

**Must Conditional Cash Transfer Programs be conditioned to be effective?
The impact of conditioning transfers on school enrollment in Mexico**

Preliminary draft

Alan de Brauw, IFPRI

John Hoddinott, IFPRI

June 13, 2007

Acknowledgements:

We thank Ariel Fiszbein, Santiago Levy and Norbert Schady and for helpful comments, Dan Gilligan for his invaluable assistance with programming propensity score matching and the Research Committee of the World Bank for funding this work. We are responsible for all errors.

Address for correspondence:

de Brauw and Hoddinott. International Food Policy Research Institute, 2033 K Street NW, Washington DC, 20006, USA. Em: A.debrauw@cgiar.org; J.Hoddinott@cgiar.org

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 begins to fill this gap by explicitly testing 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* CCT program. To assess the importance of conditionality, we exploit the fact that some *PROGRESA* 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 propensity score matching, to show that the absence of these forms reduced the likelihood that children attended school. Moreover, the effect of receiving forms is most pronounced when children are transitioning to lower secondary school. We further show that imposed conditions are more important for households with illiterate household heads. Throughout the paper, we provide evidence that suggests the findings are not driven by unobservable characteristics of households or localities.

1. Background

Conditional cash transfers (CCTs) are becoming an increasingly popular tool for poverty alleviation. Drawing on lessons learnt from a variety of countries – most notably Mexico’s *PROGRESA* program¹ – CCTs are now found in almost all parts of the developing world. As implied by their name, conditional cash transfer programs give cash transfers to households that meet specific conditions; beneficiary households *must undertake* certain activities or investments. Such activities include ensuring school-age children go to school and/or ensuring pre-school children regularly see a nurse or doctor and receive necessary immunizations. In some programs, adults are required to attend community meetings or lectures and/or have regular health check-ups; in others, concurrent improvements are made to the supply of public health and school services. A striking feature of many of these programs, particularly in Latin America, has been the careful attention paid to evaluating their effectiveness.² These evaluations normally treat the CCT as a “black box;” that is, they assess the combined effect of all of the CCT components on a given outcome, without considering which aspects of CCTs make them successful at improving target outcomes.³ As a result, little is known, for example, about whether the imposition of conditions on beneficiaries improves the effectiveness of

¹ The program was renamed Oportunidades and expanded to urban areas when Vicente Fox became president of Mexico in 2000.

² For example, see the work on *PROGRESA* summarized in Skoufias (2005) and Levy (2006) and on Nicaragua’s RPS found in Maluccio and Flores (2005). Studies that focus on the impact of *PROGRESA* on schooling related outcomes include Schultz (2004), Behrman, Sengupta and Todd (2005) and de Janvry and Sadoulet (2006).

³ 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).

CCTs, a topic that is becoming increasingly controversial (see Szekely, 2006, and Samson, 2006).⁴

There are several compelling reasons for the conditional component to be included in CCTs. These reasons can broadly be thought of as either public or private. From the public perspective, there are three related rationales. First, governments may perceive that they know what actions or behaviors will benefit the poor 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 also help the government overcome information asymmetries in meeting priorities. 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 school enrollment or use of health clinics, and by conditioning transfers on behaviors that increase these indicators these actors can provide useful evidence of accomplishments long before more important evidence of poverty reduction in the form of increased productivity or better adult health is available.

⁴ An exception is work by Schady and Araujo (2006). Using data from the *Bono de Desarrollo Humano* (BDH) cash transfer program operating in Ecuador, they find that the BDH had a large positive effect on enrollment and that the magnitude of the effect reflects, in part, the fact that some households believed that there was a condition – a school enrollment requirement – attached to the transfer. Todd and Wolpin (2006) and Bourguignon, Ferreira and Leite (2003) construct simulations that indicate that in Mexico and Brazil that the impact of CCTs in those countries is largely the result of the imposition of conditionality in *PROGRESA* and the *Bolsa Escola* programs respectively.

As such, the conditioning component can be seen by politicians as a useful tool to stay in office.

From the private perspective, the conditions in CCTs can also have potential benefits. First, disagreements may exist within the household 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, and who may otherwise lack bargaining power within the household. Second, conditioning may overcome stigma effects otherwise associated with welfare payments. In some countries, it is argued that the stigma attached to welfare payments discourages 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.

Although the reasons for conditionality may be compelling, a number of concerns about the imposition of conditionality also exist. First, conditionality is expensive; it increases the administrative costs and complexity of running the cash transfer program. If the actual or perceived benefits do not outweigh the additional costs, conditioning the transfers may not be worthwhile. Second, when meeting conditions imposes direct costs on beneficiaries, conditionality reduces the benefits that would otherwise accrue to beneficiaries. Third, if 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 reduce their total welfare gains. Fourth, some households may find the conditions too difficult to meet, and if these households are among the poorest households in the program, imposing conditions may detract from the targeting of the CCT. Finally,

conditioning transfers can be perceived as being demeaning to the poor; it can be taken, for example, to imply that the problem with the poor is that they simply do not know what is good for them.

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 regarding the effects of conditioning transfers on desired outcomes from CCTs. In this case, 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 data collected as part of the evaluation of *PROGRESA*, we empirically assess the impact of imposing education-related conditions on school enrollment and attendance. We do so using a variety of techniques, including propensity score matching, and find that the absence of these forms reduced the likelihood that children attended school. The effect is most pronounced among children making the transition to lower secondary school. We further find evidence that conditionality is more important for households with illiterate household heads. Throughout the paper, we provide several different pieces of evidence that imply our results are not due to unobserved heterogeneity,

2. Data

PROGRESA was introduced by the Federal Government of Mexico as part of an effort to break the intergenerational transmission of poverty. The program had multiple objectives, primarily aimed at improving the educational, health and nutritional status of poor families, and particularly of children and their mothers, initially 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. This constituted around 40% of all rural families and one ninth of all families in Mexico. Beneficiaries received cash transfers on a bi-monthly basis. There were three components to these transfers: 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, and Hoddinott and Skoufias (2004) describe how payments worked in practice.

