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Can Cash Transfers Help Households Escape an Intergenerational Poverty Trap?

M. Caridad Araujo, Mariano Bosch, and Norbert Schady

10.1 Introduction

A substantial proportion of households in developing countries are poor.[1] These households suffer from multiple deprivations—low income, poor health, low education levels, poor housing conditions, and inadequate access to a variety of services like potable water and sanitation. Many poor households are also liquidity constrained, and are not able to borrow to invest in the human capital of their children, even if the returns to these investments are high.[2] This, in turn, could result in an intergenerational poverty trap: the children of poor households are more likely to be poor in adulthood in part because of failures in credit and other markets. Programs that directly transfer cash to households are one way of attempting to break the cycle whereby poverty is transmitted from one generation to the next.

Cash transfer programs have become very popular in many developing countries. In Latin America, the largest programs have budgets close to one-

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1. The World Bank estimates that in 2012, 12.7 percent of the world's population was extremely poor (living below a poverty line of US$1.90 per capita per day), while 35 percent lived below a poverty line of US$3.10 (World Bank 2016).

2. There are other reasons why poor parents may underinvest in the human capital of their children, including incomplete altruism or mistaken beliefs about the returns to investments in children.

half point of gross domestic product (GDP) (Levy and Schady 2013). Do cash transfers reduce current poverty? And do they reduce the likelihood that the children of currently poor households will be poor in the future, thus helping households escape an intergenerational poverty trap?

Whether cash transfers reduce *current* poverty depends primarily on the magnitude of the transfer, and on the extent to which households offset transfer income by working less. In practice, a number of evaluations and simulations suggest that cash transfers reduce current income or consumption poverty, especially when the amount transferred is large (Fiszbein and Schady 2009). Separate evidence shows that cash transfers do not reduce work effort in the short run (Banerjee et al. 2017) or medium run (Araujo, Bosch, and Schady 2017).[3]

The extent to which cash transfers reduce *future* poverty hinges largely on whether the children of households that received transfers accumulate more human capital.[4] There is considerable evidence that cash transfers increase school enrollment.[5] Having children enroll in school, however, may not be enough to improve their life chances in adulthood if these children do not complete more years of schooling, or learn little while they are in school.

To credibly assess whether cash transfers can help children escape an intergenerational poverty trap, one needs panel data that follow children from the period in which their parents received cash transfers into adolescence or adulthood. Such data are very infrequent (Molina-Millan et al. [2016] is a review).

Barham, Macours, and Maluccio (2013, 2016) study the long-term effects of cash transfers made in Nicaragua. In one paper (Barham, Macours, and Maluccio 2013) they compare outcomes for children whose families received cash transfers during the potentially critical "first 1,000 days" window (while the child was in utero and in the first two years of life) with children in families that received transfers somewhat later. They find that receiving cash transfers earlier in life raised performance on tests of cognition of boys by 0.15 standard deviations ten years later. Barham, Macours, and Maluccio (2016) focus on transfers received in late childhood. They find that boys who

3. Cash transfers do reduce child labor, as intended. See Attanasio et al. (2010) on Colombia, Edmonds and Schady (2012) on Ecuador, and Maluccio and Flores (2005) on Nicaragua. However, the reductions in child labor are not large enough to offset the effect of the transfer on total household income. A separate question is whether transfer income is spent in the same way as other sources of income. There is some evidence from developing countries that this is not the case. For example, a disproportionate proportion of the transfer appears to be spent on food and on goods that benefit children (see Angelucci and Attanasio [2013] and Attanasio and Lechene [2014] on Mexico; Attanasio et al. [2010] on Colombia; Macours, Schady, and Vakis [2012] on Nicaragua; and Schady and Rosero [2008] on Ecuador). It is unclear whether this is a result of the fact that transfers are made to women, who are likely to have different preferences than men, or the fact that transfers are frequently conditional or "labeled." Conversely, there is no evidence that cash transfers are disproportionately spent on "sin goods" like alcohol and tobacco (Evans and Popova 2014).

4. Cash transfers could also reduce future poverty if households invest the transfer in a productive asset that yields a stream of income in the future. The evidence on this is mixed. See Gertler, Martínez, and Rubio-Codina (2012) on Mexico, and Maluccio (2010) on Nicaragua.

5. Baird et al. (2014) and Fiszbein and Schady (2009) review the evidence.

benefited from cash transfers complete 0.5 more years of schooling, have test scores that are 0.2 standard deviations higher, and have 10–30 percent higher monthly off-farm income ten years later.

However, the results from other evaluations have been less encouraging. In Mexico, Behrman, Parker, and Todd (2009, 2011) conclude that three years of cash transfers (relative to no transfers) resulted in approximately 0.3 more grades of completed schooling, but did not increase performance on tests of reading, writing, and math. In Cambodia, a program that made transfers to families of girls in middle school increased school attainment by 0.6 years, but did not improve test scores or labor market outcomes (employment and earnings) three years after the program had ended (Filmer and Schady 2014).

In this chapter, we study the long-term (ten-year) effects of transfers made by what at the time of our analysis was the largest (in proportional terms) cash transfer program in Latin America, the Bono de Desarrollo Humano (BDH; Human Development Bond) in Ecuador. The BDH made generous transfers—by 2010 transfers accounted for 20 percent of pretransfer income of recipient households, on average. At its peak, the program covered 40 percent of households in the country, and had a budget of 0.7 percent of GDP. Unlike some of the better-known programs in Latin America (like the PROGRESA program in Mexico or the Bolsa Familia program in Brazil), BDH transfers were not explicitly conditional on prespecified behaviors like school enrollment, although households were encouraged to spend transfer income on children (Schady and Araujo 2008).

