Policy Evaluation in the Presence of Spatial Externalities: Reassessing the Progresa Program

Matteo Bobba, Jérémie Gignoux

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PARIS-JOURDAN SCIENCES ECONOMIQUES
48, BD JOURDAN – E.N.S. – 75014 PARIS
TÉL. : 33(0) 1 43 13 63 00 – FAX : 33 (0) 1 43 13 63 10
www.pse.ens.fr

Policy Evaluation in the Presence of Spatial Externalities: Reassessing the *Progresa* Program*

Matteo Bobba† Jérémie Gignoux‡

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Abstract

This paper considers whether and how the evaluation results of a large-scale social policy in rural Mexico are sensitive to the local concentration of program beneficiaries. Combining evaluation survey data with geo-referenced information on the areas covered by the intervention, we find evidence of positive spillovers across villages on school participation decisions. Those effects occur exclusively between households entitled to receive the program, and they tend to vanish among less adjacent villages and at higher degrees of program density. Analyzing treatment impacts at the level of sufficiently extended geographical areas seems crucial to account for such spatial externalities.

Keywords: spatial externalities; peer effects; SUTVA; policy evaluation; conditional cash transfers.

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†Inter-American Development Bank, 1300 New York Avenue 20577 Washington DC. E-mail: matteob@iadb.org, Tel: +1 202 623 1873.

‡Paris School of Economics (INRA), 48 boulevard Jourdan 75014 Paris France (e-mail: gignoux@pse.ens.fr).

1 Introduction

Evaluation studies increasingly rely on randomized control trials. Implementation constraints usually require working in a small number of selected sites, and hence evaluation units are often spatially close to each other. The presence of spatial interdependence resulting from treatment-induced feedback effects bears different implications for the evaluation according to whether they occur among units sharing or not sharing the same treatment status. As is well known, contamination of the control group can bias estimates of treatment impacts and thereby directly threaten the internal validity of the evaluation results.[1] On the other hand, the existence of interactions among treated units supports the notion that policy impacts are not invariant to the scale of the intervention and hence they may be informative as to the optimal treatment density at which the policy intervention should be conducted.

A trade-off between internal and external validity may thus arise in the evaluation design, depending on context specific factors such as the nature of the intervention as well as the nature of the interactions between the individuals to be treated. For instance, researchers seeking to minimize cross-treatment contamination tend to set up pilot evaluations in controlled environments that feature limited interdependence among subjects at the cost of being potentially less informative about program impacts under large-scale implementation of the policy. At the same time, the spatial proximity of other program recipients in evaluation studies conducted in the midst of the roll-out phase of one intervention may potentially trigger a variety of both market-mediated and non-market externalities (or spillovers) which can affect non-participants' behaviors and thereby contaminate treatment impacts.

The geographic scope of treatment externalities also has practical implications for the design choice of the level of randomization. For example, Miguel and Kremer [2004] found much larger effects of deworming drugs than did earlier evaluations that randomized across individuals. While such spillovers are not necessarily absent when randomizing at larger levels (for example, Miguel and Kremer do show spillover across schools in their samples), they are typically much smaller. This can be an argument for randomizing at a level that captures any large

[1] The absence of interdependence among units resulting from common, site-specific unobservables or feedback effects, or Stable Unit Treatment Value Assumption (SUTVA), is widely invoked in the statistics and econometrics literature (see, e.g., Holland [1986]; Rubin [1986]; Cox [1986]).

spillover effects at the cost of preventing the assessment of the relative contribution of (direct) treatment effects into overall program impacts.[2]

This paper revisits through the lens of spatial externalities the evaluation of one of the most studied policy intervention in developing countries over the last two decades: the *Progresa-Oportunidades* Conditional Cash Transfer (CCT) program (see, e.g., Schultz [2004] and Parker et al. [2008], among others). Spillovers have been examined in the context of *Progresa-Oportunidades* using partial-population designs [Moffitt, 2001], as the program features a selection into treatment at the cluster (village) level but also at the household level using an objective poverty eligibility threshold. Accordingly, Bobonis and Finan [2009] and Lalive and Cattaneo [2009] find evidence of spillovers through peer effects in school enrollment while Angelucci and De Giorgi [2009] and Angelucci et al. [2010] report evidence of transfers within both village and household-level networks.[3]

While informative about the presence of interdependence among individuals within treatment clusters, partial-population designs in the context of *Progresa-Oportunidades* mechanically validate the village-level randomization of the program evaluation.[4] Yet, as in many rural regions of Latin America and elsewhere, 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 paper we examine the extent to which treatment density in broader areas (i.e., village neighborhoods) influenced the responses to the program, and how this affects the interpretation of the estimated treatment impacts. For this purpose, we combine data from the experimental evaluation with information on the geo-referenced locations of the program-beneficiary villages. We then exploit the village-level randomization of program assignment as a source of exogenous variation of the local density of the treatment across village neighborhoods.

[2]To partially overcome this issue, researchers have recently begun to inject experimental variations directly into the intensity of spillover effects by varying the saturation of individuals treated within treated clusters (e.g., Gine and Mansuri [2011]; Crepon et al. [2013]).

[3]Beyond the *Progresa* program and its experimental design, other recent examples from the literature include Duflo and Saez [2003], who examine the take-up of retirement plans within academic departments, and Kuhn et al. [2011] who study spillover effects of lottery winnings within Dutch postal codes.

[4]For instance, Angelucci and De Giorgi write (*AER, 99(1), pag. 487*):

> "[...]we point out that the unit of analysis to evaluate this class of policies is the entire local economy, rather than only the treated. The implication for the design of policy evaluations is that the experimental data should be randomized at the village level, as done in the Progresa evaluation, rather than within a given locality, as is often the case."

We focus our analysis on the secondary school participation decisions of program-eligible children, the primary short-run outcome of the intervention and one key requirement associated to the largest component of the in-cash transfer. We find that the estimated positive school enrollment effects of village-level program treatment encompass some indirect effects of the intervention stemming from the presence of other beneficiary neighboring villages. Positive cross-cluster externalities appear to occur predominantly among treated villages, thereby suggesting that policy impacts are not affected by the contamination of the control group. Yet, existing networks of potential beneficiaries extended across villages seem to have contributed to increasing program take-up and impacts. The presence of other villages assigned to the treatment group within a 5km radius explains about a third of the estimated impacts of the intervention on enrollment rates. This effect tends to vanish when considering larger radiuses (5-10km), thereby suggesting a strong geographic decay of program externalities in this setting.