In order to receive the *beca*, school-aged children in grades three and higher had to maintain an attendance record of 85 per cent or better and parents had to attend monthly meetings called *platicas*. To ensure compliance with this condition, parents were supposed to receive a form called the E1 when they were inducted into *PROGRESA*. This form was then given to school officials who registered their children. Parents then returned the E1 form to *PROGRESA* showing that they had registered their children while teachers kept track of children's attendance at school and submitted separate forms showing that children had attended. *PROGRESA* had to match parental

registration with the information provided by teachers, confirm that attendance was satisfactory, and then arrange for the payment of the *beca*.

As part of the implementation of *PROGRESA*, a series of evaluation surveys were fielded in seven states in central and southern Mexico. The evaluation survey conducted in May and June of 1999 asked whether *PROGRESA*-eligible households received the E1 form, where households considered *PROGRESA*-eligible resided in localities where *PROGRESA* was distributing cash benefits, had been identified as being eligible for *PROGRESA* benefits, and had school age children.⁵ Using the household identifiers found in these data, it is possible to match the eligible households to *PROGRESA*'s administrative records on *beca* payments made to beneficiary households for school attendance.

It turns out that a significant number of households received the *beca* transfer even though they did not receive the E1 form and could not have had the attendance of their children monitored by the program.⁶ Specifically, there are 464 households with school age children that did not receive the E1 form and received at least one *beca* payment for children's school attendance between March and August 1999 (i.e. the period two months prior to the start of the May-June evaluation survey to two months after the survey). We label these households as Group 1. There are 3919 households with school age children that received the E1 form and received at least one *beca* payment for children's school attendance between March and August 1999, and we call these households Group 2. Households in Groups 1 and 2 share important similarities: they are

⁵ Specifically, they had at least one child age 6 to 17.

⁶ We discussed this issue with Santiago Levy – the architect of *PROGRESA* – and Emmanuel Skoufias – the person responsible for leading the evaluation of *PROGRESA*. Both confirmed that if the household did not receive an E1 form, no monitoring of attendance was possible.

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 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 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 was monitoring them somehow, and therefore the E1 would be unnecessary. As a result the 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 an alternative 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. We can then take the sample of households with school-age children and who were eligible for *PROGRESA* benefits, living in localities where *PROGRESA* provided cash transfers and who received at least one transfer for school attendance between March and August 1999 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 forms to enroll their children *and* knew that they were required to send their children to school in order to receive school benefits.⁷ Since households in Group 3 neither received the form necessary for the transfer to be

⁷ To provide a clean basis for comparison between Groups 3 and 4, from the full sample of *PROGRESA*-eligible households 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.

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, one should not immediately attribute the difference to conditionality. Observable or unobservable characteristics of either households or municipalities may have played a role in determining these differences, and might explain the difference, rather than conditionality. In order to ensure that our results are due to the lack of conditionality rather than other observables or unobservables, we proceed as follows. After describing differences in unconditional means between groups, we initially condition on differences in observable characteristics between households, using probit models. Since one might think that there was something akin to endogenous program placement bias, we examine the share of households that did not receive Form E1 by both state and municipality, to ensure that all of the Group 1 and 3 members did not reside in the same state or in a few municipalities. To further ensure that the probit results are not caused by unobserved heterogeneity, we perform propensity score matching to re-estimate the effect of not receiving forms on school enrollment and attendance. To provide a last check on unobservable heterogeneity, we repeat the analysis on an outcome unrelated to school attendance, caloric consumption, which was positively affected by PROGRESA (Hoddinott and Skoufias, 2004). If caloric consumption was affected by the receipt of Form E1, one would be concerned that all of the results reflect some other aspect of PROGRESA, or household unobservables, rather than the lack of conditionality on schooling related transfers.

3. Results

a) Basic findings

To analyze the effect of conditionality on school enrollment, we begin by examining unconditional mean school enrollment, by the Groups defined above (Table 1). Among children 8-16 years of age, 85.2 percent of children in Group 1 households were enrolled in school, while 88.4 percent of children in Group 2 households were enrolled.⁸ Even after taking into account the clustered nature of the survey, this difference is statistically significant at the 5% level. The difference is even larger when we also consider whether or not households understood the conditions. The enrollment rate among children in households in Group 3 was 82.2%, whereas it was 89.2% for children in Group 4 households. The differences in mean enrollments therefore are suggestive that conditionality does affect enrollment, but still could be explained by a myriad of other factors.

The unconditional means also mask striking differences by grade level. We next plot the difference between means for Groups 1 and 2 (Figure 1) and Groups 3 and 4 (Figure 2) by completed grade level. Primary school is completed after grade 6, and we find the largest difference in school enrollment between the groups for children who have completed primary school and should be entering lower secondary school. Children in households who did not receive form E1 are much less likely—by 18 to 20 percent—to enroll in school, whether or not parents are aware of the attendance conditionality. These differences are significant at the 1 percent level. For other grade levels, the differences are not nearly as large, not always statistically significant, and in some cases children in

⁸ 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. Localities where all beneficiaries received the E1 form are dropped from the sample.

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.

