

Endogenous Peer Effects in School Participation*

Gustavo J. Bobonis and Frederico Finan

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Abstract: A remaining obstacle in the literature on peer effects has been the inability to distinguish between peer effects that are determined by a person's reference group behavior (endogenous peer effects), and effects that are generated as a result of specific background characteristics of the groups themselves (contextual peer effects). This paper identifies and estimates endogenous peer effects on children's school participation decisions using evidence from the Mexican PROGRESA program, a school subsidy program targeted at children of the rural poor. Because program eligibility was randomly assigned, we use this exogenous variation in school participation to identify peer effects on the school enrollment of ineligible children residing in the same communities. We find that peers have considerable influence on the enrollment decision of program-ineligible children, and these effects are concentrated among children from relatively poorer households. Our findings imply that educational policies aimed at encouraging enrollment can produce large social multiplier effects.

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Contacts: G. Bobonis, Department of Economics, University of Toronto, Sidney Smith Hall, 100 Saint George Street Room 4057, Toronto, Ontario, M5S 3G3, Canada. Tel: 416-946-5299. E-mail: gustavo.bobonis@utoronto.ca
F. Finan, Department of Economics, UCLA, Bunche Hall, Box 951477, Los Angeles, CA 90095-1477, USA. Tel: 310-794-5958. Fax: 310-825-9528. E-mail: ffinan@econ.ucla.edu.

1. Introduction

Recent empirical studies have made important contributions towards identifying the causal impact of neighborhood or peer effects on individual behavior. Sacerdote (2001) and Zimmerman (2003), using the random assignment of roommates in U.S. colleges, find evidence of peer effects on the level of academic effort and membership in social organizations of college students. Kling, Ludwig, and Katz (2005) use experimental variation in assignment to different types of voucher relocation programs in five U.S. cities to identify long-term neighborhood effects on youth crime and find differential effects among females and males.¹

While the use of experimental designs have enabled these studies to properly identify social interactions, a remaining obstacle in the literature has been the inability to distinguish between peer effects that are determined by the behavior of a person's reference group (endogenous peer effects), from those that are generated as a result of specific background characteristics of the groups themselves (contextual peer effects) (Manski 1993). The distinction between these two effects has important policy implications because endogenous peer effects imply potentially large social multiplier effects and efficiency gains through the feedback in the behavior of individuals within an existing social network (e.g., positive student behavior leads to more positive behavior in the network) (Hoxby 2000), whereas contextual peer effects do not have these dynamic implications.²

This distinction may be of particular importance for education policy. For instance, understanding whether pupil achievement is mainly the result of endogenous interactions such as strategic complementarities in student effort (Kremer, Miguel, and Thornton 2004) or negative externalities from students' disruption of learning activities, or whether it results from interacting with smarter peers (i.e. peers with greater cognitive development), may have important implications for the determination of optimal class sizes (Lazear 2001). Moreover, if behavioral peer effects of a specific sort exist at the neighborhood-level, then understanding which policies encourage the internalization of these will make human capital investments more efficient, and will increase macroeconomic growth (Bénabou 1996).

In this paper, we identify endogenous peer effects in children's school participation decisions using evidence from a human development program in rural Mexico. The PROGRESA program, initiated by the Mexican government in 1997, provides cash transfers to marginalized households in rural areas. The transfer is paid to mothers contingent on their children's primary and secondary school attendance

¹ On the other hand, Oreopoulos (2003) uses quasi-experimental variation in assignment to different types of public housing units in Toronto and finds no long-term neighborhood effects on individuals' labor market outcomes. A complementary literature on classroom-based peer effects also finds mixed evidence regarding the existence and magnitude of peer effects. Although an exhaustive list of empirical papers in this literature is difficult to construct, recent experimental and non-experimental studies examining peer effects in school achievement (in both developed and less developed countries) include Angrist and Lang (2004), Boozer and Cacciola (2001), Ding and Lehrer (2005), Graham (2005), and Kremer, Miguel, and Thornton (2004).

² Note that contextual peer effects can lead to potential multiplier effects as a result of sorting into peer groups (Epple and Romano 1998; Bayer, Ferreira, and McMillan 2004).

and family visits to health services. Five hundred and six communities were selected to participate in an experimental evaluation of the program; the communities were randomly divided into two groups, with the treatment group being phased-in to the program in March-April 1998 and the control group in November-December 1999. Within these selected communities, a poverty indicator was constructed at baseline to classify eligible and ineligible households. While household eligibility was determined within all (treatment and comparison group) communities, only households below a welfare threshold and within the treatment villages became program beneficiaries during the evaluation period.

Using experimental variation in the induced school participation of the subset of eligible children in these communities, we can identify the endogenous peer effects in school enrollment among children who were ineligible for the program within the program communities. Our main results suggest that children have an increased likelihood of attending secondary school of approximately 5 percentage points as a result of a 10 percentage-point increase in the network enrollment rate, which represents an increase of 8.5 percent from baseline. Substantially larger effects of approximately 6.5 percentage points are also found for children of relatively poorer households within the ineligibles group - a subgroup of children that are more likely to interact with treated children in these villages. These estimates indicate that policy intervention benefited from important social multipliers, as endogenous social interactions in effect doubled the direct effects of the school enrollment subsidy in the absence of peer interactions.

Despite the use of experimental variation, a potential concern with our identification strategy is that the program may have affected ineligible children through other mechanisms. The focus of PROGRESA was not limited strictly to education, but also encouraged investments in health and nutrition while providing eligible households with substantial monthly payments. With the program inducing behavioral changes among eligible households along several dimensions, it is conceivable that the increase in enrollment among ineligible households was not necessarily due to peer effects, but rather a response to some other change in the behavior of eligible households.

We use several approaches to demonstrate that this is not the case. First, we exploit the richness of the data to test for other potential program externalities, which may have affected the school enrollment behavior of ineligible households. We do not find any evidence of program-induced improvements in school quality, or that the program affected either the consumption of ineligible households or children's health, which may have led to greater school enrollment rates. Secondly, we condition on a large number of predetermined mean village-level contextual and environmental characteristics that may be correlated with the impacts of the intervention, and show that the effects are robust to these specifications. Lastly, we present evidence inconsistent with a relative reduction in transportation costs faced by program village children, and with potential contamination bias concerns. This sensitivity analysis confirms the validity of the identifying assumptions of the model.

The paper is structured as follows. Section 2 provides a brief discussion of the PROGRESA program and its evaluation component, as well as the data used in the analysis. In Section 3, we present an empirical model of social interaction effects and discuss its identification problems. We then describe our research design, and how it avoids these identification pitfalls. The main estimates are reported in Section 4, followed by sensitivity tests of the identifying assumption in Section 5, and Section 6 concludes.

2. PROGRESA Program, Evaluation, and Data

2.1 Background on the PROGRESA Program Evaluation

In 1997, the Mexican government initiated a large-scale education, health, and nutrition program (the PROGRESA Program) aimed at improving human development among children in rural Mexico. The program targets the poor in marginal communities, where 40 percent of the children from poor households drop out of school after the primary level. The program provides cash transfers to the mothers of over 2.6 million children conditional on school attendance, health checks and health clinics participation, at an annual cost of approximately one billion dollars, or 0.2 percent of Mexico's GDP in 2000. The education component of PROGRESA consists of providing subsidies, ranging from \$70 to \$255 pesos per month (depending on the child's gender and grade level), to children attending school in grades three to nine of primary and lower secondary school. Overall, the program transfers are sizeable, representing 10 percent of the average expenditures of beneficiary families in the sample.

A distinguishing characteristic of PROGRESA is that it included a program evaluation component from its inception. PROGRESA was implemented following an experimental design in a subset of 506 communities located across seven states: Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí, and Veracruz. Among these communities, 320 were randomly assigned into a treatment group, with the remaining 186 communities serving as a control group, thus providing an opportunity to apply experimental design methods to measure its impact on various outcomes. In addition, within these selected communities, a poverty indicator was constructed using the household income data collected from the baseline survey in 1997. A discriminant analysis was then separately applied in each of the seven regions in order to identify the household characteristics that best classified poor and non-poor households. These characteristics, which were unknown to the households, were then used to develop an equation for computing a welfare index that determined eligibility into the program (see Skoufias et al. 2001 for a more detailed description of the targeting process).³ While household eligibility was determined within all (treatment and comparison group) communities, only households classified as eligible and within the treatment villages became program beneficiaries during the evaluation period. That the eligibility classification exists for both treatment and control communities and treatment was

³ In addition to capturing the multidimensionality of poverty, another advantage of a welfare index is that it permits the classification of new households according to their socio-economic characteristics, other than income.

randomly assigned are critical design aspects for the identification of the endogenous peer effects, as will be discussed in Section 3.

An issue in the initial implementation (during the first-year) of the program involved an increase (by the program administrators) in the number of eligible households, after it was discovered that households with certain characteristics – namely, the elderly poor who no longer lived with their children – were excluded from the initial eligibility criteria. Because of this oversight, a new discriminant analysis was conducted, and households were reclassified as either eligible (poor) or non-eligible (non-poor) households. Households that were originally classified as non-poor but included in this second set of eligible households - called the ‘densificado’ group – became program beneficiaries approximately 8 months after the start of the program (Skoufias, Davis, and de la Vega 1999). As a result of this change in program implementation, there are eligible households above and below the initial region-specific eligibility thresholds. For our analysis we classify these ‘densificado’ households as eligible, since these are eligible for treatment at some point during the evaluation period.

2.2 Data and Measurement

Since the baseline census in October 1997, extensive biannual interviews were conducted during October 1998, May/June 1999, and November 1999, on approximately 24,000 households of the 506 communities.⁴ Each survey is a community-wide census containing detailed information on household demographics, income, expenditures and consumption, and individual socio-economic status, health and school behavior. More specifically, the surveys in October 1997, October 1998, May/June 1999, and November 1999 collected information on the school enrollment and grade completed of each child in the household between 6 and 16 years old. We thus have information on enrollment during three consecutive school years (1997-98, 1998-99, and 1999-2000). Since primary school enrollment is almost universal in rural Mexico, we restrict our interest to the enrollment decisions of children who have attained at least a primary education but have not completed secondary school at baseline. Secondary school enrollment is the most problematic decision for school attainment⁵, and also the grade levels where PROGRESA has had its greatest impact among eligible households (Schultz 2004). In our sample, this concerns approximately 2,738 children who are eligible at baseline to enter any of three lower secondary school grade levels. By selecting the sample based on grade completed at baseline rather than including children who start completing their primary schooling during the post-treatment evaluation period, we avoid issues of dynamic selection into secondary school (Cameron and Heckman 1998). Also, with village-level censuses, we can reliably construct village-level means of household and individual characteristics - including school behavior and contextual variables that may affect it.

