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Article

Neighborhood Effects in Integrated Social Policies

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Abstract

When potential beneficiaries share their knowledge and attitudes about a policy intervention, their decision to participate and the effectiveness of both the policy and its evaluation may be influenced. This matters most notably in integrated social policies with several components. We examine spillover effects on take-up behaviors in the context of a conditional cash transfer program in rural Mexico. We exploit exogenous variations in the local frequency of beneficiaries generated by the program's randomized evaluation. A higher treatment density in the areas surrounding the evaluation villages increases the take-up of scholarships and enrollment at the lower-secondary level. These cross-village spillovers operate exclusively within households receiving another component of the program, and do not carry over larger distances. While several tests reject heterogeneities in impact due to spatial variations in program implementation, we find evidence to suggest that spillovers stem partly from the sharing of information about the program among eligible households.

JEL classification: C9, I2, J2, O2

Keywords: take-up, social policy, spatial externalities, knowledge spillovers, policy evaluation, conditional cash transfers

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 a large effect 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 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 of the specific networks within which those interactions occur and the underlying mechanisms at play.

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The sharing of knowledge and attitudes about policy interventions among networks of potential beneficiaries is one set of social interaction 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, that is, 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 of some subsidies, notably schooling subsidies, and 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 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 (Akerlof 1997). Hence, demand-side schooling interventions are likely to both 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.

In this paper, we examine the role of spillover effects in the form of information sharing within networks of potential beneficiaries and in shaping the take-up of the schooling subsidy component of the *Progresa-Oportunidades* CCT 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 un-enrolled.

The program targets poor households in small villages located in rural areas of Mexico. 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 with a high density of beneficiary households. In this context, program beneficiaries living in neighboring villages are likely to interact in several ways, thereby potentially sharing information about the program. In order 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 affects the take-up response of potential beneficiaries.

Spillovers have previously been examined in the context of *Progresa-Oportunidades* by comparing the outcomes of ineligible and eligible households in the same villages by means of a partial-population design (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. (2015) provide evidence of transfers within both village and household-level networks.[1] However, in the *Progresa-Oportunidades* setting, many beneficiary communities are very close to each other, thus spillovers may occur not only within, but also across, villages.

1 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.

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. In particular, we exploit the randomized evaluation design and the clustered spatial distribution of the villages in our sample in order to identify the causal effects of program externalities generated by those neighboring villages selected in the experimental treatment group. Next, we investigate whether spillovers arise in this setting because of social interactions between program beneficiaries or as a result of other changes associated with variations in the local density of the program across areas surrounding villages.

We find evidence of a positive effect of the local frequency of participants in the program over short distances (0–5 km) on secondary school participation decisions, which tend to quickly dissipate at larger distances (5–10 km). With estimated effects of respectively one or two or more treated villages in the neighboring area on secondary school enrollment of 6.1 and 8.0 percent, this spillover effect does not increase linearly with the number of treated villages. But the magnitude of the indirect effect of the program is substantial when compared to the direct effect of own village treatment of 9.7 percent.

Crucially, these spatial externalities appear to exclusively affect 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 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 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 sort of spatial variation in the delivery of the program among evaluation villages could, in principle, explain the observed relationship between the local density of treatment and the take-up of schooling subsidies. This may occur if, for instance, areas with more evaluation villages benefit from more efficient program operations or receive 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 schooling infrastructures, we find little support in the data for this alternative interpretation.

Our results thus provide evidence of the effect of the local frequency of treatment on the take-up of the different components of social policies. We find evidence to suggest that knowledge spillovers among networks of beneficiaries is likely to be driving those effects. Our findings also relate to other studies which have used experimental variations of treatment frequency to identify the effects of the spillover 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

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 (Baird et al. 2015; Crepon et al. 2013).

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.

I. Setting and Data

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. The program grants scholarships and school supplies to children aged under 17 conditional on regular attendance at one of the four last grades of primary schooling (grades three to six) or one of the three grades of junior secondary schooling (grades seven to nine). The scholarships increase in amount with school grade level achieved, and in grades seven to nine the scholarships are larger for girls than boys. The program also distributes cash transfers for the purchase of food, provides food supplements, and promotes healthcare through free preventive education intervention on hygiene and nutrition. The distribution of the food stipends and nutritional supplements are conditional on healthcare 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 at 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 encompassed three main steps. First, villages were ranked by a composite index of marginality 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 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 sample of 506 villages was randomly drawn from a set of localities that had been selected to be incorporated into the program, which were located in seven central states of Mexico (Guerrero, Hidalgo, Queretaro, Michoacan, Puebla, San Luis de Potosi, and Veracruz) after stratification by geographic region (which coincide roughly with the States) and population size. The randomness of the evaluation sample is corroborated in section S1 of the supplementary online appendix (available at https://academic.oup.com/wber). We document in particular that evaluation localities do

3 Overall 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.

4 Localities 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 inhabitants live in neighboring sets of living quarters and 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.

5 A 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 until at least November 1999, irrespective of any variation in household income.

not have different observable characteristics compared to non-evaluation localities located in the same neighborhoods. Also, the characteristics of evaluation localities and their population are not statistically significantly associated with the number of evaluation localities once the number of non-evaluation localities in their neighborhood are controlled for. Of those villages, 320 localities were randomly assigned to the treatment group and started receiving the program's benefits in March and April 1998; the remaining 186 formed the control group and were thus prevented from receiving the program benefits until November 1999.

Program 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 are largely dependent 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 use data to document take-up from the administration of the program on the distribution of the different transfers in the 320 treatment localities of the evaluation. These data confirm 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 indicate that, respectively, 83.0 and 91.3 percent of households that are eligible for a scholarship for at least one child enrolled at the primary or secondary level received one. However, only 63.7 percent of children who were eligible for a scholarship for secondary-level school attended school in 1998 with 61.9 percent attending 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 school. However, once they are incorporated into the program, recipients can further adjust their behavior 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.

