

# SOCIAL INTERACTIONS AND SCHOOLING DECISIONS

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## Abstract

The aim of this paper is to study whether schooling choices are affected by social interactions. Such social interactions may be important because children enjoy spending time with other children or parents learn from other parents about the ability of their children. Identification is based on a randomized intervention that grants a cash subsidy encouraging school attendance among a sub-group of *eligible* children within small rural villages in Mexico. Results indicate that (i) the *eligible* children tend to attend school more frequently, (ii) but also the *ineligible* children acquire more schooling when the subsidy is introduced in their local village, (iii) social interactions are economically important, and (iv) they may arise due to changes in parents' perception of their children's ability.

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# 1 Introduction

”Curiously enough, when one of the students would get sick, one or two of his closest friends would also not show up to school.” – School principal in a rural school in Mexico (Adato *et al.*, 2000, p. 54)

All social sciences agree that education is very important in every person’s life. Understanding when and why children leave school for work is thus crucial. Economists have been quite successful in developing a framework that explains schooling decisions (Becker, 1964). Yet, while economists have typically focused on the *individual* costs and benefits of acquiring further schooling (Card, 1999), sociologists have long pointed out that schooling decisions could be affected by the social environment of a child (Coleman, 1961). Specifically, children meet their friends at school and the school can be important in shaping a child’s identity.<sup>1</sup> Likewise, school meetings are the most important setting where parents interact with other parents to share information regarding their child’s ability, and the potential benefits costs of schooling.<sup>2</sup>

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. A comprehensive understanding of the factors shaping education investment decisions is particularly needed in this setting. Children are often absent from school and many leave school when they complete primary school at the age of 12 years – notably two years before they reach the minimum age required for employment. To address these schooling problems, the Mexican ministry of education devised in the late 1990s an innovative program – the PROGRESA program – designed to encourage school attendance among poor families living in small villages in rural Mexico. The program consists of a cash grant paid to the mother 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, i.e. children living in households that are classified as poor. 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.<sup>3</sup>

The basic idea of this paper is to assess the response of the ineligible children – living in a

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

<sup>2</sup>See Manski (2004) for a recent theoretical discussion of social learning.

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

household that has not been classified as poor – to introducing the program in their peer group. Clearly, these children’s families do not receive additional income when they send their child to school. However, these children might want to spend more time in the classroom because their peers – children in the same grade living in the same village – attend school more frequently. Also, parents might perceive the expected gain to acquiring schooling to have increased when they see that other parents send their children to school. Thus, the spillover effect of the program on the ineligible children can be used to measure the role of social interactions in schooling decisions (Moffitt, 2001).

Identifying social interactions is difficult because individuals tend to select into similar groups – an “omitted variable” problem – and because if the group is affecting the individual the reverse is also true – the “reflection problem” (Manski, 1993).<sup>4</sup> PROGRESA is ideally suited to address these two important identification problems. First, the program was randomly implemented. Randomization balances all determinants of school attendance directly addressing the “omitted variable” problem. The “reflection problem” can be solved because PROGRESA grants are only paid to a sub-group of *eligible* children within small villages. Thus, PROGRESA increases peer group school attendance while leaving unaffected the *ineligible* child’s monetary incentive to attend school.<sup>5</sup> This means that the response among ineligible children provides information on how strongly the peer group affects the individual and not vice versa.

Our empirical results confirm previous findings of a sizeable effect of schooling subsidies on school attendance among *eligible* children. With regard to social interactions, we find, 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, the social interaction effect is sizeable. 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 1 percentage point increase in school attendance for every 2 percentage point increase in schooling among peers. Third, 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.

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<sup>4</sup>Manski (1993) called this the “reflection problem” because solving it is analogous to determining whether the mirror image is causing me to act like I do or vice versa. Also see the more recent discussions in Manski (1995) and Manski (2000) on the identification problems with endogenous social interactions.

<sup>5</sup>The 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.

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. Decomposing the total effect of a targeted intervention is crucial when thinking about targeting of programs. If the indirect effect is small, targeting does not matter for the size of the total effect. If the indirect effect is large, targeting matters. A program that is spread out over a few people living in many villages generates smaller average effects on the eligible population than a program that targets all individuals living in a few villages.

From a policy point of view it is crucial to understand the reasons for these endogenous social interactions (Manski, 2000). If these interactions arise because parents learn from other parents about the value of schooling for their child, policy could also consider alternative interventions to increase schooling in rural Mexico. On the other hand, if children simply like to spend time with their classmates, such interventions will not work. Interestingly, PROGRESA allows us to go one step towards understanding endogenous social interactions in rural Mexico. After the program was introduced, parents were asked about the highest level of schooling they believe that their child is able to reach. Analyzing this subjective measure of a child's ability we find that PROGRESA increases parents' subjective evaluation of the ability of their children. This finding supports the idea that at least to some extent endogenous social interactions arise because parents' perceptions of child ability are shaped by the social environment.

Our findings are important for at least four reasons. First, these findings suggest that the social environment is relevant in affecting one of the most important investment decisions. This implies that economists should pay attention to social interactions in analyzing schooling choices both theoretically and empirically. Second, endogenous social interactions in schooling decisions entail amplification of the effects of schooling interventions because endogenous social interactions give rise to a social multiplier (Glaeser *et al.*, 2003). 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. Fourth, our findings that social interactions arise due to learning interactions suggest that it is possible to consider alternative interventions that increase schooling in rural Mexico.

