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“Spillover effects and take-up of transfers in integrated social policies: Evidence from Progresa”

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Spillover effects and take-up of transfers in integrated social policies: Evidence from *Progresa* *

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Abstract

When potential beneficiaries share knowledge and attitudes about a policy intervention, that can influence their decisions to participate and, in turn, change the effectiveness of both the policy and its evaluation. This matters notably in integrated social policies with several components. We examine spillover effects on the take-up of the schooling subsidy component of the Progresa-Oportunidades program in rural Mexico by exploiting exogenous variations in the local frequency of beneficiaries generated by the program’s randomized evaluation. Higher treatment frequency in the areas surrounding the evaluation villages increases the take-up of scholarships and enrollment at the junior-secondary level. These cross-village effects exclusively operate on households receiving another component of the program, and do not carry over larger distances. While several tests reject heterogeneities in impacts due to spatial variations in program implementation, we find suggestive evidence that spillovers stem partly from the sharing of information about the program among eligible households.

Keywords: spatial externalities; knowledge spillovers; peer effects; take-up of social policies; policy evaluation; conditional cash transfers.

JEL Codes: C9, I2, J2, O2.

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1 Introduction

Demand-side schooling interventions have now become an important component of social policies in developing countries. The available empirical evidence suggests that cash subsidies in particular can have large effects on schooling decisions (e.g. Glewwe and Kremer 2006). These interventions have been found to be effective devices for encouraging the human capital investments of poor households (e.g. Parker et al., 2008 and Fiszbein and Schady, 2009). Recent studies have documented that they can also induce, in beneficiary areas, a set of non-market interactions that can further increase their effects (Angelucci et al., 2010, Bobonis and Finan, 2009, and Lalive and Cattaneo, 2009). Social interactions affecting preferences for investments in education and transfers within extended families have in particular been posited and documented. However, there is still incomplete knowledge on the specific networks within which those interactions occur and the specific mechanisms at play.

The sharing of knowledge and attitudes about the policy interventions among networks of potential beneficiaries is one set of social interactions that remains under-documented in the setting of social policies in developing countries. The role of information-sharing and initial preferences and prejudices in determining program participation has been emphasized in the context of social policies in the United States. For instance, Bertrand et al. (2000) and Aizer and Currie (2004) find evidence of networks effects, i.e. correlations in program take-up decisions within neighborhoods and ethnic groups. In the case of the Food Stamp Program, Daponte et al. (1999) find that ignorance about the program contributes to non-participation.

The conditional cash transfer (CCT) programs that have been recently implemented in developing countries create many opportunities for knowledge spillovers between beneficiaries. These opportunities are likely to affect the take-up rates of the schooling subsidies and they are influenced by three types of factors that span both supply and demand sides. First, in integrated social policies, cash subsidies for schooling tend to be associated with complementary interventions for the provision of health care or support for better nutrition. Beneficiaries do not necessarily participate in all the interventions, so that there is an intensive margin for potential recipients to increase their participation in the program by taking up more components. Second, the recipients of the transfers, notably women and mothers, regularly encounter each other during program operations, for instance in meetings of beneficiaries or during activities of complementary interventions, such as visits to health centers. Third, the targeting of those interventions implies that participants often have similar socioeconomic backgrounds and are thus likely to identify with each other (Ak-

erlof, 1997). Hence, the demand-side schooling interventions are likely both to enhance the existing interactions among groups of beneficiaries, and to further shape those groups, thus producing externalities that would not occur were individuals treated in isolation.

As with other mechanisms of social interactions, interactions based on sharing of information about the policy intervention and attitudes can explain variations across areas and groups in the take-up of schooling subsidies which do not stem from cost-benefit factors such as the differences in the supply of schooling or the opportunity costs of schooling. However, they depart from other interactional mechanisms because they take place among networks of potential beneficiaries of the interventions. Interactions between potential beneficiaries have different implications than other sorts of social interactions for the design and implementation of the interventions. The targeting of a program and the way it mobilizes potential beneficiaries is particularly likely to affect the extent of those interactions and thus it is crucial for its effectiveness.

In this paper, we examine the presence of neighborhood effects, in the form of social interactions within networks of potential beneficiaries, affecting the take-up of the schooling subsidy component of the *Progres-a-Oportunidades* social program (see, e.g., Schultz, 2004, and Parker et al., 2008). The program entails several unbundled components in addition to the schooling subsidies, notably food stipends conditional on health checks. While the take-up of the nutrition and health component is almost 100 percent, a large share of children eligible for transfers for secondary schooling remain unenrolled.

The program targets poor households in small villages located in rural areas of Mexico, and we base our analysis on the study of cross-village spillovers. Due to the high level of program penetration and geographic targeting, the topography of the area covered by the program consists of clusters of neighboring villages rather than isolated villages with a high density of beneficiary communities within treated regions. In this context, beneficiaries living in neighboring villages are likely to interact in several ways, and share information on the program. To examine the effects of those interactions, we investigate the extent to which variations in the local frequency of the program in areas surrounding beneficiary villages influenced the take-up response of potential beneficiaries.

Spillovers have previously been examined in the context of *Progres-a - Oportunidades* by comparing the outcomes of ineligible and eligible households in the same villages using partial-population designs (Moffitt, 2001). Accordingly, Bobonis and Finan (2009) and Lalive and Cattaneo (2009) have found evidence of spillovers through peer effects in school enrollment, and Angelucci and De Giorgi (2009), Angelucci et al. (2010) and Angelucci et al.

(2012) evidence of transfers within both village and household-level networks.¹ However, in the *Progres-Oportunidades* setting, because many beneficiary communities are very close to each other, spillovers may occur not only within but also across villages.

To investigate the presence of neighborhood effects, we combine data from the experimental evaluation of the program with information on the geo-referenced locations of the villages benefitting from it. We focus our analysis on the secondary school participation decisions of program-eligible children, which is the primary short-run outcome of the intervention and the key requirement associated with the largest component of the in-cash transfer.