We present results from two different data sources, two identification strategies, and corresponding to two critical stages in the accumulation of human capital over the life cycle. The first set of results focuses on children who became eligible for transfers in early childhood. Many authors in a number of disciplines have stressed the importance of health and development in the first years of life.[6] Our results are based on data from an evaluation that randomly assigned households to an "early" and "late" treatment group in 2003. All households in the evaluation sample had at least one child under the age of six years at baseline. The early treatment group began to receive BDH transfers in 2004, while the late treatment group only became eligible for transfers three years later. We use data collected in a household survey in 2014 to test whether children in the early treatment group outperform those in the late treatment group in various dimensions. By 2011, the last year in which we have administrative data on BDH payments, the early treatment group had received about twice as much in total transfers as the late treatment group. Despite this, we find no difference between children in the two groups in performance on tests of language, math, attention, working memory, fluency of recovery, and in behavioral outcomes.

6. See, among many important references, Almond and Currie (2010), Cunha and Heckman (2007), and Shonkoff and Phillips (2000).

In Ecuador, like many other middle-income countries, elementary school completion rates are essentially universal. The first critical decision point that determines how much schooling a child attains occurs in secondary school. For this reason, in our second set of results we focus on children in households that were eligible for transfers when these children were of an age at which they were making decisions about secondary school enrollment and completion. We make use of the fact that the BDH program has used a poverty score to determine eligibility for transfers since 2003. This poverty score creates a sharp cutoff in eligibility. We compare the school attainment and employment status of young adults, ages nineteen to twenty-five in 2013/14, in households that were just-eligible and just-ineligible for cash transfers between 2003 and 2009.

We find that young adults in households that were just-eligible for transfers are more likely to have completed secondary school. However, the magnitude of the effect is modest, between 1 and 2 percentage points (from a counterfactual of 75 percent). Program impacts appear to be somewhat larger (and are only significant) among women than men. On the other hand, we do not find that BDH transfers increased employment among young adults. This does *not* appear to be because transfer recipients are more likely to continue on to tertiary education—there is no effect of the transfer on the probability that a young adult is enrolled in an educational institution in 2013/14. Rather, it appears that cash transfers prevented a small fraction of women from dropping out of school before completing secondary school, but did not have a measurable effect on their later education or work choices.

In sum, our analysis shows that children in households that received larger cash transfers in early childhood do not have better outcomes in late childhood than those who received substantially smaller transfers, while cash transfers received in late childhood had small effects on the school attainment of young adults. We conclude that, although our results are not definitive, it is likely that cash transfers will have at most a modest effect on the probability that the children of poor households in Ecuador will escape poverty in the future.

The rest of the chapter proceeds as follows. In section 10.2, we describe the BDH program, earlier evaluations, and education in Ecuador. Section 10.3 discusses our identification strategy, and section 10.4 presents results. We conclude in section 10.5.

10.2 Context

10.2.1 Cash Transfers in Ecuador

The current welfare system in Ecuador, which includes the BDH program, dates back to the late 1990s. In 1999 the country suffered from a severe banking crisis, GDP per capita fell by 32 percent in a single year, and unemployment increased from 9 to 17 percent. In this context, the Ecuadorean government created a cash transfer program, the Bono Solidario (Solidarity Bond). Payments were intended to go to poor households. However, because the program did not have clear selection criteria, many recipients were non-poor, and many poor households did not receive transfers.

In 2000/02, the government carried out a "poverty census" known as the Selben; the Selben covered about 90 percent of households in rural areas, and about the same fraction of households in select urban areas that were judged to have a high incidence of poverty. It gathered information on household composition, education levels, work, dwelling characteristics, and access to services. This information was aggregated into a poverty score by principal components. Beginning in March 2003, this poverty score was used to determine eligibility for transfers. The name of the program was also changed from Bono Solidario to Bono de Desarrollo Humano.

New poverty censuses were carried out in 2007/08 and 2013/14. Once again, the information was aggregated by principal components, and new poverty scores were calculated in 2009 and 2015. In both cases, the change in the poverty score resulted in considerable reshuffling of households in and out of eligibility.[7]

Transfer payments in Ecuador have grown in magnitude over time. Bono Solidario began with a seven dollar transfer per household. With the creation of the BDH in 2003, the transfer increased to fifteen dollars, was revised upward in 2009 (to thirty-five dollars), and increased again in 2014 (to fifty dollars). Payments have also grown as a proportion of household income of the poor—from 13 percent of the pretransfer income of the poorest 40 percent of the population in 1999 to 20 percent a decade later.

10.2.2 Earlier Evaluations of the BDH Program

There are numerous evaluations of the impact of BDH transfers on a variety of outcomes. These are based on different samples and different identification strategies. Paxson and Schady (2010) use a randomized experiment to evaluate the short-term effects of transfers on the development of young children. They find no effects of the transfers, on average. However, among the poorest households, BDH transfers improved child physical development (by 0.16 standard deviations) and cognitive and socioemotional development (by 0.18 standard deviations).[8] Using data from the same

7. For example, 36 percent of all households in the first poverty census had scores that placed them within 5 points of the cutoff that determined eligibility for transfers. Among these households, 46 percent of those eligible for transfers by the first poverty census became ineligible, and 42 percent of households who were ineligible became eligible.