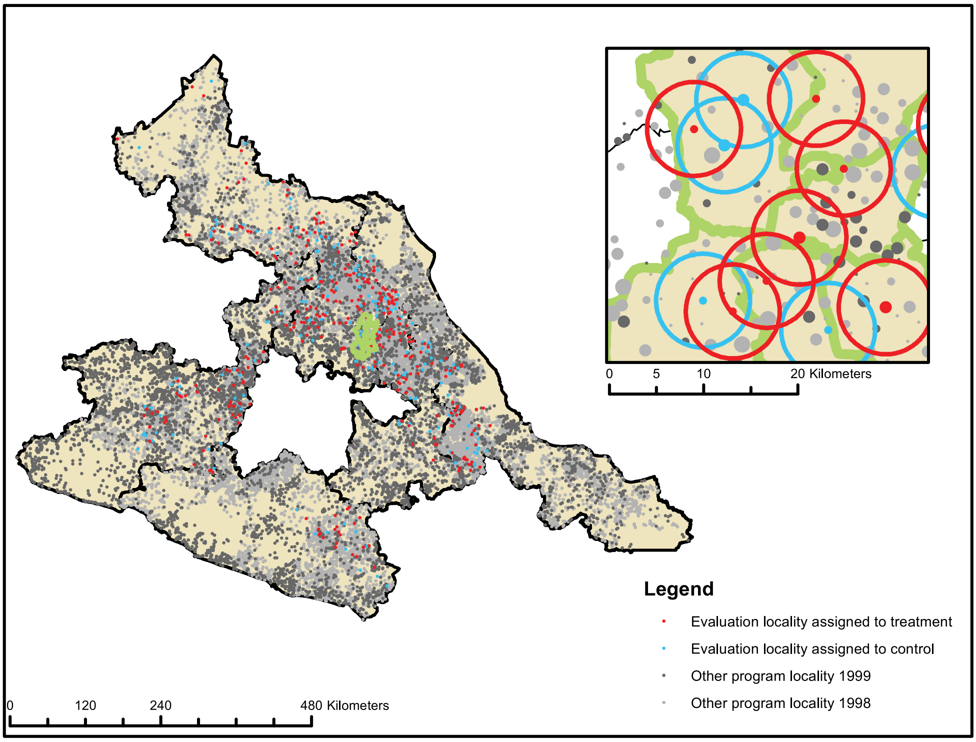
Village Neighborhoods

In this paper, we use the term "neighborhood" to describe areas within a given radius around each evaluation village. We borrow this terminology from a literature based mainly on urban data, but, in our context, "neighborhood" means an area or cluster 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 all the rural localities in Mexico while the school census provides the coordinates of all secondary schools. The geo-referenced data further allow us to identify the locations of the evaluation localities.[6]

6 We have used official information on the listing of all rural localities receiving the program (broken down by each program component) at the closing of each fiscal year in 1998 and 1999 in order 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

Figure 1. Program Coverage (1998–1999)



Source: List of localities receiving the Progresa-Oportunidades at end of 1998 and 1999, list of localities selected in the treatment and control group of the randomized evaluation, and 2000 population census.

Note: This figure 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 upper right hand 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 five-kilometer radiuses are displayed around each evaluation village.

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 five-kilometer radius from each evaluation village. This proximity favors the interactions between inhabitants of neighboring villages.

Looking now at the intervention, figure 1 depicts the geographic scope of the *Progresa* 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 a five-kilometer radius are drawn around each evaluation village.

As both maps reveal, beneficiary and evaluation villages tend to be geographically clustered with more deprived areas featuring a higher program frequency. These 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, ten program-beneficiary localities within a neighborhood defined by a five-kilometer radius

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.

Table 1. Treatment Frequency in Neighborhood 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					
# Evaluation villages	0.62	0.44	0.80	0.63	0.61
	(0.92)	(0.68)	(1.08)	(0.94)	(0.87)
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 evaluation villages)	0.11	0.09	0.13	0.11	0.12
	(0.32)	(0.29)	(0.34)	(0.31)	(0.33)
Prob(3+ evaluation villages)	0.03	0.01	0.06	0.03	0.03
	(0.17)	(0.06)	(0.24)	(0.17)	(0.18)
Distribution of evaluation villages assigned to treatment in neighborhood					
# Treated villages	0.40	0.27	0.53	0.40	0.39
	(0.71)	(0.52)	(0.84)	(0.74)	(0.65)
Prob(1 treated village)	0.24	0.19	0.28	0.24	0.25
	(0.43)	(0.43)	(0.40)	(0.42)	(0.43)
Prob(2+ treated villages)	0.07	0.04	0.09	0.07	0.06
	(0.25)	(0.19)	(0.29)	(0.25)	(0.25)
Total Villages in Evaluation Sample	506	253	253	320	186

Source: *Progresa* October 1998 evaluation survey. The sample in column 1 contains all evaluation localities. In columns 2–3, we split the sample of evaluation localities according to the median of the mean 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.
Note: This table reports means and standard deviations (in parentheses) for the numbers of neighboring beneficiary villages, children (aged 6–14) in those villages, and scholarship recipients, and the mean numbers and distribution of neighboring evaluation and treatment group villages within areas delimited by a five-kilometer radius around evaluation localities.

around each evaluation village. Those localities have an average total population of 834 children aged six to 14, of which, on 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 five kilometers, 57 (11 percent) have two such localities, and 16 (three percent) have three or more. Thus, 212 (41 percent) villages in the experiment have other evaluation villages in a five-kilometer radius. Our empirical analysis identifies the effects of cross-village externalities for these villages. On average, evaluation villages have, respectively, 0.62 other evaluation

7 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).

localities and 0.40 localities allocated to the experimental treatment group within a five-kilometer radius. 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 and 3). This is consistent with the targeting design of the *Progresa* intervention discussed above. In addition, and 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 and 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 five kilometers. Similarly, most localities do not have a junior secondary school—only 17 percent in the evaluation sample—while 93 percent have access to one or more junior secondary schools in other villages within five kilometers. 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 most 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.

