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Are There Diminishing Returns to Transfer Size in Conditional Cash Transfers?

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Abstract

There is increasing evidence that conditional cash transfer programs can have large impacts on school enrollment, including in very poor countries. However, little is known about which features of program design—including the amount of the cash that is transferred, how frequently conditions are monitored, whether non-complying households are penalized, and the identity or gender of the cash recipients—account for the observed outcomes. This paper analyzes the impact of one feature of program design—namely, the magnitude of the transfer. The analysis uses data from a program in Cambodia that deliberately altered the transfer amounts received by otherwise comparable households. The findings show clear evidence of diminishing marginal returns to transfer size despite the fact that even the larger transfers represented on average only 3 percent of the consumption of the median recipient households. If applicable to other settings, these results have important implications for other programs that transfer cash with the explicit aim of increasing school enrollment levels in developing countries.

This paper—a product of the Human Development and Public Services Team, Development Research Group—is part of a larger effort in the department to study the impact of social programs and their role in promoting human development. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org . The authors may be contacted at dfilmer@worldbank.org and nschady@worldbank.org.

Are There Diminishing Returns to Transfer Size in Conditional Cash Transfers?[♣]

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1. Introduction

Conditional cash transfers (CCTs)—programs that transfer cash to poor households, conditional on them making pre-specified investments in the education and (sometimes) health of their children—have become very popular in developing countries. A recent review (Fiszbein and Schady 2009) estimates that at least 29 developing countries have a CCT in place. In many countries, including Brazil, Ecuador, and Mexico, the CCT is the largest social assistance program, covering millions of households, and accounting for as much as 0.5 percent of GDP.

There is by now a good deal of evidence that indicates that CCTs can have significant impacts on the school enrollment of children in recipient households. That evidence spans countries as varied as Mexico (Schultz 2004), Ecuador (Schady and Araujo 2008), Brazil (de Janvry, Finan, and Sadoulet 2008), Colombia (Attanasio, Fitzsimmons, and Gómez 2005), Nicaragua (Maluccio and Flores 2005), Pakistan (Chaudhury and Parajuli 2008), and Cambodia (Filmer and Schady 2008). The estimated program effects can be quite large—for example, Filmer and Schady (2008) estimate that a program that gave transfers to poor families in Cambodia conditional on them keeping teenage girls enrolled in school raised enrollment rates by between 20 and 30 percentage points.

Despite the popularity of CCTs, little is known about what features of program design—including the amount of the cash that is transferred, how frequently conditions are monitored, whether non-complying households are penalized, and the gender of the recipients of the cash (often, but not always, the mother of the child)—account for the observed outcomes. And yet, identifying the features of the CCT "package" that explain the increase in school enrollment is indispensable to designing more effective and efficient programs.

This paper analyzes the impact of one feature of program design—namely, the magnitude of the transfer. Evidence on the relative importance of the cash amount in a CCT is scarce. de Janvry and Sadoulet (2006) focus on the much-studied PROGRESA program in Mexico. They exploit the fact that there is a cap on the total amount of transfers that a household can receive from the program, which generates different per-child transfers depending on the number of children in a household. de Janvry and

Sadoulet conclude that the marginal effect of the transfer amount is high—in the transition from 6th to 7th grade, when dropout rates in the absence of PROGRESA transfers are high, every US $10 result in an additional 1.42 percentage points in enrollment. (The mean per student transfer is $169.) However, de Janvry and Sadoulet impose linearity in the relationship, so the model does not allow for declining marginal impacts; moreover, the cap in transfer amount is a function of family size and composition, and these characteristics may themselves not be orthogonal with school enrollment decisions, independent of the transfer. Todd and Wolpin (2006) also analyze PROGRESA, but use a structural dynamic model that incorporates fertility and schooling choices to simulate various counterfactual policy alternatives, and allow for varying marginal impacts. After estimating the structural parameters of their model using data from the experimental control group, they simulate halving and doubling the transfer amounts. They find that the impact of the program on mean years of schooling completed is linearly increasing in the amount of the transfer up to the actual transfer amount, and increasing but at a slightly diminishing rate thereafter (up to their simulated doubling of the transfer). Bourguignon, Ferreira, and Leite (2003) analyze the Bolsa Escola CCT program in Brazil. They simulate the effects of alternative transfer amounts allowing for nonlinear effects, and find that doubling, and then quadrupling, the transfer amount leads to successively smaller impacts on the probability of attending school. Despite these earlier contributions, however, there is to date no study that analyzes a program that deliberately altered the transfer amounts for comparable households with the explicit aim of estimating whether households that received larger transfers in fact were more likely to send their children to school than those which received smaller ones.