There is a rapidly expanding literature on social interactions in schooling.<sup>6</sup> There is a first important strand of the literature that addresses the "omitted variable problem" using panel

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

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, while these studies address the "omitted variable bias", using panel data does not address the "reflection problem". The earliest study that address the reflection problem is Case and Katz (1991) who use instrumental variables to study neighborhood effects in the Boston area. Duflo and Saez (2003) study the role of information and social interactions in retirement plan decisions in a field experiment. Angrist and Lang (2004) measure peer effects in academic achievement using quasi-experimental features in the placement of disadvantaged children in a Boston desegregation program. 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.<sup>7</sup>

The paper that is closest to ours is Bobonis and Finan (2005) who study social interactions in schooling decisions using PROGRESA limiting the analysis to children having completed primary education. This paper complements the analysis in Bobonis and Finan (2005) in at least three important ways. First, this paper focuses on both children who are still in primary school and on children facing the transition from primary to secondary school. This is important because the aim of the PROGRESA program is not only to increase secondary school enrollment but also school attendance in primary school. Thus, the analysis of this paper provides a comprehensive understanding of the relevance of social interactions. Second, this paper discusses how to use both the program status of a village (with program or without program) as well as the (pre-determined) composition of the peer group as instruments of average peer group schooling. Using both pieces of information instead of just the program status of the village is critical because endogenous social interactions can be identified more convincingly using only within village information on peer groups and individuals. Third, to our knowledge, this is the first paper that uses subjective information on parents' perception of children's ability to understand the reasons for endogenous social interactions in schooling decisions.<sup>8</sup>

The paper is organized as follows. Section 2 presents background information 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

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<sup>7</sup>A related issue arises in studying the impacts of de-worming treatment in developing countries. Miguel and Kremer (2004) find strong epidemiological spillovers generated by de-worming treatment in rural Kenya.

<sup>8</sup>See Munshi and Myaux (2006) for a paper that explains why there are social interactions in fertility.

concludes.

## 2 Background

### 2.1 Primary and Secondary Education in Mexico

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. 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. Moreover, 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).

### 2.2 PROGRESA

In order to encourage enrollment 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)<sup>9</sup> 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

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<sup>9</sup>The Program was re-named Oportunidades under the Fox administration.

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 (Table A1).<sup>10</sup> 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 that the child has been present on more than 85% of all school days in successive bi-monthly periods during 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 very 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.<sup>11</sup> 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.<sup>12</sup> These 506 villages are located in seven states (*Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí and Veracruz*) which are loosely clustered around Mexico City.

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 by condensing this information into a so-called "poverty index". A household

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<sup>10</sup>Beneficiary families also receive funding to pay for school material of 135 pesos for children in primary school and 170 pesos for children in secondary school.

<sup>11</sup>See Skoufias (2001) for an in-depth discussion of the implementation of the program.

<sup>12</sup>The 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.

was classified as poor if its poverty index exceeded a state specific poverty threshold. On average, this procedure led to 52 % of all households being classified as poor but there is also substantial variation with respect to the percentage of households that are eligible within a village.

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 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.<sup>13</sup>

In the second year of the program, (August 1999 to June 2000), 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. However, by the year 2000, PROGRESA staff found that none of the newly admitted households had collected any cash benefits. Apparently, none of the newly admitted households had been notified of their eligibility for the program (Buddelmeyer and Skoufias, 2003). Because an in-depth understanding of how the program was implemented is crucial but missing, we limit all analyses to the first year of the program.

### 2.3 Social Interactions

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? There are two possible explanations. First, 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.

Second, there can also be social spillovers on non-beneficiary parents due to the program. Suppose PROGRESA changes the way parents from beneficiary household evaluate the career prospects of their children. This could happen for a number of reasons. On one hand, beneficiary parents frequently interact with highly educated program staff and doctors potentially informing them of the benefits of acquiring further education. On the other hand, since beneficiary children are attending school more regularly teachers might be more optimistic about the educational career prospects of a beneficiary child. How can beneficiary optimism spill

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

over to non-beneficiary parents? Parents tend to have a more precise notion of the ability of their child *relative* to other children's ability rather than the *absolute* ability of their child. If that is true, non-beneficiary parents will be more optimistic regarding the educational career prospects of their children when other parents' evaluation of their children's ability increases.

Clearly, such social interactions are expected to be strong among children who have reached the same grade level and who are living in the same village. These children are likely to be classmates because most villages are so small that they only have one local school. Moreover, parents will be affected strongly by their child's classmates parents because they tend to meet these parents more frequently than parents of other children who are not in the same grade at school meetings. Thus, it is likely that the relevant peer group consists of all children living in the same village who have reached the same grade level.

Is this program useful in identifying social interactions between poor and non-poor families? 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.<sup>14</sup> 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.<sup>15</sup>

## 2.4 Confounding Effects

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 (2005) 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

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<sup>14</sup>"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)

<sup>15</sup>"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)

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. Nevertheless, it is possible to investigate whether there are indirect effects of PROGRESA on the non-poor households. Angelucci and De Giorgi (2006) find that consumption of the non-poor also increases as a response to introducing the program. This effect only arises in the *second* year of the program. Analyzing this second year is difficult for two reasons. On one hand PROGRESA staff extended eligibility between the first and the second year. On the other hand, Buddelmeyer and Skoufias (2003) report that most of the newly admitted households never claimed PROGRESA benefits. Thus, it is not clear how this extension of PROGRESA was communicated and enacted. The analysis of this paper therefore focuses exclusively on the first year of the program. Moreover, we will use the detailed consumption information compiled by Angelucci and De Giorgi (2006) to identify social interactions *conditional* on household consumption to assess the robustness of our estimates in a sensitivity analysis.<sup>16</sup>

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.

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<sup>16</sup>Theoretically, a spillover effect on ineligible children's school attendance could also be due to a health spillover when the incidence of communicable diseases is reduced among beneficiary children (Miguel and Kremer, 2004). However, (Gertler, 2000) does not find an effect of PROGRESA on health among beneficiary children aged 6-17 years. This suggests that health spillovers can not explain a potential school attendance spillover effect.

