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## **Must Conditional Cash Transfer Programs Be Conditioned to Be Effective?**

The Impact of Conditioning Transfers on School Enrollment in Mexico

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## **INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE**

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## ABSTRACT

A growing body of evidence suggests that conditional cash transfer (CCT) programs can have strong, positive effects on a range of welfare indicators for poor households in developing countries. However, the contribution of individual program components toward achieving these outcomes is not well understood. This paper contributes to filling this gap by explicitly testing the importance of conditionality on one specific outcome related to human capital formation (namely school enrollment), using data collected during the evaluation of Mexico's *Programa de Educación, Salud, y Alimentación (PROGRESA)* CCT program. We exploit the fact that some *PROGRESA* beneficiaries who received transfers did not receive the forms needed to monitor their children's attendance at school. We use a variety of techniques, including nearest neighbor matching and household fixed effects regressions, to show that the lack of these forms reduced the likelihood of children attending school, with this effect being most pronounced among children who were transitioning to lower secondary school. We provide substantial evidence that these findings are not driven by unobservable characteristics related to households or localities.

**Keywords:** conditionality, cash transfers, school enrollment, school attendance, *PROGRESA*, Mexico

## 1. BACKGROUND

Conditional cash transfers (CCTs) are becoming an increasingly popular tool for poverty alleviation. Drawing on lessons learned from programs in a variety of countries—most notably Mexico’s *PROGRESA* program<sup>1</sup>—CCTs are now found throughout the developing world. As implied by their name, conditional cash transfer programs give cash transfers to households that meet specific conditions or undertake certain actions, such as ensuring that school-age children go to school or that preschool children regularly see a nurse or doctor. Many of these programs have been carefully evaluated in order to demonstrate their effectiveness.<sup>2</sup> However, such evaluations normally treat the CCT as a “black box,” assessing the combined effect of all of the components of the CCT on a given outcome, without considering which aspects of CCTs make them successful at improving target outcomes.<sup>3</sup> As a result, little is known regarding the importance of the individual CCT components. For example, there is a lively debate regarding the desirability of imposing conditions on beneficiaries (Szekely 2006; Samson 2006; Freeland 2007), but there is little evidence to bring to this debate.

Both public and private perspectives provide good rationales to impose conditions on the receipt of cash transfers. From a public perspective, there are three related rationales. First, governments may perceive that they know which actions or behaviors will benefit the poor better than do the poor themselves; consequently, conditioning transfers can induce changes in recipients’ behavior that led to outcomes that governments regard as desirable. For example, governments may place greater weight on the intrinsic value of educating girls than their families do. Second, conditioning may help the government overcome information asymmetries. Governments may be aware of the benefits associated with immunization or screening for chronic diseases but individuals may be unaware or unconvinced of these benefits. When other approaches to such informational problems—such as public health campaigns—have failed, conditioning transfers can be seen as a means of changing behaviors. Finally, conditioning may be required for political economy reasons. Politicians and policymakers are often evaluated by performance indicators such as changes in school enrollment or health clinic use. By conditioning transfers on behaviors that increase these indicators, politicians and policymakers can provide useful evidence of accomplishments long before the indicators show more important evidence of poverty reduction (for example, increased productivity or better adult health). Therefore, politicians can perceive that conditioning of transfers is a useful tool to help them stay in office.

From the private perspective, the conditional component of CCTs can also have potential benefits. First, disagreements may exist within households regarding the allocation of resources. Imposing conditionality on cash transfers can strengthen the bargaining position of individuals whose preferences are aligned with the government’s preferences, but who may otherwise lack bargaining power within the household. Second, conditioning may overcome stigma effects otherwise associated with welfare payments. The stigma attached to welfare payments may discourage those with valid claims from taking benefits. From the beneficiary’s point of view, conditioning can be seen as part of a social contract with the state, thereby legitimizing the transfer and overcoming the stigma. Finally, recent work in behavioral economics emphasizes that when households have hyperbolic discount functions, they undertake actions that can reduce their own welfare (Laibson 1997). In such circumstances, households are better off under constraints designed to reduce or limit their ability to trade future consumption for present consumption. Conditionality can be seen as such a constraint.

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<sup>1</sup> *Programa de Educación, Salud, y Alimentación*. The program was renamed *Oportunidades* and expanded to urban areas when Vicente Fox became president of Mexico in 2000.

<sup>2</sup> For example, see the work on *PROGRESA* summarized in Skoufias (2005) and Levy (2006), and on Nicaragua’s *Red de Protección Social (RPS)* found in Maluccio and Flores (2005). Studies focusing on the impact of *PROGRESA* on schooling-related outcomes include those of Schultz (2004), Behrman, Sengupta, and Todd (2005), and de Janvry and Sadoulet (2006).

<sup>3</sup> A general discussion of the merits and limitations of experimental approaches to the evaluation of social programs is found in Burtless (1995) and Heckman and Smith (1995).

Although the rationales for conditionality may seem compelling, there are a number of concerns regarding the imposition of conditionality. First, conditionality increases the administrative costs and complexity of running a cash transfer program. Caldés, Coady, and Maluccio (2006) show that monitoring conditionality represents approximately 18 percent of *PROGRESA*'s administrative costs and 2 percent of total program costs. If the actual or perceived benefits of conditionality do not outweigh the additional costs, it may not be worthwhile to condition the transfers. Second, when the act of meeting conditions imposes direct costs on beneficiaries, conditionality reduces the benefits accruing to the participants. Molyneux (2007) notes that such costs are not necessarily shared equally among household members, as mothers often accompany children to health clinics or attend community meetings. Third, if the preferences of the poor do not align with the conditions placed on their behavior by the government, the restrictions that conditionality imposes on the poor will reduce their total welfare gains, thus decreasing the net benefits of the CCT. Fourth, some households may find the conditions too difficult to meet; if these are among the poorest households in the program, imposing conditions may detract from the targeting of the CCT. Fifth, conditionality can create an opportunity for corruption, whereby individuals who are responsible for certifying that conditions have been met could demand payments for doing so. Sixth, conditioning transfers can be perceived as being demeaning to the poor. Conditioning can be seen as implying that the poor simply do not know what is good for them. Finally, Frelander (2007) describes the imposition of conditionality as "morally atrocious," arguing that because social protection falls under the Universal Declaration of Human Rights, it is indefensible to attach conditions to their receipt.

Since conditionality is always part of the CCT package, it is not clear whether the benefits of conditionality actually outweigh the costs outlined above. The objective of this paper is to begin to provide evidence on the impact of conditioning transfers in the context of a conditional cash transfer program. We exploit the fact that some beneficiaries of Mexico's *PROGRESA* program did not receive the forms needed to monitor the attendance of their children at school. Using administrative data on transfers in combination with data collected as part of *PROGRESA* evaluation, we assess the impact of imposing education-related conditions on school enrollment and attendance empirically. Regardless of the technique we use, we find that on average, the absence of the forms reduces the likelihood of school attendance. The effect of conditionality depends upon the grade level of the student, and the absence of conditionality has the strongest impact on the enrollment of children making the transition to lower secondary school, whereas it has no measurable impact on children continuing in primary school. As the nonreceipt of forms is not random, several robustness checks are completed to ensure that our results are not due to unobserved heterogeneity at either the household or community level. Finally, we provide evidence suggesting that the effect is more pronounced among households with illiterate heads and among households in which the head does not perform agricultural labor, indicating that the results may be partially due to informational problems and the opportunity cost of schooling for such children.<sup>4</sup>

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<sup>4</sup> Only one other study has attempted to estimate the impacts of conditionality on conditioning cash transfers using a similar strategy (Schady and Aruajo 2006). That study used the fact that many people in Ecuador believed that transfers in *Bono de Desarrollo Humano* were conditioned, when in reality they were not. The study found that conditioning had a positive effect.



## 2. PROGRAM DESCRIPTION AND DATA

*PROGRESA* was introduced by the Federal Government of Mexico as part of an effort to break the intergenerational transmission of poverty. The program was primarily aimed at improving the educational, health, and nutritional status of poor families, particularly children and their mothers. The program was initially implemented in poor rural communities with fewer than 2,500 inhabitants. Households were selected for inclusion on the basis of both locality and household characteristics. Implementation began in August 1997 with the incorporation of approximately 140,000 households into the program. By early 2000, *PROGRESA* included nearly 2.6 million families in 72,345 localities in all 31 Mexican states, constituting around 40 percent of all rural families and one-ninth of all families in Mexico. Beneficiaries received cash transfers on a bimonthly basis. The transfers had three components: a scholarship tied to the continued attendance of children at school (the *beca*), money for school supplies, and a cash transfer for food (the *alimento*). *PROGRESA* (1997) provides a more detailed description of the program.

To receive the *beca*, school-aged children in grades 3 and higher had to maintain an attendance record of 85 percent or better, and parents had to attend monthly meetings called *platicas*. To ensure compliance with this condition, parents were supposed to initially receive an “E1” form when they were inducted into *PROGRESA*. Parents were to take this form to the teacher, who would sign the form and register the child for attendance monitoring, and then parents were to return the E1 form to *PROGRESA* officials. Usually, the form was actually returned to the local *promotora* (a woman in the community who acted as a liaison between beneficiaries and *PROGRESA*). *PROGRESA* officials then were supposed to match E1 forms with school attendance records that the teachers kept separately for *PROGRESA* enrollees. After confirming that attendance was satisfactory, officials arranged for the payment of the *beca*. The payment of the *beca* occurred every two months; *promotoras* spread the word in the community that payments would be made on a certain date at a specific place, and *PROGRESA* officials would then set up portable tables and hand the beneficiaries envelopes containing their payments.

Our study hinges on the fact that a significant proportion of *PROGRESA* households never received the E1 form, yet still received transfers. These payments could not have been conditioned, since teachers would not have had reason to monitor the attendance of children in these families.<sup>5</sup> We argue that the failure to receive an E1 form cannot be related to any household- or community-level unobservables, and as a result we can use an indicator variable for the receipt of an E1 form to measure the effect of conditionality on school enrollment in the *PROGRESA* program.

We use two data sources for our study. First, we use administrative data on *beca* payments made between March and August of 1999 to measure which households received transfers.<sup>6</sup> We then use household identifiers to match the households receiving transfers with those interviewed during the evaluation surveys completed as part of *PROGRESA*. The bulk of the data used herein are from the evaluation survey conducted in May and June of 1999, as this survey (the *seguimiento*) asked beneficiaries about their experiences with *PROGRESA*.<sup>7</sup> The *seguimiento* specifically asked whether households had received the E1 form, as well as a series of questions about the conditions the households were supposed to meet in order to receive transfers.<sup>8</sup> For the purposes of our analysis, we only use households that actually received transfers that were supposed to be tied to school attendance (the *beca*).

