

SOCIAL INTERACTIONS AND SCHOOLING DECISIONS

Rafael Lalive*, University of Zurich, CESifo, and IZA

Alejandra Cattaneo, University of Zurich

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Abstract

This paper provides evidence on the role of a child's peers in determining the decision to acquire education. Such social interactions may be important because children enjoy being similar to others or learn from other children. Identification is based on a randomized intervention that grants a cash subsidy encouraging school attendance among a sub-group of children within small rural villages in Mexico. Results indicate that (i) the *ineligible* children's decision to attend school is affected strongly by the school attendance choices in the peer group, (ii) social interactions are equally important among boys and girls, and in the upper grades of primary school and lower grades of secondary school, and (iii) the direct effect of the cash grant on the eligible children is roughly of equal size as the effect generated via social interactions.

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1 Introduction

”Curiously enough, before, when one of the students would get sick, one or two of his closest friends would also not show up to school. When PROGRESA began to pay, well, one way or another they were given warning that absences will not be looked over or unnoticed, right?” – School director cited in Adato *et al.* (2000, p. 54)

The decision to acquire knowledge is perhaps one of the most important decisions of a person’s life. Economics has been particularly influential in developing a *human capital* framework that explains schooling decisions (Becker, 1964). This framework holds that a child will acquire education to the extent that the benefits of doing so outweigh the costs. Economists have typically focused on the *individual* monetary benefits to additional schooling – the return to education – and on individual foregone earnings to model education decisions (Card, 1999). Yet it is also true that there are substantial social benefits to schooling and that acquiring education is a process that is affected by social exchange of information. With respect to social benefits, it is important to bear in mind that the school is a setting where children meet their friends and have an opportunity to play with other children.¹ Likewise solving the schooling problem requires information. The school is the most important setting where parents interact with other parents to share information regarding the potential benefits and costs of schooling.²

This paper aims to provide evidence of the importance of the social determinants of schooling among children enrolled in the upper grades of primary school in rural Mexico. Children in the small villages we investigate start leaving school for work when they reach age 12 – notably two years before they reach the minimum age required for employment. To address this schooling problem, the Mexican ministry of education devised in the late 1990s an innovative program – the PROGRESA program – designed to encourage school attendance among the very poor. The program consists of a cash grant paid to the mother of a poor household for each child in grade 3-6 of primary school or grade 1-3 of secondary school who attends school regularly, i.e. more than 85 % of all school days during every 2 month period of the school year. Clearly, this program directly increases the incentive to attend school among eligible children. Indeed, the existing evidence indicates that this program is highly successful in increasing primary and secondary school attendance rates by around 6-10 percentage points.³ The basic

¹Akerlof and Kranton (2002) for a model that discusses how identity affects the economics of education.

²See Manski (2004) for a recent theoretical discussion of social learning.

³See Behrman *et al.* (2001), Buddelmeyer and Skoufias (2003), and Attanasio *et al.* (2003) for evaluations of this program.

idea of this paper is to assess the response of the ineligible children – living in a household that has not been classified as poor – to introducing the program in their peer group. The peer group comprises all children who live in the same small village with the same gender who have attained the same grade level. Whereas the program does not change the monetary benefit of attending school among the non-poor children, the program increases the social benefits of staying in school and it might change the expected payoff to acquiring schooling as perceived by the parents. Thus, the spillover effect of the program on the ineligible children provides evidence on the role of social interactions in schooling decisions – an approach that has recently been put forward by Moffitt (2001).

PROGRESA is ideally suited to address the two important problems in identifying social interactions – the "omitted variable" problem and the "reflection problem" (Manski, 1993, 1995, 2000). First, the program was implemented at random in 320 rural villages and denied at random to 186 remaining villages. The fact that the program was implemented at random in some villages but not in other villages because randomization ensures balancing of all determinants of school attendance. Thus, randomization directly addresses the "omitted variable" problem. The "reflection problem" holds that it is difficult to disentangle the effect of group behavior on individual behavior since the individual is also affecting the behavior of group members – a classical simultaneity problem. Yet, PROGRESA manipulates peer group school attendance while leaving unaffected the ineligible child's monetary incentive to attend school.⁴ This means that the response among ineligible children provides information on how strongly the peer group affects the individual and not vice versa. This addresses the "reflection problem".

Results indicate, first, that there is a positive and statistically significant average spillover effect of the program on ineligible children. We also find that the ineligible child's response is larger the higher is the eligible fraction of children among their classroom peers. Second, when we combine the response of the ineligible student with information on the effect of PROGRESA on peer group schooling, we find that the ineligible students' schooling decisions are strongly, and statistically significantly, affected by their peer's decision. There is a 6 percentage point increase in school attendance for every 10 percentage point increase in schooling among peers. Third, we find that a higher fraction of parents in PROGRESA villages think their child

⁴The ineligible children might be affected indirectly by the program if the poor share their grants with the non-poor. However, there is no evidence of direct sharing of the benefits. Furthermore, the poor appear to have used PROGRESA transfers to cover schooling costs (school material and transportation) or purchase children's clothes. Only about 20 out of the 506 villages have local markets where children clothes are sold. This means that the additional income of the poor is unlikely to have altered income among the non-poor.

will reach the upper secondary level supporting the idea that the program affects the parents expectations regarding schooling choices. Fourth, we find that the direct effect of the cash subsidy on school attendance is about as large as the social spillover effect among children from poor households. Thus, the average total effect of PROGRESA on eligible children can be explained via both the direct effect of the subsidy on the individual and the indirect effect of the subsidy on the social environment of poor children.

These findings are important for at least three reasons. First, our findings imply that the social environment is relevant in affecting one of the most important investment decisions. Second, strong social interactions in schooling decisions imply that the effects of educational interventions are amplified due to a social multiplier process.⁵ Third, evidence on the quantitative importance of social interactions is essential in thinking about targeting of this program. PROGRESA was set up to target poor households. If social interactions are important, the average effect of the program on the poor is larger if many other poor households within the same village are eligible for the program.

There is a rapidly expanding literature on social interactions in schooling.⁶ Important studies address the relevance of unobserved determinants in generating peer effects using panel data. Hoxby (2000) identifies peer effects from gender and race variation. Sacerdote (2001) studies peer effects among college freshman at Dartmouth college who are assigned to dorms at random. Hanushek *et al.* (2001) study how peer ability affects student achievement. Betts and Zau (2004) use administrative data to study peer groups and academic achievement. However, using panel data does not address the "reflection problem". The earliest study that uses instruments to solve the endogeneity problem is Case and Katz (1991) who use instrumental variables to study neighborhood effects in the Boston area. Using PROGRESA data, Bobonis and Finan (2002) document strong spillover effects on the ineligible students. Duflo and Saez (2003) study the role of information and social interactions in retirement plan decisions in a field experiment. Miguel *et al.* (2004) study a merit-based incentive program in Kenya that generates strong effects among eligible girls but also spillover effects on ineligible boys.⁷

This paper contributes to the literature in three important ways. First, the empirical evidence is based on a social experiment that manipulates the actions and decisions of a students' peers while leaving unaffected the student. Such an empirical design is essential in solving the "reflection problem". Second, this paper highlights the social component in

⁵See Glaeser *et al.* (2003) for a discussion of the social multiplier.

⁶See Glaeser *et al.* (1996) on social interaction in crime, and Lalive (2003), Topa (2001), and Topa and Conley (2002) for social interaction in unemployment.

⁷A related issue arises in studying the impacts of deworming treatment in developing countries. Miguel and Kremer (2004) find strong epidemiological spillovers generated by deworming treatment in rural Kenya.

the spillover effect due to PROGRESA by focusing on the differential impact of the program along the eligible fraction dimension. This is important because spillovers on the aggregate level may be due to a number of reasons that are not related to the peer group. Third, this paper provides empirical evidence on the relevance of social forces in affecting one of the most important decisions – the amount of time to be invested in acquiring knowledge.

The paper is organized as follows. Section 2 presents background on Mexico and PROGRESA. Section 3 discusses the data and presents descriptive evidence. Section 4 discusses the identification strategy. Section 5 presents the main results, and Section 6 concludes.