We use a simple empirical framework that allows us to disentangle the effects of the incentives resulting from the program eligibility of the household (and the village it resides in) from the indirect effects arising from the local density of program recipients at the level of areas surrounding targeted villages. Exploiting the randomized evaluation design and the clustered spatial distribution of the villages in our sample, we identify the causal effects of program externalities across neighboring villages. The allocation of evaluation localities between the treatment and control group is random within each area surrounding villages. These exogenous variations enable us to identify the spillovers induced by the relative frequency of program delivery within any distance from the villages in our sample. Next, we investigate whether spillovers arise in this setting because of social interactions between program beneficiaries, or other changes associated with variations in the local density of the program across areas surrounding villages.

We find evidence of a positive and robust effect of the local frequency of participants in the program on secondary school participation decisions: the presence of other potential beneficiaries in nearby villages further increases the effect of the intervention. This neighborhood effect is large, with a marginal effect of an additional treated village in the neighboring area of 2.9 percentage points on secondary school enrollment – to be compared with a direct effect of own village treatment of 9.7 percentage points – and it is present only over short distances (0–5 km) but vanishes over larger radii (5–10 km). Crucially, these spatial externalities appear to affect exclusively 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 for the program. This remarkable heterogeneity sheds some light on the mechanisms behind program externalities. Interactions within networks of potential beneficiaries spanning across villages seem to have contributed to increase the take-up of

¹Other 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.

the educational component of the program and heighten its impacts on schooling. We argue that, while interactions through preexisting social networks should affect all households that share local resources, social interactions that are restricted to program beneficiaries are likely to be associated with knowledge and attitudes toward the program. Accordingly, we find that our variation in local treatment frequency is associated with increased knowledge, among eligible households, about the different components of the program – notably the schooling subsidies.

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 take-up of schooling subsidies. 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 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 in the data for such implementation variations.

Our results thus provide evidence of the effects of the local frequency of treatment on the take-up of the different components of social policies. We find suggestive evidence that knowledge spillovers among networks of beneficiaries is likely to be driving those effects. Our findings confirm, in the context of a developing country, that social interactions associated with higher treatment frequency can increase individual responses to a social policy.

Our findings also relate to other studies which have used experimental variations of treatment frequency to identify the effects of spillovers of interventions (e.g. Miguel and Kremer, 2004, Banerjee et al., 2010, and Ichino and Schundeln, 2012). However, those studies were conducted during small-scale interventions, and hence potentially miss important effects that occur during the full-scale implementation of a program.² Our results shed light on those scaling-up effects by examining spatial externalities in an experimental sample surveyed in the midst of the implementation of the policy on a large scale.

²To 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 (Baird et al., 2014; Crepon et al., 2013).

2 Setting and Data

2.1 Program features

Initiated in 1997 and still in effect, *Progresa-Oportunidades* 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 behaviors in the key areas of health and education. It grants scholarships and school supplies to children aged under 17, 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). The scholarships increase in amount with school grade, and in grades 7 to 9 are larger for girls than boys. The program also distributes cash transfers for the purchase of food, provides food supplements, and promotes health care through free preventive education interventions on hygiene and nutrition. The distribution of the food stipends and nutritional supplements is conditional on health care visits at public clinics. The 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.³

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 were ranked by a composite index of marginality, which was computed using information on socioeconomic characteristics and access to the program infrastructures from the censuses of 1990 and 1995.⁴ 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.⁵

The program started in 1997 in 6,300 localities with about 300,000 beneficiary households,

³Overall cash transfer amounts can be substantial: the median benefits are 176 pesos per month (roughly 18 USD in 1998), equivalent to about 28 percent of the monthly income of beneficiary families.

⁴Localities with fewer than 50 or more than 2,500 inhabitants were excluded during the first years of the program. We use the words “locality” and “village” interchangeably when 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 (including hamlets, villages, farms, and other clusters). Rural localities (also called rural communities), or villages, are defined as having fewer than 2,500 inhabitants.

⁵A proxy-mean index was computed as a weighted average of household income (excluding children), household size, durables, land 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.

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). The expansion of the program continued in subsequent years both in rural and urban areas.

An experimental evaluation of the program was conducted during this phase of geographical expansion in rural areas. A random sample of 506 villages was drawn from a set of program-eligible localities situated in seven central states of Mexico (Guerrero, Hidalgo, Queretaro, Michoacan, Puebla, San Luis de Potosi, and Veracruz). From 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 formed the control group and were thus prevented from receiving the program's benefits until November 1999.

2.2 Partial take-up

Importantly for our purposes, the two transfer components are unbundled. Households declared eligible to receive benefits can take up food stipends, scholarships, or both. They can also choose 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 children do not seem to incur a high cost to households, because school enrollment at that level is almost 100 percent. We used data from the administration of the program on the distribution of the different transfers in the 320 treatment localities of the evaluation to document take-up. This data confirms the complete take-up of the food stipends: at the end of 1998 and 1999, respectively 97.1 and 98.0 percent of eligible households in those localities received the transfers.

In contrast, the conditionality of the scholarships at the secondary level is binding for many households whose eligible school-age children would not have gone to school in the absence of the program. The same data indicates that respectively 83.0 and 91.3 percent of households eligible to a scholarship for at least one child enrolled at the primary or secondary level receive one. But only 63.7 percent of kids who are eligible for a scholarship for secondary-level school do attend school in 1998, and 61.9 percent of them do in 1999.

Hence, partial take-up of the program benefits is prevalent in this setting, whereby some eligible households comply with the food stipend conditions but refrain from enrolling some or all of their children in secondary schools. However, once they are incorporated into the

program, recipients can further adjust their behaviors by enrolling some of their program-eligible children. While take-up of the food transfers is almost complete, there is thus a margin for increasing the take-up of the schooling component, which can be seen as an intensive margin of program participation.

2.3 Village Neighborhoods

In this paper, we use the term of 'neighborhood' for areas within a given radius around each evaluation village. We borrow this terminology from a literature based mainly on urban data, but in our contexts, 'neighborhood' means areas or clusters of villages.

In order to characterize the local densities of the intervention (in the neighborhoods), we combine information from the program administration, indicating which localities were eligible for the program at the end of 1998 and 1999, with information from the 2000 population census and the annual school census. The population census provides the geographical coordinates (latitudes and longitudes) for the universe of rural localities, and the school census the coordinates of all secondary schools. The geo-referenced data further allows us to identify the locations of the evaluation localities.⁶

As in many rural regions of Latin America and elsewhere, the topography of the area covered by the program consists of clusters of villages 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. This proximity favors the interactions between inhabitants of neighboring villages.