In this paper, we use data from Cambodia, where a program known as the CESSP Scholarship Program (CSP) made cash transfers of different magnitudes to observationally very similar households, based on an index of the likelihood of dropping out of school.[1] The program design lends itself to an identification strategy based on regression discontinuity. The intuition is that we compare the school enrollment of children "just" above and just below the cutoff for receiving a "large" versus a small transfer—US $60 versus US$45 per year, conditional on school enrollment—and then compare those just

[1] CESSP stands for Cambodia Education Sector Support Project.

above and just below the cutoff for receiving a "small" transfer—US $45 versus no scholarship. We find clear evidence of diminishing marginal returns to transfer size. Indeed, we cannot rule out that the additional $15 has no effect on enrollment over and above the first $45.

The rest of the paper proceeds as follows. In Section 2 we briefly discuss the CSP program, the data, and our identification strategy. Section 3 presents our main set of results and robustness checks. We conclude in Section 4.

2. Program, data, and identification strategy[2]

Program: The program we analyze, the CSP, offers "scholarships" to 6^{th} grade students conditional on enrolling in school in 7^{th} grade, the first year of lower secondary school. These scholarships are renewable for the three years of lower secondary school, conditional on enrollment, regular attendance, and on-time promotion from one grade to the next. Although they are referred to as "scholarships" by the government and donors, these are effectively child-specific CCTs.[3] The money is transferred to the parents, conditional on school enrollment and regular attendance, and program officials do not monitor how the money is spent.

In the initial program period we evaluate, the program operated in 100 of the approximately 800 lower secondary schools in Cambodia. These schools were selected on the basis of administrative data that indicated that poverty rates in the areas served by these schools were high and, by implication, secondary school enrollment rates low. The selection of CSP recipients within eligible schools was done in three stages. First, using administrative data from the 100 CSP schools, program officials identified all of the primary "feeder" schools for every CSP school. (A primary school was designated a feeder school if it had sent graduating students to a given CSP school in recent years.) Second, within feeder schools *all* 6^{th} graders were asked to complete a CSP "application" form—regardless of whether these students or their parents had previously expressed an interest in attending secondary school. The application form

[2] This description of program design and data sources draws from Filmer and Schady (2009a).
[3] The is in contrast to many other CCT programs which identify households for program participation, and then make transfers conditional of the behaviors of all children (and adults) within those households.

consisted of 26 questions about characteristics that were highly correlated with the probability of school dropout, as indicated by an analysis of recent nationwide household surveys; the questions were also reasonably easy for students of this age to answer, and for peers and teachers to validate during the verification stage of the application process.[4] Once completed and validated at the classroom level, forms were collected by head-teachers, and sent to the capital, Phnom Penh. There, a firm contracted for this purpose "scored" them, using the responses and the set of weights that reflected how well each characteristic predicted the likelihood of school dropout in the nationwide household survey analysis. The formula used was the same for every school and, once calculated, the scores could not be revised.[5]

Finally, within every CSP school, all applicants were ranked by the score, regardless of which feeder school they came from. In "large" CSP schools, with total enrollment above 200, 25 students with the lowest value of the score were then offered a scholarship of $60, and the next 25 students with the lowest score were offered a scholarship of $45; in "small" schools, with total enrollment below 200 students, 15 students were offered the $60 scholarship, and 15 were offered the $45 scholarship. In total, just over 3800 scholarships were offered.[6] The list of students offered scholarships was then posted in each CSP school, as well as in the corresponding feeder schools. Once families had been offered a scholarship, those who accepted received the cash award three times a year at widely-publicized ceremonies held in their school.

Data: We analyze the impact of the program among the first cohort of eligible children. These children filled out application forms in May 2005, and the list of scholarship recipients was posted in November 2005. Data are available at numerous points in time. First, we have access to the composite dropout-risk score, as well as the individual characteristics that make up the score for all 26,537

[4] In practice, the form elicited information on household size and composition, parental education, the characteristics of the home (the material of roof and floors), availability of a toilet, running water, and electricity, and ownership of a number of household durables. Forms were filled out in school, on a single day. Students and parents were not told beforehand of the content of the forms, nor were they ever told the scoring formula—both decisions designed to minimize the possibility of strategic responses; for example, by a student seeking to maximize her chances of receiving the award.

[5] The program did allow for a complaints mechanism if an applicant felt they had been wrongly denied a scholarship, but there were virtually no revisions made as a result of this process.