2 Mexico's Education System and PROGRESA

According to the general education act from 1993 the educational system in Mexico comprises 3 levels: basic, which is sub-classified in pre-primary, primary and lower secondary; upper secondary and tertiary. However the only two types that are obligatory for all Mexican citizens according to the Mexican political constitution are primary and lower secondary school. Primary school is imparted for children and adults in three different modalities: general; bilingual-bicultural, provided for indigenous people; and community courses, provided for people in small or isolated communities. According to data from the secretariat of public education (SEP), 92.8% of children were covered by primary education in the periods 1997-1998 and 1998-1999. The ratio of successfully completed courses was slightly higher for the period 1998-1999, 85.8% against 84.9% in 1997-1998. The lower secondary school comprises three grades and it is provided to those children who have previously completed primary school. According to the SEP, the absorption rate for the secondary level was 87.8% and 90% for the periods 1997-1998 and 1998-1999 respectively. The coverage also increased in the same period from 75.7% to 77.7% as well as the ratio of successfully completed courses which increased from 73.8% to 76.1%. The Mexican school system is quite centralized. Schools have limited decision-making autonomy, with only 22 % of all decisions taken at the school level, 45 % of all decisions taken at the state level and 30 % taken at the central level (OECD, 2004).

Whereas Mexico has made substantial progress in terms of average educational attainment, there is still a strong discrepancy in terms of education attainment between rural and central areas (Hanson, 2002). This is because many rural villages have a local primary school but the secondary schools are only present in larger villages or small cities. Thus, distance to secondary school is an important factor explaining low educational attainment. Distance to school is perceived to be particularly problematic and dangerous for girls (Adato *et al.*, 2000, p. 73). Also, poor families simply can not afford to send all children to primary and secondary

school. Yet, economic constraints are not the only reasons cited for not attending the secondary school but also children do not want to continue with school due to laziness, boredom with school and preference for work, girls would rather be with their boyfriend than in school, teachers treat children badly, children want their own income rather than study (Adato *et al.*, 2000, p. 72). After school, most children in rural areas are expected to perform a variety of household chores such as taking care of animals, help out in the kitchen, gather firewood, help out in building a fence, etc. These tasks are demanding and require balancing the schoolwork and the housework schedules (Adato *et al.*, 2000, p. 66).

In order to encourage enrolment and permanence in school of children and teenagers under 18 years old who attend grades between third of primary and third of secondary the Mexican government created PROGRESA (Programa de Educacion, Salud y Alimentacion)⁸ which is a program aimed at increasing the opportunities and complementing the income of Mexican families living in conditions of extreme poverty. It has three components: education, health and nutrition. PROGRESA's health and nutrition components primarily target children aged 0-60 months (Gertler, 2004). Other family members visit clinics once a year for checkups and receive information concerning health prevention and nutrition at monthly meetings (*pláticas*).

PROGRESA's educational component consists of cash transfers which are provided to poor families every two months during the school year (August to June) conditional on sending their children to school. The cash grants have two particularities: the sums granted increase as children reach higher grades and – in the secondary school – the sums awarded to girls are slightly higher than those for boys to compensate for the slightly higher proportion of girls dropping out of school. In the period August-December 1998, the amount of the grants increased from 70 to 135 pesos per month between the third and sixth grade of primary school and almost doubles for children attending the first grade of secondary school.⁹ In order to avoid reducing a household's incentives for self-help the total monthly monetary transfer a family could receive was limited at 525 pesos in August-December 1998. The nominal values of the cash transfers are adjusted every 6 months to take into account changes in cost of living. The cash subsidy is handed out to the mother because of the belief that the mother is usually better administrating the household resources and because women are disproportionately vulnerable to poverty. The grants are awarded only after confirming the children's attendance to school at

⁸The Program was re-named Oportunidades under the Fox administration.

⁹Specifically, the cash subsidy is 70 pesos in grade 3, 80 pesos in grade 4, 95 pesos in grade 5, and 115 in grade 6 of primary school. The cash subsidy then jumps to 200 (210) pesos in grade 1 of secondary school for boys (girls) with marginal increases in grades 2 and 3 averaging again 10-20 pesos. Beneficiary families also receive help for the school material, which equaled 135 pesos for primary school and 170 for secondary school for the period August-December 1998

least for the 85% of the school year. If they fail to fulfill this requirement they lose the grant, at first temporarily and then permanently. Attendance is monitored by school teachers. There are only few reports of parents trying to influence teachers to misreport attendance (Adato *et al.*, 2000). The PROGRESA transfers go directly from the Federal Budget to beneficiary households.

The most important advantage of this program from the perspective of this paper is that PROGRESA is a partial-population intervention that was phased-in at random.¹⁰ Specifically, the program was implemented in three steps. In the first step, the Mexican government selected an initial set of 506 rural villages which are characterized by a high degree of "marginality", but with access to education and health facilities, on the basis of the 1990 and 1995 census.¹¹ These 506 villages are located in seven states: Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí and Veracruz. These states form a loose cluster around Mexico City and they are broadly representative of Mexico in terms of the production structure.

In the second step, PROGRESA determined the poverty status of each of about 24,000 households living in these 506 villages based on survey information collected in October 1997. Basically, the poverty status of the household was determined using information on educational attainment of the household head and her or his partner, dwelling characteristics, and other information that predicts the per capita household income. The poverty status of a household was determined using the same information and the same procedure in all seven states. On average, this procedure led to 52 % of all households being classified as poor.¹² Yet, because the poverty status of all families was determined using the same procedure, there is substantial variation with respect to the percentage of households that are eligible within a village. This variation arises primarily due to the fact that within some villages, the mass of all households is located in the area characterized as poor, and in other villages, the mass of all households is classified as non-poor. In the empirical analysis, we will use the information on the poor fraction within a child's grade level and gender as a measure of how strongly PROGRESA can be expected to have changed schooling decisions in the peer group.

In a third step, PROGRESA determined at random a set of 320 villages where the program was implemented as of August 1998. The remaining 186 villages were excluded from the list of

¹⁰See Skoufias (2001) for an in-depth discussion of the implementation of the program.

¹¹The marginality index compresses information on literacy, share of dwellings without water, drainage or electricity, average number of occupants in one room, share of dwellings with dirt floor, and share of population working in primary sector into one variable by means of a principal components analysis.

¹²PROGRESA staff determined the poverty threshold such that 52 % of all households would be classified as poor. In July 1999, PROGRESA added a further 26 % of all household to the list of beneficiaries due to complaints that the initial procedure discriminated against households whose children had already left home.

PROGRESA villages until the end of the 1999 / 2000 school year. This randomized phasing-in of the program allows evaluating the impacts of PROGRESA in a randomized design.¹³

Thus, PROGRESA is a program that affects only a part of the entire population of a village directly – it is a partial population intervention. Is this program useful in identifying social interactions? If children from poor households only interact with other children from poor households, there could be important social spillover effects that can not be detected with the PROGRESA experiment. However, any social interactions that we find can be thought to represent a lower bound on within poor / within non-poor social interactions. Moreover, there is strong evidence that the inhabitants of the 506 rural and extremely poor villages in Mexico felt that the selection of poor families was quite arbitrary.¹⁴ This suggests that social relationships exists also between the poor and the non-poor. Moreover, the fact that the program was implemented did not seem to change existing social relationships.¹⁵

The basic idea of this paper is that children from non-poor households also might decide to stay in school longer when PROGRESA leads children from poor households within their village to stay in school longer. What are the likely reasons for this? Children like to spend time with their friends as the statement by the school director cited in the introduction clearly shows. Thus, when prior to PROGRESA a child from a poor household reported sick and some of his or her friends from non-poor households curiously also missed school, the non-poor children now have no reason to miss school when all children from poor households attend school regularly. This explains why the rate of school attendance might be affected by social interactions. A more important issue refers to why school enrolment might be affected by social interactions. With respect to enrolment there are two important considerations. First, there is strong evidence suggesting that children in rural areas in Mexico have a substantial degree of influence on school enrolment decisions. Thus, to the extent that spending time with classmates is an important reason for attending school, school enrolment of the non-poor will increase when fewer friends among the poor decide to leave school. Second, PROGRESA might change the way parents from non-beneficiary household evaluate the career prospects of their children in several ways. They might be worried about a decrease in the chances of their children to secure good jobs, they might receive new information concerning the value of education, and they might imitate the actions of other parents who are sending their children

¹³Randomized evaluation proved crucial in securing a loan from the World Bank to roll out the program in all of Mexico in 2000.

¹⁴”Among beneficiaries, non-beneficiaries and *promotoras*, there was a strong view expressed that ‘everyone is poor’ – a sense of common identity in poverty.” (Adato, 2000, p. vi)

¹⁵”Many comments were made suggesting that beneficiaries and non-beneficiaries continue to get along with each other fine and ‘the same’ as before.” (Adato, 2000, p. vi)

to school more frequently.

