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The effect of social programs and exposure to professionals on the educational aspirations of the poor

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ABSTRACT

Investment in human capital is an important tool for reducing poverty. However, the poor may lack the capacity to aspire, which often results in underinvestment in their children's education. This paper studies the effect of a social program on the educational aspirations poor parents have for their children, and explores the role of exposure to educated professionals as a possible channel for increasing these aspirations. First, using differences-in-differences, we show that the Mexican antipoverty program PROGRESA raises the educational aspirations of beneficiary parents for their children of a third of a school year. Then, exploiting PROGRESA's mandated differential exposure to professionals, using triple differences, we find evidence suggesting that educational aspirations for children from high-exposure households are almost half of a school year higher six months after the start of the program. Finally, we show that there is a positive correlation between parental aspirations and children's educational attainment.

1. Introduction

Many have argued that education specifically and investment in human capital more generally could be the most effective way to reduce poverty (e.g., Becker, 1995). However, poverty may not only create constraints that limit the ability to invest in human capital, but it may also affect people's attitudes and interest in education. If the poor perceive a narrower range of life options or possibilities, they may lack the capacity to aspire, which leads to underinvestment in their children's education (Appadurai, 2004; Ray, 2006).

Research shows that parents' educational aspirations for their children are positively correlated with their children's educational outcomes (Goodman & Gregg, 2010; Gregg & Washbrook, 2009; Gutman & Akerman, 2008a, 2008b), and that higher aspirations can lead to an increase in investment in human capital (Macours & Vakis, 2009) and have a significant effect on labor supply (Datcher-Loury & Loury, 1986). Hence, helping the poor to enhance their aspirations may have a positive effect on reducing poverty. In particular, understanding whether aspirations can be changed and identifying the channels through which this change can occur are essential. Unfortunately, little research is available on the evolution of aspirations.

This paper studies the effect of an antipoverty program on poor parents' educational aspirations for their children and explores the role of mandated exposure to educated professionals as a possible way to increase aspirations. In particular, it analyzes whether poor parents' aspirations for the educational attainment of their children can be improved as a result of exposure to doctors and nurses—a group of individuals with much higher educational level and economic status than theirs.

Exposure to educated professionals has been shown to be important by both psychologists and economists.

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Studies in psychology show that outstanding others can exert a positive impact on individuals (Lockwood & Kunda, 1997; Major, Sciacchitano, & Crocker, 1993), and, within economics, Cutler and Glaeser (1997) find that neighborhood segregation is harmful to blacks because they have less exposure to educated people.[1]

First, we analyze the effect of the Mexican antipoverty program PROGRESA on the number of years of schooling parents aspire their children to study. Using differences-in-differences we compare parental aspirations of households that had been randomly selected to receive the benefits of PROGRESA, against the parental aspirations of households that had not been selected to participate in the program. As an alternative aspiration variable, we also consider the proportion of parents who declared that they wanted their children to at least finish college in order to see the impact of the program on the proportion of households that aspired for their children to complete college.

Second, we explore the role of mandated exposure to educated professionals on parental aspirations. We exploit the design of PROGRESA whose requirements cause its target population to have different levels of exposure to doctors and nurses. We divide the sample into two groups: households whose youngest child is less than five years of age—which have a high level of exposure to health personnel because they must visit the clinic at least four times per year—and households whose youngest child is five years of age or older—which have a low level of exposure to health personnel because they must visit the clinic only once or twice per year. To identify the effect of the differential exposure to educated professionals on parents' aspirations, we use a triple differences estimator. That is, we estimate the change in average aspirations before and after the introduction of PROGRESA for households with high exposure to highly educated professionals relative to households with low exposure in treatment villages relative to control villages.

Finally, we present evidence on the link between parental aspirations and objective educational outcomes, both in the short- and in the long-run. In the short-run, we analyze the correlation between parental aspirations and the number of minutes children spend doing their school homework, and the time children spend working. In the long-run, we consider the correlation between parental aspirations in 1998 and their children's educational attainment by 2007.

Identifying a possible channel through which aspirations of the poor can be modified adds a new tool to the existing options that try to promote increased investments in human capital and productive assets as a means to escape poverty. Also, by design, a number of anti-poverty programs expose their target populations to doctors, nurses, teachers, and many other highly educated professionals. Policy-makers could harness the potential benefit of increased aspirations that are associated with exposure to highly educated professionals by encouraging or requiring that the beneficiaries of anti-poverty programs meet with such professionals a sufficient number of times. Finally, in highly segregated environments or in contexts in which there is low social interaction or lack of leadership, promoting exposure to external educated professionals may have important consequences with respect to the aspirations of the population.

The effect of exposure to professionals on aspirations might operate through a number of different mechanisms. Exposure to highly educated professionals may cause information flows that allow parents to learn about (previously unknown or previously considered unattainable) opportunities for their children and the investment it takes to reach these opportunities (Jensen, 2012); it may change the consideration sets for people who have limited knowledge or bounded rationality (Jensen, 2010; Nguyen, 2008); and/or it may change the socioeconomic environment of the poor.

This study is linked to the theoretical work on why exposure to individuals with a higher educational level and economic status may matter for increasing aspirations and decreasing poverty. In this sense, our paper brings support to the ideas developed in Ray (2006) and previously by anthropologists such as Appadurai (2004), which assign a central role in the formation and evolution of individual aspirations to the socioeconomic environment.[2] This paper also relates to the active discussion on the fact that people's choices are affected by a limited considerations set. This basic idea has been discussed under a range of forms e.g., the literature on bounded rationality, narrowing bracketing, and limited attention (Barberis, Huang, & Thaler, 2006; Conlisk, 1996; DellaVigna, 2009; Frank, 1985, 1997; Gabaix, Laibson, Moloche, & Weinberg, 2006; Kahneman, 2003; Rabin & Weizsäcker, 2009; Rubinstein, 1998). Likewise, our research is connected to studies on how people's choices are conditioned by their sense of identity (Hoff & Pandey, 2004; Munshi & Rosenzweig, 2005), their perceived returns (Jensen, 2010), limited knowledge and role models (Jensen, 2012; Nguyen, 2008)). Also, our study is linked to the empirical literature on social interactions and peer effects, which shows that residents of poor neighborhoods achieve lower socioeconomic outcomes and attain lower educational levels than do the residents of more affluent neighborhoods (Case & Katz, 1991; Gould, Lavy, & Paserman, 2009; Kling, Liebman, & Katz, 2007; Sánchez-Peña, 2007). In fact, our paper suggests that social exposure could be a way to attain better behavioral outcomes in poor areas as in Nguyen (2008). Moreover, findings from this study are in line with previous research showing that the environment—the community of residence, attendance to a community college, cultural interactions, and resource availability—affects educational aspirations, especially of students from disadvantaged backgrounds (Binder, 2008; Leigh & Gill, 2004; Tramonte & Willms, 2010; Unnever, Kerckhoff, & Robinson, 2000). Finally, this paper also relates to the study by Mora and Oreopoulos (2011) showing that educational aspirations of peers play a role on students' aspirations.

[1] From a theoretical perspective, Genicot and Ray (2010) model one's aspirations as a function of one's social environment, i.e. the lifestyles and experiences of others.

[2] Additional papers are Mookherjee, Napel, and Ray (2010), and Genicot and Ray (2009).

The following section describes PROGRESA and explains how the program promotes the exposure of the beneficiaries to individuals with higher educational levels and economic status as well as how we identify the subgroups subject to a higher level of exposure. Section 3 describes the data. Section 4 outlines the empirical strategy and shows the results. Section 5 performs some robustness checks. Section 6 shows the relationship between educational aspirations and behavioral outcomes. Finally, Section 7 summarizes the results and concludes.

2. PROGRESA and beneficiaries' exposure to health personnel[3]

In 1997, the Mexican government started the *Programa de Educación, Salud y Alimentación* (PROGRESA) in rural Mexico in an effort to break the intergenerational transmission of poverty. The primary objective of the program is to improve the educational, health, and nutritional status of poor families, particularly of children and mothers (Skoufias, 2005). PROGRESA's two main components are health and education. In this paper, we exploit a key feature of the former: the requirement for every family member to visit his or her locality's health centers for individual preventive and/or monitoring check-ups. The frequency of the visits for each member depends on his or her age.[4] Households that satisfy the health component's requirements secure a small monetary (health-conditional) transfer of a fixed amount regardless of household size.[5]

The main reason for which the program started providing health-conditional transfers was to stimulate health clinic attendance and regular check-ups, which were both very low in rural Mexico (see Gertler, 2000). PROGRESA seems to have successfully changed its beneficiaries behavior. In fact, Adato, Coady, and Ruel (2000) report that the introduction of the program caused a big difference in attendance to the health clinics with beneficiaries regularly attending their programmed visits.[6] For the purposes of our paper, beneficiaries' change in behavior is relevant because, by attending their scheduled visits, they became exposed to nurses and doctors.

Mandated exposure to the health personnel is important for at least two reasons. First, health professionals have attained higher educational levels and higher economic status than the individuals in our sample. In Mexico, nurses have at least 14 years of education and doctors at least 18. These education levels are much higher than those of the adult population under consideration, which has, on average, three years of schooling (see Table 1). Second, the communities in our sample are very isolated with very little exposure to any educated professionals, e.g. only 1% of the communities has secondary schools, there are no schools beyond secondary, no banks, and no pharmacies or hospitals besides the health clinics PROGRESA's beneficiaries have to attend (Skoufias, 2005). In addition, in our localities less than 1% of the adults have more than 12 years of schooling (high school). Thus, educated people are individuals with whom beneficiaries did not have regular contact before the start of PROGRESA.

Furthermore, an important aspect of health clinics in rural Mexico is that in most cases the clinics have a staff of two: one doctor and one nurse (Adato et al., 2000). This implies that PROGRESA's beneficiaries are always exposed to the same health professionals. Thus, it is not surprising that the programmed visits eventually cause "communication and bonding between the health center [staff] and the community" (Adato et al., 2000, p. 92). Moreover, "many doctors talk about the changes in peoples' thinking and attitudes brought about by PROGRESA" (Adato et al., 2000, p. 90).

The frequency of the visits to the health clinics for each member depends on his or her age. Thus, mandated exposure to doctors and nurses differs among beneficiary households depending on their demographic structure. The extent of exposure is higher for households with children less than five years old compared to households with older or no children at all.[7] Specifically, households whose youngest child is less than five years old must go to the health clinics at least four times per year. In contrast, households whose youngest child is five or older must visit the health clinics twice per year, and households without children only once.

Finally, the other main component of the program is the educational component. Beneficiary households with children ages 9–17 who are enrolled in school and attending at least 85% of the school days each month as well as during the academic year receive an education-conditional grant. The grant increases with grade and, for secondary education, is slightly higher for girls than for boys. In

[3] This section draws on Gertler (2000) and Skoufias (2005) who provide a much more detailed description of the program and evaluation data set.

[4] An additional requirement of the health component is the monthly attendance by every female household head to group educational talks about vaccinations, nutrition, contraception, and hygiene. The content and frequency of group talks is the same for all households independently of their demographic structure. In addition, while mandatory check-up visits to the health clinics are strictly with doctors and nurses, group talks can be given by people with minimal training and even possibly limited formal education (Adato et al., 2000).