To control for observable differences between children, households, and localities, we estimate probits where the dependent variable equals one if the child is enrolled, zero otherwise (Tables 2a and 2b). Our primary explanatory variable of interest is an indicator variable denoting households that did not receive the E1 form in the first specification (Table 2a), and households who neither received the form nor knew the conditions (Table 2b). We further include characteristics of the child (age dummies, gender); the household head (age, gender, occupation, indigenous status and literacy); the spouse of the head (indigenous status and literacy); the household (log per capita consumption and log household size, both measured using the previous October 1998 survey round)⁹, and the locality (indicators for the state of residence) measured in the previous, October 1998, survey round. The general findings do not change when we replace the state-level indicators with *municipia* level indicators.¹⁰

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 was 3.2 percent, when we control for the full set of child parental, and household controls the estimated coefficient implies that the lack of an E1 form makes children 2.9 percent less likely to enroll in school, on average. When we add that

⁹ See Hoddinott and Skoufias (2004) for details on the construction of these variables.

¹⁰ A *municipia* is approximately equivalent to a US county.

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 2b, column 4) we find that children were 5.3 percent less likely to enroll in school on average, as compared to the unconditional difference of 7.0 percent. Clearly, 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.

We next replicate the probits for different completed grades, controlling for the full set of characteristics (Tables 3a and 3b). As in Figures 1 and 2, we find that the effect of conditionality is strongest among children making the transition from primary to lower secondary school. Among children who had completed grade 6, in either sample we find that children not receiving forms were about 20 percent less likely to enroll in the lower secondary school. For children continuing primary school (having completed grades 4 or 5), there is no evidence that conditionality has a significant effect on school enrollment.

While these comparisons of means and probits 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. 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 lead 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 these forms.

However, evidence in the data that the non-receipt of the E1 form is not driven by unobservable differences in administration by locality. First, consider the distribution of households not receiving the E1 form by state (Table 4). 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 *municipias* 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 (Figure 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 (Figure 4), using the logarithm of per capita consumption measured in the previous survey round. 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 (Figure 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. Controls include characteristics of the household head (log age, sex, literate, occupation, indigenous person) and characteristics of the household measured approximately seven months earlier (log per capita household consumption and log household size), in October 1998. We also control for location via the inclusion of *municipia* (county of residence) level dummy variables and account for the clustered survey design when calculating the standard errors. When we run these regressions, we find that households headed by older individuals have a slightly lower probability of receiving these forms and that richer and larger households have a slightly larger chance of receiving these forms. All three variables are significant at the 10% level.

b) Results from propensity score matching

Because these results suggest that non-receipt of these forms may not have been completely random, we extend our analysis by using propensity score matching. 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 it is a recipient of the E1 form and let 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 just 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 literature on evaluation of social programs, 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—the average impact of the treatment on the treated (ATT):

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

where X is a vector of control variables. Because $E(Y_t^0 | X, D = 1)$ is not observed, we estimate impact using propensity score matching as a method for estimating the counterfactual outcome for participants (Rosenbaum and Rubin 1983). Let $P(X) = \Pr(D = 1 | X)$ be the probability of receiving the E1 form. Propensity score matching constructs a statistical comparison group by matching observations on form recipients to observations on non-recipients with similar values of $P(X)$. The validity of this approach rests in part on two assumptions:

$$(2) \quad E(Y_t^0 | X, D = 1) = E(Y_t^0 | X, D = 0), \text{ and}$$

$$(3) \quad 0 < P(X) < 1.$$

Expression (2) assumes “conditional mean independence”, that conditional on X non-recipients have the same mean outcomes as recipients would have if they did not receive the E1 form. Expression (3) assumes that valid matches on $P(X)$ can be found for all values of X . Rosenbaum and Rubin show that if outcomes are independent of program participation after conditioning on the vector X , then outcomes are independent of program participation after conditioning only on $P(X)$. If (2) and (3) are true, propensity

score matching provides a valid method for estimating $E(Y_t^0 | X, D = 1)$ and obtaining unbiased estimates of *ATT*.

Propensity score matching provides reliable, low-bias estimates of program impact provided that (i) the same data source is used for participants and non-participants, (ii) participants and non-participants have access to the same markets, and (iii) 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 (i). Criterion (ii) is met by restricting the set of households/children that could not be monitored to be potentially selected as comparison observations to households/children that were monitored to households that resided in localities where *PROGRESA* was providing benefits and who were already enrolled in *PROGRESA*. The evaluation surveys fielded in May-June 1999 round 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 (iii).

The propensity score matching procedure involves several steps. We begin by estimating the propensity score for receipt of the E1 form using a probit model including both determinants of receipt and factors that affect enrollment. Heckman, Ichimura and Todd (1997, 1998) emphasize that the quality of the match can be improved by ensuring that matches are formed only where the distribution of the density of the propensity scores overlap between treatment and comparison observations, or where the propensity score densities have “common support.” Common support can be improved by dropping treatment observations whose estimated propensity score is greater than the maximum or less than the minimum of the comparison group propensity scores. Similarly, comparison

group observations with a propensity score below the minimum or above the maximum of the treatment observations can be dropped. A shortcoming of this approach identified by Heckman, Ichimura and Todd (1997) is that treatment observations near these cut points face a potential comparison group with propensity scores that are either all lower or all higher than that of the treatment observation. To account for this problem, we modified this “min/max” approach to identifying a region of common support using the following procedure. We first estimate the probit model for receipt of the E1 form and identified the lower and upper cut points in the non-receipt and receipt groups. Typically only comparison observations were dropped in the left of the distribution and treatment observations were dropped on the right. We then added back the five percent of observations from each tail that had been dropped that were closest in terms of propensity score. In addition, we trimmed the treatment observations from the interior of the propensity score distribution that had the lowest density of comparison observations. We choose to drop two percent of treatment observations with this trimming procedure. On this common support sample, the probit model was estimated again to obtain a new set of propensity scores to be used in creating the match. We also tested the “balancing properties” of the data by testing that 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 the balancing tests.