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<sup>5</sup> We confirmed this point in discussions with both Santiago Levy, the architect of *PROGRESA*, and Emmanuel Skoufias, who was responsible for leading the evaluation of *PROGRESA*. Both indicated that if the household did not receive an E1 form, no monitoring of attendance was possible.

<sup>6</sup> Note that subsequent payments to households included in our data set likely became conditioned soon thereafter as administration of *PROGRESA* improved; we do not observe whether they received an E1 form after receiving payments in the period we study around the evaluation survey.

<sup>7</sup> The sampling frame only includes households with at least one child age 6 to 17.

<sup>8</sup> We also use several variables from the October 1998 evaluation survey round, such as per capita expenditures and household size.

We find that 4,383 households received at least one *beca* transfer for children's school attendance between March and August of 1999, and among them, 464 did not receive the E1 form. Children in these households, designated Group 1, could not have had their attendance monitored by *PROGRESA*. The remaining 3,919 households, which received the E1 form and received at least one *beca* payment for children's school attendance between March and August 1999, are designated Group 2. Households in Groups 1 and 2 share important similarities: they are all beneficiaries of the *PROGRESA* program, they all have school-age children, and they all received *beca* payments from *PROGRESA* for school attendance by their children. The difference is that the behavior of households in Group 1 could not be monitored, and by extension, these transfers could not be conditioned on attendance. As such, comparing outcomes among children of households in Groups 1 and 2 constitutes a potential way to assess the impact of conditionality on school attendance.

Although the comparison of Groups 1 and 2 may suggest that conditionality affects schooling-related outcomes, it is possible that household members who understood the conditions might assume that the program was somehow monitoring them, rendering the E1 unnecessary and meaning that comparison of Groups 1 and 2 would not be a true test of conditionality. To address this concern, we use the May-June 1999 survey to develop a second test of conditionality. The survey asked beneficiary households to list the conditions that they were required to fulfill in order to receive the *beca* payment. Using this information, we take the same sample of households and create two further groups for comparison. Households in Group 3 neither received form E1, *nor did* they know that they were required to send their children to school in order to receive the *beca* payment. Households in Group 4 received the E1 and *did* know that they were required to send their children to school in order to receive school benefits.<sup>9</sup> Since households in Group 3 neither received the form necessary for the transfer to be conditional nor knew the conditions for the transfer, the transfers they received were clearly unconditional.

Even if we can demonstrate a difference in average school enrollment or attendance between Groups 1 and 2 and/or Groups 3 and 4, the difference should not be immediately attributed to conditionality. There are several plausible reasons that some households received the E1 form while others did not. Some of these reasons might be related to observable or unobservable household characteristics, whereas others would suggest that the lack of an E1 form is quasi-experimental. For example, specific communities might simply have not received E1 forms, which would imply that endogenous program placement might have occurred. Alternatively, a given household might have missed the *platica* at which the E1 form was distributed, due to potentially observable (for example, environmental shock) or unobservable reasons. It could also be that at some *platicas*, the *promotoras* simply ran out of forms. In this latter case, the households that did not receive forms would essentially be selected randomly.

To ensure that our results are due to the lack of conditionality rather than differences in either observables or unobservables, we proceed as follows. We condition the unconditional means between groups with differences in observable characteristics, using both probit and nearest neighbor matching methods. To ensure that the differences we find are not due to a lack of forms or other unobservables, in a few specific communities or a few communities within one particular state we examine the rate of E1 form receipt at both the state and *municipio* level.<sup>10</sup> We then provide several robustness checks to ensure that our results are not due to household-level unobservables; in one such test, we control for household-level fixed effects, which should control for any time-invariant unobservable differences at the household level.

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<sup>9</sup> To provide a clean basis for comparison between Groups 3 and 4, we take the full sample of *PROGRESA*-eligible households and drop all households that did not receive the E1 but knew the conditions for receiving the *beca*, and all households that received the E1 but did not know the conditions.

<sup>10</sup> A *municipio* is approximately equivalent to a U.S. county.

### 3. RESULTS

#### Basic Findings

We begin by examining unconditional mean school enrollment by the groups defined above. Among children 8-16 years of age who have completed grades 3-8, 83.2 percent of children in Group 1 households are enrolled in school, while 88.6 percent of children in Group 2 households are enrolled (Table 1).<sup>11</sup> Even after taking into account the clustered nature of the survey, this difference is statistically significant at the 5 percent level.<sup>12</sup> The difference is even larger when we also consider whether or not the households understood the conditions. The enrollment rate among children in households in Group 3 is 80.1 percent, whereas it is 89.2 percent for children in Group 4 households. The differences in mean enrollments are therefore suggestive that conditionality does affect enrollment, but the differences could still be explained by a myriad of other factors.

**Table 1. Enrollment rates of children aged 8-16 who have completed grades 3-8, by household receipt of an E1 form**

Group	Sample size	Enrollment rate (percent)	Wald test on differences in enrollment rate
1 (Household did not receive an E1 form)	547	83.2	8.63**
2 (Household received an E1 form)	5,090	88.6	
3 (Household did not receive an E1 form and could not describe conditions)	261	80.1	13.44**
4 (Household received an E1 form and could describe conditions)	2,870	89.2	

Notes: All children reside in households receiving *PROGRESA* payments for school attendance. The Wald test for equivalence of enrollment rates controls for intracluster correlation within localities. \*\* indicates significance at the 1 percent level.

The unconditional means mask striking differences by grade level. We next calculate the share of children in *PROGRESA* households found in Groups 1 and 3 by completed grade level (Table 2). We find that the incidence of Group 1 and Group 3 membership is approximately the same for all grade levels. Next, we calculate the mean enrollment rate by grade level and group (Table 3), and plot the differences between means for Groups 1 and 2 (Figure 1) and Groups 3 and 4 (Figure 2). We find the largest between-group difference in school enrollment among children who have completed grade 6, that is, those who have finished primary school and should be entering lower secondary school. Children in households that did not receive the E1 form are much less likely—by 17 to 20 percent—to enroll in lower secondary school, whether or not their parents are aware of the attendance conditionality. These differences are significant at the 1 percent level (Table 3). For other grade levels, the differences are not nearly as large, not always statistically significant, and in some cases children in Groups 1 and 3 are slightly *more* likely to enroll than children in Groups 2 and 4. Therefore, the data suggest that the conditionality of transfers could be quite important when students move from primary school to lower secondary school in this specific case, or in general when students enter a higher level of schooling. However, our caveats regarding both observable and unobservable differences between households remain.<sup>13</sup>

<sup>11</sup> We use age 8 as the lower age cutoff as this is the lowest age where we observe children in grade 3, the first grade for which *PROGRESA* conditionality applies. Localities where all beneficiaries received the E1 form are dropped from the sample.

<sup>12</sup> Only one round of the survey provides information on receipt of the E1 form, knowledge of conditionality, and administrative data by type of transfer received. Therefore it is inappropriate to perform the difference-in-difference estimation, as used in many other papers on *PROGRESA* (for example, Schultz 2004 and Todd and Wolpin 2006). Nonetheless, when we examine average differences in school attendance between Groups 1 and 2 and 3 and 4 in earlier surveys, we find no significant difference.

<sup>13</sup> It is worth noting that the heterogeneity in differences by grade level strengthens our contention that the differences are

**Table 2. Share of children in *PROGRESA* households that did not receive the E1 form, by grade level and understanding of conditions**

Last grade level completed	Share in Group 1 (household did not receive the E1 form)		Share in Group 3 (household did not receive the E1 form and could not describe conditions)	
	Share	Number of observations	Share	Number of observations
3	0.103	1,278	0.085	691
4	0.091	1,097	0.081	621
5	0.087	1,022	0.070	575
6	0.107	1,342	0.103	728
7	0.102	489	0.078	271
8	0.081	409	0.065	245

Notes: All children reside in households receiving *PROGRESA* payments for school attendance.

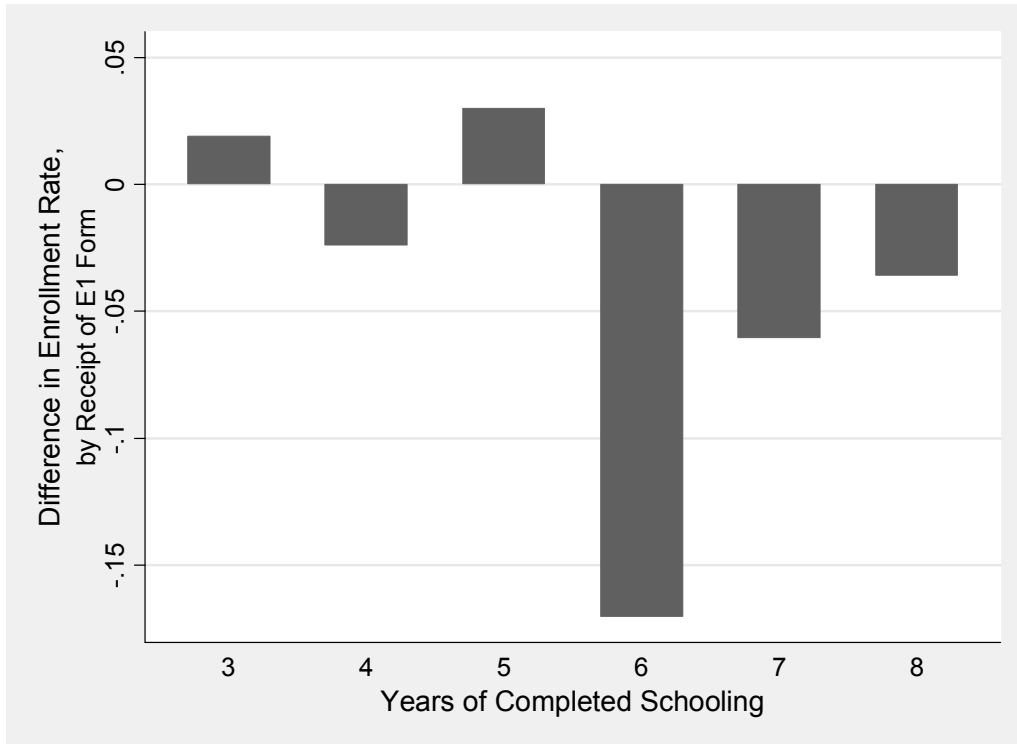
**Table 3. School enrollment rates among *PROGRESA* households, by completed grade and group**