We then exploit the simultaneous implementation of the program in the areas surrounding the evaluation villages to inspect how marginal externalities vary with the intensity of the treatment. We find that their effect decreases at higher degrees of local program density. Other studies have used experimental variations of treatment density to identify spillovers (e.g., Miguel and Kremer [2004]; Banerjee et al. [2010]; Ichino and Schundeln [2012]), but they were conducted during small-scale interventions, and hence they potentially miss important scale or congestion effects that occur during the full-scale implementation of a program. Our results shed some light on those scaling-up effects by examining spatial externalities in an experimental sample observed in the midst of the roll-out of a large-scale policy intervention.

Some sorts of spatial variations in the delivery of the program among evaluation villages could in principle explain the observed relationship between the local density of the treatment and program impacts. This may occur if, for instance, areas with more evaluation villages benefited from more efficient program operations, or received larger investments in supply infrastructure, thereby helping recipients to comply with the schooling requirements of the program. However, using direct measures of efficiency of program operations or investigating whether spillovers are apparent only within the administrative units that supply social services, we find little support for differential treatment effects in the data.

Instead, further evidence suggests that, in such a context of sparse and coarse knowledge about the benefits of the intervention, information sharing among potential beneficiaries is likely to have played a role, notably among women who are the primary recipients of the transfers and who regularly meet during program operations. In particular, we report that our variation in local treatment density is associated with increased knowledge, among eligible households, about the health and nutrition components of the program (beyond the educational component). In addition, this effect occurs predominantly in village neighborhoods which are homogenous in terms of ethnic composition, a factor that is likely to facilitate social interactions. Our results thus relate to the evidence on "networks effects" on the take-up of social policies, which has been documented notably in the United States (e.g. Bertrand et al. [2000]; Aizer and Currie [2004]). Several of those studies point out the role of information on program participation. In the context of the Food Stamp Program, for instance, Daponte et al. [1999] find that ignorance about the program contributes to non-participation.

Albeit specific to the *Progresa-Oportunidades* setting, this study carries broader implications for similar programs in Latin America or other regions (e.g., Fiszbein and Schady [2009]). In particular, the presence of substantial spatial externalities over small areas surrounding the villages in our sample suggests that the level of randomization should be chosen carefully when conducting impact evaluations and should in some cases comprise extended areas. In terms of policy, those spatial spillovers might be incorporated into the targeting design of social policies to increase program take-up and impacts.

2 Setting and Data

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 both cash and in-kind benefits, which are conditional on specific family members' behavior in the key areas of health, and education. 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). Scholarships increase with school grade and are larger for girls than for boys for grades 7 to 9. The program also brings basic attention to health issues and promotes health care through free preventive interventions, such as nutritional supplements, and education on hygiene and nutrition as well as monetary transfers for the purchase of food. Receipt of food stipends and nutritional supplements are conditional on health care visits to public clinics. Program benefits 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.[5]

These components are only partly bundled. Households can receive the food stipends and the nutritional supplements without claiming the scholarships and school supplies, but not vice-versa. They can also claim the scholarships for some but not all of their eligible school-age children. While the conditions attached to the health and nutrition components do not seem to represent a burden for many households, those related to its educational components are binding for many households whose eligible school-age children would have not gone to school in the absence of the program.[6]

The targeting of the program relies upon a centralized process encompassing three steps. First, rural localities (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.[7] Second, potentially eligible localities were grouped based on geographical proximity, and relatively isolated communities were excluded from the selection process. Third, households were classified as either being eligible or ineligible according to a poverty proxy-means index based on information from the population census of 1990 and 1995.[8]

[5]Overall cash transfer amounts can be substantial: median benefits are 176 Pesos per month (roughly 18 USD in 1998), equivalent to about 28 percent of the monthly income of beneficiary families.

[6]In our sample, take-up rates for the health and nutrition component reach 85 percent over a period of 18 months after the inception of the program. However, only 66 percent of those households with children who completed primary schooling took up the education components. Within-household partial take-up is also common: 26 percent of the scholarship recipient households have one or more eligible school-age children not enrolled in school.

[7]We use, in an interchangeable way, the words locality and village for referring to distinct census-designated rural population clusters, i.e. settlements in which the inhabitants live in neighboring sets of living quarters and that have a name and locally recognized status (incl. hamlets, villages, farms, and other clusters). Rural localities (also called rural communities), or villages, are defined as having less than 2,500 inhabitants.

[8]This index is a weighted average of household income (excluding children), household size, durables, land

The program started 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, coverage increased to 48,700 localities (2.3 million households). An experimental evaluation of the program was conducted during this phase of geographical expansion in rural areas. A random sample of 506 villages stratified by population size 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.

2.2 Village Neighborhoods

In order to characterize the spatial distribution of the intervention, we combine information from the program administration of the localities targeted by the program by the end of 1998 and 1999 with the 2000 population census and the annual school census, featuring geographical coordinates (latitudes and longitudes) for the universe of rural localities and secondary schools, respectively. The geo-referenced data further allows us to identify the respective locations of the evaluation localities.[9]

As in many rural regions of Latin America and elsewhere, the topography of the area covered by the program consists of village clusters with a quasi-continuum of dwellings rather than isolated villages. On average, there are 22 localities with an overall population of roughly 6,400 inhabitants within an area defined by a 5 km radius from each evaluation village. Basic education and health infrastructures serve areas that comprise several neighboring villages. For instance, only 14 percent of the villages in the evaluation sample have a health clinic. Yet, 68 percent of them have access to such a facility within 5 km. Similarly, most localities do not

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and livestock, education, and other physical characteristics of the dwelling. Households were informed that their eligibility status would not change at least until November 1999, irrespective of any variation in household income.

[9] We used official information on the listing of the universe of rural localities receiving the program (broken down by each program's component) at the closing of each fiscal year in 1998 and 1999 to verify which localities were receiving the program in late 1998 and 1999. A small fraction (about 20%) of control localities started receiving the program's food stipends by November 1999, but none of those villages had received any scholarship by that date. We thus continue to treat those observations as belonging to the control group in November 1999.

have a junior secondary school (only 17 percent have one in the evaluation sample), while 93 percent of them have access to one or more of those schools situated in other villages within 5 km.

Figure 1 depicts the geographic scope of the *Progresa* intervention during the first two years of program roll-out in the seven central States of Guerrero, Hidalgo, Queretaro, Michoacan, Puebla, San Luis de Potosi, Veracruz where the program evaluation took place. The rural localities targeted by the program in 1998 and 1999 are shown in light and dark grey respectively while treatment and control localities are reported in red and blue respectively. In order to provide a more in-depth overlook of the areas surrounding evaluation villages, the map features a zoom in view of a region in the State of Michoacan whereby circles of 5 km radius are drawn around each evaluation village.