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 within the evaluation localities.

The evaluation survey was intended to cover all 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 that 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. This answers 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. Also, secondary grade levels are 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

8 We 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.

9 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.

grades five or six of primary school or the first grade of secondary school.[10] We further reduce the number of observations in the data in order to generate a balanced panel of children observed at all rounds.

The resulting sample contains 6,690 children who are making the transition from primary to secondary school, remaining in secondary education or dropping out of school during the academic years 1998–1999 and 1999–2000. For 807 (12.6 percent) of children, no information was collected on either school participation or parental education, thereby leaving 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).

II. Program Externalities Across Villages

Empirical Strategy

Our identification strategy exploits two features of the program evaluation design: (i) the proximity between many evaluation villages; and (ii) village-level random assignment to treatment. The key intuition is that, after conditioning for 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 effect 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 distance 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 in the evaluation of the program, and not all beneficiary villages participated in the evaluation. Hence, neighborhoods of evaluation villages are comprised of other evaluation villages, non-evaluation beneficiary villages, and non-eligible villages. Let then $N^B_{j,d,t} = N^T_{j,d,t} + N^{NE}_{j,d,t}$ 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, $N^T_{j,d,t}$ is the number of evaluation villages that are randomly assigned to the treatment group of the evaluation and $N^{NE}_{j,d,t}$ is the number of other neighboring (non-evaluation) villages that are targeted by the intervention during each post-treatment period t. Now let $N^P_{j,d,t} = N^T_{j,d,t} + N^C_{j,d,t} + N^{NE}_{j,d,t}$ denote the number of *potential* program villages situated at distance d from village j in a given post-treatment period t, where we have added $N^C_{j,d,t}$, to indicate 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:

$$Y_{i,j,t} = \alpha_1 T_j + \alpha_2 N^B_{j,d,t} + \alpha_3 N^P_{j,d,t} + \alpha_4 X_{i,j,d} + \varepsilon_{i,j,d,t}, \tag{1}$$

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 that denotes whether or not locality j receives the program, $X_{i,j,d}$ is a column-vector of baseline characteristics at the individual, household, village, and neighborhood levels while $\varepsilon_{i,j,d,t}$ captures other unobservable determinants of the school participation decision which are potentially correlated with the targeting of the program.

In this framework, the parameter α_1 captures the sum of the average direct effect of program eligibility and the average indirect effects that stem from treatment of other households in the same village. Due to

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

11 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 may potentially introduce some measurement error in neighborhood characteristics and generate some attenuation biases in our estimates.

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 neighborhood-level spillovers stemming from the allocation of treatment among the evaluation localities. Finally, the parameter α_3 captures the effects of any unobserved determinant of the school participation decision that are correlated with the program geographic targeting.

The identification challenge is that more marginalized regions tend to have higher treatment densities (see table 1) due to a variety of unobserved factors associated with the geographic roll-out of the intervention, which are also likely to affect program outcomes. However, the random program assignment within the subset of evaluation villages provides some exogenous variation in 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 for the potential treatment frequency in the neighborhood $N^P_{j,d,t}$, cross-neighborhood variations in the frequency of the program are solely determined by the random allocation of neighboring evaluation villages 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: $N^B_{i,j,t} = N^P_{i,j,t} - N^C_{i,j,t}$. 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 $N^B_{d,t}$, are independent of that realized treatment frequency when controlling for targeted neighborhood treatment frequency $N^P_{d,t}$. Formally:

$$E[y^{T,N^B}_{i,j,t} \left| N^B_{j,d,t}, N^P_{j,d,t}\right.] = E[y^{T,N^B}_{i,j,t} \left| N^P_{j,d,t}\right.]. \tag{2}$$

Under this conditional independence property, comparisons of average outcomes across different levels of actual treatment frequency $N^B_{i,j,t}$, for example, n^B_1 and $n^B_2 > n^B_1$, at a given level of potential treatment frequency $N^P_{i,j,t}$, capture the causal effect of an increase in actual treatment frequency from n^B_1 to n^B_2. Formally (and omitting the indexes):

$$E[y^{n^B_2}|N^B = n^B_2, N^P] - E[y^{n^B_1}|N^B = n^B_1, N^P] = E[y^{n^B_2}|N^B = n^B_2, N^P] - E[y^{n^B_1}|N^B = n^B_2, N^P] = E[y^{n^B_2}|N^P] - E[y^{n^B_1}|N^P].$$

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 the balancing of baseline covariates with respect to the variation in local treatment frequency generated by the randomized experiment. Table 2 reports the means and standard deviations for those variables (columns 1 and 2), along with the associated OLS coefficients of the neighborhood treatment density term ($N^B_{j,d,t}$). 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 frequency. Consistent 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 ($N^P_{j,t}$). An F-test of joint significance

12 A partial population approach, exploiting the presence of ineligible households in beneficiary villages, can be followed, as it has been in previous studies. However, it 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.

Table 2. Neighborhood Treatment Frequency and Baseline Characteristics

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

Source: *Progresa* 1997 targeting and baseline survey and geo-referenced list of beneficiary localities. The sample contains program-eligible children in evaluation villages observed in October 1998 and November 1999, who, at baseline, are aged less than 18 and have completed grades five or six of primary school and the first grade of secondary school.