Last grade level completed	Share enrolled in school		Share enrolled in school	
	Group 1	Group 2	Group 3	Group 4
3	0.977	0.958	0.966	0.956
4	0.930	0.956	0.900	0.956
5	0.978	0.942	0.950	0.942
6	0.521	0.691**	0.520	0.715**
7	0.860	0.920	0.714	0.916**
8	0.879	0.915	0.937	0.913

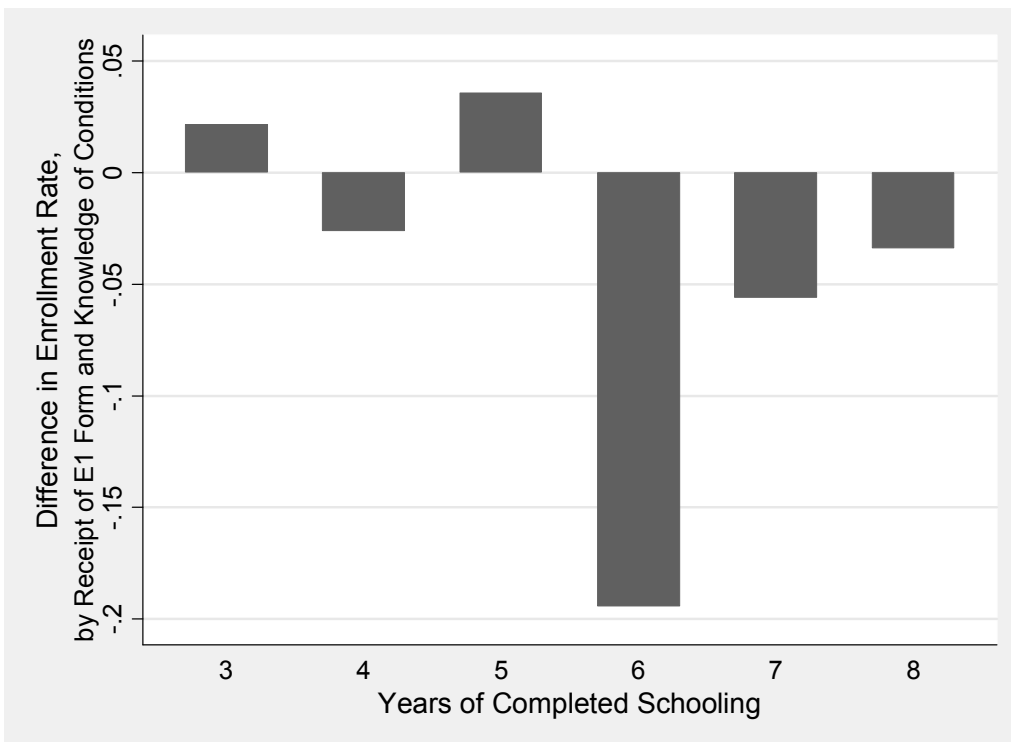
Notes: Group 1 households did not receive the E1 form. They are compared with Group 2 households, which did receive the E1 form. Group 3 households did not receive the E1 form and could not describe the *PROGRESA* conditions, whereas Group 4 households both received the E1 form and could describe the conditions. \*\* indicates that the difference between the share enrolled is significant at the 5 percent level, taking into account the clustering of the data.

not the result of unobservables. If unobservables explain the difference in enrollment between Groups 1 and 2 and/or Groups 3 and 4, they would have to specifically affect children who had completed grade 6 and not children completing other grades.

**Figure 1. Difference in school enrollment between those who received the E1 form to enforce conditionality and those who did not, among *PROGRESA* transfer recipients**



**Figure 2. Difference in school enrollment between those who received the E1 form and could name conditions and those who did not receive forms and could not name conditions, among *PROGRESA* transfer recipients**



To control for observable differences between children, households, and localities, we estimate probits where the dependent variable equals one if the child is enrolled and zero otherwise (Table 4). Our primary explanatory variable of interest is an indicator variable denoting households that did not receive the E1 form in the first specification (Panel A), and households who neither received the form nor knew the conditions (Panel B). In successive specifications, we build up the set of observable variables we use as controls. We initially control for state of residence, then include child characteristics (age dummies, gender), characteristics of the household head and spouse (age, gender, occupation, indigenous status, and literacy of the head; and indigenous status and literacy of the head's spouse), basic household characteristics (the logarithm of household size and consumption per capita, both measured in the earlier October 1998 survey round),<sup>14</sup> indicator variables measuring whether the household received the *PROGRESA* manual, whether or not the household had a health register, whether the household contained someone who served as a *promotora* for *PROGRESA*; and the number of *platica* meetings attended/missed by household members, household-level shocks (indicator variables for drought, flood, fire, frozen crops, crop disease, and earthquake tremors), and finally, several community-level characteristics (indicator variables for the presence of electricity, a primary school, a lower secondary school, and a secondary school).<sup>15</sup>

The estimated coefficients imply a change in enrollment broadly consistent with the difference in unconditional means reported in Table 1. Whereas the difference in unconditional means is 5.4 percent, after we control for child characteristics, we find that children in households lacking an E1 form are 4.6 percent less likely to enroll in school, on average (Table 4, Panel A, column 2). Adding additional parental, household, and community controls has little effect on the magnitude of the estimated coefficient; the coefficient estimated when using the full set of controls implies that the lack of an E1 form makes children 4.4 percent less likely to enroll in school, on average (Table 4, Panel A, column 6).

When we add that households did not know the conditions to the definition of the indicator variable for conditionality in the probit estimation controlling for the full set of characteristics (Table 4, Panel B, column 6), we find that children were 7.0 percent less likely to enroll in school, on average, as compared to the unconditional difference of 9.1 percent. While controlling for observable characteristics accounts for some of the difference between school enrollment among children in households who received the E1 form and those who did not, much of the difference is not accounted for by these control variables. Children whose attendance was not being monitored have lower enrollment rates than children whose attendance could be monitored, even when we control for observable household characteristics.

**Table 4. Probit estimates of the impact of nonreceipt of the E1 form on school enrollment of children who had completed grades 3-8**

	<b>Specification</b>					
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>	<b>(6)</b>
Household did not receive an E1 form	-0.054 (3.37)**	-0.046 (3.68)**	-0.045 (3.53)**	-0.046 (3.62)**	-0.049 (3.79)**	-0.044 (2.56)**
State controls	Yes	Yes	Yes	Yes	Yes	Yes
Child controls	No	Yes	Yes	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes	Yes	Yes
Basic household controls	No	No	No	Yes	Yes	Yes
Household-level additional and shock controls	No	No	No	No	Yes	Yes
Community controls	No	No	No	No	No	Yes

<sup>14</sup> See Hoddinott and Skoufias (2004) for details on the construction of these variables.

<sup>15</sup> Replacing the state-level indicators and the community-level characteristics with a full set of *municipio* or locality dummies does not change the general estimation results.

**Panel B: Comparing Group 3 (did not receive E1 form and did not know conditions) and Group 4 (received E1 form and knew conditions)**

	Specification					
	(1)	(2)	(3)	(4)	(5)	(6)
Household did not receive an E1 form	-0.090 (4.23)**	-0.067 (3.97)**	-0.064 (3.90)**	-0.066 (3.95)**	-0.074 (4.08)**	-0.070 (3.95)**
State controls	Yes	Yes	Yes	Yes	Yes	Yes
Child controls	No	Yes	Yes	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes	Yes	Yes
Basic household controls	No	No	No	Yes	Yes	Yes
Household-level additional and shock controls	No	No	No	No	Yes	Yes
Community Controls	No	No	No	No	No	Yes

Notes: Marginal effects are reported, cluster-robust z statistics are in parentheses. See Appendix Tables 1 and 2 for the full results of Table 4 Panels A and B, respectively, as well as the full list of variables included in these regressions. The sample sizes are 5,637 in Panel A and 3,131 in Panel B. \*\* indicates significance at the 1 percent level.

However, the results shown in Table 4 do not account for the potential heterogeneity in the effects of receiving the E1 form, which are suggested in Figures 1 and 2. Therefore we next replicate the probits for different completed grades, controlling for the full set of state, child, parent, household, and community characteristics (Table 5). We find that conditionality has the strongest effect among children who had completed grade 6, that is, children transitioning from primary to lower secondary school. When comparing Groups 1 and 2, we find that children in households that did not receive the E1 form were about 21 percent less likely to enroll in lower secondary school, and when comparing Groups 3 and 4, we find that children in households that did not receive the E1 form and were unaware of the conditions were 18 percent less likely to enroll. For children continuing primary school (having completed grades 3, 4, or 5), there is no evidence that conditionality has a significant effect on school enrollment.

**Table 5. Probit results of the impact of nonreceipt of the E1 form on school enrollment, by completed grade**

Completed grade	Household did not receive E1 form	Household did not receive E1 form and could not recite conditions
3	0.002 (1.01)	<0.001 (0.042)
Number of observations	1,243	411
4	0.003 (0.35)	0.001 (0.04)
Number of observations	969	385
5	0.013 (1.16)	0.004 (0.20)
Number of observations	927	504
6	-0.211 (4.15)**	-0.183 (2.91)**
Number of observations	1,308	703
7	-0.044 (1.30)	-0.255 (2.95)**
Number of observations	453	227

**Table 5. Continued**

Completed grade	Household did not receive E1 form	Household did not receive E1 form and could not recite conditions
8	0.012 (0.34)	X
Number of observations	393	209

Notes: Marginal effects are reported; cluster-robust z statistics are in parentheses. Each cell represents a separate regression. All regressions include all controls in column 6 of Table 4 Panels A and B. No result is available for members of Group 3 who had completed grade 8, because the “successes” were perfectly determined. \*\* indicates significance at the 1 percent level.