It is essential to discuss alternative hypotheses that motivate a spillover effect on the children living from ineligible households. It might, first, be possible that poor households share the education grants with non-poor households. In the official evaluation Adato *et al.* (2000) and Bobonis and Finan (2002) do not find sharing of benefits to any substantial extent. This is probably due to the fact that the transfers are not perceived as salient. According to in-depth focus group research, these benefits are helpful in financing a child's education but they do not pay for much else (Adato *et al.*, 2000). Second, even if the poor do not share the grants directly with the non-poor, they might spend the additional income in shops owned by the non-poor thus also directly affecting the incomes of the non-poor. The existing evidence shows that the transfers are mainly used to finance the children's education and clothes for children (Bobonis, 2004). Detailed village level data suggests that the grants are spent outside the village rather than in the local village. Only 20 out of the 506 villages have a local supermarket or street market. This means that it is not likely that the program has changed incomes of the non-poor noticeably. Third, it might be that the non-poor misunderstood the working of the program and believed that they are eligible as well. This is unlikely to be the case. Between October 1997 and the start of the program in August 1998, PROGRESA held public meetings in which the eligibility status of each household was clearly communicated. Moreover, the education subsidy was administered using two forms. The E1 form recorded background information on all children from eligible household. These E1 forms were the basis of the E2 form, a list of eligible children, which was sent out to each school which is attended by eligible children. The E1 forms were not distributed to non-beneficiary households. Fourth, ineligible children may also have attended school more regularly due to increases in the quality of teaching, or the quality of schools. However, in their in-depth evaluation of PROGRESA, Adato *et al.* (2000) do not find any change in terms of the overall quality of education in PROGRESA villages compared to control communities. In PROGRESA villages, 54.9 % of all school directors state that the overall conditions of the school has improved since PROGRESA started. In control villages, the corresponding figure is 9 percentage points *higher* (63.9 %) motivating a concern with underestimation of the spillover effect. Also, with respect to teachers there were no differences in terms of the number of teachers nor with respect to additional incentives for teachers. This fact can, arguably, be explained via the relatively centralized nature of the Mexican school system that leaves little autonomy to the local schools.

3 Data and Descriptive Evidence

3.1 Data

The official PROGRESA evaluation database contains annual survey information on school attendance, socio-economic characteristics, and localities between October 1997 and November 1999.¹⁶ The empirical analysis primarily uses information on two waves, October 1997 and October 1998. The first wave provides information on school attendance and socio-economic background before the program was implemented. The second wave is useful in assessing the effect of the program because the program was implemented in August 1998. We also use information on the second year of the program, collected in November 1999, in a sensitivity analysis that discusses the longer term impact of the program.

We concentrate on children living with their mother who have completed grades 3 to 6 of primary school in October 1997.¹⁷ This sample consists of children from poor households whose family directly became eligible when PROGRESA was introduced, and children from non-poor households whose family did not become eligible in August 1998 but who are expected to know eligible children in their classroom and village. Moreover, this sample covers children who have not yet finished primary school (children having completed grades 3 and 4) as well as children making the transition from primary school to secondary school or child labor (children having completed grades 3 or 6). Thus, the sample allows discussing whether social interactions are relevant in attendance and drop-out decisions. Second, we concentrate on children aged 6 to 16 years in October 1998 because the outcome indicator – school attendance – was only collected for children in this age group. We end up with a sample of 15,653 children of which 9,690 live in "treated" villages – where the program was implemented in August 1998 – and 5,963 live in "control" villages that were denied access to the program in August 1998. Note that this

¹⁶To our knowledge, the administrative data generated in paying out the subsidy is not available for researchers. PROGRESA also collected information during the spring. The number of completed interviews in the spring is, however, 20 % lower than in the fall. This likely reflects the seasonal pattern of field work in the villages. Since non-response may be non-random, we focus on the interviews conducted in the fall rather than in spring.

¹⁷The sample selection can not be based on the grade attained in October 1998 because grade attainment in 1998 is already affected by PROGRESA and, therefore, endogenous. Grade attained in 1997 is also determined by a number of factors we do not observe. However, randomized implementation of the program ensures that grade attainment is the same in PROGRESA villages and control villages. Moreover, this criterion rules out children who have completed grades 1-3 of secondary school in the year prior to the start of the program. However, note that (i) for many of these children we have no information on school attendance in October 1998 (this item is only available for children aged 6-16), and (ii) many villages do not have a local secondary school – implying that the children in the local village are only a part of the social network.

sample comprises both children who are still enrolled in school as well as children who have already left school either temporarily or permanently. This is advantageous since our focus is to study the effect of social interactions on overall schooling decisions. Moreover, the data indicate that temporary school exits are quite common, especially among children who have completed primary school. Thus, non-enrolment in a particular year is not an indicator of permanent school exit.

3.2 Descriptive Evidence

Table 1 provides descriptive statistics on the background characteristics of the children in the sample. Panel A in Table 1 reports statistics for the 10,484 children living in poor households. The distribution of children across grades is slightly skewed to the right. The fraction of children having completed grades 3, 4, and 5 is about 6 percentage points lower than the fraction of children having completed grade 6. This is due to the fact that some children for whom the highest grade attained is the sixth grade in primary school have already left the education system. Parental education – a powerful predictor of household income – is very poor.¹⁸ Roughly 16 percent of all children have a mother or father who has completed primary school, i.e. has reached grade 6 of primary school or a higher grade level.¹⁹ Children in poor households also do not tend to live in dwellings with a cement floor or firm roof. (The omitted categories concerning the roof type refer to roofs made of cardboard or palm leaves.) Whereas all villages have a local primary school, many villages do not have a local secondary school as evidenced by the fact that only about one quarter of all children live in a village with a local secondary school. Thus, the costs of attending secondary school are large and the incentive to finish primary school is weak.

Table 1 about here

Comparing Panel A and Panel B in Table 1 allows to infer to what extent our proxies for household income predict poverty status of the household. There is strong evidence that poverty status is related to parental education, and dwelling characteristics. The percentage of

¹⁸We have constructed a direct measure of household income. It turns out that this measure does not add to the empirical analysis we report below. We do not report the constructed household income measure for two reasons. First, household income is much more likely subject to reporting bias. Second, it is well known that measuring household income is difficult in agricultural societies due to the importance of home production.

¹⁹Note that parental education is affected by non-response. For about 33 percent of all children, there is no information regarding parental education. Further analysis (not shown) of this fact indicates that non-response increases strongly with age. This suggests that non-response refers to no schooling at all. Results are not sensitive to adding an indicator that reflects non-response concerning parental education.

children with a mother having completed primary education is about 16 percent among poor children (Panel A), and 22 percent among non-poor children (Panel B). Father primary school completion follows a similar pattern. With respect to dwelling characteristics, we find that whereas only about 25 percent of all poor children live in a dwelling that has a cement floor (as opposed to a dirt floor), the corresponding figure is roughly 60 percent among non-poor children. The fraction of children living in a dwelling with a permanent roof type is much higher among non-poor children than among poor children. There is also an interesting difference between poor and non-poor children with respect to grade. We find that the fraction of children having completed grade 6 is 34 percent among children from non-poor households but only 28 percent among children from poor households. This is consistent with a higher fraction of children dropping out of school before completing primary school among poor children.

The main result in Table 1, however, is that there is no difference between villages with PROGRESA and control villages with respect to any of these important background characteristics of poor and non-poor children. This suggests that randomization successfully generated independence between PROGRESA status of the village and observed (and potentially also unobserved) characteristics. Thus, the effects of PROGRESA on school attendance can be identified convincingly because treatment differences in terms of school attendance are likely due to implementing the program rather than due to differences in terms of the average poverty level.

Table 2 reports descriptive evidence on the effect of the cash subsidy on school attendance. School attendance is a binary indicator variable taking the value 1 if the child attends school at the date of the interview, and zero otherwise.²⁰ School attendance reflects both attendance and enrolment. Panel A in Table 2 reports effects for the eligible children living in poor households. On average, only about 77 % of all children in grades 3-6 attend school in control villages in October 1997. In treated villages, school attendance is slightly higher, 78 %, one year prior to the start of the program. However, the treatment contrast is not significant at any conventional level of significance. One year later, in October 1998, school attendance is 69 % in control villages – 8 percentage points lower than the year before. This means that a substantial fraction of children in our sample have dropped out of school in control villages. In contrast, in treated villages school attendance is 76 % – only 2 percentage points lower than the year before. This means that the program increased school attendance by 6 percentage points – a significant impact both in the economic and statistical sense.

²⁰A second important outcome is achievement test scores. However, there is no effect of PROGRESA on test scores (Behrman *et al.*, 2000).