Turning now to the intervention, Figure 1 depicts the geographic scope of the *Progreso* penetration during the first two years of program roll-out in the seven central states where the 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. In order to provide a more in-depth depiction of the areas surrounding evaluation villages, the map features a smaller-scale view of a region in the State of Michoacan in which 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 frequency. Those patterns are

⁶We used official information on the listing of the universe of rural localities receiving the program (broken down by each program 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 fraction (about 20 percent) 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.

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 a neighborhood 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 in average 386 (46 percent) receive scholarships from *Progresa* (column 1).⁷ Moreover, several evaluation villages are indeed located very close together. Of the 506 evaluation localities, 139 (27 percent) have another evaluation locality within 5 km, 57 (11 percent) have two such localities, and 16 (3 percent) have three or more. Thus 212 (41 percent) villages in the experiment have other evaluation villages in a 5 km radius; our empirical analysis will identify the effects of cross-village externalities for these villages. The density of the program, as captured by the numbers of both non-evaluation and evaluation beneficiary villages, roughly doubles in areas 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 neighborhoods with treated or control centroids (columns 4–5).

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 have access to such a facility within 5 km. Similarly, most localities do not have a junior secondary school – only 17 percent do in the evaluation sample – while 93 percent have access to one or more in other villages within 5 km. Hence, households from different program localities located in the same area can interact when utilizing social infrastructure. Furthermore, some operations which are specific to the program are also organized in conjunction for several neighboring villages. This is notably the case of the distribution of transfers in temporary and mobile outposts – located in hub localities – which serve an additional function to assist beneficiaries and disseminate information on the program. Hence, program beneficiaries from different neighboring villages can interact in a number of places.

⁷Evaluation 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).

2.4 Sample Description

We combine the geo-referenced locality data mentioned above with three of the five rounds of the evaluation survey, collected in October 1997 (from the baseline targeting ENCASEH survey), October 1998 (second round of the ENCEL evaluation surveys), and November 1999 (fourth round of the ENCEL surveys).⁸ The resulting dataset contains detailed information on the outcomes of children and socioeconomic characteristics of a panel of households that 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. Some attrition occurred, 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.⁹ Our main outcome of interest is school enrollment, for which we also use the term “school participation” interchangeably. 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 area 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.¹⁰ We also restrict to a balanced panel of children observed at all rounds.

The sample contains 6,690 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. For 807 (12.6 percent) of those, no information was collected on

⁸We have discarded the March 1998 and June 1999 rounds of the survey because we only have information on the roll-out of the program at the end of each year.

⁹About 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.

¹⁰The sample selection cannot be based on the grade during the follow-up period because that grade is potentially affected by the treatment.

either school participation or parental education, thereby leaving us with a final sample of 5,883 children observed in both 1998 and 1999. At baseline, the average enrollment rate is 63.8 percent (59.3 percent for girls and 68.5 percent for boys).

3 Program Externalities Across Villages

3.1 Empirical Strategy

Our identification strategy exploits two features of the program evaluation design: the proximity between many evaluation villages and village-level random assignment to treatment. Its key intuition is that, 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, on schooling decisions, of the variations in treatment frequency induced by the randomized evaluation within any neighborhood of an evaluation village.

Neighborhoods are defined as concentric circles around each evaluation village using geodesic distances d as the radius.¹¹ Program treatment T_j is administered at the village level. It is randomly assigned only within the subset of 506 villages which participated to the evaluation of the program, and not all beneficiary villages participated to the evaluation. Hence, as described in subsection 2.3, neighborhoods of evaluation villages comprise other evaluation villages, non-evaluation beneficiary villages, and non-eligible villages. Let then $N_{j,d,t}^B = N_{j,d,t}^T + N_{j,d,t}^{NE}$ denote the total number of program beneficiary villages situated within distance d from evaluation village j in a given post-treatment period t . Among those, $N_{j,d,t}^T$ is the number of evaluation villages which are randomly assigned to the treatment group of the evaluation and $N_{j,d,t}^{NE}$ the number of other neighboring (non-evaluation) villages which are targeted by the intervention during each post-treatment period t . Now let $N_{j,d,t}^P = N_{j,d,t}^T + N_{j,d,t}^C + N_{j,d,t}^{NE}$ denote the number of *potential* program villages situated at distance d from village j in a given post-treatment period t , where we have added $N_{j,d,t}^C$, the number of villages randomly assigned to the control group of the evaluation.

To estimate the spillover effect of the program on school participation, we use the following linear regression model:

$$Y_{i,j,t} = \alpha_1 T_j + \alpha_2 N_{j,d,t}^B + \alpha_3 N_{j,d,t}^P + \alpha'_4 X_{i,j,d} + \epsilon_{i,j,d,t}, \quad (1)$$

¹¹Due 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.

where $Y_{i,j,t}$ is a dummy indicating that program-eligible child i in evaluation village j in a given post-treatment period t is going to school, T_j is the randomly assigned treatment indicator which denotes whether locality j receives the program, $X_{i,j,d}$ is a column-vector of baseline characteristics at the individual, household, village and neighborhood level, and $\epsilon_{i,j,d,t}$ captures other unobservable determinants of the school participation decision which are potentially correlated with the program's targeting.

In this framework, the parameter α_1 captures the sum of the average direct effect of program eligibility and the average indirect effects which stem from treatment of other households in the same village. Due to the fact that program treatment status varies at the village level, it is not possible to separately identify these two effects.¹² The main parameter of interest is α_2 , which captures the marginal effect of treatment of one additional village (or eligible households) in the neighborhood. Finally, the parameter α_3 captures the effects of any unobserved determinant of the school participation decision which is correlated with the program geographic targeting.