Both maps reveal that beneficiary and evaluation villages tend to be geographically clustered - with more deprived areas featuring higher program density. Those patterns are confirmed by descriptive statistics of the areas surrounding the evaluation sample, which are shown in Table 1. By the end of 1998, there are on average 10 program-beneficiary localities within an area defined by a 5 km radius around each evaluation village. Those localities have an average total population of 834 children aged 6 to 14, out of which 386 (46%) receive scholarships from *Progresa* (column 1).[10] Moreover, several evaluation villages are indeed located very close the ones to the others. Of the 506 evaluation localities, 139 (27%) have another evaluation locality within 5 km, 57 (11%) have two such localities, and 16 (3%) have three or more. Thus 212 (41%) villages in the experiment have other evaluation villages in a 5 kilometers radius - our analysis will focus on these villages. The density of the program (both non-evaluation and evaluation villages) roughly doubles in neighborhoods with more marginalized localities (columns 2-3). This is consistent with the targeting design of the *Progresa* intervention discussed above. In addition, as expected by the village-level random program assignment among the evaluation localities, there are virtually no differences in the density of the program between

[10]Evaluation villages tend to be less populated than non-evaluation villages (average total population in the two groups is 258 and 338, respectively) while the marginalization index is on average very similar (4.66 vs. 4.72, respectively). Accordingly, there are on average slightly more scholarship recipients in non-evaluation villages (49.2) than in evaluation villages (34.5). This implies that our estimates of marginal externalities should be interpreted as a lower bound.

neighborhoods with treated or control centroids (columns 4-5).

2.3 Sample Description

We combine the geo-referenced locality data mentioned above with three of the five rounds of the evaluation survey, collected respectively in October 1997 (baseline and first round), October 1998 (third round), and November 1999 (fifth round).[11] The resulting dataset contains detailed information on the outcomes of children and socioeconomic characteristics of a panel of households who reside in the evaluation localities.

The evaluation survey was 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 some attrition, due in part to migration out of the villages, and in part to errors in identification codes that occurred for a few enumerators: 8.4 percent of the 1997 households cannot be followed and matched in all three rounds of the survey. Yet, this is unrelated to the treatment assignment.

At baseline (October 1997), 60 percent of the households in evaluation localities were classified as eligible to receive program benefits. In this paper, we study the schooling decisions of the children of those eligible households.[12] Our main outcome of interest is school enrollment, for which we also use, in an interchangeable way, the term "school participation" . It is the answer to the question *Does the child currently attend school?*, which tracks information regarding both enrollment and overall attendance in school (but not regular attendance). Primary school enrollment is almost universal in rural Mexico, while secondary school enrollment is the most problematic decision for school attainment and also the grade levels where *Progresa* has had its greatest impact among eligible children [Schultz, 2004]. We thus restrict our attention to the enrollment decisions of children who, at baseline, are aged less than 18 and have either completed grades 5 or 6 of primary school or the first grade of secondary school.[13] This sam-

[11]We have discarded the March 1998 and June 1999 rounds of the survey because we only have information on the the roll-out of the program at the end of each year.

[12]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.

[13]The sample selection cannot be based on the grade during the follow-up period because this is affected by

ple contains 6,731 children who are making the transition from primary to secondary school, remaining in secondary or dropping out of school during the academic years 1998-1999 and 1999-2000. Of those, 1,616 (12 percent) do not respond to the school participation question in the survey and 3 do not report information on parental education thereby leaving us with a final sample of 11,843 observations. At baseline, the average enrollment rate is 64% (59% for girls and 68% for boys).

3 Program Externalities Across Villages

3.1 Empirical Strategy

Program treatment T_j is administered at the village-level and it is randomly assigned within a relatively small subset of villages which participated to the evaluation of the program. Neighborhoods are defined as concentric circles from each evaluation village using geodesic distances as the radius.[14] In this setting, $N^B_{j,d,t} = N^T_{j,d} + N^K_{j,d,t}$ denotes the number of program beneficiary villages situated at distance d from evaluation village j in a given post-treatment period t. $N^T_{j,d}$ is the number of those which are randomly assigned to the treated group and $N^K_{j,d,t}$ captures the remaining portion of neighboring villages which are targeted by the intervention during each post-treatment period t. As an alternative measure of program density, we use the numbers of scholarship recipient children who reside in neighboring villages. This definition takes into account the process of program targeting within-villages and hence provides a more accurate measure of exposure to spillovers., at the cost of adding some measurement error. As we don't have access to baseline information on program eligible children for each village in our sample, we rely on the number of actual scholarship recipients observed in 1998 and 1999.[15]

Although on average more marginalized regions tend to feature higher program density (see Table 1), there are a variety of other unobserved factors underlying the geographic roll-out of

the treatment.

[14]Due to data limitation, we do not take into account the local geography (natural obstacles or communication axes such as mountains, rivers, or valleys) or transportation networks. This restriction can potentially introduce some measurement error in neighborhood characteristics and generate some attenuation biases in our estimates.

[15]The number of scholarships is potentially endogenous due to migration. To check this within the experimental villages, we have alternatively employed the unrestricted pooled sample of all valid child observations. Results (available upon request) are very similar to the ones obtained with the panel database.

the intervention which are also likely to affect (directly or indirectly) program outcomes. Yet, the random program assignment within a subset of the villages targeted by the intervention can be used as a source of exogenous variation for the density of the program within the geographic areas in which the evaluation took place. Let $N^P_{j,d,t} = N^T_{j,d} + N^C_{j,d} + N^K_{j,d,t}$ denote the number of *potential* program villages situated at distance d from village j in a given post-treatment period t, where $N^C_{j,d}$ is the number of those which are assigned to the control group.[16] Conditional on $N^P_{j,d,t}$, cross-neighborhood variations in the local density of the program are orthogonal to any determinant of individual outcomes:

$$\mathbb{E}[y^n_{i,j,t}|N^B_{j,d,t}, N^P_{j,d,t}] = \mathbb{E}[y^n_{i,j,t}|N^P_{j,d,t}], \tag{1}$$

where $y^n_{i,j,t}$ denotes the potential schooling outcome for child i in village j with a neighborhood treatment density of $N^B_{j,d,t} = n$. Note that the conditional independence assumption (1) does not depend on the treatment status T_j of village j.