⁴ There was a round of data collection in March of 1998 just prior to the start of the intervention.

⁵ In 1997, primary school enrollment was close to 96.5%, compared to 65% enrollment into secondary school.

Table 1 presents the mean of various individual and household-level characteristics for both eligible and non-eligible children and their differences between treatment and control villages. The first row in the table demonstrates the hurdle that secondary school represents for children in rural Mexico, and highlights a clear objective of the program (Table 1, Panel A). In 1997, the enrollment rate of eligible children in secondary school is 66 percent, on average. Although enrollment rates are on average 4 percentage points higher among ineligible children, only 70 percent of these were enrolled in secondary school. As one would expect from the random assignment, the pre-program difference in enrollment rates between treatment and control villages among both eligible and ineligible households is small and statistically insignificant. In addition, the simple difference in 1998 and 1999 enrollment rates between treatment and control communities provides a straightforward measure of the program's impact on school participation. In both years, enrollment rates in treatment villages were roughly 6 percentage points higher than in control villages among the beneficiary households. Table 1 also shows our first indication of a possible spillover effect. Although the difference is statistically insignificant (in the second year), secondary school enrollment in the treatment villages is approximately 6 and 4 percentage points higher than in control villages among children of ineligible families in 1998 and 1999, respectively. Given these low enrollment rates, it is perhaps not too surprising that the mean educational level of heads of households are also quite low, as heads of eligible and ineligible households have only completed 2.6 and 3.2 years of schooling, respectively (Panel B). These children also tend to come from large households, as the mean number of household members in these villages is 7.3 for eligible households and 6.8 for ineligible ones.

We also compare mean attributes at baseline (October 1997) across treatment and control villages to evaluate the randomization of our sample (Table 1, columns 2-4, 6-8). As one would hope from the random assignment, there are no statistically significant differences in the observed characteristics of these individuals in most dimensions.⁶

In addition to the village-census data, we use administrative data on the amount of PROGRESA transfers received by the households per survey-round. As expected, the administrative transfers data shows that eligible households in treatment villages received 170 pesos per month (on average) during the April 1998-December 1999 period (Table 1, Panel B). Average transfers for control households are non-zero because they begin to receive program transfers by December 1999. The difference in transfers between the two groups is large and substantial. More importantly, the administrative data shows no evidence of program leakage, i.e., ineligible households receiving cash transfers.⁷

⁶ Behrman and Todd (1998) conduct an exhaustive analysis of the degree of success of the random assignment of villages in the PROGRESA Program, and conclude that the randomization was successful.

⁷ Although this does not prove that leakage was not an issue in the program's implementation, there is no evidence of it at the central level.

Finally, we also make use of administrative data on secondary schools in the evaluation regions (which contain information on number of pupils by grade, teachers, number of classrooms, and other infrastructure characteristics of the schools). Without information on which school each child attends, we match – using GPS data – children from the same village to the secondary school closest in distance to the village.⁸ This administrative data allow us to rule out alternative hypotheses and to test our identifying assumptions (see discussion in Section 5). Means of characteristics of schools attended by the children in the sample are reported in Table 2, and there are no systematic differences between treatment and control villages as expected.

Given our panel data structure, an important issue in the empirical analysis is the extent of sample attrition. If being out-of-sample is correlated with the likelihood of being in the program (treatment) group, then this could lead to bias in the coefficient estimates. Sample attrition rates through the two post-treatment survey rounds are approximately 20 percent for the sample of children in secondary school, both in eligible and ineligible households (Table A1, columns 1 and 4), and the likelihood of attrition is highly correlated with individuals’ observable characteristics (columns 2 and 5). Fortunately, across program and comparison groups, attrition rates are balanced and the observables correlates of attrition are not significantly different (columns 3 and 6). We use baseline individual, household, and community characteristics to control for any potential attrition bias in all our estimations.

3. Identification of Endogenous Peer Effects

In this section, we discuss the econometric models used to estimate endogenous peer effects and the assumptions needed for identification. The standard approach used to estimate endogenous peer effects assumes that individuals’ school enrollment decisions follow a simple linear-in-means model:

$$y_{ic} = \alpha + \beta X_{ic} + \gamma \bar{X}_c + \lambda Z_c + \theta \bar{y}_c + u_{ic} \quad (1)$$

where y_{ic} is an indicator variable for the school enrollment behavior of child i in village c ; X_{ic} are exogenous characteristics of the individual; \bar{X}_c are the mean exogenous characteristics of the reference group; Z_c are characteristics of the environment (e.g., village) that may influence individuals’ school enrollment decisions; and \bar{y}_c is the enrollment rate of the reference group.⁹ This linear-in-means model provides a formal expression to three hypotheses often advanced to explain the common observation that individuals belonging to the same group tend to behave similarly. The first, *correlated effects*, proposes

⁸ Even though there maybe some measurement error associated with matching children to their geographically closest school, there are at least two reasons why the misclassification should be minimal: (i) households in these villages have a very limited choice of schools, due to the scarce number of secondary schools in these marginal areas (only 10 percent of households have access to a secondary schools in their village); (ii) based on fieldwork conducted by the authors in 2003, we were able to perfectly match the villages visited to the secondary schools reported as attended in informal interviews with village members.

⁹ Note that in this specification we are assuming that the reference group and the environment are one in the same. This clearly need not be the case.

that individuals in the same group tend to behave similarly because they have similar characteristics or face similar environments; these are represented in the model by the vector of parameters β and λ . The second, *contextual peer effects*, proposes that exogenous characteristics of the reference group (e.g., parental involvement in children's education in the village) influence individual behavior; the vector of parameters γ captures these contextual effects. Finally, the hypothesis of *endogenous peer effects* proposes that the behavior of the group influences individual behavior; the parameter θ in the model captures this effect.¹⁰

As Manski (1993) shows, OLS estimation of the linear-in-means model cannot separately identify the two types of social interaction effects as a result of the simultaneity of individuals' actions.¹¹ Equation (1) represents individual i 's school enrollment best-response function given peers' potential school enrollment decisions and exogenous characteristics. However, the data consist of equilibrium behavioral choices of all individuals in a reference group, and therefore the individuals' school enrollment decisions are jointly determined, leading to simultaneity bias (Moffitt 2001).

Identification of parameter θ is possible, however, under a partial-population experiment setting—whereby the outcome variable of some randomly chosen members of the group is exogenously altered (Moffitt 2001). Formally, we can assume that individuals' school enrollment decisions follow model (1) augmented for the existence of an exogenous treatment T_{ic} which equals unity for a subset of individuals in the reference group c and zero otherwise. The individual characteristics of this subgroup are denoted by superscript E :

$$y_{ic}^E = \alpha + \beta X_{ic}^E + \gamma \bar{X}_c + \lambda Z_c + \theta \bar{y}_c + \delta T_{ic}^E + u_{ic}^E \quad (1')$$

In addition, there are individuals within the same reference group c (denoted with superscript NE) who do not receive treatment:

¹⁰ There are several theories as to why the school enrollment decisions of one peer's may affect one own enrollment status. For instance, strategic complementarities in either the education production function, children's social preferences (e.g., conformist preferences, or information transmission regarding the payoffs to schooling may lead to social interactions (Akerlof, 1997; Cooper and John, 1988). Unfortunately, we cannot distinguish among these competing hypotheses in this econometric framework. We refer the interested reader to Becker and Murphy (2000), Durlauf and Young (2001), and Glaeser and Scheinkman (2003) for a thorough discussion of the literature on choice in the presence of social interactions. Duflo and Saez (2003) examine 'reduced-form' endogenous interaction effects with respect to retirement savings decisions in the U.S. using an analogous experimental design.

¹¹ To see this, take the expectation of equation (1) conditional on X and Z , integrating over Z , and solving for \bar{y}_c results in the mean equilibrium outcome in group c , which, substituted in equation (1) yields the reduced form for individual outcomes:

$y_{ic} = \frac{\alpha}{1-\theta} + \beta X_{ic} + \frac{\gamma + \beta\theta}{1-\theta} \bar{X}_c + \frac{\lambda}{1-\theta} Z_c + u_{ic}$. Manski (1993) shows that, conditional on $\theta \neq 1$, this equation has a unique solution, parameters γ and θ are unidentified, but composite parameters $\frac{\alpha}{1-\theta}$, $\frac{\gamma + \beta\theta}{1-\theta}$, and $\frac{\lambda}{1-\theta}$ are identified. Although the

identification of the composite parameters does not allow one to distinguish between endogenous and contextual social interaction effects, it permits one to determine whether some social effect is present.

$$y_{ic}^{NE} = \alpha + \beta X_{ic}^{NE} + \gamma \bar{X}_c + \lambda Z_c + \theta \bar{y}_c + u_{ic}^{NE} \quad (1'')$$

Using equations (1') and (1''), and recalling that group averages are related to within-village treated (E) and untreated (NE) group averages by:

$$\bar{y}_c = m_c^E \bar{y}_c^E + (1 - m_c^E) \bar{y}_c^{NE} \quad (2)$$

$$\bar{X}_c = m_c^E \bar{X}_c^E + (1 - m_c^E) \bar{X}_c^{NE}$$

where m_c^E is the share of treated individuals in the reference group c , we can show, based on Moffitt (2001), that the mean equilibrium outcome in the reference group satisfies the following condition:

$$\bar{y}_c = \frac{\alpha}{1 - \theta} + \frac{\beta + \gamma}{1 - \theta} \bar{X}_c + \frac{\lambda}{1 - \theta} Z_c + \frac{\delta}{1 - \theta} m_c^E T_c \quad (3)$$

Substituting equation (3) in equation (1''), we can solve for the reduced-form relationship of the school enrollment outcomes of untreated individuals as a function of the partial-population treatment in the reference group, and exogenous individual, reference group, and environmental characteristics:

$$y_{ic}^{NE} = \frac{\alpha}{1 - \theta} + \beta \bar{X}_{ic}^{NE} + \frac{\theta \beta + \gamma}{1 - \theta} \bar{X}_c + \frac{\lambda}{1 - \theta} Z_c + \frac{\theta \delta}{1 - \theta} m_c^E T_c + u_{ic}^{NE} \quad (4)$$

The partial-population treatment terms in the two reduced-form equations have intuitive interpretations. In equation (3), the $(\delta/(1 - \theta))m_c^E$ term can be decomposed into two additive terms: (i) the direct effect of the treatment on the mean enrollment of the reference group, which is assumed to affect a sub-sample of the reference group (δm_c^E), and (ii) the indirect effect as a result of endogenous social interactions ($(\theta/(1 - \theta))\delta m_c^E$). For the untreated group (equation (4)), the partial-population treatment term accounts for the fact that the untreated group is not directly affected by the treatment (by definition), and only includes the indirect effect: the endogenous social interactions.