## 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.<sup>17</sup> 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 concentrate on children living with their mother who have completed grades 3 to 6 of primary school in October 1997.<sup>18</sup> 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 of primary school) as well as children making the transition from primary school to secondary school or drop out of school (children having completed grades 5 or 6 of primary school). 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 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 exists are quite common, especially among children who have completed primary school. Thus, non-enrollment

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<sup>17</sup>To 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.

<sup>18</sup>The 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.

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.<sup>19</sup> 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.<sup>20</sup> 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. 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 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

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<sup>19</sup>We 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.

<sup>20</sup>Note 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.

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. School attendance reflects both attendance and enrollment. 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.

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 control villages, and about 78 % in treated 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 reduces school drop out 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.<sup>21</sup>

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. In the following section we discuss how to use information on this spillover effect to identify endogenous social interactions.

## 4 Identification

### 4.1 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 1998, and  $T_i = 0$  otherwise. Let  $S_{ig}$  be the average change in school attendance in the relevant peer group, excluding individual  $i$ . The linear model of social interactions (Manski, 1993) can be used to characterize the trends in school attendance as follows

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

There are two salient parameters of interest in equation (1). The parameter  $\gamma$  measures the extent to which individual change in school attendance is affected by peer group average

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<sup>21</sup>Note, however, that our control variables are important predictors of the level of school attendance. Our finding that control variables are weak predictors of trends in school attendance implies that their effects on the level of schooling are time invariant.

change in school attendance. The parameter  $\delta$  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  $S_{ig}$ , provided that social interactions are relevant, i.e.  $\gamma \neq 0$ . The remaining parameters in equation (1) capture omitted differences across regions ( $\alpha_1$ ) and omitted and actual differences across poor and non-poor children ( $\alpha_2$ ).

The evidence in Table 3 can be used to illustrate how we can identify the social interactions parameter  $\gamma$ . Contrasting the change in school attendance among non-poor children in villages with PROGRESA and control villages measures

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

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. 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.

Equation (2) shows that we can identify  $\gamma$  using the PROGRESA status of a village,  $T_i$ , as an instrument for average trend in schooling in the peer group  $S_{ig}$ . As will be shown in Table 4, the PROGRESA status of a village is a strong predictor of the average trend in school attendance in the peer group. Moreover, as we have argued above, the PROGRESA status of a village does not appear to affect individual schooling of the ineligible children directly. We call the identification strategy that uses  $T_i$  as an instrument for  $S_{ig}$  our "IV1 strategy".

Our "IV2 strategy" recognizes that there is tremendous pre-program 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 eligible fraction because PROGRESA applied the state poverty line to a set of villages which differ with respect to their location relative to the poverty line. Clearly, the "eligible fraction" is an important predictor of the effect of PROGRESA on peer group schooling.

Moreover, the pre-program eligible fraction in the peer group is exogenous for two reasons. First, the pre-program composition of peer groups can not be affected by relocation before the treatment status of the village was determined.<sup>22</sup> Second, randomization ensures that villages with PROGRESA and villages without PROGRESA are balanced with respect to pre-program composition of peer groups. Indeed, the average eligible fraction in villages with program is .553 – identical to the average eligible fraction in villages without the program of .550. Moreover, the correlation between pre-program school attendance and the eligible fraction in the peer group is identical both for villages with program and villages without the program.<sup>23</sup>

This means that the eligible fraction in the peer group in villages with the program is a valid instrument for peer group schooling, conditional on the direct effect of the eligible fraction

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<sup>22</sup>Note that using the pre-program (October 1997) eligible fraction rather than the during-program (October 1998) eligible fraction leads to a weaker instrument because the pre-program eligible fraction proxies for during-program eligible fraction. Arguably, the advantage in terms of the pre-program eligible fraction being exogenous greatly outweighs this cost.

<sup>23</sup>We also find that the correlation between all the observed characteristics listed in Table 1 is identical in villages with program and villages without program with the exception of the "floor: cement" variable. Children with a high eligible 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 eligible fraction in peer groups due to imbalance with respect to observed characteristics.

in the peer group on school attendance. We therefore use "village with program" ( $T_i$ ) and the interaction term "eligible fraction \* village WITH program" ( $T_i * P_{ig}$ ) to instrument the change in school attendance in the peer group ( $S_{ig}$ ) and we enter the "eligible fraction" as a separate regressor to all models that apply our "IV2 strategy".

There are two important advantages to applying the "IV2 strategy" rather than the "IV1 strategy". First, the "IV2" estimates are expected to be more precisely estimated than the "IV1" estimates because the precision of instrumental variables estimates depends on the predictive power of the instrument. Improving precision is crucial since instrumental variable estimates are generally characterized by low precision. Second, with the "IV2" estimates we can identify endogenous social interactions based on within village variation in the individual schooling, peer group schooling, and peer group composition. This is impossible with the "IV1" strategy since the program status of a village  $T_i$  does not vary within villages. Nevertheless, it is crucial to understand whether identification is driven by differences across villages rather than within villages since the program might be run very differently between villages. Note, however, that within village variation in peer group composition and average peer group schooling is quite small leading to imprecise estimates. We therefore report baseline estimates that use within state variation and report estimates that use within village variation as a sensitivity analysis.

## 4.2 Identifying the Direct Effect of PROGRESA

The second aim of the empirical analysis is to identify the direct effect of the PROGRESA transfer. This can be achieved by estimating equation (1) instrumenting the endogenous regressor  $S_{ig}$  with the program status of the village and with the program status of the village interacted with the eligible fraction in the peer group (IV2). This identification strategy is potentially problematic if there is a heterogeneous response of the eligible children that is correlated with the eligible fraction in the peer group.