As a falsification test for expression (1), we use data from the baseline collected in October 1997 on children's school participation as well as the full set of covariates that we employ in the empirical analysis. Table 2 reports means and standard deviations for those variables (columns 1-2) along with the associated OLS coefficients of the neighborhood treatment density term ($N^B_{j,t}$). In column 3, we display the unconditional marginal effects which reveal the presence of systematic differences in observable characteristics across neighborhoods with different degrees of program density. Consistently with the targeting design of the program, treatment density correlates positively both with the level of deprivation in the centroid village and with the density of villages/population in the neighborhood. As reported in column 4, those differences tend to fade out once we further control for the potential treatment density in the neighborhood ($N^P_{j,t}$).[17]

[16] When we use the number of scholarships received in each neighboring village as an alternative definition of treatment density, we have imputed the corresponding figures for 2000 and 2001 for those villages which are assigned to the control group.

[17] Two of those baseline variables (the share of eligible households and the number of secondary schools) remain marginally statistically associated (at the 10% confidence level) with the density of the program. For consistency with our main estimates we estimate those placebo regressions using a 5km radius ($d = 5$). Results (available upon request) are very similar when considering alternative radiuses.

Motivated by assumption (1), we consider the following linear regression model:

$$Y_{i,j,t} = \alpha_1 T_j + \alpha_2 N^B_{j,d,t} + \alpha_3 N^P_{j,d,t} + \mathbb{X}_{i,j,d,t_0}\alpha_4 + \epsilon_{i,j,d,t}, \tag{2}$$

where $Y_{i,j,t}$ is a dummy equal to one if program-eligible child i in evaluation village j in a given post-treatment period t is going to school and zero otherwise, $\mathbb{X}_{i,j,d,t_0}$ is a vector of baseline characteristics at the individual, household, village and neighborhood level and $\epsilon_{i,j,d}$ captures other unobservable determinants of the school participation decision which are partly correlated with the program's targeting design. The inclusion of $\mathbb{X}_{i,j,d}$ in equation (2) is only meant to increase precision in the estimation. The following control variables are included, all of which are measured at baseline using the 1997 data: child's gender and age (both in levels and squares), 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, total population and the mean degree of marginalization in the neighborhood. We also include state and time dummies.

In order to account for the fact that evaluation villages may belong to multiple neighborhoods, we cluster standard errors for groups of partially overlapping neighborhoods. Those groups are defined as sets of evaluation villages such that each village lies within the radius-based neighborhood of another village of the set. Intuitively, as soon as an evaluation village belongs to two radius-based neighborhoods, those two neighborhoods will belong to the same cluster. This allows for correlations beyond single radiuses. In the empirical analysis, we use two specific values of the radius, 5 and 10 kilometers. Considering a larger radius leads to a smaller number of clusters. In particular, the 506 villages in the experiment belong to 358 clusters of partially overlapping 5-kilometers neighborhoods - the 320 treatment villages belong to 249 such clusters -, and this number reduces to 180 when considering clusters formed by overlapping 10-kilometers neighborhoods.

In this framework, the α_1 coefficient captures both the Intent-to-Treatment effect (ITT) of the program as well as any indirect effect which stems from treatment of individuals in the same village. The α_2 coefficient captures the marginal effect emanating from treatment of program-eligible individuals in one additional neighboring village. Finally, the α_3 coefficient captures any unobserved determinant of the school participation decision which is correlated with the program geographic targeting. As this targeting is partly correlated with local poverty, we expect the estimate of α_3 to be biased downward. Yet, the bias of this coefficient is orthogonal to both the T_j and $N^B_{j,d,t}$ terms and hence it does not contaminate the estimates of the α_1 and α_2 parameters.

The presence of spatial externalities in this setting implies that the α_1 coefficient may also capture a portion of program spillovers across villages insofar as those have a different impact for children who are assigned to the treatment or control group. To inspect this possibility, we consider the following variant of equation (2):

$$Y_{i,j,t} = \beta_1 T_j + \beta_2 N^B_{j,d,t} + \beta_3[T_j \times N^B_{j,d,t}] + \beta_4 N^p_{j,d,t} + \beta_5[T_j \times N^P_{j,d,t}] + \mathbb{X}_{i,j,d,t_0}\beta_6 + u_{i,j,d,t}, \quad (3)$$

where the village-level treatment assignment term (T_j) is interacted with the density of both actual ($N^B_{j,d,t}$) and potential ($N^P_{j,d,t}$) neighboring beneficiary localities so that the effects of cross-village externalities are identified separately for the control and treatment groups. This specification allows to simultaneously test whether program externalities differentially vary with treatment assignment ($\beta_3 \neq 0$), and how netting out the exogenous portion of cross-village externalities affects the conventional difference-in-means estimator of the impact of village-level treatment ($\beta_1 \neq \alpha_1$).

Externalities are likely to vary with the local density of the treatment but this relationship does not need to be linear, as assumed in equations (2)-(3). We thus consider the following partially-linear variant of equation (2):

$$Y_{i,j,t} = \gamma_1 T_j + f(N^B_{j,d,t}) + g(N^P_{j,d,t}) + \mathbb{X}_{i,j,d,t_0}\gamma_2 + e_{i,j,d,t}. \quad (4)$$

The functions f and g can be estimated using standard polynomials or series approximations which are linear in parameters: $f(N^B_{j,d,t}) + g(N^P_{j,d,t}) \cong \sum_{l=1}^{L} \lambda_l \rho_l(N^B_{j,d,t}) + \sum_{l=1}^{L} \delta_l \phi_l(N^P_{j,d,t})$, where $\{\rho_l\}$ and $\{\phi_l\}$ are appropriate basis functions.

3.2 Empirical Evidence

Column 1 of Table 3 reports the OLS estimates of the coefficients α_1 and α_2 of equation (2) for the post-treatment period (1998–1999). Living in a treated community increases school participation by 9.5 percentage points and having an additional treated village within a 5 km radius further increases enrollment rates by 3.6 percentage points. The estimated coefficient for the numbers of treated villages located at a distance between 5 and 10 km is small and statistically insignificant, thereby suggesting the presence of a strong decay rate in spatial externalities. Since we find no evidence of spillovers of beneficiaries over the larger 5 and 10 km distance, we thereafter restrict the analysis to spillovers operating over the shorter 0 to 5 km distance.

To document the heterogeneity of cross-village externalities by treatment status, Column 2 reports estimates of the model specified by equation (3). Program externalities appear to matter only for children who live in treatment group localities, with a point estimate for the coefficient of the interaction term (β_3) which is statistically significant at the 5 percent level. Accordingly, the relative OLS coefficient of the village-level treatment assignment term (β_1) decreases to 8.5 percentage points, thereby revealing that estimated program effects are partly driven by cross-village externalities. As we find no evidence of spillovers affecting school enrollment of children in control villages, we thereafter restrict the analysis to spillovers in treatment villages.