Table 2 about here

Panel B in Table 2 discusses the spillover effect of the cash subsidy on the ineligible children whose household was not classified as poor. The idea is that children from non-poor households in villages with PROGRESA do not receive a cash subsidy but they are living in the same village as children whose school attendance has been strongly increased. The control villages provide information on the counterfactual situation without PROGRESA. The data indicate that school attendance is about 76 % in treated villages, and about 78 % in control villages about one year before the program was introduced – the treatment contrast being insignificant. By October 1998, school attendance has dropped by 7 percentage points in control villages but only by 5 percentage points for ineligible children in treated villages. Thus, the program appears to have increased school attendance slightly, by 2 percentage points, among ineligible children. However, this "spillover" effect is not significantly different from zero. Thus, results in Table 2 indicate that PROGRESA strongly increases schooling for the eligible children but only very weakly for the ineligible children suggesting weak or no social spillovers of the program.

Table 3 reports the effect of PROGRESA on poor and non-poor children's change in school attendance using linear regression analysis that controls for all the observed characteristics of children. The change in school attendance is the difference between the school attendance indicator between October 1998 and October 1997 for each child. This implies that Table 3 reports a within individual difference-in-difference analysis of the effect of PROGRESA on school attendance. It is advisable to use the difference-in-difference strategy since evidence in Table 2 indicates that school attendance levels are slightly higher in treated villages compared to control villages before the program was implemented. Inference is based on robust standard errors allowing for clustering at the village level.

Results indicate that there is a statistically significant and quantitatively important increase in school attendance trends among poor children (Table 3 Panel A). Villages with program experience a 5.8 percentage points weaker downward trend in school attendance compared to villages without the program. Second, controlling for observed characteristics has virtually no impact on the estimated effect of PROGRESA among poor children. There is a strong grade related pattern in school attendance trends. Children who have completed grade 4 are affected by a slightly higher drop in school attendance than the reference – children who have completed grade 3. Children in grade 6 (having completed grade 5) experience a 19 percentage points stronger reduction in school attendance than children in grade 4 (having completed grade 3). This shows that the decision to leave school is primarily taken in grade 6. Interestingly, upon

primary school completion (children who have completed grade 6) there is a slightly more favorable trend in school attendance than for children having completed grade 3. A more detailed analysis (not shown) indicates that there is an equal proportion of students attending school in October 1997 but not attending school in October 1998 (school leavers) and students with exactly the reverse pattern (school entrants) among the children who have completed grade 6. This shows that PROGRESA not only affects those who are currently in school but also those who do not attended school. In addition to grade level, missing father information reduces the trend in school attendance and local presence of the secondary school increases the trend in school attendance. There are no further statistically important determinants of the trend in school attendance. This is in line with an interpretation that analyzing the trend in school attendance is robust to (observed and unobserved) time-invariant determinants of schooling.

Table 3 about here

The second column in Table 3 reports results for the non-poor. Implementing PROGRESA increases the trend in school attendance in villages with program by 2.1 percentage points compared to the villages without the program. This estimate is slightly higher than the estimate reported in Table 2, and it is statistically significant at the 10 percent level. The results concerning the correlation between trends in school attendance are similar among the non-poor as among the poor, except for grade 6 (not significantly different from zero), and missing father information (not significant). Moreover, father education status is positively correlated with the trend in school attendance. Children whose father has completed primary school tend to have 3.3 percentage points higher change in school attendance.

An interesting first result emerges from this discussion. In the context of the PROGRESA experiment, we not only find an effect among eligible children but a weaker effect is also present among ineligible children. This suggests that the direct monetary incentive effect of PROGRESA is smaller than the 5.8 percentage point average effect of treatment on the treated reported in Table 3 Panel A. The reported effect appears to capture not only the direct effect via financial incentives but also also an additional social effect that is not limited to beneficiaries. In the following section we discuss how to identify the social effect from data on the non-poor and how to separate the direct effect of the transfer from the social effect.

4 Identification

4.1 Identifying Social Interactions

This section discusses how the preliminary analysis from the previous section can be used to identify the relevance of social interactions in schooling decisions. Let S_i denote the change in school attendance between October 1997 and October 1998 of child i . Let $P_i = 1$ if the child lives in a household classified as poor by PROGRESA, and $P_i = 0$ otherwise. Let $T_i = 1$ if PROGRESA has been implemented in the village in August 1997, and $T_i = 0$ otherwise. Let $S_{(-i)g}$ reflect the average change in school attendance in the peer group, excluding individual i . We define the peer group of child i to contain all other children in the same grade *level* and same *gender*. Grade level refers to completion of grades 3-4 or completion of grades 5-6. Both characteristics are very important in defining the social group that individuals identify with. From the social learning perspective, this definition of the peer group is reasonable since it connects each individual i with other individuals who are *similar* in terms of the incentives to leave school (which differ strongly across grades and gender). We exclude 28 children for whom the peer group is empty.²¹

The linear model of social interactions (Manski, 1993) can be used to characterize the salient determinants of school attendance as follows

$$S_i = \alpha_0 + \alpha_1 T_i + \alpha_2 P_i + \delta T_i * P_i + \gamma S_{(-i)g} + \epsilon_i \quad (1)$$

There are two salient parameters of interest in equation (1). The parameter γ measures the extent to which individual change in school attendance is affected by peer group average change in school attendance. Such an effect can be due to either students conforming to the actions of their peers or students/parents learning from the actions of other students who are similar to student i . The parameter δ measures the direct effect of the schooling subsidy. This parameter captures the extent to which school attendance trends differ between the children from poor households in treated villages who are receiving the cash grant from the counterfactual trend in schooling for such children in control villages. Note that identifying the extent to which the cash grant changes individual decisions requires controlling for any change in the social environment $S_{(-i)g}$, provided that social interactions are relevant, i.e. $\gamma \neq 0$. The remaining parameters in equation (1) capture omitted differences across regions (α_1) and omitted and

²¹In defining peer groups we balance the cost of losing observations due to empty peer groups with the benefits of tightly defined peer groups. While defining the peer group to contain the children in the same grade does not change results in important respects, this results in a substantially stronger loss in terms of the number of observations with non-empty reference groups.

actual differences across poor and non-poor children (α_2).

The empirical analysis we report proceeds in two steps. Step 1 aims at identifying the social interactions parameter γ . The evidence in Table 3 can be used to illustrate how we can identify this parameter. Contrasting the change in school attendance among non-poor children in villages with PROGRESA and control villages measures

$$\begin{aligned} E[S_i|T_i = 1, P_i = 0] - E[S_i|T_i = 0, P_i = 0] &= \\ &= \alpha_1 + \gamma * (E[S_{(-i)g}|T_i = 1, P_i = 0] - E[S_{(-i)g}|T_i = 0, P_i = 0]) \end{aligned} \quad (2)$$

Equation (2) shows that the treatment contrast among non-poor children reflects social interactions provided that there are no direct changes in villages with program, i.e. $\alpha_1 = 0$. Let us discuss first the sense in which we can learn about social interactions among non-poor children and then discuss the potential threats to the validity of the identifying assumption. The spillover among ineligible children is informative on social interactions to the extent that implementing PROGRESA affects the average change in school attendance in the peer group. Thus social interactions can only be detected if the cash subsidy program is salient and if child i is interacting with a peer group which decides to acquire significantly more schooling. With respect to salience, we have shown strong evidence in Table 3 that PROGRESA affects schooling decisions among children from poor households. Furthermore, about 54 percent of all children in the peer group of the average non-poor child are from eligible households. This means that PROGRESA is suited, in principle, to identify social interactions in school attendance decisions.

With respect to the identifying assumption, note that PROGRESA was randomized over villages. Randomization will balance all (observed and unobserved) characteristics of villages. Our analysis, however, is based on the individual school attendance decision. Thus, the first argument questioning the validity of the identifying assumption is that randomization at the village level may not balance characteristics at the individual level. However, randomization at the village level also ensures balancing of characteristics as the number of villages increases. The PROGRESA program is a very large scale randomized intervention with 506 villages in total. This suggests that randomization at the village level also successfully achieves independence at the individual level. Furthermore, the evidence in Table 1 suggests that observed characteristics are balanced at the individual level. Nevertheless, we address possible imbalance with respect to individual characteristics by weighting each observation with the inverse of the total number of children in the village in the empirical analysis. Section 2 has discussed three further concerns with the identifying assumption: sharing of benefits, income spillovers,

and misperceived eligibility status. A priori, none of these concerns seem warranted. In the empirical analysis, we address the income spillover and the misclassification hypothesis directly. In addition, we also address long-run impacts, potential differences in local labor market conditions between PROGRESA villages and control villages, and focus on the subset of children who are on grade level.

Equation (2) shows that we can identify γ in a standard binary instrument instrumental variables setting, where the instrument is the PROGRESA status of the village T_i . This indicator is a predictor of the average trend in school attendance in the peer group provided that the transfer is salient and the peer group is characterized by a sufficiently high poor fraction. We thus apply standard instrumental variable methods to recover the social interactions parameter γ . This is our IV1 strategy.

