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Journal of Development Economics xxx (2010) xxx–xxx

Contents lists available at ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Must conditional cash transfer programs be conditioned to be effective? The impact of conditioning transfers on school enrollment in Mexico

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ARTICLE INFO

Article history:
Received 9 November 2007
Received in revised form 16 August 2010
Accepted 18 August 2010
Available online xxxx

Keywords:
Conditionality
Cash transfers
Matching
PROGRESA
School enrollment

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, there is little evidence about how important each component of these programs is towards achieving these outcomes. This paper tests the importance of conditionality on one specific outcome related to human capital formation, school enrollment, using data collected during the evaluation of Mexico's *PROGRESA* program. We exploit the fact that some beneficiaries who received transfers did not receive the forms needed to monitor the attendance of their children at school. We use a variety of techniques, including nearest neighbor matching and household fixed effects regressions, to show that the absence of these forms reduced the likelihood that children attended school with this effect most pronounced when children are transitioning to lower secondary school. We provide substantial evidence that these findings are not driven by unobservable characteristics of households or localities.

1. Introduction

Conditional cash transfers (CCTs) have become a popular tool for poverty alleviation throughout the developing world. As implied by their name, CCTs give cash transfers to households that meet specific conditions or undertake certain actions such as ensuring school-aged children go to school or ensuring that pre-school children regularly see a nurse or doctor. Many of these programs have been carefully evaluated to demonstrate their effectiveness.[1] However, evaluations usually such evaluations treat the CCT as a "black box," assessing the combined effect of all their components without considering which features make them successful at improving child or household welfare. As a result, little is known about whether the imposition of conditions on beneficiaries improves the effectiveness of CCTs, an issue of considerable controversy.

Both public and private perspectives provide good reasons for CCTs to be conditional. From the public perspective, governments may perceive that they know what actions or behaviors will benefit the poor better than the poor do themselves, and that conditioning transfers can modify behavior to better match those perceptions. For example, governments may place greater weight on the intrinsic value of educating girls than do families. 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 help required for political economy reasons. Politicians and policy makers are often evaluated by performance indicators such as changes in school enrollment or use of health clinics. By conditioning transfers on behaviors that increase these indicators, politicians and policy makers can potentially demonstrate accomplishments long before the more important evidence of poverty reduction, in the form of increased productivity or better adult health, occurs. Therefore, politicians can perceive that conditioning 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. 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, who may otherwise lack bargaining power within the household. Conditioning may overcome stigma effects otherwise associated with welfare payments. The stigma attached to welfare payments may discourage those with valid claims from taking them up. From the beneficiary's point of view, conditioning can be seen as part of a social contract between themselves and the state and may legitimize the transfer, overcoming the stigma. Finally, work in behavioral economics emphasizes that when households have

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[1] Fiszbein and Schady (2009) and Adato and Hoddinott (2010) provide summaries of many CCT impact evaluations.

0304-3878/$ – see front matter
doi:10.1016/j.jdeveco.2010.08.014

hyperbolic discount functions, they undertake actions that can reduce their own welfare (Laibson, 1997). In such circumstances, households are better off when constraints are imposed that reduce or limit their ability to trade-off future for present consumption. Conditionality can be seen as such a constraint.

There are drawbacks to imposing conditionality. Conditionality increases the administrative costs and complexity of running a cash transfer program. Caldes et al. (2006) show that monitoring conditionality represented approximately 18% of PROGRESA's administrative costs and 2% of total program costs. Meeting conditions imposes direct costs on beneficiaries, and such costs are not necessarily shared equally among household members; for example, mothers often accompany children to health clinics or attend community meetings (Molyneux, 2007). If preferences of the poor do not align with the conditions placed on their behavior, the restrictions that conditionality imposes on the poor reduce their welfare gains from the CCT. Some households may find the conditions too difficult to meet, and if such households are among the poorest households eligible for the program, imposing conditions may detract from the effectiveness of the CCT's targeting. 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. Conditioning transfers can be perceived as being demeaning to the poor; for example, conditioning can be understood to imply that the poor simply do not know what is good for them. Finally, because social protection falls under the Universal Declaration of Human Rights, some argue that it is indefensible to attach conditions to the receipt of social transfers (Freelander, 2007).

This paper brings empirical evidence to this debate. We exploit the fact that some beneficiaries of Mexico's pathbreaking CCT program, *PROGRESA*, did not receive the forms needed to monitor the attendance of their children at school and as a result, payments made to these households were effectively unconditional. In households where these forms were received, and payments were conditional on school attendance, we find that the likelihood that children attended school was higher. The effect of conditionality depends upon the grade level of the student; 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 non-receipt of forms is not random, we complete several robustness checks to ensure that our results are not due to unobserved heterogeneity at either the household or community level. We provide evidence that the effect is more pronounced among households with illiterate heads and among households in which the head did not perform agricultural labor, indicating the results may be partially due to informational problems and to the opportunity cost of schooling for such children.

2. Program description and data

PROGRESA was introduced by the Government of Mexico in 1997 as part of an effort to break the intergenerational transmission of poverty.[2] The program was primarily aimed at improving the educational, health and nutritional status of poor families, and particularly of children and their mothers. Beneficiaries received cash transfers on a bi-monthly basis, and transfers had three components: a scholarship tied to the continued attendance of children at school (the *beca*, or the education transfer), 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 education transfer, school-aged children in grades three and higher had to maintain an attendance record of 85% or better and parents had to attend monthly meetings (*platicas*). Parents were supposed to receive a form called the E1 in the general assembly when they were inducted into *PROGRESA* to ensure compliance with the attendance condition. This form was taken to the teacher, who signed the form to register the child, and parents returned the signed E1 form to *PROGRESA* officials (usually the local promoter). *PROGRESA* officials then were supposed to match E1 forms with school records of attendance (the E2 form). The E2 form was solely for *PROGRESA* enrollees and was kept separately from other attendance records. After confirming that attendance was satisfactory, officials arranged for the payment of the education transfer. Payments occurred bi-monthly; promoters spread word in the community that payments would occur on a certain date at a specific place, and *PROGRESA* officials then set up portable tables and handed envelopes holding payments to beneficiaries.