Interestingly, there is a second strategy that measures the direct effect of PROGRESA on eligible children that does not suffer from this potential problem. This strategy estimates equation (1) using within peer group information on poverty status of a child. The basic idea of this strategy is that adding peer group fixed effects controls for the social effect ( $\gamma * S_{ig}$ ). Any differences in school attendance between eligible and ineligible children in villages with program can thus be attributed to the cash transfers. Moreover, this "within peer group" strategy does not require that the eligible fraction is orthogonal to effect heterogeneity. Note, however, that the resulting estimate of the direct effect of the subsidy is downward biased

since the social environment of a child,  $S_{ig}$ , is not constant within peer groups. Specifically, there is a below average social effect on poor children and an above average social effect on non-poor children leading to a negative correlation between the group fixed effect error term and the interaction term  $T_i * P_i$  that identifies the direct effect of the cash subsidy. This means that the within peer group estimator identifies a *lower bound* on the true direct effect of the policy. Nevertheless, it is important to consider a second estimate of the direct effect of the cash subsidy on poor children to assess the robustness of our findings.

Information on the direct effect of PROGRESA and on the importance of social interactions can be combined to decompose the total effect of the program on eligible children (Table 3, Column A). The total effect is

$$\begin{aligned} E[S_i|T_i = 1, P_i = 1] - E[S_i|T_i = 0, P_i = 1] &= \\ &= \delta + \gamma * (E[S_{ig}|T_i = 1, P_i = 1] - E[S_{ig}|T_i = 0, P_i = 1]) \end{aligned} \quad (3)$$

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 (3) is informative on the relative importance of the individual and social determinants of school attendance decisions. Moreover, equation (3) also highlights why information on the relevance of social interactions – the parameter  $\gamma$  – is important for policy design. Granting access to the transfer for only one household in each village will generate the effect  $\delta$  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  $S_{ig}$  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.<sup>24</sup> The first Column in Table 4 provides the reduced form estimate for the IV1 strategy – the treatment contrast of the trend

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<sup>24</sup>Note that all estimates control for the full set of control variables shown in Table 3. Inference is based on robust standard errors that allow for clustering at the village level. The analysis is confined to the 5,143 children for whom we find at least one other classmate within the village.

in school attendance among the non-poor children with at least one child in their peer group.<sup>25</sup> 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).<sup>26</sup>

Table 4 about here

The second column in Table 4 reports the effect of PROGRESA on peer group average change in school attendance. The cash transfer increases the trend in school attendance among peers by 3.9 percentage points. This effect is statistically significant and quantitatively important. Taken together, the evidence in Table 4 suggests that a change by 3.9 percentage points in peer's school attendance trend generates a 2.1 percentage points increase in ineligible students' school attendance trend.

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  $\gamma$ . Results indicate the marginal effect of peer group schooling on individual schooling of the ineligible student,  $\gamma$ , is .534. 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 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 eligible fraction from peer groups with high eligible 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 eligible fraction.

Figure 1 investigates the spillover effect among non-poor children conditional on the eligible fraction in the peer group. Specifically, in preparing the evidence in Figure 1 we regress the change in school attendance  $S_i$  among ineligible children on the eligible fraction in the peer group  $P_{ig}$  (excluding individual  $i$ ), separately for children living in villages with the program and for children in villages without the program.<sup>27</sup> Figure 1 shows the difference in the two resulting regressions. Clearly, individual school attendance appears to increase as the eligible

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<sup>25</sup>There are 16 children with an empty peer group (living in small villages). The average peer group consists of 12.55 children, with 10 % of all children having a peer group smaller or equal to 4 children and 90 % of all children having a peer group that is smaller or equal to 24 children (standard deviation 9.27).

<sup>26</sup>This suggests that limiting the sample to those children with a non-empty reference group does not significantly affect results.

<sup>27</sup>The non-parametric kernel regressions use an Epanechnikov kernel with bandwidth of .5.

fraction in the peer group increases. For instance, an ineligible child with fewer than 1 out of 5 eligible peers is not shown to experience a different change in school attendance in a treated village compared to the control village. However, when more than 4 out of 5 peers are eligible for the school subsidy, ineligible children appear to be going to school much more likely in treated villages than in control villages. This shows that the spillover effect of the program on ineligible children arises for children with many eligible peers.

Figure 1 about here

The evidence in Figure 1 motivates using the interaction term between the eligible fraction in the peer group and the treatment status of the village 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 eligible fraction as an instrument. Note that the IV2 regression adds the eligible fraction in the peer group to the list of control variables. 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 2 percentage points change in the trend in school attendance in the peer group leads to a 1 percentage point change of schooling trends at the individual level. Moreover, IV2 estimates are statistically significantly different from zero at the 1 % level rather than at the 5 % level. We therefore apply the IV2 strategy to identify social interactions.

## 5.2 Sensitivity Analysis on Social Interactions

Table 5 discusses three important concerns with the identification strategy. Column A in Table 5 reproduces 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, the program may also have increases consumption of the non-poor indirectly. We investigate this concern by controlling for monthly per adult equivalent household consumption using the food and non-food expenditure measures derived by Angelucci and De Giorgi (2006).<sup>28</sup> This expenditure measure is available for 4847 out of the total 5143 children in the sample due to non-response to consumption questions. When we add total food

<sup>28</sup>Descriptive analysis of the total food and non-food expenditures show that the non-poor spend more (193 Pesos) than the poor (154 Pesos). Moreover, consistent with the expected program impacts, we find that the poor in treated villages consume more (158 Pesos) than the poor in control villages (147 Pesos). Yet, the non-poor in treated villages consume slightly less (192 Pesos) than the non-poor in control villages (194 Pesos).

and non-food expenditure to the main analysis, we find that consumption is an important predictor of school attendance trends. Households with higher consumption also see their children leaving school to a lesser extent. However, adding total expenditure to the regression does not affect the result concerning the social interactions parameter. This parameter remains virtually unchanged suggesting that there is a one percentage point change in individual school attendance as a response to every 2 percentage point increase in peer group school attendance.