We match treatment and comparison observations by local linear matching with an Epanechnikov kernel using *Stata*'s `psmatch2` command (Leuven and Sianesi, 2003). Heckman, Ichimura and Todd (1997) and Smith and Todd (2005) argue in favor of local

linear matching over other matching techniques and Frölich (2004) provides evidence in support of the finite-sample properties of local linear matching relative to most other matching estimators. Standard errors of the impact estimates are estimated by bootstrap using one hundred replications.

Table 5 provides the results of these estimations for all children aged eight to 16 as well as disaggregating by age and grade attainment. The results are remarkably similar to those of the simple differences between means and the estimated coefficients from the probits. Non-receipt of the E1 form reduces the likelihood of being enrolled by about three percentage points across the full sample. The effect is most marked 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.¹¹ In results not reported here, we assessed whether these results differed by sex but did not find large differences between males and females in the magnitudes of these effects.

Table 6 explores heterogeneity in a different way, by disaggregating by household characteristics – occupation of the head, whether the head is literate and whether the head is an indigenous person - and re-estimating the propensity score matching procedure. There are some variations in impact. Non-receipt of the E1 form is associated with lower enrollment levels for children residing in households where the head is illiterate or where the head is an indigenous person.

d) An alternative robustness check

¹¹ 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.

Our principal finding is that receipt of the E1 form, which implied that their actions were monitored, increased the likelihood that children were enrolled in school. This effect is most pronounced at the point where children transition from primary to lower secondary school. We obtain this result from simple descriptive statistics, from probits and from our propensity score matching estimates. 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.

Controlling for unobservables in a non-experimental setting is hard. There is, however, an indirect way of considering this issue. As part of the *PROGRESA* program, beneficiaries had to attend monthly lectures, “*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 this conditioning should not change caloric acquisition. Since these conditions attached to schooling have nothing to do with patterns of food consumption, rejecting this null would

suggest that receipt/non-receipt of the E1 form is actually capturing some sort of unobservable characteristic such as those described above.

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 asked with reference to 35 different foods. To convert these data into calories, initially different units of measurement were converted into a common measure for each food item. Volumes were converted to weights using the *Tablas de valor nutritivo* for Mexico (Muñoz de Chávez, *et. al.* 1996). The acquisition of each food item, now 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 Nutritivo*. These 35 food variables and their aggregate, expressing calories per family per week were then converted to daily amounts and divided by household size to get caloric availability per person per day. Hoddinott and Skoufias (2004) describe this procedure in more detail.

Using the same propensity score matching technique as above, 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, fruit and vegetables, animal products and other foods. As Table 7 shows, receipt of the E1 form has no effect on the acquisition of calories even from those sources such as fruit, vegetables and animal products that Hoddinott and Skoufias (2004) show are affected by exposure to the *platicas*. As such, this exercise provides further indirect evidence that our findings are related to conditionality and are not a consequence of unobserved household characteristics.

4. Conclusion

There exists a growing body of evidence suggesting that conditional cash transfer programs can have powerful 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* CCT program. We exploit the fact that some *PROGRESA* beneficiaries did not receive the forms needed to monitor the attendance of their children at school. Using a variety of techniques, including propensity score matching, we show that the absence of these forms reduced the likelihood that children attended school. We note that this effect was most pronounced at the point where children transition to lower secondary school. Receiving the form, and understanding the conditions being imposed exerts a stronger effect on enrollments and there is some suggestion that imposing these conditions has a larger effect when the household head is not literate. Further, we present evidence suggesting 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 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 policy makers to assess whether the effectiveness of conditionality can be strengthened.

References

- Behrman, J. and J. Hoddinott, 2005. "Program evaluation with unobserved heterogeneity and selective implementation: The Mexican Progresa impact on child nutrition." *Oxford Bulletin of Economics and Statistics* 67: 547-569.
- Behrman, J., P. Sengupta and P. Todd, 2005. "Progressing through PROGRESA: An impact assessment of a school subsidy experiment in rural Mexico." *Economic Development and Cultural Change* 54: 238-275.
- Bourguignon, F., F. Ferreira and P. Leite, 2003. "Conditional cash transfer, schooling and child labor: Micro-simulating Brazil's Bolsa Escola program." *World Bank Economic Review* 17: 229-254.
- Burtless, G., 1995. "The case for randomized field trials in economic and policy research." *Journal of Economic Perspectives* 9(2): 63-84.
- Coady, D., M. Grosh and J. Hoddinott, 2004a. "Targeting outcomes redux." *World Bank Research Observer* 19: 61-85.
- Coady, D., M. Grosh and J. Hoddinott, 2004b. *The Targeting of Transfers in Developing Countries: Review of Experience and Lessons*, (Washington: World Bank and IFPRI).
- de Janvry, A., and E. Sadoulet, 2006. "Making conditional cash transfers more efficient: Designing for the maximum effect of the conditionality." *World Bank Economic Review* 20(1): 1-29.
- Frölich, M. 2004. "Finite-sample properties of propensity score matching and weighting estimators." *Review of Economics and Statistics* 86(1): 77-90.
- Gilligan and J. Hoddinott. 2007, "Is there persistence in the impact of emergency food aid? Evidence on consumption, food security and assets in rural Ethiopia." *American Journal of Agricultural Economics*, forthcoming.
- Heckman, J. and J. Smith, 1995. "Assessing the case for social experiments." *Journal of Economic Perspectives* 9(2): 85-110.
- Heckman, J.J., H. Ichimura, and P.E. Todd. 1997. "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Program." *Review of Economic Studies* 64:605-654.
- _____. 1998. "Matching as an Econometric Evaluation Estimator." *Review of Economic Studies* 65:261-294.
- Heckman, J., H. Ichimura, J.A. Smith, and P.E. Todd. 1998. "Characterizing Selection Bias Using Experimental Data." *Econometrica* 66:1017-1098.