Our second strategy recognizes the tremendous variation in the percentage of children from poor households within a non-poor child's peer group. Essentially, peer groups differ with respect to the poor fraction because PROGRESA applied the same poverty line to a set of villages which differ with respect to their location relative to the poverty line. This procedure led to a "poor fraction" in the peer group of a non-poor child between 0 and 100 percent, the average being 54 percent. The idea of the IV2 strategy is that the "poor fraction" is an important indicator of the salience of PROGRESA at the peer group level. This means that PROGRESA will lead to a stronger increase in schooling among peer groups where the poor fraction is high compared to peer groups with a low poor fraction. Thus, the poor fraction in the peer group also satisfies the first IV condition that an instrument must be correlated with the endogenous regressor.

The crucial question is whether the poor fraction in the peer group also satisfies the second instrumental variable condition that there is no direct effect of the poor fraction on school attendance among the non-poor. This condition need not be satisfied because PROGRESA was not randomized conditional on the poor fraction in the peer group. We thus investigate in detail whether the control villages are similar to villages with PROGRESA when we condition on the eligible fraction. To do this, we regress each of the observed characteristics listed in Table 1 on the "poor fraction in the peer group", the "Village WITH program" dummy variable, and the interaction term "poor fraction in the peer group * Village WITH program". The focus of this analysis is, primarily, on the interaction term which reflects the differences between control and treated villages with respect to the observed characteristic. We find that this interaction term is not significantly different from zero at the 5 percent level for any of the observed covariates. At the 10 percent level, the interaction term for the "floor: cement"

variable is positive and significantly different from zero suggesting that children with a high poor fraction in PROGRESA villages are more likely to live in dwellings with a cement floor. The evidence in Table 3, however, shows that living in a dwelling with a cement floor does not predict the change in school attendance. Thus, there is no evidence supporting a direct effect of the poor fraction in peer groups due to imbalance with respect to observed characteristics.²² We therefore use the interaction term "poor fraction * village WITH program" to instrument the change in school attendance in the peer group – our IV2 strategy.

4.2 Identifying the Direct Effect of PROGRESA

The second step of the empirical analysis identifies the direct effect of the PROGRESA transfer. This direct effect needs to be identified conditional on the change in the average peer group schooling. A straightforward approach to identifying the direct effect is thus a difference-in-difference strategy using fixed group effects to control for changes in the social environment. Thus, we identify the parameter δ estimating the following equation

$$S_i = \alpha_0 + \alpha_1 T_i + \alpha_2 P_i + \delta T_i * P_i + X_i' \beta + \theta_g + \epsilon_i \quad (3)$$

where θ_g is a fixed peer group effect.²³ This model identifies the direct effect of financial transfers on individual school attendance decisions conditional on a vector of observed characteristics X_i and conditional on all differences at the peer group level. We also compare this result to the effect of PROGRESA that we identify when we do not hold changes at the peer group level constant. This allows to assess to what extent information on the peer group is important in measuring the direct effect of the transfer.

Information on the direct effect of PROGRESA and on the importance of social interactions can be combined to understand the treatment contrast among children from poor households (Table 3, Column A). Provided that there are no direct effects in treated villages ($\alpha_1 = 0$) and assuming symmetric social interactions (γ not individual specific), this contrast captures

$$E[S_i | T_i = 1, P_i = 1] - E[S_i | T_i = 0, P_i = 1] = \quad (4)$$

²²Note that this analysis neither rules out imbalance with respect to other characteristics nor direct effects due to implementing the program. However, with respect to direct effects we stress that the design of the program does not give rise to concern. There was no direct sharing of benefits, no income spillover effects, and households were informed regarding their eligibility status. Moreover, it is possible to assess the bias due to imbalance along dimensions that we do not condition on by comparing the IV1 estimate with the IV2 estimate.

²³Note that the parameter α_1 is not identified since the treatment status of the individual T_i is collinear with the peer group of the individual i .

$$= \delta + \gamma * (E[S_{(-i)g}|T_i = 1, P_i = 1] - E[S_{(-i)g}|T_i = 0, P_i = 1])$$

This contrast therefore captures both, the direct effect of PROGRESA that is due to transfers as well as the social effect of PROGRESA that arises because the program changes the average change in school attendance of other children in the same grade and gender cell. Thus, comparing the direct effect of PROGRESA with the contrast (4) is informative on the relative importance of the individual and social determinants of school attendance decisions. Moreover, equation (4) also highlights why information on the relevance of social interactions – the parameter γ – is important for policy design. Granting access to the transfer for only one household in each village will generate the effect δ among eligible children. In contrast, granting access to the program to an increasing number of households within the village generates an additional social effect via the change in the social environment $S_{(-i)g}$ combined with the relevance of social interactions.

5 Results

5.1 Main Result on Social Interactions

Table 4 reports the main results of the empirical analysis. All estimates control for the full set of control variables shown in Table 3. Inference is based on robust standard error that allow for clustering at the village level.

The first Column in Table 4 provides the reduced form estimate for the IV1 strategy – the treatment contrast of the trend in school attendance among the non-poor children with a non-empty reference group. The analysis indicates that school attendance drops by 2.1 percentage points less strongly among non-poor children. This effect is identical to the effect reported in the universe of all non-poor children (Table 3, Panel B).²⁴

Table 4 about here

The second column in Table 4 reports the effect of PROGRESA on peer group average change in school attendance, i.e. $E[S_{(-i)g}|T_i = 1, P_i = 0] - E[S_{(-i)g}|T_i = 0, P_i = 0]$. The cash transfers increases the trend in school attendance among peers by 3.6 percentage points. This effect is statistically significant and quantitatively important. Taken together, the evidence in Table 4 suggests that a change by 3.6 percentage points in peer’s school attendance trend generates a 2.1 percentage points increase in ineligible students’ school attendance trend. Thus,

²⁴This suggests that limiting the sample to those children with a non-empty reference group does not significantly affect results.

there is an increase of .6 percentage points at the individual level for every increase in peer group school attendance by 1 percentage point.

The IV1 strategy in Column 3 of Table 4 combines the reduced form estimate and the effect of PROGRESA on the average change in school attendance to estimate the social interactions parameter γ . Results indicate the marginal effect of peer group schooling on individual schooling of the ineligible student, γ , is .595. The parameter estimate is significantly different from zero at the 5 % level. The parameter estimate thus indicates that social interactions among students are important.

Yet the IV1 strategy does not seriously investigate the extent to which the social interactions parameter is identified from the salience of PROGRESA at the peer group level. The identification merely compares students in treated villages to other students in villages who do not have access to the program while disregarding the eligibility structure of the peer group. A more refined identification strategy distinguishes peer groups with low poor fraction from peer groups with high poor fraction. To the extent that the effect is driven by a process at the peer group level, we should see a weak spillover among ineligible students whose peer group contains a low eligible fraction and a strong spillover effect among ineligible students whose peer group contains a high poor fraction.

Figure 1 investigates the spillover effect among non-poor children conditional on the poor fraction in the peer group. This is the reduced form estimate for our IV2 strategy. Figure 1 distinguishes four levels of impact of the program on peer group schooling. School attendance in peer groups with an eligible fraction between 0 percent and 25 percent is not expected to change much because most children in the peer group are ineligible. The average response of ineligible children in peer groups with this very low eligible fraction is -0.8 percentage points (not significantly different from zero). The second level of social impact arises in peer groups with an eligible fraction between 26 percent and 50 percent. In these groups, the school subsidy has a larger impact on average schooling in the peer group compared to the lowest level of social influence. Interestingly, the average response of ineligible children in these groups is now positive – school attendance increases by 1.4 percentage points due to the program (not significantly different from zero). The third level of social impact arises in peer groups with an eligible fraction of between 51 to 75 percent. The ineligible children’s average response to the program is, again, positive and amounts to 1.3 percentage points (not significantly different from zero). The highest level of social influence is exerted in peer groups with eligible fraction exceeding 75 percent. Clearly, the school subsidy has a tremendous impact in these peer groups. It turns out that the response of the ineligible children in these peer groups is

also strongest. School attendance increases by almost 4.7 percentage points due to the school subsidy.²⁵ This effect is economically and statistically significant. Thus, descriptive evidence in Figure 1 is consistent with an interpretation that the ineligible students' decision to attend school are affected by the corresponding decisions of other students in the same grade level and with the same gender.