Table 5 about here

Column C investigates the sensitivity of our findings to village specific fixed effects in school attendance trends. Such effects could arise due to the PROGRESA program being run differently in villages with many eligible households compared to villages with only a few eligible households. Moreover, there could also be differences in local labor market trends across treated and control communities invalidating our identification strategy. We therefore add a fixed effect for every village in the dataset. Results indicate that the point estimate of the social interactions parameter is about .595 which is very similar to the baseline estimate of .546. This shows that the social interactions parameter point estimate is *very robust* to local heterogeneity in villages. On the other hand, controlling for village fixed effects appears to remove too much of the identifying variation leading to a large standard error of .798. Thus, the within-village estimate of the social interactions parameter is not significantly different from zero. Column D checks whether adding some variation increases the precision of the estimates. The estimates in Column D identify endogenous social interactions within 191 local regions ("municipalidad"). Results indicate that the social interactions parameter is .560 (with standard error .274) which is identical to baseline parameter estimate of the social interactions parameter of .546. The within region endogenous social interactions estimate is clearly statistically significantly different from zero.<sup>29</sup>

So far the empirical analysis has assumed that the social interactions parameter  $\gamma$  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

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<sup>29</sup>We have also performed a variety of additional sensitivity analyses (results not shown but available upon request from the corresponding author). The first analysis excludes children at the poverty threshold finding a social interactions parameter of .596 with standard error .249. The second analysis weights ineligible children with the inverse of the probability of being in the sample finding a social interactions parameter of .520 with standard error .309. The third restricts the sample to children on grade level finding a social interactions parameter of .554 with standard error of .229. Thus, all the analyses suggest that endogenous social interactions are economically important and statistically significantly different from zero at the 10 % level.

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 but it is significantly different from zero. 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 the two difference-in-difference estimates of the direct effect of PROGRESA transfers. Column A shows results that instrument the endogenous average school participation according to the IV2 strategy. Column B shows results based on the within peer group approach (section 4).

Table 7 about here

The results of the IV2 difference-in-difference analysis indicate that the financial transfers due to PROGRESA lead to a 3.2 percentage points increase in the school attendance trend between October 1997 and October 1998. Moreover, the social interactions parameter is .513 which is almost identical to the baseline estimate. There appears to be no significant effect of the poverty status of the individual on school attendance. The results from the within peer group estimates indicate that the direct effect of the program on eligible children is at least 3.0 percentage points. Thus, the direct effect of the program on eligible children appears to be robustly identified from the IV2 estimates.

It is now possible to decompose the total effect of PROGRESA on children who live in eligible families into a direct effect that arises due to the financial incentive and an indirect

effect arising due to the effect of PROGRESA on the social environment of the children (Table 8).

Table 8 about here

The direct effect of the program on eligible children is on the order of 3.2 percentage points. The indirect effect is the product of the social interactions parameter (.513) with the peer group response to PROGRESA. The peer group response to PROGRESA can be identified contrasting average school attendance in treated and control villages (conditional on all control variables). It turns out that PROGRESA led to an increase in peer group average school attendance of about 4.8 percentage points. Thus, the total predicted effect of the PROGRESA program on eligible children is 5.7 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.

Moreover, Table 8 shows that the indirect effect of PROGRESA is larger for eligible children than for ineligible children. This is due to the fact that the typical peer group of an eligible child is treated at a much larger rate than the typical peer group of an ineligible child leading to a stronger peer group impact of PROGRESA for eligible children. This highlights the main contribution of our results to the literature concerned with targeted interventions.<sup>30</sup> If social interactions are relevant, targeting is an important determinant of the average effect of the program among eligible individuals. Thus, a program that reaches the same number of individuals will generate a different effect on treated individuals 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.

#### 5.4 Is there Evidence for Social Learning Interactions?

The existing evidence suggests that the decision to stay in school is very strongly affected by the corresponding choices in the peer group in rural Mexico. Moreover, such endogenous social interactions are economically important accounting for nearly one half of the overall effect of PROGRESA on eligible children. It is thus crucial to understand the reasons for such interactions. There are at least two possible explanations. First, children may conform to average choices of their classmates because they like spending time with them – a preference interaction. Second, parents could learn from other parents about the abilities of their children.

Interestingly, official PROGRESA data allows investigating whether this social learning explanation has any explanatory power or not. In October 1998, parents were asked

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

”What level of schooling do you believe your child is able to reach?”

for each child that is attending school in October 1998. Literally, this survey item intends to capture parents’ perceptions of children’s ability to succeed at school.<sup>31</sup> However, it is also possible that the responses to this survey item are affected by what level of schooling parents want their child to reach.

Figure 2 displays the answers to this question, separately for eligible children and ineligible children, and by village program status.

Figure 2 about here

Introducing PROGRESA leads to a more favorable perception of the ability of children living in eligible households (subfigure A). The proportion of parents stating that their child can reach the post secondary school level is 29.4 % in villages without the program but 34.1 % in villages with program. There is also a slightly higher proportion stating that their child is able to succeed in secondary school 55.6 % in villages with PROGRESA compared to 54.3 % in villages without PROGRESA.