Hoddinott, J. and E. Skoufias, 2004. "The impact of PROGRESA on food consumption." *Economic Development and Cultural Change* 53: 37-61.

Leuven E. and B. Sianesi. 2003. "PSMATCH2: Stata Module to Perform Full Mahalanobis and Propensity Score Matching, Common Support Graphing, and Covariate Imbalance Testing". <http://ideas.repec.org/c/boc/bocode/s432001.html>. Version 1.2.3.

Levy, S., 2006. *Progress against poverty: Sustaining Mexico's Progresa-Oportunidades Program*. Brookings Institution, Washington DC.

Maluccio, J. and R. Flores. 2005. *Impact Evaluation of a Conditional Cash Transfer Program: The Nicaraguan Red de Protección Social*. International Food Policy Research Report 141, Washington DC.

Muñoz de Chávez, M., et. al. 1996. *Tablas de valor nutritivo de los alimentos de mayor consumo en México*. Edición Internacional, México City

Rosenbaum, P. and D.B. Rubin. 1983. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika* 70:41-55.

Samson, M., 2006. "Are conditionalities necessary for human development." Presentation at the Third International Conference on Conditional Cash Transfers, Istanbul, Turkey, June 26-30.

Schady, N. and M. Araujo, 2006. "Cash transfers, conditions, school enrollment and child work: Evidence from a randomized experiment in Ecuador." World Bank Policy Research Paper 3930, World Bank, Washington DC.

Schultz, T.P., 2004. "School subsidies for the poor: Evaluating the Mexican Progresa poverty program. *Journal of Development Economics* 74: 199-250.

Skoufias, E. 2005. *PROGRESA and Its Impacts on the Welfare of Rural Households in Mexico*. International Food Policy Research Report 139, Washington DC.

Szekely, M., 2006. "To condition ... or not to condition." Presentation at the Third International Conference on Conditional Cash Transfers, Istanbul, Turkey, June 26-30.

Smith, J.A. and P.E. Todd. 2001. "Reconciling Conflicting Evidence on the Performance of Propensity-Score Matching Methods." *American Economic Review* 91:112-118.

_____. 2005. "Does Matching Overcome LaLonde's Critique of Nonexperimental Estimators?" *Journal of Econometrics* 125:305-353.

Todd, P. and K. Wolpin, 2006. "Using experimental data to validate a dynamic behavioral model of child schooling: Assessing the impact of a school subsidy program in Mexico." *American Economic Review*, forthcoming.

Table 1: Enrollment rates of children 8-16 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)	786	85.2%	4.50*
2 (Household received E1 form)	7457	88.4	
3 (Household did not receive E1 form and could not describe conditions)	382	82.2	15.96**
4 (Household received E1 form and could describe conditions)	4159	89.2	

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

Table 2a: Probit estimates of the impact of non-receipt of the E1 form on school enrollment of children 8-16

	Specification			
	(1)	(2)	(3)	(4)
Household did not receive E1 form	-0.031 (2.35)*	-0.029 (2.66)**	-0.027 (2.61)**	-0.029 (2.76)**
State controls	Yes	Yes	Yes	Yes
Child controls	No	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes
Household controls	No	No	No	Yes

Table 2b: Probit estimates of the impact of non-receipt of the E1 form and absence of knowledge of conditions on school enrollment of children 8-16

	Specification			
	(1)	(2)	(3)	(4)
Household did not receive E1 form and could not list conditions	-0.067 (4.38)**	-0.054 (4.15)**	-0.051 (4.16)**	-0.053 (4.25)**
State controls	Yes	Yes	Yes	Yes
Child controls	No	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes
Household controls	No	No	No	Yes

Notes: Marginal effects are reported, cluster-robust z statistics on parentheses. State controls are dummy variables denoting residence by state. Child controls are: age (dummy variables) and sex. Parental controls are characteristics of head – log age, sex, literate, works as agricultural laborer, indigenous; and characteristics of spouse of head - literate, works as agricultural laborer. Household controls are log per capita consumption, October 1998 and log household size, October 1998. Sample size, 8190. * significant at the 5% level; ** significant at the 1% level.

Table 3a: Probit estimates of the impact of non-receipt of the E1 form on school enrollment by completed grade

	Completed Grade			
	4	5	6	7
Household did not receive E1 form	0.003 (0.32)	0.014 (0.88)	-0.205 (3.94)**	-0.057 (1.55)
State controls	Yes	Yes	Yes	Yes
Child controls	Yes	Yes	Yes	Yes
Parental controls	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes
Sample size	1095	965	1346	472

Table 3b: Probit estimates of the impact of non-receipt of the E1 form and absence of knowledge of conditions on school enrollment by completed grade

	Completed Grade			
	4	5	6	7
Household did not receive E1 form and could not list conditions	-0.013 (0.87)	-0.008 (0.32)	-0.196 (3.11)**	-0.254 (3.19)**
State controls	Yes	Yes	Yes	Yes
Child controls	Yes	Yes	Yes	Yes
Parental controls	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes
Sample size	581	518	727	254

Notes: Marginal effects are reported, cluster-robust z statistics on parentheses. State controls are dummy variables denoting residence by state. Child controls are: age (dummy variables) and sex. Parental controls are characteristics of head – log age, sex, literate, works as agricultural laborer, indigenous; and characteristics of spouse of head - literate, works as agricultural laborer. Household controls are log per capita consumption, October 1998 and log household size, October 1998. * significant at the 5% level; ** significant at the 1% level.