Figure 1 about here

The evidence in Figure 1 motivates using the interaction term between the poor fraction in the peer group and the treatment status of the village as an instrument for peer group average schooling. The fourth Column in Table 4 reports the result of the IV2 analysis that uses the treatment status of the village interacted with the poor fraction as an instrument.²⁶ Results indicate that the average trend in school attendance in the peer group significantly affects the trend in individual school attendance. These results suggest that a 10 percentage point change in the trend in school attendance in the peer group leads to a 5.86 percentage point change of schooling trends at the individual level.

5.2 Sensitivity Analysis on Social Interactions

Table 5 discusses six important concerns with the identification strategy. Column A in Table 5 reports the baseline result from Table 4 for convenience (Table 4 Column 4). Column B in Table 5 addresses the concern that PROGRESA grants increase income of poor families considerably. These additional resources could spill over to non-poor families in at least two ways. First, eligible families might share the resources directly with other families they know within the village. Adato (2000) does not find evidence for such direct sharing of resources. Second, these monetary transfers may be spent on purchasing goods that are produced or sold mainly within the village. This will increase also household income of the non-poor. Bobonis (2004) finds that these transfers are used mainly to purchase children's clothes. The official PROGRESA database contains detailed information on the number and types of stores. Whereas 183 villages have a local grocery store, 96 villages have a local shop provided by the federal alimentation program (DICONSA), and 88 villages are visited by travelling salespeople, only 20 villages have some type of local market where clothes are sold. This suggests that a dominant share of the benefit was not spent locally. Nevertheless, the sensitivity analysis addresses this concern by directly measuring the transfers to all children who are living in a poor household, have

²⁵Note that a stronger response of ineligible children could also indicate that the treatment effect is stronger in peer groups with a high eligible fraction. The analysis in section 5 accounts for this fact.

²⁶The IV2 regression adds the poor fraction in the peer group to the list of control variables.

reached grade 3-6 of primary school or grades 1-3 of secondary school in October 1998, and who attend school in October 1998. This PROGRESA transfer index is average over all children in the village, excluding child i . The index can be computed for both, treated villages and control villages. The index reflects the grade and gender composition among poor children. In treated villages, the index not only reflects the composition of the village but also the amount of money flowing per child that resides in the village. Thus, the sensitivity analysis adds the PROGRESA transfer index and the interaction term between the treatment status of the village and the PROGRESA transfer index to the baseline analysis. Clearly, if additional resources are shared within the village, this index reflects the extent to which PROGRESA transfers flow to non-poor families. Yet, note that due to the strong correlation between the eligible fraction in the peer group and the PROGRESA transfer index, the sensitivity analysis can not identify the social interactions parameter precisely (the correlation is .86). Results indicate that the concern with the additional resources flowing to villages is not warranted. Adding the index that captures the per child increase in transfers does not affect – at the qualitative level – the estimated importance of social interactions. Whereas the baseline result indicates that the social interactions parameter is .586, the sensitivity analysis identifies a social interactions parameter of .969.²⁷

Table 5 about here

Column C in Table 5 reports the results of two analyses addressing the possible anticipation effects among ineligible families in treated villages. This concern holds that non-poor households might expect to be added to the list of beneficiaries at some point in the future. This may generate an incentive to keep the child in school even if there are no transfers flowing to the household. One way to address this issue is to identify the social interactions parameter using children whose household is not on the margin of being classified as poor. Poverty status of a household was determined using a poverty index reflecting all information on the household collected in October 1997 that predicts household income. A household became eligible for PROGRESA when the poverty index was below 750. Column C focuses on the 3805 children from households whose poverty index exceeds 770. Findings indicate that the marginal households are not driving the result that social interactions are relevant in schooling

²⁷We have also explored an alternative approach to addressing this important concern in adding village fixed effects to the baseline strategy. The social interactions parameter is then only identified from the difference in the salience of PROGRESA at the peer group level. This fixed-effects approach identifies a social interactions parameter of .807 albeit with a very large standard error of .843. This means that there is insufficient variation in the salience of PROGRESA within villages to precisely identify the social interactions parameter. Nevertheless, the point estimate is consistent with an interpretation of strong social interactions in schooling decisions.

decisions. Removing about 26 percent of the original sample, results indicate a robust and strong importance of social interactions in schooling. The social interactions parameter in Column C is .579 which is very much in line with the baseline result of .586.

Column D addresses the issue that the program may have generated first year impacts which differ from second year impacts. In particular, ineligible households may not have been properly informed on the actual characteristics of the program. Since the October 1998 survey was collected at about the same time as the first transfers were handed out, ineligible households might have expected to be treated in the same manner as eligible households. We address this concern with information on the second year of the program. The analysis uses information on 1779 children from households that were not classified as poor in November 1999. PROGRESA admitted additional households to the set of beneficiaries in July 1999. This explains why the number of observations is lower in this analysis compared to the baseline analysis. In line with the set-up of the baseline analysis, we focus on children enrolled in grades 2-5 in October 1997 and investigate the change in school attendance between October 1997 and November 1999 – over a two year period. The social interactions parameter is estimated to be slightly weaker. Whereas the baseline result indicates that individual schooling increases by 5.8 percentage points for a 10 percentage point increase in group schooling, two year results in Column D suggest that the increase is on the order of 4.7 percentage points. The parameter is not as precisely identified in the two year analysis as in the one year analysis because the number of children from ineligible households is much lower. Nevertheless, the concern with the potentially different operation of the program in the second year does not appear to be warranted.

Column E addresses the issue of differences in local labor market trends across treated and control communities.²⁸ There could be changes in the local labor market that affect schooling decisions differently in treated locations compared to control locations. This concern can be addressed by controlling for state * local region (*municipalidad*) interactions. This procedure identifies 191 local regions (compared to only 7 states used in all analyses so far). This means that the effect of group schooling on individual schooling is only identified from the variance in the salience of the program within very narrowly defined regions. Results indicate that social forces are important in schooling decision. The parameter estimate on the coefficient that captures the importance of social interactions is larger than the corresponding baseline estimate. Whereas the baseline parameter estimate of the social interactions parameter is .584, this sensitivity analysis finds a parameter of .818. This means that baseline estimates tend to

²⁸For instance, Bobonis (2004) finds evidence of rainfall shortages in the period between 1997 and 1999.

give a lower bound on the actual relevance of social forces in schooling.

Column F addresses the issue of randomization across villages rather than individuals by weighting each observation with the inverse of the number of children in the village observed in the sample. This means that weight given to children from large villages and from small villages is the same. Results indicate that the concern with differences in population size across villages is not warranted. The social interactions parameter is .621 which is very close to the baseline analysis.

Column G focuses on children who have progressed through primary school without repeating a grade – who are on grade level. There are two reasons for this analysis. We first find that the higher rate of school attendance in October 1997 – before the program started – in villages with program compared to villages without the program arises among children who are not on grade level. In the grade level sample, the difference in school attendance between treated and control villages is -.7 percentage points. This compares favorably with the overall difference of 2.4 percentage points in the entire sample that we report in Table 2, Column B (first row). Furthermore, focusing on the grade level sample reduces the heterogeneity of children in terms of their school career. Findings in Column G indicate that the social interactions parameter is .538 which is very much in line with the baseline result of .586.

So far the empirical analysis has assumed that the social interactions parameter γ is identical across individuals. This assumption motivates a standard constant coefficient regression model for identification. Table 6 explores the extent to which this assumption is true along the two important dimensions gender and grade level. Panel A in Table 6 reports the social interactions coefficient by completed grade level. Results indicate that social interactions are significantly different from zero and almost equally important for children in primary school (grade level 3-4) and children transiting from primary school to secondary school (grade level 5-6). This finding is important in the sense that social interactions not only affect the drop out decision (captured in grades 5 and 6) but also the regular attendance decision (captured in grades 3 and 4).

Table 6 about here

Gender is a second important dimension for two reasons. First, the labor market opportunities are expected to differ strongly between boys and girls. Second, the secondary school PROGRESA transfer is higher for girls than for boys. Panel B in Table 6 therefore provides separate results for girls and for boys. Results for girls suggest that the social interactions parameter point estimate is slightly lower than in the baseline estimate failing the threshold for statistical significance at the 10 % level. Results for boys are significantly different from

zero and slightly larger than the baseline estimate. Thus, results by gender do not suggest any important differences in the extent to which social forces affect human capital decisions.

In sum, the results in Table 5 and Table 6 are consistent with a strong, robust, and universal importance of social forces in individual schooling decisions.

5.3 The Direct Effect of PROGRESA

Table 7 reports our estimates of the direct effect of PROGRESA on the school attendance decisions of children from poor households. This is the change in school attendance we observe among eligible children compared to ineligible children holding constant the social environment of these children. This contrast is not valid because poverty status of the household may be related in important and direct ways with trends in school attendance. This critique can be addressed using information on villages without PROGRESA. Here, we can again compare poor and non-poor children to assess the validity of the claim that school attendance trends differ by poverty status. This means that a difference-in-difference analysis identifies the effect of PROGRESA on the poor children's school attendance.