One could consider the differences in enrollment rates we find between children in Groups 1 and 2 and Groups 3 and 4 as the difference between the effect of conditioning transfers and the effect of increased income on school enrollment for those children completing grade 6. As the point estimate for the effect of conditioning is large—17 percentage points—one might be concerned that the income effect is negative. While Schultz (2004) finds that *PROGRESA* causes an 8.3 percentage point increase in enrollment among children who have completed grade 6, other estimates of the effect of *PROGRESA* on enrollment suggest larger impacts. Behrman, Sengupta, and Todd (2005) use a Markov transition analysis to show that when one considers a larger range of potential educational transitions, the increase in enrollment due to conditionality is much higher than Schultz finds with the more limited difference-in-difference estimator. de Janvry and Sadoulet (2006) also consider the effect of heterogeneity on the impact of conditional cash transfers by transfer level among children leaving grade 6, and find that a conditional transfer of \$200/year is associated with a 14-percentage-point increase in the probability of enrollment. Since the average transfer amount is \$200/year in their sample, one would expect an even larger impact of income on enrollment, *ceteris paribus*. They also present evidence suggesting that unconditional transfers should have a small, positive impact on enrollment through the household expenditure variable. Therefore, their estimates imply a small positive income effect. These findings are quite consistent with ours, as the magnitude of our coefficient estimate is similar to theirs.<sup>16</sup>

### Initial Controls for Unobservables

Although the unconditional means and the probit results provide *prima facie* evidence that conditionality affects enrollment, they implicitly assume that nonreceipt of the E1 form is uncorrelated with unobservable characteristics at the household or locality level. This assumption is quite strong, and it is not difficult to think of reasons why it might be violated. For example, suppose that there were administrative problems in one location that led to poor distribution of the E1 forms. Suppose, too, that this location had poor quality schools, or schools that were difficult to get to. If so, the differences in enrollment rates would reflect these factors and not the absence of the E1 forms.

However, there is evidence in the data that the nonreceipt of the E1 form is not driven by unobservable differences in administration by community. First, consider the distribution of households not receiving the E1 form by state (Table 6). The share of households that did not receive an E1 form is spread out nearly evenly across the seven states. Still, it could be that a few *municipios* in each state did not distribute E1 forms, and hence those states drive the distribution. We therefore illustrate the proportion of households not receiving the E1 form by *municipio* (Figure 3), which shows that that nonreceipt of forms is distributed widely across the sample.<sup>17</sup> Therefore, a bias similar to endogenous program placement bias does not seem to exist for the nonreceipt of E1 forms.

<sup>16</sup> It is further worth mentioning that Bobonis and Finan (2006) find a positive peer effect of *PROGRESA* on school enrollment among children in *PROGRESA* communities who lived in ineligible households.

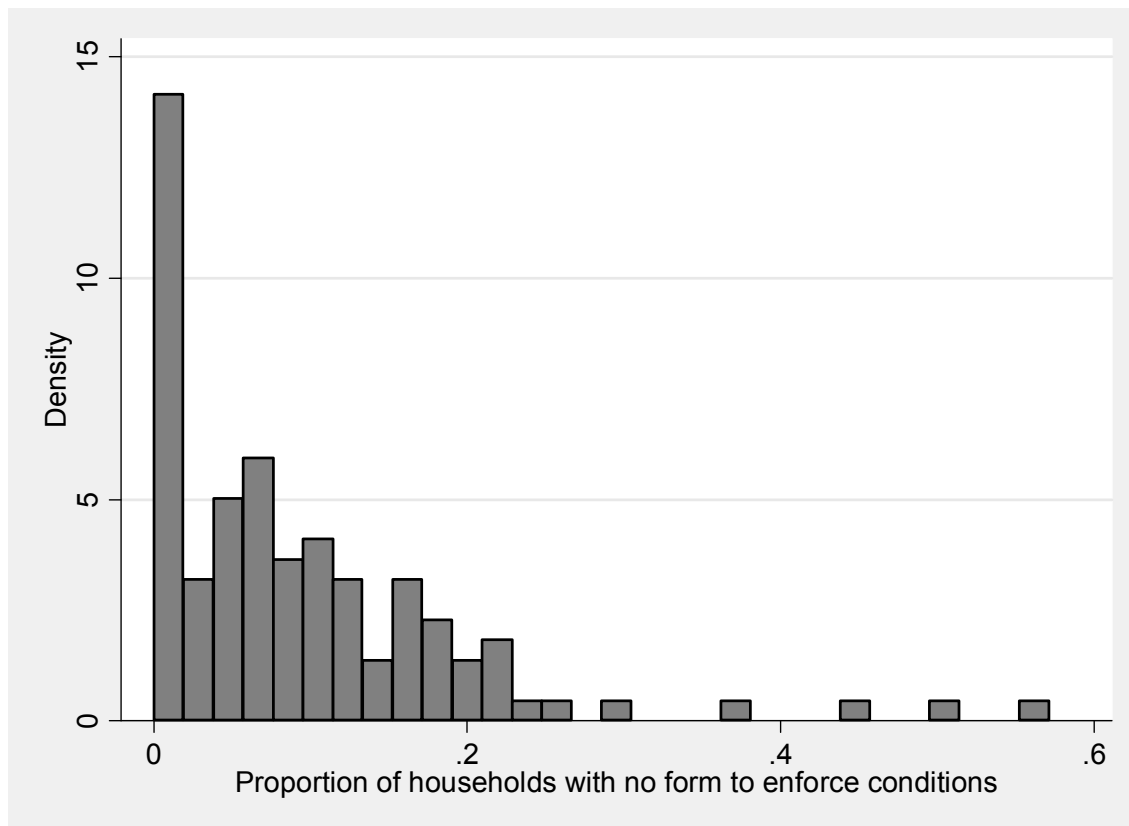
<sup>17</sup> Figure 3 does not differ when we plot the proportion of households not receiving the E1 form by locality.



**Table 6. Percentage of *PROGRESA* households receiving transfers for school attendance but not receiving E1 forms to monitor attendance, by state**

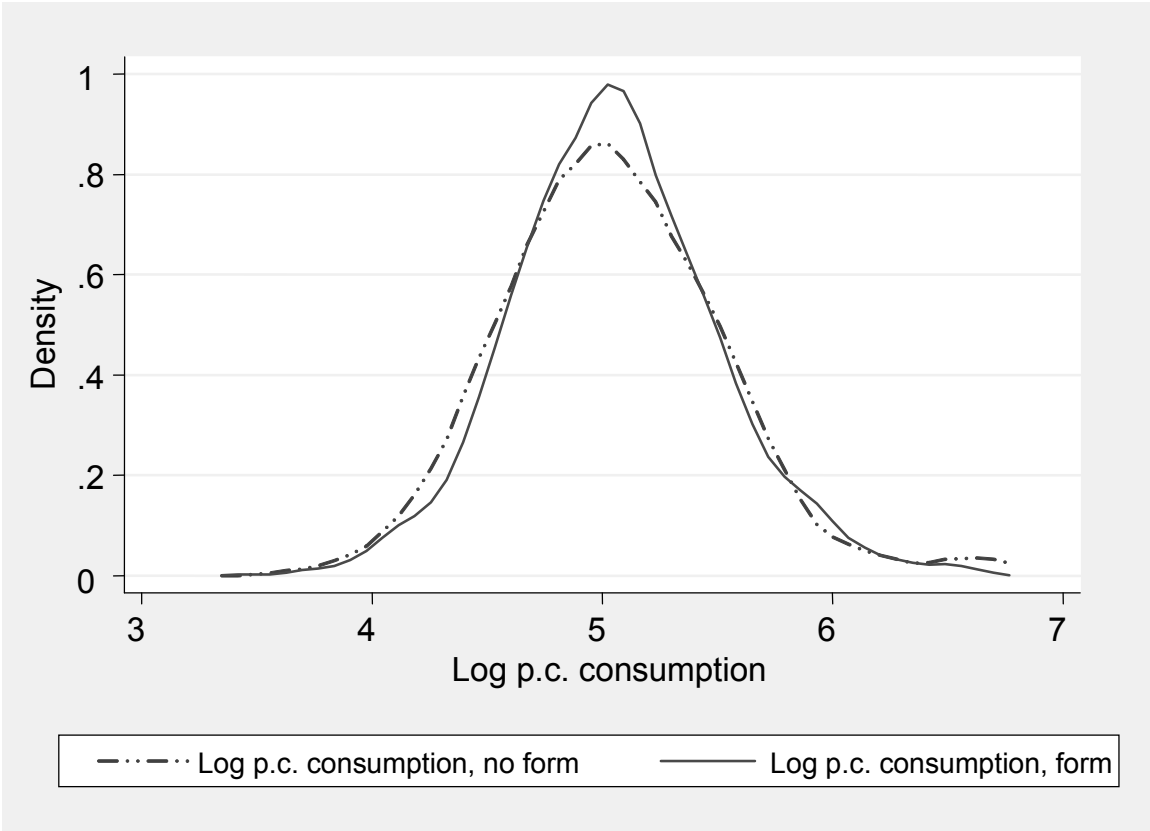
State	Percent
Guerrero	5.9
Hidalgo	10.9
Michoacán	11.5
Puebla	8.7
Querétaro	11.1
San Luis	8.5
Veracruz	9.8
All states	9.7