8. The measure of child physical development is based on three outcomes: child height, elevation-adjusted hemoglobin, and a measure of fine motor control. The measure of child cognitive and behavioral development is based on five outcomes: language development, tests of short- and long-term memory, a test in which a child is asked to find patterns in pictures, and the Behavioral Problems Index (BPI), a commonly used scale that is based on the frequency that a child displays each of twenty-nine behaviors, as reported by her mother. Within a composite, each individual outcome receives the same weight.

experiment, Fernald and Hidrobo (2011) show that the program improved outcomes of infants and toddlers, while Hidrobo and Fernald (2013) argue that the intervention reduced domestic violence. Schady (2012a) shows that BDH transfers reduced the proportion of adult women who were anemic. In our chapter, we use panel data from this evaluation. Specifically, we follow children who were five years of age or younger at baseline into late childhood to test for program effects ten years later.

A second randomized experiment of the BDH focused on households with school-age children at baseline. With these data, Schady and Araujo (2008) find that BDH transfers substantially increased school enrollment, especially among households who (erroneously) believed that the transfers were conditional on enrollment. Positive effects of transfers on school enrollment are also reported by Oosterbeek, Ponce, and Schady (2008). Edmonds and Schady (2012) show that BDH transfers substantially reduced child labor. Schady and Rosero (2008) find that transfers resulted in an upward shift of the food Engel curve—households who were eligible for transfers spent a higher fraction of income on food.

Others have exploited the fact that the BDH program used a poverty score to determine eligibility as a source of identification. Ponce and Bedi (2010) report positive program effects on tests of language and math achievement. An important limitation of their paper, however, is that they do not have data on the actual poverty score of households. Rather, they attempt to recreate poverty scores with data from a household survey. Araujo, Bosch, and Schady (2017) analyze whether transfers affected the work decisions of adults. The data they use include the household poverty scores that the BDH program used to determine eligibility for transfers. Their analysis, which is similar in spirit to that which we carry out in this chapter, finds that the BDH did not reduce work effort. However, transfers appear to have shifted some women from formal to informal employment as a way of hiding income.

In sum, there are a number of earlier evaluations of the BDH program, including on schooling outcomes. However, with the exception of Araujo, Bosch, and Schady (2017), all of these evaluations have focused on short-term impacts. The most important contribution of our chapter is that we study the effects of cash transfers on young children (birth to five years of age) and somewhat older children (nine to fifteen years of age) after ten years.

10.2.3 Schooling in Ecuador

Schooling in Ecuador is compulsory from five to fourteen years of age. The elementary school cycle runs from kindergarten to 6th grade, and secondary school from 7th through 12th grades. Eighty percent of school-age children are enrolled in public schools, with the remainder in private schools. After secondary school, there are a large number of vocational colleges, technical schools, and universities, both public and private.

Ecuador has made considerable progress expanding the coverage of the education system, as can be seen in figure 10.1.[9] Panel A shows that school enrollment of children of elementary school age is close to universal—over 99 percent of children age six to eleven are enrolled in school. Panel B shows there have been substantial increases in elementary school completion over time. Averaging across men and women, the proportion who graduated from elementary school increased from 65 percent for the cohort born in 1950–1954 to 94 percent for the cohort born in 1985–1989. Importantly, this suggests that there is little room for a cash transfer program like the BDH (or any other program) to affect the school enrollment of young children or elementary school completion rates.

Turning to somewhat older children, figure 10.1, panel A, shows that, after age eleven, school enrollment declines gradually: at age fifteen, 92 percent of children are enrolled in school, and at age eighteen, only 49 percent of individuals are enrolled in some educational institution. Panel C, finally, shows that secondary school completion rates have gone up sharply over time—from 24 percent for the cohort born in 1950–1954 to 58 percent for the cohort born in 1985–1989. However, even in the most recent cohorts, a substantial proportion of adults dropped out before completing secondary school. It follows that cash transfers could, in principle, increase school enrollment and attainment for this age group.[10]

Finally, figure 10.1 shows that educational gaps between men and women have closed over time. As is the case in other Latin American countries, there are now no substantive differences in enrollment rates of boys and girls. For the most recent cohorts, there are no differences by gender in graduation rates from elementary or secondary school, either.

Although school enrollment rates in Ecuador have gone up, the quality of education is a serious challenge. Ecuador does not participate in the international PISA (Programme for International Student Assessment) tests, so it is hard to benchmark the performance of children in high school in Ecuador relative to other countries. However, Ecuador was one of fifteen countries in Latin America that participated in TERCE, a test of 3rd and 6th grade children carried out in 2013. In 3rd grade math, 47.8 percent of children in Ecuador had the lowest of the four levels of performance on the test, very similar to the average for Latin America (47.2 percent), but substantially more than higher-performing countries like Costa Rica (23.1 percent) or Chile (15.4 percent) (UNESCO 2015). Results are very similar for 6th graders.

9. To carry out these calculations, we used the 2015 Encuesta Nacional de Empleo, Desempleo y Subempleo (ENEMDU), a nationally representative household survey in Ecuador.

10. Evidence from a number of evaluations shows that, unsurprisingly, cash transfers have the biggest effects on school enrollment for children in grades where dropout is high. See Barrera-Osorio et al. (2011) on Colombia, de Janvry and Sadoulet (2006) and Schultz (2004) on Mexico, Filmer and Schady (2008, 2014) on Cambodia, and Schady and Araujo (2008) on Ecuador.