[5] Households also receive nutritional supplements for children less than two years old and pregnant and lactating women. Nutritional supplements are also provided for children ages 2–5 if they present stunting symptoms.

[6] Our data does not allow us to analyze how many visits on average did families do the health centers before and after the implementation of PROGRESA. Data on household visits to the health clinics is available only for the fourth survey round of the program's evaluation sample (ENCEL99M). Four weeks prior to this survey round, the average number of visits for treatment households is 0.13, while for the control households is 0.04. The difference is statistically significant (t-stat = 5.99). Gertler (2000) using non-public administrative records of 3541 public clinics from January 1996 to December 1998 uses difference-in-difference analysis to compare the change (before and after PROGRESA) in visits per day to clinics in treatment localities versus control localities and shows that visits in treatment localities were 2.1 higher than in control localities.

[7] In particular, children less than two years old must visit the clinic every two months for growth monitoring, immunizations, and well-baby care; children 2–5 years old must visit the clinic every three months for growth monitoring, well-child care, and immunizations; children 5–16 must visit the clinic once every six months; and other adolescents and adults must visit the clinic for annual physical check-ups (Gertler, 2000; PROGRESA, 1999). Additionally, in the case of women, the frequency of the visits increases if the women are pregnant or have recently given birth.

Table 1
Descriptive statistics by treatment status, fixing household structure as of baseline (1997).

	Obs.	Mean		*t*-Stat
		Treatment	Control	
(a) Characteristics of the head of the household				
Age	8089	41.66	42.39	−2.09**
Educational level in years	8078	2.88	2.78	0.85
Literate	8100	0.72	0.71	0.26
Indigenous	8096	0.41	0.42	−0.07
(b) Characteristics of the spouse of the head of the household				
Age	7361	36.70	36.86	−0.59
Educational level in years	7348	2.65	2.63	0.16
Literate	7359	0.63	0.62	0.56
Indigenous	7353	0.41	0.41	0.00
(c) Characteristics of the household				
Mean age of adults	8104	36.16	36.55	−1.46
Mean educational level of adults	8103	3.24	3.16	0.66
Proportion of literate adults	8103	0.71	0.70	0.48
Proportion of indigenous adults	8095	0.40	0.41	−0.06
Income	8106	922.90	946.03	−0.56
(d) Household structure				
Size	8106	6.75	6.75	−0.02
Number of adults	8106	2.68	2.68	0.15
Number of female adults	8106	1.37	1.38	−0.44
Number of male adults	8106	1.31	1.29	0.83
Proportion of male adults	8102	0.48	0.48	0.61
Number of children	8106	4.06	4.06	−0.10
Number of female children	8106	1.96	2.01	−1.13
Number of male children	8106	2.09	2.05	1.25
Proportion of male children	8069	0.52	0.51	2.20**
Proportion of households with children less than 5 years old	8106	0.65	0.63	1.18
Birth spacing between children				
Between 1st and 2nd child	7326	3.23	3.35	−1.24
Between 2nd and 3rd child	6423	2.90	2.88	0.32
Between 3rd and 4th child	4884	2.80	2.81	−0.19
Between 4th and 5th child	3240	2.64	2.72	−1.29
Between 5th and 6th child	1953	2.54	2.63	−1.25
Between 6th and 7th child	1014	2.38	2.43	−0.61
Between 7th and 8th child	467	2.34	2.31	0.30
Between 8th and 9th child	184	2.12	2.19	−0.37
Between 9th and 10th child	94	1.95	1.76	0.89
(e) Aspirations				
Parental aspirations for all children	8106	11.43	11.58	−0.97
Parental aspirations for daughters	8038	11.32	11.48	−1.06
Parental aspirations for sons	8044	11.55	11.67	−0.80

Note: *t*-Statistics of difference in means computed clustering at the village level.
** Differences significant at the 5% level.

addition, households with the appropriate school-age children receive a grant for school supplies. In general, all transfers are received by the female household head.

On average, beneficiary households receive about 197 pesos monthly (expressed in November 1998 pesos)[8]; this represents 19.5% of the mean value of consumption of eligible households in control localities (Skoufias, 2005). The program has survived two changes of administration in Mexico. However, at its inception, beneficiaries were granted the program's benefits for only a three-year period. This was a credible threat because, prior to PROGRESA, social programs in Mexico used to dissolve as soon as there was a change in the political administration (Barajas, 2002; Levy & Rodríguez, 2005; Lustig, 2011).[9]

[8] The calculation of this average includes households that did not receive any benefits due to nonadherence to the conditions of the program or delays in the verification of the requirements of the program or in the delivery of the monetary benefits (Skoufias, 2005). The exchange rate at the time was about MX$10.00 = US$1.00.

[9] Mexico's anti-poverty programs tended to start and disappear with each six-year term of the Mexican presidency (even when much of the same technocracy remained in place because the incumbent and the incoming presidents belonged to the same party). The social program COPLAMAR (National Plan for Depressed Areas and Marginalized Groups) established by president López Portillo in 1976 was terminated soon after de la Madrid began its presidency in 1982. Similarly, the *Programas de Desarrollo Regional* (Programs for Regional Development) started by president de la Madrid in 1983 was terminated in 1988 with the arrival of president Salinas de Gortari. PRONASOL (National Solidarity Program), Salinas' flagship program, disappeared in 1994 when president Zedillo went to power (Barajas, 2002; Lustig, 2011). However, PROGRESA, started

3. The data

An experimental design was adopted for PROGRESA's evaluation, exploiting its sequential expansion. A subset of 506 eligible localities in Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí, and Veracruz was randomly chosen to participate in the evaluation sample: 320 localities were randomly chosen as treatment and started receiving benefits in May 1998; 186 were used as controls and started receiving benefits in December 1999. In the control localities, no household was informed that PROGRESA would have provided benefits at a later date. In every locality where the program is implemented, households are selected as eligible to receive PROGRESA's benefits based on their poverty level.

For its evaluation, PROGRESA has collected household data over eight survey rounds. The data used in this paper come mainly from the first four survey rounds.[10] The first two rounds are baseline surveys. That is, they were carried out before the program started giving benefits to the eligible treatment households. The next two rounds were carried out once PROGRESA had started giving benefits to the eligible treatment households, but before control households were incorporated into the program. The first survey round contains important household and individual characteristics information, but no data on aspirations. The second, third, and fourth survey rounds contain data on aspirations. Hence, we consider the sample of households in the second round as our baseline sample and recover household and individual characteristics from the first round.

In the second round, 22,021 households successfully completed their interview. Furthermore, 38 households, whose interview was classified as "not completed successfully" by the program, were also considered in this paper as they have all the information we need. This gives us a total of 22,059 households. From this sample, we consider only those households that were classified as eligible at baseline unless otherwise noted.[11] This leaves us with a sample of 11,336 households.[12] Out of this sample, only 8106 households have data on aspirations. This is the sample we use in the first part of the paper in order to identify the effect of PROGRESA on parental educational aspirations.

Out of the 8106 eligible households with aspirations data we have at baseline, 7396 and 7031 are found in the third and fourth survey rounds, respectively. In the third survey round, the attrition rate for households in treatment localities is 8.2%, while for households in control localities is 9.7%. In the fourth survey round, the attrition rates are 12.5% and 14.5% for households in treatment and control localities, respectively. None of the differences in attrition rates are statistically significant.[13]

In the second part of the paper, in order to divide our sample into high- and low-exposure households, we use the age of the youngest child in the third and fourth survey rounds. However, this information is missing for some households in those rounds. Hence, the number of households that we consider in any regression depends on the attrition rate between survey rounds and the missing information on the age of the youngest child. Thus, when studying differences in aspirations between the second (baseline) and third (six months after the start of the program) survey rounds, we consider all eligible households with information on the age of their youngest child in the third survey round, found both in the second (7833) and in the third survey rounds (7434).[14] Similarly, when studying differences in aspirations between the second (baseline) and fourth (one year after the start of the program) survey rounds, we consider all eligible households with information on the age of their youngest child, found at baseline (7029) and in the fourth survey round (7394).[15]

The data we have on aspirations is as follows. The second baseline survey asks the following two questions to the respondent: "Up to what level would you like your daughters to study?" and "Up to what level would you like your sons to study?"[16] In the third and fourth rounds, the respondent was asked the same question for each of her daughters (sons).

Responses are coded by education level: elementary school (6 years of schooling), secondary school (9), high school (12), technical degree (12), college (16), and other

in 1997 during the Zedillo administration, survived the transition to presidents Fox in 2000 and Calderón in 2006. Hence, differently from previous Mexican anti-poverty programs, PROGRESA has been shielded from political changes, surviving three administrations since its creation.

[10] The first four survey rounds are: ENCASEH97,conducted in November 1997; ENCEL98M, conducted in March 1998; ENCEL98O, conducted in October/November 1998; and ENCEL99M, conducted in May 1999. In addition, in Section 6 of the paper we also use data from the eighth survey round, ENCEL2007.

[11] By July 1999, the program's administration had added new households to the list of beneficiaries because it felt that the original selection method was biased against the elderly poor who no longer lived with their children (Skoufias, 2005). These households started receiving the benefits of PROGRESA about eight months after the original beneficiaries did (Skoufias, Davis, & de la Vega, 2001). For our analysis, we classify these households as non-eligible given their late admission.

[12] About 98% of the eligible households living in treatment localities chose to enroll in the program. All eligible households within a treatment locality started receiving the program's benefits at the same time (Skoufias, 2005).

[13] Running a simple regression of whether a household remains in the sample in rounds three and four, respectively, against a treatment dummy, the t-statistic of the difference in attrition rates in 1.63 for round three and 1.37 for round four. If we add controls to the regression, the t-statistics become 1.60 and 1.33 for rounds three and four, respectively. The controls included in the regressions are the head and spouse's age, schooling level, literacy, and indigenous status, number of male and female adults and children, and the household's monthly income. Regressions also include an indicator for whether data on parental characteristics were unavailable. These households are assigned the cross-sectional mean sample values of the variables.

[14] In this case, the attrition rate for households in treatment and control localities is 5.5% and 5.8%, respectively. The difference is not statistically significant (t-stat = 0.50). The t-statistic becomes 0.38 once we include controls in the attrition regression.

[15] The attrition rates in this case are 5.1% and 6.1% for treatment and control localities, respectively. The difference, again, is not statistically significant (t-stat = 1.60). The t-statistic becomes 1.37 once we include controls in the attrition regression.

[16] The exact questions in Spanish are: *¿Hasta qué nivel le gustaría que estudiaran sus hijas?* and *¿Hasta qué nivel le gustaría que estudiaran sus hijos?*

(up to 21 if Ph.D.). For estimation purposes, we translated each of these levels into years of education as specified in the parentheses next to each level.