We thus restrict the sample to treatment group localities and evaluate how marginal externalities vary with the local density of the treatment, as depicted by equation (4).[18] We hereby consider the number of scholarship recipients in a 5-km radius as an alternative measure of local treatment density. Column 3 reports OLS coefficients for a quadratic polynomial in the treatment density term which reveals the presence of an inverted U-shape relationship between the local density of scholarship recipients and school participation decisions. As an alternative specification, we generate linear spline functions defined over 3 equally spaced intervals of the support of the local treatment density distribution.[19] Splines are constructed so that the resulting OLS coefficients represent the change in the slope from the preceding interval. Results

[18]Non-linear effects in the control villages were tested but no evidence of such effects was found - the corresponding estimates are available from the authors.

[19]The knots of the linear splines were obtained using the *mkspline* command in Stata 12 and they are located at the values of 899 and 1798 scholarship recipients. Those numbers respectively correspond to the 80th and 97th percentiles of the distribution of scholarship recipients in the 5km neighborhoods,

reported in column 4 supports the notion of decreasing marginal externalities, as confirmed by the negative coefficients of the second and third intervals (although only the coefficient of the second interval is statistically significant due to the limited sample size in the third interval).

As a specification check, we construct treatment density measures over a 20-km radius and weight the observations in each village by the inverse of the distance to the centroid. Results are reported in Table 4 for both measures of treatment density: villages (columns 1-2) and scholarship recipients (columns 3-4). The estimated coefficients are in line with the ones reported in Table 3, in both magnitudes and precision.

4 Further Evidence

We argue that this evidence supports a simple model of peer effects among potential participants in program take-up decisions.[20] Yet, strategic complementarities in the supply-side of the program evaluation may also explain the observed relationship between the local density of the treatment and program impacts. In what follows, we use additional information gathered from both program operational surveys and administrative sources in order to shed some light on the interpretation behind the patterns uncovered in Section 3.

4.1 Peer Effects Among Potential Participants

In spite of the emphasis placed on informing the potential participants about the objectives, design and requirements of the intervention, concerns have been expressed by those involved in the practice of program induction at the locality level regarding the effectiveness of the information-provision process [Adato et al., 2000]. To further corroborate this anecdotal evidence, we use information from an operational follow-up survey collected in treated villages in May 1999 (i.e. 14 months after the inception of the program) in which program beneficiaries were asked to identify the three key components of the benefit package: (i) scholarship and school supplies, (ii) food stipends and nutritional supplements, and (iii) preventive health care

[20] Non-market interactions may affect take-up decisions through two channels: information and social norms. While conceptually different, those two forms of social behaviors can hardly be distinguished empirically. We thus broadly refer to the influence of others on individual responses as peer effects.

and health check-ups. While 98 percent of the respondents were able to correctly and spontaneously mention the nutrition component, only 63 percent identified the health and education components.

In such a context of sparse and coarse knowledge about the benefits of the intervention, information sharing among potential beneficiaries is likely to have played a role, notably among women who are the primary recipients of the transfers and regularly encounter during program operations. When asked to mention the most significant changes in their communities in the operational follow-up survey, half of the beneficiaries report that the program has increased the degree of cooperation among women.

We estimate marginal treatment externalities on those proxy variables of recipients' knowledge about the three program components. Table 5 reports the associated OLS coefficients. Having one additional neighboring program village increases by 4.9 percentage points the share of recipients who are aware about the education component (column 1), by 7.8 percentage points the share of those who are aware about the health component (column 2) and by 1.2 percent the corresponding share for the nutritional component (column 3) - although the latter coefficient is not statistically different from zero.

Given the evidence of program externalities on school participation decisions presented in Section 3, the effect on the knowledge of the education component does not necessarily reflect the presence of peer effects among potential beneficiaries. Yet, the observed increase in the knowledge indicators of the other program components (health and nutrition) makes it difficult to interpret this evidence as purely stemming by corresponding variations in school enrollment among program-eligible children.

We then partition our measure of program density according to one household characteristic that is likely to identify social networks among individuals living in the same areas. We define a proxy of ethnic affiliation according to whether the household head solely speaks an indigenous language or not, and construct the relative shares of indigenous and non-indigenous program-eligible households who reside in nearby villages. Columns 4-6 of Table 5 report evidence of heterogeneous effects in program externalities along those ethnolinguistic clusters within village neighborhoods on the knowledge indicators of the health and nutrition component.[21]

The results reported in Table 5 provide suggestive evidence that social interactions and information sharing on the program components, among eligible households, are driving at least partially the spatial spillovers we observe. While we cannot report direct evidence of this - as we don't have measures of the occurrences of interactions of beneficiaries from different neighboring villages - spatial spillovers are associated with improved knowledge of the components of the program and those effects increase when the other beneficiaries in neighboring villages belong to the same linguistic groups (a factor that should facilitate social interactions).

4.2 Supply-side Effects

Areas with more numerous villages receiving the program benefits might have benefited from more efficient program operations, or received larger investments in supply infrastructure, thereby helping neighboring recipients to comply with the schooling requirements of the program. While this notion of implementation scale gains seems a priori reasonable, we argue that it is unlikely to explain our results. In fact, we identify spatial spillovers using only the variation in program density generated by the randomized experiment. This variation is small compared to the overall scale of the program (as seen in Table 1, there are on average 10 beneficiary villages in a 5 km-radius but only 0.6 evaluation villages, among which 0.4 are assigned to the treatment group and 0.2 to the control group). Thus program density is not very different around villages which have more treatment-group villages than around those which have more control-group evaluation villages in their neighborhood and hence the resulting infra-marginal changes in the scale of the program are unlikely to trigger any supply-side efficiency gain.