Table 4: Percentage of *PROGRESA* households receiving transfers for school attendance but not receiving E1 forms to monitor attendance by state

State	Percent
Guerrero	7.1
Hidalgo	10.8
Michoacan	10.8
Puebla	9.2
Queretaro	9.4
San Luis	9.4
Veracruz	10.9
All states	10.0

Table 5: Propensity Score Matching estimates of the impact of receiving E1 Forms on school enrollment, by age and grade attained

Sample Used	Treatment: Households that did not Receive Forms	Treatment: Households did not receive forms and did not know conditions
Full sample of children between 8 and 16 years old	-0.038 (0.012)**	-0.069 (0.019)**
Sample of Children aged 13 to 16	-0.078 (0.027)**	-0.151 (0.040)**
Children 13 years old	0.017 (0.034)	0.043 (0.049)
Children 14 years old	-0.158 (0.059)**	-0.294 (0.101)**
Children 15 years old	-0.118 (0.072)	-0.300 (0.101)**
Children 16 years old	-0.044 (0.080)	-0.100 (0.146)
Sample of Children completing grades 5 through 8	-0.105 (0.025)**	-0.132 (0.038)**
Completed Grade 5	0.021 (0.022)	-0.015 (0.041)
Completed Grade 6	-0.180 (0.044)**	-0.179 (0.055)**
Completed Grade 7	-0.067 (0.061)	-0.211 (0.142)
Completed Grade 8	0.011 (0.063)	0.117 (0.156)

Notes: Bootstrapped Standard Error based on 100 replications in parentheses; matching by local linear regression. * significant at the 5% level; ** significant at the 1% level.

Table 6: Propensity Score Matching estimates of the impact of receiving E1 Forms on school enrollment, by consumption tertile, literacy of head and indigenous status

Outcome Variable	Treatment: Households that did not Receive Forms	Treatment: Households did not receive forms and did not know conditions
<i>Impact by occupation of household head</i>		
Head is an agricultural laborer	-0.033 (0.017)	-0.047 (0.021)*
Head is not an agricultural laborer	-0.056 (0.018)**	-0.104 (0.034)**
<i>Impact by Literacy of Household Head</i>		
Head is Illiterate	-0.089 (0.026)**	-0.138 (0.039)**
Head is Literate	-0.018 (0.014)	-0.041 (0.020)*
<i>Impact by Head is Indigenous</i>		
Head is Indigenous	-0.052 (0.018)**	-0.059 (0.025)*
Head is not Indigenous	-0.018 (0.015)	-0.060 (0.023)*

Notes: Bootstrapped Standard Error based on 100 replications in parentheses; matching by local linear regression. * significant at the 5% level; ** significant at the 1% level.

Table 7: Propensity Score Matching 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
Total calorie consumption	38.91 (37.61)
Calories from grains	36.91 (36.09)
Calories from fruit and vegetables	-0.11 (1.74)
Calories from animal products	-1.92 (5.95)
Calories from other foods	4.04 (7.92)

Notes: Bootstrapped Standard Error based on 100 replications in parentheses; matching by local linear regression. * significant at the 5% level; ** significant at the 1% level.

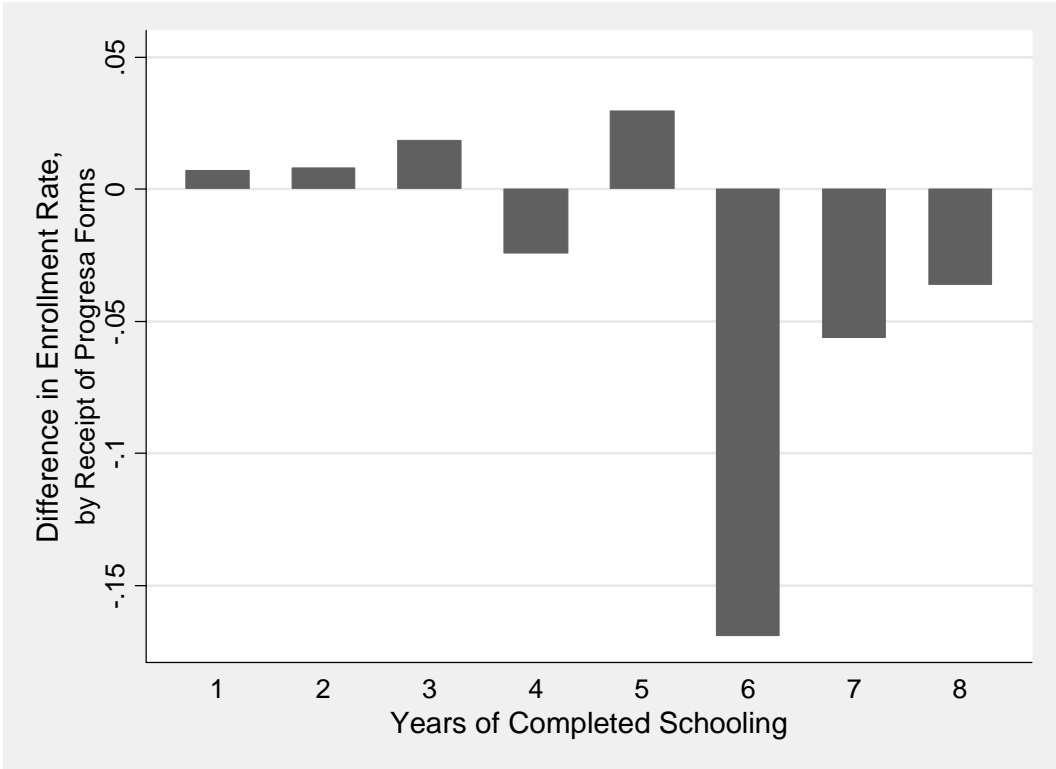


Figure 1. Difference in School Enrollment between those who received *Progresa* forms to enforce conditionality and those who did not, among *Progresa* transfer recipients