Also note that one could use coefficient estimates from equations (3) and (4) to identify the direct treatment and endogenous peer effects parameters. Specifically, note that the ratio of the $m_c^E T_c$ reduced-form coefficients from equations (3) and (4) is equal to θ , the endogenous peer effects parameter.

The specifications that we adopt in this paper are based on equation (1'') and a slight variant of equation (3):

$$y_{ic}^{NE} = \alpha + \beta X_{ic}^{NE} + \gamma \bar{X}_c + \lambda Z_c + \theta \bar{y}_{-i,c} + u_{ic}^{NE} \quad (1''')$$

$$\bar{y}_{-i,c} = \tilde{\alpha} + \tilde{\beta}_1 X_{ic}^{NE} + \tilde{\beta}_2 \bar{X}_c + \tilde{\lambda} Z_c + \tilde{\delta} T_c + \tilde{\varepsilon}_c \quad (3')$$

where T_c is the PROGRESA treatment village indicator variable and composite coefficients $\tilde{\alpha} = \frac{\alpha}{1-\theta}$,

$\tilde{\beta}_2 = \frac{\theta\beta + \gamma}{1-\theta}$, $\tilde{\lambda} = \frac{\lambda}{1-\theta}$, and $\tilde{\delta} = \frac{\delta m_c^E}{1-\theta}$. Note that equation (3') uses T_c rather than the interaction term

$m_c^E T_c$ as the instrumental variable. We allow for this discrepancy in the model because the share of treated individuals in the reference group, m_c^E , (in this case the share of PROGRESA-eligible children in the village) may not be exogenous if there is any sorting of individuals into and out of the village based on unobservable characteristics of the households or villages. However, estimates which use $m_c^E T_c$ as the IV provide quantitatively similar estimates to those reported in the results sections below.

Under the conditions of (i) robust partial correlation between the instrumental variable and the endogenous regressor ($\tilde{\delta} \neq 0$), and (ii) lack of correlation between the excluded IV and the disturbance term in equation (1'') ($E[T_c u_{ic}^{NE}] = 0$), IV estimation is a consistent estimator of parameter θ . Condition (i) can be tested in the data, and results will be discussed in Section 4. Condition (ii), the exclusion restriction, is not directly testable and is a maintained assumption of the model; the random assignment of the program across villages is not sufficient to ensure that this condition holds.

The IV exclusion restriction relies on the assumption that an increase in school participation among ineligible children in treatment villages is the effect of the exogenous increase in school participation among the eligible secondary-school children within the village and not the result of changes in contextual variables affected by the program. Since it is possible, however, that the program affected ineligible children through other channels, we follow various strategies to provide evidence that this is not the case. First, using rich micro data for both eligible and ineligible households, we directly test whether other potential externalities from program impacts or particular intricacies of the program had an effect on ineligible households. We do not find any evidence of changes in the consumption patterns or health status of ineligible households, or in measures of school quality, for instance. Secondly, we condition on a large number of predetermined mean village-level contextual (\bar{X}_c) and environmental (Z_c) characteristics that may be correlated with the impacts of the intervention, and show that the effects are robust to these specifications. We do not find any evidence of alternative mechanisms, and defer discussion of these results to Section 5.¹²

¹² We present in the Appendix a more general linear-in-means model of social interactions that allows for direct treatment effects on children's contextual characteristics. To identify endogenous peer effects in this model, we need to assume that the other variables affected have neither direct nor contextual social interaction effects on children's school enrollment decisions. If the condition fails to hold, we can still identify the presence of peer effects, but we cannot distinguish between endogenous and contextual peer effects. We estimated reduced-form equations consistent with this more flexible model, in which we directly explore the relationship between school enrollment and $m_c T_c$. Our results, while less precisely estimated, are consistent with the estimates reported in Section 4. These results are available upon request.

Finally, note that we also assume endogenous peer effects to be at the village-level. Although we lack information on the specific individuals who belong to a child's reference group, we believe that the assumption of village-level effects may not be problematic for the following reasons. As is common in village economies in less-developed countries, there is substantial ethnographic evidence documenting social interactions at the village level in rural communities in Mexico (e.g., Foster 1967). Furthermore, rural villages in this sample are quite small, with 47 households per village and only 20 children of secondary-school age per village, on average. Thus in the context of Mexico, village peer effects may be a more credible assumption than studies that use city blocks (Case and Katz 1991), census tracts (Topa 2001; O'Reagan and Quigley 1996), or schools (Evans, Oates and Schwab 1992; Hoxby 2000; Gaviria and Raphael 2001).

4. Estimates of Spillovers and Endogenous Social Interaction Effects

4.1 Estimates of Reduced-Form Spillover Effects

In this section, we present evidence on the reduced-form spillover effects of the program on school enrollment. We start the discussion with a graphical analysis to shed light on the patterns in the data. Figure 1 presents a series of graphs, based on nonparametric estimates, depicting enrollment rates in secondary school by the welfare index used to classify eligible and ineligible households.¹³ Enrollment rates do not differ at baseline among eligible children in program and comparison villages (Figure 1, Panel A), and the difference is positive but small and insignificant among ineligible children (Panel B). However, for 1998 and 1999, enrollment rates in program villages among both eligible and ineligible children increase substantially relative to the comparison group (Panels C and D). Within the ineligible children group, we observe a striking difference in enrollment rates between treatment and control villages among relatively poorer households. This enrollment difference remains until a household welfare index of approximately 900 units (the median welfare index of ineligible households), at which point the enrollment rates tend to converge. This figure suggests that any spillovers of the program may have been concentrated among ineligible households with welfare characteristics relatively similar to the eligible households but classified above the welfare qualification.

Parametric linear probability estimates of the reduced-form relationship between program and comparison villages enrollment rates mirror the results depicted in the Figure 1. As documented by Schultz (2004) and Behrman, Sengupta and Todd (2005), children in eligible households increased their school enrollment by 7.6 percentage points relative to eligible children in control villages (Table 3, Panel A, regression 1). The point estimate with household and village-level controls implies an effect of 8.3

¹³ The conditional means are estimated by taking the mean enrollment within a bandwidth of 0.5. The figure is robust to perturbations to the bandwidth size.

percentage points, or 14 percent (Panel B, regression 1). Overall, children from ineligible households residing in the PROGRESA villages increased their secondary school enrollment rate by 5.0 percentage points relative to ineligible households in control villages (Panel A, regression 2); however, the effect is imprecisely measured (significant at 89 percent confidence) and not robust to individual, household, and village-level controls (Panel B, regression 2).¹⁴ There are significant differential effects on school enrollment by household's welfare index level (regressions 3 and 4). Among ineligible households with a below-median welfare index, PROGRESA increased secondary school enrollment by 5.5 percentage points (statistically significant at 90 percent confidence), but had no effect for children among the upper welfare-index group (-0.9 percentage points and not statistically significant).¹⁵ Also, despite the fact that PROGRESA had a larger impact on eligible girls (Schultz, 2004; Behrman, Sengupta and Todd, 2005), we do not find a similar differential spillover effects between boys (the point estimate is 0.033, standard error 0.030, not statistically significant) and girls (point estimate of 0.027, standard error 0.031, not statistically significant) once we include household and village-level controls (not reported in the tables).

4.2 Estimates of Direct and Endogenous Peer Effects

Table 4 reports the estimates of endogenous peer effects (θ) from OLS and IV estimation of equations (1'') and (3'). The OLS estimate of the overall endogenous effect for the control villages, which does not take into account the problems of self-selection into reference groups, the reflection problem, and unobserved heterogeneity in the population, implies a 0.72 percentage point increase in a child's probability of enrollment as a result of a 1 percentage point increase in the reference group's enrollment rate (significant at 99 percent confidence, Table 4, Panel A, regression 1). In contrast, IV estimates of the overall endogenous effect imply an effect of 0.65 without any control-adjustment (regression 2), but an effect of a 0.49 increase in the probability of enrollment in secondary school once household and village-level controls are included in the model (regression 3). The latter estimate, while significant at only 89 percent confidence, does suggest that endogenous peer effects are quite large for this population. And, even though we cannot necessarily reject that the OLS and 2SLS estimates are significantly different from each other, the results do suggest that the OLS estimates are biased upwards. Note that we include individual and household-level controls, and village-level predetermined contextual variables: the proportion of secondary school-age girls and the proportion of indigenous children in the village, mean village-level family size and educational level, age, and gender proportions of heads of households.¹⁶

¹⁴ This result is consistent with Behrman, Sengupta, and Todd (2005)'s lack of an overall effect among ineligible children. That said, we find positive spillover effects among children in the 10-13 years age group, consistent with their finding of a spillover effect for 12 year olds. Our effects are more precisely estimated due to the fact that we concentrate on individuals of secondary-school age and that we pool observations across age-specific groups.

¹⁵ The difference in effects is statistically significant at 90 percent confidence.

¹⁶ A specification which uses $m_c T_c$ as the excluded instrument gives an estimate of the endogenous peer effects (θ) of 0.481 (standard error = 0.276, significant at 92 percent confidence).

Substantially larger peer effects are found among the relatively poorer children within the ineligible group. The point estimate on the effect for children in the below-median welfare-index group is 0.671 (regression 5). The correlation of own and social network enrollment rate for this subgroup in control villages implies an effect of 0.750 (regression 4). Again, the experimental evidence suggests that the OLS estimates are biased upwards, although we cannot reject that the coefficients are equal.¹⁷ Note that we cannot identify the effect on children with a high household welfare index, since the first-stage correlation is weak for this subgroup (Panel B, regression 6). The average enrollment rate effect is small and indistinguishable from zero in these villages. Therefore, no inferences can be made on the peer effects for children in the wealthier households; the point estimate for this high welfare index group is -5.112 (not statistically significant).

That there exists a differential effect by the household welfare index is consistent with at least two explanations. First, this differential effect may simply suggest that households that are relatively poor and more credit constrained are more responsive to a positive inducement of attending school. Alternatively, these differential effects may reflect differences in social ties between ineligible households that are just above the welfare cutoff and those that are better off. In particular, if children from ineligible households that are slightly above the cutoff are more likely to interact with eligible children in the village, then the induced school participation of eligible children should have a more pronounced effect on this subgroup of children.