A more plausible explanation hinges upon the presumption that program implementation practices may have differed between the (treated) villages which took part to the evaluation and the rest of program-beneficiary villages. In this case, our estimates could partly capture cross-neighborhood differences in either the intervention's effectiveness or school supply responses to the local density of evaluation villages. Yet, we find little support for this interpretation

[21] In our sample, 7.3 percent of households are reported being indigenous. We don't have information on the specific indigenous group to which each household belongs to and hence this measure is clearly a coarse indicator of ethnic affiliation.

in the data. First, any variation in program delivery in a given geographic area should benefit evenly those program recipients who reside in that area. This is at odd with the evidence reported in column 1 of Table 3 in which program externalities appear spatially concentrated within relatively small areas surrounding the evaluation villages. Second, if cross-village externalities are mainly driven by supply-side factors, then we should not observe any systematic difference across characteristics that are predictors of social interactions among potential participants without altering the local scale of the program. Yet, the evidence presented above (see Table 5) suggests that cross-village externalities appear to vary according to the ethnic composition in village neighborhoods. Third, the federal resources for health and education services are independent of the program's budget, and they are transferred to local governments (municipalities) through separate channels.[22] Hence, cross-neighborhood variations in school participation decisions driven by local supply improvements should be partially diluted across municipalities so long as the associated supply shocks are not perfectly correlated. For checking this, we split village neighborhoods according to whether they cross or not some municipal boundary, and estimate treatment externalities on school participation in the two cases. As shown in column 1 of Table 6, the program externalities do not seem to systematically vary along this dimension. If anything, they seem larger for those village neighborhoods which share an administrative border - although the difference between the two OLS coefficients is not statistically significant.

We next run a battery of complementary tests aimed at detecting the presence of supply-side responses associated with experimental variations in the density of the treatment in the surroundings of the evaluation villages. According to qualitative interviews with beneficiaries, local program staff, school teachers and health staff (see Adato et al. [2000]), one major source of inefficiency in program delivery were the observed delays in the delivery of the form for school attendance monitoring (E1 form) and the associated delays in the payment of the scholarships. We use program administrative data on the monetary transfers delivered to eligible households in order to compute the number of months since incorporation after which the first

[22]The federal government also provides health services in marginalized rural communities though the Social Security Institute (IMSS). Yet, the bulk of educational and health services are provided by local governments [Levy, 2006].

disbursements were made to the localities assigned to the treatment group. While food stipends were distributed to all villages assigned to the treatment group at the same time in March 1998, only 56 percent of the those 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. We further use information from the operational follow-up survey on whether program recipients received the E1 form or not. As of May 1999, 24 percent of beneficiaries report not having received such a form. As documented in columns 2-4 of Table 6, this variation in program implementation seems unrelated to our measure of treatment density in the surroundings of the evaluation villages.[23]

We finally investigate the presence of any effects of treatment density on the delivery of education services, although those would likely affect the enrollment decisions of both recipients and non-recipients, and are thus difficult to reconcile with the evidence of heterogeneous externalities reported in column 2 of Table 3. Columns 5-6 of Table 6 display the resulting estimates for the relative density of schools and teachers located within 5-km from each evaluation village during the post-intervention period (1998-1999). None of those factors seem related to our measure of treatment density.

While each piece of evidence presented in this Section may not be sufficient to rule-out the role of supply-side factors and to establish the notion of peer effects in program take-up decisions, the weight of the evidence makes it difficult to interpret our findings as solely driven by program implementation differences.

5 Conclusion

In the context of a large-scale social policy implemented in rural Mexico we documented whether the presence of other beneficiaries in areas comprising several villages influences the schooling outcomes of the participants to its experimental evaluation, and how those spatial spillovers affect the interpretation of the evaluation results. Combining evaluation survey data with geo-referenced information on the area covered by the intervention, we found evidence of

[23]Those administrative delays appear concentrated in some regions, and notably in the States of *Queretaro* and *San Luis Potosi*. As a further check, we have re-estimated equation 2 without those two States. Results (available upon request) are very similar to those reported in Table 3.

positive spillover effects across villages. Higher treatment densities in 5-km village neighborhoods increased the take-up of the scholarships for secondary schooling offered by the program and enrollment rates at that level. Those indirect treatment impacts occur predominantly among treated villages, suggesting that the large effects of the program on secondary school enrollment decisions are driven in part by cross-village spillovers. According to our estimates, the effect of an additional treatment group village in a 5km neighborhood is associated with an increase of 9.5 percentage points in secondary school enrollment of children in treated villages, which is comparable to the direct effect of own-village treatment (see column 1 of Table 3). These marginal effects stem from variations in program density induced by the experimental design of the evaluation and hence they cannot be extrapolated to recover the overall externalities of the intervention as a whole. In addition, we provide evidence that spatial spillover effects are decreasing with treatment intensity so the total effect of all beneficiary villages in the neighborhood is certainly less than the product of that marginal effect times the local program density.

These findings suggest that the large effects of the program on secondary school enrollment decisions are driven in part by cross-village spillovers. They are consistent with the results reported in Angelucci et al. [2010] and Angelucci et al. [2012], whereby *Progresa* transfers are shared among program-eligible family members but not among non-eligible members. However, while those studies have restricted the analysis to spillovers within villages assuming *a priori* that those do not operate at a larger level, we find that spillovers extend across neighboring villages.

We did not find evidence in favor of differential treatment impacts due to spatial variations in the implementation of the program evaluation. In particular, we found no effects on measures of efficiency of program operations, and rejected that the effects we find are restricted to neighborhoods comprised within the same municipalities, which are the administrative units in charge of delivering social services. At the opposite, we found evidence that spatial spillovers stem from interactions occurring within networks of beneficiaries spanning across villages, affecting the take-up of the scholarship component of the program. In particular, we find that higher treatment density in village neighborhoods are associated with increased knowledge, among eligible households, about the different components of the program, and spillovers occur predominantly in village neighborhoods which are homogenous in terms of ethnic composition, a factor that facilitates cross-village interactions. Some related evidence of social interactions affecting the take-up of social policies was reported in the different context of urban areas of the United States (e.g. Bertrand et al. [2000]; Aizer and Currie [2004]). A more in-depth analysis of the mechanisms at play would nevertheless require data on social networks (e.g. Banerjee et al. [2012]).

Our results suggest that the program leveraged the spatial spillovers generated by its geographic targeting in rural areas so as to increase schooling impacts. This has implications both for the implementation and the evaluation of social policies. First, we show that treatment impacts at the level of larger geographical areas allows recovering policy parameters that internalize spatial spillovers. Second, albeit specific to the *Progresa-Oportunidades* setting, the finding of non-linear marginal externalities may inform the current debate on the extrapolation of treatment impacts obtained under a small-scale experiment to those that would obtain with a fully scaled-up policy (e.g. Banerjee and Duflo [2009], Deaton [2010]). Third, from the policy perspective our findings suggest that there can be rather large gains from the spatial concentration of program beneficiaries insofar as social networks might propagate information about the program and enhance the take-up of social benefits.