As we are studying parental aspirations, we conduct the analysis at the household level. For the second survey round parental aspirations are already expressed for all daughters (sons) at the household level. Instead, for the third and fourth survey rounds we compute the average years of education that the respondent declared that she would like her daughters (sons) to study if the household has more than one daughter (son).[17] Finally, in order to analyze changes in parental aspirations for all children, we compute the average years of education that the respondent declared she would like any of her children to study.[18]

Table 1 shows descriptive statistics by treatment status at baseline. It presents information on parental and household characteristics, on the households' demographic structure, and on parental aspirations. Treatment and control households do not seem to differ significantly except for the fact that treatment households appear to have a somewhat younger head and a slightly higher proportion of male children. In the empirical analysis, we control for such differences. With respect to aspirations, parents aspire for their children to complete about 11.5 years of schooling. Differences between treatment and control households are not statistically significant.

4. Empirical strategy and results

This section is divided into two parts. First, we study the effect of PROGRESA on the educational aspirations of the poor. Then, we investigate exposure to educated professionals as a possible channel through which aspirations might change.

4.1. PROGRESA's effect on aspirations

Having information on the parents' aspirations for their children's education from three periods (one before and two after the implementation of the program) allows us to estimate the impact after six months and after one year from the start of PROGRESA using differences-in-differences. In particular, we estimate the following reduced form regression:

$$ASP_{ivt} = \alpha_0 + \alpha_1 X_{ivt} + \alpha_2 \tau_t + \alpha_3 T_v + \alpha_4 (T_v \times \tau_t) + \varepsilon_{ivt} \quad (1)$$

where ASP_{ivt} denotes the educational aspirations of the parents of household i in village v at time t; X_{ivt} represents the set of observable characteristics statistically different between control and treatment households[19]; τ_t is a time dummy; T_v is a village dummy that equals one for households in treatment villages; and ε_{ivt} is an idiosyncratic error term. The coefficient of interest is α_4, which estimates the impact of PROGRESA on the educational aspirations of the beneficiaries toward their children.

The estimates of regression (1) are reported in Table 2. We analyze the impact of PROGRESA on the educational aspirations of parents toward all their children (columns (1)–(3)), and toward their daughters (columns (4)–(6)) and their sons (columns (7)–(9)) separately. Panels A and B in Table 2 present the regression results six months and one year after the start of the program, respectively. We estimate the effect of PROGRESA without controlling for unbalanced household characteristics in columns (1), (4), and (7), and including controls in columns (2), (5), and (8). In all specifications standard errors are clustered at the village level.

PROGRESA is associated with an increase in educational aspirations of about a third of a school year. Columns (1), (4), and (7), show that the magnitude of the effect is 0.34 and 0.30 years of schooling for all children six months and one year after the start of the program, respectively; 0.38 and 0.34 years of schooling for daughters; and 0.26 and 0.22 years of schooling for sons. These numbers are statistically significant at conventional levels when considering all children and daughters. Including the control variables alters neither the magnitude nor the precision of the coefficients of interest in any of the regressions.

In general, we find that the coefficients for daughters are greater in magnitude than the coefficients for sons.[20] To better understand the reason why changes in parental aspirations are greater for daughters than for sons, it would be useful to have data on the gender of the personnel in the health clinics in the 1998–1999 period. Unfortunately, this type of data is available for 2007 only. In this year, while 46% of the doctors were female doctors, 93% of the nurses were female nurses. Thus, to the extent that the proportion of female doctors and nurses in 1998–1999 was similar to the one in 2007, a plausible explanation of our results would be that parents are changing their aspirations for daughters due to their exposure to female health

[17] When asked for all daughters (sons), a respondent may say she would love all of them to be rocket scientists because she values education. However, when asked about a specific child, as in survey rounds three and four, her aspirations may also reflect her expectations about the child's abilities, and she may say she would be happy if the child just finished high school. If this were to be the case, the maximum number of years of education that the respondent declared that she would like her daughters (sons) to study would differ from the minimum number of years of education that the respondent declared that she would like her daughters (sons) to study. We do not find this. In fact for only 4% of the households the maximum differs from the minimum. Nevertheless, when the maximum differs from the minimum, we use the average. The correlation between the average and the maximum is 0.98, and the correlation between the average and the minimum is 0.97. Also, the results of the paper do not change when using as alternative variables the minimum or the maximum instead of the average.

[18] Results do not change if we use the maximum or the minimum years of education.

[19] The variables that are statistically different are the age of the head of the household and the proportion of male children in the household. Data on the age of the head of the household and on the proportion of male children in the household are missing for 66 and 95 observations, respectively. There are no statistical differences in the number of missing values between treatment and control groups. We replace the missing values by the cross-sectional mean sample values of the variables, and include a dummy for these observations in the regressions. Running the regressions without these observations does not change the results.

[20] However, we can reject the null that the difference between daughters and sons' coefficients are equal to zero only for the coefficients reported in Table 2 (the t-statistic for the difference is 1.64).

Table 2
Differences-in-differences estimates.

	All children			Daughters			Sons		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: impact after 6 months									
PROGRESA effect (Time × Treatment)	0.338**	0.335**	0.338**	0.383**	0.379**	0.381**	0.261	0.266	0.270
	(0.159)	(0.159)	(0.160)	(0.165)	(0.164)	(0.165)	(0.167)	(0.167)	(0.167)
Time dummy	1.118***	0.843***	1.108***	1.164***	0.711***	1.145***	1.161***	0.994***	1.14***
	(0.127)	(0.220)	(0.127)	(0.130)	(0.248)	(0.130)	(0.134)	(0.258)	(0.134)
Treatment dummy	−0.146	−0.149	−0.159	−0.160	−0.163	−0.173	−0.123	−0.126	−0.135
	(0.151)	(0.151)	(0.145)	(0.151)	(0.151)	(0.144)	(0.154)	(0.154)	(0.148)
Parents' highest educational level in years			0.166***			0.167***			0.163***
			(0.013)			(0.014)			(0.014)
Constant	11.576***	11.513***	11.004***	11.484***	11.449***	10.907***	11.674***	11.600***	11.111***
	(0.115)	(0.179)	(0.119)	(0.114)	(0.183)	(0.120)	(0.117)	(0.183)	(0.121)
Controls for unbalanced household characteristics[a]	No	Yes	No	No	Yes	No	No	Yes	No
Probability value for controls[b]	–	0.002	–	–	0.006	–	–	0.194	–
Obs.	15,502	15,502	15,502	13,415	13,415	13,415	13,801	13,801	13,801
R^2 (overall)	0.048	0.050	0.069	0.050	0.051	0.070	0.045	0.045	0.064
Panel B: impact after 1 year									
PROGRESA effect	0.299**	0.299**	0.298*	0.343**	0.342**	0.344**	0.215	0.217	0.215
	(0.152)	(0.152)	(0.153)	(0.159)	(0.159)	(0.160)	(0.156)	(0.156)	(0.156)
Time dummy	0.677***	0.751***	0.668***	0.768***	0.753***	0.748***	0.644***	0.719***	0.636***
	(0.119)	(0.212)	(0.119)	(0.123)	(0.237)	(0.124)	(0.123)	(0.241)	(0.124)
Treatment dummy	−0.146	−0.149	−0.158	−0.160	−0.163	−0.172	−0.123	−0.126	−0.135
	(0.151)	(0.151)	(0.145)	(0.151)	(0.151)	(0.145)	(0.154)	(0.154)	(0.148)
Parents' highest educational level in years			0.155***			0.155***			0.156***
			(0.014)			(0.015)			(0.014)
Constant	11.576***	11.515***	11.040***	11.484***	11.449***	10.949***	11.674***	11.600***	11.137***
	(0.115)	(0.179)	(0.120)	(0.114)	(0.183)	(0.122)	(0.117)	(0.183)	(0.122)
Controls for unbalanced household characteristics[a]	No	Yes	No	No	Yes	No	No	Yes	No
Probability value for controls[b]	–	0.164	–	–	0.558	–	–	0.119	–
Obs.	15,137	15,137	15,137	13,324	13,324	13,324	13,641	13,641	13,641
R^2 (overall)	0.022	0.023	0.041	0.026	0.026	0.044	0.016	0.017	0.035

Note: Robust standard errors clustered at the village level in parenthesis.

[a] Age of the head of the household, age of the head of the household interacted with a time dummy, proportion of male children, and proportion of male children interacted with a time dummy. Regressions also include an indicator for whether the age of the head of the household or the proportion of children were unavailable, these households are assigned the cross-sectional mean sample values of the variables.

[b] Probability value of joint *F* test for exclusion of all control variables.

* Each individual coefficient is statistically significant at the 10% level.

** Each individual coefficient is statistically significant at the 5% level.

*** Each individual coefficient is statistically significant at the 1% level.

personnel. This behavior would be consistent with (Jensen, 2012) who finds that making employment opportunities for women more salient to parents increases human capital investments in their daughters.

PROGRESA's impact of about a third of a school year on the educational aspirations parents have for their children is not trivial. In columns (3), (6), and (9) we include the parents' highest educational level in years. The magnitude of the coefficients indicates that, *ceteris paribus*, PROGRESA's effect on aspirations for all children is comparable to that associated with parents having two extra years of schooling (considering column (3): 0.338/0.166 = 2.04). As the average education of adults in our sample is about three years (as shown in Table 1), this is quite relevant.

As it can be seen in Table 1 and by the sign of the treatment dummies in Table 2, parental aspirations before the start of PROGRESA were lower for the treatment group than the control group. Thus, one might worry that the differences-in-differences results presented are statistically different from zero due to this initial difference in aspirations between treatment and control groups. However, we have evidence we interpret as suggestive that this might not be the case. In Table 3, columns (1), (5), and (9), we present the results of regressions of parental aspirations six months (Panel A) and one year (Panel B) after the start of the program on a treatment dummy, parental aspirations at baseline, and the same controls used in Table 2. While smaller in magnitude and less precisely estimated, the coefficients, in general, seem to be consistent with the results shown in Table 2. For example, when considering all children after six months, PROGRESA's impact is 0.201 (significant at the 10% level) whereas the corresponding differences-in-differences coefficient shown in Table 2 is 0.338 (significant at the 5% level).

Hence, overall, the evidence seems to suggest that PROGRESA has increased parental aspirations for their children's education. Given the program's emphasis on education, this is a positive/encouraging result. Our finding is in line with results by Schultz (2004) showing

Table 3
Cross-section estimates.