Table 7 reports two analyses. The first column provides a difference-in-difference analysis of the effect of PROGRESA transfers. This difference-in-difference analysis compares, in essence, the average change in school attendance among poor children in villages with the program to the average change in school attendance observed among non-poor children in villages with program.

Table 7 about here

The results of this first difference-in-difference analysis indicate that the financial transfers due to PROGRESA lead to a 3.7 percentage points increase in the school attendance trend between October 1997 and October 1998. Moreover, there is, again, strong evidence of a spillover effect among non-poor children on the order of 2.1 percentage points (parameter estimate "Village WITH Program"). Note that this estimate is of exactly the same magnitude as in Table 4, Column A. There is, however, no evidence of a significant difference in school attendance trends between poor and non-poor children (parameter estimate "Poor").

Our first approach to measuring the direct effect of transfers on school attendance decisions suffers, however, from an important problem. This first strategy does not account for the fact that implementing PROGRESA not only changes individual financial incentives of attending school but also the social determinant of schooling – school attendance in the peer group. Since the poor fraction in the peer group of the average poor child is 72 percent, and the poor fraction

in the peer group of the average non-poor child is merely 54 percent, the first strategy generates an upward biased estimate of the direct effect of PROGRESA transfers. We address this issue by adding 1910 peer group fixed effects to the difference-in-difference analysis in Column 1 of Table 7. This means that the direct effect of PROGRESA transfers is identified within peer groups rather than across peer groups.²⁹ Results indicate that PROGRESA increases school attendance by a 2.9 percentage points. This estimate is statistically significant and quantitatively important. However, the refined estimate in Column 2 is much weaker than the 3.7 percentage points reported in Column 1 of Table 6.

In concluding this section, it is tempting to assess to what extent the estimate of the direct effect of PROGRESA on the order of 2.9 percentage points and the social interactions parameter estimate on the order of .586 can be combined with the effect of PROGRESA on the peer group level among poor children of 5.3 percentage points (estimates are not shown) can be combined to rationalize the total effect of PROGRESA we report in Table 3. Our discussion in Section 4 shows that these three components add up to the total effect. We find that the sum of the direct effect of PROGRESA of 2.9 percentage points and the social effect of PROGRESA of 3.1 percentage points ($= .586 * 5.3$ points) is rather in line with the total effect of PROGRESA on the order of 5.8 percentage points. This shows that the direct effect of the monetary transfer due to PROGRESA is roughly of equal size as the indirect effect of PROGRESA via the change in peer group decisions. Note that this conclusion is driven entirely by the *salience* of the program at the peer group level. We have argued that the social effect among non-poor children is 2.1 percentage points. This is rather weaker than the 3.1 percentage points that can be attributed to the social effect among poor children. Thus, the main determinant of the size of the social effect is the salience of the program at the peer group level. This highlights the main contribution of our results to the literature concerned with targeted interventions.³⁰ If social interactions are relevant, the salience of the program at the group level is an important determinant of the average effect of the program among eligible and ineligible individuals. Thus, a program that reaches the same number of individuals will generate a different effect when spread out over individuals who are not connected to each other compared with a strategy of strong concentration of the program within tightly defined groups rather than individuals.³¹

²⁹Note that the coefficient "Village WITH program" is no longer identified. The coefficient "Village WITH program * Poor" is identified since there are children from poor households and non-poor households in the same peer group.

³⁰See Behrman *et al.* (1999) on a discussion of targeting issues in the PROGRESA context.

³¹Note that we have only focused on generating a strong impact in this discussion. In designing actual programs, a wealth of other conflicting issues are relevant.

6 Conclusions

This paper argues that individual schooling decisions and peer group schooling decisions may be related in important ways for at least two reasons. First, students may conform to the choices in their peer group because they expect to be popular with them. Second, students and their parents may learn from the choices of other, similar students. The relevance of the resulting social interactions can be studied in the context of an experiment that grants a cash subsidy to a subgroup of students in villages across rural Mexico. This subsidy encourages the eligible students to remain in school rather than to start working. Moreover, if social interactions are relevant, the ineligible students may also decide to acquire more schooling provided that a salient fraction of their peer are eligible for the subsidy.

Results indicate first that there is a positive average effect of the program on ineligible individuals. We also find that the ineligible students' response to the school subsidy is larger the larger is the eligible fraction of students in the peer group – the students in the same grade level with the same gender. Second, when we combine the response of the ineligible student with information on the effect of PROGRESA on peer group schooling, we find that the ineligible students' schooling decisions are strongly, and statistically significantly, affected by their peer's decision. Third, we also identify the effect of the cash subsidy program on the eligible students. Comparing this direct effect of the program with the indirect effect of the program, we find that both are equally relevant among eligible children. This finding is important for education policy. Strong social interactions in schooling decisions imply that the salience of the program matters for the average effect of the program on the eligible students.

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Figure 1:
The Spillover Effect of PROGRESA on *Non-Poor* Children

