings (see, e.g., Schultz [2004]).

alternative definition of program density. The results match remarkably well the previous ones in terms of both sign and magnitude across the various samples and specifications. A one-standard deviation increase in the number of neighboring program-eligible households leads to an additional 3.5 percentage points increase in school enrollment rates for the whole sample of secondary school children (column 1) and a 4.7 percentage points increase when we restrict the sample to children who reside in treated villages (column 5).

As mentioned in Section 2, our main sample consists of all observations of program-eligible child of primary and secondary school levels. Since the sample includes children who start their primary schooling during the first post-treatment evaluation period, it is subject to a potential bias due to the dynamic selection into secondary school [Cameron and Heckman, 1998]. For checking the robustness of our results, we reestimate our models using the longitudinal database, thereby selecting our sample based on grade completed at baseline. Table A.1 in the Appendix presents the corresponding estimates, which are very much in line with those obtained from the pooled database.

Given the discrete nature of our dependent variable, one may wonder whether the linear form we have imposed for estimation is the appropriate specification. Table A.2 in the Appendix displays the probit estimates for the marginal effects of the parameters of interest. Both direct and indirect treatment effects appear slightly larger, yet they remain largely consistent with our preferred estimates as discussed previously.

5   Interpretation

Our main estimates show that individual school participation decisions are not only affected by the program's benefits but also respond to the density of neighboring beneficiaries. In addition, we do not find evidence of such program externalities either for children who live in villages assigned to the control group or for children who live in treated villages but are not eligible to receive the program's benefits. This suggests that the program has induced some form of social interactions among the targeted population, thereby further enhancing beneficiaries' demand for schooling for children not yet enrolled.

In what follows, we seek to corroborate this evidence by considering an intermediate outcome which is likely to affect schooling behaviors: aspirations

of future educational attainment. We then discuss additional results which rule
out several alternative explanations for our findings.

5.1 Educational Aspirations

As documented in Section 2, many parents in this setting do not assign much
value to education and withdraw their children from school after primary school
or during junior secondary school. This is particularly true for girls. Parents'
aspirations for their children's school attainment are very heterogeneous among
the targeted population. Before the program started, nearly half of them do not
wish for their children to pursue more than a junior secondary education. This
may explain why some beneficiaries choose not to enroll their children in school
in spite of the cash incentives provided by the program.

Periodic interactions with other beneficiaries in the context of some program-
related operations (i.e. health checks, school meetings, collection of transfers)
may enhance parents' positive feelings about their children's education. In this
sense, learning through social interactions about the positive experiences of suf-
ficiently close others can play an important role in the decision to take up sec-
ondary school scholarships.

We thus evaluate whether parental educational aspirations are sensitive to
the density of the program in village neighborhoods. Table 7 reports the re-
sults. When considering treated and control villages altogether, both direct and
indirect treatment effects are positive and significant only for girls (column 2).
An increase of one standard deviation in the number of treated localities in the
neighborhood increases parents' desired attainments for their daughters by 0.2
years (0.77×0.23), the standard deviation of the number of treatment localities
for the sample of children for which educational aspirations are reported being
0.77 , which corresponds to roughly the same increase in aspirations due to the
direct exposure to the program. In addition, here again, when we split the sam-
ple into treatment and control villages, we find that density matters only for the
schooling aspirations of program-eligible parents who live in treatment villages
(0.35 years), while the corresponding estimate is close to zero for those who
reside in control villages.

These findings suggest that the program has induced some social interactions
that have propagated higher aspirations for girls' education among the targeted

population, thereby further relaxing the conditionality constraint and persuading some initially reluctant parents to enroll their daughters in school.

5.2 Context-Based Interactions

The nonresponse to a higher local density of program delivery of children in control villages might a priori also be explained by some form of complementarity between liquidity constraints and social interactions taking place within existing context-based networks of neighbors or relatives. Accordingly, it could be that all children in our sample were sensitive to the changes, induced by the program, in their neighbors' behaviors, but that those children who do not receive the transfers were unable to adjust their enrollment decisions because of liquidity constraints. This alternative explanation would be consistent with the assumption of credit constraints embedded in the program design and the estimates of its direct impacts. However, this argument is not compatible with the finding that children of households who are not eligible for the program, who are thus less credit constrained, do not respond to the externalities generated by neighboring beneficiaries, as reported in column (4) of Table 5. An additional indirect test for the absence of such a complementarity is provided by the results on educational aspirations. Provided that all individuals positively respond to the schooling behaviors of peers, we should observe some positive neighborhood effects on parental aspirations regarding future schooling among nonbeneficiaries. However, here again, as reported in column (5) of Table 7, we find no evidence of any effects of local treatment density on the educational aspirations of parents residing in control villages.[16]