Results for ineligible children indicate that – on average – parents from ineligible families are more optimistic regarding the cognitive abilities of their children (subfigure B). Whereas only 32.5 percent of all eligible families believe that their child is able to succeed at post secondary level, about 40.6 percent of eligible households believe that their child is up to the challenge. Surprisingly, introducing the program in the local village also seems to improve ineligible parents’ perception of their child’s ability . The proportion stating that their child can succeed at the post-secondary level is 43.0 % in villages with the program compared to 37.0 % in villages without PROGRESA. In contrast to the eligible families, the increase in the upper end of the ability distribution is accompanied by both a reduction of the proportion of parents thinking their child can succeed at the primary school and the secondary school level.

Are these results significant? Table 9 displays the results of a set of linear probability regressions for the event that the child succeeds at the secondary school level (Columns 1 and 3) and for the event that the child succeeds at the post secondary level (Columns 2 and 4).<sup>32</sup>

Table 9 about here

Results for eligible and ineligible children show that the increase at the top end of the ability distribution is statistically significant from zero. Both, eligible parents and ineligible parents’

<sup>31</sup>In Spanish, this item reads *¿Hasta qué grado cree que podría llegar [su hija / su hijo]?*, item r049 in survey wave October 1998.

<sup>32</sup>These results can also be obtained using an ordered probit model for the level of schooling parents believe that their child can reach. Results are identical to those reported in Table 9.

perception of their child's ability to succeed at the post secondary level increases statistically significantly and on the order of 5 to 6 percentage points. The effects of PROGRESA in the middle of the ability distribution do not appear to be statistically significant.

These results admit two interesting conclusions. First, the results for eligible children suggests that the cash subsidy program not only frees up household resources allowing poor families to send children to school but it also increases parents' subjective evaluations of their child's ability. Sending your child to school can improve your subjective evaluation of that child's ability because the child studies more or because you acquire more information on the child's true ability. Second, the results for ineligible children suggest that parents' evaluations of their child's ability increase one for one when eligible parents' perception of child ability increases. This is indeed consistent with *relative* evaluation of a child's ability.

Taken together, we have shown that the endogenous social interaction effects may arise in the PROGRESA setting because the program improves eligible parents perception of child quality. Because subjective evaluations of a child's ability is relative the program-induced increase in the perception of eligible children's ability also leads to improved evaluation of ineligible children. Thus, ineligible parents are more likely to send their children to school because they feel their child can succeed at higher levels. This line of reasoning suggests that there is at least some scope for alternative policy measures that target parent perceptions of child ability to improve schooling outcomes in rural Mexico. This is an important result because if the endogenous social interactions in schooling were purely a result of preference interactions, such a policy would not have an effect.

Note that there is an important limitation of this analysis. Parent perceptions are only available for children attending school, the responses to this question are selective. However, since the program increases school attendance and since parents send the most able children to school, the effect of the program on perceptions estimated in the selective sub-sample is likely to be a lower bound on the shift in the population distribution of parents perceptions.

## 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. 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 living in the same village. 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. Fourth, data on parents subjective evaluation of their children's ability indicates that both, eligible families and ineligible families subjective evaluation of the quality of their children is more favorable when the program is introduced. This suggests that endogenous social interactions arise because parents' perceptions of child quality are affected by other parents' evaluation of the quality of their children – evaluations are relative.

These findings are important for education policy. Strong social interactions in schooling decisions imply that the targeting of the program matters for the average effect of the program on the eligible students. Moreover, because social interactions arise to some extent due to changes in parents subjective evaluation of the ability of their children, alternative school attendance programs could focus on informing parents about the ability of their children.