	All children				Daughters				Sons			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: impact after 6 months												
Exposure effect (Treatment × Exposure)		0.331**	0.334**	−0.063		0.455***	0.454***	−0.174		0.274*	0.277*	−0.123
		(0.133)	(0.134)	(0.174)		(0.164)	(0.164)	(0.226)		(0.154)	(0.154)	(0.205)
Exposure		−0.227**	−0.022	0.012		−0.328**	−0.075	0.008		−0.173	−0.005	0.005
		(0.110)	(0.137)	(0.137)		(0.136)	(0.171)	(0.182)		(0.128)	(0.161)	(0.161)
Treatment	0.201*	−0.007	−0.012	0.201	0.227*	−0.052	−0.054	0.293*	0.146	−0.026	−0.031	0.179
	(0.119)	(0.126)	(0.126)	(0.127)	(0.127)	(0.146)	(0.145)	(0.150)	(0.123)	(0.136)	(0.136)	(0.146)
Age effect			0.047***				0.061***				0.040**	
			(0.016)				(0.021)				(0.020)	
Constant	11.008***	9.960***	9.672***	10.812***	10.940***	10.083***	9.717***	10.271***	11.309***	10.013***	9.768***	11.363***
	(0.222)	(0.289)	(0.310)	(0.380)	(0.252)	(0.348)	(0.369)	(0.442)	(0.253)	(0.332)	(0.364)	(0.454)
Controls[a]	*Yes*	Yes	Yes	Yes	*Yes*	Yes	Yes	Yes	*Yes*	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.000		0.000	0.000	0.000		0.000	0.000	0.000	
Obs.	7396	7394	7394	4257	5369	5367	5367	2732	5744	5743	5743	2896
R^2 (overall)	0.02	0.051	0.052	0.044	0.019	0.050	0.051	0.053	0.016	0.048	0.048	0.045
Panel B: impact after 1 year												
Exposure effect (Treatment × Exposure)		−0.216	−0.218	−0.159		−0.233	−0.237	0.091		−0.265*	−0.263*	−0.182
		(0.135)	(0.134)	(0.180)		(0.167)	(0.167)	(0.215)		(0.159)	(0.159)	(0.219)
Exposure		0.117	0.147	−0.055		0.124	0.154	−0.250		0.162	0.201	−0.081
		(0.114)	(0.126)	(0.153)		(0.142)	(0.154)	(0.187)		(0.138)	(0.153)	(0.174)
Treatment	0.171	0.288**	0.291**	0.049	0.209*	0.338**	0.342**	−0.101	0.107	0.250*	0.247*	0.147
	(0.109)	(0.123)	(0.123)	(0.124)	(0.116)	(0.143)	(0.143)	(0.134)	(0.112)	(0.132)	(0.131)	(0.141)
Age effect			0.007				0.007				0.010	
			(0.015)				(0.018)				(0.019)	
Constant	10.865***	10.089***	10.060***	10.774***	10.654***	9.908***	9.873***	10.629***	11.091***	10.330***	10.301***	10.600***
	(0.234)	(0.319)	(0.333)	(0.345)	(0.255)	(0.357)	(0.378)	(0.395)	(0.267)	(0.345)	(0.366)	(0.408)
Controls[a]	*Yes*	Yes	Yes	Yes	*Yes*	Yes	Yes	Yes	*Yes*	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.000		0.000	0.000	0.000		0.000	0.000	0.000	
Obs.	7031	7029	7029	3825	5278	5276	5276	2582	5581	5580	5580	2718
R^2 (overall)	0.020	0.047	0.048	0.056	0.025	0.052	0.052	0.065	0.017	0.041	0.042	0.062

Note: Robust standard errors clustered at the village level in parenthesis.

[a] For columns (1), (5), and (9) controls are: parental aspirations, head's age, and proportion of male children at baseline. For columns (2), (4), (6), (8), (10), and (12) the controls are parental aspirations, head and spouse's age, head and spouse's schooling, head and spouse's literacy, head and spouse's indigenous status, number of male and female adults, number of male and female children, and household's monthly income at baseline. For columns (3), (7), and (11) the controls are the same as for the latter columns and also include the age of the youngest child. All columns also include an indicator for whether individual controls were unavailable, these households are assigned the cross-sectional mean sample values of the variables.

[b] Probability value of joint *F* test for exclusion of all control variables.

* Each individual coefficient is statistically significant at the 10% level.

** Each individual coefficient is statistically significant at the 5% level.

*** Each individual coefficient is statistically significant at the 1% level.

that PROGRESA's cumulative effect on child schooling attainment is of 0.66 years.

Although this is an interesting result, it does not enable us to pinpoint the driving force behind the change. In fact, given the design of a conditional cash transfer program like PROGRESA, there could be many channels causing an increase in the educational aspirations of the poor. In the following subsection, we explore one possible channel: mandated exposure to doctors and nurses.

4.2. The effect of differential exposure to educated professionals on aspirations

Exploiting the design of PROGRESA, we divide the sample into two groups with different levels of mandated exposure to nurses and doctors. We consider high-exposure households to be those whose youngest child is less than five years of age and that must go to the health clinics at least four times per year. Furthermore, we consider low-exposure households to be those whose youngest child is five or older and that are required to attend health clinics only once or twice per year.[21]

Thus, we divide our sample based strictly on the requirements of the program. Nevertheless, households may need or may choose to visit the health clinics more often than what PROGRESA's requirements mandate. Ideally, we would like to know how the actual number of times each type of household (high- and low-exposure) visited the health centers changes over time. Unfortunately, our data does not allow us to analyze this. Indeed, data on the number of household visits to the health centers is only available in the fourth survey round. This enables us to see whether there are any differences in health clinic consultations between high- and low-exposure households, only in the four weeks prior to this survey round. Considering the treatment group, the average number of visits for high-exposure households is 0.14, while for the low-exposure households is 0.10. The difference is statistically significant (t-stat = 1.99). Considering the control group, the average number of visits for high-exposure households is 0.05, while for the low-exposure households is 0.02. The difference is statistically significant (t-stat = 3.33). Hence, the data on the fourth survey round suggests that actual visits to the health centers (and presumably) exposure to doctors and nurses is higher for high-exposure households than for low-exposure ones.

To identify the effect of differential exposure to educated professionals on parents' aspirations for their children's education, we need to control for any systematic variation to the aspirations of households with high exposure in the treatment villages that are correlated with, but not due to, the introduction of PROGRESA. We do this using a "differences-in-differences-in-differences" (or triple difference) estimator as in Gruber (1994). We run the following regression:

$$\begin{aligned} ASP_{ivt} = {} & \beta_0 + \beta_1 X_{ivt} + \beta_2 \tau_t + \beta_3 T_v + \beta_4 EX_i + \beta_5(\tau_t \times T_v) \\ & + \beta_6(\tau_t \times EX_i) + \beta_7(T_v \times EX_i) \\ & + \beta_8(\tau_t \times T_v \times EX_i) + \xi_{ivt} \end{aligned} \quad (2)$$

where ASP_{ivt} denotes the educational aspirations of the parents of household i in village v at time t; X_{ivt} includes household's monthly income and age of parents; τ_t is a time dummy; and T_v is a village dummy that equals one for households in treatment villages; EX_i is a dummy that equals one for households with high exposure to health professionals; and ξ_{ivt} is an idiosyncratic error term. The fixed effects control for the time-series changes in aspirations (β_2), the time-invariant characteristics of the treatment villages (β_3), and the time-invariant characteristics of the high-exposure households (β_4). The second-level interactions control for changes over time in the treatment villages (β_5), changes over time for the high-exposure households (β_6), and time-invariant characteristics of the high-exposure households in the treatment villages (β_7).

The third-level interaction, β_8, is the coefficient of interest. It captures all variation in aspirations specific to the high-exposure households (relative to the low-exposure households) in the treatment villages (relative to the control villages) six months or one year after the introduction of PROGRESA (relative to before the introduction of PROGRESA).

As Gruber (1994) points out, one identifying assumption of this triple difference estimator is that there are no contemporaneous shocks that affect the aspirations of the high-exposure households relative to the low-exposure households in the same village-time at the start of PROGRESA. Another identifying assumption is that high-exposure households would not otherwise have changed differently over time than low-exposure households. This is a stronger assumption than the previous one. In Table 4, we show descriptive statistics of households in both treatment and control groups by level of mandated exposure at baseline. High-exposure households (i.e., whose youngest child is less than five) are, on average, "younger" than low-exposure households (i.e., whose youngest child is five or older). Also, high-exposure households have fewer adults and more children than low-exposure households.[22] Thus, it might be the case that households with different characteristics will respond differentially over time to exposure to health professionals. In order to account for any possible differential trends in the outcomes by these factors, we also include in the regressions interactions between a year indicator and household's monthly income, parents' age, education, literacy, whether parents are indigenous, number of male and female adults, and number of male and

[21] We consider only these two categories because we did not find any differential effect of exposure between households with children less than two years of age and households with children 2–5. As explained later in this section, this might be due to the fact that both types of households have already had high exposure to doctors and nurses six months after the start of the program. Results are available upon request. Furthermore, since the aspirations questions in rounds three and four were asked only for children 6–16 years old, we cannot compare households with children less than seventeen years of age and households with no children less than seventeen years of age.

[22] The last two columns of Table 4 show that high- and low-exposure households are similar across treatment and control villages.

female children.[23] In this way we are able to control not only for differences in observables between high- and low-exposure households, but also for differential trends based on initial values of the variables listed above.

It is important to note that, as shown in Table 4, treatment households with *high* exposure receive, on average, *lower* cash transfers than do treatment households with low exposure, and this difference is statistically significant.[24] The difference in the amount of cash received is explained by the fact that households whose youngest child is less than five have younger children. This implies that these households have fewer children of school age who would be eligible to receive the educational cash transfers, which constitute the largest component of PROGRESA's transfers. Despite these differences, the monthly income (not including transfers) of high- and low-exposure households is *not* very different, and the null that the monthly income of high- and low-exposure households is the same cannot be rejected. Thus, the marginal utility of the cash transfers may be higher for low-exposure households.

Finally, the last section of Table 4 contains data on aspirations. At baseline low-exposure households have higher parental aspirations than high-exposure households. This holds both for treatment and control groups. However, the last two columns of Table 4 show that there are no statistically significant differences in the differences between high- and low-exposure households in treatment and control villages.

Table 5 shows the estimates of regression (2) six months (Panel A) and one year (Panel B) after the start of PROGRESA. The first row of Table 5, Panel A, presents the estimates of the third-level interaction, the effect of exposure six months after the start of PROGRESA, β_8, whereas the second row presents the estimates of the PROGRESA effect for low-exposure households, β_5. Results in columns (1), (4), and (7), correspond to a version of regression (2) without controls, for all children, daughters and sons, respectively. For all children, average aspirations of high-exposure households relative to low-exposure households in treatment villages relative to control villages are 0.4 school years higher six months after the start of the program. This difference is statistically significant and seems to suggest that aspirations are not driven by a "PROGRESA effect" but by exposure to highly educated professionals given that low-exposure households receive, on average, more cash from PROGRESA's transfers than do high-exposure households. Introducing the set of controls specified above—with which we control not only for differences in observables between high- and low-exposure households, but also for differential trends based on the initial values of the variables—(columns (2), (5) and (8)) does not have a sizeable impact on either the exposure effect coefficient or the coefficient denoting the PROGRESA effect on low-exposure households. Still, a valid concern with our results is that there can be time-varying unobservables that are correlated with our outcome variable. However, the fact that the exposure and PROGRESA's coefficients are not sensitive to the introduction of our controls is reassuring and we interpret it as suggestive evidence that the effects of any omitted variable in our regression should not be large.

In columns (3), (6), and (9) we control for the parents' highest educational level in years. For all children, the magnitude of the coefficients indicates that, *ceteris paribus*, being exposed to educated professionals leads to the same increase in aspirations for children as would be associated with parents who had two extra years of schooling (0.430/0.176 = 2.4). Thus, exposure to educated professionals seems to have almost the same effect on aspirations as average parental education (three years).

Hence, six months after the start of the program, differential exposure seems to play some role since there is no statistically significant effect of PROGRESA per se. Considering daughters only, differential exposure seems to increase aspirations by half of a school year. For the case of sons, differential exposure seems to increase aspirations by a third of a school year.