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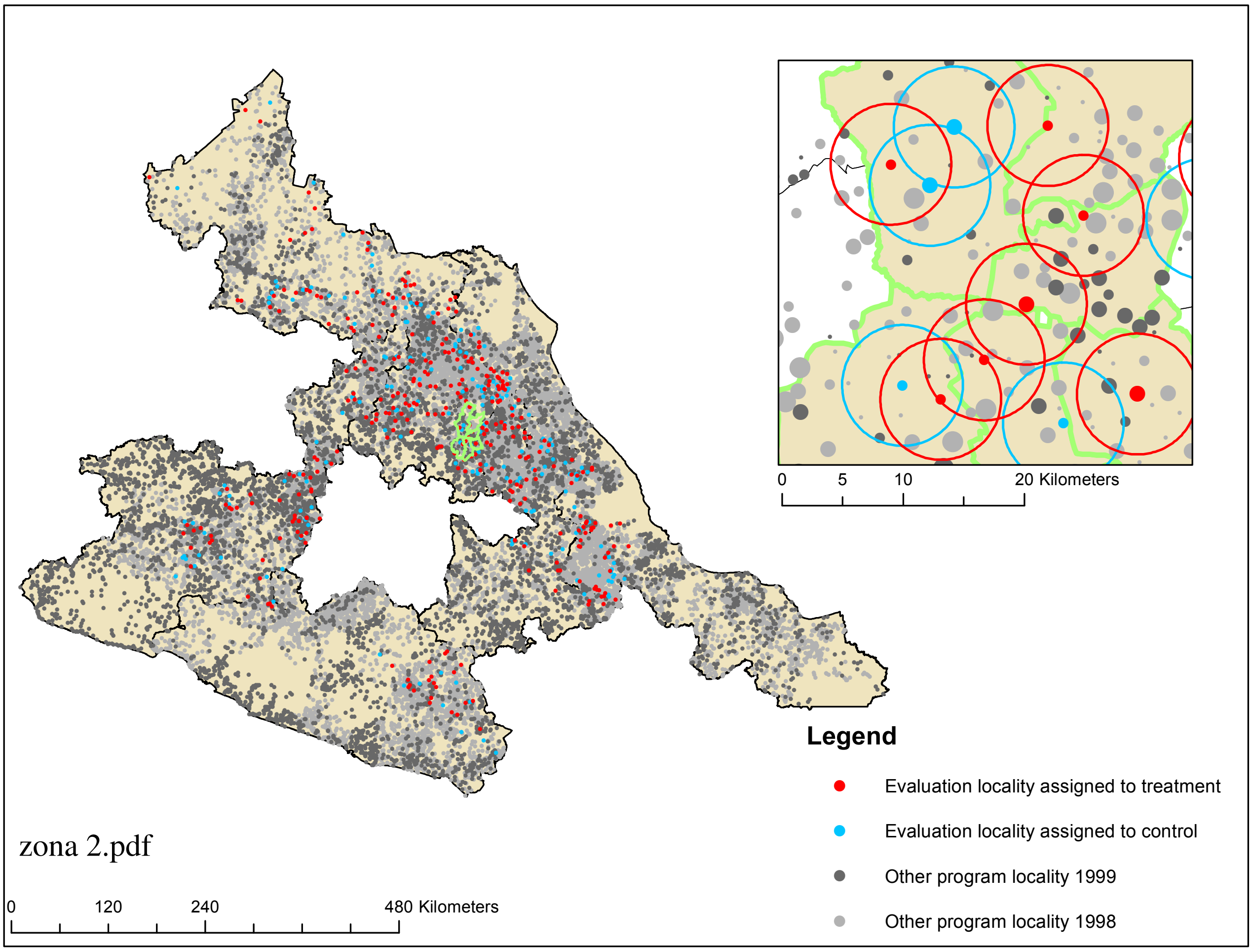
Figure 1: Program Coverage (1998-1999)



NOTE: This map reports the geographic locations of the villages targeted by the program during the period 1998-1999 in the seven central States of Mexico in which the evaluation of the program took place. The quadrant in the up-right corner displays a close-up view of a region in the State of Michoacan in which the size of the markers has been adjusted for the relative population size and 5km radiuses are displayed around each evaluation village.

Table 1: Treatment Density around Evaluation Villages

Sample	All	Neighborhood Poverty		Treat Assignment	
		Low	High	Treat	Control
	(1)	(2)	(3)	(4)	(5)
Numbers of beneficiaries in neighborhood					
# Beneficiary villages	10.0	6.66	13.3	10.2	9.64
	(8.13)	(5.07)	(9.19)	(8.20)	(8.04)
# Children in beneficiary villages	834	565	1104	831	841
	(864)	(641)	(968)	(839)	(908)
# Scholarship recipients	386	252	520	385	386
	(402)	(283)	(455)	(385)	(430)
Distribution of evaluation villages in neighborhood					
Prob(1 evaluation village)	0.27	0.24	0.31	0.28	0.26
	(0.45)	(0.43)	(0.46)	(0.45)	(0.44)
Prob(2 eval. villages)	0.11	0.09	0.13	0.11	0.12
	(0.32)	(0.29)	(0.34)	(0.31)	(0.33)
Prob(3+ eval. villages)	0.03	0.01	0.06	0.03	0.03
	(0.17)	(0.06)	(0.24)	(0.17)	(0.18)
Total Villages in Evaluation Sample	506	253	253	320	186

NOTE: This table reports, using data for October 1998, means and standard deviations (in parenthesis) for the numbers of neighboring beneficiary villages, children (aged 6-14) in those villages and scholarship recipients, and the distribution of neighboring evaluation villages within areas delimited by 5 kilometers radius around evaluation localities. In columns 2-3 we split the sample of evaluation localities according to the median of the average index of marginalization in the neighborhood. In columns 4-5 we spit the sample according to the program treatment assignment indicator of the village situated in the centroid of each neighborhood.

Table 2: Baseline Characteristics

	Mean	Std. Dev.	OLS Coefficient of Neighborhood Treatment Density term ($N^b_{j,5}$)	
	(1)	(2)	(3)	(4)
School Enrollment	0.638	0.480	-0.0003	0.0191
			(0.0008)	(0.0227)
		Individual and HH Characteristics		
Age	14.53	2.035	0.0009	-0.034
			(0.003)	(0.061)
Female	0.503	0.500	-0.0007	0.007
			(0.0005)	(0.012)
Mother Education (years)	2.222	2.249	-0.0189	-0.0104
			(0.006)***	(0.137)
		Centroid Village Characteristics		
Share of Program Eligible HHs	0.591	0.193	0.004	0.037
			(0.001)***	(0.020)*
Secondary School (dummy)	0.252	0.434	-0.004	0.014
			(0.002)**	(0.050)
Distance to Nearest City	104.8	42.84	-0.260	0.907
			(0.199)	(4.61)
		Neighborhood (radius=5km) Characteristics		
Number of Secondary Schools	3.018	2.072	0.097	-0.432
			(0.013)***	(0.242)*
Mean Index of Marginalization	4.379	0.734	0.013	0.030
			(0.003)***	(0.071)
Number of Villages	22.85	13.00	0.554	0.0836
			(0.067)***	(1.22)
Population Density (thous)	7050.2	9095.5	0.433	0.22
			(0.77)***	(0.67)

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

NOTES: This table reports means and standard deviations (columns 1-2) of the the school participation (enrollment) outcome at baseline (October 1997) as well as the full set of covariates the we employ in the empirical analysis. In columns 3-4, we display the OLS coefficient of the neighborhood treatment density term (radius=5km) on each of those baseline characteristics, respectively without and with its potential counterpart as a conditioning term. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parenthesis below each OLS coefficients.