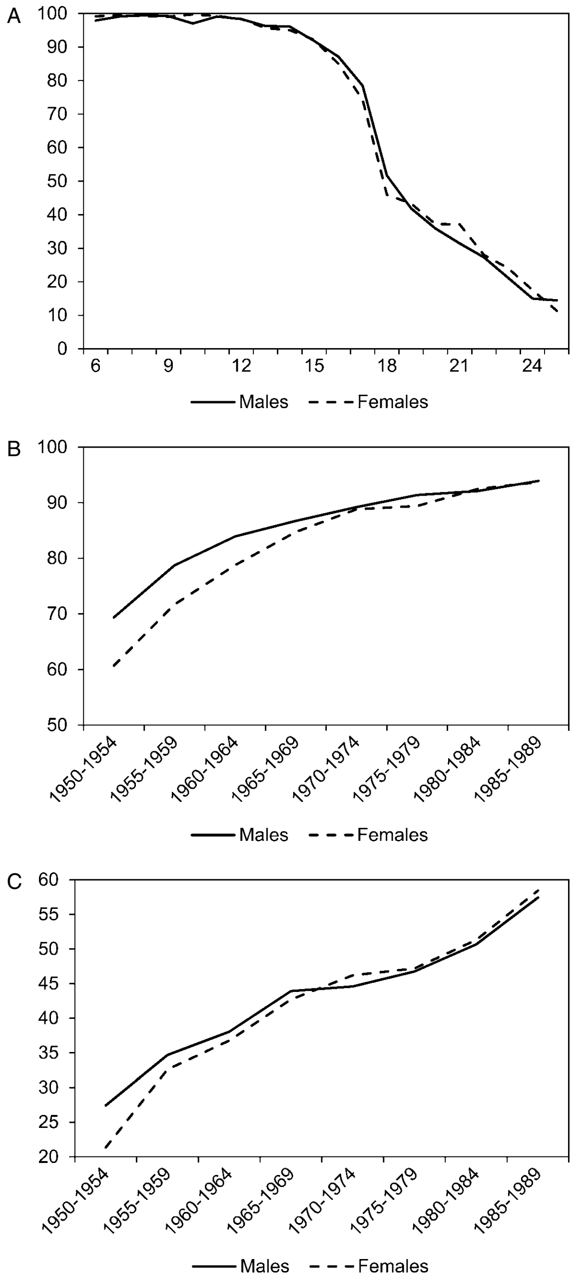

Fig. 10.1 Schooling outcomes in Ecuador by gender. *A*, school enrollment by age; *B*, elementary school completion by birth cohort; *C*, secondary school completion by birth cohort.

Note: Own calculations based on the 2016 ENEMDU household survey.

There are steep socioeconomic gradients in test scores of school-age children in Ecuador (Berlinski and Schady 2015). To a large extent, these gradients are already apparent before children enter school (Paxson and Schady 2007; Schady et al. 2015). The deep deficits in cognitive development, and the low test scores of poor children in Ecuador, suggest that cash transfers could in principle improve learning outcomes if transfer income were spent in a way that benefits children.

10.3 Data and Identification Strategy

10.3.1 Experimental Analysis

As discussed above, one set of estimates we report is based on data from a panel of households that have been followed since 2003. Households in this panel were part of a randomized evaluation of the impact of cash transfers on child health and development (see Paxson and Schady [2010] for a discussion). One group of households was randomly assigned to an early treatment group, and another to a late treatment group.[11] At baseline, all households in both groups had at least one child under the age of six years.[12] The baseline survey was collected between October 2003 and March 2004; follow-up surveys have been carried out regularly since then, most recently in 2014.[13]

Figure 10.2 shows that the proportion of households in the early treatment group that received transfers rose sharply after June 2004, when they were first made eligible; by March 2005, roughly 50 percent of households in this group received transfers in any given month. The figure also shows that the proportion of households in the late treatment group that received transfers increased steadily after March 2007, when they in turn were first made eligible; however, the take-up of the BDH increased more slowly in this group, and never fully caught up with the early treatment group. It is likely that this occurred because some households in the late treatment group never realized that their eligibility status had, in fact, changed. In any event, by the end of 2011 (the last point at which we have payment data for this sample) households

11. Random assignment was done at the parish level. Parishes are the smallest administrative units in Ecuador. Fifty-one parishes were assigned to the early treatment and twenty-six to the late treatment group. Within these parishes, a sample of households who were in principle eligible for transfers given their poverty score, but had never received payments, was selected.

12. An additional requirement was that households in the sample did not have any children six years of age or older. Payments made by the BDH are not conditional on any prespecified household behaviors. At an early stage, however, program administrators considered making the program conditional on regular health checkups for households with young children, and on school attendance for households with older children. It was not clear which condition would apply to households that had both younger and older children. For this reason, the evaluation design required that households in the sample have young children, but not older children.

13. The original sample included households in urban and rural areas. Since 2005, however, only households in rural areas have been followed. For this reason, our analysis is restricted to households in the rural sample.