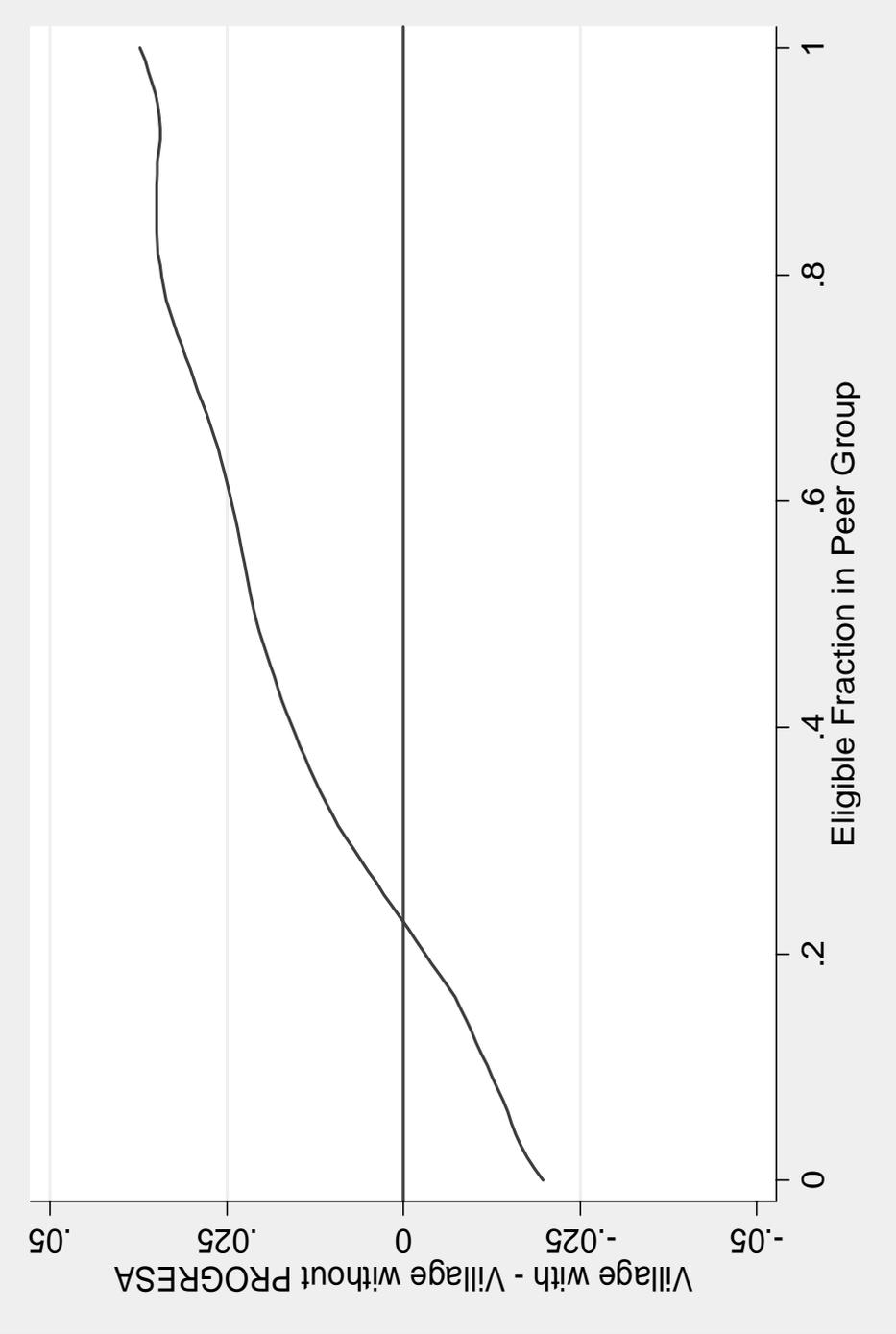
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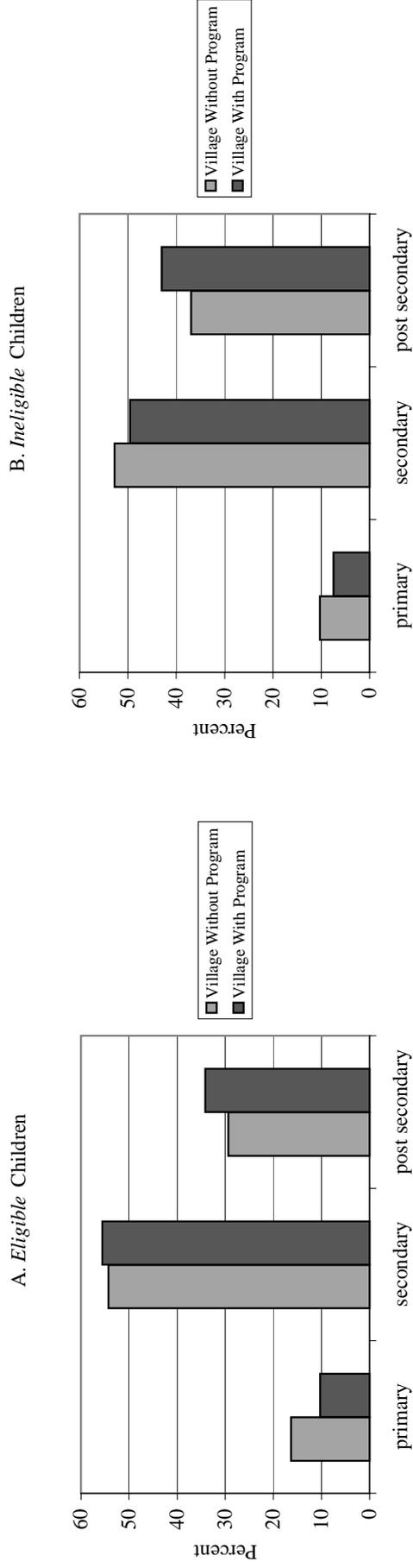
Figure 1:  
Explaining the Spillover Effect on *Ineligible* Children



Notes: Epanechnikov-Kernel Regression (bandwidth .5) of change in school attendance on the eligible (=poor) fraction in children attending same grade in same village (=peer group).

Source: Own Calculation, Based on Progresa Evaluation Data.

Figure 2:  
 What level of schooling do you believe your child is able to reach?  
 Survey Response in October 1998 (During Program)



Notes: Figure displays the answer to question "¿Hasta qué grado cree que podría llegar?", item r049 in survey wave October 1998. Post secondary level combines "preparatoria", "técnico", "normal", "profesional", and "otro".

Source: Own Calculation, Based on Progesa Evaluation Data.

Table 1:  
Descriptive Statistics:  
Pre-Program Characteristics of Children

Village with PROGRESA	A. Eligible			B. Ineligible		
	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)
Observations	3880	6604		2083	3086	

Notes: Eligible means household is classified as poor. Mothers in eligible households receive a cash grant for each child attending school more than 85 % of each quarter in grades 3 to 6 of primary school and grades 1 to 3 of secondary school. All characteristics are measured in October 1997, i.e. about 8 months before PROGRESA started. Sample refers to children who have completed grades 3 to 6 of primary school who are living with their mother.

Source: Own Calculation, Based on Progresa Evaluation Data.

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

	<i>A. Eligible</i>		<i>B. Ineligible</i>	
	No	Yes	No	Yes
Village with PROGRESA				
		Difference (abs. z-Value)		Difference (abs. z-Value)
School Attendance in 1997 (Pre Program)	0.769	0.782	0.758	0.782
		(0.897)		(1.305)
School Attendance in 1998 (During Program)	0.685	0.757	0.691	0.733
		(3.798)		(1.907)
Change in School Attendance 1998-1997	-0.084	-0.025	-0.067	-0.049
		(5.511)		(1.484)
Observations	3880	6604	2083	3086

Notes: School attendance is the item "Does your child currently attend school?" that was posed to the parents. "Yes" is coded as a value of 1, "No" is coded as a value of 0. October 1997 is 8 months before PROGRESA started, October 1998 is 2 months after PROGRESA started. See notes to Table 1 for definition of the program and eligibility.

Source: Own Calculation, Based on Progres Evaluation Data.