Our study hinges on the fact that a significant proportion of households report never receiving the E1 form, but the administrative data from *PROGRESA* indicate that these households received education transfers. As a result, payments to such households could not have been conditioned, since teachers would not have monitored their children's attendance on the E2 form. According to Adato et al. (2000), some households reported receiving education transfers, yet they report never having received the E1 form. It seems likely that as the program began, administrative failures allowed these transfers to occur; as the administration improved such households might have either received an E1 or been dropped from the payment rolls.[3] We provide evidence that households who report failing 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 matched data sources for our study. First, we use administrative data on education transfer payments made between March and August of 1999 to measure which households received transfers.[4] We then use household identifiers to match the households that received transfers with those that were interviewed in the evaluation surveys completed as part of *PROGRESA*. The bulk of the data we use are from the evaluation survey conducted in May and June of 1999 (the *seguimento*) which included a set of questions on beneficiaries experiences with *PROGRESA*.[5] The *seguimento* specifically asked households whether or not they had received the E1 form, as well as a series of questions about the conditions households were supposed to meet in order to receive transfers.[6] In our analysis, we use households that received the education transfer according to the administrative database on transfers and were found in the *seguimento*.[7]

We find that of the 4383 households that received at least one education transfer between March and August of 1999 for children's school attendance, 464 of them did not receive the E1 form. These

[2] The program was renamed *Oportunidades* when Vincente Fox became president of Mexico in 2000.

[3] We discussed this hypothesis 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, and that it was possible that households received transfers despite not having received the E1 form.

[4] 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.

[5] The sampling frame for the *seguimento* only included households with at least one child age 6 to 17.

[6] We also use several variables from the October 1998 evaluation survey round, such as per capita expenditures and household size.

[7] We drop localities from the sample if every household in the locality received an E1 form, as locality level dummy variables would fully explain whether they received the E1.

4383 households include 5686 children of school age (ages 8–16) who have completed grades 3 through 8 and are therefore eligible to be monitored. In the 464 households that did not receive the E1, children could not have had their attendance monitored by *PROGRESA*; we label these households as Group 1. The remaining 3919 households with school-aged children that received the E1 form and received at least one *beca* payment for children's school attendance between March and August 1999 are called Group 2. Households in Groups 1 and 2 share the following similarities: they are all beneficiaries of the *PROGRESA* program, they all have school-aged children, and they all received *beca* payments from *PROGRESA* for school attendance by their children. The difference is that the behavior of Group 1 could not be monitored and by extension, their transfers could not be conditioned on attendance. As such, comparing outcomes among children of households in Groups 1 and 2 constitute 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, one might be concerned that households who understood the conditions might assume that the program somehow monitored them, rendering the E1 form unnecessary. If true, the comparison of Groups 1 and 2 would not test the conditionality of the education transfer. To address this concern, we develop a second test of conditionality using the *seguimento.* It asked beneficiary households to list the conditions that they were required to fulfill in order to receive the education transfer. Some households could immediately list conditions, whereas others could not. With this information, we take the same sample of households and create a further 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 education transfer. Households in Group 4 received forms to enroll their children *and* knew that they were required to send their children to school in order to receive school benefits.[8] 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, whereas others did not. Some 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, households might have simply missed the meeting at which the E1 form was distributed, for potentially observable (for example, an environmental shock) or unobservable reasons.

To ensure that our results are due to the lack of conditionality rather than differences in either observables or fixed unobservables, we condition unconditional means between groups with differences in observable characteristics, using both probit and nearest neighbor matching methods. We ensure that the differences are not driven by a few specific communities by examining the receipt of E1 forms at both the state and locality level. 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 control for any fixed unobservable differences at the household level.

[8] To provide a cleaner comparison between Groups 3 and 4, for this comparison we drop all households that did not receive Form E1 but knew the conditions for receiving the *beca*, and all households that received Form E1 but did not know the conditions.

Table 1
Enrollment rates of children 8–16 who have completed grades 3–8, by household receipt of E1 forms.

Group	Sample size	Enrollment rate (%)	Wald test on differences in enrollment rate
1 (Household did not receive E1 form)	547	83.2	8.63**
2 (Household received E1 form)	5090	88.6	
3 (Household did not receive E1 form and could not describe conditions)	261	80.1	13.44**
4 (Household received E1 form and could describe conditions)	2870	89.2	

Notes: Wald test for equivalence of enrollment rates controls for intracluster correlation within localities. **—indicates significance at the 1% level.

3. Results

3.1. Basic findings

Among children 8–16 years of age who have completed grades 3–8, 83.2% of children in Group 1 households were enrolled in school, while 88.6% of children in Group 2 households were enrolled (Table 1).[9] Even after accounting for the clustered nature of the sample, this difference is statistically significant at the 5% level.[10] The difference is larger when we consider whether or not households understood the conditions. The enrollment rate among children in households in Group 3 was 80.1% compared to 89.2% for children in Group 4 households. The differences in mean enrollments are suggestive that conditionality does affect enrollment.

The unconditional means mask striking differences by grade level. We 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 by group (Table 3), and plot the differences between means for Groups 1 and 2 (Fig. 1) and Groups 3 and 4 (Fig. 2). The largest difference in school enrollment is between the groups for children who have completed grade 6; that is they finished primary school and should be entering lower secondary school. Children in households who did not receive Form E1 are much less likely—by 17 to 20%—to enroll in lower secondary school, whether or not parents are aware of the attendance conditionality. These differences are significant at the 1% 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. The data therefore suggest that conditionality is important when students move from primary to lower secondary school, but not necessarily at other levels. However, caveats regarding both observable and unobservable differences between households remain.

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). We include 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 observables we use as controls. We initially control for state of

[9] We use age 8 as the lower age cut-off as this is the lowest age where we observe children in grade 3, the first grade for which *PROGRESA* conditionality applied.