The identification challenge is that more marginalized regions tend to have higher treatment densities (see Table 1), and there are a variety of other unobserved factors associated with the geographic roll-out of the intervention which are also likely to affect program outcomes. The random program assignment within the subset of evaluation villages provides some exogenous variation for the local density of treatment in the geographic areas surrounding the evaluation villages over and above the (endogenous) spillover effects coming from the non-evaluation beneficiary villages. After conditioning on the potential treatment frequency in the neighborhood $N_{j,d,t}^P$, cross-neighborhood variations in the frequency of the program are solely determined by the random allocation to the treatment and control groups. Indeed, the number of program beneficiary villages in the neighborhood is given by the difference between the number of potential beneficiary (or targeted) villages and the number of villages selected into the control group for the randomized evaluation: $N_{i,j,t}^B = N_{i,j,t}^P - N_{i,j,t}^C$. Hence, because the number of villages allocated to the control (and treatment) group is random, the potential schooling outcomes of child i who reside in time t in village j with program treatment status $T_j = 0, 1$ and neighborhood treatment frequency $N_{d,t}^B$, are independent of that realized treatment frequency when controlling for targeted neighborhood treatment

¹²A partial population approach, exploiting the presence of ineligible households in beneficiary villages, can be followed, and it has been by previous studies. It however requires some assumptions, notably that spillovers affect both eligible and ineligible individuals, and is thus not well-suited for investigating spillovers on the take-up of program components.

frequency $N_{d,t}^P$. Formally:

$$E[y_{i,j,t}^{T,N^B} | N_{j,d,t}^B, N_{j,d,t}^P] = E[y_{i,j,t}^{T,N^B} | N_{j,d,t}^P]. \quad (2)$$

This is equivalent to conditioning on the number of evaluation villages, $N_{i,j,t}^E$, and examining differences in potential outcomes with the number of treated villages, $N_{i,j,t}^T$. With this conditional independence property, comparisons of average outcomes across different levels of actual treatment frequency $N_{i,j,t}^B$, e.g. n_1^B and $n_2^B > n_1^B$, at a given level of potential treatment frequency $N_{i,j,t}^P$ capture the causal effect of an increase in actual treatment frequency from n_1^B to n_2^B . Formally (and omitting the indexes):

$$\begin{aligned} E[y^{n_2^B} | N^B = n_2^B, N^P] - E[y^{n_1^B} | N^B = n_1^B, N^P] &= E[y^{n_2^B} | N^B = n_2^B, N^P] - E[y^{n_1^B} | N^B = n_2^B, N^P] \\ &= E[y^{n_2^B} | N^P] - E[y^{n_1^B} | N^P]. \end{aligned}$$

As a validation test of the property depicted in equation 2, 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, and estimate equation 1 using those baseline characteristics as outcomes. This amounts to a test of balancing of baseline covariates with respect to the variation in local treatment frequency generated by the randomized experiment (that we exploit for identifying spillovers). Table 2 reports the means and standard deviations for those variables (columns 1–2) along with the associated OLS coefficients of the neighborhood treatment density term ($N_{j,d,t}^B$). 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 frequency. Consistently with the targeting design of the program, treatment frequency correlates positively both with the level of deprivation in the centroid village and with the density of villages/population in the neighborhood. However, as reported in column 4, those differences disappear once we control for the potential treatment frequency in the neighborhood ($N_{j,t}^P$). A F-test of joint significance of all baseline characteristics does not reject the null hypothesis that the entire set of variables is equal to zero (p-value=0.227) with this specification.¹³

Our econometric model is thus a linear regression in which we are interested in the parameter of a regressor, the density of actual program villages $N_{j,d,t}^B$, which is exogenous

¹³Two of those baseline variables (the share of eligible households and the number of secondary schools) remain marginally statistically associated (at the 10 percent confidence level) with the density of the program. For consistency with our main estimates we estimate those placebo regressions using a 5 km radius ($d = 5$). Results (available upon request) are very similar when considering alternative radiuses.

once controlling for another regressor, the density of potential program villages $N_{j,d,t}^P$ (note T_j is exogenous with or without any conditioning variable). As program targeting is partly correlated with local poverty levels, we expect the estimated parameter of $N_{j,d,t}^P$ to be biased downward. However, the bias on that parameter is orthogonal to both the T_j and $N_{j,d,t}^B$ terms and hence it does not contaminate the estimates of the α_1 and α_2 parameters. This is verified by writing the asymptotic limits of the ordinary least squares estimates of the set of parameters (this calculation is available upon request from the authors).

Furthermore, in equation 1, neighborhood treatment frequency is orthogonal to village-level program treatment assignment, so that the spillover 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. We thus consider the following variant of equation 1:

$$Y_{i,j,t} = \beta_1 T_j + \beta_2 N_{j,d,t}^B + \beta_3 [T_j \times N_{j,d,t}^B] + \beta_4 N_{j,d,t}^P + \beta_5 [T_j \times N_{j,d,t}^P] + X_{i,j,d,t} \beta_6 + u_{i,j,d,t}, \quad (3)$$

where the village-level treatment assignment term (T_j) is interacted with the density of both actual ($N_{j,d,t}^B$) and potential ($N_{j,d,t}^P$) neighboring beneficiary localities so that the effects of cross-village externalities are identified separately for the control and treatment groups. This specification allows to test whether program externalities differentially vary with treatment assignment ($\beta_3 \neq 0$).

Several features of this specification should finally be noted. In practice, the parameter α_2 in equation 1 is estimated out of the subset of eligible households of the controlled experiment that have other evaluation villages in the neighborhood of radius d . For a radius of 5km, we have such identifying variation for 42% of the evaluation villages.

The inclusion of the vector of socio-demographic variables $X_{i,j,d}$ in equations 1 and 3 is meant to increase the precision of the estimates. The controls are all measured at baseline using the 1997 data to avoid any endogeneity concern and, taking advantage of the panel dimension of the data, include in particular baseline school enrollment. The remaining control variables are: 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 in the locality, the number of localities, total population and the mean degree of marginalization in the neighborhood. We also include for state and year fixed effects.

Lastly, 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, our preferred specification uses a 5 km radius but we also use radiuses of 10 and 20 km. 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 km neighborhoods – the 320 treatment villages belong to 249 such clusters – and this number reduces to 180 when considering clusters formed by overlapping 10 km neighborhoods, and 45 with 20 km ones.