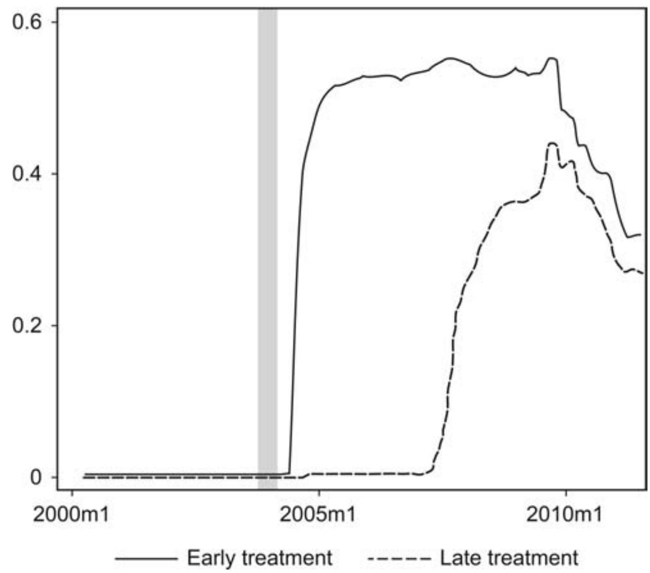

Fig. 10.2 Cash transfers received by the early and late treatment groups, experimental sample

Note: Sample size is 1,338 children, 898 in the early treatment and 440 in the late treatment group. These sample sizes are somewhat smaller than those in the estimation sample in figure 10.5 and table 10.2 because we could not merge all of the households in the survey with the monthly payment data from the BDH. The discrepancy is likely due to errors in reporting, collecting, or inputting the data on national identifiers (*cédulas*) during the household survey.

in the early treatment group had received approximately twice as much in transfers as those in the late treatment group (US$1,200, compared to $625, on average).[14]

Paxson and Schady (2010) show that the characteristics of the early and late treatment groups were balanced at baseline. Attrition between the baseline survey and the 2014 follow-up ten years later was modest, 19 percent, and is uncorrelated with assignment to the early or late treatment groups. Moreover, the characteristics of attritors in the early and late treatment groups are similar. The 2014 survey, which is the basis of the analysis we carry out in this chapter, administered a particularly rich set of tests, includ-

14. Figure 10.2 also shows that, beginning in December 2009, the proportion of households in the evaluation sample that received payments began to decline, and by September 2011 had fallen by roughly 20 percentage points in the early treatment group (15 percentage points in the late treatment group, where take-up was lower). This decline is a result of the change in the poverty score from the first to the second poverty census. The change in the score meant that a substantial proportion of households in the sample were no longer eligible for payments (because their score on the second poverty census placed them above the cutoff for eligibility); no new households entered the evaluation sample.

ing three language tests; four math tests; tests of attention and working memory; and two other tests that measure fluency of recovery and the incidence of behavior problems, respectively. Details of the tests we use in our analysis are given in the data appendix.

We transform the raw scores on each test into a z-score with zero mean and unit standard deviation. We then construct three test aggregates (for language, math, and "other tests"). Each test within an aggregate receives the same weight, and the aggregate, in turn, is standardized so it too has a mean of zero and a standard deviation of one. We also construct an overall aggregate, which equally weights the three groups of tests.

Given random assignment, identification is straightforward. We report the results of intent-to-treat regressions that take the following form:

$$Y_{ihp} = \alpha_c + Z_{ihp}\beta_1 + \boldsymbol{X}_{ihp}\beta_2 + \varepsilon_{ihp} \quad (1)$$

where the i, h, and p subscripts refer to individuals, households, and parishes; Y_{ihp} is one of the four test aggregates; α_c is a set of canton fixed effects;[15] Z_{ihp} is a dummy variable for whether the child in question is in a household that was assigned to early or late treatment groups; $\boldsymbol{X}_{ihp}$ is a vector of baseline characteristics, which we include to correct for any possible imbalance between early and late treatment groups and to increase precision; and ε_{ihp} is the error term. We run regressions by ordinary least squares (OLS), and cluster standard errors at the parish level. The parameter of interest is β_1, the intent-to-treat estimate of the effect of being assigned to the early treatment group on test scores.

10.3.2 Regression Discontinuity (RD) Analysis

To generate the data set for the second set of estimates, we merged data from three different sources:[16] data on a household's poverty score calculated from the 2000/02 poverty census; monthly data on welfare payments (from BDH administrative records); and data on education and work outcomes, as reported in the 2013/14 poverty census.[17]

The approach we take is straightforward. We compare outcomes for households who were just-eligible or just-ineligible for transfers between

15. Cantons are administrative units at a higher level than parishes, comparable to municipalities.

16. All of these data are confidential. The process of merging the various data sets was carried out by staff of the BDH program and the Ministry of Social Development in Ecuador. The data set we use has been made anonymous by removing the *cédula*, the unique individual identifier that is present in all these data sets and is used to merge them.

17. An advantage of the RD sample is the large number of observations. The disadvantage, on the other hand, is the small number of outcomes. To define school enrollment we use a single question on the poverty census: "Are you currently attending some educational institution?"; similarly, to define "work" we use a single question: "In the last week, did you work at least one hour, with or without pay?" Both questions are asked of all household members ages five and older.