Another explanation related to existing context-based interactions - which is however, not consistent with the heterogeneous impacts uncovered above - may be that cash injections into the local economy might have altered the functioning of some markets and thereby affected households' constraints and choices beyond schooling [Angelucci and De Giorgi, 2009]. We have inspected this mechanism by evaluating whether program density in treated village neighbor-

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[16]We have also investigated the presence of externalities for relatively richer households in control group villages by splitting the sample according to the distribution of a composite asset index for household wealth, and we have found no evidence of any statistically significant effects on the schooling outcomes of the children of those households (results available upon request).

hoods is associated with a comprehensive set of market-related household variables, including total labor income, a dummy for access to (formal and informal) credit, hours worked by the head of household in his or her main occupation, net sales of agricultural products and working animals, and a composite price index at the village-level based on 36 food commodities. Table A.3 in the Appendix reports the results. There seem to be no statistically significant effects of the local density of the program on these market-related economic outcomes.

5.3 Differences in Program Effectiveness

An alternative interpretation of our findings requires further scrutiny. Areas with more numerous villages assigned to the treatment group might have been better assisted by the program administration—for instance, through prompter delivery of the cash transfers, more skilled local staff, or improvements in the intervention's supply side (e.g., increased school resources), thereby inducing some complementarity between the effects of program receipt and local treatment density.

If this were the case, our estimates would reflect the heterogeneity in program impacts that are (positively) correlated with treatment density across village clusters. In order to evaluate the possibility of differences in program effectiveness across areas, we combine objective and subjective measures of efficiency. First, we use administrative data on transfer payments made during the experimental period to compute the number of months since incorporation after which the first disbursements were made to the localities assigned to the treatment group. While the food stipend was distributed to all villages at the same time in March 1998, there was substantial variation in the delivery of scholarships and school supplies across localities. Thus, only 56 percent of the treated localities received the first scholarship transfer in March 1998, 36 percent received them two months later, and the remaining 8 percent not before six months after incorporation into the program.

Second, we consider the answers elicited from beneficiary households, in March 1999, to a set of survey questions on the perceived quality of program implementation. This survey included the following measures: a dummy variable indicating whether or not eligible children received the form for school attendance monitoring (E1 form), a dummy variable indicating whether or not

the job performed by the local staff member (i.e., *promotora*) was satisfactory, and a composite index intended to capture the overall perceived effectiveness of the program in accomplishing its objectives.

We thus re-estimate equation (1) using these indicators as outcome variables. Table 8 reports the results. As documented in columns 1 and 2, administrative delays seem more frequent in some states, and notably in Queretaro and San Luis Potosi. However, this variation is not related to treatment density in the surroundings of the evaluation villages. From the perspective of beneficiaries, the only outcome that appears to be positively and significantly related to experimental variation in treatment density is receipt of the E1 form.[17]

We further investigate the presence of any effects of treatment density on local schooling conditions, although those effects would likely affect both beneficiaries and non-beneficiaries, and thus seem incompatible with the finding of spillover effects restricted to beneficiaries. For this purpose, we complement the survey information on the quality of the schools attended by beneficiaries with a secondary school census in order to construct objective neighborhood-specific measures of the program's school supply-side component both before and after intervention. Table A.4 in the Appendix displays the results. None of those indicators of local supply of education is statistically significantly related to our measure of program density in village neighborhoods.[18]

6 Conclusions

We have exploited the dense coverage of the *Progresa* program in rural areas and its experimental evaluation design to assess and quantify the effects of the local density of the policy treatment on school participation decisions. We found evidence of a substantial magnification effect of the program: an increase of one standard deviation in the number of beneficiaries in the surroundings of each village increases enrollment rates of treated children by 5.4 percentage points.

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[17]As a further check that program effectiveness is not driving our results, we have re-estimated our school enrollment model while using as an additional control variable receipt of the E1 form. Results (available upon request) are consistent with previous findings on the positive and significant effect of receipt of the E1 form [de Brauw and Hoddinott, 2011]. Yet, the estimated coefficient for the effect of program density remains unchanged.