[6] Occasionally, there were tied scores at the cut-off. In these cases, all applicants with the tied score at the cut-off were offered the scholarships.

scholarship applicants. Second, we conducted four unannounced visits to the 100 CSP schools (in February, April, and June 2006, and in June 2007). These data allow us to physically verify school attendance of applicants, and are the main source of data we use to estimate the program effects. Finally, we fielded a household survey of almost 3500 randomly selected applicants and their families in five provinces.[7] The household survey was collected between October and December of 2006, approximately 18 months after children filled out the application forms. For the present analysis, we use these data mainly to check the robustness of the findings from the school visits.

Table 1 summarizes the characteristics of applicants who were offered a $45 scholarship compared with those who were offered no scholarship (left-hand panel), and applicants who were offered a $45 scholarship compared with those who were offered a $60 scholarship—among all applicants (first three columns of each panel) and among applicants who are within 10 ranks of the school-specific cut-off (next three columns of each panel). In each case and for each characteristic, we report three values: the relevant means, and the coefficient on the indicator of a particular "treatment" (a $45 scholarship or a $60 scholarship) from a "dummy RD" regression of the characteristic on the treatment indicator, school-specific intercepts, and a school-specific quartic in the dropout-risk score. This corresponds to our basic estimation specification, discussed in more detail below, and is a standard check on the validity of the regression discontinuity (RD) specification (Imbens and Lemieux 2008).[8] A statistically significant coefficient in these regressions indicates a potential violation of the identification assumption that observations "just above" and "just below" the cutoff are equivalent except for their program participation status.

[7] The provinces are Battambang, Kampong Thom, Kratie, Prey Veng, and Takeo, and the sample was based on randomly selected schools in these five provinces. The survey was limited to applicants ranked no more than 35 places above the cutoff in these schools. This restriction was imposed to maximize the number of schools, while maintaining the density of observations "around" the cut-off—an important consideration when estimating program effects based on regression-discontinuity, as discussed below.

[8] As we discuss below, the regressions that compare $60 and $45 scholarship recipients do not include children who were offered no scholarship at all, while those that compare children who were offered a $45 scholarship with those who were turned down for scholarships exclude children who were offered a $60 scholarship.

Table 1 shows that, as expected, children who were offered a \$60 scholarship tend to be poorer, on average, than those who were offered a \$45 scholarship; those who were offered a \$45 scholarship, in turn, are poorer than children who were offered no scholarship at all. For example, the proportion of children whose mothers attended any schooling is 0.63 among non-recipients, 0.38 among children offered a \$45 scholarship, and 0.23 among children who were offered a \$60 scholarship; the proportion of households that have either a flush toilet or a pit latrine is 0.31 among those who were not offered a scholarship, 0.11 among those offered a \$45 scholarship, and 0.04 among those offered a \$60 scholarship. As expected, these differences become smaller as the sample is restricted to applicants with values of the dropout-risk score that is within ten ranks of the cut-off. Most importantly from the perspective of our identification strategy, once we control flexibly for the composite dropout-risk score, the regression coefficients in the "dummy RD" regressions which allow for discrete jumps at either of the two cut-offs are rarely significant at conventional levels. In the full sample, six coefficients (out of a total of 52) are significant at the 5 percent level or less; when the sample is limited to observations within ten ranks of the cut-off, only one coefficient is significant. As a robustness check on our estimates of CSP program effects, we therefore also present results for this smaller sample.

<u>Identification strategy</u>: The dependent variable throughout our analysis is an indicator variable that takes on the value of one if a child was present on the day of the unannounced school visit, and zero otherwise. In our basic specification, we pool the results from all four visits; as a robustness check, we also run separate regressions by visit.

The basic identification strategy we use in this paper is based on regression discontinuity (RD). We run separate regressions that compare applicants who were offered a \$60 scholarship and those offered a \$45 scholarship; and those that were offered a \$45 scholarship compared to those who were not offered a scholarship. The regressions take the following form:

(1) $$Y_{is} = \alpha_s + f(S_s) + \mathbf{T}_{is}\beta + \varepsilon_{is}$$

where Y_{is} is an indicator variable which takes on the value of one if child (i) is present at school during the unannounced visit, and zero otherwise; α_s is a set of CSP school fixed effects; $f(S_s)$ is the control function, a flexible parameterization of the dropout-risk score. In our main results, we use a school-specific quartic in the score; we also test for the robustness of the results to this choice of functional form. $\mathbf{T}_{is}$ is an indicator variable that takes on the value of one if a student was offered a $60 scholarship (in the regressions that limit the sample to children who were offered either a $60 or a $45 scholarship), or an indicator variable that takes on the value of one if a student was offered a $45 scholarship (in the regressions that limit the sample to children who were offered either a $45 scholarship or no scholarship at all); and ε_{is} is the regression error term. In this set-up, the coefficient β is a measure of the impact of receiving a given scholarship. Standard errors account for clustering at the level of the primary feeder school.[9]