3.2 Empirical Evidence

Table 3 reports the OLS estimates of the spillover effects of the program on eligible children’s school participation decisions. The estimates are obtained using the data for the post-treatment period (October 1998 and November 1999). We report and discuss only the estimates of the parameters α_1 and α_2 but, as discussed above, the regressions further include baseline characteristics observed in October 1997, notably baseline school enrollment.¹⁴

The estimates are obtained with two alternative measures of program frequency in the areas surrounding the evaluation villages: the models in columns 1–3 use the numbers of villages treated in a 5 km radius, while the ones in columns 4–6 use instead the numbers of eligible households within the same radius. This second definition takes into account the variations in population density across neighborhoods and hence possibly better captures the extent of potential interactions among program beneficiaries. Columns 1 and 4 report the estimates for the baseline model in equation 1. In order to document the heterogeneity of cross-village externalities by treatment status, columns 2 and 5 report OLS estimates of the augmented model specified in equation 3 and columns 3 and 6 estimates of the model in equation 1 obtained after restricting the sample to the treatment group.¹⁵

Beginning with the results obtained when measuring program frequency by the numbers of villages, column 1 indicates that, when considering the entire sample of children in treat-

¹⁴The last rows of Table 3 report the estimated coefficients of the conditioning term N^P in equation 2, split into its two components: N^E and N^{NE} . Those are in general negative and significant, suggesting the presence of strong downward biases stemming from the process of geographic targeting of evaluation villages and non-evaluation villages.

¹⁵We also ran probit estimates of the same models and obtained very similar estimates of the effects of spillovers – the results are available upon request.

ment and control villages, while living in a treated community increases school participation by 9.7 percentage points, having an additional treated village within a 5 km radius further increases enrollment rates by 2.9 percentage points (this spillover effect is statistically significant at 10 percent). Program externalities appear to matter only for children who live in treatment group localities. Column 2 indeed shows no evidence of spillovers affecting school enrollment of children in control villages (the parameter for the main effect of program frequency has a negative point estimate and it is not statistically significant), but evidence of strong spillovers on the treatment group. The point estimate for the differential effect of spillovers in treatment villages as compared to control villages (given by the parameter for the interaction term β_3 in equation 3) reaches 7.8 percentage points, and this estimated differential effect is statistically significant at the 5 percent level. The finding of spillovers restricted to the control group is confirmed by the estimates reported in column 3, in which we restrict the sample to the treatment group. The effect on school enrollment of having an additional treated village within a 5 km radius is estimated at 5.6 percentage points, and it is statistically significant at the 1 percent level. The specifications reported in columns 4-6 of Table 3 which use the numbers of households, normalized by 100, for measuring program frequency in the areas surrounding the evaluation villages give very similar results.

For investigating whether spillovers operate over relatively short or larger distances, Table 4 reports the OLS estimates of the model in equation (1) using measures of program density in neighborhoods covering larger distances than 5 km around. Those estimates are also obtained using the two measures of program density, the numbers of villages (columns 1-4) and eligible households (columns 5-8). Columns 1 and 5 use the same baseline specification as columns 1 and 4 of Table 3 with the entire sample. Columns 2 and 6 use the restricted sample of children in treated villages (as columns 3 and 6 of Table 3). For the estimates in columns 3, 4, 7 and 8, we measure program frequency over a 20 km radius and weight the observations in each village by the inverse of the distance to the centroid. Now, as those are defined as the set of villages within a given distance to at least another village in the set, the number of clusters decreases in those estimates as we increase the size of the neighborhoods: with 10 km radiuses, we have 180 clusters for the entire sample and 137 for the sample of treatment villages, and with 20 km radiuses the numbers of clusters fall to 45 and 36. Otherwise, these specifications are estimated using the same data and include the same control variables as the ones in Table 3. The estimated coefficient for the numbers of treated villages located at a distance between 5 and 10 km is small and statistically insignificant. This suggests the presence of a strong decay rate in spatial externalities. However, the specifications using the

distance-weighted density measures computed over the 20 km radius confirm the presence of positive spillovers on school participation in treatment group localities. The estimates are statistically significant at 5 percent level, consistently with the results presented in Table 3. Overall, these results indicate that spillovers operate over relatively short distances. Since we find no evidence of spillovers of beneficiaries over larger distances, in the rest of the analysis we restrict to those operating over 0 to 5 km.

3.3 Discussion

The estimated own-village treatment effect of the program is in line with the results obtained in previous studies (e.g. Schultz (2004)). Note that, when we allow for heterogenous effects of program spillovers, the relative OLS coefficient of the village-level treatment assignment term (β_1) decreases only slightly (to 8.1 percentage points). We argue that this is due to the simultaneous presence of non-evaluation treated neighboring villages together with the fact that program spillovers accrue exclusively between beneficiaries. In this view, the estimated own-village effect of the program on school enrollment would also embed a portion of the program spillovers stemming from the non-evaluation treated neighbors.

Extrapolating the estimated marginal spillover effect to the total number of program villages or households in the neighborhoods of the evaluation villages would indicate an implausibly large effect of more than 50 percentage points. This is clear at odds with both previous studies that in the same setting document the effect of program spillovers within-villages (from beneficiaries to non-beneficiaries) to be around 5 percentage points (Bobonis and Finan, 2009; Lalive and Cattaneo, 2009). One way to interpret the observed magnitudes is through a strongly non-linear (convex) shape of the spillover function with respect to the frequency of nearby program beneficiaries.¹⁶ In this sense, the variation in treatment frequency that we exploit for identification only allows us estimating the marginal spillovers over and above the (endogenous) spillover effects coming from the non-evaluation treated neighbors. Hence, our setting does not allow us validating the presence of such non-linearities.

¹⁶A broad class of models of discrete decisions with strategic complementarities display a convex multiplier effect, and/or the presence of threshold effects in the spillover function – see, e.g., Brock and Durlauf (2001); Glaeser and Scheinkman (2000).

4 Further Evidence

We now 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. The finding of spillovers on school enrollment operating over short distances supports a simple model of peer effects on program take-up decisions of eligible households.¹⁷ As we don't have measures of the occurrences of interactions of beneficiaries from different neighboring villages, we cannot report direct evidence of this. We hence conduct several indirect checks for the presence of such interactions. On the other hand, some spatial variations in the local implementation of the program could also a priori explain the observed relationship between the local density of the treatment and program impacts. We thus also test for the presence of such spatial variations in the implementation of the intervention under study.