**Figure 3. Proportion of households that did not receive forms to enforce *PROGRESA* conditions, by *municipio***

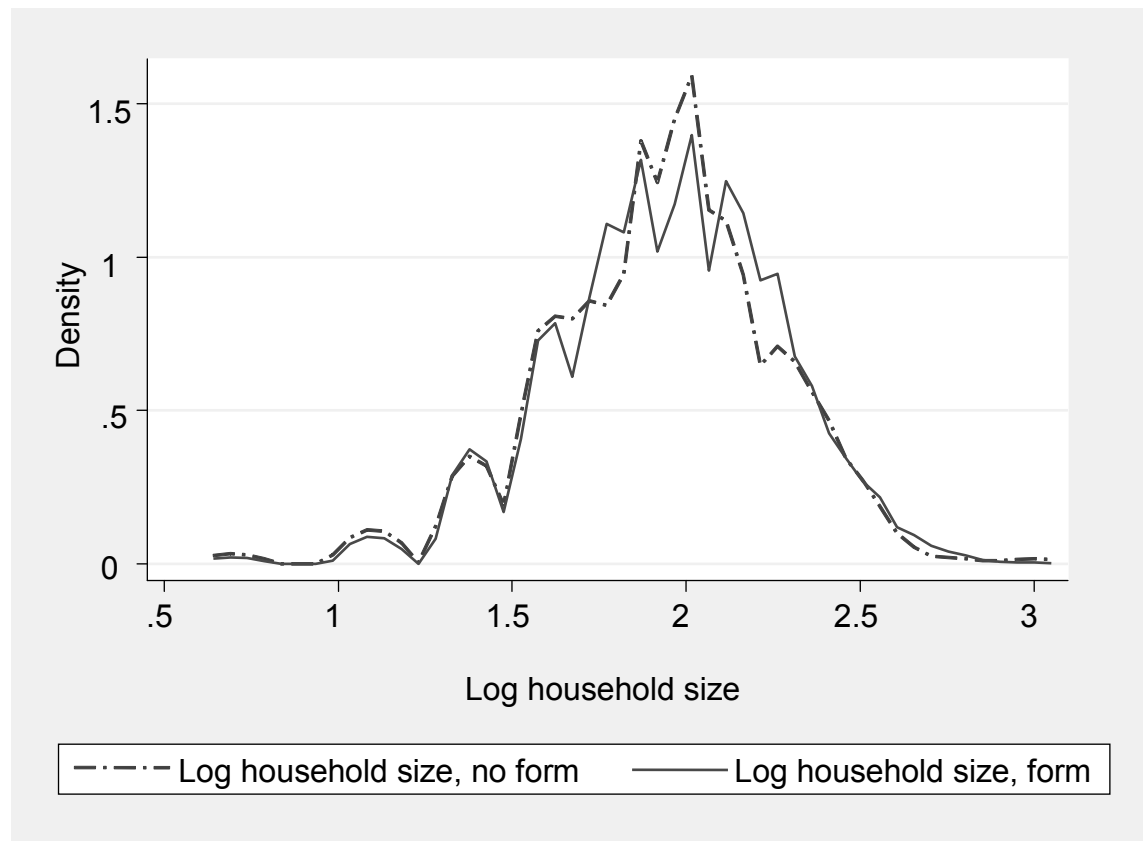


To examine household-level unobservables that might affect the results, we first consider whether households that failed to receive an E1 form were systematically poorer than households that did receive an E1 form, using the logarithm of per capita consumption as measured during the previous survey round in October 1998 (Figure 4). We find little difference in the kernel density of the consumption distribution for households that did and did not receive an E1 form. We might also consider that smaller households might not have received forms, so we next show the distribution of the logarithm of household size, again measured in October 1998, by receipt of forms (Figure 5). Again, there is little obvious difference in these distributions.

**Figure 4. Kernel density of logarithm of per capita consumption, by whether or not the household received an E1 form**



**Figure 5. Logarithm of household size, by receipt of an E1 form, among *PROGRESA* households**



While these distributions do not provide obvious evidence of observable differences between household in Groups 1 and 2, if we estimate probits where the dependent variable equals one if the household is in Group 1 (receives the E1 form) and zero if the household is in Group 2 (does not receive the E1 form), some significant differences emerge.<sup>18</sup> Observables that are found to significantly affect Group 1 membership include whether the household head and spouse were agricultural laborers (both have negative effects), whether the household experienced an earthquake in the previous growing season (negative), whether the household received the *PROGRESA* manual (negative); and the number of *platica* meetings the household members missed (positive). It is worth noting specifically that shocks, such as earthquakes, have a negative and significant association with receipt of the E1 form, suggesting that some households simply could not attend the *platicas* where the E1 forms were distributed.

### Matching Results

Because these results suggest that nonreceipt of the E1 form may not have been completely random, we extend our analysis by using nearest neighbor matching. Matching methods provide reliable, low-bias estimates of program impact, provided that (1) the same data source is used for participants and

<sup>18</sup> We estimate these probits with exactly the same controls as found in column 6 of Table 4, except for the dependent variable. We also find significant differences if we estimate probits that attempt to explain Group 3 membership, using Group 4 as the control. In this regression, whether or not the head of household's spouse is literate, the logarithm of per capita consumption, whether or not the household includes a *PROGRESA promotora*, and the number of missed *platicas* all have significant influences on the probability of Group 3 membership.

nonparticipants, (2) participants and nonparticipants have access to the same markets, and (3) the data include meaningful  $X$  variables capable of identifying program participation and outcomes (Heckman, Ichimura, and Todd 1997, 1998; Heckman et al. 1998). The evaluation surveys clearly meet criterion (1). Criterion (2) is met by restricting the set of households/children that could not be monitored to be potentially selected as comparison observations to those households/children that were monitored and who lived in the same localities where *PROGRESA* was providing benefits and who were also already enrolled in *PROGRESA*. The evaluation surveys fielded in May-June 1999 as well as those fielded prior to that date provide a very rich set of variables that we use to identify receipt of the E1 forms, as required by criterion (3).

The estimator can be derived following Heckman, Ichimura, and Todd (1997) and Smith and Todd (2001, 2005). Let  $Y_t^1$  be a child's enrollment status in time period  $t$  if her household receives the E1 form and  $Y_t^0$  be that child's enrollment status in time period  $t$  if the household does not receive the form. The impact of receipt of the form (the imposition of conditionality) is simply the change in the outcome:  $\Delta = Y_t^1 - Y_t^0$ . However, for each child, only  $Y_t^1$  or  $Y_t^0$  is observed in any period,  $t$ . Let  $D$  be an indicator variable equal to 1 if the household receives the E1 form and 0 otherwise. In the evaluation literature,  $D$  is an indicator of receipt of the "treatment." We would like to construct an estimate of the average impact of imposing conditionality (via the receipt of the E1 form) on those that receive it, that is, the average impact of the treatment on the treated (*ATT*):

$$ATT = E(\Delta | X, D = 1) = E(Y_t^1 - Y_t^0 | X, D = 1) = E(Y_t^1 | X, D = 1) - E(Y_t^0 | X, D = 1), \quad (1)$$

where  $X$  is a vector of observable control variables. Because  $E(Y_t^0 | X, D = 1)$  is not observed, we estimate the impact using nearest neighbor matching with replacement to estimate the counterfactual outcome for participants (Abadie and Imbens 2006). The validity of any matching approach rests in part on two important assumptions. First, we assume "conditional mean independence," implying that conditional on the observables ( $X$ ), nonrecipients have the same mean outcomes as recipients would have if they did not receive the E1 form. Second, we assume that for all  $X$ , the probability of receiving the treatment cannot be exactly zero or one. If these assumptions are not violated, nearest neighbor matching with a bias correction is root- $N$  consistent for estimates of the *ATT* (Abadie and Imbens 2007).

We follow the following procedure to come up with our matching estimates. First, we estimate the propensity score for receipt of the E1 form (or receipt of the E1 form and a lack of knowledge of conditions) using a probit model. The controls include all of the variables in column 6 of Table 4.<sup>19</sup> We initially ensure that the propensity scores balance; that is, we test whether or not the treatment and comparison observations have the same distribution (mean) of propensity scores and of control variables within the quantiles of the propensity score. All results presented below are based on specifications that pass balancing tests. The distributions of propensity scores overlap each other for almost all of the ranges for Groups 1 and 2 (Figure 6) and Groups 3 and 4 (Figure 7).

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<sup>19</sup> The one difference is that we use child age as a continuous variable rather than as a set of dummy variables, to ensure that pass balancing tests.

Figure 6. Kernel density of propensity scores, by receipt of an E1 form

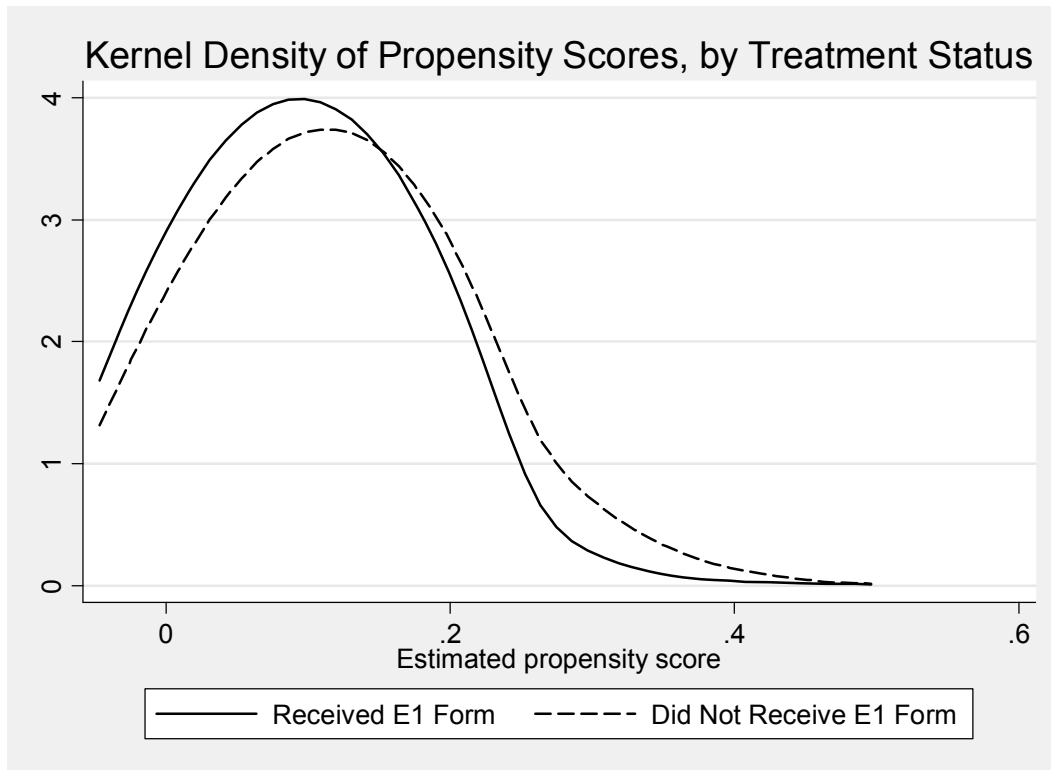
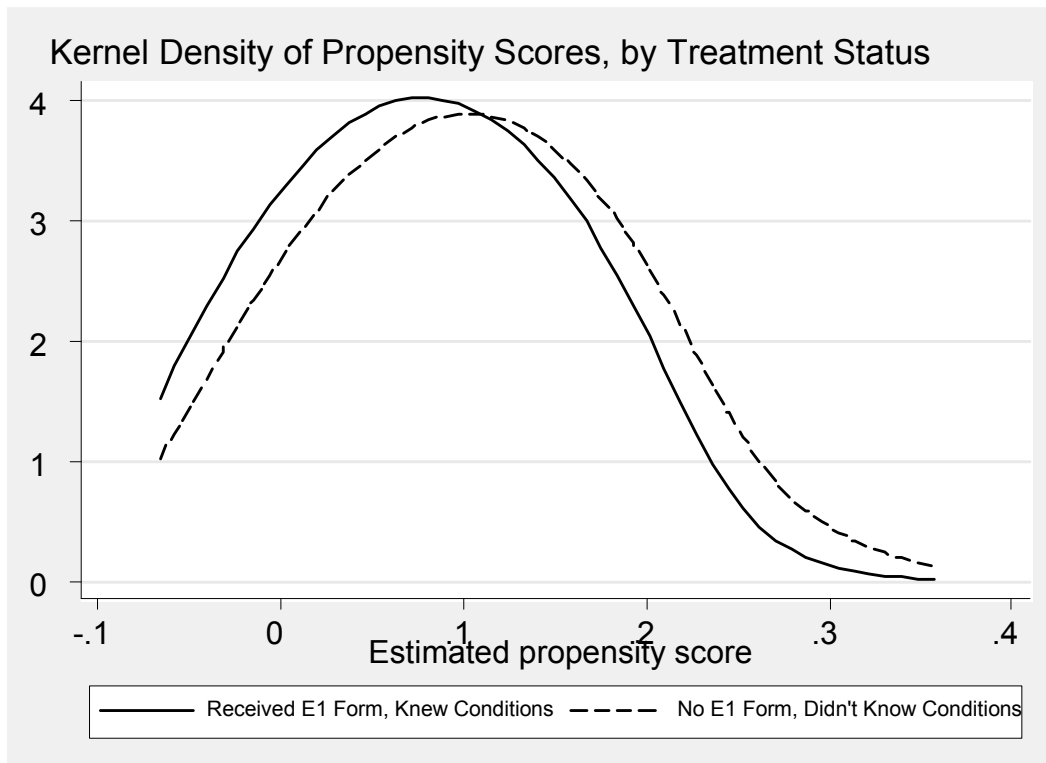