[10] There is only one round where we have information on Form E1 receipt, knowledge of conditionality *and* administrative data by type of transfer received. Therefore difference-in-difference estimation is inappropriate, as used in many papers on impacts of *PROGRESA* (e.g. Schultz 2004). Nonetheless, we examined average differences in school attendance between Groups 1 and 2 and 3 and 4 in earlier surveys and found no significant difference.

Please cite this article as: de Brauw, A., Hoddinott, J., Must conditional cash transfer programs be conditioned to be effective? The impact of conditioning transfers on school enrollment in Mexico, J. Dev. Econ. (2010), doi:10.1016/j.jdeveco.2010.08.014

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

Last grade level completed	Share in Group 1 (household did not receive Form E1)		Share in Group 3 (household did not receive Form E1 and could not describe conditions)	
	Share	Number of obs.	Share	Number of obs.
3	0.103	1278	0.085	691
4	0.091	1097	0.081	621
5	0.087	1022	0.070	575
6	0.107	1342	0.103	728
7	0.102	489	0.078	271
8	0.081	409	0.065	245

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

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 Form E1, and are compared with Group 2 households which did receive Form E1. Group 3 households did not receive Form E1 and could not describe the *PROGRESA* conditions, whereas Group 4 households both received Form E1 and could describe the conditions. **—indicates the difference between the share enrolled is significant at the 5% level, accounting for clustering.

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); additional household characteristics (an indicator that the household received the *PROGRESA* manual; whether or not the household had a health register; an indicator for households who served as *PROGRESA* promoters; and the number of meetings attended and missed by household members); household level shocks (indicators for shocks due to drought, flood, fire, frozen crops, crop disease and earthquake tremors); and finally, several community level characteristics (indicators for the presence of electricity, a pre-school, a lower secondary school, and a secondary school).[11]

Controlling for child characteristics, we find that children in households lacking an E1 form are 4.6% less likely to enroll in school, on average (Table 4, Panel A, column 2). Adding parental, household, and community controls has little effect on the magnitude of the estimated coefficient; when using the full set of controls, the results imply that the lack of an E1 form makes children 4.4% less likely to enroll in school, on average (Panel A, column 6). This difference is similar to the difference in unconditional means, 5.4%. 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 (Panel B, column 6) we find that children were 7.0% less likely to enroll in school on average, as compared to the unconditional difference of 9.1%.

The results in Table 4 do not account for potential heterogeneity in the effects of receiving E1. Therefore we 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, which are the children making the transition from primary to lower secondary school. When comparing Groups 1 and 2, we find that children not receiving forms were about 21% less likely to enroll in the lower secondary school, and when comparing Groups 3 and 4, we find that children not receiving forms and in households unaware of the conditions were 18% 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. We may not find an effect of conditionality at these grade levels in part because almost all children were already completing these grades.

One could consider the difference 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 children who have completed grade 6 have an 8.3 percentage point increase in enrollment, other estimates of the effect of *PROGRESA* on enrollment suggest larger impacts, which are in line with either a negligible or slightly positive income effect. Behrman et al. (2005) show that when one considers a larger range of potential educational transitions, the increase in enrollment due to the CCT is much higher than Schultz finds with the more limited difference-in-difference estimator. de Janvry and Sadoulet (2006) also consider 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 find suggestively that unconditional transfers should have a small, positive impact on enrollment, implying a small positive income effect. These findings are quite consistent with ours, as the magnitude of our coefficient estimate is similar to theirs.

3.2. Initial controls for unobservables

Although the unconditional means and the probit results provide *prima facie* evidence that conditionality affects enrollment, they implicitly assume that non-receipt of these forms is uncorrelated with unobservable characteristics at the household or locality level. It is not difficult to think of reasons why it might be violated. Suppose that there were administrative problems in one location that lead to poor

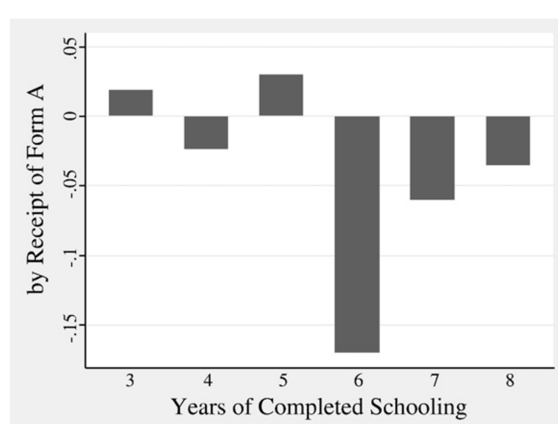

Fig. 1. Difference in school enrollment between those who received *PROGRESA* forms to enforce conditionality and those who did not, among *PROGRESA* transfer recipients.

[11] 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.

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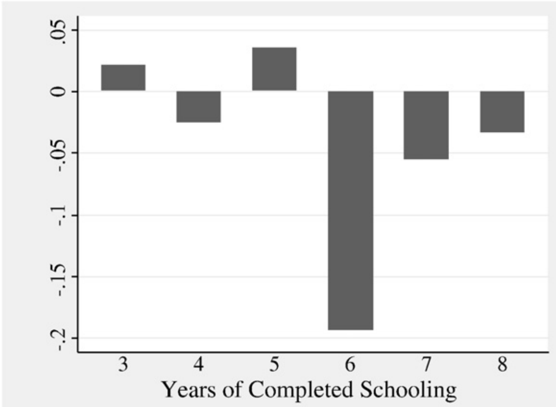

Fig. 2. Difference in school enrollment, between those who received *PROGRESA* enforcement forms and could name conditions and those who did not receive forms and could not name conditions, among *PROGRESA* transfer recipients.

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 these forms.

There is evidence in the data that the non-receipt 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 E1 forms is spread out nearly evenly across the seven states. Still, it could be that there were a few *municipios* in each state that 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 locality (Fig. 3), which shows that that non-receipt of forms is distributed widely across the sample. Therefore, a bias similar to endogenous program placement bias does not seem to exist for the non-receipt of forms.