4.1 Knowledge Spillovers among Program 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 initial phases of the implementation regarding the effectiveness of the diffusion of information about the program among targeted households (Adato et al., 2000). To further corroborate this anecdotal evidence, we use information from an operational follow-up survey conducted among eligible households in the evaluation treatment-group villages in May 1999 (i.e., 14 months after the inception of the program). Program beneficiaries were asked to identify three sets of benefits distributed by *Progresa*: (i) scholarships and school supplies, (ii) food stipends and nutritional supplements, and (iii) preventive health care and health check-ups. Most of the respondents were the mothers who were to receive the transfers. While 98 percent of the respondents were able to spontaneously and correctly mention the nutrition component, only 60 percent correctly identified both the health and education components. Knowledge of the program components was thus incomplete in treatment villages at that time.

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 each

¹⁷Non-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.

other during program operations. When asked, in the same operational follow-up survey, to mention the most significant changes in their communities, half of the beneficiaries report that the program had increased the degree of cooperation among women.

For further evidence on this, we estimate the effects of spatial spillovers on those measures of eligible households' knowledge of the different program components. Table 5 reports the estimates, which are obtained using the model in equation 1 estimated at the household-level on the October 1998 data. Having one additional neighboring program village increases by respectively 4.5 and 8.2 percentage points the shares of eligible households who are aware of the education and health components (column 1 and 2), and has a smaller and no significant effects on the share of those aware of the nutritional component (column 3).

Given the evidence of program externalities on school participation decisions presented in Section 3, the observed variations of the knowledge of the education component could reflect increases in school attendance rather than the presence of peer effects among potential beneficiaries. Yet, the take-up patterns of the program discussed in section 2.2 make it difficult to interpret the evidence on the knowledge indicators of the other program components (such as health benefits) as purely stemming from corresponding variations in school enrollment among program-eligible children.

4.2 Supply-side Effects

Areas with higher densities of program participants might have benefited from more efficient program operations, or from improvements in the supply of education or health services, thereby helping some eligible households to comply with the schooling requirements of the program. A related alternative explanation is that the implementation of the program may have been more efficient when the number of evaluation treatment group villages in the neighborhood (rather than the total number of beneficiaries) was higher. While both notions of implementation scale gains seem a priori reasonable, we argue that they are unlikely to explain our results.

To examine the presence of differences in implementation and supply, two preliminary points should be noted. First, any variation in program delivery in a given geographic area should benefit evenly those program recipients who reside in it. This is at odds with the evidence reported in column 1 of Table 4, that program spillovers appear spatially concentrated within relatively small areas surrounding the evaluation villages. According to this line of thought, school supply-side changes would likely affect the enrollment decisions of both recipients and non-recipients, an idea difficult to reconcile with the evidence of

heterogeneous externalities reported in column 2 of Table 3.

Second, we identify spatial spillovers using only the variation in program frequency generated by the randomized experiment. This variation is small compared to the overall scale of the program (as seen in Table 1, in a given 5 km radius there are on average 10 beneficiary villages, but only 0.6 evaluation villages, among which 0.4 are assigned to the treatment group and 0.2 to the control group). program frequency is thus not very different around villages which have more treatment-group villages than around those which have more control-group villages in their neighborhood, and the resulting infra-marginal changes in the scale of the program are unlikely to trigger any supply-side efficiency gain.

We next run a battery of complementary tests aimed at detecting the presence of supply-side responses associated with experimental variations in the frequency of the treatment in the areas surrounding evaluation villages. We begin with two measures of implementation efficiency at the village-level. 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 was the delays in the delivery of the form for school attendance monitoring (E1 form) and the associated delays in the payment of scholarships. First, we use program administrative data on the monetary transfers (for scholarships and school supplies) delivered to eligible households to compute the number of months from incorporation until the first disbursements were made to the beneficiary households in treated villages.¹⁸ Second, we use information from the operational follow-up survey in order to construct the share of program recipients in treated village that received the E1 form as of May 1999.

In order to maintain full comparability with the estimates of program spillover reported in the rest of the paper, we estimate at the village-level the linear model reported in equation 1 using those program implementation measures as dependent variables on the sample of treated villages that we consider in the main empirical analysis.¹⁹ The dependent variables are time-invariant, and hence we match this information with only the first round (1998) of the data of the program roll-out. As documented in columns 1–3 of Table 6, these three

¹⁸While 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 six months or more after incorporation into the program. 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 1 without those two states. Results (available upon request) are very similar to those reported in Table 3

¹⁹Results (available upon request) barely change when we instead consider the entire sample of 320 treated villages. Due to a few missing values in the additional data sources that we employ, we lose some village-level observations in the regressions displayed in Panel A of Table 2: 3 villages for scholarship disbursements, 4 villages for school supply disbursements and 7 villages for the receipt of the school attendance (E1) form.

measures of efficiency of program implementation are unrelated to the frequency of the treatment in the areas surrounding evaluation villages.

We next use the yearly secondary school census in order to construct school supply aggregate measures for the 358 evaluation neighborhoods in our sample (with both treated and control villages as centroid) and estimate at the neighborhood-level the linear model reported in equation 1 over the two years of the program roll-out (1998-1999). In all specifications we control for the baseline (1997) value of the dependent variable. The OLS estimates are reported in panel B of Table 6, which are very small in magnitudes and not statistically different from zero.

5 Conclusion

We have examined the *Progres-a-Oportunidades* conditional cash transfer program to see whether the take-up of the schooling component of the program is influenced by the presence of other beneficiaries in areas comprising several villages. We found evidence of large positive spillovers within networks of beneficiaries spanning those areas. Higher local frequency of program beneficiaries increase the take up of the scholarships for secondary schooling offered by the program and, accordingly, school enrollment at that level. In contrast, these effects do not affect the schooling decisions of households in the control group villages that were not yet incorporated to the program.

To better understand our findings, we tested and found suggestive evidence for the presence of knowledge spillovers among program-eligible households. While we could not directly test for the presence of social interactions, we found that higher treatment densities in the areas are associated with increased knowledge among eligible households about the schooling and health components of the program. We also tested the alternative hypothesis that the spillover effect that we estimate reflect instead heterogeneities in direct treatment impacts due to spatial variations in the implementation of the program. The evidence that we have presented is not consistent with this interpretation of our findings.