To test this hypothesis – without information on the exact peer network of each student – we construct a measure of the number of extended-family members who live in different households and can enroll into secondary school for each child in the village; this measure serves as a proxy for a child’s number of family-related peers in the village (a potential subset of a child’s peer group).¹⁸ Comparing ineligible children from households below median welfare-index to those above median welfare-index, we find that the number of eligible extended-family links at baseline is significantly greater for ineligible children in the first group (0.97 children) relative to the latter group (0.65 children), among children with some extended-family link in the village. This difference of approximately 0.31 children (standard error, 0.09, significant at 99 percent confidence; not reported in the tables), implies that the number of eligible

¹⁷ A specification which uses $m_c T_c$ as the excluded instrument gives an estimate of endogenous peer effects (θ) of 0.573 (standard error = 0.258, significant at 97 percent confidence). In specifications that include baseline enrollment as an additional regressor (to take into account potential pre-treatment differences), the estimated effects vary between 0.370 (standard error = 0.236; significant at 89 percent confidence) and 0.595 (standard error = 0.279; significant at 97 percent confidence) given small perturbations in the welfare cutoff. Moreover, none of these specifications suffer from weak-IV problems (results available from the authors upon request).

¹⁸ We construct identifiers for extended-families in the villages by grouping children according to unique identifiers of their parents’ last names. In Latin America, each individual has two last names, the first being the father’s first last name and the second the mother’s first last name. Therefore, we can construct the households where individuals are related (within reasonable errors) by using unique numerical identifiers of each combination of last names.

links is 48 percent higher among households classified in below the median welfare-index.¹⁹ While we do not expect all interactions to occur in these villages solely at the extended-family level, this evidence is consistent with poorer ineligible children tending to interact more with eligible children.

As noted by other researchers (e.g., Graham 2005; Hoxby and Weingarth 2006), the linear-in-means model is unable to provide answers to the equity-efficiency tradeoffs that pervade in theoretical discussions of peer effects. Kling, Liebman, and Katz (2006), using experimental variation in the poverty rates of neighborhoods in which individuals reside in the U.S., find no evidence of non-linear poverty effects. For comparability reasons, we assess whether there are non-linearities in endogenous peer effects by allowing the parameter estimates to vary according to (i) children's baseline enrollment decision, and (ii) baseline village-level enrollment rates. Although point estimates suggests that effects are greater among children in communities with low baseline enrollment (results not shown), we cannot reject the linearity assumption.²⁰

Weak instruments are not a main concern in the estimation. There is a robust partial correlation between the program village treatment indicator and the potentially endogenous regressor, the village-level enrollment rate. The F-test statistics reflecting the significance of the IV in the first stage equations excluding and including controls are 8.74 and 7.60 in the overall effect model (Panel B, regressions 2 and 3), and the F-statistic for the poorer ineligible group is 13.92 (the first-stage coefficient is significant at 99 percent confidence) (regression 5).²¹

In summary, this evidence is consistent with the hypothesis that changes in reference groups' school enrollment behavior affects children's own enrollment behavior, and that these effects differ depending on children and their family's inherent opportunity costs, as well as by the types of peers they interact with. As will be shown in Section 5, these results are robust to specifications, alternative measures of peer behavior, and identifying assumptions.

5. Sensitivity Analyses and Tests of Identifying Assumptions

It has been well documented that the impact of PROGRESA was not restricted to schooling. That the program may have affected ineligible children in ways other than an increase in the enrollment rates of their reference groups remains a potential concern for our identification strategy. Such a situation would invalidate our exclusion restriction and we would be mistakenly attributing the effects of other

¹⁹ Assuming that other children who are not matched to an extended-family network actually have no extended-family eligible links, (therefore, we can impute a zero number of extended-family links for all these children), we can construct measures for all ineligible children in the village. We also find a greater number of links for children in the below-median welfare index group (0.58 children) relative to other ineligible children (0.41 children); a difference of 0.16 children (standard error 0.06, significant at 99 percent confidence).

²⁰ Estimates are available from authors upon request.

²¹ The LIML estimates of equations (1'') and (3'), which are robust to the weak instruments problem (under certain conditions, see Hayashi 2000) give endogenous interaction effects very similar to the IV results reported in the text. Results are available upon request.

mechanisms to peer effects. In this section, we present a series of robustness checks and tests of our underlying counterfactual assumption to show that we are in fact providing consistent estimates of endogenous peer effects.

5.1 Reduced-Form Tests of Alternative Mechanisms

In order for the treatment village indicator to serve as a valid instrument, the program cannot have indirectly affected other determinants of an ineligible child's enrollment decisions. This is a substantive assumption in the case of PROGRESA, where the program's multidimensionality affected the livelihoods of beneficiary households through a series of mechanisms. Apart from the increases in secondary school enrollment rates among eligible children (Schultz 2004), researchers have found significant increases in household consumption levels, food consumption, and food quality (Hoddinott and Skoufias 2004), improvements in health status, and increases in health care utilization (Gertler 2004; Gertler and Boyce 2001).²² If any of these program impacts create externalities - in the form of, for example, inter-household resource transfers, correlated positive shocks to income, or positive health externalities - that increase school enrollment rates for ineligible children, then we would be confounding endogenous peer effects with the positive externalities from these other mechanisms.

In addition to other program externalities, changes in environmental or institutional factors affecting children's school enrollment decisions may also pose concerns. A set of particularly important changes affecting school enrollment decisions were school supply-side interventions which accompanied the implementation of the program. Although this was done to mitigate potential congestion effects due to the expected increase in schooling demand, the improvement in schooling facilities may have attracted children from ineligible households.

Consumption Externalities and Relaxation of Credit Constraints

To verify whether any of these factors play a role in explaining the school enrollment spillover effect, we test for the existence of any post-treatment differences in household consumption and expenditures, health status of children, and certain school characteristics which may have been affected by the program (Table 5). We do not find any evidence that monthly household expenditures increased in the two post-treatment periods among ineligible households in program relative to comparison villages (the point estimate reported in Table 5 is -12.93, and not statistically significant). Since expenditures do not take into account consumption from household production, we also estimate household consumption in the first post-treatment period, and, again, find no significant difference in total consumption among

²² There is also evidence that the program improved women's relative bargaining power within the household (see Adato et al., 2000 and Bobonis 2004 for a discussion). Evidence of program impacts on other outcomes, including children and adults' labor supply (Parker and Skoufias 2000), migration patterns (Angelucci 2004), ability to mitigate shocks (de Janvry et al. 2004), and inter-household transfers (Attanasio and Rios-Rull 2000) suggest relatively small changes in these margins.

these households (point estimate is -50.56, not statistically significant).²³ Moreover, differential estimates by welfare-index subgroups also results in insignificant differences in expenditures and consumption (rows 1-2, columns 2-5). These expenditure and consumption patterns, as well as the evidence from the transfers data, provide evidence inconsistent with the possibility of inter-household income transfers from beneficiary to non-beneficiary households, correlated positive income shocks at the village-level, or evidence of program leakage (where some ineligible households may have been able to receive program transfers).

Households may be substituting expenditures in different areas as a result of the children's school enrollment. Consistent with the evidence on increased school participation, estimates suggest an increase in the share of the household budget spent on educational expenses (e.g., school supplies, school contributions). The point estimate implies an increase of 0.5 percentage points (9 percent, or approximately 5 pesos) on educational expenditures among all ineligible households and 0.4 percentage points (9 percent, approximately 4 pesos) among poorer ineligible households (row 3, columns 1 and 2). However, none of the estimates are significant at conventional confidence levels.

It is also possible that the liquidity injection from the program may have relaxed lending constraints of eligible households, enabling ineligible households to borrow when hit by negative idiosyncratic shocks, and making them less likely to remove their children from secondary school in the event of a shock (Jacoby and Skoufias 1997; Angelucci and De Giorgi 2005). We examine this potential alternative channel by showing evidence of the expenditure responses of ineligible households to natural shocks in both program and comparison villages for our sub-sample (Table 5, Panel B).²⁴ If the liquidity constraint hypothesis was correct, we would expect a relative positive effect on expenditures and school enrollment among households who suffer a shock in program villages. A potential concern to this test is that natural shock measures may not be very reliable: ineligible households seem to increase household expenditures in response to natural shocks (Panel B, row 2), and we observe a similar pattern using household consumption data (not reported in the tables). Given this caveat, we do not find evidence that ineligible households in program villages who suffer natural shocks have higher expenditure levels than those in comparison villages (Panel B, row 3). Furthermore, the school enrollment effect is lower among 'shock' than among 'no-shock' households: the estimated reduced form effects are -0.057 (standard error 0.032, significant at 90 percent confidence) and 0.083 (standard error 0.029, significant at 99 percent

²³ We use household expenditures and consumption as proxies for household income, since income is usually measured with substantial error in agricultural households, and these may better represent permanent incomes of households. Unfortunately, we only have home production data for the October 1998 survey round, and therefore, cannot estimate the consumption models in the second post-treatment round.

²⁴ We use household survey data to construct the shock measure, following Angelucci and De Giorgi (2005). The survey recorded whether the household has been hit by any of the following natural disasters in the six months preceding the interview: drought, flood, hail, fire, plague, earthquake, and hurricane. We create a variable which indicates whether the household has been hit by any natural disaster.

confidence), respectively (not reported in the tables).²⁵ We conclude that a potential relaxation of liquidity constraints did not positively affect the school enrollment outcomes for the sub-sample of ineligible households with secondary school-aged children.

Health Externalities

Regarding potential health externalities as a result of reduced contagion of communicable diseases (Miguel and Kremer 2004), or improved health status as a result of potential improvements in access to health facilities, we do not find evidence that any of these mechanisms took place, or at least that it led to significant improvements in the health status of secondary school-aged children. Unfortunately, the survey collected data from different questions across rounds regarding the self-reported health status of children. Therefore, we show evidence from the first post-treatment round (October 1998) on the number of days the child was ill in the past four weeks, and on answers to questions of difficulty with activities of daily living in the last survey round (November 1999).²⁶ There is no overall significant reduction or increase in the number of days ill reported among ineligible children in October 1998 (the point estimate is 0.10, not statistically significant; row 5, column 1). Differential effects by welfare subgroups suggest no difference in the morbidity of relatively poorer and wealthier households (Panel A, row 5, columns 2-3). Similar results are found using the ADL measures in November 1999 (rows 6-8). In summary, we find evidence inconsistent with any positive health externality hypothesis.

Changes in School Characteristics

The evidence from administrative data on school characteristics is also inconsistent with supply-side interventions potentially affecting the school enrollment decisions of these children. The number of teachers in secondary schools did not significantly increase in program schools relative to comparison schools (Panel A, row 9, column 1). The differential effects across schools attended by different subgroups of children are not significantly different from zero either (row 9, columns 2-3). Interestingly, there is an (insignificant) increase in the mean pupil-teacher ratio in program schools (the point estimate is 1.19). Moreover, the secondary schools attended by the poorer ineligible children suffered a (marginally significant) increase in pupil teacher ratios of 1.78, as expected from the increased school enrollment among eligible and ineligible children from these villages. These estimated increases are within the expected range from the household-survey estimates of increases in school enrollment; a back-

²⁵ School enrollment effects are similar among relatively poor ineligible children: estimates are -0.029 (standard error 0.038) and 0.101 (standard error 0.036) for 'shock' and 'no-shock' household children, respectively.