One year after the start of the program, however, the coefficients measuring the exposure effect are close to zero and not statistically significant for all children, daughters and sons (Panel B of Table 5). On the other hand, we do observe a positive and statistically significant effect of PROGRESA for all children and daughters of a third of a school year. Thus, the data seems to suggest that one year after the start of the program the aspirations of parents from low-exposure households might be catching up with those of parents from high-exposure households. This evidence would be consistent with the hypothesis that it is the amount of exposure (i.e., the number of meetings) that affects aspirations and not the frequency of these meetings. Indeed, psychology studies show that only after a certain amount of exposure do outstanding individuals become relevant to others and can induce changes in them (Lockwood & Kunda, 1997; Major et al., 1993).

These latter findings also seem, in general, to be robust to the inclusion of our controls in the regression specification (columns (2), (5) and (8)). Additionally, Table 3, columns (2), (6), and (10), presents the estimates from running cross-sectional regressions six months (Panel A) and one year (Panel B) after the start of the program, considering only follow-up survey rounds. In this case the coefficients are admittedly smaller in magnitude (up to 0.09 years of schooling) but consistent with the results shown in Table 5.

A valid concern is whether PROGRESA, in its effort to make parents send their children to school, made the

[23] Data on the age of the head's spouse is missing for 2048 observations; on the education of the head for 28; on the education of the spouse for 758; on the literacy of the head for 6; on the literacy of the spouse for 747; on the head's indigenous status for 10; on the spouse's indigenous status for 753. There are no statistical differences in the number of missing values between treatment and control groups. We replace the missing values by the cross-sectional mean sample values of the variables, and include a dummy for these observations in the regressions. Running the regressions without these observations or without spouse's controls does not change the results.

[24] The cash transfers (educational and health components) presented in Table 4 are calculated considering the household's demographic structure and assuming that each household complies with all of PROGRESA's requirements. Considering this measure, *high*-exposure households receive, on average, 32 pesos *less* per month than do low-exposure households.

Table 4
Descriptive statistics by treatment status and household composition, fixing household structure as of baseline (1997).

	Treatment				Control				DD	*t*-Stat
	Obs.	Mean		*t*-Stat	Obs.	Mean		*t*-Stat		
		Low exposure HHs	High exposure HHs			Low exposure HHs	High exposure HHs			
(a) Characteristics of the head of the household										
Age	5053	46.51	39.07	19.52***	3036	46.53	40.01	15.03***	0.93	1.61
Educational level in years	5044	2.36	3.16	−10.29***	3034	2.36	3.02	−7.09***	−0.13	−1.11
Literate	5062	0.67	0.74	−5.27***	3038	0.67	0.73	−3.69***	−0.01	−0.47
Indigenous	5060	0.39	0.43	−1.72*	3036	0.41	0.42	−0.46	−0.02	−0.80
(b) Characteristics of the spouse of the household head										
Age	4615	41.70	34.20	21.90***	2746	41.06	34.64	16.88***	1.10	2.14**
Educational level in years	4606	2.26	2.84	−6.60***	2742	2.20	2.85	−6.71***	0.07	0.54
Literate	4616	0.60	0.65	−2.59***	2743	0.59	0.63	−2.47**	0.00	0.06
Indigenous	4611	0.38	0.42	−2.21**	2742	0.41	0.41	0.01	−0.04	−1.48
(c) Characteristics of the household										
Mean age of adults	5064	39.50	34.37	19.21***	3040	39.53	34.85	14.30***	0.45	1.06
Mean educational level of adults	5063	3.02	3.36	−5.30***	3040	2.92	3.30	−5.07***	0.04	0.36
Proportion of literate adults	5063	0.68	0.72	−3.47***	3040	0.67	0.71	−3.14***	0.00	0.07
Proportion of indigenous adults	5060	0.38	0.42	−2.21**	3035	0.40	0.41	−0.21	−0.04	−1.31
Monthly income	5065	932.44	917.81	0.43	3041	945.76	946.18	−0.01	15.05	0.29
Monthly transfers received in round 2	4918	340.78	308.70	6.19***	2917	0.00	0.00	.	32.08	6.19***
Monthly transfers received in round 3	4667	362.40	330.82	5.50***	2769	0.00	0.00	.	31.57	5.50***
(d) Household structure										
Size	5065	5.79	7.26	−20.04***	3041	5.83	7.28	−16.90***	−0.01	−0.12
Number of adults	5065	2.76	2.64	3.26***	3041	2.74	2.64	2.12**	0.02	0.32
Number of female adults	5065	1.40	1.36	1.96*	3041	1.39	1.38	0.46	0.03	0.92
Number of male adults	5065	1.35	1.28	2.85***	3041	1.35	1.26	2.79***	−0.02	−0.45
Proportion of male adults	5063	0.48	0.48	−0.65	3039	0.48	0.47	1.33	−0.01	−1.44
Number of children	5065	3.03	4.61	−27.73***	3041	3.08	4.63	−26.01***	−0.03	−0.36
Number of female children	5065	1.41	2.26	−21.33***	3041	1.51	2.30	−15.62***	−0.05	−0.81
Number of male children	5065	1.61	2.35	−18.55***	3041	1.57	2.32	−15.19***	0.01	0.24
Proportion of male children	5040	0.55	0.51	4.09***	3029	0.51	0.50	0.77	0.02	1.60
Birth spacing between children										
Between 1st and 2nd child	4585	3.37	3.17	2.04**	2741	3.63	3.20	3.26***	−0.24	−1.45
Between 2nd and 3rd child	4049	2.89	2.91	−0.31	2374	2.79	2.93	−1.54	0.12	1.08
Between 3rd and 4th child	3068	2.63	2.86	−3.62***	1816	2.63	2.87	−3.27***	0.01	0.13
Between 4th and 5th child	2014	2.50	2.68	−2.39**	1226	2.45	2.80	−3.90***	0.17	1.43
Between 5th and 6th child	1227	2.27	2.61	−3.65***	726	2.37	2.69	−2.96***	−0.02	−0.13
Between 6th and 7th child	638	2.24	2.40	−1.51	376	2.43	2.43	0.04	−0.17	−0.89
Between 7th and 8th child	304	1.97	2.39	−2.28**	163	2.30	2.31	−0.05	−0.41	−1.70*
Between 8th and 9th child	125	2.00	2.13	−0.27	59	2.22	2.18	0.09	−0.17	−0.25
Between 9th and 10th child	65	2.00	1.95	0.41	29	1.00	1.85	−1.60	0.89	1.67*
(e) Aspirations										
Parental aspirations for all children	5065	11.78	11.25	5.58***	3041	11.77	11.46	2.72***	0.23	1.54
Parental aspirations for daughters	5024	11.66	11.14	5.43***	3014	11.67	11.38	2.58**	0.23	1.54
Parental aspirations for sons	5025	11.90	11.37	5.3***	3019	11.89	11.55	2.83***	0.20	1.28

Note: *t*-Statistics of difference in means computed clustering at the village level.
* Differences significant at the 10% level.
** Differences significant at the 5% level.
*** Differences significant at the 1% level.

reporting of high educational aspirations a socially desirable behavior for beneficiary parents. Our results do not seem to support this. In fact, if this were to be the case, low- and high-exposure beneficiary households would change their answers after the start of the program. Another concern is whether the force behind the change in aspirations is an income effect due to the transfers. This does not seem to be the case either, as households that received higher transfer amounts (those with older children) should be the ones reporting higher aspirations. A final concern is whether parents were changing their aspirations based on a correctly estimated present-discounted value of schooling. However, if this were to be the case, then the education-conditional cash transfer would have been greater for low-exposure households. Indeed, as the benefits of the program were granted by the government for a three year period only, low-exposure households had a greater incentive to overstate their educational aspirations, given that they would have gained the most in terms of the education-conditional cash transfers. In fact, had the three years promise applied, by the time the children of high-exposure households had reached the grade to start

Table 5
Triple differences estimates.

	All children			Daughters			Sons		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: impact after 6 months									
Exposure effect (Time × Treatment × Exposure)	0.422**	0.424**	0.430**	0.532**	0.540**	0.555**	0.361*	0.362*	0.357*
	(0.195)	(0.194)	(0.195)	(0.227)	(0.226)	(0.226)	(0.208)	(0.209)	(0.208)
PROGRESA effect (Time × Treatment)	0.090	0.075	0.085	0.065	0.047	0.050	0.056	0.051	0.068
	(0.187)	(0.193)	(0.187)	(0.207)	(0.211)	(0.207)	(0.201)	(0.206)	(0.200)
Parents' highest educational level in years			0.176***			0.178***			0.174***
			(0.013)			(0.014)			(0.014)
Constant	11.757***	12.052***	11.215***	11.663***	11.951***	11.113***	11.868***	12.186***	11.332***
	(0.133)	(0.211)	(0.134)	(0.132)	(0.214)	(0.136)	(0.134)	(0.215)	(0.136)
Controls for observable characteristics[a]	No	Yes	No	No	Yes	No	No	Yes	No
Probability value for controls[b]	–	0.000	–	–	0.000	–	–	0.000	–
Obs.	15,227	15,227	15,227	13,143	13,143	13,143	13,527	13,527	13,527
R^2 (overall)	0.051	0.067	0.073	0.053	0.066	0.075	0.047	0.060	0.068
Panel B: impact after 1 year									
Exposure effect (Time × Treatment × Exposure)	−0.028	−0.032	−0.026	−0.064	−0.089	−0.069	−0.061	−0.075	−0.063
	(0.193)	(0.195)	(0.193)	(0.210)	(0.211)	(0.210)	(0.216)	(0.218)	(0.217)
PROGRESA effect	0.309*	0.307	0.310*	0.379*	0.383*	0.384*	0.244	0.243	0.248
	(0.187)	(0.194)	(0.187)	(0.200)	(0.207)	(0.200)	(0.197)	(0.203)	(0.197)
Parents' highest educational level in years			0.163***			0.162***			0.165***
			(0.014)			(0.015)			(0.015)
Constant	11.663***	11.928***	11.159***	11.579***	11.824***	11.076***	11.764***	12.065***	11.255***
	(0.135)	(0.217)	(0.138)	(0.135)	(0.222)	(0.141)	(0.136)	(0.221)	(0.139)
Controls for observable characteristics[a]	No	Yes	No	No	Yes	No	No	Yes	No
Probability value for controls[b]	–	0.328	–	–	0.389	–	–	0.184	–
Obs.	14,463	14,463	14,463	12,656	12,656	12,656	12,971	12,971	12,971
R^2 (overall)	0.024	0.039	0.045	0.028	0.040	0.047	0.018	0.030	0.039

Note: Robust standard errors clustered at the village level in parenthesis.

[a] The controls are: head and spouse's age, head and spouse's age interacted with a time dummy, head and spouse's schooling interacted with a time dummy, head and spouse's literacy interacted with a time dummy, head and spouse's indigenous status interacted with a time dummy, number of male and female adults interacted with a time dummy, number of male and female children interacted with a time dummy, household's monthly income, and household's monthly income interacted with a time dummy. Regressions also include an indicator for whether data on parental characteristics were unavailable. These households are assigned the cross-sectional mean sample values of the variables.