Next, we consider whether those who did not receive forms were systematically poorer than households who did receive Form E1, using the logarithm of per capita consumption measured during the October 1998 survey round (Fig. 4). There is little difference between the kernel density of the consumption distribution for households receiving and not receiving Form E1. 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 (Fig. 5). Again, there is little obvious difference in these distributions.

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 do emerge.[12] Observables found to be significantly related to Group 1 membership include whether or not the household head and spouse were agricultural laborers (both negative); whether a household experienced an earthquake in the previous growing season (negative); whether the household received the *PROGRESA* manual (negative); and the number of meetings the household missed (positive). Shocks, such as earthquakes, have a negative and significant association with E1 form; it could be that some households simply could not attend the general assembly at which the E1 form was distributed (Adato et al., 2000).

3.3. Matching results

Because these results suggest that non-receipt of these forms may not have been completely random, we extend our analysis by using nearest neighbor matching (Abadie and Imbens, 2006).[13] We estimate the impact of not receiving the E1 form as an average treatment effect on the treated (ATT). To ensure that outliers do not affect our results, we first estimate a propensity score for the receipt of the E1 form, all of the variables in column 6 of Table 4.[14] We then ensure that the propensity scores balance; that is, we test whether or not the treatment and comparison observations had the same distribution (mean) of propensity scores and of control variables within quantiles of the propensity score. All results presented below are based on specifications that passed balancing tests. The distributions of propensity scores, in fact, overlap each other for almost all of the range for Groups 1 and 2 (Fig. 6) and Groups 3 and 4 (Fig. 7).

We then match treatment and control observations using nearest neighbor matching with bias adjustment (Abadie and Imbens, 2006, 2007).[15] The estimator matches each observation to its four nearest neighbors with replacement, and standard errors account for heteroscedasticity.[16] We provide estimates 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 computed the variance for trims at 0.01 intervals from 0 to 0.1 using the formula found in Crump et al. (2009), and determined that we should drop observations with a propensity score below 0.04 for the comparison of Groups 1 and 2 and below 0.03 for the comparison of Groups 3 and 4.

On average, the matching results imply 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 non-receipt of the E1 form coupled with the lack of knowledge of *PROGRESA* conditions reduces the enrollment likelihood by 9.6 percentage points. Again there is a great deal of heterogeneity when we estimate separate coefficients for children by grade completed.[17] We find that the effect is again largest at the point where children transition from primary to lower secondary school and there is some suggestion that non-receipt of the forms together with absence of knowledge of conditions has an even larger effect on attendance than non-receipt by itself.[18] Further, 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 affected continuing primary school enrollment. In results not reported here, we assessed whether these results differed by gender, but did not find

[12] These models are estimated with all of the control variables found in column 6 of Table 4. We also find significant differences if we estimate probits that attempt to explain Group 3 membership against Group 4 as the control. In the latter regression, the literacy of the head's spouse, the logarithm of per capita consumption, *PROGRESA* promoter status, and the number of missed meetings all have significant influences on the probability of Group 3 membership.

[13] Our data meet the criteria required for the validity of matching methods as set out in Heckman et al. (1997, 1998a) and Heckman et al. (1998b): (i) the same data source is used for participants and non-participants, (ii) participants and non-participants had access to the same markets, and (iii) the data include meaningful variables capable of identifying program participation and outcomes.

[14] The one difference is that we use child age as a continuous variable rather than as a set of dummy variables, to ensure that we pass the balancing tests.

[15] We use nearest neighbor matching 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. Estimates using propensity score matching methods are nearly identical.

[16] Results are robust to using one-to-one matching, or additional nearest neighbor matches.

[17] 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.

[18] We also explored whether, conditional on enrollment, receipt of the E1 forms increased attendance. In general, we find a positive effect but not one that is statistically significant.

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

	Specification					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: comparing Group 1 (did not receive E1 form) with Group 2 (received E1 form)						
Household did not receive 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
Panel B: comparing Group 3 (did not receive E1 form and did not know conditions) and Group 4 (received E1 form and knew conditions)						
Household did not receive 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 on parentheses. See Appendix Tables A1 and A2 for full results of Table 4A and B, respectively, as well as the full list of variables included in these regressions. Sample size is 5637 in Panel A and 3131 in Panel B. **—indicates significance at the 1% level.

large differences between males and females in the magnitudes of these effects.

3.4. Further robustness checks

Our principal finding is that receipt of the E1 form increased the likelihood that children were enrolled in school. However, the average effect masks significant heterogeneity across children in different grade levels; there appears to be little effect of E1 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. Perhaps households that did not receive the E1 form are different from other households in subtle ways. For example, perhaps they are just unable to understand how the program is supposed to work. Or perhaps they are recalcitrant individuals who just do not like having to follow rules or procedures like going to meetings to pick up forms or send their children to school because the government tells them to do so. To further ensure that our results are not driven by unobservables, we report three robustness checks in this sub-section.

First, we assess whether selection on unobservables could explain our results by computing an informal test statistic suggested by Altonji et al. (2005). They 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 2 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.[19] Even if household unobservables positively bias our estimates, 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 only observed at the household level, 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).[20] 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 probits or nearest neighbor matching.[21] As the fixed effects in these

Table 5
Probit results of the impact of non-receipt 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 obs.	1243	411
4	0.003 (0.35)	0.001 (0.04)
Number of obs.	969	385
5	0.013 (1.16)	0.004 (0.20)
Number of obs.	927	504
6	−0.211 (4.15)**	−0.183 (2.91)**
Number of obs.	1308	703
7	−0.044 (1.30)	−0.255 (2.95)**
Number of obs.	453	227
8	0.012 (0.34)	X
Number of obs.	393	209

Notes: Marginal effects are reported, cluster-robust z statistics on parentheses. Each cell represents a separate regression. All regressions include all controls in column 6 of Table 4a 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% level.