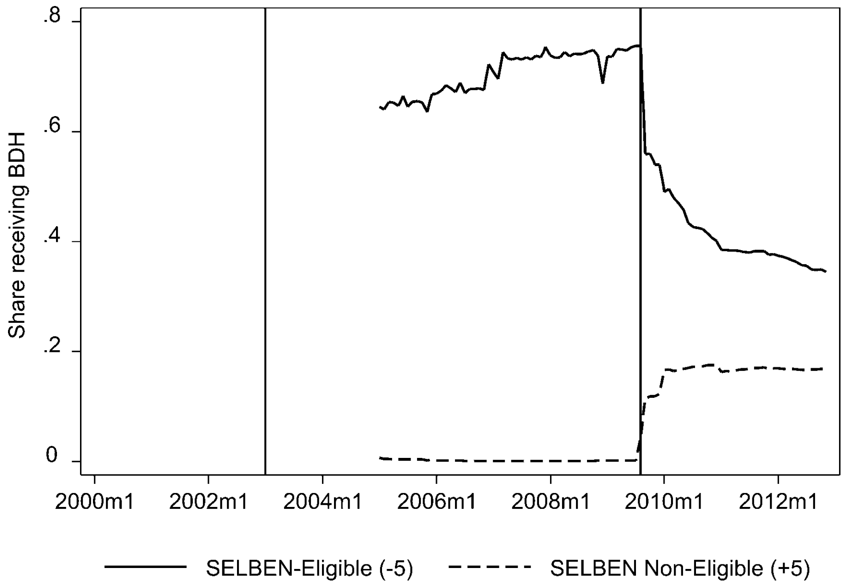

Fig. 10.3 Cash transfers received by eligible and ineligible households, regression discontinuity sample

Note: The figure depicts the share of eligible and ineligible households (within 5 points of the eligibility cutoff, as determined by the 2000/02 poverty census) receiving BDH cash transfers. Sample size is 249,846 households (41,530 transfer-ineligible and 208,316 transfer-eligible). Calculations are based on the administrative data on payments kept by the BDH.

2003 and 2009 (based on their poverty score, calculated with the 2000/02 poverty census). To assess program impacts on school enrollment, educational attainment, and work, we use the responses on the 2013/14 poverty census.

We begin by verifying that the 2000/02 poverty score was in fact used to determine eligibility for cash transfers in the 2005–2009 period (data on payments for this sample are not available before 2005). Figure 10.3 clearly indicates that this was the case. The proportion of eligible households who received transfers in any given month is between 70 and 80 percent, while the proportion of ineligible households who received transfers is essentially zero. On the other hand, the differences in transfers between the two groups are much smaller after 2009 (when a new poverty score, based on the 2007/08 poverty census, was used to determine eligibility).

The regressions we run to estimate BDH program impacts take the following form:

$$(2) \qquad Y_{ihc} = \alpha_c + S_{ihc}\beta_1 + I(S_{ihc} < C)\beta_2 + I(S_{ihc} < C) * S_{ihc}\beta_3 + \varepsilon_{ihc}$$

where Y_{ihc} is an outcome for young adult i in household h and canton c; α_c is a set of canton fixed effects; S_{ihc} is a parametrization of the control function, the poverty score calculated on the basis of the 2000/02 poverty census; $I(S_{ihc} < C)$ is an indicator variable that takes on the value of one for individuals whose 2000/02 poverty score placed them below the cutoff for eligibility; $I(S_{ihc} < C) * S_{ihc}$ is an interaction term between the control function and the eligibility dummy; and ε_{ihc} is the error term. We run regressions by OLS, and cluster standard errors at the parish level. The parameter of interest is β_2, the intent-to-treat effect of cash transfers on enrollment, educational attainment, and employment in young adulthood.

As in other applications of RD, it is important to ensure that results are not driven by a particular parametrization of the control function. In our preferred specification, we run local linear regressions (LLRs) and determine the optimal bandwidth using the approach recommended in Imbens and Kalyanaraman (2012). To check for robustness, we also report results with different bandwidths, as well as estimates that use the full sample of young adults and control for a quartic (rather than just a linear term) in the control function. Also, as in other applications of RD, it is important to note that the results we present are local in the sense that they only apply to individuals at the 2000/02 eligibility cutoff.

We report the results from two standard RD checks. First, panel A of figure 10.4 shows that there is no unusual heaping of households on one or the other side of the eligibility cutoff. Second, we test for differences in the observable baseline characteristics of households who are just-eligible and just-ineligible for transfers. Table 10.1 shows that the differences at the cutoff are generally small, and are only significant at conventional levels for one out of sixteen characteristics (two if we include differences that are borderline significant).

In interpreting our RD estimates, two additional considerations should be kept in mind. First, if there are positive spillovers from eligible to ineligible households, as has been suggested for secondary education by Bobonis and Finan (2009) and Lalive and Cattaneo (2009) using data from the PROGRESA cash transfer program in Mexico, then our estimates would be a lower bound on the true underlying effect of transfers on enrollment and secondary school completion. If, on the other hand, there are negative spillovers, as might occur because of crowding of classrooms, then the estimates we report would be an upper bound on the effects of BDH transfers.

Second, an additional concern arises because of the way we merge the different data sets we use in our analysis. In each household covered by the 2000/02 poverty census, the BDH recorded at least one *cédula* (national ID number), generally of one adult woman (who would then become the recipient of BDH transfers if her poverty score placed her below the eligibility cutoff). We use the *cédula* to merge data from the two poverty censuses. In our analysis, we then test whether young adults in households of adults for whom we have the *cédula*, as recorded in 2000/02, have different schooling