[b] Probability value of joint F test for exclusion of all control variables.

* Each individual coefficient is statistically significant at the 10% level.

** Each individual coefficient is statistically significant at the 5% level.

*** Each individual coefficient is statistically significant at the 1% level.

receiving the transfer, the program would have already been discontinued. Only in the case in which there had been 100% certainty that the program would have been continued for a longer time period, high-exposure households would have had an incentive to overstate their aspirations. This last case seems highly unlikely as, at that time in Mexico, social programs used to disappear as soon as there was a change in administration (Barajas, 2002; Levy & Rodríguez, 2005; Lustig, 2011).

In summary, results seem to suggest that mandated exposure to nurses and doctors has a positive effect on parental aspirations. This is consistent with the evidence by Adato et al. (2000) showing that the programmed visits caused communication and bonding between the health personnel and the beneficiaries, contributing to changing people's thinking. Furthermore, the evidence appears to be consistent with the hypothesis that it might be the amount and not the frequency of exposure that drives the change in parental aspirations. This hypothesis is in line with studies in psychology which find that individuals who interact more with people from higher status reference groups develop higher aspiration levels than those who interact less with these groups (Bell, 1963; Lockwood & Kunda, 1997; Major et al., 1993).

4.3. Alternative aspiration outcomes

An increase of 0.3–0.5 years of schooling may be difficult to interpret. Thus, instead of converting the data on aspirations into years of schooling, we created the variable "at least college," a dummy that equals one if the respondent aspires to at least 16 years of schooling for her children.[25] Working with this variable allows us to see what proportion of households changed their responses as a result of differential mandated exposure to nurses and doctors. These results, in turn, may be easier to interpret than the changes in aspirations in years of schooling, and

[25] We do not consider the variable "at least primary education" and "at least secondary education" because more than 99% and 90% of the respondents declared that they wanted their children to at least finish their primary and secondary education, respectively. Given that the proportions were already high, the introduction of PROGRESA did not have any sizeable effect on modifying them. Also, we do not consider the results for the variable "at least technical school" because the proportion of households that aspired for their children to complete at least a technical degree was low (less than 13%) and was not affected by PROGRESA. Finally, results for "at least high school" are similar to the ones reported below on "at least college" and are available upon request.

Table 6
Differences-in-differences estimates (at least college).

	All children		Daughters		Sons	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: impact after 6 months						
PROGRESA effect (Time × Treatment)	0.040*	0.040*	0.046*	0.046*	0.033	0.034
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Time dummy	0.117***	0.067*	0.123***	0.063*	0.133***	0.080**
	(0.019)	(0.035)	(0.019)	(0.037)	(0.019)	(0.040)
Treatment dummy	−0.017	−0.017	−0.020	−0.021	−0.016	−0.017
	(0.019)	(0.020)	(0.018)	(0.018)	(0.019)	(0.019)
Constant	0.281***	0.280***	0.248***	0.257***	0.273***	0.277***
	(0.015)	(0.024)	(0.014)	(0.023)	(0.015)	(0.024)
Controls for unbalanced household characteristics[a]	No	Yes	No	Yes	No	Yes
Probability value for controls[b]	–	0.003	–	0.129	–	0.103
Obs.	15,438	15,438	13,411	13,411	13,799	13,799
R^2 (overall)	0.023	0.024	0.027	0.028	0.026	0.027
Panel B: impact after 1 year						
PROGRESA effect	0.050**	0.050**	0.057**	0.058**	0.038	0.039
	(0.024)	(0.024)	(0.024)	(0.024)	(0.025)	(0.025)
Time dummy	−0.001	0.002	0.021	0.002	0.011	0.003
	(0.019)	(0.035)	(0.019)	(0.040)	(0.020)	(0.038)
Treatment dummy	−0.017	−0.017	−0.020	−0.021	−0.016	−0.017
	(0.019)	(0.020)	(0.018)	(0.018)	(0.019)	(0.019)
Constant	0.281***	0.280***	0.248***	0.257***	0.273***	0.277***
	(0.015)	(0.024)	(0.014)	(0.023)	(0.015)	(0.024)
Controls for unbalanced household characteristics[a]	No	Yes	No	Yes	No	Yes
Probability value for controls[b]	–	0.380	–	0.652	–	0.662
Obs.	15,137	15,137	13,320	13,320	13,639	13,639
R^2 (overall)	0.002	0.002	0.005	0.005	0.002	0.002

Note: Robust standard errors clustered at the village level in parenthesis.

[a] Age of the head of the household, age of the head of the household interacted with a time dummy, proportion of male children, and proportion of male children interacted with a time dummy. Regressions also include an indicator for whether the age of the head of the household or the proportion of children were unavailable, these households are assigned the cross-sectional mean sample values of the variables.

[b] Probability value of joint *F* test for exclusion of all control variables.

* Each individual coefficient is statistically significant at the 10% level.

** Each individual coefficient is statistically significant at the 5% level.

*** Each individual coefficient is statistically significant at the 1% level.

may help us understand what is driving the increases of 0.3–0.5 years of schooling.

We first consider the effect of PROGRESA on raising the proportion of parents who would like their children to complete at least college. Table 6, column (1), shows that, at baseline, more than 28% of the households want their children to at least complete college. The program seems to increase by 14% ((0.040/0.281) × 100) and 18% ((0.050/0.281) × 100) the proportion of parents who aspire for their children to finish college six months and a year after the start of the program, respectively. Looking separately at daughters and sons, the program seems to increase by 19% ((0.046/0.248) × 100) and 23% ((0.057/0.248) × 100) the proportion of parents who aspire for their daughters to finish college six months and a year after the start of the program, respectively; the effects are smaller (12% and 14%) for sons and not statistically significant.

We now consider the effect of exposure to educated professionals on raising the proportion of parents who would like their children to complete at least college. Table 7 shows the results. Six months after the start of the program, we see a differential impact on parental aspirations. As summarized by the triple difference estimator, 6% more of high-exposure households that received PROGRESA declared that they wanted their children to at least finish college. This coefficient is significant at the 10% level. This 6% increase corresponds to a 20% ((0.059/0.294) × 100) increase in the proportion of parents who aspire for their children to finish college. When looking separately at daughters and sons, results are similar, and the triple difference estimator shows a statistically significant increase (at the 10% level) of 6% for both daughters and sons. This effect corresponds to a 23% (22%) increase in the proportion of parents who aspire for their daughters (sons) to finish high school. One year after the start of the program, we see, again, what might be interepreted as a catch up effect, as the triple difference estimator decreases in magnitude and becomes not statistically significant.

Thus, it seems to be the case that the increase in parental aspirations of 0.4–0.5 schooling years, due to exposure to educated professionals, is the result of a 20% increase in the proportion of households that aspire to see their children finishing college. These results are, in general, robust to the inclusion of controls in the regression (columns (2), (4), and (6)). When we include our controls in the regression for all children, the PROGRESA effect is no longer significant at the 10% level.

Table 7
Triple differences estimates (at least college).

	All children		Daughters		Sons	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: impact after 6 months						
Exposure effect (Time × Treatment × Exposure)	0.059*	0.061*	0.061*	0.064*	0.063*	0.065*
	(0.032)	(0.032)	(0.035)	(0.035)	(0.035)	(0.034)
PROGRESA effect (Time × Treatment)	0.004	0.001	0.009	0.006	−0.004	−0.007
	(0.030)	(0.030)	(0.032)	(0.032)	(0.031)	(0.031)
Constant	0.294***	0.327***	0.261***	0.303***	0.285***	0.323***
	(0.018)	(0.030)	(0.017)	(0.029)	(0.018)	(0.030)
Controls for observable characteristics[a]	No	Yes	No	Yes	No	Yes
Probability value for controls[b]	–	0.000	–	0.000	–	0.000
Obs.	15,163	15,163	13,139	13,139	13,525	13,525
R^2 (overall)	0.024	0.038	0.028	0.039	0.027	0.040
Panel B: impact after 1 year						
Exposure effect (Time × Treatment × Exposure)	0.000	0.001	−0.002	−0.001	−0.005	−0.006
	(0.032)	(0.032)	(0.035)	(0.035)	(0.035)	(0.035)
PROGRESA effect	0.048*	0.046	0.058*	0.057*	0.039	0.037
	(0.029)	(0.029)	(0.031)	(0.031)	(0.030)	(0.030)
Constant	0.279***	0.313***	0.248***	0.287***	0.267***	0.305***
	(0.018)	(0.03)	(0.017)	(0.028)	(0.017)	(0.029)
Controls for observable characteristics[a]	No	Yes	No	Yes	No	Yes
Probability value for controls[b]	–	0.036	–	0.070	–	0.037
Obs.	14,463	14,463	12,652	12,652	12,969	12,969
R^2 (overall)	0.003	0.014	0.006	0.016	0.003	0.013

Note: Robust standard errors clustered at the village level in parenthesis.

[a] The controls are: head and spouse's age, head and spouse's age interacted with a time dummy, head and spouse's schooling interacted with a time dummy, head and spouse's literacy interacted with a time dummy, head and spouse's indigenous status interacted with a time dummy, number of male and female adults interacted with a time dummy, number of male and female children interacted with a time dummy, household's monthly income, and household's monthly income interacted with a time dummy. Regressions also include an indicator for whether data on parental characteristics were unavailable. These households are assigned the cross-sectional mean sample values of the variables.

[b] Probability value of joint F test for exclusion of all control variables.

* Each individual coefficient is statistically significant at the 10% level.

*** Each individual coefficient is statistically significant at the 1% level.

5. Robustness checks

5.1. Age effect

In the second part of the paper, our analytical strategy takes advantage of the mandated visits to the health centers—which differ by age of the youngest child—to identify the effect of exposure to doctors and nurses on parental aspirations. Hence, it is important to know how educational aspirations differ within households as children age, or that is, they mature. It is possible that the older the children, the higher the educational aspirations may be. However, it is also feasible that as children grow, parents learn more about their children's abilities, and are more realistic on future options for them (or at least they believe they are), and therefore, they are more likely to adjust their aspirations downward.

In Table 8 we report aspirations by age of the youngest child for the entire sample, and by treatment and control groups, separately. We use the age of the youngest child for two reasons. First, we need a measure at the household level because our aspirations measure is built at the household level. Second, we need to be consistent about the way in which we construct the exposure dummy, which divides households into low- and high-exposure based on the age of the youngest child. Regression analysis shows that, indeed, aspirations are non-decreasing in age (results not shown). Thus, it does not seem that as children grow, parents adjust their aspirations downward.[26]

In any case, Table 9 reports regressions with controls similar to those in Table 5. The difference is that, in Table 9, we check whether parental aspirations about their children's education depend on their children's age. Hence, in order to control for a possible age component, we add as additional regressor the age of the youngest child, and the age of the youngest child interacted with τ_t, a time dummy.[27] Table 9 shows that the results do not change once we control for a possible age component. The age component is, as expected, positive and statistically significant, signaling that the older the child, the greater the educational aspirations of the parents.[28] Moreover, running the regressions in simple differences (Table 3, columns (3), (7), and (11)) results are smaller in magnitude, but appear to be consistent to the ones shown in Table 9.

[26] Analyzing aspirations by age of the youngest for girls and boys separately gives similar results.