Note: This table reports means and standard deviations (columns 1–2) of the school participation (enrollment) outcome at baseline (October 1997) as well as the full set of covariates we employ in the empirical analysis. In columns 3–4, we display the OLS coefficient of the neighborhood treatment frequency term ($N^B_{j,5}$) on each of those baseline characteristics, respectively, without and with its potential counterpart ($N^P_{j,5}$) as a conditioning term. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parenthesis below each OLS coefficients. *significant at 10%; **significant at 5%; ***significant at 1%.

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 $N^B_{j,d,t}$, which is exogenous once controlling for another regressor—the density of potential program villages $N^P_{j,d,t}$ (note that 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 $N^P_{j,d,t}$ to be biased downward. However, the bias on that parameter is

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

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.[14]

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 exclusively affect the outcomes of children and families who are included in the program. We thus consider the following variant of equation (1):

$$Y_{i,j,t} = \beta_1 T_j + \beta_2 N^B_{j,d,t} + \beta_3 [T_j \times N^B_{j,d,t}] + \beta_4 N^P_{j,d,t} + \beta_5 [T_j \times N^P_{j,d,t}] + X_{i,j,d,t_0}\beta_6 + u_{i,j,d,t}, \tag{3}$$

where the village-level treatment assignment term (T_j) interacts 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 us to test whether or not program externalities differentially vary with treatment assignment ($\beta_3 \neq 0$).

To be more explicit on the parameter we estimate, note that our model is equivalent to one in which we are interested in the effects of the neighboring evaluation villages assigned to the treatment group, $N^T_{i,j,t}$, and we condition for the numbers of evaluation villages, $N^E_{i,j,t}$, and non-evaluation beneficiary villages, $N^{NE}_{i,j,t}$. This model writes:

$$Y_{i,j,t} = \alpha_1 T_j + \alpha_2 N^T_{j,d,t} + \alpha_3 N^E_{j,d,t} + \alpha_4 N^{NE}_{j,d,t} + \alpha_5 X_{i,j,d} + \varepsilon_{i,j,d,t}. \tag{4}$$

The same conditional independence property that stems from the randomized allocation into treatment of neighboring evaluation localities implies that $N^T_{i,j,t}$ is random conditional on $N^E_{j,d,t}$ and $N^{NE}_{j,d,t}$, that is:

$$E[y^{T,N^T}_{i,j,t} \,|\, N^T_{j,d,t}, N^E_{j,d,t}, N^{NE}_{j,d,t}] = E[y^{T,N^T}_{i,j,t} \,|\, N^E_{j,d,t}, N^{NE}_{j,d,t}]. \tag{5}$$

The α_2 parameter in equations (1) and (4) capture the effects of the same exogenous variation in neighborhood treatment density (that is, the spillover effect of the experimental treatment group villages), and the estimates obtained with these two models are the same.

In addition, we do not assume that the effects of spillovers are linear. We can account for non-linearities by using discrete variables indicating the specific numbers of neighboring treatment villages and use a flexible (or "granular") specification for the numbers of evaluation or non-evaluation localities in the neighborhood. Below, we report the estimates of equation (1) with one single parameter for the number of beneficiary villages as well as those of equation (4) with fully discretized controls for the numbers of experimental treatment, evaluation, and non-evaluation beneficiary localities.[15] While the former provides an average spillover effect, the later specification allows us to check for the presence of non-linearities in the marginal effects of neighboring evaluation localities assigned to treatment.

Finally, several other features of the empirical specifications depicted above should be noted. First, 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 five kilometers, we have such identifying variation for 42 percent of the evaluation villages.

Second, the inclusion of the vector of sociodemographic variables $X_{i,j,d}$ in equations (1), (3), and (4) is meant to increase the precision of the estimates. The control variables are all measured at

14 This statement is formally verified in section S2 of the supplementary online appendix.

15 Given the small number of experimental treatment localities within the neighborhoods in our sample (see table 1), we group them into two binary categorical variables according to the presence of one or two or more such localities (vis-a-vis zero) in the neighborhood.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Own Village Treated	0.097***	0.081***		0.095***	0.081***	
	(0.014)	(0.025)		(0.014)	(0.023)	
Actual Treatment Frequency		Villages			Eligible households (x100)	
# Treated in 0–5km	0.029*	−0.020	0.056***	0.039	−0.026	0.070**
	(0.015)	(0.023)	(0.020)	(0.024)	(0.042)	(0.035)
(# Treated in 0–5 km) × Treat		0.078**			0.10*	
		(0.033)			(0.056)	
Potential Treatment Frequency		Villages			Eligible households (x100)	
# Evaluation in 0–5km	−0.020	0.013	−0.047**	−0.017	0.038	−0.050
	(0.014)	(0.021)	(0.020)	(0.024)	(0.038)	(−.037)
(# Evaluation in 0–5 km) × Treat		−0.055*			−0.083	
		(0.030)			(0.056)	
# Non-Eval in 0–5km	−0.031**	0.017	−0.058***	−0.039	0.026	−0.069*
	(0.015)	(0.023)	(0.020)	(0.024)	(0.042)	(0.035)
(# Non-Eval in 0–5 km) × Treat		−0.077**			−0.097*	
		(0.033)			(0.057)	
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

Source: *Progresa* evaluation surveys and geo-referenced census of localities and secondary schools. The sample contains program-eligible children in evaluation villages, observed in October 1998 and November 1999, who, at baseline, are aged less than 18 and have completed grades five or six of primary school and the first grade of secondary school. It is restricted to treatment villages in columns 3 and 6.