Spillover effects on program take-up have implications for the design and implementation of social policies in developing countries. The magnitude of the estimated effect suggests that there can be large gains from the spatial concentration of an intervention’s target population, as local networks of potential beneficiaries can act as social multipliers in the take-up of the proposed benefits. Spillover effects also have implications for the evaluation of social policy interventions, notably in settings where a program is implemented over an extended area

and treatment frequency is high. In particular, capturing those effects across villages so as to recover impact evaluation parameters that incorporate spillovers, will require analyzing the impacts of the program at the level of relatively large geographical areas or clusters. Of course, the feasibility of that option will depend on the scale of the program that is being evaluated, and statistical power considerations may weigh for a narrower definition of the evaluation clusters. These considerations will be important in particular for evaluations conducted as the interventions are being scaled up on a large scale, a setting which likely differs from the evaluation of small pilot programs.

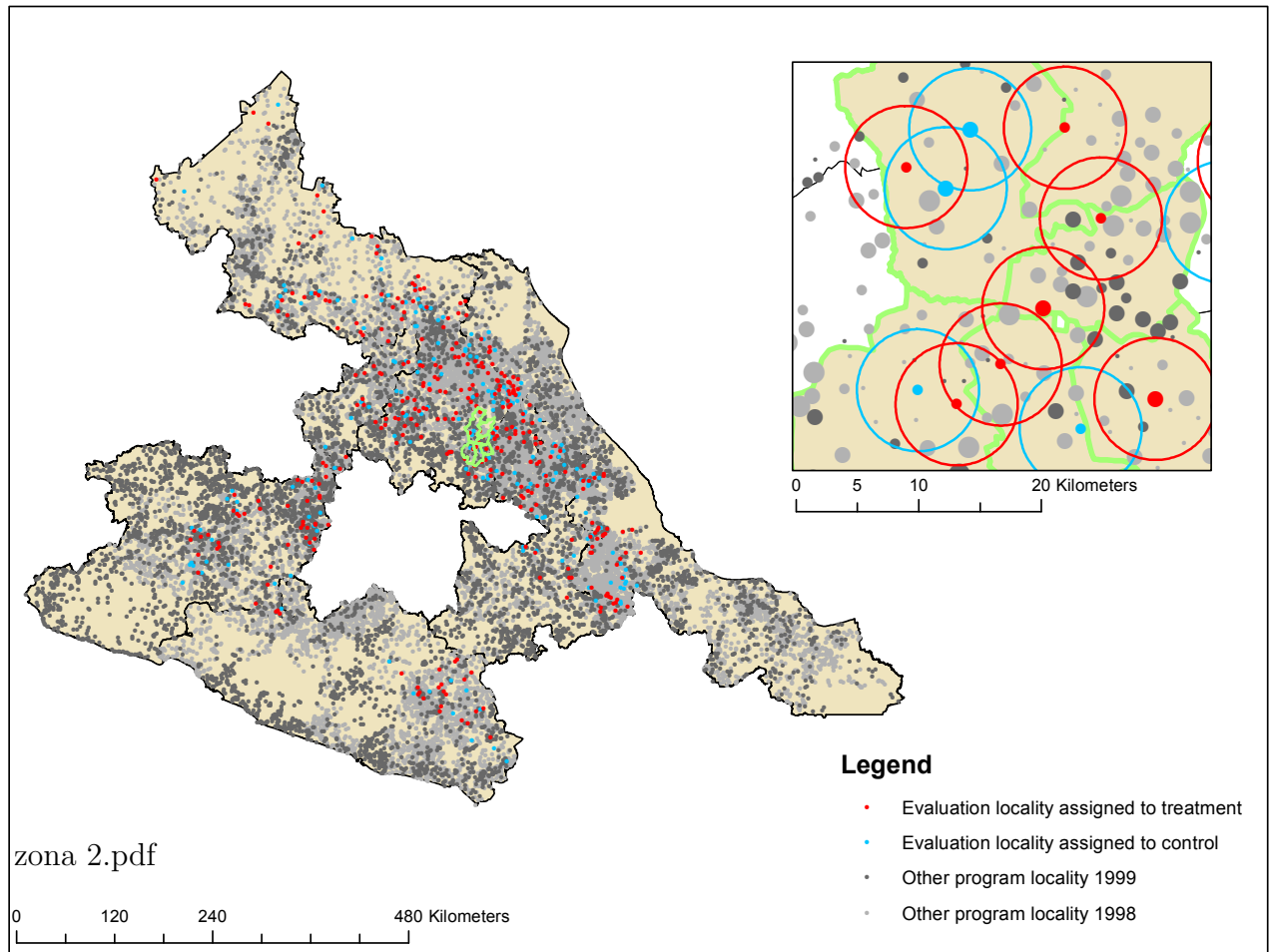
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Figure 1: Program Coverage (1998-1999)



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.

program frequency

Table 1: Treatment Frequency in Neighborhood around Evaluation Villages

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

Table 2: Neighborhood Treatment Frequency and Baseline Characteristics

	Mean	Std. Dev.	OLS Coeff (std.err) of $N_{j,5}^B$ Term	
			Unconditional	Conditional on $N_{j,5}^P$
	(1)	(2)	(3)	(4)
Individual and HH Characteristics (N=11,766)				
School Enrollment	0.68	0.47	0.000 (0.001)	0.018 (0.024)
Age	14.31	1.91	0.005 (0.003)*	0.045 (0.076)
Female	0.49	0.50	-0.001 (0.001)	0.006 (0.014)
Mother Education (years)	2.30	2.27	-0.015 (0.008)*	-0.001 (0.164)
Centroid Village Characteristics (N=11,766)				
Share of Program Eligible HHs	0.59	0.19	0.007 (0.001)***	0.054 (0.020)**
Secondary School (dummy)	0.25	0.44	-0.007 (0.003)**	-0.013 (0.069)
Distance to Nearest City	104.37	42.91	0.129 (0.298)	2.901 (5.662)
Neighborhood (radius=5km) Characteristics (N=11,766)				
Number of Secondary Schools	3.03	2.08	0.092 (0.019)***	-0.528 (0.274)*
Mean Index of Marginalization	4.38	0.73	0.029 (0.004)***	0.041 (0.066)
Number of Villages	22.76	12.90	1.125 (0.059)***	-0.119 (1.041)
Population Density (thous)	7.13	9.33	0.425 (0.077)***	-0.984 (0.999)
F-Test of Joint Orthogonality			49.90	1.274
P-values			(0.000)	(0.227)

* 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 frequency term ($N_{j,5}^B$) on each of those baseline characteristics, respectively without and with its potential counterpart ($N_{j,5}^P$) as a conditioning term. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parenthesis below each OLS coefficients. The sample contains program-eligible children in evaluation villages, observed in October 1998 and November 1999, and 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.