Three things are worth noting about this specification. First, because the score perfectly predicts whether an applicant is offered a $60 scholarship, a $45 scholarship, or no scholarship at all, this is a case of sharp (as opposed to fuzzy) RD. Second, because we focus on the impact of being *offered* a scholarship, rather than that of actually *taking up* a scholarship, these are Intent-to-Treat (ITT) estimates of program impact. Third, as with every approach based on RD, the estimated effect is "local". Specifically, it is an estimate of the impact of the scholarship program around the cut-off. However, where the cut-off falls in terms of the dropout-risk score varies from school to school. This is because the number of students offered a scholarship was the same in every large and small CSP school, respectively, but both the number of 6th graders and the distribution of the underlying characteristics that make up the dropout-risk score varied.[10] The estimates of β are therefore weighted averages of the impacts for these different cut-off values. We discuss this point in more detail below.

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[9] In the models with data pooled across visits, the regression also includes indicator variables indicating which of the school visits the observation corresponds to.

[10] All else being equal, in CSP schools that received more applications, and in those in which children have characteristics that make it more likely they will drop out, a child with a high dropout-risk score is more likely to be turned down for a scholarship than a similar child applying to a school that receives fewer applications or serves a population with a lower average dropout-risk score.

3. Results

Before turning to the estimates of equation (1) we motivate our results by showing school attendance for applicants as a function of the ranking based on the dropout-risk score, relative to the cut-off. We do this showing both the raw average attendance at each value of the relative rank, as well as a quartic in the relative rank.[11,12] Figure 1 has two panels, corresponding to differences between children offered $45 scholarships and those offered no scholarship (left-hand panel), and those offered $60 scholarships compared with those offered $45 scholarships (right-hand panel). In each case, distinct "jumps" at the cut-off would suggest that the program affected the behavior of applicants.

The left-hand panel of Figure 1 clearly suggests that children who were offered the $45 scholarships were more likely to be attending school on the day of the visit than those who were not offered a scholarship; the difference in attendance rates is large, around 25 percentage points. The right-hand side of the panel suggests very little (if any) difference between children who were offered a $60 scholarship and those who were offered a $45 scholarship.

Our main set of results is presented in Table 2. For every specification, we present three panels. In the top panel, the sample is limited to children who were offered a $45 scholarship and those who were offered no scholarship (excluding children who were offered a $60 scholarship); in the middle panel, the sample is limited to children who were offered a $45 or a $60 scholarship (excluding children who were not offered any scholarship); the bottom panel compares the magnitude of the program effects from these two specifications by reporting the average percentage point increase in attendance per dollar of scholarship transfer.

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[11] Because the cut-off falls at different values of the underlying score in different schools, depending on the number of applications, the mean characteristics of applicants, and whether a school was defined as "large" or "small", it is not informative to graph outcomes as a function of the score. Rather, for these figures we redefine an applicant's score in terms of the distance to the school-specific cut-off, so that (for example), a value of -1 represents the "next-to-last" applicant to be offered a $45 (or $60) scholarship within a school, 0 the "last" applicant offered a scholarship, and a value of +1 represents the "first" applicant within a school who was turned down for a scholarship of that amount. The figures then graph outcomes as a function of this relative rank.

[12] As in our main analysis, these figures pool data from the four visits. The regression lines in these figures are based on a single quartic in the relative rank, rather than the school-specific parametrization of the dropout-risk score; also, they do not include the vector of school fixed effects or indicator variables for school visit.

We present the results from six specifications. Specification (1) includes a single control function for all schools (a quartic in the composite dropout-risk score). Specification (2) adds a set of school fixed effects. While this allows for different intercepts in different schools, it still imposes the restriction that a given change in household socio-economic status (as measured by the composite score) is associated with an increase in the probability of enrollment of the same magnitude across all schools. Conceivably, such an assumption of equal control functions may not do justice to the data. For example, there may be differences in school quality that affect not only whether school enrollment is higher in some schools than in others at all levels of socio-economic status (implying different intercepts across schools), but also the gradients between socio-economic status and enrollment (different slopes across schools). Specification (3), our preferred specification, therefore relaxes this assumption by allowing for school-specific quartics in the dropout-risk score (in addition to the school fixed-effects).