Note: This table reports OLS estimates of cross-village program spillovers on school participation decisions following the specifications in equations (1) and (3). The dependent variable equals 1 if the child currently attends school. Columns 1–3 use the numbers of villages treated in a five-kilometer radius as a measure of treatment frequency. Columns 4–6 use the corresponding numbers of households over the same radius. Parameters are reported for the number of evaluation and non-evaluation potential beneficiary villages (columns 1–3) and households (columns 4–6) within the radius controls. Other controls include: baseline school enrollment; child's gender; age and age squared; parental education; distance to the nearest city; the share of eligible households; the presence of a secondary school in the locality; total population; the number of secondary schools; the mean degree of marginalization in the radius; state dummies; and a dummy for year 1998. Standard errors that are clustered at the level of groupings of partially overlapping neighborhoods are reported in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

baseline using the 1997 data in order 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 as follows: child's gender and age (both in levels and squares), parental education, distance to the nearest city, the share of eligible households, 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 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. These 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 five-kilometer radius, but we also use concentric radiuses of ten and 20 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 five-kilometer neighborhoods—the 320 treatment villages belong to 249 such clusters—and this number reduces to 180 when considering clusters formed by overlapping ten-kilometer neighborhoods and 45 with 20-kilometer ones.

Table 4. Granular Model of Spatial Spillovers

	(1)	(2)
Own village treated	0.091***	0.121***
	(0.013)	(0.018)
1 treated village in 0–5 km	0.061***	−0.016
	(0.022)	(0.030)
2 or more treated villages in 0–5 km	0.080**	−0.009
	(0.033)	(0.044)
(1 treated village in 0–5 km) × Treat		0.105**
		(0.044)
(2 or more treated villages in 0–5 km) × Treat		0.153**
		(0.065)
1 evaluation village in 0–5 km	−0.053**	0.074***
	(0.023)	(0.028)
2 evaluation villages in 0–5 km	−0.082**	−0.001
	(0.032)	(0.037)
3 evaluation villages in 0–5 km	0.066	0.122*
	(0.043)	(0.064)
4 evaluation villages in 0–5 km	−0.065	0.151
	(0.111)	(0.111)
5 evaluation villages in 0–5 km	−0.050	−0.058
	(0.048)	(0.069)
6 evaluation villages in 0–5 km	−0.021	−0.107
	(0.056)	(0.065)
dummies for # non-evaluation villages	Yes	Yes
dummies for # villages dummies	Yes	Yes
Number of Observations	11766	11766
R-squared	0.385	0.388
Number of Clusters	358	358

Source: *Progresa* evaluation surveys and geo-referenced census of localities and secondary schools. The sample contains program-eligible children in evaluation villages observed in October 1998 and November 1999, who, at baseline, are aged less than 18 and have completed grades five or six of primary school and the first grade of secondary school.

Note: This table reports OLS estimates of cross-village program spillovers on school participation decisions using a "granular," or fully discretized, specification of equation (4). The dependent variable equals 1 if the child currently attends school. The measures of treatment are the numbers of evaluation villages allocated to treatment within five kilometers (1 or 2 or more), and controls include indicating variables for the fully discretized numbers of evaluation villages, non-evaluation beneficiary villages, and total number of villages in a five-kilometer radius. Controls are also included for child's school enrollment at baseline, 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. Standard errors that are clustered at the level of groupings of partially overlapping neighborhoods are reported in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

Main Results

Tables 3 and 4 report 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).

The estimates in table 3 correspond to the model in equation (1) with continuous variables for the numbers of beneficiary, N^B, and potential beneficiaries, N^P. The estimates are obtained with two alternative measures of program frequency N^B in the areas surrounding the evaluation villages: the models in columns 1–3 use the numbers of villages treated in a five-kilometer radius, while those in columns 4–6 instead use 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. We report and discuss only the estimates of the parameters α_1 and α_2 but, as explained above, the regressions further include controls for the numbers of potential beneficiaries and for baseline characteristics observed in October 1997, notably baseline school enrollment.[16]

Column 1 of table 3 reports the estimates for the baseline model in equation (1) when measuring program frequency by the numbers of villages. It indicates that when considering the entire sample of children in treatment and control villages while living in a treated community increases school participation by 9.7 percent, having an additional treated village within a five-kilometer radius further increases enrollment rates by 2.9 percent (this spillover effect is statistically significant at the ten percent level). The estimated own-village treatment effect of the program is in line with the results obtained in previous studies (e.g., Schultz [2004]).

In order to document the heterogeneity of cross-village externalities by treatment status, column 2 of table 3 reports OLS estimates of the augmented model specified in equation (3) and column 3 estimates of the model in equation (1) obtained after restricting the sample to the treatment group.[17] 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 it does show evidence of strong spillovers for 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 percent, and this estimated differential effect is statistically significant at the five percent level.[18] 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 five-kilometer radius is estimated at 5.6 percent, and it is statistically significant at the one 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 to the corresponding ones in columns 1–3.

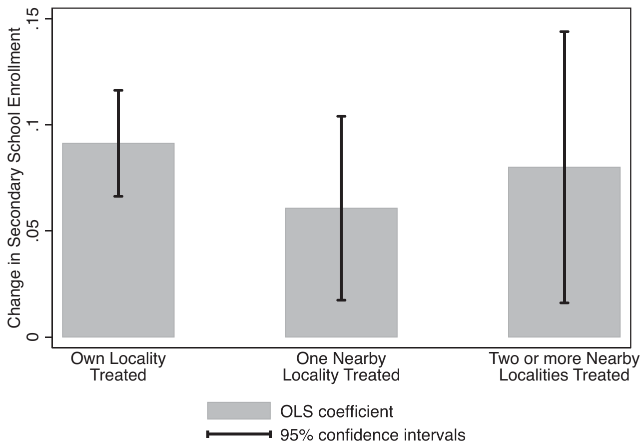
Table 4 reports the estimates for the model in equation (4); the effects of program frequency are captured directly by the number of treatment group villages N^T, which, as discussed above, is the source of variation in local treatment frequency that serves to identify spillovers in all our specifications. We use two indicator variables that indicate the presence of one or two or more treatment group villages in the neighborhood. These estimates also incorporate fully discrete controls for the numbers evaluation and non-evaluation beneficiary villages. We report and discuss the estimates not only of the parameters α_1 and α_2 but also of α_3 for the main conditioning variable, which is the number of evaluation localities in the neighborhood. The regressions further include indicator variables for each non-evaluation beneficiary village and total numbers of villages (we do not report the corresponding parameter estimates as these numbers can be very large), as well as for baseline characteristics observed in October 1997, notably baseline school enrollment. Figure 2 shows the visual representation of the estimates of the average spillover effects reported in column 1 of table 4. They indicate that a treatment group village in the neighborhood increases school participation by 6.1 percent, while two or more neighboring treatment group villages