²⁶ See Gertler and Boyce (2001) for a detailed discussion of this self-reported data in the PROGRESA evaluation surveys, and a thorough analysis of the health impacts on eligible households.

of-the-envelope calculation implies an expected increase of 1.42 in the pupil teacher ratio.²⁷ If any negative congestion effect took place, we would expect a reduction in school enrollment among ineligible children (in equilibrium), biasing our peer effects estimates downwards.

Secondly, both teachers and school directors, in separate focus groups, voiced that the improvements in educational outcomes resulted from improved student interest and attendance rather than improvements in school inputs. Skoufias and McClafferty (2001) reports from a series of interviews with school teachers that “[t]he general perception was that [the] supply-side [intervention] was not sufficient to deal with the increase demand, although better attendance and attitudes towards schooling made teaching easier and more rewarding.” Unfortunately, we do not have quantitative data to test whether there were systematic improvements in teacher motivation, a factor that may have affected school enrollment decisions of all children.

Program Eligibility Expectations

Another concern may be that instability in the implementation of the program during its first year. Since some ineligible households were phased into the program during the first and second years (the ‘densificado’ households) this could have lead to uncertainty about the potential future eligibility of other non-beneficiary households. In addition, a large proportion of eligible households (27% of the total eligible population and mostly ‘densificado’ households) never received program payments during the evaluation period.²⁸ To the extent that this mismanagement led to uncertainty and changes in expectations about future eligibility, ineligible households could have increased their children’s school participation in order to maximize their opportunity of becoming beneficiaries (although it is equally plausible that they would have reduced their children’s school participation as well).

Although expectations of program eligibility are unfortunately unobserved, rendering this hypothesis non-testable, we do provide some indirect evidence to address this issue. If the extent of uncertainty surrounding the implementation of the program was more prevalent in villages where the incorporation of ‘densificado’ households was higher, we should expect higher increases in the school participation of ineligible children in these specific villages. However, when we estimate a schooling decision reduced-form model with an interaction term of the PROGRESA treatment indicator and the proportion of ‘densificado’ households in the village, we find that the interaction term is small and not

²⁷ Approximately 76 percent of children in the villages were eligible, and approximately 59 percent of the ineligibles belonged to the below-median welfare-index group. In addition, there are approximately 20 children of secondary-school age per village. Using the estimate of program impacts among eligible children of 8.3 percentage points increase in the secondary school enrollment rate, and the 5.5 percentage point increase among the below-median welfare-index group of ineligibles, we can estimate the mean increase in the number of pupils as $20 * [(0.76) * 0.083 + (0.14) * 0.055] = 1.42$ pupils.

²⁸ Previous researchers of the program suspect that these households were never formally incorporated into the program (Hoddinott and Skoufias, 2004).

significantly different from zero (not reported in the tables).²⁹ Additionally, if uncertainty about future program eligibility during the year 1998 was the main mechanism at play, we should not observe positive school enrollment outcomes during the second year of the program, once the uncertainty had been resolved. However, we do find positive spillover effects on school enrollment and grade promotion during the second year, especially among the relatively poor ineligible children. Reduced-form point estimates suggest average effects on school enrollment of 4.3 percentage points (8.7 percent, not significant), and more importantly, grade promotion effects of 5.6 percentage points (13 percent, significant at 90 percent confidence). Although these results do not disprove the eligibility expectations hypothesis, they diminish its plausibility.

5.2 Robustness Checks to Contextual Effects and Other Correlated Unobservables

In addition to these reduced-form tests, we report estimates of the endogenous peer effect conditioning on a series of expenditure-related village contextual controls (in addition to the pre-determined contextual controls): mean village-level household expenditures, mean educational, food, boys and girls' clothing, alcohol and tobacco expenditure shares, and an indicator variable for whether the village suffered a rainfall shock (i.e., flood) in the past six months.³⁰ Panel A in Table 6 reports estimates of θ from a series of regressions which gradually condition on village-level predetermined and expenditure-related contextual variables, and also compares these to OLS estimates of θ . Conditioning on these sets of contextual variables reduces the point estimate of the overall effect slightly, from 0.54 to 0.49, and the latter becomes insignificantly different from zero (Table 6, Panel A, column 1). However, the point estimates for the below-median welfare-index group do not vary significantly with the inclusion of additional controls (Panel A, column 2). Also note that the F-statistics of the first-stage regression coefficients (reported in brackets) do not vary substantially once we condition on potential exogenous interaction factors. This exercise suggests that the estimates are robust to these potential contextual effects, especially among the specific subgroup with significant endogenous effects.

Changes in School Reference Composition

One potential source of bias could stem from changes in the composition of students attending secondary school. If children (or parents) base their enrollment decisions on the cognitive ability or socio-economic background composition of their potential classmates (i.e. changes in contextual characteristics at the school level), then our estimates could be confounded by the composition changes in the student body that PROGRESA induced. Although baseline measures of cognitive ability or school achievement

²⁹ Estimates available from the authors upon request.

³⁰ We do not control for mean village-level measures of health status in the population of interest, because the survey questions assessing health status vary across survey rounds. However, as seen in Section 5.1, there is no evidence of positive health externalities among this group of children (see discussion above).

are not available, we can verify whether the socio-economic composition of children attending secondary school changed in the PROGRESA villages using the pre-determined contextual characteristics defined above. As expected, children attending secondary schools in treatment villages are disproportionately selected from lower-SES households – households with larger family sizes, lower school attainment of the head of household – relative to children attending secondary school in comparison villages.³¹ To the extent that the reduction in the mean “quality” or achievement of students lowers the incentives for children to enroll in secondary school, this potential mechanism would bias our estimates downward.³² We test for this possibility by estimating models that condition on the mean contextual characteristics of children in the village attending secondary school; the point estimates in these specifications increase slightly to 0.56 for the overall sample and to 0.69 for the below-median welfare index groups (Table 6, Panel A, row 7).

Transportation Costs

Another potential concern is that the program somehow reduced school transportation costs, and this induced ineligible children in the program villages to enroll. Although data on school transportation costs were, unfortunately, not collected, one possibility is to test whether or not there is a differential effect on children who live less than one kilometer from a secondary school, and presumably do not require school transportation.³³ As shown in Table 6, although there appear to be differential magnitude of effects in the overall sample (statistically insignificant), there is a small 0.06 percentage point and statistically insignificant difference in the estimated effect between children living less than a kilometer away from a secondary school and those who live further out among the below-median welfare-index group (Panel B, row 1). In a specification that includes controls for school composition characteristics, the point estimate of the differential effect is 0.03 percentage points greater for children residing within 1 km of secondary school (row 2).³⁴

Program Contamination

Another concern is the potential contamination of the experimental design, given that some children from treatment and control villages attended the same secondary school. Among the ineligible

³¹ Estimates available from the authors upon request.

³² On the other hand, alternative models of peer effects hypothesize that individuals might prefer peers in schools more similar in terms of cognitive ability or other characteristics (Hoxby and Weingarth 2006), therefore leading to increased incentives for the marginal ineligible children to enroll in school.

³³ Based on the March 1998 survey, 97 percent of students attending secondary school walked to school. Even though we do not have this information for post-treatment, it seems unlikely that the program would have increased the demand for public or private transportation as to be able to explain the magnitude of the spillover effect. Note also, that this is an admittedly fairly weak test. Even if there is a more pronounced effect among children without a secondary school in their village it still does not discredit a possibility of peer effects. That PROGRESA had a higher impact among eligible children without a secondary school in their village could lead to a differential effect among the ineligibles.

³⁴ The results are similar if we distinguish between children with and without a school in their village; or alternatively, between children who live less than and more than 2 km from a secondary school.

children in both treatment and control villages, 9.6 percent of the overall sample and 11.9 percent of the sample below the median welfare index were matched to the same secondary school. While these interactions could bias our results in either direction, if children from the control villages experienced a crowd effect as a result of PROGRESA, then our estimates may be overstated. To test for this bias, we re-estimate the endogenous peer effects model for the sample of villages in treatment and control groups that are not assigned the same secondary school. Our estimates for this sub-sample, which are similar to those presented above (Panel B, rows 3 and 4), suggest that the possibility of contamination is not a particular source of bias.

6. Conclusions

In 1997, the Mexican government introduced a randomly phased-in human development program designed to increase human capital among the rural poor. This study uses experimental variation in the school enrollment rates among program eligible households to estimate how endogenous social interactions affect the school decisions of children ineligible to receive these program benefits. Our findings suggest that the enrollment behavior of one's peers has an important role on a child's decision to enroll into school. A 10 percentage point increase in the enrollment rate of a child's reference group increases his likelihood of attending secondary school by approximately 5 percentage points. These endogenous peer effects are more pronounced among children of relatively poorer households within the ineligibles group. Furthermore, we are able to reject other potential contextual interaction effects hypotheses using rich micro data on household consumption and expenditures, health of individual members, and administrative data on program transfers and school characteristics. These sensitivity analyses confirm the validity of the identifying assumptions of the empirical social interactions model.

Our estimates – the first available estimates of endogenous peer effects in schooling in a developing country (to our knowledge) – lie in the upper range of existing social multiplier estimates of school enrollment/dropout behavior in both neighborhood-based and school-based contexts. The point estimates imply social multiplier effects in the range of 2.0 and 3.0, with a preferred estimate of approximately 2.5 (i.e., endogenous peer effects of 0.595). This indicates that endogenous social interaction effects approximately doubled the direct effects of school enrollment subsidies among secondary school-aged children in these marginalized areas. In contrast, Case and Katz (1991)'s estimates of peer effects in 'idleness' among youth in high-poverty neighborhoods in Boston imply a social multiplier effect of 1.33 (endogenous peer effects estimate of 0.25). However, Ginther, Haveman, and Wolfe (2000) and Aaronson (1998) report small – in the range of 1.02-1.06 (often statistically insignificant) – estimates of social multipliers in peers' dropout behavior from a sample of youth in the PSID. Estimates based on school-based reference groups suggest that high school dropout behavior of

students in the U.S. follows social multiplier of approximately 1.20 (Gaviria and Raphael 2001). Our estimates for children in marginal villages in rural Mexico, although not directly comparable to these, are more in line with Case and Katz (1991)'s, who report estimates for a sample of marginalized youth. Notwithstanding the differences in sample and methodology, our results suggest that endogenous peer effects may be much more prevalent for marginal populations in less developed countries, and consequently have important implications for the design of education policy especially in these contexts.