Table 3: Spatial Spillovers of the Program on Secondary School Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
Own Village Treated	0.097*** (0.014)	0.081*** (0.025)		0.095*** (0.014)	0.081*** (0.023)	
Actual Treatment Frequency	Villages			Eligible households (x100)		
# Treated in 0-5km	0.029* (0.015)	-0.020 (0.023)	0.056*** (0.020)	0.039 (0.024)	-0.026 (0.042)	0.070** (0.035)
(# Treated in 0-5km)×Treat		0.078** (0.033)			0.010* (0.057)	
Potential Treatment Frequency	Villages			Eligible households (x100)		
# Evaluation in 0-5km	-0.020 (0.014)	0.013 (0.021)	-0.047** (0.020)	-0.017 (0.024)	0.038 (0.038)	-0.050 (-.037)
(# Evaluation in 0-5km)×Treat		-0.055* (0.030)			-0.083 (0.056)	
# Non-Eval in 0-5km	-0.031** (0.015)	0.017 (0.023)	-0.058*** (0.020)	-0.039 (0.024)	0.026 (0.042)	-0.069* (0.035)
(# Non-Eval in 0-5km)×Treat		-0.077** (0.033)			-0.097* (0.057)	
Baseline enrollment	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	11766	11766	7317	11766	11766	7317
R-squared	0.364	0.365	0.383	0.364	0.365	0.382
Number of Clusters	358	358	249	358	358	249

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

NOTES: This table reports OLS estimates of cross-village program spillovers on school participation decisions. The dependent variable equals 1 if the child currently attends school. Columns 1-3 use the numbers of villages treated in a 5km radius as a measure of treatment frequency, while columns 4-6 use the corresponding numbers of households over the same radius. The sample contains program-eligible children in evaluation villages, observed in October 1998 and November 1999, and 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. It is restricted to treatment villages in columns 3 and 6. Standard errors are clustered at the level of groupings of partially overlapping neighborhoods are reported in parenthesis. All specifications control for baseline school enrollment. The other control variables include: 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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own Village Treated	0.094*** (0.016)		0.096*** (0.009)		0.092*** (0.015)		0.094*** (0.010)	
Actual Treatment Frequency	Villages				Eligible households (x100)			
# treated in 0-5km	0.025* (0.015)	0.062*** (0.020)			0.040* (0.021)	0.078** (0.036)		
# treated in 5-10km	-0.0058 (0.016)	0.014 (0.015)			-0.004 (0.019)	0.008 (0.020)		
# treated distance-weighted			0.029 (0.025)	0.087** (0.042)			0.032 (0.026)	0.110** (0.043)
Baseline enrollment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	11766	7317	11766	7317	11766	7317	11766	7317
R-squared	0.364	0.384	0.365	0.381	0.365	0.384	0.365	0.380
Number of Clusters	180	137	45	36	180	137	45	36

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

NOTES: This table reports OLS estimates of cross-village program spillovers on school participation decisions. The dependent variable equals 1 if the child currently attends school. As measures of treatment, columns 1-4 use the numbers of villages treated, respectively within 5km, 10km, and 20km with distance weights, while columns 5-8 use the numbers of eligible households respectively within 5km, 10km, and 20km with distance weights. The sample contains program-eligible children in evaluation villages, observed in October 1998 and November 1999, and 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. Standard errors are clustered at the level of groupings of partially overlapping neighborhoods are reported in parenthesis. All specifications control for baseline school enrollment. The other control variables include: the potential number of villages (columns 1-4) or eligible households (columns 5-8) 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)
# Treated Villages 0-5km	0.045** (0.022)	0.082*** (0.029)	0.011 (0.007)
Baseline enrollment	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes
Number of Obs	3858	3858	3858
R-squared	0.073	0.053	0.026
Number of Clusters	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 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, 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: Supply-Side Responses

Panel A: Indicators of Efficiency of Program Delivery at the Village Level (centroid)			
Dependent Variable	Months of Delays in Monetary Transfers		Share of HHs who Received of E1 Form
	Scholarships	School Supplies	
	(1)	(2)	(3)
# Villages treated in 0-5km	-0.007 (0.140)	-0.043 (0.132)	0.001 (0.024)
Village controls	Yes	Yes	Yes
Neighborhood controls	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes
Mean of Dep. Var.	1.1	6.4	.56
Number of Observations	306	305	302
R-squared	0.343	0.111	0.080
Number of Clusters	247	246	243

Panel B: Aggregate School Characteristics at the Neighborhood Level (0-5km)			
	Number of Schools	Number of Teachers	Share of High Ed. Teachers
	(1)	(2)	(3)
# Villages treated in 0-5km	0.045 (0.080)	-1.121 (0.767)	-0.030 (0.022)
Baseline Value of Dep. Var.	Yes	Yes	Yes
Neighborhood Controls	Yes	Yes	Yes
Year Dummy (1998)	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes
Mean of Dep. Var.	3	15	.2
Number of Observations	716	716	716
R-squared	0.953	0.968	0.589
Number of Clusters	358	358	358

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

NOTES: This table reports OLS estimates the effects of local treatment frequency on measures of the program supply-side. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parenthesis. Sample of treated villages observed in 1998 in Panel A and sample of evaluation neighborhoods observed in both 1998 and 1999 in Panel B. Source: geo-referenced census of localities and secondary schools, and *Progres*a follow-up survey of recipients.