[27] This analysis is akin to a regression discontinuity design using the age of the youngest as the running variable. Including a second and third order polynomial of the age of the youngest child in the regressions, results do not change.

[28] Regression results do not change when controls are not included.

Table 8
Aspirations by age of the youngest child and treatment status at baseline (1997).

Age of the youngest child	Obs.	Mean			T vs. C
		All	Treatment	Control	*t*-Stat
0+years old	1222	11.19	11.24	11.12	0.54
1 year old	1316	11.25	11.14	11.45	−1.35
2 years old	1131	11.30	11.13	11.60	−2.02**
3 years old	848	11.48	11.39	11.63	−1.02
4 years old	716	11.54	11.45	11.70	−0.91
5 years old	545	11.82	11.95	11.63	1.08
6 years old	506	11.85	11.92	11.75	0.56
7 years old	399	11.74	11.60	11.91	−0.93
8 years old	337	11.73	11.62	11.94	−0.92
9 years old	241	11.83	11.90	11.71	0.47
10 years old	255	11.60	11.62	11.58	0.09
11 years old	210	11.79	11.38	12.35	−2.14**
12 years old	148	11.50	11.66	11.23	0.80
13 years old	100	12.18	12.24	12.02	0.32
14 years old	58	12.14	12.08	12.25	−0.20
15 years old	28	10.96	11.63	9.56	1.42
16 years old	12	12.38	13.14	11.30	1.32

Note: *t*-Statistics of difference in means computed clustering at the village level.
** Differences significant at the 5% level.

Table 9
Age effect.

	All children	Daughters	Sons
Panel A: impact after 6 months			
Exposure effect (Time × Treatment × Exposure)	0.429**	0.542**	0.366*
	(0.194)	(0.225)	(0.208)
PROGRESA effect (Time × Treatment)	0.072	0.042	0.050
	(0.193)	(0.210)	(0.206)
Age effect	0.049***	0.048***	0.052***
	(0.017)	(0.018)	(0.018)
Constant	11.815***	11.719***	11.94***
	(0.229)	(0.234)	(0.234)
Controls for observable characteristics[a]	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.000
Obs.	15,227	13,143	13,527
R^2 (overall)	0.068	0.068	0.061
Panel B: impact after 1 year			
Exposure effect (Time × Treatment × Exposure)	−0.028	−0.084	−0.068
	(0.195)	(0.211)	(0.218)
PROGRESA effect (Time × Treatment)	0.305	0.381*	0.239
	(0.194)	(0.206)	(0.203)
Age effect	0.048***	0.048***	0.052***
	(0.016)	(0.016)	(0.017)
Constant	11.726***	11.625***	11.848***
	(0.231)	(0.237)	(0.235)
Controls for observable characteristics[a]	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.000
Obs.	14,463	12,656	12,971
R^2 (overall)	0.040	0.041	0.032

Note: Robust standard errors clustered at the village level in parenthesis.
[a] The controls are: age of the youngest child interacted with a time dummy, head and spouse's age, head and spouse's age interacted with a time dummy, head and spouse's schooling interacted with a time dummy, head and spouse's literacy interacted with a time dummy, head and spouse's indigenous status interacted with a time dummy, number of male and female adults interacted with a time dummy, number of male and female children interacted with a time dummy, household's monthly income, and household's monthly income interacted with a time dummy. Regressions also include an indicator for whether data on the age of the youngest child and parental characteristics were unavailable. These households are assigned the cross-sectional mean sample values of the variables.
[b] Probability value of joint *F* test for exclusion of all control variables.
* Each individual coefficient is statistically significant at the 10% level.
** Each individual coefficient is statistically significant at the 5% level.
*** Each individual coefficient is statistically significant at the 1% level.

5.2. Children's health and parental aspirations

In Section 4.2 we explored the effect of differential exposure to educated professionals on aspirations by comparing households with high and low exposure to nurses and doctors. A possible concern could be that visits to the health clinics not only expose parents to professionals, but also enable children to get health treatments in the mean time. These treatments may improve children's health status and so have an impact on the educational aspirations of the parents, other than through exposure to professionals. We may think of a simple human capital argument: the healthier the children, the more likely parents wish to invest in them since their life horizon increases.

The ideal experiment to isolate the causal impact of exposure to professionals on educational aspirations would entail visits to professionals that are utterly useless in themselves. In this experiment, we would be sure that the effect would simply come from interacting with trained professionals, not from a treatment these professionals may give. As such an experiment is not available, we study if there is any relationship between children's health and educational aspirations of the parents. Since data on children being ill is available in round three, we consider households living in control villages as they are not influenced by their exposure to professionals, or by PROGRESA's conditionality requirements.

We consider two different children health measures: a dummy equal to one if the household had a child ill at most 3 days during the previous month, and a dummy equal to one if the household had a child ill at least 20 days during the previous month. The former intends to measure a minor illness, such as a simple cold, while the latter intends to measure a more serious illness that could have strong repercussions.[29]

Table 10 shows the results of running OLS regressions of parental aspirations on the children's health measures and controls.[30] Parental aspirations do not seem to be linked to their children's health status, independently of whether we consider a minor or a serious illness. These regressions do not have a causal interpretation and only intend to show that there does not seem to be any significant relationship between parents' educational aspirations and children's health. Thus, it does not seem that plausible that health improvements caused by visits to the health clinic have an impact on the educational aspirations of the parents.

5.3. Falsification test

In order to check whether our results are spurious, we perform the same analysis that we have conducted thus far on the non-eligible households.[31] That is, we check whether the parental aspirations of those households that are not eligible to receive the benefits of PROGRESA, and, hence, are not required to send their children to school or regularly present at the health clinics for check-ups, are also changing.

Table 11 shows that, after the start of PROGRESA, non-eligible parents did not change the aspirations they had for their children. Consequently, the triple difference estimators are not statistically different from zero, which indicates that neither after six months nor after one year from the start of PROGRESA did non-eligible parents change their aspirations for their children's education.[32] Moreover, results when running the regressions in simple differences are also not statistically different from zero (Table 3, columns (4), (8), and (12)). Hence, our findings do not seem to be the result of some circumstance that occurred in the treatment villages that may have been affecting households whose youngest child is less than five years of age differently relative to households whose youngest child is five or older.

5.4. Alternative subsamples

We consider alternative subsamples in order to analyze households with more comparable family structures who have parents with more similar characteristics, e.g. age and educational level. In particular, we focus on the following four subsamples: (i) households with exactly two children less than 11 years of age; (ii) households with exactly three children less than 11 years of age; (iii) households with one child of age five and other siblings; (iv) households with one child of age six and other siblings. For all four subsamples, we obtained similar results to those reported above for the whole sample. Nevertheless, when reducing the sample size, the variability increases causing the estimates to not always be statistically significant.[33]

Furthermore, we also consider additional subsamples comparing high- and low-exposure households whose youngest children are closer to the mandated age cutoff (age five). Specifically, we compare the following high- versus low-exposure groups: (i) households whose youngest child is 2–4 years of age vs. households whose youngest child is 5–7 years of age; (ii) households whose youngest child is 3–4 years of age vs. households whose youngest child is 5–6 years of age; (iii) households whose youngest child is 4 years of age vs. households whose youngest child is 5 years of age. When we focus on these specific households, our sample size decreases; however, differences in observables between high- and low-exposure households get reduced. With these three subsamples we obtained similar results to the ones for the whole sample (comparison (iii) restricts the sample too much, and as a result the coefficients of interest are not precisely estimated).

[29] Regression results using different health measures (e.g. having a child ill at least 5, 10, and 15 days) show very similar results and are available upon request.

[30] Regression results do not change when controls are not included.

[31] Within every locality where the program is implemented, households are non-eligible to receive PROGRESA's benefits if they are above the poverty level as determined by discriminant analysis on census data.

[32] Regression results do not change when controls are not included.

[33] Regression results for the subsamples are available upon request.

Table 10
Effect of children's health on parental aspirations at the household level in round 2.

	At least one child ill at most 3 days during the previous month			At least one child ill at least 20 days during the previous month		
	All children	Daughters	Sons	All children	Daughters	Sons
At least one child ill	−0.009	0.159	−0.078	0.718	0.678	0.790
	(0.236)	(0.291)	(0.271)	(0.477)	(0.602)	(0.518)
Constant	11.004***	11.159***	11.187***	10.989***	11.148***	11.167***
	(0.388)	(0.436)	(0.447)	(0.386)	(0.437)	(0.446)
Controls for observable characteristics[a]	Yes	Yes	Yes	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.000	0.000	0.000	0.000
Obs.	2746	2011	2093	2746	2011	2093
R^2 (overall)	0.040	0.038	0.042	0.040	0.039	0.043

Note: Robust standard errors clustered at the village level in parenthesis.
[a] The controls are: head and spouse's age, head and spouse's schooling, head and spouse's literacy, head and spouse's indigenous status, number of male and female adults, number of male and female children, and household's monthly income. Regressions also include an indicator for whether data on parental characteristics were unavailable. These households are assigned the cross-sectional mean sample values of the variables.
[b] Probability value of joint *F* test for exclusion of all control variables.
*** Each individual coefficient is statistically significant at the 1% level.

Table 11
Triple differences estimates (non-eligible households).

	All children	Daughters	Sons
Panel A: impact after 6 months			
Exposure effect (Time × Treatment × Exposure)	−0.342	−0.452	−0.431
	(0.246)	(0.280)	(0.283)
PROGRESA effect (Time × Treatment)	0.226	0.332	0.203
	(0.196)	(0.218)	(0.209)
Constant	13.111***	13.114***	13.160***
	(0.236)	(0.251)	(0.235)
Controls for observable characteristics[a]	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.000
Obs.	8990	7406	7572
R^2 (overall)	0.050	0.048	0.043
Panel B: impact after 1 year			
Exposure effect (Time × Treatment × Exposure)	−0.337	−0.062	−0.382
	−0.260	(0.286)	(0.294)
PROGRESA effect	0.023	−0.144	0.091
	(0.205)	(0.218)	(0.217)
Constant	13.038***	13.015***	13.120***
	(0.240)	(0.259)	(0.236)
Controls for observable characteristics[a]	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.000
Obs.	8204	6908	7048
R^2 (overall)	0.039	0.036	0.032

Note: Robust standard errors clustered at the village level in parenthesis.
[a] The controls are: age of the youngest child interacted with a time dummy, head and spouse's age, head and spouse's age interacted with a time dummy, head and spouse's schooling interacted with a time dummy, head and spouse's literacy interacted with a time dummy, head and spouse's indigenous status interacted with a time dummy, number of male and female adults interacted with a time dummy, number of male and female children interacted with a time dummy, household's monthly income, and household's monthly income interacted with a time dummy. Regressions also include an indicator for whether data on the age of the youngest child and parental characteristics were unavailable. These households are assigned the cross-sectional mean sample values of the variables.
[b] Probability value of joint *F* test for exclusion of all control variables.
*** Each individual coefficient is statistically significant at the 1% level.