Specification (4) is comparable to (3), but limits the sample to applicants whose score placed them within ten ranks of the cut-off. Specification (5) builds directly on the visual evidence in Figure (1); instead of modeling the control function in terms of the dropout-risk score, we include a quartic in each applicant's relative rank—literally, the rank-distance from the cut-off. Finally, specification (6) is based on the household survey rather than the school visits.[13] We note that it is unusual to have data from both unannounced school visits and an independently administered household survey to compare results.

Table 2 makes clear that the $45 scholarship had a very large effect on school attendance. The impact varies from 18 to 28 percentage points across the various specifications. Model (3), our preferred specification with school intercepts and school-specific quartic control functions suggests an impact of 25 percentage points. In contrast, we can never reject the null that the impacts of the $45 and $60

[13] The household data have two potential disadvantages for our purpose. First, the sample is considerably smaller (a sample of just under 3500 applicants, rather than the entire universe of more than 26,500 applicants in the first year of the program). Second, since it is based on reported current enrollment status it is conceivable parents might misreport the school enrollment status of their children, and that this misreporting is itself correlated with whether or not they were offered a scholarship, and of what magnitude. Specifically, the concern is that parents who have received a scholarship are more likely to report that their children are enrolled in school, even if they are not, than are parents who were not offered a scholarship (or who were offered a smaller scholarship); in this case, our estimates of the CSP program effects might be biased upwards. On the other hand, the household survey has an advantage in that it asks parents about enrollment in *any* school, not just a CSP school.

scholarships are equal. The models relying on unannounced school visits, where the sample sizes are large, are estimated quite precisely. The average per-dollar percentage point increase in attendance that results from the first \$45 ranges from 0.40 to 0.62; in contrast, the average per-dollar percentage point increase in attendance that results from the additional \$15 ranges from –0.07 to 0.31. Broadly speaking, the results in Table 2 are consistent with very sharply decreasing marginal returns in school attendance to transfer size in Cambodia.

We extend our basic results in two ways. First, we present separate estimates for each school visit. These results are in Table 3, which shows that we find a similar pattern of effects for each of the four school visits. However, there is some evidence that the impact of the \$45 scholarship diminishes over time: The coefficient on the CSP treatment from the June 2007 visit, when applicants would have been enrolled in 8th grade if they continued in school and did not repeat grades, suggests a program effect of approximately 20 percentage points—as compared to a program effect of approximately 26 percentage points for the three earlier visits, all of which took place when applicants would have been enrolled in 7th grade, if they continued in school. We note that the estimated program effect from the basis of the June 2007 school visit is close in magnitude to that estimated using the household survey, 18 percentage points; the household survey also corresponds to enrollment in 8th grade for children who do not repeat grades. Most importantly for the discussion in this paper, and as with the specifications that pool data from all of the school visits, we find no evidence that the additional \$15 significantly affected school attendance in any of the results based on a single school visit.

The second way in which we extend our results is by considering the question of the possible heterogeneity of program effects by the underlying socio-economic status of recipients. This is important because, by construction, the average applicant who was offered a \$60 scholarship had lower socio-economic status than the average applicant who was offered a \$45 scholarship. Therefore, if scholarships generally have larger effects on the school attendance of better-off children, our results could confound the effect of the size of the scholarship (\$60 versus \$45) with the underlying socio-economic status (very poor versus somewhat less poor)—and bias the results towards a finding of a low marginal impact of the higher transfer amount.

Although this is possible in principle, it seems unlikely on a number of counts. First, a scholarship of a given magnitude will tend to be a higher proportion of income or consumption of poorer families, so we might expect it to have a larger effect on the school enrollment of children in these families. Second, school enrollment and attendance rates tend to be lower among poorer families, so there is more margin for improvement. And finally, a number of studies have shown that transfers tend to have larger impacts on the school enrollment of poorer children, including in Nicaragua (Maluccio and Flores 2005), Mexico (Sengupta, Todd, and Wolpin 2005) and Cambodia (Filmer and Schady 2008, which focuses on an earlier scholarship program, the JFPR, which did not make payments of varying magnitudes).[14]