Table 3: Spatial Externalities of the Program on School Enrollment

Sample	All Villages		Treated Villages	
	(1)	(2)	(3)	(4)
Own Village Treated	0.095***	0.085**		
	(0.020)	(0.033)		
# Treated Villages in 0-5km	0.036**	-0.018		
	(0.018)	(0.028)		
# Treated Villages in 10-5km	-0.010			
	(0.018)			
(# Treated Villages in 0-5km)×Treat		0.095**		
		(0.041)		
# Scholarship recip. in 0-5km			0.0025***	
			(0.0007)	
(# Scholarship recip. in 0-5km)$^2\times$ 1000			-0.0009**	
			(0.0003)	
# Scholarship recip. in 0-5km - Spline 1				0.0016***
				(0.0006)
# Scholarship recip. in 0-5km - Spline 2				-0.0013**
				(0.0005)
# Scholarship recip. in 0-5km - Spline 3				-0.0020
				(0.0037)
Number of Obs	11843	11843	7364	7364
R-squared	0.259	0.260	0.275	0.274
Number of Clusters	180	358	249	249

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

NOTES: This table reports OLS estimates of cross-village externalities on school participation decisions. The dependent variable equals 1 if the child currently attends school. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parenthesis. Sample of program-eligible children observed in October 1998 and November 1999 who, at baseline, are aged less than 18 and have completed grades 5 or 6 of primary school and the first grade of secondary school. The following baseline control variables are included in each specification: the potential number of villages (columns 1-2) or scholarship recipients (columns 3-4) in child's gender age and age squared, 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 number of secondary schools and the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Source: *Progresa* evaluation surveys and geo-referenced census of localities and secondary schools.

Table 4: Specification Checks

Sample	All Villages		Treated Villages	
	(1)	(2)	(3)	(4)
Own Village Treated	0.098***	0.095***		
	(0.011)	(0.012)		
# Treated Villages 0-20km - dist. weighted	0.039		0.112***	
	(0.024)		(0.039)	
# Scholarship recip. 0-20km - dist. weighted		0.0006**		0.0014***
		(0.0002)		(0.0005)
Number of Obs	11843	11843	7364	7364
R-squared	0.259	0.259	0.268	0.269
Number of Clusters	45	45	36	36

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

NOTES: This table reports OLS estimates of cross-village externalities on school participation decisions. The dependent variable equals 1 if the child currently attends school. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parenthesis. Sample of program-eligible children observed in October 1998 and November 1999 who, at baseline, are aged less than 18 and have completed grades 5 or 6 of primary school and the first grade of secondary school. The following baseline control variables are included in each specification: the potential number of villages (columns 1-3) or scholarship recipients (columns 2-4) in child's gender age and age squared, 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 number of secondary schools and the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Source: *Progresa* evaluation surveys and geo-referenced census of localities and secondary schools.

Table 5: Knowledge About the Program Components

	Scholarship (1)	Health (2)	Nutrition (3)	Scholarship (4)	Health (5)	Nutrition (6)
# Treated Villages 0-5km	0.049**	0.078***	0.012			
	(0.023)	(0.029)	(0.007)			
(Share of Indig. in Treated 0-5km)×I[HH Indig=1]				-0.036	0.230**	0.180*
				(0.183)	(0.106)	(0.101)
(1-Share of Indig. in Treated 0-5km)×I[HH Indig=0]				-0.142	-0.009	0.007
				(0.185)	(0.190)	(0.018)
Number of Obs	7766	7766	7766	7766	7766	7766
R-squared	0.069	0.053	0.025	0.067	0.054	0.027
Number of Clusters	242	242	242	242	242	242

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

NOTES: This table reports OLS estimates of cross-village externalities on dichotomous indicators of whether recipients' know the various components of the benefits package of the program. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parenthesis. Sample of program-eligible households residing in treated villages observed in October 1998 and November 1999 whose children, at baseline, are aged less than 18 and have completed grades 5 or 6 of primary school and the first grade of secondary school. The following baseline control variables are included in each specification: the potential number of villages in 0-5km (columns 1-3) or the share of program-eligible indigenous households in evaluation villages in 0-5km (columns 4-6), child's gender age and age squared, 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 number of secondary schools and the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Source: *Progresa* evaluation surveys, geo-referenced census of localities and secondary schools, and *Progresa* follow-up survey of recipients.

Table 6: Program Effectiveness

	Across Municipalities	Delays in Transfers		Share of Eligibles who receive E1 form	Number of Schools in 0-5km	Number of Teachers in 0-5km
		Scholarships	School supplies			
	(1)	(2)	(3)	(4)	(5)	(6)
# Treated Villages 0-5km - Same Muni	0.089***					
	(0.034)					
# Treated Villages 0-5km - Diff Muni	0.135***					
	(0.047)					
# Treated Villages 0-5km		-0.018	-0.048	0.001	0.122	1.413
		(0.136)	(0.131)	(0.023)	(0.097)	(1.136)
Sample Mean of Dependent Variable	0.81	0.68	0.98			
Number of Obs	7364	612	610	604	618	618
R-squared	0.276	0.342	0.112	0.080	0.943	0.955
Number of Clusters	249	247	246	243	249	249

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

NOTES: This table reports OLS estimates of cross-village externalities on various measures of program effectiveness in the delivery of public services. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parenthesis. Column 1: sample of program-eligible children residing in treated villages observed in October 1998 and November 1999 who, at baseline, are aged less than 18 and have completed grades 5 or 6 of primary school and the first grade of secondary school. Columns 2-6: sample of treated villages observed in October 1998 and November 1999. Baseline control variables include: child's gender and age, parental education (column 1 only), 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, *Progresa* administrative transfer database, *Progresa* follow-up survey of recipients and geo-referenced census of localities.