16 The last rows of table 3 report the estimated coefficients of the conditioning term *NP* in equation (2), split into its two components, *NE* and *NNE*. 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.

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

18 Note that, when we allow for heterogenous effects of program spillovers, the relative OLS coefficient of the village-level treatment assignment term (β1) decreases only slightly (to 8.1 percent). 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. With 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.

Figure 2. Marginal Spillover Effects - Granular Specification



Source: Evaluation surveys and geo-referenced census of localities and secondary schools. The sample contains program-eligible children in evaluation 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.

Note: This figure depicts the OLS coefficients and the associated 95 percent confidence intervals on school participation decisions of the own village treatment term, along with those for the indicator variables for having one and two or more neighboring treated villages, respectively. Standard errors are clustered at the level of groupings of partially overlapping neighborhoods. All specifications control for baseline school enrollment. The full set of estimated coefficients of equation (4) is reported in table 4.

increase it by 8.0 percent. These estimates show that spatial spillovers do not increase linearly with the number of treated villages in the neighborhood. The point estimates reported in column 2 for the differential effect of spillovers in treatment villages as compared to control villages are larger in magnitude with increased enrollment rates of, respectively, 10.5 and 15.3 percent with one and two or more neighboring treatment villages but reveal a similar pattern.[19]

Further Evidence

To investigate whether spillovers operate over relatively short or larger distances, table 5 reports the OLS estimates of the model in equation (1) using measures of program frequency in neighborhoods covering larger distances over and above those of program frequency in the zero to five kilometer radius. As in table 3, we use the number of villages (columns 1–4) and eligible households (columns 5–8) as two alternative measures of program frequency. 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 do columns 3 and 6 of table 3). For the estimates in columns 3, 4, 7, and 8, we measure program frequency over a 20-kilometer radius and weight the observations in each village by the inverse of the distance to the centroid.

The estimated coefficients for the numbers of treated villages located at a distance between five and ten kilometers are small and statistically insignificant, whereas the corresponding estimates at a distance between zero and five kilometers barely change with respect to those presented in table 3. This suggests the presence of a strong decay rate in spatial externalities. However, the specifications using the

19 This non-linear shape of the spillover effects with respect to the frequency of nearby program beneficiaries can be related to a broad class of models of discrete decisions with strategic complementarities and/or the presence of threshold effects in the spillover function; see, for example, Brock and Durlauf (2001) and Glaeser and Scheinkman (2000). These cross-village effects are of the same magnitude or slightly higher than the ones that have been documented for the effects of program spillovers within-villages (from beneficiaries to non-beneficiaries) in the same setting, that is, around five percent (Bobonis and Finan 2009; Lalive and Cattaneo 2009).

Table 5. Specification Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own Village Treated	0.094***		0.096***		0.092***		0.094***	
	(0.016)		(0.009)		(0.015)		(0.010)	
Actual Treatment Frequency	Villages				Eligible households (x100)			
# treated in 0–5 km	0.025*	0.062***			0.040*	0.078**		
	(0.015)	(0.020)			(0.021)	(0.036)		
# treated in 5–10 km	−0.0058	0.014			−0.004	0.008		
	(0.016)	(0.015)			(0.019)	(0.020)		
# treated distance-weighted			0.029	0.087**			0.032	0.110**
			(0.025)	(0.042)			(0.026)	(0.043)
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

Source: *Progresa* evaluation surveys and geo-referenced census of localities and secondary schools. The sample contains program-eligible children in evaluation villages observed in October 1998 and November 1999, who, at baseline, are aged less than 18 and have completed grades five or six of primary school and the first grade of secondary school.

Note: This table reports OLS estimates of cross-village program spillovers on school participation decisions following the specifications in equations (1) and (3). 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 five kilometers, ten kilometers, and 20 kilometers with distance weights while columns 5–8 use the numbers of eligible households respectively within five kilometers, ten kilometers, and 20 kilometers with distance weights. Standard errors that are clustered at the level of groupings of partially overlapping neighborhoods are reported in parentheses. Control variables include: baseline school enrollment; the numbers of potential beneficiary villages (columns 1–4) or eligible households (columns 5–8); child's gender age and age squared; parental education; distance to the nearest city; the share of eligible households; the presence of a secondary school in the locality; total population; the number of localities; the number of secondary schools; the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. *significant at 10%; **significant at 5%; ***significant at 1%.

distance-weighted density measures computed over the 20 kilometer-radius confirm the presence of positive spillovers on school participation in treatment group localities. 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 focus on those operating over zero to five kilometers.

As a robustness check, we consider an alternative specification that instruments the actual treatment frequency in the neighborhood with the randomized treatment frequency. However, if we control for both the number of evaluation localities and non-evaluation beneficiary localities, the first stage becomes evidently mechanical with an additional treatment locality increasing by one the number of beneficiary localities in the neighborhood. In order to avoid such a mechanical first-stage relationship, we remove from the specification the variable corresponding to the number of non-evaluation program villages (N^{NE}). Note that we still have to control for the number of neighboring evaluation villages (N^{E}) in order to account for the fact that the identifying variation is non-zero for 42 percent of the evaluation villages (i.e., those that have at least another evaluation village in their radius of zero to five kilometers) and in order to assure that the exclusion restriction is valid (because of the correlation across village neighborhoods between N^{E} and N^{NE}).