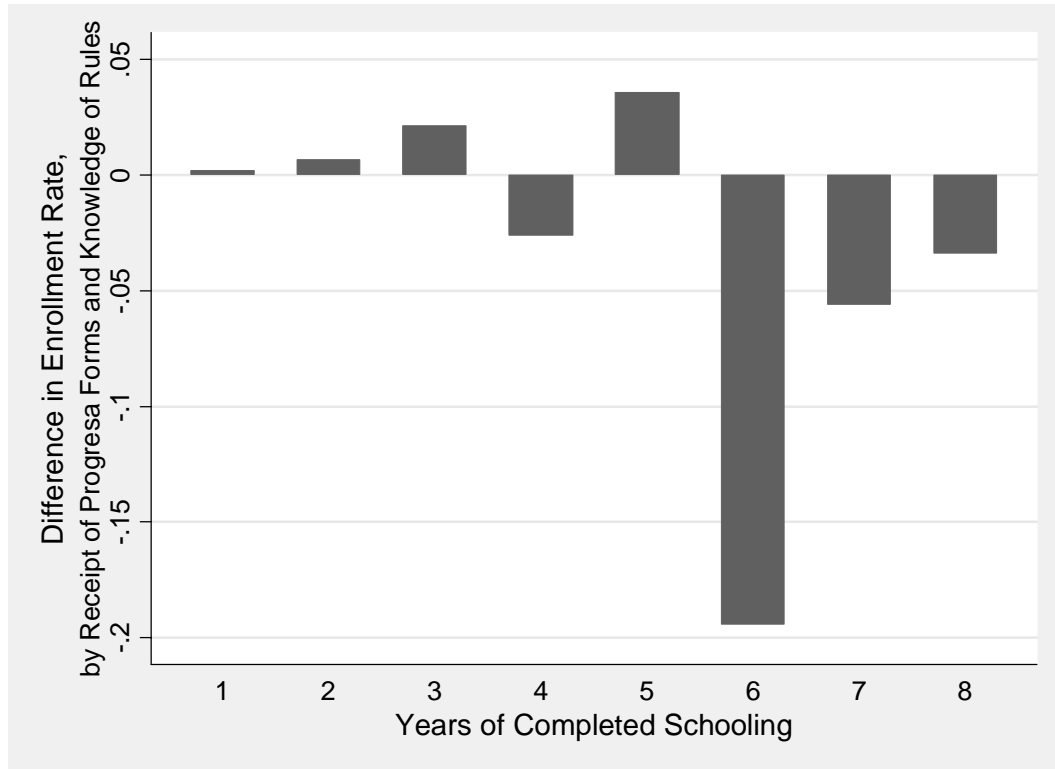


Figure 2. Difference in School Enrollment, between those who received *Progresá* enforcement forms and could name conditions and those who did not receive forms and could not name conditions, among *Progresá* transfer recipients

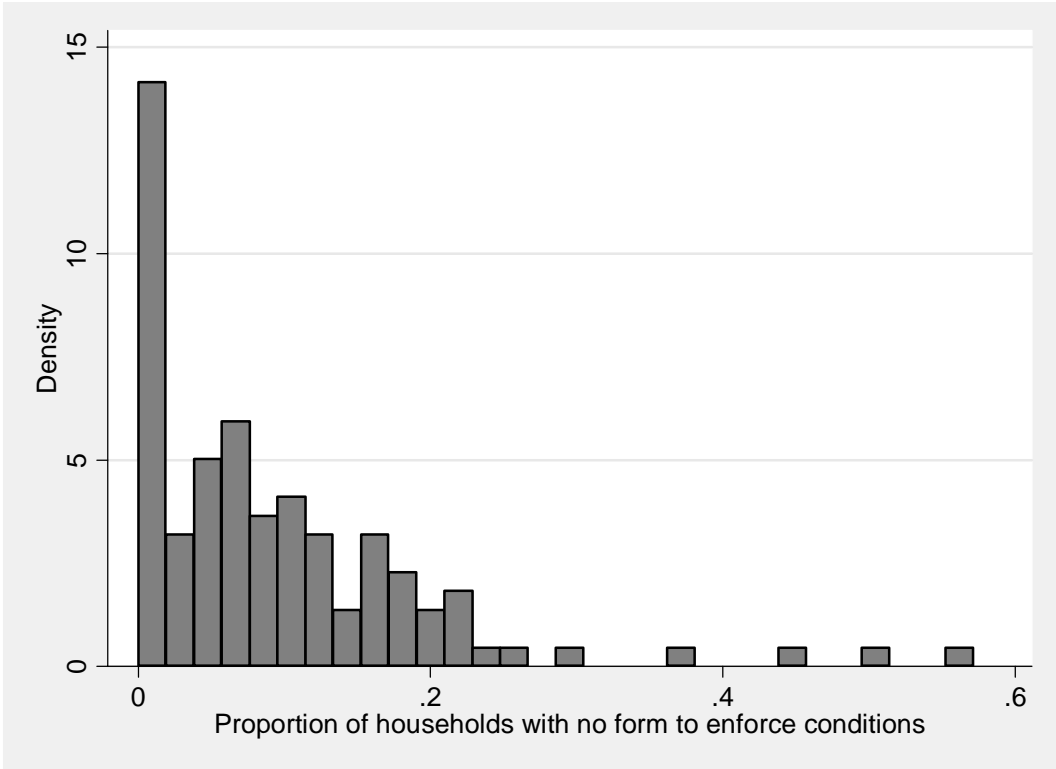


Figure 3. Proportion of households that did not receive forms to enforce *Progresá* conditions, by locality