6. Educational aspirations and behavioral outcomes

While previous research has shown that parents' educational aspirations for their children are positively correlated with their children's educational outcomes (Goodman & Gregg, 2010; Gregg & Washbrook, 2009; Gutman & Akerman, 2008a, 2008b), it is important to check whether this correlation holds in rural Mexico. Trying to link parental aspirations to objective educational outcomes in our context is difficult because of PROGRESA itself.[34] Thus, in order to analyze the correlation between aspirations and educational outcomes, we need to use data on households that have not been beneficiaries of PROGRESA.

[34] For example, an increase in school attendance could be caused by exposure to doctors and nurses as well as by the educational cash transfers received for attending school.

Table 12
Parental aspirations and children's time spent doing homework and working.

	All children	Daughters	Sons
Panel A: effect of parental aspirations on their children's time spent doing homework			
Parental aspirations	1.289**	0.986**	1.603***
	(0.554)	(0.394)	(0.456)
Controls for observable characteristics[a]	Yes	Yes	Yes
Probability value for controls[b]	0.007	0.047	0.000
Obs.	2601	1237	1268
R^2 (overall)	0.018	0.031	0.046
Panel B: effect of parental aspirations on their children's time spent working (at home and outside)			
Parental aspirations	−1.477	−0.264	−0.686
	(1.035)	(0.429)	(0.956)
Controls for observable characteristics[a]	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.000
Obs.	2601	1778	1850
R^2 (overall)	0.062	0.029	0.045

Note: Robust standard errors clustered at the village level in parenthesis.

[a] The controls are: head and spouse's age, head and spouse's schooling, head and spouse's literacy, head and spouse's indigenous status, number of male and female children, and household's monthly income. Regressions also include an indicator for whether data on parental characteristics were unavailable. These households are assigned the cross-sectional mean sample values of the variables.

[b] Probability value of joint F test for exclusion of all control variables.

** Each individual coefficient is statistically significant at the 5% level.

*** Each individual coefficient is statistically significant at the 1% level.

In this section, we first study the correlation between parental aspirations and children's schooling/working behavior in the short-run, using data of households living in control villages in the fourth survey round (i.e. before they were incorporated into the program). Then, we study the correlation between parental aspirations and children's educational attainment in the long-run, using data of non-elegible households (i.e. households that were never incorporated into the program).

6.1. Aspirations and behavior in the short-run

The fourth survey round contains information about the time each household member allocated to 18 different activities during the previous day. Using data of households living in control villages we are able to check whether there is any relationship between parental aspirations toward their children's education and the time children spend doing school homework and working. As mentioned above, we consider households from control villages because their behavior was not influenced by PROGRESA's conditionality requirements, since they were not receiving the benefits of the program.

Table 12 shows the results of running OLS regressions of the time used by children doing homework and working, on parental aspirations. In particular, Panel A of Table 12 shows that there is a positive and significant relationship between parents' educational aspirations and the number of minutes children spend doing their homework. In contrast, Panel B of Table 12 outlines a negative link between parents' educational aspirations and the number of minutes their children spend working at home or outside. These regressions however, do not have a causal interpretation. For example, children that do not work and spend their afternoons doing homework may do well in school, and this good performance may increase the educational aspirations their parents have for them. Still, the positive (for homework) and negative (for work) signs of the coefficients suggest that an increase in parental educational aspirations might result in a decrease in child labor and in an increase in the time children spend studying.

6.2. Aspirations and behavior in the long-run

The eighth and latest survey round of PROGRESA's evaluation sample was carried out in 2007. Using data of non-eligible households we analyze whether the educational aspirations parents had in 1998 for their children predict their children's schooling by 2007. Here, we consider non-eligible households because their behavior has not been influenced directly by the program's conditionality requirements. However, Bobonis and Finan (2009) and Lalive and Cattaneo (2009) have shown that PROGRESA has had spillover schooling effects from eligible to non-eligible children. Hence, the results presented below must be interpreted with caution.

Table 13 explores whether parental aspirations predict schooling for all children, daughters and sons (Panels A, B, and C, respectively). We consider four different measures of schooling: whether the child has completed secondary school, whether the child has completed high school, whether the child has obtained a bachelor degree, and years of schooling. Overall, aspirations in 1998 seem to predict schooling by 2007. Considering all children (Panel A), the regressions with controls show positive and statistically significant relations between parental aspirations and all four schooling outcomes.[35] Having a parent aspiring for her child to complete at least secondary school, increases the probability of the child having graduated from secondary school by 2007 by 22%. Having a parent aspiring for her child to complete at least high school, increases the probability of the child having graduated from high school by 2007 by 4%. And, having a parent aspiring for her child to

[35] Regression results do not change when controls are not included.

Table 13
Parental aspirations and children's schooling.

	Completed secondary school	Completed high school	Completed college	Years of schooling
Panel A: all children				
Parental aspirations	0.223***	0.041***	0.007**	0.093***
	(0.075)	(0.015)	(0.003)	(0.018)
Constant	−0.268**	−0.741***	−0.083***	3.089***
	(0.114)	(0.068)	(0.027)	(0.57)
Controls for observable characteristics[a]	Yes	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.665	0.000
Obs.	2882	2882	2882	2941
R^2 (overall)	0.093	0.154	0.044	0.162
Panel B: daughters				
Parental aspirations	0.168**	0.024	0.006	0.074***
	(0.076)	(0.019)	(0.004)	(0.019)
Constant	−0.087	−0.724***	−0.086***	4.093***
	(0.124)	(0.082)	(0.030)	(0.602)
Controls for observable characteristics[a]	Yes	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.474	0.000
Obs.	2125	2125	2125	2125
R^2 (overall)	0.089	0.156	0.058	0.156
Panel C: sons				
Parental aspirations	0.194**	0.015	0.010***	0.082***
	(0.091)	(0.016)	(0.004)	(0.020)
Constant	−0.330**	−0.749***	−0.099***	2.930***
	(0.134)	(0.076)	(0.033)	(0.604)
Controls for observable characteristics[a]	Yes	Yes	Yes	Yes
Probability value for controls[b]	0.000	0.000	0.638	0.000
Obs.	2163	2163	2163	2163
R^2 (overall)	0.099	0.163	0.059	0.175

Note: Robust standard errors clustered at the village level in parenthesis.

[a] The controls are: head and spouse's age, head and spouse's schooling, head and spouse's literacy, head and spouse's indigenous status, number of male and female adults, number of male and female children, and household's monthly income. Regressions also include an indicator for whether data on parental characteristics were unavailable. These households are assigned the cross-sectional mean sample values of the variables.

[b] Probability value of joint F test for exclusion of all control variables.

** Each individual coefficient is statistically significant at the 5% level.

*** Each individual coefficient is statistically significant at the 1% level.

complete at least college, increases the probability of the child having graduated from college by 2007 by less than 1%. Furthermore, one additional year of education that a parent aspires her child to complete increases schooling years by 0.09 years of schooling. When considering daughters and sons separately, in Panels B and C, results are somewhat similar.

Once again, these regressions do not have a causal interpretation as, for example, parents may have had high aspirations in 1998 because their children were good in school then and the children kept doing well later on. Nevertheless, these results seem to suggest that parental aspirations do have some predictive value. In any case, it is encouraging to note that, at the very least, both short- and long-run results seem to be consistent with previous findings in the literature that suggest the existence of a positive correlation between parental educational aspirations and children's educational outcomes.

7. Conclusions

Poverty almost certainly affects the way people think and make decisions (Duflo, 2006), which causes the poor to have limited aspirations, and, as a result, might cause them to underinvest in the education of their children thereby generating a self-sustaining poverty trap (Appadurai, 2004; Ray, 2006). Understanding if the aspirations of the poor can be increased and, if so, through which channel(s) is an important tool for reducing poverty.

This paper shows that PROGRESA's beneficiary parents have higher educational aspirations for their children of about a third of a school year than do non-beneficiary parents. Then, it shows suggestive evidence that PROGRESA's differential mandated exposure to highly educated professionals results in an increase of almost half of a school year for children of high-exposure households (relative to low-exposure households) in treatment villages (relative to control villages) six months after the start of the program (relative to before its introduction). This finding seems to suggest that exposure to educated professionals might be one of the possible channels through which parental aspirations are changing. This is consistent with the evidence by Adato et al. (2000) showing that PROGRESA's mandated visits to the clinic caused communication and bonding between the health personnel and the beneficiaries, contributing to changing people's thinking.

Interestingly, a year after the start of the program, the aspirations of parents from low-exposure households might be catching up with those of high-exposure households. This evidence would be consistent with the hypothesis that it is the amount of exposure (i.e., the number of meetings) that affects aspirations and not the frequency of these meetings. This hypothesis is in line with studies in psychology which find that only after a certain

amount of exposure outstanding individuals become relevant to others and can induce changes (Lockwood & Kunda, 1997; Major et al., 1993).

We also consider as an alternative aspiration variable the proportion of parents who declare they want their children to finish at least college. We find that PROGRESA seems to have increased the proportion of parents who aspire for their children to finish college six months and a year after the start of the program by 14% and 18%, respectively. On the other hand, differential mandated exposure to doctors and nurses appears to be one of the channels driving the results, contributing to raise the proportion of households that aspire for college completion for their children six months after the start of the program by 20%.

Our findings seem to be robust to a number of robustness checks. In particular, our results do not seem to be due to an income effect from the cash transfers received by the households, nor by an age effect, nor by a health effect, nor because of some other circumstance occurring in the treatment villages that may have affected households with high exposure to doctors and nurses differently than households with low exposure.

Importantly, we also present suggestive evidence linking parental aspirations to objective educational outcomes. In the short-run, parents' educational aspirations seem to be positively related to the number of minutes children spend doing their school homework and negatively related to the time children spend working. In the long-run, there seems to be a positive and significant correlation between parental aspirations in 1998 and their children's educational attainment by 2007.

An important caveat of this paper is that there certainly are a number of channels through which a conditional cash transfer program like PROGRESA can affect parental educational aspirations. While the evidence presented in this paper seems to be consistent with one of those channels—mandated exposure to educated professionals—other channels, that we are not able to detect, may exist.

Even if other channels may play a role in changing aspirations, we believe that further studies on the importance of exposure of the poor to educated professionals are important for at least three reasons. First, identifying exposure as a possible channel through which aspirations of the poor can be modified could add a new tool to the existing options that try to promote increased investments in human capital and productive assets as a means to escape poverty. Second, by design, a number of anti-poverty programs expose their target populations to doctors, nurses, teachers, and many other highly educated professionals. Policy-makers could harness the potential benefit of increased aspirations that may be associated with exposure to highly educated professionals by encouraging or requiring that the beneficiaries of anti-poverty programs meet with such professionals a sufficient number of times. Third, if true, our finding would suggests that, in highly segregated environments or in contexts in which there is low social interaction or lack of leadership, promoting exposure to external educated professionals may have important consequences with respect to the aspirations of the population.

Future research will aim at getting a deeper understanding of the precise mechanism(s) through which aspirations change. The possible mechanisms suggested in the literature for why exposure to highly educated professionals could influence aspirations are many. First, according to Ray (2006), exposure stimulates social interactions, which, in turn, increase individuals' aspiration windows. Second, exposure causes information flows that allow individuals to learn about opportunities that they might engage in or the investment it takes to achieve the associated goals. Third, exposure increases the set of alternatives that people consider because they have bounded rationality.

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