Figure 7. Kernel density of propensity scores, by whether or not households received the E1 form and whether they could recite *PROGRESA* conditions



We then match treatment and control observations using nearest neighbor matching with bias adjustment (Abadie and Imbens 2007).<sup>20</sup> The estimator matches each observation to its four nearest neighbors with replacement, and standard errors account for heteroscedasticity.<sup>21</sup> We provide estimates based both on the full sample for which common support exists (Table 7, column 1) and on a trimmed sample, which minimizes the variance of the estimator by trimming observations with theoretically imprecise estimates of the propensity score. To determine the optimal amount of trimming, we compute the variance for trims at 0.01 intervals from 0 to 0.1, using the formula found in Crump et al. (2006). This calculation reveals that we should drop observations with a propensity score below 0.04 for comparison of Groups 1 and 2 and below 0.03 for comparison of Groups 3 and 4.<sup>22</sup>

**Table 7. Matching estimates of the impact of receiving E1 forms on school enrollment for the full sample and by grade obtained**

Sample used	Treatment: Group 1 (households that did not receive the E1 form)		Treatment: Group 3 (households did not receive the E1 form and did not know conditions)	
	Full sample	Trimmed sample	Full sample	Trimmed sample
	(1)	(2)	(3)	(4)
Full sample of children completing	-0.072 (0.018)**	-0.072 (0.019)**	-0.092 (0.028)**	-0.096 (0.029)**
By grade				
Completed Grade 3	-0.007 (0.013)	-0.007 (0.013)	-0.010 (0.023)	-0.010 (0.023)
Completed Grade 4	-0.025 (0.026)	-0.024 (0.027)	-0.037 (0.045)	-0.039 (0.048)
Completed Grade 5	0.011 (0.020)	0.011 (0.020)	0.007 (0.034)	0.006 (0.034)
Completed Grade 6	-0.158 (0.048)**	-0.160 (0.048)**	-0.185 (0.064)**	-0.189 (0.066)**
Completed Grade 7-8	-0.027 (0.043)	-0.053 (0.045)	-0.131 (0.079)*	-0.143 (0.076)*

Notes: Matching by nearest neighbor with bias correction (see Abadie and Imbens 2006, 2007). Standard errors in parentheses are corrected for heteroscedasticity. In columns (2) and (4), the sample was trimmed to minimize the variance of estimation, using the procedure described in Crump et al. (2006). We trim any observations with a propensity score below 0.04 in column (2) and below 0.03 in column (4). \* significant at the 5 percent level; \*\* significant at the 1 percent level.

The matching estimates are remarkably similar to those found with the simple differences and the estimated coefficients from the probits (Table 7). On average, the matching results suggest that children in households that did not receive the E1 form are 7.2 percentage points less likely to enroll in school (Table 7, column 2), and nonreceipt of the E1 form coupled with the lack of knowledge of *PROGRESA* conditions reduces the enrollment likelihood by 9.6 percentage points (Table 7, column 4). As with the results discussed above, we find a great deal of heterogeneity when we estimate separate coefficients for children by grade completed.<sup>23</sup> When we do so, we find that the effect is again largest when children

<sup>20</sup> We use nearest neighbor matching as opposed to other matching methods because it is root-N consistent when we adjust for the potential bias in convergence, it works particularly well when the number of treatment observations is small relative to the control, and because it avoids making parametric assumptions about relationships between the *X* variables in the model. Nonetheless, when we use propensity score estimation with local linear regression or kernel matching, as preferred by some authors (Frölich 2004; Smith and Todd 2005), our results are nearly identical.

<sup>21</sup> The results are robust to using one-to-one matching or additional nearest neighbor matches.

<sup>22</sup> Theoretically, we would have also dropped any observations with propensity scores above 0.96 for the comparison of Groups 1 and 2 and above 0.97 for Groups 3 and 4, but none of the estimated propensity scores exceed 0.5 in these data.

<sup>23</sup> Because very few children who had completed grade 8 were included in Group 3, we estimate matching results for completion of grades 7 and 8 together, including a dummy variable for completion of grade 7 in the estimation of the propensity

transition from primary to lower secondary school, and there is some suggestion that nonreceipt of the E1 form together with absence of knowledge of conditions has an even larger effect on attendance than nonreceipt alone.<sup>24</sup> Furthermore, the estimated coefficients are remarkably consistent with the unconditional means and the results from the probits. As with the probit results, we find no evidence that conditionality affects continuing primary school enrollment. In results not reported here, we assessed whether these results differed by gender, but did not find large differences between males and females in the magnitudes of these effects.

### Three Further Robustness Checks

Our principal finding is that, on average, receipt of the E1 form increases the likelihood that children are enrolled in school. The average effect masks significant heterogeneity across children in different grade levels; there appears to be little effect of E1 form receipt among children continuing primary school, while the effect of receiving the E1 form is quite large for children making the transition from primary to lower secondary school. The results are remarkably consistent whether we consider simple descriptive statistics, probit regressions, or nearest neighbor matching. As such, these results are robust even after we condition on a wide range of observable characteristics. However, as is well known, these approaches do not condition out unobservable characteristics. Households that did not receive the E1 form may differ from other households in subtle ways. For example, household members may not understand how the program is supposed to work, or they may be recalcitrant individuals who simply do not like having to follow rules or procedures, such as going to meetings to pick up forms, or sending their children to school because the government tells them to do so. To further ensure that our results are not driven by household- or individual-level unobservables, we perform three additional robustness checks.

First, we assess whether selection on unobservables could explain our results by computing an informal test statistic suggested by Altonji, Elber, and Taber (2005), who demonstrate how to estimate the ratio of selection on unobservables to observables that would be necessary to explain an entire coefficient estimate of interest. We calculate this statistic for both the average effect on enrollment using the variables in column 6 of Table 4, and for children who have completed grade 6 in Table 5. To fully explain the coefficients found in Table 4, selection on unobservables into Group 1 would have to be 9 to 13 times larger than selection on observables, and 6 to 7 times larger to fully explain the result for children completing grade 6.<sup>25</sup> Even if household unobservables have a positive bias on our estimates, the effect of selection on unobservables cannot be large enough to account for the entire estimated coefficients.

Second, we exploit the fact that some households have more than one child in grades 3 to 8. While our treatment is unobserved at the household level, the results from Tables 5 and 7 tell us that the impact of the treatment varies by the grade attainment of the child. Therefore, we can interact the last completed grade with either Group 1 or 3 membership and use the linear probability model to regress enrollment on completed grade level, the interactions described above, and household-level fixed effects (Table 8).<sup>26</sup> Whether or not we control for age dummies and the child's gender, the coefficients we estimate on the interaction between either Group 1 or 3 membership and completion of grade 6 are negative, statistically significant, and strikingly consistent with the coefficients estimated with either

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score.

<sup>24</sup> We also explore whether, conditional on enrollment, receipt of the E1 form increases attendance. In general, we find a positive effect, but it is not statistically significant.

<sup>25</sup> In a linear probability model version of the regression in column 6 of Table 4, the  $R^2$  is 0.2, meaning that selection on observables accounts for 20 percent of the variation in enrollment. This finding indicates that even if selection on unobservables accounts for the remaining 80 percent of the variation in enrollment, the coefficient for Group 1 membership would still be negative. For grade 6 completion, the  $R^2$  is 0.29, again indicating that selection on unobservables could not completely eliminate the negative coefficient.

<sup>26</sup> Note that the inclusion of household fixed effects here accounts for any household-level unobservables that we cannot account for in the probit or matching models. For example, one might argue that if the survey respondent frequently consumes a lot of alcohol, he/she could be less likely to enroll his/her children in school. This is captured in the household fixed effect.

probits or nearest neighbor matching.<sup>27</sup> As these regressions account for household-level unobservables with the household-level fixed effects, they provide strong evidence that unobservables do not drive our results.

Third, we consider an indirect approach. As part of the *PROGRESA* program, beneficiaries had to attend *platicas* where information and training on health, good diets, and nutrition was given by a doctor and/or nurse from the health clinic serving the community. While Hoddinott and Skoufias (2004) show that attendance at *platicas* was causally associated with the acquisition of calories from fruits, vegetables, and animal products, even after controlling for *PROGRESA*'s income effect, eating a better diet was *encouraged* but not *monitored*. This suggests the following robustness check: does receipt of the E1 form affect food acquisition? Our null hypothesis is that conditioning educational transfers should not change caloric acquisition. Since the conditions attached to schooling have nothing to do with patterns of food consumption, rejecting this null would suggest that receipt/nonreceipt of the E1 form is actually capturing some sort of unobservable characteristic such as those described above.