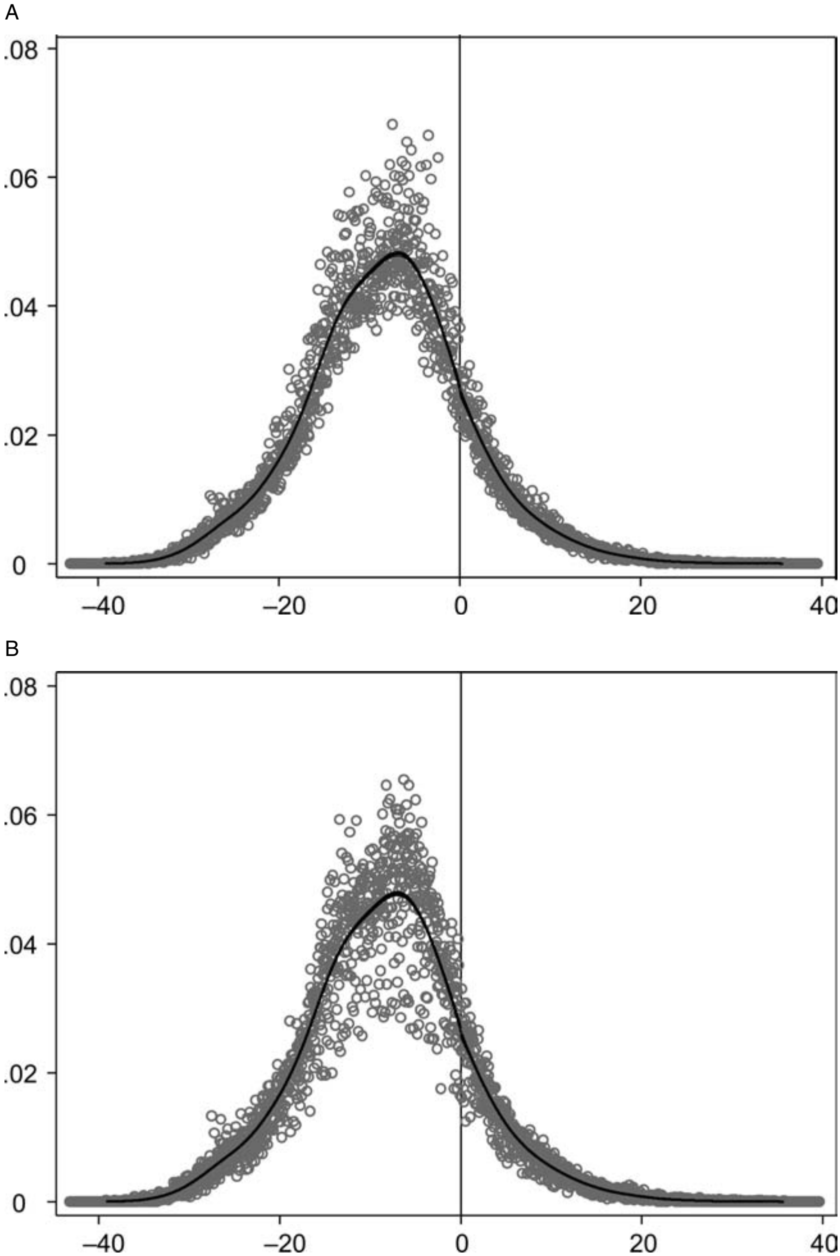

Fig. 10.4 Density of observations around eligibility cutoff. *A*, households with at least one young adult age nineteen to twenty-five; *B*, young adults age nineteen to twenty-five.

Note: This graph includes all young adults age nineteen to twenty-five in 2013/14 in the merged sample. Sample size is 307,394 observations (B) in 249,846 households (A). The McCrary test is −0.020 (0.019) in A and −0.023 (0.017) in B.

Table 10.1 **Balance in the regression discontinuity sample**

	Eligible households	Ineligible households	Difference
Data from 2000/02 poverty census			
Urban	0.784	0.748	0.020***
Dwelling is house or apartment	0.852	0.777	−0.011*
Has unfinished floors	0.242	0.383	0.004
Has toilet indoors	0.573	0.387	0.002
Has shower indoors	0.304	0.145	0.005
Has gas stove	0.983	0.964	0.002
Has electricity	0.998	0.992	−0.001
Owns lands	0.173	0.167	−0.002
Number of rooms	2.563	2.189	0.010
Individual-level data (for those with *cédula*)			
Age	0.41	0.47	0.025
Years of schooling	5.26	6.51	0.203
Household head	0.47	0.50	0.022
Working	0.45	0.47	0.024
Data from 2013/14 poverty census			
Household size	4.099	4.154	−0.015
Number of children ages 0–15	0.777	0.878	−0.007
Number of young adults ages 19–25	1.444	1.481	0.013

Note: Sample size for all calculations is 249,846 households. The values for the columns labeled "eligible households" and "ineligible households" are means for all households below and above the eligibility cutoff, respectively. The value for the column labeled "difference" is the coefficient on the eligibility dummy, based on our preferred LLR as discussed in the text. These regressions also include the poverty score, the interaction between the poverty score and the eligibility dummy, and canton fixed effects. Standard errors are clustered at the parish level.

***Significant at the 1 percent level.

*Significant at the 10 percent level.

and work outcomes, as recorded in the 2013/14 poverty census, depending on eligibility for transfers during the 2003–2009 period.[18]

The fact that we merge observations in the two censuses using a woman's *cédula* is potentially important. It means that our estimates are only based

18. We were able to merge 55 percent of all households in the 2000/02 poverty census into the 2013/14 census. There are many reasons why one would not expect a perfect merge: First, the geographic coverage of the 2013/14 poverty census was smaller than that of the 2000/02 poverty census. Second, participation in the two poverty censuses was not mandatory. Households that reasoned that, given their socioeconomic status, they would be unlikely to be eligible for BDH transfers may simply have chosen not to participate in the 2013/14 census. (In actual fact we do find that relatively wealthier households in 2000/02 are less likely to be found in 2013/14, although there are no differences at the cutoff.) Third, there may have been keying errors in the *cédula* in either census. Fourth, some household members may have died, or "aged out" of being the household head or his spouse (in which case, enumerators may have registered the *cédula* of some other household member). We do not believe that the less-than-perfect merge of households across the two censuses affects the internal consistency of our results because there is no unusual heaping of mass on one or the other side of the cutoff, and because the characteristics of individuals on both sides of the cutoff are very similar, as discussed in the main body of the

on, and are potentially only relevant for, the behavior of young adults who continue to live in the household they were in as children (and not for those who start their own household or move into a different one). This may limit the generalizability of our results.[19]