The corresponding estimation results are reported in table 6. The point estimate of the first-stage parameter for the effect of an additional treatment group village on neighborhood treatment density is estimated at 1.43 (column 1). The corresponding t-statistic is 1.97 and the F test of the excluded instrument is 3.9, which is statistically significant at the five percent level. The reduced form coefficient for the effect of the number of treated villages in the zero- to five-kilometer radius on secondary school enrollment is 0.028 (column 2), which is very much in line with the corresponding estimate reported in column 1 of table 3. Column 3 reports the IV point estimate of the relationship between the frequency of neighboring program localities and secondary school enrollment, which is positive but not zsignificantly different from zero. The IV point estimate has a smaller magnitude than the OLS one reported in column 2, which reflects

Table 6. Spatial Spillovers of *Progresa* on Secondary School Enrollment: IV Estimates

Dependent Variable Estimator	Treatment intensity OLS (1)	School Enrollment OLS (2)	School Enrollment 2SLS (3)
Randomized Treatment Frequency	1.436**	0.028*	
(0–5 km)	(0.729)	(0.016)	
Actual Treatment Frequency			0.019
(0–5 km)			(0.018)
Own Village Treated	1.078**	0.096***	0.075***
	(0.526)	(0.014)	(0.023)
Weak identification tests:			
Kleibergen-Paap Wald rk F statistic	3.87		
$Prob > F(1, 79)$	0.0498		
Mean Dependent Variable	11.807	0.582	
Number of Observations	11766	11766	11766
R-squared	0.707	0.364	0.319
Number of Clusters	358	358	358

Source: *Progresa* evaluation surveys and geo-referenced census of localities and secondary schools. The sample contains program-eligible children in the *Progresa* evaluation localities with at least another evaluation locality in their close proximity (five kilometers), observed in October 1998 and November 1999, who, at baseline, are aged less than 18 and have completed grades five or six of primary school and the first grade of secondary school.

Note: This table reports OLS and 2SLS estimates of cross-village spillovers on school enrollment decisions. The dependent variable equals 1 if the child currently attends school. All specifications include the treatment assignment indicator at the locality-level and the number of evaluation villages in the neighborhood, and 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; the presence of a secondary school in the locality; total population; the number of localities; the number of secondary schools; the mean degree of marginalization in the radius; state dummies and a dummy for year 1998. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

the larger-than-one first-stage relationship and is estimated with more noise due to the reduced statistical power in this specification.

As a last robustness exercise, we consider alternative measures of program take-up based on the health component of the program. We use household-level information from the post-program survey round of October 1998 on the uptake of three screening tests that form part of the health requirements of the *Progresa* program: hypertension (blood pressure test), diabetes (blood sugar test), and cervical cancer (via the PAP smear test).[20] Table 7 reports the OLS estimates of both own-village treatment effects and the spatial spillover effects stemming from neighboring program villages on the probability that the households in the sample comply with the health conditionality of the program. The estimates reveal the presence of a strong effect of the program on the probability that eligible households comply with its health requirements, as confirmed by the positive and large marginal effects associated with the variable indicating whether or not the own village of residence of the households in the sample received the program.[21] The estimated coefficients of cross-village externalities are reported in columns 1, 3, and 5 of table 7. These are also positive but not significantly different from zero with the exception of the specification using the

20 The household respondents were asked whether or not any household member had been screened for these conditions in the previous six months. In order to maintain full comparability with the estimates of program spillover reported in the paper, we estimate at the household-level the linear model reported in equation (1) in the paper on the same sample that we consider in the main empirical analysis. Results (available upon request) are very similar in both magnitude and precision if we instead consider the larger sample of all program-eligible households in October 1998.

21 The fact that program treatment status varies at the village level implies that such estimates capture both the direct effect of the program as well as the average indirect effects, which stem from the treatment of other individuals in the same village.

Table 7. Spatial Spillovers of *Progresa* on the Uptake of the Health Screening Tests

Chronic Disease Screening Test	Hypertension Blood Pressure		Diabetes Blood Sugar		Cervical Cancer PAP smear	
	(1)	(2)	(3)	(4)	(5)	(6)
Own Village Treated	0.205***	0.241***	0.162***	0.192***	0.134***	0.204***
	(0.021)	(0.032)	(0.024)	(0.039)	(0.023)	(0.038)
# Treated Villages in 0–5 km	0.039	−0.031	0.058*	−0.017	0.033	0.008
	(0.032)	(0.042)	(0.032)	(0.043)	(0.031)	(0.027)
# Treated Villages in 0–5 km × Treat		0.114**		0.123**		0.044
		(0.053)		(0.056)		(0.046)
Mean of Dep. Var. in Control Group	0.374		0.298		0.252	
Number of Observations	4522	4522	4523	4523	4522	4522
R-squared	0.102	0.108	0.073	0.078	0.091	0.094
Number of Clusters	358	358	358	358	358	358

Source: *Progresa* evaluation surveys and geo- referenced census of localities and secondary schools. The sample contains program-eligible households residing in *Progresa* evaluation villages observed in October 1998 whose children, at baseline, are aged less than 18 and have completed grades five or six of primary school and the first grade of secondary school.

Note: This table reports OLS estimates of cross-village spillovers on the likelihood of compliance with three screening tests that form part of the health requirements of the *Progresa* program following the specification in equation (1). The dependent variables equals 1 if at least one household member reports having being screened in the previous six months for the corresponding chronic condition. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parentheses. All specifications control for baseline school enrollment. The other control variables include: the potential number of beneficiary villages; parental education; distance to the nearest city; the share of eligible households; the presence of a secondary school in the locality; total population; the number of localities; the number of secondary schools; the mean degree of marginalization in the radius; and state dummies. *significant at 10%; **significant at 5%; ***significant at 1%.

uptake of the blood sugar test as dependent variable (column 3), which features an estimated externalities coefficient that is significant at the ten percent level.

We next estimate the heterogenous effect model reported in equation (3). The corresponding estimates are reported in columns 2, 4, and 6 of table 7. Externalities coefficients for households residing in treated villages are more precisely estimated and larger in magnitude when compared to their average counterparts (except for the PAP smear test) whereas those for control villages cannot be distinguished from zero. An additional neighboring program village increases compliance with health screening by roughly ten percent for households who reside in a program village or about half of the “own-village” treatment effect and a 20–30 percent increase vis-a-vis the corresponding mean in the control group. These estimates of health spillovers are broadly consistent with those of the enrollment spillovers reported in table 3 both in terms of incidence (they accrue exclusively among program participants) and of the magnitude. They are slightly more imprecise though, possibly due to the smaller sample size resulting from household-level regressions and one year of program follow-up data.

III. Mechanisms

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 above. 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.[22] As we do not have measures of the occurrences of interactions of beneficiaries from different neighboring villages, we cannot report direct evidence of this.

22 Non-market interactions may affect take-up decisions through two channels: information and social norms. While conceptually different, these 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.

Table 8. Knowledge About the Program Components

	Scholarship (1)	Health (2)	Nutrition (3)
# Treated Villages 0–5 km	0.045**	0.082***	0.011
	(0.022)	(0.029)	(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

Source: Progresa evaluation surveys, geo-referenced census of localities and secondary schools, and *Progresa* follow-up survey of recipients. The sample contains program-eligible households residing in treated villages observed in October 1998 whose children, at baseline, are aged less than 18 and have completed grades five or six of primary school and the first grade of secondary school.

Note: This table reports OLS estimates of cross-village externalities on dichotomous indicators of whether or not recipients know the various components of the benefits package of the program following the specification in equation (1). Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parentheses. The following baseline control variables are included in each specification: the potential number of beneficiary villages within five kilometers; child's gender age and age squared; parental education; distance to the nearest city; the share of eligible households; the presence of a secondary school in the locality; total population; the number of localities; the number of secondary schools; the mean degree of marginalization in the radius; state dummies; and a dummy for year 1998. *significant at 10%; **significant at 5%; ***significant at 1%.

Hence, we 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.

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 healthcare and health check-ups. Most of the respondents who were to receive the transfers were mothers. 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 the women who are the primary recipients of the transfers and regularly encounter each other during program operations. When asked, in the same operational follow-up survey, to mention the most significant changes within their communities, half of the beneficiaries reported 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 8 reports the estimates, which are obtained using the model in equation (1) estimated at the household-level using the October 1998 data. Having one additional neighboring program village increases by 4.5 and 8.2 percent, respectively, the share of eligible households that are aware of the education and health components (columns 1 and 2), and has a smaller and no significant effect on the share of households aware of the nutritional component (column 3).

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

Supply-Side Effects

Areas with higher densities of program participants may 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 evenly benefit those program recipients who reside within it. This is at odds with the evidence reported in column 1 of table 5 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 five-kilometer radius there are on average ten 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 much different in neighborhoods that have more treatment-group villages compared to those that have more control-group villages, 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.[23] Second, we use information from the operational follow-up survey in order to construct the share of program recipients in treated villages that received the E1 form as of May 1999.

23 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 eight percent six months or more after incorporation into the program. These administrative delays appear concentrated in some regions, 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.

Table 9. 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
	Scholarships	School Supplies	Received of E1 Form
	(1)	(2)	(3)
# Villages treated in 0–5 km	−0.007	−0.043	0.001
	(0.140)	(0.132)	(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 (0–5 km)

	Number of Schools	Number of Teachers	Share of High Ed. Teachers
	(1)	(2)	(3)
# Villages treated in 0–5 km	0.045	−1.121	−0.030
	(0.080)	(0.767)	(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

Source: geo-referenced census of localities and secondary schools, and Progresa follow-up survey of recipients. The treated villages observed in 1998 are in panel A and the evaluation neighborhoods observed in both 1998 and 1999 are in panel B.

Note: This table reports OLS estimates of the effects of local treatment frequency on measures of the program's supply-side. Standard errors clustered at the level of groupings of partially overlapping neighborhoods are reported in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%.

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.[24] 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 9, these three 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 neighborhoodlevel the linear model reported in equation (1) over the two years of the

24 Results (available upon request) barely change when we consider instead 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: three villages for scholarship disbursements, four villages for school supply disbursements, and seven villages for the receipt of the school attendance (E1) form.

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 9, which are very small in magnitude and not statistically different from zero.

IV. Conclusion

We examined, in the context of the *Progresa-Oportunidades* conditional cash transfer program, whether or not 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 positive spillovers within networks of beneficiaries spanning those areas. A higher local frequency of program beneficiaries increases 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 into 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 neighborhoods were associated with increased knowledge among eligible households of the schooling and health components of the program. We also tested the alternative hypothesis that the spillover effect that we estimate instead reflects heterogeneities in direct treatment impacts due to spatial variations in the implementation of the program. The evidence we obtained 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 the target population of an intervention 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 requires the analysis of the impacts of the program at the level of relatively large geographical areas or clusters. The feasibility of this option critically hinges upon the scale of the program that is being evaluated, and statistical power reasons may push the researcher to opt for a narrower definition of the evaluation clusters. These considerations can be particularly important for policy interventions that are evaluated at scale, a setting which likely differs from the evaluation of small pilot programs.

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