**Table 8. Impact of receipt of E1 forms, by grade completed, OLS with household fixed effects**

Specification	Treatment: households that did not receive forms		Treatment: households that did not receive forms and did not know conditions	
	(1)	(2)	(3)	(4)
Completed Grade 4	-0.016 (0.86)	0.013 (0.67)	0.003 (0.12)	0.031 (1.33)
Completed Grade 5	-0.026 (1.90)*	0.054 (2.09)**	-0.017 (0.93)	0.063 (2.04)**
Completed Grade 6	-0.303 (14.74)**	-0.056 (1.68)*	-0.267 (9.59)**	-0.025 (0.70)
Completed Grade 7	-0.126 (6.42)**	0.182 (4.87)**	-0.116 (3.78)**	0.208 (4.09)**
Completed Grade 8	-0.161 (7.51)**	0.295 (6.93)**	-0.139 (4.71)**	0.338 (7.19)**
Completed Grade 4*treatment	-0.034 (0.56)	-0.018 (0.35)	-0.099 (1.18)	-0.055 (0.69)
Completed Grade 5*treatment	0.059 (1.33)	0.025 (0.59)	0.007 (0.09)	-0.028 (0.40)
Completed Grade 6*treatment	-0.178 (2.85)**	-0.183 (3.18)**	-0.231 (2.81)**	-0.211 (2.75)**
Completed Grade 7*treatment	-0.03 (0.55)	-0.035 (0.57)	-0.274 (2.04)**	-0.146 (1.10)
Completed Grade 8*treatment	0.043 (0.84)	0.002 (0.03)	-0.066 (0.88)	-0.071 (0.82)
Age, Gender Dummies?	No	Yes	No	Yes
Number of observations	5,656	5,656	3,131	3,131

Notes: Regressions are estimated using ordinary least squares with household-level fixed effects. Standard errors are clustered at the locality level. \* indicates significance at the 10 percent level; \*\* indicates significance at the 5 percent level.

<sup>27</sup> Although the fixed effects regression accounts for household-level unobservables, the results could potentially be explained if initial lower secondary school enrollment is more sensitive to other variables that might be correlated with the lack of forms. To test whether lower secondary school enrollment is simply more sensitive, we interact several variables (for example, income and literacy of the household head) with the levels of grade completion and reestimate the household fixed effects model. The estimated coefficients are typically insignificant; where we find significant coefficients (for example, substituting the literacy of the head for Group 1 enrollment), we also include Group1 enrollment interacted with grade completion, and find no qualitative difference between the reported coefficients in Table 8 and the estimated coefficients.



The May 1999 survey contained a set of questions of the following form: “In the last seven days, how much have you consumed of the following foods.” This question was asked with reference to 35 different foods. To convert these data into calories, disparate units of measurement were converted into a common measure for each food item. Volumes were converted to weights using the *Tablas de Valor Nutrivo* for Mexico (Muñoz de Chávez et al. 1996). The acquisition of each food item, expressed in kilograms, was multiplied by the percentage weight of the food deemed edible, and these edible kilograms of food were converted to kilocalories, again based on information found in the *Tablas de Valor Nutrivo*. These 35 food variables and their aggregate, calories per family per week were then converted to daily amounts and divided by household size to yield caloric availability per person per day. Hoddinott and Skoufias (2004) describe this procedure in more detail.

Using both the same matching technique as above and an Ordinary Least Squares regression, we consider the impact of receipt of the E1 form on total calorie consumption per capita as well as calories from the following food groups: grains, fruits and vegetables, animal products, and other foods (Table 9). The estimation results demonstrate that receipt of the E1 form does not affect the acquisition of calories, and, in particular, does not affect the acquisition of calories from sources such as fruits, vegetables, and animal products, which Hoddinott and Skoufias (2004) show are affected by exposure to the *platicas*. As such, it is unlikely that recalcitrant individuals unlikely to follow directions were more likely to not receive the E1 form. From our perspective, this exercise provides further indirect evidence that our findings are related to conditionality and not unobserved household characteristics.

**Table 9. Estimates of the impact of receiving E1 forms on household caloric access, by type of food**

Sample used	Treatment: households that did not receive forms	
	Matching	OLS
Total calorie consumption	48.6 (32.6)	76.1 (50.8)
Calories from grains	42.2 (31.0)	65.8 (49.9)
Calories from fruit and vegetables	0.53 (1.51)	0.20 (1.92)
Calories from animal products	-2.80 (5.47)	1.57 (6.18)
Calories from other foods	8.72 (6.09)	8.52 (7.26)

Notes: Standard errors are robust using nearest neighbor matching and are clustered at the *municipio* level in the OLS regression. \* significant at the 5 percent level; \*\* significant at the 1 percent level.

### Heterogeneity by Parental Characteristics

We have shown that the effect of receiving the E1 form is heterogeneous by grade level completed; in this subsection, we explore whether or not coefficient estimates are heterogeneous by parental characteristics. We explore heterogeneity using three dimensions: whether or not the household head is literate, whether or not the head is an agricultural laborer; and whether or not the head is indigenous. If the household head is not literate, *PROGRESA* might not be as well understood by the household, and as a result one might think that the lack of form E1 might have stronger effect on enrollment. Indigenous households might have a similar problem understanding the program. If the head is not an agricultural laborer, he or she might have additional information about off-farm jobs, and therefore households that include children completing primary school might perceive a higher opportunity cost of continuing in lower secondary school.

We add an interaction between the three indicator variables and Group 1 and 3 membership, sequentially, in probit regressions (Table 10). We use both the whole sample (Table 10, columns 1, 3, and 5), and the sample of children who had completed grade 6 (Table 10, columns 2, 4, and 6).<sup>28</sup> We find some evidence that literacy matters, and the estimated coefficients suggest that literacy matters a great deal, particularly for children who have completed grade 6. The marginal effect is only significant for the comparison of Groups 1 and 2 (Table 10, Panel A), but it implies that among children living in households that did not receive the E1 form, a child with a literate household head is 27 percentage points more likely to enroll in school than a child with an illiterate head. Children in households with illiterate heads are 46 percentage points less likely to enroll in lower secondary school. Clearly, conditionality is particularly important for such households. We also find some evidence that when the head has off-farm work, conditionality is more important. When households did not receive the E1 form or understand the conditions, children completing grade 6 were more than 40 percentage points less likely to enroll in lower secondary school, while children whose parents were agricultural laborers were only 16 percentage points less likely to enroll (Table 10, Panel B, rows 1 and 3).

**Table 10. Probit estimates of the impact of receiving E1 forms on school enrollment, by literacy of head, agricultural labor, and indigenous status**

**Panel A: Groups 1 and 2 (Group 1 did not receive E1 forms)**

Sample	Completed		Completed		Completed	
	Grades 3-8	Grade 6	Grades 3-8	Grade 6	Grades 3-8	Grade 6
	(1)	(2)	(3)	(4)	(5)	(6)
Member of Group 1 (1 = yes)	-0.093 (3.35)**	-0.461 (3.81)**	-0.076 (3.65)**	-0.334 (4.40)**	-0.032 (2.00)*	-0.212 (2.91)**
Group 1*Head is literate	0.068 (1.50)	0.270 (2.17)*				
Group 1*Head is agricultural laborer			0.044 (1.29)	0.131 (1.58)		
Group 1*Head is indigenous					-0.037 (0.81)	-0.003 (0.01)
Head is literate (1 = yes)	0.012 (1.15)	-0.019 (0.46)	0.017 (1.86)	0.016 (0.38)	0.017 (1.89)	0.017 (0.41)
Head is agricultural laborer (1 = yes)	<0.001 (0.01)	0.01 (0.29)	-0.004 (0.44)	-0.01 (0.27)	<0.001 (0.00)	0.008 (0.26)
Head is indigenous (1 = yes)	0.028 (2.86)**	0.131 (3.19)**	0.028 (2.89)**	0.132 (3.21)**	0.032 (3.09)**	0.135 (3.03)**
Observations	5,503	1,308	5,503	1,308	5,503	1,308

<sup>28</sup> Estimating the marginal effects using probit regressions is not trivial, due to the nonlinearity of the estimator. We use the procedure developed by Norton, Wang, and Ai (2004) to compute the marginal effects and their standard errors.

**Table 10. Continued**

**Panel B: Groups 3 and 4 (Group 3 did not receive E1 forms and did not know conditions)**

Sample	Completed		Completed		Completed	
	Grades 3-8	Grade 6	Grades 3-8	Grade 6	Grades 3-8	Grade 6
	(1)	(2)	(3)	(4)	(5)	(6)
Member of Group 3 (1 = yes)	-0.142 (4.16)**	-0.386 (2.56)*	-0.143 (4.39)**	-0.408 (3.47)**	-0.052 (2.40)*	-0.174 (2.13)*
Group 3*Head is literate	0.095 (1.64)	0.202 (1.37)				
Group 3*Head is agricultural laborer			0.107 (1.82)	0.248 (2.06)*		
Group 3*Head is indigenous					-0.043 (0.67)	0.015 (0.22)
Head is literate (1 = yes)	0.007 (0.56)	-0.028 (0.51)	0.012 (1.14)	-0.006 (0.13)	0.013 (1.20)	-0.004 (0.09)
Head is agricultural laborer (1 = yes)	0.006 (0.55)	0.016 (0.33)	0 (0.00)	-0.015 (0.28)	0.006 (0.58)	0.015 (0.31)
Head is indigenous (1 = yes)	0.024 (1.87)	0.125 (2.58)**	0.024 (1.92)	0.128 (2.65)**	0.028 (2.13)*	0.128 (2.48)*
Observations	3,071	715	3,071	715	3,071	715

Notes: All coefficients presented are marginal effects; interaction terms are computed using the procedure outlined in Norton, Wang, and Ai (2004). t-statistics based on standard errors accounting for clustering at the locality are given in parentheses. \* indicates significance at the 5 percent level; \*\* indicates significance at the 1 percent level.

## 4. CONCLUSION

A growing body of evidence suggests that conditional cash transfer programs can have powerful positive effects on a wide range of welfare indicators. However, there is much less evidence on the specific contributions that individual components of these programs make toward achieving these outcomes. The contribution of this paper is to assess the impact of conditionality on one dimension of human capital formation, school enrollment, using data from Mexico's *PROGRESA* CCT program. We exploit the fact that some *PROGRESA* beneficiaries did not receive the form needed to monitor their children's school attendance. Using a variety of techniques, we show that the absence of this form reduced the likelihood that children attended school, on average, and the likelihood of school attendance was severely reduced when children were making the transition to lower secondary school. We use a variety of techniques to ensure that our findings are not driven by unobservable household characteristics.