Future research should empirically differentiate the specific mechanisms for which we observe these 'reduced-form' endogenous interactions. Theoretical models within economics incorporate endogenous peer effects as a result of identity formation behavior (Akerlof and Kranton 2002), conformity behavior (Bernheim 1994; Akerlof 1997), and informational externalities (Bikhchandani, Hirschleifer, and Welch 1992), among others. Current work attempting to distinguish these effects, such as Akerlof and Kranton (2002), Kremer and Miguel (2006), and Munshi and Myaux (2005), could serve researchers as guides for these types of studies. Moreover, future researchers could address the issue of distinguishing between endogenous and contextual peer effects in richer empirical models of social interactions.

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Appendix: Identification of Endogenous Peer Effects in a Partial-Population Experiment Model with Potential Contextual Characteristics Effects

We present a more flexible linear-in-means model of social interactions that allows for direct treatment effects on children's contextual characteristics. We derive equilibrium reduced-form equations relating the partial-population treatment to individuals' school enrollment behavior given the potential effect of a second mechanism through a contextual variable. This model allows us to formally determine the conditions under which we can identify endogenous peer effects.

We start with our linear-in-means partial-population experiment model, where PROGRESA treatment essentially works as a subsidy for secondary-school enrollment of eligible children. The school enrollment best response functions are:

$$(A1') \quad y_{ic}^E = \alpha + \beta X_{ic}^E + \gamma \bar{X}_{-i,c} + \lambda Z_c + \theta \bar{y}_{-i,c} + \delta T_{ic}^E + u_{ic}^E$$

$$(A1'') \quad y_{ic}^{NE} = \alpha + \beta X_{ic}^{NE} + \gamma \bar{X}_{-i,c} + \lambda Z_c + \theta \bar{y}_{-i,c} + u_{ic}^{NE}$$

However, we now allow for the PROGRESA treatment to also affect other contextual characteristics of eligible children. We assume for simplicity that the program only affects one contextual characteristic, X_{1c} , and the vector of contextual determinants of enrollment can be decomposed into $X_c = [X_{1c}, X_{2c}]$. Moreover, the choice of X_{1c} is itself a linear function of individuals' pre-determined contextual characteristics (X_{2c}), environmental factors affecting all children in the village (Z_c), and the schooling price subsidy (T_{ic}^E):

$$(A2') \quad X_{1,ic}^E = \pi_0 + \pi_1 X_{2,ic}^E + \pi_2 \bar{X}_{1,-i,c} + \pi_3 \bar{X}_{2,-i,c} + \pi_4 T_{ic}^E + \pi_5 Z_c + v_{ic}^E$$

$$(A2'') \quad X_{1,ic}^{NE} = \pi_0 + \pi_1 X_{2,ic}^{NE} + \pi_2 \bar{X}_{1,-i,c} + \pi_3 \bar{X}_{2,-i,c} + \pi_5 Z_c + v_{ic}^{NE}$$

Based on equations (A1')-(A2'') and equations (3') and (3''), we can solve for the reduced-form equilibrium school enrollment choices of children based on these best-response functions. These reduced-form equations are a complex function of the potential direct impacts of the subsidy on y and X_{1c} , endogenous social interactions in y , and potential contextual and endogenous social interactions in X_{1c} .

The solution involves simple algebra on the simultaneous equation model. First, averaging equations (A1')-(A2'') at the village-level, we get:

$$(A4') \quad \bar{y}_c^E = \alpha + \beta_1 \bar{X}_{1c}^E + \beta_2 \bar{X}_{2c}^E + \gamma_1 \bar{X}_{1,c} + \gamma_2 \bar{X}_{2,c} + \lambda Z_c + \theta \bar{y}_c + \delta T_c^E$$

$$(A4'') \quad \bar{y}_c^{NE} = \alpha + \beta_1 \bar{X}_{1c}^{NE} + \beta_2 \bar{X}_{2c}^{NE} + \gamma_1 \bar{X}_{1,c} + \gamma_2 \bar{X}_{2,c} + \lambda Z_c + \theta \bar{y}_c$$

$$(A4''') \quad \bar{X}_{1c}^E = \pi_0 + \pi_1 \bar{X}_{2c}^E + \pi_2 \bar{X}_{1,c} + \pi_3 \bar{X}_{2,c} + \pi_4 T_c^E + \pi_5 Z_c$$

$$(A4''') \quad \bar{X}_{1c}^{NE} = \pi_0 + \pi_1 \bar{X}_{2c}^{NE} + \pi_2 \bar{X}_{1,c} + \pi_3 \bar{X}_{2,c} + \pi_5 Z_c$$

Substituting these conditions into equations (3') and (3'') yield:

$$(A5) \quad \bar{y}_c = \frac{\alpha}{1-\theta} + \left(\frac{\beta_1 + \gamma_1}{1-\theta} \right) \bar{X}_{1,c} + \left(\frac{\beta_2 + \gamma_2}{1-\theta} \right) \bar{X}_{2,c} + \frac{\lambda}{1-\theta} Z_c + \frac{\delta}{1-\theta} m_c T_c$$

$$(A6) \quad \bar{X}_{1,c} = \frac{\pi_0}{1-\pi_2} + \left(\frac{\pi_1 + \pi_3}{1-\pi_2} \right) \bar{X}_{2,c} + \frac{\pi_5}{1-\pi_2} Z_c + \frac{\pi_4}{1-\pi_2} m_c T_c$$

Finally, substituting equation (A6) in equation (A5) gives us the reduced-form equilibrium school enrollment equation at the village-level.

$$(A7) \quad \bar{y}_c = \left[\frac{\alpha}{1-\theta} + \frac{\pi_0}{1-\pi_2} \right] + \left[\frac{\beta_2 + \gamma_2}{1-\theta} + \left(\frac{\beta_1 + \gamma_1}{1-\theta} \right) \left(\frac{\pi_1 + \pi_3}{1-\pi_2} \right) \right] \bar{X}_{2,c} + \\ + \left[\frac{\lambda}{1-\theta} + \left(\frac{\beta_1 + \gamma_1}{1-\theta} \right) \left(\frac{\pi_5}{1-\pi_2} \right) \right] Z_c + \left[\frac{\delta}{1-\theta} + \left(\frac{\beta_1 + \gamma_1}{1-\theta} \right) \left(\frac{\pi_4}{1-\pi_2} \right) \right] m_c T_c^E$$

The reduced-form equilibrium condition has a very intuitive explanation, since it shows how the partial-population price subsidy affects equilibrium enrollment decisions through different channels. $[\delta m_c / (1-\theta)]$ represents the direct effect of the subsidy on school enrollment, augmented by the school enrollment social multiplier effect; $[\pi_4 m_c / (1-\pi_2)]$ represents the direct effect of the subsidy on the contextual variable X_l , compounded by the endogenous peer effects or externalities in the choice of X_l of individuals in the reference group. Finally, $[m_c (\beta_1 + \gamma_1) / (1-\theta)]$ represents how these effects on X_l are channeled to affect school enrollment, through direct effects (β_1), contextual peer effects (γ_1), and endogenous peer effects $[1/(1-\theta)]$.

We can also solve for the equilibrium school enrollment choice of ineligible children as a function of the PROGRESA subsidy and other exogenous determinants of school enrollment:

Substituting equations (A4'''), (A6) and (A7) in equation (A1'') and rearranging the structural coefficients (in order to assess the potential biases in the IV estimator), we can see that the reduced-form relationship is quite complex:

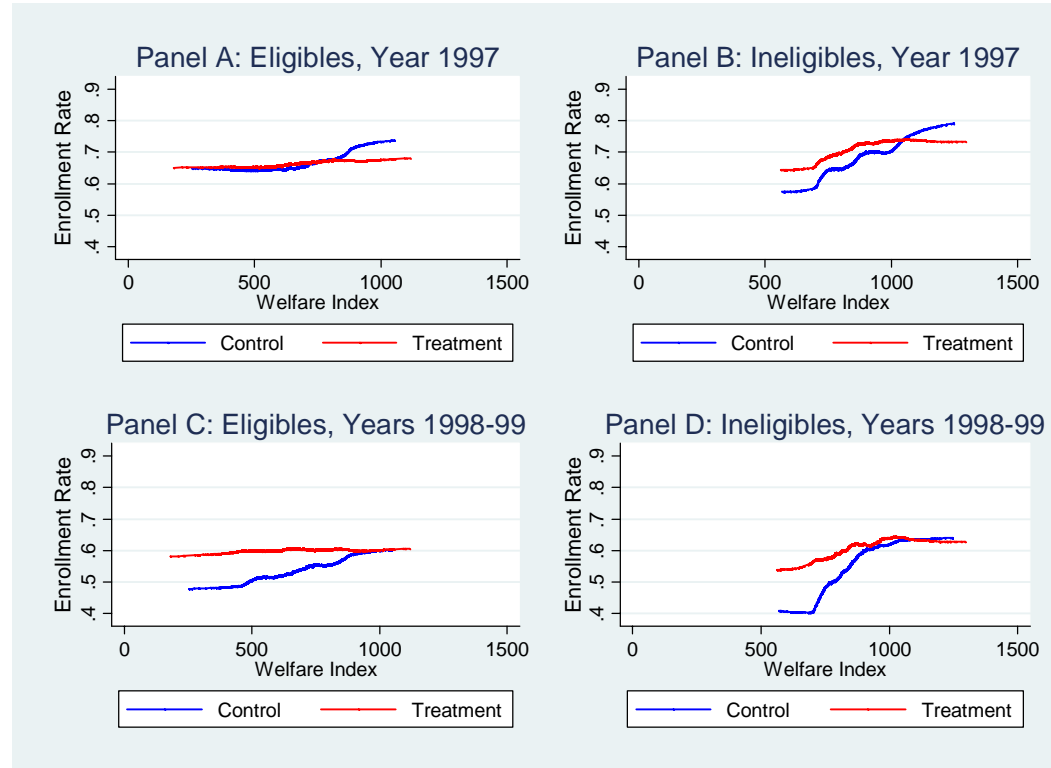
$$(A8) \quad \bar{y}_c^{NE} = G + \left[\frac{(\gamma_1 + \beta_1 \pi_2) \pi_4}{1-\pi_2} + \theta \left(\frac{\delta}{1-\theta} + \left(\frac{\beta_1 + \gamma_1}{1-\theta} \right) \left(\frac{\pi_4}{1-\pi_2} \right) \right) \right] m_c T_c^E$$

Where G is a linear function of $X_{2,ic}^{NE}$, $\bar{X}_{2,c}$ and Z_c . Therefore, the IV estimator leads to the estimate of the following composite parameter:

$$\text{plim } \hat{\theta}_{IV} = \left[\theta + \frac{(1-\theta)(\gamma_1 + \beta_1 \pi_2) \pi_4}{(\gamma_1 + \beta_1) \pi_4 + \delta(1-\pi_2)} \right].$$

As can be seen from equation (8), in order to get a consistent estimate of θ , one of the following two conditions needs to hold: (i) $\pi_4 = 0$, that is, that the PROGRESA subsidies cannot have any impact on other contextual variables, or (ii) $(\gamma_1 + \beta_1 \pi_2) = 0$, that the other variables affected have neither direct nor contextual social interaction effects on children's school enrollment decisions.

Figure 1: Nonparametric Estimates of Enrollment Rates by Household Eligibility Index, Years 1997-1999



Notes to Figure 1: Locally weighted smoothing of the proportion of individuals enrolled in secondary school by the welfare index of program eligibility; bandwidth = 0.8. The numbers of ineligible and eligible children are 2,738 and 11,147, respectively. Vertical lines are drawn at welfare index levels 550 and 822.

Table 1: Individual and Household Characteristics across Program and Comparison Villages

	-----Ineligible Households-----				-----Eligible Households-----			
	Mean [Std.Dev.]	Program	Comparison	Difference	Mean [Std.Dev.]	Program	Comparison	Difference
Panel A: Child Characteristics								
School Enrollment in 1997	0.699 [0.459]	0.712	0.680	0.032 (0.029)	0.663 [0.473]	0.664	0.662	0.002 (0.020)
School Enrollment in 1998	0.655 [0.475]	0.679	0.618	0.061* (0.033)	0.635 [0.481]	0.661	0.592	0.069*** (0.024)
School Enrollment in 1999	0.515 [0.500]	0.532	0.489	0.042 (0.034)	0.516 [0.500]	0.540	0.479	0.061*** (0.023)
Child's age in 1997	13.43 [1.72]	13.41	13.46	-0.05 (0.07)	13.36 [1.67]	13.36	13.35	0.02 (0.04)
Grade completed in 1997	6.25 [1.01]	6.27	6.23	0.05 (0.05)	6.03 [0.93]	6.03	6.04	-0.01 (0.03)
Gender (Boy)	0.495 [0.500]	0.497	0.494	0.003 (0.020)	0.504 [0.500]	0.511	0.492	0.019* (0.010)
Indigenous	0.115 [0.319]	0.129	0.093	0.036 (0.040)	0.306 [0.461]	0.305	0.308	-0.003 (0.052)
Panel B: Household Characteristics								
Head of Household's Schooling	3.19 [2.97]	3.25	3.10	0.15 (0.20)	2.57 [2.39]	2.58	2.57	0.01 (0.11)
Head of Household's Gender (Male)	0.926 [0.261]	0.932	0.918	0.014 (0.013)	0.921 [0.269]	0.921	0.922	-0.001 (0.007)
Head of Household's Age	48.78 [10.65]	48.82	48.73	0.08 (0.62)	45.88 [10.84]	45.62	46.30	-0.68** (0.33)
Household size	6.85 [2.32]	6.78	6.97	-0.19 (0.17)	7.34 [2.36]	7.33	7.38	-0.05 (0.09)
Total Household-level PROGRESA Transfers (Post-treatment)	-	-	-	-	111.48 [131.44]	170.27	14.93	155.34*** (5.84)

Notes to Table 1: Standard deviations of variables reported in brackets. Differences estimated in OLS regression models. Robust standard errors in parentheses; disturbances are allowed to be correlated within village; significantly different from zero at (*) 90%, (**) 95%, and (***) 99% confidence. The numbers of ineligible and eligible children are 2,738 and 11,147, respectively.

Table 2: School Characteristics across Program and Comparison Villages

	All Villages		
	Program	Comparison	Difference
Tele-secondary school	0.85	0.88	-0.03 (0.03)
General sec. school	0.05	0.04	0.01 (0.02)
Technical sec. school	0.09	0.07	0.02 (0.02)
Rural	0.93	0.95	-0.02 (0.02)
Semi-urban	0.06	0.04	0.02 (0.02)
Classrooms in Grade 7	1.14	1.15	-0.01 (0.08)
Classrooms in Grade 8	1.05	1.02	0.03 (0.08)
Classrooms in Grade 9	0.98	0.94	0.04 (0.08)
Number of teachers	3.10	3.06	0.04 (0.37)
Pupil teacher ratio	22.14	21.59	0.55 (0.92)

Notes to Table 2: Differences estimated in OLS regression models. Robust standard errors in parentheses; disturbances are allowed to be correlated within village; significantly different from zero at (*) 90%, (**) 95%, and (***) 99% confidence. The number of secondary schools is 506.

Table 3: Reduced-Form Treatment and Spillover Effects Estimates among Eligible and Ineligible Children

Sample	Dependent variable: School enrollment indicator			
	All Eligible Children	All Ineligible Children	Welfare < Median	Welfare > Median
	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	OLS
Panel A: No Controls				
Treatment indicator	0.064*** (0.022)	0.050 ⁺ (0.031)	0.085** (0.035)	-0.043 (0.036)
Individual & household controls	No	No	No	No
Village contextual controls	No	No	No	No
State indicators	No	No	No	No
Municipality indicators	No	No	No	No
Panel B: Including Controls & State Fixed Effects				
Treatment indicator	0.070*** (0.016)	0.028 (0.025)	0.055* (0.029)	-0.009 (0.044)
Individual & household controls	Yes	Yes	Yes	Yes
Village contextual controls	Yes	Yes	Yes	Yes
State indicators	Yes	Yes	Yes	Yes
Municipality indicators	No	No	No	No
Panel C: Including Controls & Municipality Fixed Effects				
Treatment indicator	0.046*** (0.017)	0.042 ⁺ (0.028)	0.086** (0.035)	-0.062 (0.052)
Individual & household controls	Yes	Yes	Yes	Yes
Village contextual controls	Yes	Yes	Yes	Yes
State indicators	No	No	No	No
Municipality indicators	Yes	Yes	Yes	Yes
Mean of dependent variable	0.577	0.587	0.559	0.642
N Observations	17494	4211	2757	1454
N Individuals	8828	2116	1382	734

Notes to Table 3: Coefficient estimates from OLS regressions are reported. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages, but not across villages; significantly different from zero at (ˆ) 85%, (*) 90%, (**) 95%, (***) 99% confidence. Individual and HH-level controls are the child's gender, indigenous status, the household's welfare index, education, age, and gender of the head of household, family size, and distance to secondary school. Village contextual controls are the proportion of secondary-school-age girls and the proportion of indigenous children in the village, mean village-level family size and educational level, age, and gender proportion of heads of households.

Table 4: OLS and IV Estimates of Endogenous Peer Effects among Ineligible Children

Panel A: IV Estimates

Sample	<u>Dependent variable:</u> School enrollment indicator					
	All Children in Control Group	All Children	All Children	Welfare<Median Control Group	Welfare < Median	Welfare > Median
	(1) OLS	(2) IV	(3) IV	(4) OLS	(5) IV	(6) IV
Social Network Enrollment Rate	0.716*** (0.076)	0.649*** (0.239)	0.492 ⁺ (0.310)	0.750*** (0.092)	0.671*** (0.246)	-5.112 (18.120)
Individual & household controls	Yes	No	Yes	Yes	Yes	Yes
Village contextual controls	Yes	No	Yes	Yes	Yes	Yes
State indicators	Yes	No	Yes	Yes	Yes	Yes
First-stage F-statistic	-	8.74	7.60	-	13.92	0.94
[p-value]		[0.003]	[0.006]		[<0.001]	[0.332]
Mean of dependent variable	0.557	0.587	0.587	0.507	0.559	0.642
Observations	1678	4211	4211	1075	2757	1454

Panel B: First Stage Regressions

	<u>Dependent variable:</u> Social Network Enrollment Rate				
Treatment indicator		0.077*** (0.026)	0.057*** (0.021)	0.082*** (0.022)	0.025 (0.026)
Individual & household controls		No	Yes	Yes	Yes
Village contextual controls		No	Yes	Yes	Yes
State indicators		No	Yes	Yes	Yes
Observations		4211	4211	2757	1454

Notes to Table 4: Coefficient estimates from OLS and IV regressions are reported. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages, but not across villages; significantly different from zero at (⁺) 85%, (*) 90%, (**) 95%, (***) 99% confidence. Individual and HH-level controls are the child's gender, indigenous status, the household's welfare index, education, age, and gender of the head of household, family size, and distance to secondary school. Village contextual controls are the proportion of secondary-school-age girls and the proportion of indigenous children in the village, mean village-level family size and educational level, age, and gender proportion of heads of households.

Table 5: Tests of Alternative Mechanisms for Spillover Effect**Panel A:** Alternative Channels, Reduced Form

Dependent variables	Sample	Coefficient Estimate on Treatment Village Indicator (s.e.)			Mean of dep. variable (4)
		All children (1) OLS	Welfare < Median (2) OLS	Welfare > Median (3) OLS	
Total HH Expenditures		-12.93 (38.22)	19.00 (44.38)	-44.56 (56.62)	1029.0
Total HH Consumption, October 1998		-50.56 (58.10)	-7.76 (67.10)	-98.56 (85.36)	1201.8
Schooling Expenditure Share		0.005 (0.005)	0.004 (0.005)	0.007 (0.008)	0.052
Food Expenditure Share		0.010 (0.009)	0.013 (0.010)	0.003 (0.011)	0.654
Days ill, October 1998		0.10 (0.11)	-0.01 (0.14)	0.25 (0.19)	0.357
Days of Difficulty with Daily Activities due to Illness, Nov. 1999		-0.047 (0.061)	-0.127 (0.093)	0.025 (0.073)	0.178
Days of No Daily Activities due to Illness, November 1999		0.029 (0.046)	-0.029 (0.071)	0.100 (0.080)	0.093
Days in Bed due to Illness, November 1999		0.030 (0.044)	-0.031 (0.067)	0.106 (0.079)	0.046
Number of teachers		0.05 (0.26)	0.06 (0.29)	-0.01 (0.34)	3.45
Pupil teacher ratio		1.19 (1.01)	1.78* (1.04)	0.12 (1.32)	21.96

Panel B: Treatment and Shocks Interactions in HH Expenditures Regressions

	Dependent variable: Total HH Expenditures			
Treatment indicator	2.07 (44.18)	23.55 (45.00)	-15.19 (81.82)	1029.0
Shock indicator	36.31 (44.81)	80.26 (56.05)	-51.26 (68.52)	
Treatment * Shock	-37.79 (60.76)	-14.44 (72.65)	-68.15 (99.02)	

Notes to Table 5: Each coefficient in Panel A is from a separate regression. Columns in Panel B report coefficient estimates from one regression. Coefficient estimates from OLS regressions are reported. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages, but not across villages; significantly different from zero at (*) 90%, (**) 95%, and (***) 99% confidence levels. Individual and HH-level controls are the child's gender, indigenous status, the household's welfare index, education, age, and gender of the head of household, family size, and distance to secondary school. Village contextual controls are the proportion of secondary-school-age girls and the proportion of indigenous children in the village, mean village-level family size and educational level, age, and gender proportion of heads of households.

Table 6: Robustness Checks of Endogenous Peer Effects

Specification (dependent variable is the school enrollment indicator)	Sample:	Coefficient Estimate on Social Network Enrollment Measure (s.e.)	
		All children (1)	Welfare < Median (2)
Panel A: Village-level Enrollment Rate			
OLS ^a		0.668*** (0.042)	0.660*** (0.053)
IV, no contextual controls ^a		0.541** (0.264) [8.91]	0.652*** (0.235) [14.37]
IV, predetermined contextual controls ^{a,b}		0.492 (0.310) [7.60]	0.671*** (0.246) [13.92]
IV, predetermined & exp. related contextual controls ^{a,b,c}		0.486 (0.357) [6.70]	0.657*** (0.282) [12.53]
IV, predetermined contextual & school characteristics controls ^d		0.495 (0.305) [7.83]	0.636** (0.263) [13.31]
IV, predetermined, exp. related contextual controls, & school chars. ^{a,b,c,d}		0.486 (0.367) [6.70]	0.630** (0.302) [11.60]
IV, predetermined contextual controls & characteristics of other children attending secondary school ^{a,b,c}		0.560 (0.393) [4.76]	0.691** (0.311) [9.82]
Mean of dependent variable		0.587	0.559
Observations		4211	2757

Notes to Table 6: Each coefficient estimate is from a separate regression. Coefficient estimates from OLS and 2SLS regressions are reported. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages, but not across villages; significantly different from zero at (*) 90%, (**) 95%, and (***) 99% confidence. First stage F-statistics of significance of partial correlation between IV (treatment indicator) and social network measure are reported in brackets.

(a) Individual and HH-level controls are the child's gender, indigenous status, the household's welfare index, education, age, and gender of the head of household, family size, and distance to secondary school. These are included in all specifications.

(b) Village predetermined contextual controls are the proportion of secondary-school-age girls and the proportion of indigenous children in the village, mean village-level family size and educational level, age, and gender proportion of heads of households.

(c) Expenditure related contextual characteristics are mean village-level HH expenditures, mean educational, food, boys' clothing, girls' clothing, alcohol and tobacco expenditure shares, and an indicator variable for whether the village suffered a flood shock.

(d) School characteristics are indicator variables for general, technical, secondary schools (relative to 'tele-secundaria' schools), urban and semi-urban school indicators (relative to rural schools), school-level pupil/teacher ratio, and the number of home teachers, teaching assistants, PE teachers, and art teachers in school.

(e) School composition controls are the mean village-level contextual characteristics mentioned above for children enrolled in secondary school.

Table 6: Robustness Checks of Endogenous Peer Effects (cont.)

Specification (dependent variable is the school enrollment indicator)	Coefficient Estimates (s.e.):			
	Social Network Enrollment Rate	Social Network Enrollment * School Dist. < 1km	Social Network Enrollment Rate	Social Network Enrollment * School Dist. < 1km
Sample:	All children		Welfare < Median	
	(1)	(2)	(3)	(4)
Panel B: Village-level Enrollment Rate, Subsamples				
IV, predetermined contextual controls ^{a,b}	0.516 (0.317) [8.61] [‡] {p = 0.072}	-0.286 (0.785)	0.679*** (0.251) [9.54] [‡] {p = 0.049}	-0.066 (0.744)
IV, predetermined contextual & school characteristics controls ^{a,b,d}	0.508 (0.318) [13.16] [‡] {p = 0.011}	-0.174 (0.708)	0.631** (0.272) [13.79] [‡] {p = 0.008}	0.031 (0.718)
IV, sample with no overlap in secondary schools ^{a,b}	0.342 (0.448) [4.73]	-	0.646** (0.312) [9.92]	-
IV, sample with no overlap in secondary schools, school characteristics controls ^{a,b,d}	0.352 (0.433) [5.00]	-	0.610* (0.336) [9.52]	-

Notes to Table 6: Each coefficient estimate is from a separate regression. Coefficient estimates from OLS and 2SLS regressions are reported. Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages, but not across villages; significantly different from zero at (*) 90%, (**) 95%, and (***) 99% confidence. First stage F-statistics or χ^2 -statistics of significance of partial correlation between IV (treatment indicator) and social network measure are reported in brackets.

(a) Individual and HH-level controls are the child's gender, indigenous status, the household's welfare index, education, age, and gender of the head of household, family size, and distance to secondary school. These are included in all specifications.

(b) Village predetermined contextual controls are the proportion of secondary-school-age girls and the proportion of indigenous children in the village, mean village-level family size and educational level, age, and gender proportion of heads of households.

(c) Expenditure related contextual characteristics are mean village-level HH expenditures, mean educational, food, boys' clothing, girls' clothing, alcohol and tobacco expenditure shares, and an indicator variable for whether the village suffered a flood shock.

(d) School characteristics are indicator variables for general, technical, secondary schools (relative to 'tele-secundaria' schools), urban and semi-urban school indicators (relative to rural schools), school-level pupil/teacher ratio, and the number of home teachers, teaching assistants, PE teachers, and art teachers in school.

(e) School composition controls are the mean village-level contextual characteristics mentioned above for children enrolled in secondary school.

[‡] = χ^2 (4) statistic from Seemingly Unrelated Regression of two first-stage regressions with (village-level) bootstrapped-clustered standard errors, 1000 replications.

Table A1: Relationship between attrition and characteristics of children at baseline

	<u>Dependent variable:</u> Attrition indicator							
	-----Ineligible Children-----				-----Eligible Children-----			
	Treatment	Correlates	Main effect of correlates	Interaction of correlates with treatment	Treatment	Correlates	Main effect of correlates	Interaction of correlates with treatment
	(1)	(2)	(3)	(3)	(4)	(5)	(6)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Treatment village	-0.006 (0.005)	0.008 (0.010)	0.102 (0.122)		0.000 (0.003)	0.005 (0.006)	0.016 (0.075)	
Treatment * Year 1998	0.008 (0.020)		-0.005 (0.021)		0.002 (0.011)		0.004 (0.010)	
Treatment * Year 1999	0.034 (0.025)		0.017 (0.026)		0.022 (0.014)		0.029** (0.014)	
Year 1998	0.208*** (0.016)	0.150*** (0.010)	0.154*** (0.016)		0.196*** (0.009)	0.140*** (0.005)	0.138*** (0.008)	
Year 1999	0.201*** (0.018)	0.095*** (0.013)	0.087*** (0.018)		0.199*** (0.011)	0.096*** (0.007)	0.078*** (0.010)	
Child's age		0.062*** (0.003)	0.058*** (0.004)	0.005 (0.005)		0.059*** (0.002)	0.061*** (0.003)	-0.005 (0.003)
Grade completed in 1997		-0.029*** (0.005)	-0.027*** (0.008)	-0.004 (0.010)		-0.019*** (0.003)	-0.026*** (0.004)	0.012** (0.005)
Gender (boy)		-0.013* (0.008)	-0.022* (0.012)	0.016 (0.015)		-0.021*** (0.004)	-0.017*** (0.006)	-0.008 (0.008)
Indigenous		0.034 (0.021)	0.019 (0.031)	0.029 (0.041)		0.003 (0.007)	0.011 (0.012)	-0.014 (0.015)
Family size		-0.004** (0.002)	-0.001 (0.003)	-0.005 (0.004)		-0.002** (0.001)	-0.002 (0.002)	0.000 (0.002)
HOH education		0.002 (0.002)	0.003 (0.003)	-0.002 (0.004)		0.000 (0.001)	-0.001 (0.002)	0.001 (0.002)
HOH gender (male)		-0.010 (0.017)	-0.012 (0.023)	0.006 (0.034)		-0.029*** (0.010)	-0.027* (0.016)	-0.004 (0.020)
HOH age		0.001* (0.000)	0.001 (0.001)	0.000 (0.001)		0.000 (0.000)	0.000 (0.000)	-0.001 (0.000)
Dist. to sec. school		0.003	0.004	0.000		0.003* (0.003)	0.003	0.000

		(0.003)	(0.004)	(0.005)	(0.002)	(0.003)	(0.003)
Dist. to urban center		0.000	0.001**	-0.001*	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)	0.000	0.000	0.000
General sec. school		-0.053	-0.012	-0.056	0.016	0.002	0.012
		(0.034)	(0.072)	(0.083)	(0.019)	(0.028)	(0.038)
Technical sec. school		-0.067**	-0.073	0.026	0.001	-0.014	0.030
		(0.027)	(0.051)	(0.066)	(0.015)	(0.025)	(0.033)
Urban school		0.011	0.149	-0.156	-0.044**	0.078	-0.145**
		(0.029)	(0.174)	(0.176)	(0.022)	(0.059)	(0.064)
Semi-urban school		-0.003	-0.033	0.043	0.005	0.014	-0.009
		(0.023)	(0.025)	(0.041)	(0.013)	(0.018)	(0.026)
Num. of home teachers		0.001	0.009	-0.013	0.007	-0.003	0.013
		(0.012)	(0.029)	(0.032)	(0.006)	(0.014)	(0.015)
PE teachers		0.029	0.023	-0.001	0.003	-0.025	0.042
		(0.038)	(0.069)	(0.086)	(0.019)	(0.025)	(0.035)
Art teachers		-0.064*	-0.077	0.043	-0.007	-0.017	0.014
		(0.034)	(0.054)	(0.067)	(0.018)	(0.021)	(0.035)
Teaching teachers		0.021	-0.008	0.039	-0.001	0.004	-0.006
		(0.013)	(0.021)	(0.026)	(0.007)	(0.008)	(0.013)
Num. of teachers		0.007	0.003	0.008	-0.007	0.002	-0.012
		(0.013)	(0.031)	(0.034)	(0.006)	(0.014)	(0.016)
Pupil teacher ratio		0.000	0.000	-0.001	0.000	0.000	-0.001
		(0.001)	(0.001)	(0.001)	0.000	(0.001)	(0.001)
Constant	0.011**	-0.681***	-0.755***		0.006***	-0.596***	-0.605***
	(0.005)	(0.056)	(0.104)		(0.002)	(0.031)	(0.066)
State indicators	Yes	Yes	Yes		Yes	Yes	Yes
State * Treatment							
indicators	No	No	Yes		No	No	Yes
Observations	8214	8184	8184		33441	33351	33351
R-squared	0.08	0.16	0.17		0.08	0.15	0.15
Interactions F-statistic		-	1.29		-	-	1.36
P-value			[0.149]				[0.105]

Notes to Table A1: Robust standard errors in parentheses; disturbance terms are allowed to be correlated within villages, but not across villages. Significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. F-statistic of joint significance of interaction terms (F(28,379) reported at bottom of the table.