Nevertheless, to reassure ourselves that our estimates of diminishing marginal returns to transfer size are not confounded by differences in socio-economic status, we proceed as follows. We first run school-specific regressions to estimate the impact of the $45 and $60 CSP scholarship in each of the 100 eligible schools. These regressions are comparable to those described in equation (1) in that they each include a quartic in the score, as before, but the specification now allows for the coefficient β to vary by school. In Figure 2, we then plot the coefficients from these regressions (which were implicitly averaged in the aggregated results in Tables 2 and 3) against the value of the score at the cutoff, separately for the $45 scholarship (left-hand panel) and the $60 scholarship (right-hand panel). Each of these points therefore plots the impact of the program on attendance for schools with lower (left side of the x-axis) to higher (right side of the x-axis) average socio-economic status. We also graph the values from a nonparametric (Fan) regression of the program effect on the value of the score at the cutoff, and the corresponding 95 percent confidence interval, again separately for the two scholarship amounts.[15] Figure 2 makes two things obvious. First, there is considerable overlap in the value of the cut-off for the $45 and

[14] These and other examples are discussed in Fiszbein and Schady (2009).
[15] Confidence intervals are obtained from the distribution of βs from 1000 bootstrap replications of the Fan regressions.

$60 scholarships. In more than two-thirds of schools the "last" child to receive a $45 scholarship has a value of the score between 20 and 30; this is also the case in more than half of the schools for children who receive a $60 scholarship. Second, the left-hand panel of the figure shows that, if anything, the impact of the $45 scholarship *rises* as the cut-off falls at lower values of the composite dropout-risk score, corresponding to children in families who are poorer (although the change in slope is not statistically significant). We therefore conclude that our results on the diminishing marginal returns to transfer size are very unlikely to be driven by the fact that children who were offered $60 scholarships were, on average, somewhat poorer than those who were offered $45 scholarships.

4. Conclusion

A great deal is known about the impacts of CCT programs on a variety of outcomes, including school enrollment. Yet remarkably little is known about what features of program design account for the observed CCT program effects. In this paper, we focus on one important element of program design—namely, the amount of money that is transferred. We find clear evidence of diminishing marginal returns to program size. Indeed, in most specifications we cannot rule out the null hypothesis that households that received a transfer that was one-third larger than that received by other households had the same enrollment response to the program.

This is an important finding, and one which has obvious policy implications. It is particularly noteworthy because the amounts transferred by the CSP program in Cambodia are very small compared to those transferred by other CCTs, in particular in Latin America: The $45 transfer accounts for approximately 2 percent of the consumption of the median recipient household in Cambodia, while the comparable value is 22 percent for recipients of Oportunidades, and 6 percent for recipients of the Bolsa Familía program in Brazil and the Bono de Desarrollo Humano program in Ecuador.[16] This suggests the

[16] These figures are based on Fiszbein and Schady (2009), table 3.2. They are broadly consistent with those reported elsewhere (for example, Schultz 2004 on PROGRESA, and Schady and Araujo 2008 on the Bono de Desarrollo Humano). The exact values depend on the survey used, in particular the year it refers to (because many programs have expanded dramatically, taking on new population groups with different underlying characteristics) and whether

possibility that the marginal returns to transfer size in terms of school enrollment may be quite low, in particular at the high transfer levels that are common in many cash transfer programs in the developing world. Of course, larger transfers may have other positive impacts, such as reductions in consumption poverty, which are additional objectives in many CCT programs.

Further research is needed on a number of counts. It is conceivable that children who received larger transfers did better than those who received smaller transfers in other dimensions—although we have elsewhere shown that children who are brought into school as a result of programs like the CSP appear to learn very little while in school in Cambodia (Filmer and Schady 2009b). In addition, one would ideally want to evaluate a larger number of scholarship sizes to be able to trace out the enrollment impact-transfer size schedule over a broader range. Finally, additional direct experimental or quasi-experimental evidence from other programs would be informative about whether the patterns we observe in Cambodia also occur elsewhere where CCT programs are being designed or implemented.

it covers the population at large or only that included in the sample designed for a specific study of the impact of the program. Other (unconditional) cash transfer programs which have also been found to have an effect on school enrollment, such as the South Africa Old Age Pension (OAP) scheme (see Edmonds 2006) make even larger transfers—in the case of the OAP, the transfer is more than twice the income of the average African (Black) household.

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Table 1: Average characteristics around cutoffs

	No scholarship vs. $45 scholarship						**$45 scholarship vs. $60 scholarship**					
	All			Within 10 ranks of cutoff			All			Within 10 ranks of cutoff		
	No schol.	$45 schol.	Dummy RD	No schol.	$45 schol.	Dummy RD	$45 schol.	$60 schol.	Dummy RD	$45 schol.	$60 schol.	Dummy RD
Male	0.569	0.310	-0.065*	0.387	0.325	-0.022	0.310	0.160	0.057	0.305	0.218	-0.021
Live with mother	0.915	0.840	0.051**	0.839	0.846	0.035	0.840	0.709	0.006	0.837	0.755	0.046
Mother attended school	0.631	0.383	0.024	0.439	0.403	0.113	0.383	0.233	-0.007	0.376	0.281	0.000
Live with father	0.814	0.635	0.035	0.654	0.642	0.105	0.635	0.448	-0.019	0.651	0.512	0.038
Father attended school	0.760	0.458	0.017	0.567	0.470	0.074	0.458	0.287	-0.030	0.463	0.362	-0.086
Parent is civil servant	0.119	0.034	0.008	0.051	0.032	-0.043	0.034	0.027	-0.008	0.038	0.033	-0.040
Num. of other kids in hh	1.195	1.410	0.046	1.354	1.471	0.375	1.410	1.484	0.123	1.453	1.450	0.196
Num. of adults in hh	2.948	2.784	-0.027	2.724	2.815	0.283	2.784	2.634	-0.041	2.781	2.707	0.078
Disabled hh member	0.136	0.187	-0.026	0.196	0.177	-0.101	0.187	0.255	0.061	0.194	0.243	0.045
Own bicycle	0.852	0.579	0.010	0.649	0.600	0.038	0.579	0.397	0.045	0.525	0.466	0.088
Own ox/horses cart	0.386	0.247	0.026	0.297	0.262	0.061	0.247	0.175	0.011	0.248	0.211	0.040
Own motorbike	0.411	0.060	-0.011	0.101	0.072	0.044	0.060	0.018	0.002	0.051	0.025	0.038
Own car or truck	0.068	0.003	0.002	0.014	0.003	-0.003	0.003	0.001	0.001	0.002	0.002	-0.007
Own radio	0.507	0.284	0.002	0.360	0.294	0.063	0.284	0.208	0.037	0.273	0.248	0.105
Own TV	0.689	0.200	0.019	0.305	0.225	-0.034	0.200	0.056	-0.070*	0.181	0.076	-0.038
Hard roof	0.839	0.480	-0.007	0.574	0.506	-0.076	0.480	0.285	-0.043	0.472	0.328	-0.051
Finished floors	0.103	0.016	0.019*	0.019	0.018	-0.001	0.016	0.009	0.005	0.012	0.008	0.003
Wood floors	0.848	0.857	-0.033*	0.922	0.872	-0.071	0.857	0.825	0.038	0.867	0.866	0.038
Piped water	0.044	0.002	0.000	0.003	0.002	-0.002	0.002	0.001	0.000	0.002	0.001	0.003
Well/Pump water	0.749	0.701	0.010	0.700	0.685	-0.091	0.701	0.665	-0.003	0.676	0.664	0.067
Purchased water	0.031	0.026	0.001	0.019	0.029	0.001	0.026	0.021	-0.006	0.025	0.021	-0.033
Flush toilet	0.184	0.011	0.014	0.033	0.013	-0.031	0.011	0.003	0.004	0.004	0.004	-0.004
Pit latrine toilet	0.132	0.062	0.000	0.072	0.065	-0.004	0.062	0.040	0.035	0.055	0.049	0.077*
Generator for lighting	0.065	0.004	0.007	0.006	0.004	-0.009	0.004	0.002	-0.003	0.001	0.002	-0.002
Battery for lighting	0.655	0.343	-0.006	0.425	0.354	-0.041	0.343	0.192	-0.062	0.322	0.221	-0.063
Clean cooking fuel	0.042	0.004	0.008*	0.001	0.005	0.000	0.004	0.000	-0.001	0.002	0.000	0.002

Note: ** indicates that the coefficient in the “dummy RD” is significantly different from zero at the 1% level, * at the 5% level.

Table 2: Impact of different scholarship amounts on attendance—alternative specifications

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Single control function	Single control function with school FE	School-specific control function and school FE	Model (3) restricted to observations within 10 ranks of cutoff	Model (3) with relative rank instead of dropout-risk score	Model (3) using household survey data on school enrollment
			$45 versus $0			
Coefficient	0.260	0.231	0.246	0.278	0.247	0.181**
	(0.015)***	(0.016)***	(0.018)***	(0.060)***	(0.016)**	(0.055)
Observations	95493	95493	95493	8177	95493	2371
			$60 versus $45			
Coefficient	0.024	0.018	-0.010	0.009	0.010	0.047
	(0.014)*	(0.017)	(0.023)	(0.042)	(0.020)	(0.047)
Observations	15334	15334	15334	8117	15334	2162
			Average percentage point increase in attendance per $			
First $45	0.58	0.51	0.55	0.62	0.55	0.40
Next $15	0.16	0.12	-0.07	0.06	0.07	0.31

Note: School visits are pooled across four visits. Standard errors adjust for clustering at the applicant primary-school level. ** indicates that the coefficient is significantly different from zero at the 1 percent level, * at the 5 percent level.

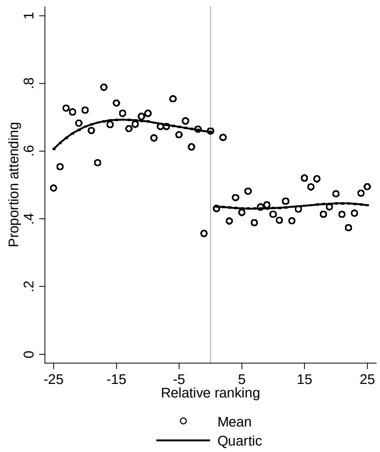
Table 3: Impact of different scholarship amounts on attendance, by school visit

	February / March 2006	April / May 2006	June 2006	June 2007
		$45 versus $0		
Coefficient	0.265	0.265	0.258	0.196
	(0.023)***	(0.022)***	(0.023)***	(0.024)***
Observations	23999	23999	23496	23999
		$60 versus $45		
Coefficient	-0.031	-0.005	-0.017	0.012
	(0.031)	(0.035)	(0.034)	(0.043)
Observations	3853	3853	3775	3853
	Average percentage point increase in attendance increase per $			
First $45	0.59	0.59	0.57	0.44
Next $15	-0.21	-0.03	-0.11	0.08

Note: Standard errors adjust for clustering at the applicant primary-school level. ** indicates that the coefficient is significantly different from zero at the 1 percent level, * at the 5 percent level.

Figure 1: Attendance above and below cutoffs (as non-parametric and quartic functions of relative ranking)

No scholarship versus $45



$60 versus $45 scholarship

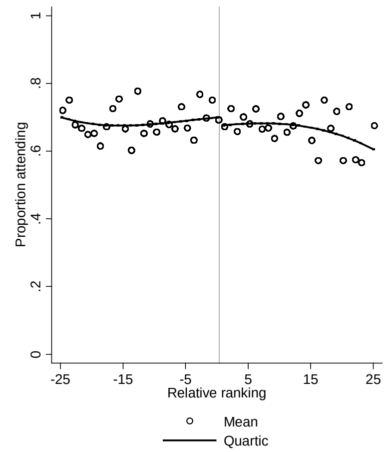
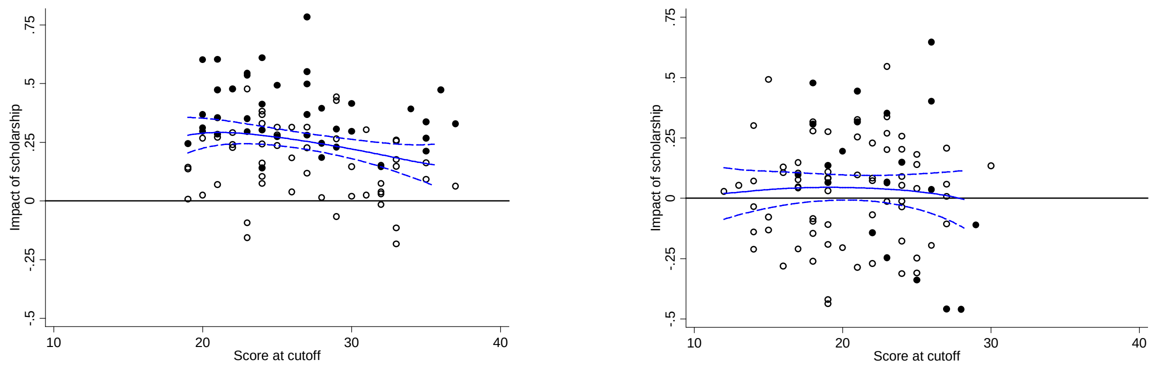


Figure 2: School-specific estimates of impact of scholarship on school attendance, as a function of the dropout-risk score at the cutoff between scholarship levels

Impact of $45 versus no scholarship   Impact of $60 versus $45 scholarship



Note: Solid points indicate that the school-specific estimate of impact is significantly different from zero at the 5% level. Line shows the smoothed relationship based on a non-parametric Fan regression. Dashed line shows 95% confidence interval based on 200 bootstrap replications. Top and bottom 5% of observations have been trimmed.