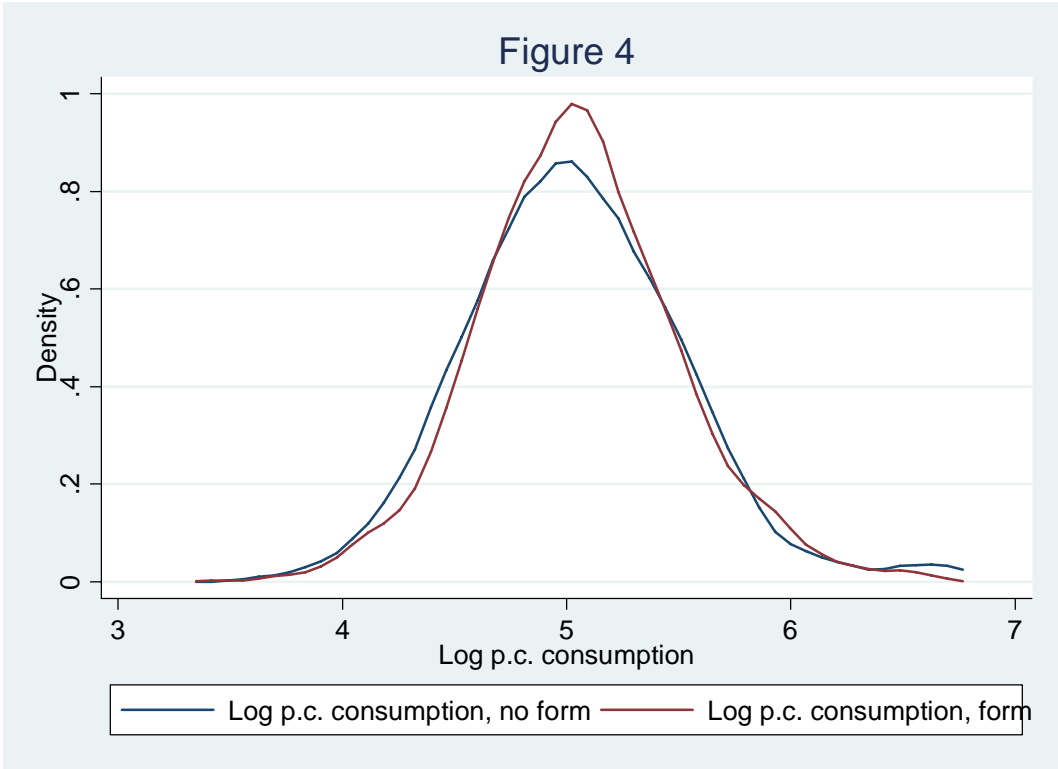


Figure 4:

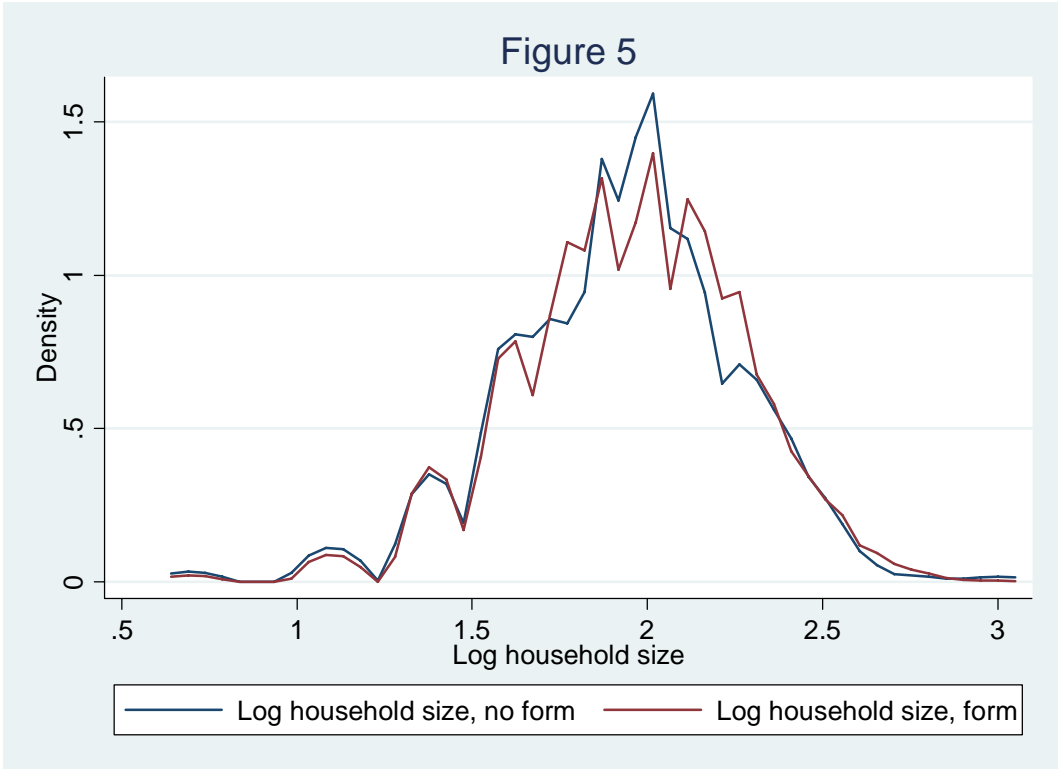


Figure 5: