Spatial Externalities and Social Multipliers of Schooling Interventions*

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Abstract

We study the effects of social interactions on school attendance decisions within a large-scale social program in rural Mexico. In order to identify program externalities across localities, we use exogenous variations in the number of beneficiary localities in the proximity of each village induced by the experimental evaluation design. We find evidence of large and positive spillover effects on secondary school enrollment decisions amongst children in the treated group, but no evidence of such effects for children in the control group. This marked heterogeneity sheds light on the underlying mechanism through which social interactions operate in our setting. Notably, individuals do not seem to influence each others through schooling behaviors. It is instead the intervention which is likely to have triggered interactions amongst the targeted population, thereby enhancing individuals' perceptions of the benefits of education. Our preferred estimate implies a substantial social multiplier effect of the program: a one standard deviation increase in the number of beneficiary localities in the surroundings of each village increases enrollment rates by 4.1 percentage points.

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

JEL Codes: C9, I2, J2, O2.

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1 Introduction

Education plays a fundamental role in the development process. At both the macro (for example, Krueger and Lindahl [2001]) and micro level (Angrist and Krueger [1991] and Duflo [2001] among others), there is strong evidence that private and social rates of return to education are high in poor countries. However, low enrollment rates remain an important concern for much of the developing world. Despite significant improvements over the past forty years, the overall secondary school gross enrollment rate in 2000 was only 54% among low-income countries [Glewwe and Kremer, 2006].

Economists have provided important insights for understanding what determines whether or up to what attainment children are educated. The classic human capital theory postulates that households decide to invest in schooling by comparing the future benefits, including non economic ones, they expect from higher educational attainments to the direct costs of enrollment and the opportunity costs of the time required to attend schools. This model also accounts for the liquidity constraints that may prevent households to invest in education. More recent studies show that intra-households decision making and imperfect information on the expected benefits of schooling can also influence the education decision made by children and their parents.

Yet, while those studies focus on the individual costs and benefits of acquiring further education, other scholars have long pointed out that schooling behaviors are also affected by the interplay between children and their families with peers in the neighborhood, community or classroom (Coleman [1961] and Wilson [1987]). Social interactions of this sort can stem from a variety of mechanisms. For instance, individuals may desire to conform with others in their reference group due to either peer pressure or social norms (Bernheim [1994], Akerlof [1997] and Akerlof and Kranton [2002]). Additionally, there may be informational externalities as parents learn about the benefits of schooling from the actions of their peers (Banerjee [1992] and Bikhchandani et al. [1992]). Finally, interactions within schools or classrooms may generate important strategic complementarities in student learning and teachers' effort [Lazear, 2001] which may attract students to school.

Whatever their underlying channel, social interactions have key policy implications, as they can potentially trigger multiplier effects of interventions aimed at stimulating school participation.

Demand-side subsidies for education are increasingly common in developing countries. These interventions seek to foster the accumulation of human capital among poor households by reducing the private cost of education (i.e. through the waving of school fees, the provision of school uniforms or books) or, going beyond zero direct cost, in making transfers to families conditional on sending their children to school (i.e. through cash grants or free lunches). Some related interventions have instead relied on merit scholarships, remedying education after the class, or vouchers for private secondary schools.

Conditional cash transfer (CCT) programs have been adopted by several countries, specially in Latin America.[1] These programs subsidize human capital investment through monetary transfers that are conditional to schooling, and in some cases also to health care. Moreover, they are usually targeted to poor households in remote rural areas, so that a large share of the local population receives them.

Interventions of this sort may also positively affect the perceived benefits of schooling by shaping parents' aspirations toward the education of their children. Moreover, while reducing the private costs of schooling is unlikely to generate any social effect, any induced change in individual perceptions toward education may be amplified through social interactions.

The delivery of monetary incentives has been proved to be an effective mean to directly encourage the schooling investments of poor households (see Parker et al. [2008] for a review). However, there is still much to learn about the indirect effects that those interventions may generate due to the interplay between program recipients.

In this paper, we study the effects of social interactions between beneficiaries living in different but neighboring localities on school attendance decisions in the context of the conditional cash transfer program *Progresa - Oportunidades* in Mexico. Two features of this intervention makes it a particularly suitable setting for our purposes. Notably, while the very high coverage of the program in rural areas makes externalities across villages likely, the concomitant randomized evaluation design allows us to identify the effects of program density on individual schooling responses.

More specifically, we use detailed GPS information to localize the rural communities in the surroundings of our villages. Random program treatment assignment implies that, conditional on the number of neighboring evaluation villages, the num-

[1] Virtually every country in Latin America has such a program. Elsewhere, there are large-scale interventions in Bangladesh, Indonesia, and Turkey, and pilots in Cambodia, Malawi, Morocco, Pakistan, and South Africa, among other countries (World Bank, 2009).

ber of those assigned to the treatment or comparison group is random, thereby allowing to isolate exogenous variations in the density of the program in the proximity of each locality in our sample.

We find evidence of large and positive program externalities on secondary school participation decisions. The magnitude of this effect is both economically and statistically important and amounts to roughly one quarter of the direct program impacts. Moreover, externalities are entirely concentrated amongst children belonging to the treatment group, while there is no evidence of such effects for children in the control group.

This marked heterogeneity sheds light on the underlying mechanisms through which social interactions operate in our setting. Notably, individuals do not seem to influence each others through schooling behaviors. It is instead the intervention which has induced interactions amongst the targeted population, thereby amplifying individuals' perceptions of the benefits of education.

In order to corroborate this hypothesis, we first show that households who benefit from the health subsidy are more likely to take-up the school scholarship and to share information between each others in neighborhoods with high program coverage, thereby providing indirect evidence of social interactions positively associated with the program exposure. As more direct evidence that those interactions have amplified the perceived benefits of schooling, we report that parents' aspirations toward the education of their children appear positively affected by the number of beneficiaries in the neighborhood.

We further provide some additional evidence that enables to discard alternative interpretations which may be potentially driving our results. First, we show that liquidity constraints are unlikely to have prevented households in the control group from adjusting their enrollment decisions in response to the density of the program. Provided that all individuals positively respond to peer group behaviors, we should observe positive neighborhood effects on our measure of parents' aspirations toward schooling, whereas we find no such effects on households who don't receive the program. Moreover, we did not find any heterogenous responses to program externalities vis-a-vis the asset holdings of households in the control group, thereby suggesting that liquidity constraints are unlikely to explain our findings.

Furthermore, we check for the presence of equilibrium effects in the local economy which may have altered households constraints and choices beyond schooling. We find no relationship between households' welfare (as proxied by food consumption and total expenditures) and our measure of program density in locality neighborhoods.

Finally, we verify that our results are not spuriously capturing differences in program implementation across regions by using a range of objective and subjective measures of program effectiveness. We find no relationship between those variables and the degree of program coverage across neighborhoods.

We argue that this evidence is consistent with the the program having induced social interactions which have in turn spread positive attitudes toward education, thereby generating some multiplier effects of the policy impacts. We suggest two self-reinforcing mechanisms which may rationalize this hypothesis. First, the program may have encouraged information flows exclusively amongst the targeted population through some of its operational and logistic requirements. Second, the intervention may have shaped recipients' reference groups through social norms and conformity behaviors.

1.1 Related Literature

This paper builds on the empirical literature devoted in detecting the existence and estimating the magnitude of social interactions in policy interventions. Manski [2000] and Moffitt [2001] suggest using group-level randomization of treatment to estimate spillover effects. Recent studies which employ random variation in peer group composition to estimate the influence of group behaviors on individual policy responses include Duflo and Saez [2003] on the role of information in retirement plan decisions, Miguel and Kremer [2004] on the effects of a deworming program on school participation and Kling et al. [2007] on residential neighborhood effects

In the context of the *Progresa* program, Bobonis and Finan [2009] and Lalive and Cattaneo [2009] exploit the randomized design of the evaluation to identify spillover effects from eligibles to ineligibles within beneficiary villages. They both find positive externalities of the program on secondary enrollment of ineligible children and interpret this as evidence of social interactions. Whereas Angelucci et al. [2010] build family networks in the same village and find that the program only raises secondary enrollment among beneficiary households that are embedded in such a network. They interpret this as indirect evidence of resource sharing within family networks between households with different exposure to the program schooling requirements.

More broadly, a substantial body of literature has studied the influence of peers for individual schooling performances within schools or classrooms. For instance, Hoxby [2000], Sacerdote [2001], Zimmerman [2003], Angrist and Lang [2004] use group composition to instrument peer outcomes, thereby making it difficult to disentangle strategic complementarities in outcomes from externalities based on individual exogenous characteristics. Cipollone and Rosolia [2007] and Kremer et al. [2009] use instead partial-population experiments which exogenously alter peers' outcomes but do not affect their characteristics.

Finally, our study is related to some recent papers which have considered the role of parents' and youths' expectations about the benefits of education in schooling decisions. Attanasio and Kaufmann [2009] find that measures of expected idiosyncratic returns to schooling matter for both high school and college decisions in Mexico. Jensen [2010] shows that students in the Dominican republic tend to underestimate the returns to education; he finds that providing information of the returns estimated from earnings data to a randomely selected subset of schools increases school enrollment. Finally, Chiapa et al [2010] reports that the exposure to educated professionals in the *Progresa* program increases parents' educational aspirations for their daughters.

This paper is organized as follows. Section 2 describes the program's setting, the data we employ and provides some descriptive statistics which shed light on our context. Section 3 presents the empirical strategy and the main estimates of spatial externalities on school enrollment, while in Section 4 we investigate the mechanisms underlying our findings and check the sensitivity of our interpretation vis-a-vis some alternative hypotheses. Section 5 concludes with some policy implications.

2 Context and Data

Several features of our context are likely to spur social interactions amongst individuals living in neighboring localities. First, the rural villages included in the program during the period under consideration are very small (the median locality population is 59 households) and literally surrounded by other beneficiary localities. Second, many infrastructures of public services are shared by neighboring villages. In particular, while most of the communities do not have schools and health clinics, individuals can access them in neighboring localities, thereby interacting with their neighbors. Finally, some of the program requirements induce beneficiaries who live in neighboring villages to encounter and share information about the program and its objectives.

In what follows, we first briefly illustrate the program and its process of geographic expansion. We then describe the experimental design of the evaluation and the various data sources that we have gathered in order to characterize the surroundings of each evaluation village. Finally, we provide some descriptive statistics on schooling and discuss in further detail some characteristics of the rural setting we consider and some of the program operations which are likely to induce interactions amongst recipients.

2.1 The *Progresa* Program

Initiated in 1997 and still ongoing, *Progresa* is a large scale social program which seeks to foster the accumulation of human capital in the poorest communities of Mexico through the provision of cash transfers, which are conditional on specific family members behaviors in the key areas of nutrition, health and education. The educational component represents the largest share of the transfers and consists in scholarships and school supplies for children aged less than 17 years, conditional on their regular attendance of one of the four last grades of primary schooling (grades 3 to 6) or one of the three of junior secondary schooling (grades 7 to 9). Scholarships amounts increase with school grades and are higher for girls than for boys in secondary school. The health and nutritional components consist in fixed value food stipends conditional on all family members making regular visits to local health centers for check-ups and preventive care. The total amount of the transfers can be substantial: median benefits are equivalent to about 28% of the monthly income (and 21% of expenditures) of beneficiary families - thereby representing a sizable increase in households' economic resources. All the benefits are given directly to mothers on a bimonthly basis after verification of attendance in the relative facility (school or health clinic). [2]

The targeting of beneficiaries encompasses two stages. First, small localities in rural areas were selected to benefit the program based on their ranking according to a marginalization composite index, and their coverage by the networks of public schools and health centers. For logistical constraints, areas with larger numbers of localities satisfying the above criteria and more easily accessible by roads were given priority. Those criteria allowed to establish groupings of localities to be incorporated into the program successively each year. Second, eligible households were selected within beneficiary localities. Some information on covariates of poverty, and a corresponding means test, were obtained from a census of households fielded

[2]For a more comprehensive presentation of the program, see Sacerdote [2001].

in every new beneficiary locality one year before program implementation and used to identify eligible households.

The program thus began in 1997 with 300,000 beneficiary households in 6,300 localities, and expanded rapidly during the following years. In 1998, it was delivered to 40,700 localities and 1.9 million households, and in 1999, those numbers increased to 53,200 localities and 2.3 million households. The geographical expansion in rural areas went on during the next years, so that coverage reached 2.6 million households by 2001. Urban areas were included after that year, and the program now covers about 5 millions households.

2.2 Experimental Design and Analysis Sample

An experimental evaluation of the program impacts was conducted during its phase of geographical expansion in rural areas from 1997 until late 1999. To this aim, 506 villages were selected among a set of localities to be included in the program in 1998 and situated in 191 municipalities of 7 central states of Mexico [3]. Amongst them, 320 localities were randomly assigned to the treatment group and started receiving the program's benefits in March-April 1998, while the remaining 186 localities formed the control group and were kept out of the program until November 1999.

The first dataset used in this paper consists in the three evaluation surveys fielded in the 506 evaluation localities respectively in October 97 (baseline and first round), October 98 (third round), and November 99 (fifth round).[4] This dataset contains detailed information on the socioeconomic characteristics of a panel of households residing in the evaluation localities. These surveys were intended to be censitory, but a small share of the population was not interviewed at baseline and there were some changes in the villages population, so that the total numbers of households observed in the data are 24,077 in October 97, 25,846 in October 1998, and 26,972 in November 1999. There is also some sample attrition, as 8.4% of the 1997 households cannot be followed and matched in all three rounds of the survey.[5] Besides, in the baseline, 60% of the households are classified as eligible to receive the program benefits.[6]

[3]Namely: Guerrero, Hidalgo, Michoacan, Puebla, Queretaro, San Luis de Potosi and Veracruz.

[4]We discard the March 98 and June 99 rounds of the evaluation surveys so as to compare schooling outcomes net of seasonal variations.

[5]The attrition is undoubtedly partly due to migration out of the villages, but is mainly a reflection of errors in identification codes which occurred for a few enumerators in the second round.

[6]Around 3,000 households were classified as non poor in the baseline but were later re-classified as eligible. In order to avoid arbitrary classifications, we exclude them from our analysis.

In most of our analyses, we consider the sample of children who live in eligible households, who are less than 16 years old in 1998 and less than 18 years old in 1999, and have completed no less than the second and no more than the eighth school grade, so that they are eligible to the program. Due to the non-negligible attrition rate, rather than matching individuals in all three rounds of the survey, we consider an unrestricted pooled sample of all valid child observations and only use the panel sample for robustness checks.[7] Our main sample thus contains respectively 23,841 primary school level children and 14,003 secondary school children (6,794 girls and 7209 boys) observed in one of the two post-implementation period (October 1998 or November 1999).

We complement this dataset with information from a survey collected in November 1999 among the 7,237 program beneficiary households residing in the 320 treated villages with specific questions on their experiences with the program.

We further employ two additional datasets to study spatial externalities across localities. The first one is the administrative listing of the localities that were receiving the program at the closing of each fiscal year (September-October) in 1998 and 1999. For each beneficiary locality, the dataset contains the numbers of families that were receiving the transfers for the nutrition and the educational components of the program each bimester during the two years of evaluation. The second one is a locality census which contains the geodesic coordinates (longitude and latitude) for all rural localities in Mexico, along with demographic information. Those two data sources can be matched with the evaluation sample, using locality identifiers that are common to all datasets, thereby allowing to characterize the surroundings of the evaluation localities, and notably to document the numbers of evaluation and other beneficiary localities located within a given radius of the evaluation ones.[8]

Finally, we use a census of secondary schools collected by the Ministry of Education which contains detailed information on schools, teachers and students' characteristics as well as geodesic coordinates which thus allows us to impute the relative supply of education in each village neighborhood.

[7]The age limitations on the children reporting in the subsequent surveys, which may make the oldest and youngest groups in the matched panel sample unrepresentative, also contribute to the attrition for the sample of children. Our main estimates are nevertheless very similar when we consider the panel sample; the results are available upon request.

[8]In the empirical analysis, we define neighborhoods using only geodesic distances, and do not take into account the local geography (natural obstacles or communication axes such as mountains, rivers, or valleys) or transportation networks. This restriction should, if anything, introduce some measurement error in the neighborhood characteristics, and create some attenuation biases in our estimates of spatial externalities.

2.3 Descriptive Trends

2.3.1 Findings from the baseline

Pre-program school enrollment among program eligible children is high at primary level (89.6% for both boys and 90.3% for girls), but it drops sharply at junior secondary secondary level to 60.4% for boys and 47.9% for girls. Moreover, respectively 22.0% and 8.1% of eligible boys and girls having completed primary school and aged less than 17 are reported to be working for a wage or on the family business.[9] Secondary school enrollment is thus the most problematic decision for school participation and, perhaps not surprisingly, the grade levels where the program has had its greatest impacts, particularly for girls [Schultz, 2004].

Parents' aspiration about their children education are very heterogenous amongst the targeted population. On average before the intervention, 8.4% of the households expect their daughters to terminate school after the completion of primary level, 37.6% after junior secondary, 23.1% after senior secondary, and 30.9% after a college degree. And similar - albeit slightly higher - figures apply for boys. Although they might also internalize some of the constraints on schooling choices, this self-reported survey question should at least partly capture exogenous parental preferences with respect to education. This is confirmed by basic regressions results (available upon request) which show that parents with lower levels of education tend to place a significantly lower value on their children's academic achievements, after controlling for family income and measures of village poverty.

2.3.2 Program take-up

Almost all households entitled to receive the program (97% in 1998 and 98% in 1999) did take-up the food stipend benefits. However, a non-negligible share of households with children eligible to the scholarships and school supplies did not receive the educational component of the transfer throughout the evaluation period. The take-up of the school subsidy amounts to an average of 85% in 1998 and 1999.

This may be due to the different opportunity costs of the health and schooling behaviors that condition the receipt of the two transfer components. In fact, in spite of the increasing profile of the transfer amounts with school grades, take up rates of the scholarships are lower (72%) for households with children having completed primary school, thereby suggesting that for those children the school subsidy does

[9] A large number of girls perform domestic work, but the information on these activities was only collected from October 1998.

not fully compensate for the opportunity cost of the time a student withdraws from other activities to attend school.

However, some logistic and administrative inefficiencies might also have caused some delays in the delivery of the transfers. Indeed, while the food stipend has been distributed to all villages at the same time in March 1998, there is substantial variation in the delivery of scholarships and school supplies across localities. Thus, only 56% of the treated localities receive the first scholarship transfer in March 1998, whereas 36% of those received them two months later, and the remaining 8% only six months after incorporation into the program.

2.3.3 Locality Neighborhoods

In our sample, there are on average 20 localities situated within five kilometers, that is a walking distance, from each evaluation village. Amongst those neighboring localities, there are respectively 9 and 11 program beneficiary localities on average in 1998 and 1999, with substantial variation across neighborhoods.[10] Because of the geographic targeting of the intervention, the associated variation in treatment density across locality neighborhoods is unlikely to be random. Table 1 presents descriptive statistics for selected village and household characteristics, observed at baseline in October 1997, across quartiles of the distribution of program coverage within a 5 kilometers radius from each evaluation locality. Overall, program coverage tends to be higher in poorer areas. For instance, children in the upper quartiles tend to live in localities that are more marginalized and less likely to have a secondary school, to belong to households with lower income and less educated mothers and to attend schools which are more congested.

Out of the 506 evaluation localities, only 89 have a secondary school and 67 have a health clinic, while the inhabitants of those localities have access to on average of 3 secondary schools within 5 kilometers.

The process of transfers delivery is administered through temporary and mobile outposts (*Modulos de Atencion Progresa*, MAP thereafter) situated in junction beneficiary localities that serve the neighboring communities and further assist beneficiaries by conveying information about the program. None of the 320 treated villages in our sample served as a MAP, so that beneficiaries living in neighboring communities gather every two months in order to collect cash transfers. Moreover, in each village with at least 10 recipient households, beneficiaries select a local rep-

[10]Due to the concomitant scaling-up of the program, the number of beneficiary localities increases from 1998 to 1999.

resentative (the *promotora*) who serves as a liaison between themselves and the program personnel, providing beneficiaries with information on how the program works, when transfers will arrive, and identifying operational problems through regular meetings. Those meetings are often used as a forum for women to talk and share their views and experiences about the program and its requirements. On average, 70% of the evaluation villages have at least one community with less than 10 beneficiary households within 5 kilometers, so that *promotoras* are often in common between villages.

3 Program Externalities across Localities

In this section, we show how we can identify program externalities by taking advantage of experimental variations in the density of the treatment in the surroundings of each village. This rests on the geographical proximity between evaluation localities: 40% of them have at least another evaluation village situated within 5 kilometers. Disaggregated data show that, out of the 506 evaluation communities, 302 do not have another evaluation localities, 139 have one such locality, 51 have two, and 14 have three or more. Random program treatment assignment assures balanced neighborhood distributions for the treatment and comparison groups. Table 2 displays descriptive statistics for beneficiary localities that did not take part to the evaluation together with the evaluation localities and their relative shares of randomized-out villages situated respectively within 5 and 10 kilometers from the evaluation localities.

3.1 Empirical Strategy

We consider a reduced-form linear probability model which relates the school participation indicator variable, $y_{i,l}$, of child i in locality l, to a randomly assigned program treatment assignment indicator T_l, the number of program beneficiary localities, $N^B_{d,l}$, situated within distance d from each evaluation locality l, and the total number of localities, $N_{d,l}$, within the same distance d:[11]

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

[11] We alternatively consider the number of beneficiary households living in neighboring localities. Also, in spite of a discrete dependent variable, we discuss and estimate linear forms for simplicity of interpretation. We use Probit estimation as robustness check.

Individual disturbance terms $\epsilon_{i,l}$ are assumed to be independent across localities, but are allowed to be correlated within localities. In this framework, α_1 measures the direct treatment effect of the program, while α_2 captures the effect of having an additional beneficiary locality within distance d from each evaluation locality. The additional term $N_{d,l}$ captures any independent effect of neighborhood village-density on outcomes.[12]

The number of beneficiaries in the neighborhood, $N^B_{d,l}$, varies with the geographical targeting of the program. However, as documented in Table 1, the naive OLS estimation of equation (1), is likely to deliver biased estimates of the α_2 parameter. This is because targeting was not random, so that the variables that determine $N^B_{d,l}$, are also likely to affect (or be correlated with unobserved determinants of) children's educational outcomes.

However, the random assignment of the evaluation localities into the treatment and control group provides a source of exogenous variation in $N^B_{d,l}$. To see this, first write the number of neighboring beneficiary localities as the sum of beneficiary localities not selected for participation to the experiment and the evaluation localities randomly assigned to the treatment group, $N^B_{d,l} = N^{NE}_{d,l} + N^T_{d,l}$. Then, consider the allocation of nearby evaluation localities into the treatment and control group, $N^E_{d,l} = N^T_{d,l} + N^C_{d,l}$, and use it in order to decompose the neighboring beneficiary localities as follows:

$$N^B_{d,l} = N^{NE}_{d,l} + N^E_{d,l} - N^C_{d,l} \tag{2}$$

Random program treatment assignment across localities implies that the number of neighboring control localities ($N^C_{d,l}$) is random conditional on the number of neighboring evaluation localities ($N^E_{d,l}$), thereby generating variations in the local density of the treatment which are simultaneously orthogonal to the other regressors and the disturbance term in equation (1). In particular, variations in $N^B_{d,l}$ associated with the randomized experiment are independent from the geographical expansion of the program and remained unchanged over the evaluation period.

In order to explicit the relative bias of the other sub-components of the N^B term, plug (2) into equation (1) and take the linear projection of the residual $e_{i,l}$ onto the explanatory variables:

$$e_{i,l} = \delta_1 T_l + \delta_2 N^{NE}_{d,l} + \delta_3 N^E_{d,l} + \delta_4 N^C_{d,l} + \delta_5 N_{d,l} + u_{i,l} \tag{3}$$

[12]Note that this section omits background characteristics at household, village and neighborhood level for ease of exposition. All estimates do take the observed characteristics into account in order to improve precision.

where $u_{i,l}$ is the projection error which is orthogonal to all the covariates of our enrollment model.

Randomization implies that $\delta_1 = \delta_4 = 0$, so that (3) becomes:

$$e_{i,l} = \delta_2 N_{d,l}^{NE} + \delta_3 N_{d,l}^{E} + \delta_5 N_{d,l} + u_{i,l} \tag{4}$$

In this equation, δ_2 captures the effects of the targeting of the program on unobserved determinants of children's outcomes, δ_3 measures the potential non-random selection of the evaluation sample from the set of program beneficiaries and δ_5 represents other neighborhood-specific factors. By substituting (4) into (1), we obtain:

$$y_{i,l} = \alpha_1 T_l + (\alpha_2 + \delta_2) N_{d,l}^{NE} + (\alpha_2 + \delta_3) N_{d,l}^{E} - \alpha_2 N_{d,l}^{C} + (\alpha_3 + \delta_5) N_{d,l} + u_{i,l} \tag{5}$$

In this equation, the OLS coefficient of $N_{d,l}^{C}$ is an unbiased estimate of program externalities across neighboring localities. The composite coefficients of $N_{d,l}^{E}$ and $N_{d,l}^{NE}$, while they do not allow to separately identify externalities, they quantify the above mentioned biases. For instance, unobservables that are positively correlated with the number of neighboring program beneficiary localities and negatively affect children's outcomes ($\delta_2 < 0$) would lead to a downward biased estimate of the coefficient of $N_{d,l}^{NE}$.

Alike the key conditioning term $N_{d,l}^{E}$, the regressor $N_{d,l}^{NE}$ does not contribute to the identification of the parameter of interest and hence could in principle be included in the disturbance term. In this case, however, we would not be able to distinguish amongst the two potentially different sources of bias. In fact, provided that $Cov(N_{d,l}^{E}; N_{d,l}^{NE}) \neq 0$, the $N_{d,l}^{E}$ parameter would capture any independent effect of local treatment density on outcomes.

As a validation test for our identifying assumption we use data from the baseline in October 1997 and estimate equation (5) using educational outcomes and some of their determinants as dependent variables. Table 3 displays OLS estimation results. There are some statistically significant relationships between the number of evaluation ($N_{5,l}^{E}$) and beneficiary ($N_{5,l}^{NE}$) localities in the 5 kilometers neighborhood and various of those observables. However, none of those variables are significantly associated with the number of nearby control localities ($N_{5,l}^{C}$). Despite this evidence is limited to observables, it provides support for the randomness of the variation in treatment density associated with the evaluation design of the *Progresa* program.

3.2 Results

Table 4 presents OLS estimates for the post-intervention period (1998-1999) of the coefficients of equation (5). Column 1 displays our preferred estimates of marginal externalities for enrollment decisions of program-eligible secondary school children. We find that an exogenous decrease in the local density of the treatment decreases secondary school enrollment by 4.9 percentage points. This point estimate implies a substantial magnification effect of the program: a one standard deviation increase in the local treatment density (0.49), increases enrollment rates by 2.4 percentage points, that is nearly one quarter of the average treatment effect of the program (9.8%). We further note that the coefficient of the $N_{5,l}^{NE}$ term is negative and significant, which is indicative of the strong downward bias due to the program targeting ($\widehat{\delta_2} = -0.003 - 0.049 = -0.052$) and thus of the misleading naive estimate of treatment externalities that we would encounter without our experimental design.

Given the marked pre-program differences in secondary school enrollment rates between boys and girls (see Section 2), we split the sample according to the gender of the child. The results reported in Columns 2-3 show that the direct effect of the program is indeed higher for girls. Moreover, while externalities increase the secondary school enrollment of girls, we find some positive but not statistically significant effects for boys. Column 4 displays the relative estimates for primary school children, and finds no evidence of neighborhood externalities. This is not surprising since average pre-program primary school enrollment rates are very high and, accordingly, both the direct and the neighborhood effects are much lower. Moreover, all evaluation localities have a primary school, therefore making treatment externalities across localities less likely at this level.

We further exploit the random program treatment assignment within our sample in order to test whether neighborhood externalities take place exclusively amongst beneficiaries or instead they accrue to the overall population of poor children. Table 5 provides estimation results for the two separate samples of program-eligible children living in treatment and control group localities respectively. We find that treatment externalities are statistically significant only for children in treatment group localities (Column 1), with a point estimate for children in control group localities which is statistically insignificant and close to zero (Column 2). The relative test confirms that the effects for the two samples are significantly different from each others at the 5% confidence level (Column 3). The point estimate in Column 1 implies a substantial magnification effect of the program. A one standard deviation increase in the local density of the treatment increases secondary school enrollment rates of program beneficiary children by 4.1 points.

We finally restrict the analysis to the treated group and perform some specification checks. Column 4 tests for externalities over larger geographical areas. We introduce explanatory variables for the presence of evaluation and treatment group localities located at a distance from 5 to 10 km, in addition to the corresponding variables within a 5 km radius. We find no evidence of neighborhood effects at those distances. The estimated parameter for the treatment group localities at a 5 to 10 km distance is close to zero and statistically insignificant. This suggests that program externalities operate within very small areas surrounding the village. Finally, in Column 5 we consider an alternative and perhaps less restrictive definition of neighborhoods and compute for each localities the average of the nearby evaluation localities within a 20 km radius linearly weighted by the relative distance. Results are largely consistent with the previous ones.

4 Mechanisms

Individual school participation decisions are not only affected by the receipt of the program's benefits, but also respond to the number of neighboring program beneficiaries. Moreover, we do not find any evidence of program externalities on children who don't receive the program.

In what follows, we analyze in further details the mechanisms underlying this magnification effect of the program in order to shed light on the channels through which the intervention affects schooling behaviors beyond direct cash incentives. We argue that the program has enhanced social interactions amongst beneficiaries which have positively affected parents' attitudes toward schooling, thereby inducing a social multiplier effect of the policy. We first present some evidence in support of this hypothesis. Beside, we provide additional evidence against some alternative explanations which may be driving our results.

4.1 Social Interactions amongst Beneficiaries

As mentioned in Section 2, take-up rates of the food stipend were nearly universal. However, some eligible households did not take the scholarship component of the intervention. This may reflect cross-households differences in the opportunity cost of schooling, but also the low benefits some households expect from the secondary schooling of their children. While the involvement with the program activities may have affected parents' attitudes toward schooling directly, social interactions may have served as vehicle to propagate educational aspirations amongst the targeted population, thereby persuading some initially reluctant parents to enroll their children in school.

Some operational and logistic features of the program may have encouraged social interactions between beneficiaries (and exclusively them). Notably, the intervention requires the targeted population to encounter in some of the shared infrastructures such as health centers, mobile outposts (MAP) in which they collect the subsidies and regular meetings with the local program staff. Social interactions may have spurred information flows affecting households' preferences for and expectation from education. Moreover, social norms and conformity behaviors vis-a-vis other beneficiaries may have also changed parents' aspirations for their children's education. At the opposite, the lack of response of households who reside in control villages may be explained by their non participation to the program activities, and/or by perceptions of social exclusion and unfair treatment.

In order to test this hypothesis, we first restrict the sample to households with scholarship-eligible children who are reported to receive the food stipend transfer and consider whether the probability of taking up the education component of the program depends on neighborhood treatment density. Table 6 reports the results. While Column 1 shows no statistically significant relationship in the aftermath of the program (1998), there is a positive and significant effect in the second year (1999), as confirmed in Column 2 by the significant and negative coefficient of the N^C term. Furthermore, the model in Column 3 employs as dependent variable the share of beneficiaries who perceive community meetings as being useful to talk and share experiences after one year of program exposure (1999). The relative estimates of the experimental treatment density term is negative and statistically significant (at 10%), thereby providing some suggestive evidence in favor of social interactions amongst individuals after some time of program exposure.

We then evaluate whether parents' aspirations toward the education of their children are sensitive to treatment density in the neighborhood.[13] Table 7 reports the estimation results. Column 1 provides evidence that the benefit of the program has indeed induced parents to aspire for higher attainments of education. The direct

[13]As this information was provided only for children currently enrolled, we impute completed attainments for drop-outs assuming that their parents do not expect them to return to school. This adjustment is important for examining the effects of externalities on this outcome as it allows to rule-out selection issues in the sample of respondents.

exposure to the program increases by 0.6 years the desired attainments. Moreover, those educational aspirations seem to be also positively affected by the number of beneficiaries in the neighborhood, as confirmed by the significant and negative coefficient of the N^C term in Column 2. One standard deviation increase in the number of beneficiary localities in the neighborhood increases by 0.17 years desired attainments, and again this effect is statistically significant only for treated localities (0.26 years) and close to zero in control ones. Moreover, consistently with the higher indirect program effects on the enrollment of girls reported in Section 3, social interactions tend to increase more the desired attainments of girls (0.36 years) than the those of boys (0.16 years).

4.2 Endogenous Social Interactions and Liquidity Constraints

The non-response of children in control villages could be alternatively explained by some form of complementarity between endogenous social interactions and liquidity constraints. Indeed, the lack of economic resources seems to be a major factor in explaining non-attendance at school in this context, especially for older children, and this is consistent with the program design and the estimates of its direct impacts. Accordingly, all children in our sample (beneficiaries and non-beneficiaries) could have been sensitive to the schooling behaviors of their neighboring peers, but those who do not receive the transfers were unable to adjust their enrollment decisions due to liquidity constraints.

An indirect test for the absence of such a complementarity is provided by the results on educational expectations. Provided that all individuals positively respond to peer group behaviors, we should observe positive neighborhood effects on our measure of parents' aspirations toward schooling. However, as the results in Column 3 of Table 7 indicate, we don't find any significant effect for parents residing in control villages. This tends to reject that social interactions affect also non-beneficiaries.

As additional evidence that liquidity constraints are unlikely to explain our findings, we split the sample of non-beneficiaries according to the distribution of a composite asset index which measures households wealth. If it is true that liquidity constraints may have prevented non-recipients to respond, we may well expect heterogenous responses to program externalities along this dimension. Table 8 reports the results of enrollment responses across terciles of the distribution of asset holdings. The estimated coefficients of interest are always non-significantly different from zero, thereby suggesting that liquidity constraints are unlikely to explain our findings. [14]

4.3 Alternative Explanations

Beyond social interactions, two alternative explanations which are consistent with our findings require further scrutiny. First, cash injections in the local economy may have altered the functioning of some market thereby affecting households constraints and choices beyond schooling. For instance, Angelucci and Giorgi [2009] have documented that transfers to eligible households positively affect the consumption of ineligible households living in the same villages, through changes in insurance and credit markets. To the extent that those equilibrium effects also take place across neighboring villages between program beneficiaries, our findings of externalities on enrollment may simply reflect indirect responses to higher welfare.

We test whether the density of the program in some regions has increased beneficiaries' welfare. However, the absence of spatial externalities on non-beneficiaries is already in contradiction with this hypothesis. For a more direct test, Table 9 displays the relationship between households per adult equivalent monthly food consumption and expenditures (both food and non-food) and the random exposure to both direct and neighborhood program effects. Columns 1 and 4 confirm that the program seems to have improved household welfare, as shown by the positive and significant coefficient of the locality treatment assignment term. However, there is no evidence of any significant neighborhood effect on those measures. This tends to reject that equilibrium effects induced by the cash transfer spill over across villages.

Second, the areas with more numerous beneficiary localities might have been better assisted by the program administration, for example through a more prompt delivery of the cash transfers or some improvements in the supply side of the intervention such as increased school resources. If this was the case, our estimates would simply capture the effects of some heterogeneity in program impacts (positively) correlated with program density instead of externalities across villages.

In order to evaluate the presence of such differences in the effectiveness of the program across areas, we combine objective and subjective (i.e. self-reported) measures of program effectiveness. First, we use administrative data on transfer payments made during the experimental period to compute the number of month since incorporation after which the first disbursements were made to the localities assigned

[14] In order to make sure that this result is not due to lower identifying variation, we also report the mean and relative standard deviation of the experimental neighborhood density term (N^C) across the various sub-samples, which are very similar across sub-samples.

to the treatment group. Second, we consider a set of survey questions elicited to beneficiary households with detailed information on the quality of program implementation, as measures by the receipt of the form for school attendance monitoring (E1 form), the quality of the job of *promotoras* in each community, the knowledge of the different components of the cash transfer, and the effectiveness of the program in accomplishing its objectives. We thus estimate equation (5) using those efficiency of delivery indicators to test for their association with treatment density in locality neighborhoods. Table 10 reports the results. As documented in Columns 1-2, administrative delays seem more frequent in some states, notably Queretaro and San Luis Potosi, but those variations are not related to treatment density in the surroundings of the evaluation localities. Moreover, from the perspective of beneficiaries, the only outcome which appear positively and significantly related to the experimental variation in treatment density is the receipt of the E1 form, thereby providing some weak evidence in favor of a better administration in more treatment dense areas. In order to check that this is not driving our results, we have re-estimated our school enrollment model for the sub-sample of treated group children with the receipt of the E1 form as additional control variable. Results (available upon request) show that, in spite of a positive and significant effect of this regressor, the estimated coefficient of the N^C term barely change.

As additional evidence, we employ the secondary school census in order to construct neighborhood-specific measures of the supply-side component of the program both pre and post-intervention. Since we are interested in post-pre program changes in school supply conditions, we also control for the respective baseline values in the specification. Table 11 displays the results. The positive and significant coefficient of the N^{NE} term suggests that the enhanced supply of schools in areas with several treated localities (column 1) seems to have only partially matched the corresponding rise in enrollment. In fact, pupils per teacher and per class ratios are higher in treated dense areas (columns 2-3). Still, schooling conditions as measured by the share of students who fail (Column 4) and a school quality index based on self-reported information (Column 5) do not appear sensitive to those congestion effects. In spite of those correlations, none of the above indicators seem causally related with the treatment density induced by the experiment, as confirmed by the estimated coefficients of the N^C term.

Overall, those results are consistent with the hypothesis that the program has triggered social interactions between beneficiaries of neighboring villages, thereby enhancing parents' educational aspirations for their children. Moreover, the fact that beneficiaries' perceptions about the effectiveness of the program are not related to program density provides support for the view that people have shared information about the benefits of education and not simply about the functioning and/or the quality of the program.

5 Conclusions and Policy Implications

We have exploited the dense geographical coverage of the *Progresa* program and its experimental evaluation design to investigate the effects of social interactions in school participation decisions. We have used the random selection of localities within the evaluation sample, and the associated exogenous variations in the number of beneficiaries in the neighborhood of each village, to identify program externalities across localities.

We find evidence of large and positive spillover effects on secondary school enrollment decisions amongst program beneficiary children, but no evidence of such effects for non-beneficiary children. Our preferred estimate implies a substantial magnification effect of the program: a one standard deviation increase in the number of beneficiaries in the close surroundings of each village increases enrollment rates by 4.1 percentage points.

We then study the mechanisms underlying the observed complementarity between direct and indirect program impacts. We document that social interactions are unlikely to take place amongst children attending the same schools in our context. Instead, we argue that some specific operational features of the program have triggered information flows about the benefits of education amongst beneficiaries. Moreover, social norms and conformity behaviors within the targeted population may have further enhanced parents' aspirations toward their children education. Some additional evidence tends to discard alternative explanations underlying our findings represented by liquidity constraints in the response to social interactions and contextual changes in program effectiveness or local markets.

Our results can inform policy in several ways. First, they show that demand-side schooling interventions can be effective in fostering education beyond their direct economic incentives. Notably, the *Progresa* program has also changed people's perceptions about the value of schooling through the provision of information. Moreover, social interactions amongst neighboring individuals have amplified this information channel, thereby inducing a social multiplier effect.

Second, social interactions have also important implications for policy evaluation. In fact, they induce spatial externalities in the program's impacts, thereby making it difficult to disentangle the direct impact of the policy from its indirect effects.

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Table 1: Baseline Characteristics at Different Degrees of Program Coverage

Share of program villages within 5km	Q1 mean (sd)	Q2 mean (sd)	Q3 mean (sd)	Q4 mean (sd)
HH income	1145 (1048)	1066 (990.7)	993.6 (966.2)	873.1 (822.8)
Mother Educ	1.424 (2.428)	1.456 (2.469)	1.326 (2.329)	1.227 (2.278)
Share of Poor (Elig)	0.477 (0.50)	0.469 (0.50)	0.523 (0.50)	0.616 (0.49)
Population	378.2 (274.1)	324.7 (169.1)	415.1 (342.9)	345.5 (228.57)
Marg. Index	0.255 (0.74)	0.345 (0.70)	0.388 (0.68)	0.692 (0.70)
School in Locality	0.303 (0.46)	0.282 (0.45)	0.320 (0.47)	0.173 (0.38)
Student/Teacher	22.47 (9.60)	21.82 (10.44)	21.27 (5.99)	24.70 (7.87)
Student/Class	23.45 (9.16)	22.22 (7.96)	22.72 (6.59)	25.08 (7.04)

Table 2: Locality Neighborhoods: Descriptive Statistics

	1998		1999	
Non-Evaluation Beneficiary Localities				
	mean	(sd)	mean	(sd)
5 km radius	8.67	(7.31)	10.62	(7.68)
10 km radius	35.17	(25.04)	43.28	(26.84)
Evaluation Localities				
	mean	(sd)	mean	(sd)
5 km radius	0.58	(0.89)	0.58	(0.89)
10 km radius	2.34	(2.39)	2.34	(2.39)
Control Localities				
	mean	(sd)	mean	(sd)
5 km radius	0.21	(0.49)	0.21	(0.49)
10 km radius	0.84	(1.11)	0.84	(1.11)

Table 3: Baseline Characteristics and the Components of Treatment Density

	$N^{C}_{5,l}$		$N^{E}_{5,l}$		$N^{NE}_{5,l}$		Obs	R-sq
Enrollment	-0.022	(0.022)	0.038**	(0.016)	0.003	(0.002)	5637	0.004
Attainment	0.004	(0.027)	0.025	(0.019)	0.003	(0.003)	5675	0.001
Father Educ	0.011	(0.096)	0.191**	(0.076)	-0.012	(0.012)	5674	0.007
Mother Educ	0.024	(0.096)	0.127*	(0.073)	-0.018	(0.016)	5674	0.006
HH Income	34.966	(21.282)	-34.049*	(18.735)	-10.494***	(2.376)	12519	0.005
Population	18.364	(23.530)	-3.718	(24.750)	-6.375***	(2.100)	12519	0.015
Distance from City	-1.129	(3.818)	1.768	(3.237)	0.189	(0.538)	12519	0.001
Marg. Index	-0.047	(0.073)	0.061	(0.058)	0.017**	(0.008)	12519	0.013
School in Locality	-0.010	(0.039)	0.001	(0.030)	-0.007*	(0.004)	12519	0.008
Number of Schools	0.197	(0.179)	0.183	(0.137)	0.083***	(0.025)	12519	0.057
Student/Teacher	-0.476	(0.654)	1.255**	(0.502)	0.167*	(0.093)	11327	0.019

* significant at 10%; ** significant at 5%; *** significant at 1%.
NOTE: OLS estimates. Standard Errors in Parenthesis Clustered at the Locality Level.

Table 4: Direct and Indirect Treatment Effects on Enrollment

Sample	Secondary School			Primary School
	All	Female	Male	All
	(1)	(2)	(3)	(4)
T_l	0.098***	0.113***	0.084***	0.025***
	(0.017)	(0.021)	(0.019)	(0.007)
$N^C_{5,l}$	-0.049**	-0.062**	-0.033	0.009
	(0.021)	(0.027)	(0.022)	(0.008)
$N^E_{5,l}$	0.011	0.018	0.005	0.002
	(0.012)	(0.015)	(0.013)	(0.005)
$N^{NE}_{5,l}$	-0.003**	-0.004*	-0.003*	0.001*
	(0.002)	(0.002)	(0.002)	(0.001)
Controls	Yes	Yes	Yes	Yes
Number of Obs	14003	6794	7209	23841
R-squared	0.266	0.261	0.271	0.321
Number of Localities	500	487	489	505

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

NOTE: OLS estimates. Standard errors clustered at the locality level. Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities, the mean degree of marginalization in the radius; state dummies and a dummy for year 1998.

Table 5: Heterogeneous Treatment Externalities. Treated and Control Samples

Sample	Treated	Control	All	Treated	Treated
Radius	5 Km	5 Km	5 Km	10 Km	20 Km (dist weighted)
	(1)	(2)	(3)	(4)	(5)
$N^C_{5,l}$	-0.078***	-0.006	-0.001	-0.081***	-0.114**
	(0.027)	(0.030)	(0.030)	(0.028)	(0.050)
$N^C_{5,l} * T_l$			-0.081**		
			(0.040)		
$N^C_{10-5,l}$				-0.000	
				(0.014)	
$N^E_{5,l}$	0.019	-0.000	-0.009	0.026*	0.053**
	(0.014)	(0.018)	(0.018)	(0.015)	(0.021)
$N^E_{5,l} * T_l$			0.034		
			(0.021)		
$N^E_{10-5,l}$				0.001	
				(0.008)	
$N^{NE}_{d,l}$	-0.003	-0.004*	-0.003**	-0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.001)	(0.000)
Controls	Yes	Yes	Yes	Yes	Yes
Number of Obs	8815	5188	14003	8815	8815
R-squared	0.290	0.229	0.267	0.288	0.289
Number of Localities	318	182	500	318	318

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

NOTE: OLS estimates. Standard errors clustered at the locality level. Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities and the mean degree of marginalization in the radius; state dummies and a dummy for year 1998.

Table 6: Scholarships Take-up Conditional on Program Incorporation and Social Interactions Amongst Beneficiaries

	Scholarship Take-up 1998 (1)	Scholarship Take-up 1999 (2)	Share of mothers that interact during meetings (3)
$N^{C}_{5,l}$	-0.018	-0.042**	-0.057*
	(0.037)	(0.017)	(0.031)
$N^{E}_{5,l}$	0.051*	0.011	0.012
	(0.029)	(0.015)	(0.019)
$N^{NE}_{5,l}$	0.006**	-0.002	-0.004
	(0.003)	(0.002)	(0.003)
Controls	Yes	Yes	Yes
Number of Obs	4627	4489	6104
R-squared	0.056	0.029	0.022
Number of Localities	316	312	312

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

Note: OLS estimates. Standard errors clustered at the locality level. Baseline control variables include: parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities and the mean degree of marginalization in the radius and state dummies

Table 7: Parental Aspirations for Educational Attainments

	All	Treated	Control	Treated: females	Treated: males
	(1)	(2)	(3)	(4)	(5)
T_l	0.592				
	(0.104)***				
$N^C_{5,l}$	-0.339	-0.532	-0.088	-0.722	-0.328
	(0.121)***	(0.152)***	(0.190)	(0.197)***	(0.161)**
$N^E_{5,l}$	0.183	0.270	0.051	0.311	0.226
	(0.079)**	(0.097)***	(0.101)	(0.113)***	(0.106)**
$N^{NE}_{5,l}$	-0.003	-0.007	0.004	-0.012	-0.004
	(0.012)	(0.016)	(0.018)	(0.021)	(0.019)
Controls	Yes	Yes	Yes	Yes	Yes
Number of Obs	13102	8262	4840	3924	4338
R-squared	0.432	0.433	0.432	0.436	0.433
Number of Localities	499	317	182	307	312

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

NOTE: OLS estimates. Standard errors clustered at the locality level. Outcome is expected educational attainment in years. Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities and the mean degree of marginalization in the radius; state dummies and a dummy for year 1998.

Table 8: Liquidity Constraints for Non-beneficiaries

Sample	T1	T2	T3
	(1)	(2)	(3)
$\overline{N_{5,l}^{C}}$ $(sd(N_{5,l}^{C}))$	0.18 (0.44)	0.22 (0.49)	0.22 (0.49)
$N_{5,l}^{C}$	-0.039	0.042	-0.018
	(0.038)	(0.047)	(0.055)
$N_{5,l}^{E}$	0.019	-0.035	0.006
	(0.022)	(0.028)	(0.027)
$N_{5,l}^{NE}$	-0.004	0.000	-0.008**
	(0.003)	(0.003)	(0.004)
Controls	Yes	Yes	Yes
Number of Obs	1659	1566	1646
R-squared	0.223	0.250	0.270
Number of Localities	147	156	149

* significant at 10%; ** significant at 5%; *** significant at 1%
NOTE: OLS estimates. Standard errors clustered at the locality level. Outcome is secondary school enrollment indicator. T1-T3 represent tertiles of the distribution of the households asset holdings (the score). Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities and the mean degree of marginalization in the radius; state dummies and a dummy for year 1998.

Table 9: Market Interactions in the Neighborhood

	Food Comsumption			Total Expenditures		
	All	Treated	Control	All	Treated	Control
	(1)	(2)	(3)	(4)	(5)	(6)
T_l	8.518***			9.978***		
	(1.979)			(3.491)		
$N^C_{5,l}$	-1.986	-3.631	1.944	-2.486	-3.529	0.117
	(2.463)	(3.489)	(3.311)	(4.406)	(6.104)	(5.810)
$N^E_{5,l}$	-0.808	0.972	-4.334*	1.567	4.466	-2.255
	(1.525)	(1.689)	(2.414)	(2.699)	(2.857)	(4.273)
$N^{NE}_{5,l}$	-0.423**	-0.270	-0.666***	5.555***	6.390***	4.378***
	(0.166)	(0.233)	(0.236)	(0.333)	(0.410)	(0.447)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	23544	14672	8872	37370	23275	14095
R-squared	0.119	0.120	0.129	0.265	0.283	0.247
Number of Localities	506	320	186	506	320	186

* significant at 10%; ** significant at 5%; *** significant at 1%.
NOTE: OLS estimates. Standard errors clustered at the locality level. Baseline control variables include: child's gender and age, parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities and the mean degree of marginalization in the radius; state dummies and a dummy for year 1998.

Table 10: Program Effectiveness and Treatment Density

	Delays in Transfers Scholarship	School Supplies	Receipt of E1 Form	Quality of *Promotora*	Knowledge of Transfer	Program Effectiveness
	(1)	(2)	(3)	(4)	(5)	(6)
$N_{d,l}^{C}$	0.204	0.009	-0.036**	-0.029	0.021	-0.031
	(0.221)	(0.133)	(0.018)	(0.024)	(0.019)	(0.038)
$N_{d,l}^{E}$	-0.095	-0.120	0.024**	0.009	-0.020*	0.025
	(0.099)	(0.075)	(0.010)	(0.011)	(0.011)	(0.019)
$N_{d,l}^{NE}$	-0.003	-0.022	-0.000	-0.001	-0.002	-0.009***
	(0.015)	(0.015)	(0.002)	(0.002)	(0.002)	(0.003)
Hidalgo	-0.031	0.474	0.146***	0.124*	-0.068***	-0.261***
	(0.635)	(0.331)	(0.036)	(0.067)	(0.026)	(0.047)
Michoacan	-0.810	-0.175	0.151***	0.212***	-0.177***	-0.189***
	(0.546)	(0.252)	(0.036)	(0.067)	(0.031)	(0.053)
Puebla	0.894*	-0.362*	0.129***	0.220***	-0.044*	-0.195***
	(0.516)	(0.213)	(0.036)	(0.068)	(0.023)	(0.062)
Queretaro	1.766**	-0.460**	0.147***	0.130*	-0.026	-0.121**
	(0.716)	(0.216)	(0.042)	(0.074)	(0.036)	(0.058)
San Luis Potosi	1.230**	-0.512**	0.139***	0.094	0.054**	-0.089**
	(0.535)	(0.226)	(0.035)	(0.070)	(0.022)	(0.045)
Veracruz	-0.627	0.665**	0.145***	0.191***	-0.103***	-0.105**
	(0.567)	(0.324)	(0.038)	(0.061)	(0.029)	(0.048)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of Obs	316	315	5017	5846	6102	6132
R-squared	0.237	0.112	0.024	0.049	0.043	0.071
N. of Localities	316	315	312	312	312	312

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

NOTE: OLS estimates. Standard errors clustered at the locality level. Baseline control variables include: distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities and the mean degree of marginalization in the radius.

Table 11: School Characteristics in the Neighborhood

	(1) N. of schools	(2) Children/Class	(3) Children/Teacher	(4) Share Failed	(5) School Index
$N^{C}_{d,l}$	-0.069*	0.216	0.006	-0.003	0.011
	(0.041)	(0.339)	(0.354)	(0.003)	(0.094)
$N^{E}_{d,l}$	0.012	-0.235	-0.116	0.002	-0.072
	(0.034)	(0.216)	(0.292)	(0.002)	(0.045)
$N^{NE}_{d,l}$	0.011**	0.138***	0.193***	-0.001	-0.008
	(0.004)	(0.038)	(0.043)	(0.000)	(0.007)
N. of schools 1997	0.992***				
	(0.019)				
Children/Class 1997		0.591***			
		(0.046)			
Children/Teacher 1997			0.283***		
			(0.074)		
Share Failed 1997				0.612***	
				(0.079)	
Controls	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	No
Number of Obs	1012	925	926	926	5053
R-squared	0.943	0.577	0.466	0.569	
Pseudo R-squared					.0178
Number of Localities	506	463	463	463	312

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

NOTE: Columns (1)-(4): OLS estimates. Robust standard errors. Control variables include: distance to the nearest city, total population in the radius, the mean degree of marginalization of localities and the number of localities in the radius; state dummies and a dummy for year 1998. Column 5: ordered Probit estimates. Standard errors clustered at the locality level. Baseline control variables include: parental education, distance to the nearest city, the share of eligible households and the presence of a secondary school in the locality; total population, the number of localities, the mean degree of marginalization in the radius and state dummies.