[18]Because our interest here centers on the difference in supply conditions before and after the program, in these estimates we also control for the values of the education supply indicators at baseline.

However, we found no evidence of externalities of neighboring beneficiaries on the school outcomes of children who do not receive the program.

This striking result that program density affects exclusively children who are included in the program, coupled with complementary evidence of similar externalities on educational aspirations, suggests that the intervention has induced some form of social interactions between beneficiaries residing in neighboring localities. Due to the multiple and unbundled components of the program's design, such intervention-based interactions have apparently encouraged some beneficiaries to take up the educational component of the program.

This evidence of spillovers on beneficiaries can inform policy in two main ways. First, it suggests that interventions can be made more effective by increasing the opportunities for information sharing and interactions between beneficiaries. In this sense, integrated social policies have the potential, by offering some benefits with no or limited conditionality, to increase the take-up of some other components that involve more binding constraints. Second, the targeting mechanism is key for the effectiveness of interventions of this sort. Social multipliers arise when the local number of recipients is sufficiently large, so that the overall effect of the policy will be greater when many households within the same area are included in the program.

From a methodological viewpoint, interactions between neighboring recipients can threaten the extrapolation of policy parameters obtained from evaluation studies that rely on few and isolated units of analysis. Our findings suggest that a more accurate assessment of the impacts of those interventions should seek to capture the externalities that would occur in a broader implementation of the policy and be based on data from geographic clusters of neighboring units.

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Table 1: Village Neighborhoods. Evaluation Sample

	5 km radius (1)	10 km radius (2)	20 km radius (3)
Any Evaluation Locality	0.403 (0.491)	0.791 (0.407)	0.955 (0.208)

Conditional Distributions (for the sub-sample of villages with at least one neighborhing evaluation locality)

	5 km radius (1)	10 km radius (2)	20 km radius (3)
Number of Evaluation Localities	1.451 (0.855)	2.960 (2.323)	7.917 (5.998)
Number of Treatment Localities	0.907 (0.839)	1.890 (1.702)	4.981 (4.054)
One Treatment Locality	0.549 (0.498)	0.405 (0.492)	0.118 (0.323)
Two Treatment Localities	0.113 (0.317)	0.205 (0.404)	0.147 (0.354)
Three or More Treatment Localities	0.034 (0.182)	0.260 (0.439)	0.714 (0.452)

NOTE: This table reports unconditional and conditional means and standard deviations (in parenthesis) for the presence and relative frequency of other neighboring evaluation villages within areas delimited by 5, 10 and 20 kilometers radiuses around each evaluation locality. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Table 2: Baseline Characteristics and Proximity Between Evaluation Villages

	1st Quartile (1)	2nd Quartile (2)	3rd Quartile (3)	4th Quartile (4)
Per-capita household income	219.3 (182.1)	212.7 (206.9)	245.3 (194.7)	204.6 (139.2)
Mother's years of education	1.28 (2.19)	1.57 (2.40)	1.07 (2.11)	1.06 (2.10)
Marginality index	0.371 (0.78)	0.374 (0.75)	0.485 (0.69)	0.671 (0.68)
Secondary school in the village	0.183 (0.39)	0.167 (0.38)	0.137 (0.35)	0.149 (0.36)
Student/teacher in nearby schools	19.9 (6.5)	21.2 (11.1)	23.1 (10.5)	26.1 (8.0)
Student/class in nearby schools	21.4 (8.6)	21.3 (7.3)	23.5 (7.7)	26.1 (7.0)

NOTE: This table reports means and standard deviations (in parenthesis) for various covariates of poverty across quartiles of the distribution of the number of evaluation localities in 5-kilometer neighborhoods around each village in our sample. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Table 3: Placebo Test. Baseline Data

	Enroll Primary (1)	Enroll Secondary (2)	Attainment (3)	Educational Aspirations (4)	Mother Education (5)	PC HH Income (6)
T_l	0.015	0.027	0.005	0.031	0.064	-0.126
	(0.010)	(0.028)	(0.039)	(0.042)	(0.140)	(0.124)
$N_{5,l}^t$	0.005	0.006	0.015	-0.008	0.025	-0.034
	(0.013)	(0.034)	(0.049)	(0.057)	(0.210)	(0.135)
$N_{5,l}^e$	0.012	0.010	0.031	-0.003	0.105	0.003
	(0.010)	(0.025)	(0.044)	(0.041)	(0.158)	(0.090)
Number of Obs	11805	5628	17548	10478	3682	3685
R-squared	0.006	0.025	0.005	0.038	0.047	0.082
Nb of Clusters	380	372	381	375	371	371

* significant at 10%; ** significant at 5%; *** significant at 1%.

NOTE: OLS estimates. Standard errors clustered at the Neighborhood level. The variable T_l denotes the village-level program treatment indicator. The variables $N_{d,l}^t$ and $N_{d,l}^e$ indicate respectively the number of evaluation and treated localities situated within distance d from locality l. State dummies included but not reported. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Table 4: Direct and Indirect Program Impacts on School Enrollment

Sample	Primary School	Secondary School			Girls	Boys
	(1)	(2)	(3)	(4)	(5)	(6)
T_l	0.026***	0.096***	0.089***	0.095***	0.111***	0.082***
	(0.007)	(0.018)	(0.019)	(0.018)	(0.021)	(0.019)
$N_{5,l}^t$	-0.008	0.046**	0.036**	0.059*	0.058**	0.031
	(0.008)	(0.020)	(0.017)	(0.030)	(0.028)	(0.021)
$N_{10-5,l}^t$			-0.017			
			(0.020)			
$(N_{5,l}^t)^2$				-0.007		
				(0.008)		
$N_{5,l}^e$	0.010*	-0.035**	-0.032**	-0.064***	-0.042*	-0.027
	(0.006)	(0.017)	(0.014)	(0.023)	(0.023)	(0.017)
$N_{10-5,l}^e$			0.017			
			(0.014)			
$(N_{5,l}^e)^2$				0.009**		
				(0.004)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	23848	13981	13981	13981	6780	7201
R-squared	0.319	0.264	0.263	0.265	0.260	0.270
Number of Clusters	382	379	188	379	369	371

* significant at 10%; ** significant at 5%; *** significant at 1%.

NOTE: OLS estimates. Standard errors clustered at the neighborhood level. The variable T_l denotes the village-level program treatment indicator. The variables $N_{d,l}^t$ and $N_{d,l}^e$ indicate respectively the number of evaluation and treated localities situated within distance d from locality l. Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities, the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Table 5: Heterogeneous Program Externalities on School Enrollment

Sample	Treated (1)	Control (2)	All (3)	Treated - Non eligibles (4)
$N_{5,l}^t$	0.075***	0.002	-0.002	0.030
	(0.025)	(0.037)	(0.037)	(0.039)
$N_{5,l}^t \times T_l$			0.080*	
			(0.044)	
T_l			0.092***	
			(0.023)	
$N_{5,l}^e$	-0.057**	-0.004	-0.009	-0.038
	(0.022)	(0.028)	(0.029)	(0.032)
$N_{5,l}^e \times T_l$			-0.047	
			(0.037)	
Controls	Yes	Yes	Yes	Yes
Nb of Obs	8807	5174	13981	2381
R-squared	0.289	0.227	0.265	0.302
Nb of Clusters	266	160	379	211

* significant at 10%; ** significant at 5%; *** significant at 1%.

NOTE: OLS estimates. Standard errors clustered at the neighborhood level. The variable T_l denotes the village-level program treatment indicator. The variables $N_{d,l}^t$ and $N_{d,l}^e$ indicate respectively the number of evaluation and treated localities situated within distance d from locality l. Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities and the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Table 6: Eligible Households in Neighboring Villages

Sample	All (1)	All (2)	Girls (3)	Boys (4)	Treated (5)	Control (6)
T_l	0.0975***	0.0977***	0.1127***	0.0831***		
	(0.018)	(0.018)	(0.021)	(0.019)		
$N_{5,l}^t$	0.0016**	0.0016**	0.0022***	0.0011	0.0020***	0.0002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$N_{10-5,l}^t$		-0.0004				
		(0.000)				
$N_{5,l}^e$	-0.0011*	-0.0011*	-0.0015**	-0.0007	-0.0012*	-0.0003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$N_{10-5,l}^e$		0.0003				
		(0.000)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	13981	13981	6780	7201	8807	5174
R-squared	0.265	0.265	0.261	0.270	0.289	0.227
Number of Localities	379	379	369	371	266	160

* significant at 10%; ** significant at 5%; *** significant at 1%.

NOTE: OLS estimates. Standard errors clustered at the locality level. The variable T_l denotes the village-level program treatment indicator. The variables $N_{d,l}^t$ and $N_{d,l}^e$ indicate respectively the number of evaluation and treated program eligible households situated within distance d from locality l. Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of schools and localities, the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Table 7: Parental Aspirations for Educational Attainments

Sample	All	Girls	Boys	Treated	Control
	(1)	(2)	(3)	(4)	(5)
T_l	0.123	0.231**	0.024		
	(0.085)	(0.102)	(0.097)		
$N_{5,l}^t$	0.185*	0.229*	0.141	0.455***	-0.141
	(0.105)	(0.127)	(0.120)	(0.117)	(0.176)
$N_{5,l}^e$	-0.128	-0.174**	-0.089	-0.298***	0.083
	(0.090)	(0.085)	(0.126)	(0.113)	(0.144)
Controls	Yes	Yes	Yes	Yes	Yes
Nb of Obs	8356	3896	4460	5485	2871
R-squared	0.051	0.053	0.050	0.058	0.065
Nb of Clusters	373	352	361	261	157

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTE: OLS estimates. Standard errors clustered at the Neighborhood level. Outcome is expected educational attainment in years. The variable T_l denotes the village-level program treatment indicator. The variables $N_{d,l}^t$ and $N_{d,l}^e$ indicate respectively the number of evaluation and treated localities situated within distance d from locality l. Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities and the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Table 8: Program Effectiveness and Treatment Density

| | Delays in Transfers | | Program | Receipt | Quality of |
| | Scholarship | School Supplies | Effectiveness | of E1 Form | *Promotora* |
	(1)	(2)	(3)	(4)	(5)
$N_{d,l}^t$	-0.222	0.227	0.022	0.040**	0.027
	(0.203)	(0.352)	(0.043)	(0.017)	(0.024)
$N_{d,l}^e$	0.129	-0.433	-0.000	-0.011	-0.020
	(0.195)	(0.396)	(0.029)	(0.016)	(0.022)
Hidalgo	-0.170	-0.112	-0.268***	0.150***	0.112*
	(0.578)	(0.569)	(0.047)	(0.029)	(0.064)
Michoacan	-0.844	-1.002	-0.167***	0.149***	0.200***
	(0.539)	(0.866)	(0.055)	(0.030)	(0.065)
Puebla	0.894*	1.426	-0.177***	0.118***	0.210***
	(0.515)	(1.812)	(0.064)	(0.029)	(0.067)
Queretaro	1.752**	-0.942	-0.110**	0.147***	0.130*
	(0.696)	(0.657)	(0.054)	(0.040)	(0.067)
San Luis Potosi	1.239**	-0.594	-0.047	0.137***	0.085
	(0.530)	(0.399)	(0.044)	(0.029)	(0.071)
Veracruz	-0.633	0.821*	-0.087*	0.141***	0.186***
	(0.569)	(0.435)	(0.051)	(0.032)	(0.059)
Controls	Yes	Yes	Yes	Yes	Yes
Nb of Obs	627	627	6114	4988	5819
R-squared	0.274	0.052	0.062	0.022	0.045
Nb of Clusters	264	264	260	260	260

* significant at 10%; ** significant at 5%; *** significant at 1%.

NOTE: OLS estimates. Standard errors clustered at the Neighborhood level. The variables $N_{d,l}^t$ and $N_{d,l}^e$ indicate respectively the number of evaluation and treated localities situated within distance d from locality l. Excluded category for state dummies is *Guerrero*. Baseline control variables include: distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities and the mean degree of marginalization in the radius. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Appendix - not for publication

Table A.1: Panel Sample

Sample	All	All	Girls	Boys	Treated	Control
	(1)	(2)	(3)	(4)	(5)	(6)
T_l	0.100***	0.098***	0.110***	0.091***		
	(0.017)	(0.017)	(0.021)	(0.020)		
$N_{5,l}^t$	0.042**	0.038*	0.066**	0.015	0.084***	-0.016
	(0.020)	(0.021)	(0.027)	(0.021)	(0.027)	(0.033)
$N_{10-5,l}^t$		-0.012				
		(0.013)				
$N_{5,l}^e$	-0.031*	-0.032*	-0.046**	-0.014	-0.067***	0.018
	(0.017)	(0.017)	(0.021)	(0.017)	(0.025)	(0.024)
$N_{10-5,l}^e$		0.013				
		(0.010)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	11890	11890	5835	6055	7410	4480
R-squared	0.251	0.252	0.244	0.261	0.258	0.240
Number of Localities	367	367	361	356	257	155

* significant at 10%; ** significant at 5%; *** significant at 1%.
NOTE: OLS estimates. Standard errors clustered at the Neighborhood level. The variable T_l denotes the village-level program treatment indicator. The variables $N_{d,l}^t$ and $N_{d,l}^e$ indicate respectively the number of evaluation and treated localities situated within distance d from locality l. Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of schools and localities, the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Table A.2: Probit Estimates

Sample	All	All	Girls	Boys	Treated	Control
	(1)	(2)	(3)	(4)	(5)	(6)
T_l	0.122***	0.119***	0.138***	0.107***		
	(0.023)	(0.022)	(0.027)	(0.025)		
$N^t_{5,l}$	0.054**	0.049*	0.066*	0.040	0.094***	
	(0.025)	(0.027)	(0.034)	(0.027)	(0.030)	
$N^t_{10-5,l}$		-0.017				
		(0.016)				
$N^e_{5,l}$	-0.043**	-0.045**	-0.050*	-0.034	-0.071**	
	(0.021)	(0.021)	(0.028)	(0.021)	(0.028)	
$N^e_{10-5,l}$		0.018				
		(0.012)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Nb of Obs	13981	13981	6780	7201	8807	2451
Pseudo R-squared	0.225	0.226	0.217	0.236	0.253	0.205
Nb of Clusters	379	379	369	371	266	55

* significant at 10%; ** significant at 5%; *** significant at 1%.

NOTE: Probit marginal effects reported. Standard errors clustered at the Neighborhood level. The variable T_l denotes the village-level program treatment indicator. The variables $N^t_{d,l}$ and $N^e_{d,l}$ indicate respectively the number of evaluation and treated localities situated within distance d from locality l. Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of schools and localities, the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Table A.3: Market Interactions in the Neighborhood. Treated Sample

	Labor income (1)	Access to credit (2)	Hours worked per-week (3)	Sales of agri products (4)	Net sales of animals (5)	Aggregate price index (6)
$N_{5,l}^t$	0.061	0.003	0.054	-0.437	0.002	-0.153
	(0.112)	(0.006)	(0.059)	(0.342)	(0.008)	(0.151)
$N_{5,l}^e$	-0.085	0.001	-0.023	0.502	0.003	0.080
	(0.104)	(0.004)	(0.039)	(0.372)	(0.008)	(0.123)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Nb of Obs	14699	14058	5889	5374	10686	13960
R-squared	0.064	0.028	0.050	0.009	0.003	0.101
Nb of Clusters	267	267	267	252	267	267

NOTE: OLS estimates. Standard errors clustered at the Neighborhood level. Outcome is expected educational attainment in years. The variable T_l denotes the village-level program treatment indicator. The variables $N_{d,l}^t$ and $N_{d,l}^e$ indicate respectively the number of evaluation and treated localities situated within distance d from locality l. Baseline control variables include: parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities and the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Sources: Progresa evaluation surveys and geo-referenced census of localities.

Table A.4: School Characteristics in the Neighborhood

	(1) Nb of schools	(2) Children/Class	(3) Children/Teacher	(4) Share Failed	(5) School Index
$N_{d,l}^t$	0.089	0.049	0.356	0.002	-0.012
	(0.056)	(0.432)	(0.455)	(0.003)	(0.042)
$N_{d,l}^e$	-0.072*	-0.212	-0.380	-0.000	-0.023
	(0.038)	(0.383)	(0.341)	(0.004)	(0.033)
Controls	Yes	Yes	Yes	Yes	Yes
Nb of Obs	1012	925	926	926	5024
R-squared	0.942	0.570	0.449	0.567	0.027
Nb of Clusters	383	348	348	348	260

* significant at 10%; ** significant at 5%; *** significant at 1%

NOTE: OLS estimates. Standard errors clustered at the Neighborhood level. The variables $N_{d,l}^t$ and $N_{d,l}^e$ indicate respectively the number of evaluation and treated localities situated within distance d from locality l. Control variables include: the relative school-supply outcome in 1997 (except for column 5), distance to the nearest city, total population in the radius, the mean degree of marginalization of localities and the number of localities in the radius; state dummies and a dummy for year 1998. Sources: Progresa evaluation surveys, geo-referenced census of localities, and Ministry of Education census of secondary schools.