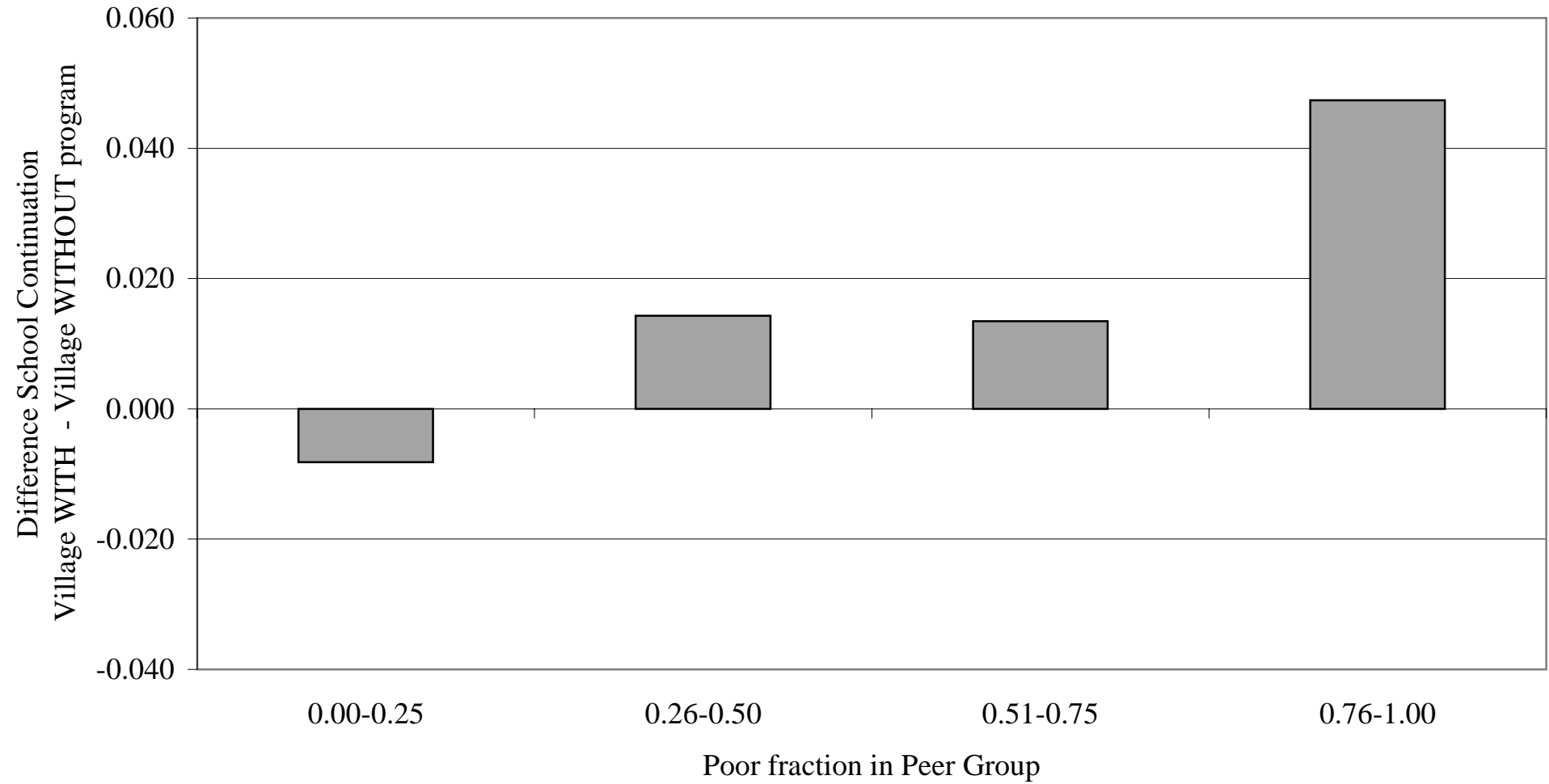


Table 1:
Descriptive Statistics

Village with program	A. <i>Poor</i>			B. <i>Not Poor</i>		
	No	Yes	Difference (abs. z-Value)	No	Yes	Difference (abs. z-Value)
Completed grade 4	0.246	0.235	-0.011 (1.25)	0.227	0.218	-0.009 (0.78)
Completed grade 5	0.224	0.212	-0.012 (1.45)	0.227	0.239	0.013 (0.99)
Completed grade 6	0.272	0.282	0.010 (0.87)	0.337	0.337	0.000 (0.01)
Girl	0.502	0.492	-0.010 (0.87)	0.497	0.507	0.010 (0.65)
Mother Ed.: Primary School or Higher	0.163	0.160	-0.003 (0.19)	0.217	0.218	0.002 (0.07)
Father Ed.: Primary School or Higher	0.165	0.169	0.004 (0.22)	0.192	0.202	0.011 (0.50)
Father Information Missing	0.099	0.095	-0.004 (0.41)	0.117	0.119	0.001 (0.13)
Floor: Cement	0.262	0.288	0.026 (0.92)	0.626	0.598	-0.027 (0.86)
Roof: Tin	0.263	0.286	0.023 (0.78)	0.315	0.304	-0.011 (0.30)
Roof: Asbest	0.129	0.131	0.002 (0.11)	0.167	0.163	-0.004 (0.12)
Roof: Tiles	0.093	0.127	0.034 (1.32)	0.084	0.092	0.008 (0.39)
Roof: Cement Blocks	0.106	0.086	-0.020 (1.15)	0.241	0.251	0.010 (0.31)
Secondary School in Village	0.260	0.256	-0.004 (0.07)	0.340	0.265	-0.075 (1.15)
N(obs.)	3880	6604		2083	3086	

Notes: Poor means Household is eligible for Cash Subsidy. All characteristics measured in October 1997.

Sample considers children in grades 3-6 in October 1997, aged 6-16 in October 1998, living with mother.

Source: Own Calculation, Based on Progresa Evaluation Data.

Table 2:
Descriptive Evidence on the Effect of Cash Subsidies on School Attendance

Village with program	<i>A. Poor</i>			<i>B. Not Poor</i>		
	No	Yes	Difference (abs. z-Value)	No	Yes	Difference (abs. z-Value)
School Attendance (SA), October 1997	0.769	0.782	0.013 (0.897)	0.758	0.782	0.024 (1.305)
School Attendance (SA), October 1998	0.685	0.757	0.072 (3.798)	0.691	0.733	0.042 (1.907)
Change in School Attendance	-0.084	-0.025	0.059 (5.511)	-0.067	-0.049	0.018 (1.484)
N(obs.)	3880	6604		2083	3086	

Notes: Eligible means Household is eligible for Cash Subsidy. October 1997 is before program, October 1998 is during program.

Source: Own Calculation, Based on Progresa Evaluation Data.

Table 3:
The Effect of PROGRESA Transfers on Poor and Non-Poor Children
Dependent Variable: Change in School Attendance

	<i>A. Poor</i>	<i>B. Non-Poor</i>
Village WTH Program	0.058 (0.010)***	0.021 (0.012)*
Completed grade 4	-0.018 (0.007)**	-0.017 (0.012)
Completed grade 5	-0.185 (0.014)***	-0.176 (0.017)***
Completed grade 6	0.045 (0.012)***	0.019 (0.014)
Girl	-0.005 (0.008)	-0.008 (0.010)
Mother Ed.: Primary School or Higher	0.003 (0.010)	0.013 (0.011)
Father Ed.: Primary School or Higher	0.015 (0.010)	0.033 (0.013)**
Father Information Missing	-0.043 (0.014)***	-0.022 (0.015)
Floor: Cement	-0.000 (0.010)	0.004 (0.013)
Roof: Tin	0.005 (0.011)	0.018 (0.017)
Roof: Asbest	0.015 (0.014)	0.032 (0.020)
Roof: Tiles	0.005 (0.017)	0.036 (0.024)
Roof: Cement Blocks	0.003 (0.015)	0.026 (0.018)
Secondary School in Village	0.036 (0.009)***	0.030 (0.014)**
Constant	-0.083 (0.019)***	-0.072 (0.046)

State Effects (7)

Observations

Yes
10484

Yes
5169

Notes: Robust standard errors in parentheses (allow for clustering at village level). * significant at 10%; ** significant at 5%; *** significant at 1%.

Source: Own Calculation, Based on Progresa Evaluation Data.

Table 4:
The Effect of Peer Group Schooling on Individual Schooling
Sample: *Non-Poor* Children

Dependent Variable	Change in School Attendance	PG Avg. Change in School Attendance	Change in School Attendance	Change in School Attendance
	OLS	OLS	IV1	IV2
Change in School Attendance in Peer Group (PG)	-	-	0.595 (0.254)**	0.586 (0.232)**
Village WITH Program	0.021 (0.012)*	0.036 (0.010)***	-	-
Control Variables	Yes	Yes	Yes	Yes
State Effects (7)	Yes	Yes	Yes	Yes
First Stage F-Statistic	-	-	3.72***	3.74***
Observations	5141	5141	5141	5141

Notes: Robust standard errors in parentheses (allow for clustering at village level). * significant at 10%; ** significant at 5%; *** significant at 1%.
IV1 uses "Village With Program as Instrument", IV2 uses "Village With Program and Village With Program * Poor Fraction in Peer Group" as instruments. Control Variables are those listed in Table 3.

Source: Own Calculation, Based on Progresa Evaluation Data.

Table 5:
Sensitivity Analysis
Dependent Variable: Change in School Attendance

	<i>A. Baseline</i>	<i>B. Income Effect</i>	<i>C. Anticipation</i>	<i>D. Second Year</i>	<i>E. Local Effects</i>	<i>F. Weighted</i>	<i>G. Grade Level</i>
Change in School Attendance in Peer Group	0.586 (0.232)**	0.969 (0.474)**	0.579 (0.276)**	0.471 (0.278)*	0.818 (0.355)**	0.621 (0.337)*	0.527 (0.274)*
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Effects (7)	Yes	Yes	Yes	Yes	No	Yes	Yes
Region Effects (191)	No	No	No	No	Yes	No	No
Observations	5141	5141	3805	1779	5141	5141	2245

Notes: Robust standard errors in parentheses (allow for clustering at village level). * significant at 10%; ** significant at 5%; *** significant at 1%
Peer Group contains children in same grade level (3-4 or 5-6) with same gender
See Table 3 for a list of all control variables. Estimates based on IV2.

Source: Own Calculation, Based on Progresa Evaluation Data.

Table 6:
The Effect of Peer Group SA on Schooling in Subgroups
Dependent Variable: Change in School Attendance

<i>A. Grade Level</i>	<i>Grades 3-4</i>	<i>Grades 5-6</i>
Change in School Attendance in Peer Group	0.592 (0.303)*	0.520 (0.301)*
Control Variables	Yes	Yes
State Effects (7)	Yes	Yes
Observations	2198	2943
<i>B. Gender</i>	<i>Girls</i>	<i>Boys</i>
Change in School Attendance in Peer Group	0.573 (0.355)	0.624 (0.268)**
Control Variables	Yes	Yes
State Effects (7)	Yes	Yes
Observations	2592	2549

Notes: Robust standard errors in parentheses (allow for clustering at village level).
* significant at 10%; ** significant at 5%; *** significant at 1%
Peer Group contains children in same grade level (3-4 or 5-6) with same gender
See Table 3 for a list of all control variables. Estimates based on IV2.

Source: Own Calculation, Based on Progresa Evaluation Data.

Table 7:
The Direct Effect of School Subsidies on School Continuation
Dependent Variable: Change in School Attendance
Sample: *Poor* and *Non-Poor* Children

Village WITH Program * Poor	0.037 (0.014)***	0.029 (0.014)**
Village WITH Program	0.021 (0.012)*	-
Poor	-0.011 (0.011)	-0.005 (0.011)
Control Variables	Yes	Yes
State Effects (7)	Yes	No
Group Fixed Effects (1910)	No	Yes
Observations	15578	15578

Notes: Robust standard errors in parentheses (allow for clustering at village level). * significant at 10%; ** significant at 5%; *** significant at 1%.

See Table 3 for a list of all control variables.

Source: Own Calculation, Based on Progresa Evaluation Data.