These results speak directly to policy debates regarding the merits of conditionality within CCT programs. They suggest that debates over "to condition or not to condition" are overly simplistic. In the case considered here, there is clearly little benefit to conditioning transfers based on enrollment in primary school. However, in terms of increased school enrollment, there are large benefits associated with conditioning transfers at entry into lower secondary school. As such, these findings are consistent with the more general argument advanced in de Janvry and Sadoulet (2006), namely that there can be considerable efficiency gains to CCTs by calibrating their design more carefully. That said, additional study of this topic would be worthwhile. Two issues would seem to be particularly valuable to explore. First, an experimental design—where conditionality was randomly assigned—would bolster the evidence base while removing any lingering doubts about the role of unobservables. Second, an experimental design in which the intensity by which information on conditions was varied across beneficiaries would allow policymakers to assess whether the effectiveness of conditionality can be strengthened.

## APPENDIX: SUPPLEMENTARY TABLES

**Table A.1. Probit estimates of the impact of nonreceipt of the E1 form on school enrollment of children who had completed grades 3-8, comparing Group 1 (did not receive the E1 form) with Group 2 (received the E1 form)**

	(1)	(2)	(3)	(4)	(5)	(6)
Group 1 (1=yes)	-0.054 (3.37)**	-0.046 (3.68)**	-0.045 (3.53)**	-0.046 (3.62)**	-0.049 (3.79)**	-0.044 (3.56)**
Child characteristics						
Gender (1=male)		0.011 (1.63)	0.011 (1.67)	0.011 (1.62)	0.01 (1.48)	0.01 (1.54)
Child is 9 years old		0.053 (1.32)	0.052 (1.33)	0.049 (1.21)	0.049 (1.18)	0.048 (1.22)
Child is 10 years old		0.049 (1.20)	0.049 (1.21)	0.044 (1.05)	0.043 (1.02)	0.044 (1.05)
Child is 11 years old		0.003 (0.06)	0.004 (0.07)	-0.003 (0.06)	-0.002 (0.04)	0.003 (0.06)
Child is 12 years old		-0.034 (0.58)	-0.033 (0.56)	-0.041 (0.66)	-0.041 (0.67)	-0.035 (0.58)
Child is 13 years old		-0.129 (1.70)	-0.127 (1.66)	-0.134 (1.70)	-0.136 (1.71)	-0.123 (1.56)
Child is 14 years old		-0.244 (2.75)**	-0.239 (2.69)**	-0.251 (2.73)**	-0.253 (2.73)**	-0.244 (2.60)**
Child is 15 years old		-0.384 (3.65)**	-0.381 (3.57)**	-0.393 (3.59)**	-0.397 (3.60)**	-0.382 (3.37)**
Child is 16 years old		-0.558 (4.48)**	-0.559 (4.39)**	-0.571 (4.40)**	-0.57 (4.36)**	-0.556 (4.14)**
Parental characteristics						
Logarithm, age of household head			0.023 (1.27)	0.023 (1.28)	0.02 (1.04)	0.017 (0.96)
Head is female (1=yes)			0.027 (2.32)*	0.019 (1.51)	0.018 (1.46)	0.017 (1.43)
Head is literate			0.018 (2.00)*	0.018 (1.99)*	0.018 (2.02)*	0.019 (2.04)*
Head is agricultural laborer			<0.001 (0.06)	0.001 (0.10)	<0.001 (0.03)	0.001 (0.09)
Head is indigenous			0.032 (3.15)**	0.03 (3.05)**	0.029 (2.91)**	0.029 (2.95)**
Spouse of head is indigenous			0.008 (0.64)	0.009 (0.69)	0.008 (0.60)	0.005 (0.37)
Spouse of head is literate			0.023 (3.23)**	0.023 (3.25)**	0.022 (2.96)**	0.021 (2.94)**
Household characteristics, measured in October 1998						
Logarithm, per capita consumption				-0.005 (0.69)	-0.006 (0.84)	-0.005 (0.73)
Logarithm, household size				-0.047 (4.11)**	-0.048 (4.03)**	-0.044 (3.77)**

**Table A.1. Continued**

	(1)	(2)	(3)	(4)	(5)	(6)
Additional household characteristics, including shocks						
Household experienced drought					-0.002 (0.21)	-0.001 (0.14)
Household experienced flood					-0.028 (0.72)	-0.028 (0.75)
Household experienced freezing crops					-0.012 (0.68)	-0.004 (0.22)
Household experienced fire					0.002 (0.05)	0.003 (0.08)
Household experienced crop epidemics					0.011 (0.79)	0.015 (1.14)
Household experienced earthquake tremors					0.015 (0.86)	0.008 (0.50)
Received <i>PROGRESA</i> book					<0.001 (0.03)	0.001 (0.05)
Received Health Register					-0.006 (0.23)	<0.001 (0.01)
Was a <i>PROGRESA</i> promoter					0.012 (0.78)	0.014 (0.92)
<i>Platicas</i> held					0.001 (0.43)	<0.001 (0.05)
<i>Platicas</i> missed					0.001 (0.40)	0.002 (0.49)
Community characteristics						
Community has electricity						0.023 (2.21)*
Community has primary school						-0.012 (0.82)
Community has lower secondary school						0.042 (4.92)**
Community has secondary school						-0.045 (1.60)
State dummies?	yes	yes	yes	yes	yes	yes
Number of observations	5,637	5,637	5,608	5,608	5,503	5,503

Notes: Group 1 refers to households that did not receive the E1 form. The results of these regressions are the full results corresponding to Table 4, Panel A; standard errors are clustered at the locality. \* indicates significance at the 5 percent level; \*\* indicates significance at the 1 percent level.

**Table A.2. Probit estimates of the impact of nonreceipt of the E1 form on school enrollment of children who had completed grades 3-8, comparing Group 3 (did not receive the E1 form and did not know conditions) with Group 4 (received the E1 form and knew conditions)**

	(1)	(2)	(3)	(4)	(5)	(6)
Group 3 (1=yes)	-0.09 (4.23)**	-0.067 (3.97)**	-0.064 (3.90)**	-0.066 (3.95)**	-0.075 (4.08)**	-0.07 (3.95)**
Child characteristics						
Gender (1=male)		0.006 (0.69)	0.006 (0.69)	0.006 (0.67)	0.005 (0.57)	0.006 (0.67)
Child is 9 years old		0.053 (1.31)	0.054 (1.40)	0.053 (1.40)	0.052 (1.39)	0.051 (1.37)
Child is 10 years old		0.064 (1.69)	0.065 (1.76)	0.064 (1.72)	0.062 (1.69)	0.059 (1.61)
Child is 11 years old		0.032 (0.67)	0.034 (0.74)	0.034 (0.73)	0.034 (0.75)	0.034 (0.74)
Child is 12 years old		0.002 (0.03)	0.006 (0.11)	0.006 (0.10)	0.003 (0.05)	0.003 (0.06)
Child is 13 years old		-0.054 (0.77)	-0.046 (0.67)	-0.044 (0.64)	-0.048 (0.70)	-0.045 (0.66)
Child is 14 years old		-0.17 (2.00)*	-0.157 (1.88)	-0.155 (1.84)	-0.159 (1.89)	-0.158 (1.84)
Child is 15 years old		-0.287 (2.76)**	-0.27 (2.60)**	-0.267 (2.56)*	-0.277 (2.66)**	-0.271 (2.54)*
Child is 16 years old		-0.431 (3.35)**	-0.416 (3.23)**	-0.415 (3.19)**	-0.422 (3.25)**	-0.416 (3.13)**
Parental characteristics						
Logarithm, age of household head			0.027 (1.33)	0.027 (1.32)	0.026 (1.21)	0.026 (1.32)
Head is female (1=yes)			0.025 (1.53)	0.022 (1.29)	0.015 (0.90)	0.015 (0.94)
Head is literate			0.012 (1.01)	0.012 (1.02)	0.014 (1.22)	0.015 (1.39)
Head is agricultural laborer			0.007 (0.62)	0.007 (0.62)	0.007 (0.57)	0.008 (0.72)
Head is indigenous			0.03 (2.23)*	0.029 (2.26)*	0.025 (1.97)*	0.027 (2.20)*
Spouse of head is indigenous			0.016 (0.97)	0.016 (0.99)	0.015 (0.91)	0.011 (0.64)
Spouse of head is literate			0.025 (2.46)*	0.025 (2.56)*	0.021 (2.12)*	0.02 (2.06)*
Household characteristics, measured in October 1998						
Logarithm, per capita consumption				-0.014 (1.51)	-0.016 (1.98)*	-0.017 (2.13)*
Logarithm, household size				-0.029 (1.68)	-0.033 (1.96)*	-0.034 (2.10)*

**Table A.2. Continued**

	(1)	(2)	(3)	(4)	(5)	(6)
Additional household characteristics, including shocks						
Household experienced drought					-0.008 (0.97)	-0.005 (0.64)
Household experienced flood					-0.014 (0.43)	-0.018 (0.58)
Household experienced freezing crops					-0.018 (1.11)	-0.008 (0.56)
Household experienced fire					0.033 (0.85)	0.033 (0.93)
Household experienced crop epidemics					0.003 (0.21)	0.008 (0.58)
Household experienced earthquake tremors					0.03 (1.80)	0.022 (1.36)
Received PROGRESA book					-0.019 (1.61)	-0.018 (1.51)
Received Health Register					0.024 (0.55)	0.023 (0.50)
Was a PROGRESA promoter					0.011 (0.66)	0.012 (0.72)
<i>Platicas</i> held					-0.003 (0.69)	-0.004 (0.99)
<i>Platicas</i> missed					0.005 (0.82)	0.006 (0.98)
Community characteristics						
Community has electricity						0.022 (1.85)
Community has primary school						-0.016 (0.78)
Community has lower secondary school						0.033 (2.98)**
Community has secondary school						-0.048 (3.04)**
State dummies?	yes	yes	yes	yes	yes	yes
Number of observations	3,131	3,131	3,121	3,121	3,71	3,071

Notes: Group 3 refers to households that did not receive the E1 form and did not know conditions. The results of these regressions are the full results corresponding to Table 4, Panel B; standard errors are clustered at the locality. \* indicates significance at the 5 percent level; \*\* indicates significance at the 1 percent level.



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