[19] In a linear probability model version of the regression in column 6 of Table 4, the R^2 is 0.2, implying that selection on observables accounts for 20% of the variation in enrollment. Therefore, even if selection on unobservables accounted for the remaining 80% of the variation in enrollment, the coefficient on Group 1 membership would still be negative. For grade 6 completion, the R^2 is 0.29, so again selection on unobservables could not completely explain the negative coefficient.

[20] 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, they could be less likely to enroll their children in school, which is captured in the household fixed effect.

[21] Although the fixed effects regression accounts for household 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, and this regression simply measures that sensitivity. To test this hypothesis, we interacted several variables (e.g. income, literacy of the head) with levels of grade completion and re-estimated the household fixed effects model. The estimated coefficients on additional interactions were typically insignificant.

Please cite this article as: de Brauw, A., Hoddinott, J., Must conditional cash transfer programs be conditioned to be effective? The impact of conditioning transfers on school enrollment in Mexico, J. Dev. Econ. (2010), doi:10.1016/j.jdeveco.2010.08.014

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
Michoacan	11.5
Puebla	8.7
Queretaro	11.1
San Luis	8.5
Veracruz	9.8
All states	9.7

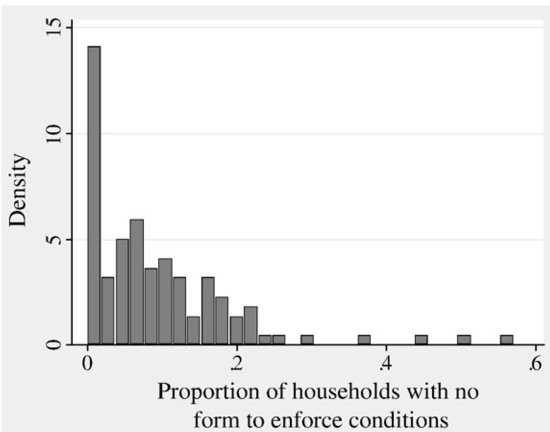

Fig. 3. Proportion of households that did not receive forms to enforce *PROGRESA* conditions, by *municipio*.

regressions account for fixed household level unobservables, they provide strong evidence that fixed unobservables do not drive our results.

Third, we consider an indirect approach. As part of the *PROGRESA* program, beneficiaries had to attend the monthly meetings, 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 monthly meetings 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 evidence 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 conditions attached to schooling have nothing to do with patterns of food consumption, rejecting this null would suggest that the variable measuring E1 form receipt captures unobservables related to recalcitrant individuals as described above.

The May 1999 survey contained a set of questions on household food consumption in the previous seven days. Following the procedure described in Hoddinott and Skoufias (2004), we use these data to calculate caloric availability per person per day. We then use nearest neighbor matching and OLS to consider whether

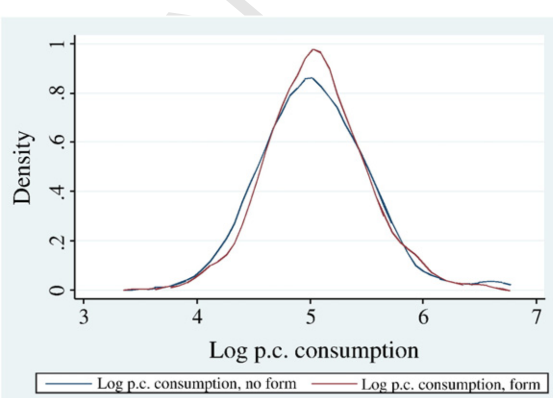

Fig. 4. Kernel density of logarithm of per capita consumption, by whether or not household received E1 form.

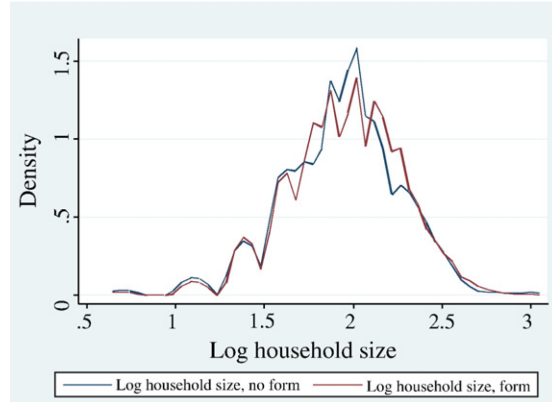

Fig. 5. Logarithm of household size, by receipt of E1 form, *PROGRESA* households.

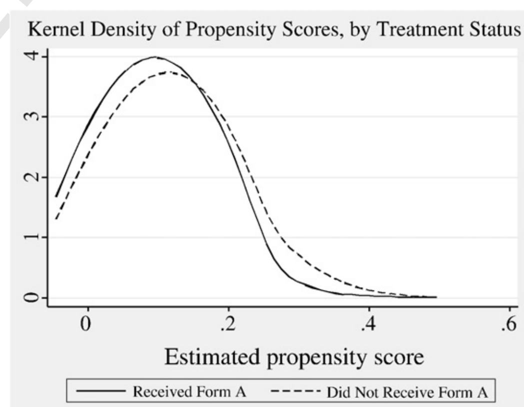

Fig. 6. Kernel density of propensity scores, by receipt of E1 form.

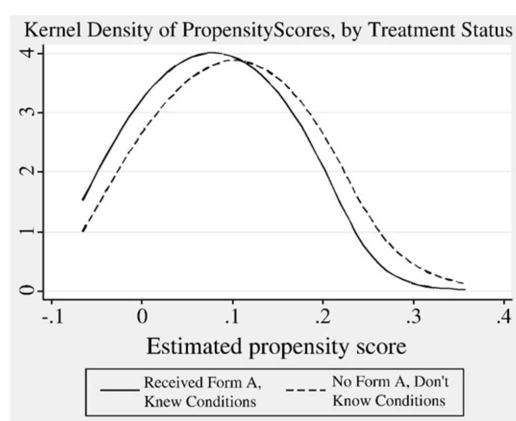

Fig. 7. Kernel density of propensity scores, by whether or not households received Form E1 and whether they could recite *PROGRESA* conditions.