Table 3:  
The Effect of PROGRESA Transfers on Poor and Non-Poor Children  
Dependent Variable: Change in School Attendance 1998-1997

	<i>A. Eligible</i>	<i>B. Ineligible</i>
Village with PROGRESA	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)	Yes	Yes
Observations	10484	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: *Ineligible* Children

Dependent Variable	Change in Individual School Attendance	Change in Peer Group School Attendance	Change in Individual School Attendance	Change in Individual School Attendance
	<i>A. OLS</i>	<i>B. OLS</i>	<i>C. IV1</i>	<i>D. IV2</i>
Change in Peer Group School Attendance	-	-	0.534 (0.235)**	0.546 (0.207)***
Village with PROGRESA	0.021 (0.012)*	0.039 (0.010)***	-	-
Control Variables	Yes	Yes	Yes	Yes
State Effects (7)	Yes	Yes	Yes	Yes
F-Statistic	7.40***	9.59***	-	-
First-Stage F-Statistic	-	-	9.59***	9.40***
Observations	5143	5143	5143	5143

Notes: Robust standard errors in parentheses (allow for clustering at village level). \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Peer group consists of all children in same grade in same village.

IV1 uses "Village with PROGRESA as Instrument", IV2 uses "Village with PROGRESA and Village with PROGRESA \* Eligible 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

Sample: *Ineligible* Children

	A. <i>Baseline Result</i>	B. <i>Controlling for Consumption Expenditures</i>	C. <i>Within Village Variation</i>	D. <i>Within Region Variation</i>
Change in Peer Group School Attendance	0.546 (0.207)***	0.554 (0.213)***	0.595 (0.798)	0.560 (0.274)**
Household consumption (/100)	-	0.031 (0.015)**	-	-
Household consumption sq.	-	-0.004 (0.002)**	-	-
Control Variables	Yes	Yes	Yes	Yes
State Effects (7)	Yes	Yes	No	No
Region Effects (191)	No	No	No	Yes
Village Effects (505)	No	No	Yes	No
Observations	5143	4847	5143	5143

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

See Table 4 for a definition of the peer group and Table 3 for a list of all control variables. Estimates based on IV2 (see Table 4 for definition).

Source: Own Calculation, Based on Progesa Evaluation Data.

Table 6:  
 The Effect of Peer Group Schooling on Individual Schooling in Subgroups  
 Dependent Variable: Change in School Attendance  
 Sample: *Ineligible* Children

	A. Grades 3-4	B. Grades 5-6	C. Girls	D. Boys
Change in Peer Group School Attendance	0.601 (0.293)**	0.484 (0.259)*	0.535 (0.276)*	0.591 (0.302)*
Control Variables	Yes	Yes	Yes	Yes
State Effects (7)	Yes	Yes	Yes	Yes
Observations	2205	2938	2589	2554

Notes: Robust standard errors in parentheses (allow for clustering at village level).

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

See Table 4 for definition of peer group.

See Table 3 for a list of all control variables. Estimates based on IV2.

Source: Own Calculation, Based on Progresa Evaluation Data.

Table 7:  
Measuring Direct and Social Interaction Effects of PROGRESA  
Dependent Variable: Change in School Attendance  
Sample: *Eligible* and *Ineligible* Children

	<u>A. IV2</u>	<u>B. Within Peer Group</u>
Village with PROGRESA * Poor	0.032 (0.015)**	0.030 (0.014)**
Change in Peer Group School Attendance	0.513 (0.246)**	-
Poor	-0.011 (0.011)	-0.009 (0.011)
Control Variables	Yes	Yes
State Effects (7)	Yes	No
Peer Group Effects (1882)	No	Yes
Observations	15574	15574

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.

Change in Peer Group School Attendance instrumented (IV2).

Source: Own Calculation, Based on Progresa Evaluation Data.

Table 8:  
Comparing Direct and Indirect Effects of PROGRESA  
Based on Results in Table 7

	<u>A. Eligible</u>	<u>B. Ineligible</u>
I. Direct Effect of Cash Grant	0.032 (0.015)**	0.000 -
II. Social Interactions Parameter	0.513 (0.246)**	0.513 (0.246)**
III. Effect of Cash Grant on Peer Group	0.048 (0.009)***	0.039 (0.010)***
IV. Implied Social Interactions Effect (II*III)	0.025 (0.011)*	0.020 (0.013)*
V. Total (I+IV)	0.057 (0.019)***	0.020 (0.013)*

Notes: I and II taken from Table 7, IIIA taken from a regression of peer group average schooling on "village with PROGRESA" and controls, IIIB taken from Table 4. Standard errors for IV and V calculated using delta method.

Source: Own Calculation, Based on Progresa Evaluation Data.

Table 9:  
The Effect of Progesa on Parent Perceptions of Child Ability

	<i>A. Eligible Children</i>		<i>B. Ineligible Children</i>	
	... secondary school	... post secondary school	... secondary school	... post secondary school
My child is able to reach ...				
Village with PROGRESA	0.015 (0.020)	0.048 (0.020)**	-0.028 (0.026)	0.060 (0.026)**
Control Variables	Yes	Yes	Yes	Yes
State Effects (7)	Yes	Yes	Yes	Yes
Observations	7473	7473	3630	3630

Notes: Robust standard errors in parentheses (allow for clustering at village level). \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Parent perceptions measured in October 1998 for children attending school.

Source: Own Calculation, Based on Progesa Evaluation Data.

Table A1:  
PROGRESA cash subsidy in August-December 1998, per child

	A. Girl	B. Boy
Primary School		
3	70	70
4	80	80
5	95	95
6	115	115
Secondary School		
1	210	200
2	235	210
3	255	225

Source: Table 1 in Behrman, Sengupta, Todd (2001)