Please cite this article as: de Brauw, A., Hoddinott, J., Must conditional cash transfer programs be conditioned to be effective? The impact of conditioning transfers on school enrollment in Mexico, J. Dev. Econ. (2010), doi:10.1016/j.jdeveco.2010.08.014

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 Form E1)		Treatment: Group 3 (households did not receive Form E1 and did not know conditions)	
	Full sample	Trimmed sample	Full sample	Trimmed Sample
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 grades 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. (2009). 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% level; **significant at the 1% level.

receiving the E1 form affects total per capita calorie consumption as well as calories from the following food groups: grains, fruits and vegetables, animal products, and other foods (Table 9). Receipt of the E1 form does not affect the acquisition of calories, and in particular it does not affect the acquisition of calories from sources such as fruit, vegetables and animal products that Hoddinott and Skoufias (2004) show are affected by exposure to the monthly meetings. As such, it is unlikely that recalcitrant individuals unlikely to follow directions are the type that did not receive the forms. This exercise provides further indirect evidence that our findings are related to conditionality and not unobserved household characteristics.

3.5. Heterogeneity by parental characteristics

We have shown that the effect of receiving the E1 form is heterogeneous by grade level completed; here, we explore whether or not coefficient estimates are heterogeneous by three types of parental characteristics: 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 the lack of Form E1 might have stronger effect on enrollment. If the head is not an agricultural laborer, they might have additional information about off-farm jobs, and as a result the opportunity cost of continuing in lower secondary school might be perceived as higher by children completing primary school. If the head is indigenous, one might expect that because school takes place in Spanish, indigenous children might be even less likely to enroll in school if their transfers are not conditioned.

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 (columns 1, 3, and 5), and the sample of children who had completed grade 6 (columns 2, 4, and 6).[22] We find that literacy matters, particularly for children who have completed grade 6. The marginal effect is only significant for the comparison of Groups 1 and 2 (in Panel A), implying that among children living in households that did not receive the E1 form, a child with a literate head is 27 percentage points more likely than a child with an illiterate head to enroll in school. 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 neither received the E1 form nor understood 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

[22] We use the procedure developed by Norton et al. (2004) to compute the marginal effects and their standard errors.

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

	Treatment: households that did not receive forms		Treatment: households did not receive forms and did not know conditions	
Specification:	(1)	(2)	(3)	(4)
Completed grade 4	−0.016	0.013	0.003	0.031
	(0.86)	(0.67)	(0.12)	(1.33)
Completed grade 5	−0.026	0.054	−0.017	0.063
	(1.90)*	(2.09)**	(0.93)	(2.04)**
Completed grade 6	−0.303	−0.056	−0.267	−0.025
	(14.74)**	(1.68)*	(9.59)**	(0.70)
Completed grade 7	−0.126	0.182	−0.116	0.208
	(6.42)**	(4.87)**	(3.78)**	(4.09)**
Completed grade 8	−0.161	0.295	−0.139	0.338
	(7.51)**	(6.93)**	(4.71)**	(7.19)**
Completed grade 4* treatment	−0.034	−0.018	−0.099	−0.055
	(0.56)	(0.35)	(1.18)	(0.69)
Completed grade 5* treatment	0.059	0.025	0.007	−0.028
	(1.33)	(0.59)	(0.09)	(0.40)
Completed grade 6* treatment	−0.178	−0.183	−0.231	−0.211
	(2.85)**	(3.18)**	(2.81)**	(2.75)**
Completed grade 7* treatment	−0.03	−0.035	−0.274	−0.146
	(0.55)	(0.57)	(2.04)**	(1.10)
Completed grade 8* treatment	0.043	0.002	−0.066	−0.071
	(0.84)	(0.03)	(0.88)	(0.82)
Age, gender dummies?	No	Yes	No	Yes
Number of obs.	5656	5656	3131	3131

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% level; **—indicates significance at the 5% level.

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% level; **significant at the 1% level.

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Grades	Completed	Grades	Completed	Grades	Completed
Sample:	3–8	Grade 6	3–8	Grade 6	3–8	Grade 6
Panel A: Groups 1 and 2 (Group 1 did not receive Form E1)						
Member of Group 1	−0.093	−0.461	−0.076	−0.334	−0.032	−0.212
(1 = yes)	(3.35)**	(3.81)**	(3.65)**	(4.40)**	(2.00)*	(2.91)**
Group 1* head literate	0.068	0.270				
	(1.50)	(2.17)*				
Received E1* head is agr. laborer			0.044	0.131		
			(1.29)	(1.58)		
Received E1* head is indigenous					−0.037	−0.003
					(0.81)	(0.01)
Head is literate	0.012	−0.019	0.017	0.016	0.017	0.017
(1 = yes)	(1.15)	(0.46)	(1.86)	(0.38)	(1.89)	(0.41)
Head is agricultural laborer (1 = yes)	<0.001	0.01	−0.004	−0.01	<0.001	0.008
	(0.01)	(0.29)	(0.44)	(0.27)	(0.00)	(0.26)
Head is indigenous	0.028	0.131	0.028	0.132	0.032	0.135
(1 = yes)	(2.86)**	(3.19)**	(2.89)**	(3.21)**	(3.09)**	(3.03)**
Observations	5503	1308	5503	1308	5503	1308
Panel B: Groups 3 and 4 (Group 3 did not receive Form E1 and did not know conditions)						
Member of Group 3 (1 = yes)	−0.142	−0.386	−0.143	−0.408	−0.052	−0.174
	(4.16)**	(2.56)*	(4.39)**	(3.47)**	(2.40)*	(2.13)*
Group 3* head is literate	0.095	0.202				
	(1.64)	(1.37)				
Group3* head is agr. laborer			0.107	0.248		
			(1.82)	(2.06)*		
Group 3* head is indigenous					−0.043	0.015
					(0.67)	(0.22)
Head is literate	0.007	−0.028	0.012	−0.006	0.013	−0.004
(1 = yes)	(0.56)	(0.51)	(1.14)	(0.13)	(1.20)	(0.09)
Head is agricultural laborer (1 = yes)	0.006	0.016	0	−0.015	0.006	0.015
	(0.55)	(0.33)	0.00	(0.28)	(0.58)	(0.31)
Head is indigenous	0.024	0.125	0.024	0.128	0.028	0.128
(1 = yes)	(1.87)	(2.58)**	(1.92)	(2.65)**	(2.13)*	(2.48)*
Observations	3071	715	3071	715	3071	715

Notes: All coefficients presented are marginal effects; interaction terms are computed using the procedure outlined in Norton et al. (2004). *t*-statistics based on standard errors accounting for clustering at the locality in parentheses. *—indicates significance at the 5% level; **—indicates significance at the 1% level.

enroll (Panel B, rows 1 and 3). Children in such households may perceive the opportunity cost of schooling as higher than children whose parents are agricultural laborers.

4. Conclusion

A growing body of evidence suggests that conditional cash transfer programs can have positive effects on a wide range of welfare indicators. There is much less evidence on the contributions that individual components of these programs make towards achieving these outcomes. The contribution of this paper has been to assess the impact of imposing conditions on one dimension of human capital formation, school enrollment, using data from Mexico's *PROGRESA* program. We exploit the fact that some *PROGRESA* beneficiaries did not receive the forms needed to monitor the attendance of their children at school. We show that on average the absence of these forms reduced the likelihood that children attended school, and the likelihood was severely reduced when children were making the transition to lower secondary school. For children making the transition to lower secondary school, the impact of the transfer on school enrollment can roughly be wholly attributed to conditionality. We use a variety of techniques to ensure that our findings are not driven by unobservables.

These results speak directly to policy debates regarding conditionality within CCT programs. They suggest that debates over "to condition or not to condition" are overly simplistic. In this case, there is little benefit to conditioning transfers based on enrollment in primary school. However, there are large benefits associated with conditioning at entry into lower secondary school. As such, these findings are consistent with the more general argument advanced in de Janvry and Sadoulet (2006), that there can be considerable efficiency gains to CCTs through more careful design. That all 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 policy makers to assess whether the effectiveness of conditionality can be strengthened.

5. Uncited references

Behrman and Hoddinott, 2005
Bourguignon et al., 2003
Coady et al., 2004a
Coady et al., 2004b
Schady and Araujo, 2006

Acknowledgements

We thank Ariel Fiszbein, Dan Gilligan, Guido Imbens, Santiago Levy, Norbert Schady, two anonymous referees, and seminar participants at the World Bank and IFPRI for helpful comments and suggestions, and the Research Committee of the World Bank for funding this work. We are responsible for all errors.

Appendix A

Table A1
Probit estimates of the impact of non-receipt of the E1 form on school enrollment of children who had completed grades 3–8, comparing Group 1 (did not receive E1 form) with Group 2 (received E1 form).

	(1)	(2)	(3)	(4)	(5)	(6)
Group 1	−0.054	−0.046	−0.045	−0.046	−0.049	−0.044
(1 = yes)	(3.37)**	(3.68)**	(3.53)**	(3.62)**	(3.79)**	(3.56)**
Child characteristics						
Gender		0.011	0.011	0.011	0.01	0.01
(1 = male)		(1.63)	(1.67)	(1.62)	(1.48)	(1.54)
Child is 9 years old		0.053	0.052	0.049	0.049	0.048
		(1.32)	(1.33)	(1.21)	(1.18)	(1.22)
Child is 10 years old		0.049	0.049	0.044	0.043	0.044
		(1.20)	(1.21)	(1.05)	(1.02)	(1.05)
Child is 11 years old		0.003	0.004	−0.003	−0.002	0.003
		(0.06)	(0.07)	(0.06)	(0.04)	(0.06)
Child is 12 years old		−0.034	−0.033	−0.041	−0.041	−0.035
		(0.58)	(0.56)	(0.66)	(0.67)	(0.58)
Child is 13 years old		−0.129	−0.127	−0.134	−0.136	−0.123
		(1.70)	(1.66)	(1.70)	(1.71)	(1.56)
Child is 14 years old		−0.244	−0.239	−0.251	−0.253	−0.244
		(2.75)**	(2.69)**	(2.73)**	(2.73)**	(2.60)**
Child is 15 years old		−0.384	−0.381	−0.393	−0.397	−0.382
		(3.65)**	(3.57)**	(3.59)**	(3.60)**	(3.37)**
Child is 16 years old		−0.558	−0.559	−0.571	−0.57	−0.556
		(4.48)**	(4.39)**	(4.40)**	(4.36)**	(4.14)**
Parental characteristics						
Logarithm, age of household head			0.023	0.023	0.02	0.017
			(1.27)	(1.28)	(1.04)	(0.96)
Head is female			0.027	0.019	0.018	0.017
(1 = yes)			(2.32)*	(1.51)	(1.46)	(1.43)
Head is literate			0.018	0.018	0.018	0.019
			(2.00)*	(1.99)*	(2.02)*	(2.04)*
Head is agr. laborer			<0.001	0.001	<0.001	0.001
			(0.06)	(0.10)	(0.03)	(0.09)
Head is indigenous			0.032	0.03	0.029	0.029
			(3.15)**	(3.05)**	(2.91)**	(2.95)**
Spouse of head is indigenous			0.008	0.009	0.008	0.005
			(0.64)	(0.69)	(0.60)	(0.37)
Spouse of head is literate			0.023	0.023	0.022	0.021
			(3.23)**	(3.25)**	(2.96)**	(2.94)**
Household characteristics, measured in October 1998						
Logarithm, per				−0.005	−0.006	−0.005
Capita consumption				(0.69)	(0.84)	(0.73)
Logarithm,				−0.047	−0.048	−0.044
Household size				(4.11)**	(4.03)**	(3.77)**
Additional household characteristics, including shocks						
Household experienced drought					−0.002	−0.001
					(0.21)	(0.14)
Household experienced flood					−0.028	−0.028
					(0.72)	(0.75)
Household experienced freezing crops					−0.012	−0.004
					(0.68)	(0.22)
Household experienced fire					0.002	0.003
					(0.05)	(0.08)
Household experienced crop epidemics					0.011	0.015
					(0.79)	(1.14)
Household experienced earthquake tremors					0.015	0.008
					(0.86)	(0.50)
Received *PROGRESA* book					<0.001	0.001
					(0.03)	(0.05)
Received health					−0.006	<0.001
Register					(0.23)	(0.01)
Was a *PROGRESA* promoter					0.012	0.014
					(0.78)	(0.92)
Platicas held					0.001	<0.001
					(0.43)	(0.05)
Platicas missed					0.001	0.002
					(0.40)	(0.49)
Community characteristics						
Community has electricity						0.023
						(2.21)*
Community has pre-school						−0.012
						(0.82)
Community has lower secondary school						0.042
						(4.92)**

Please cite this article as: de Brauw, A., Hoddinott, J., Must conditional cash transfer programs be conditioned to be effective? The impact of conditioning transfers on school enrollment in Mexico, J. Dev. Econ. (2010), doi:10.1016/j.jdeveco.2010.08.014

Table A1 (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)
Community characteristics						
Community has secondary school						−0.045
						(1.60)
State dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	5637	5637	5608	5608	5503	5503

Notes: Group 1 refers to households that did not receive the E1 form. 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% level; **—indicates significance at the 1% level.

Table A2
Probit estimates of the impact of non-receipt of the E1 form on school enrollment of children who had completed grades 3–8, comparing Group 3 (did not receive E1 form and did not know conditions) with Group 4 (received E1 form and knew conditions).

	(1)	(2)	(3)	(4)	(5)	(6)
Group 1	−0.09	−0.067	−0.064	−0.066	−0.075	−0.07
(1 = yes)	(4.23)**	(3.97)**	(3.90)**	(3.95)**	(4.08)**	(3.95)**
Child characteristics						
Gender		0.006	0.006	0.006	0.005	0.006
(1 = male)		(0.69)	(0.69)	(0.67)	(0.57)	(0.67)
Child is 9 years old		0.053	0.054	0.053	0.052	0.051
		(1.31)	(1.40)	(1.40)	(1.39)	(1.37)
Child is 10 years old		0.064	0.065	0.064	0.062	0.059
		(1.69)	(1.76)	(1.72)	(1.69)	(1.61)
Child is 11 years old		0.032	0.034	0.034	0.034	0.034
		(0.67)	(0.74)	(0.73)	(0.75)	(0.74)
Child is 12 years old		0.002	0.006	0.006	0.003	0.003
		(0.03)	(0.11)	(0.10)	(0.05)	(0.06)
Child is 13 years old		−0.054	−0.046	−0.044	−0.048	−0.045
		(0.77)	(0.67)	(0.64)	(0.70)	(0.66)
Child is 14 years old		−0.17	−0.157	−0.155	−0.159	−0.158
		(2.00)*	(1.88)	(1.84)	(1.89)	(1.84)
Child is 15 years old		−0.287	−0.27	−0.267	−0.277	−0.271
		(2.76)**	(2.60)**	(2.56)*	(2.66)**	(2.54)*
Child is 16 years old		−0.431	−0.416	−0.415	−0.422	−0.416
		(3.35)**	(3.23)**	(3.19)**	(3.25)**	(3.13)**
Parental characteristics						
Logarithm, age of household head			0.027	0.027	0.026	0.026
			(1.33)	(1.32)	(1.21)	(1.32)
Head is female			0.025	0.022	0.015	0.015
(1 = yes)			(1.53)	(1.29)	(0.90)	(0.94)
Head is literate			0.012	0.012	0.014	0.015
			(1.01)	(1.02)	(1.22)	(1.39)
Head is agr. laborer			0.007	0.007	0.007	0.008
			(0.62)	(0.62)	(0.57)	(0.72)
Head is indigenous			0.03	0.029	0.025	0.027
			(2.23)*	(2.26)*	(1.97)*	(2.20)*
Spouse of head is indigenous			0.016	0.016	0.015	0.011
			(0.97)	(0.99)	(0.91)	(0.64)
Spouse of head is literate			0.025	0.025	0.021	0.02
			(2.46)*	(2.56)*	(2.12)*	(2.06)*
Household characteristics, measured in October 1998						
Logarithm, per capita consumption				−0.014	−0.016	−0.017
				(1.51)	(1.98)*	(2.13)*
Logarithm, household size				−0.029	−0.033	−0.034
				(1.68)	(1.96)*	(2.10)*
Additional household characteristics, including shocks						
Household experienced drought					−0.008	−0.005
					(0.97)	(0.64)
Household experienced flood					−0.014	−0.018
					(0.43)	(0.58)
Household experienced freezing crops					−0.018	−0.008
					(1.11)	(0.56)
Household experienced fire					0.033	0.033
					(0.85)	(0.93)
Household experienced crop epidemics					0.003	0.008
					(0.21)	(0.58)
Household experienced earthquake tremors					0.03	0.022
					(1.80)	(1.36)
Received *PROGRESA* book					−0.019	−0.018
					(1.61)	(1.51)
Received health register					0.024	0.023
					(0.55)	(0.50)
Was a *PROGRESA* promoter					0.011	0.012
					(0.66)	(0.72)

(*continued on next page*)

Table A2 (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)
Additional household characteristics, including shocks						
Platicas held					−0.003	−0.004
					(0.69)	(0.99)
Platicas missed					0.005	0.006
					(0.82)	(0.98)
Community characteristics						
Community has electricity						0.022
						(1.85)
Community has pre-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 obs.	3131	3131	3121	3121	3071	3071

Notes: Group 1 refers to households that did not receive the E1 form. 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% level; **—indicates significance at the 1% level.

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Please cite this article as: de Brauw, A., Hoddinott, J., Must conditional cash transfer programs be conditioned to be effective? The impact of conditioning transfers on school enrollment in Mexico, J. Dev. Econ. (2010), doi:10.1016/j.jdeveco.2010.08.014