Moreover, if eligibility for transfers in 2003–2009 made it more (or less) likely that a young adult left home, our estimates could in part pick up these compositional changes. To test for this, we plot the number of young adults (as opposed to households) in panel B of figure 10.4. The panel shows there is no evidence of heaping of young adults on one or the other side of the cutoff.[20] We conclude that, while we cannot definitively rule out that there are compositional changes in households that are correlated with eligibility for transfers ten years earlier, these are unlikely to be a first-order concern.

10.4 Results

10.4.1 Results from Randomized Evaluation

The main results on the impact of BDH transfers using the randomized evaluation are in table 10.2 and figure 10.5. Table 10.2 reports ten-year program effects for the sample as a whole; separately for children who were younger than three years of age (including children who were in utero) at baseline and children who were older; for girls and boys; and for children whose mothers had at most completed elementary school and those whose mothers had higher school attainment. In each case, we report the results from regressions in which the outcome variable is total scores and, separately, language, math, or "other scores," respectively.

Table 10.2 shows that, for no sample and for no test aggregate, are there positive and significant program effects. In fact, the coefficients are overwhelmingly *negative* (albeit, they are close to zero and are not significant with one exception, corresponding to the impact of transfers on language outcomes for younger children).

chapter. In a small number of cases, 5.6 percent of the total, there is a household in the 2013/14 for whom we can merge the *cédula* of more than one individual, and where these individuals were found in two or more households in the 2000/02 census. This raises a complication because the household in which these individuals live in 2013/14 could in principle be assigned one of two or more poverty scores from the 2000/02 census. In these cases, we assign households the lowest of the relevant poverty scores. However, our results are very similar when we simply exclude these households, and the young adults in them, from our calculations.

19. We used the dates in which the two poverty censuses were carried out and the age of individuals in both censuses to see if children in the 2000/02 poverty census could be matched with young adults in the same households in 2013/14. Among households we could match, we were able to find 46 percent of children in the 2000/02 poverty census as equivalently aged young adults in the same households in the 2013/14 census.

20. It is of course possible that different kinds of young adults left just-eligible and just-ineligible households. We cannot test for this in a convincing manner because the only information we have for children at baseline is their age, gender, whether they were enrolled in school, and whether they worked. However, in the age range we consider, essentially all children were enrolled in school, and virtually none of them worked.

Table 10.2 **Experimental estimates of BDH effects on test scores after ten years**

	Total scores	Language scores	Math scores	"Other" scores
Full sample (n = 1,707)	−0.071	−0.060	−0.090	−0.023
	(0.083)	(0.068)	(0.094)	(0.064)
Children −9 to 35 months at baseline (n = 612)	−0.081	−0.170*	−0.039	−0.017
	(0.078)	(0.088)	(0.087)	(0.065)
Children 36 months or older at baseline (n = 1,095)	−0.068	−0.001	−0.125	−0.022
	(0.107)	(0.084)	(0.119)	(0.089)
Females (n = 858)	−0.050	−0.009	−0.110	0.014
	(0.078)	(0.061)	(0.093)	(0.082)
Males (n = 849)	−0.070	−0.094	−0.052	−0.041
	(0.108)	(0.102)	(0.118)	(0.079)
"Low" education mothers (n = 1,123)	−0.026	−0.021	−0.071	0.041
	(0.088)	(0.081)	(0.086)	(0.083)
"High" education mothers (n = 584)	−0.178	−0.165	−0.137	−0.167
	(0.140)	(0.120)	(0.170)	(0.102)

Note: All regressions include canton fixed effects and the following controls: gender of the child, age at baseline in months, maternal years of education, household size, and the number of durables owned by household. Standard errors are clustered at the parish level.

*Significant at the 10 percent level.

Figure 10.5 presents results by cumulative ventile of the distribution of per capita expenditures at baseline.[21] We do this in part because Paxson and Schady (2010) found that BDH transfers did not have significant effects on child development for the sample as a whole, but substantially improved outcomes for children in the lowest quartile of the distribution of per capita expenditures. Figure 10.5 shows that the estimates become more precise as we move from left to right in each panel, as expected given the larger sample sizes. However, there is no evidence that receiving transfers earlier in life, or receiving more in total transfers, improved test scores anywhere in the distribution of per capita expenditures.

21. The first (leftmost) value in each panel of figure 10.5 corresponds to coefficients and confidence intervals for regressions that limit the sample to the 5 percent poorest households, the next corresponds to the 10 percent poorest households, and so on. The rightmost value in each panel corresponds to the sample as a whole, and is equivalent to the estimates in table 10.2. Log per capita expenditures is "imputed." As Paxson and Schady (2010) discuss, the baseline 2003/04 survey collected information on housing characteristics and ownership of a list of household durables, but did not include an expenditure module. A companion study collected the same information on housing and durables and included an expenditure module. (These data are the basis for the analysis in Schady and Araujo [2008], Schady and Rosero [2008], and Edmonds and Schady [2012].) Paxson and Schady (2010) used data from this companion study to estimate a regression of the logarithm of monthly expenditure on measures of housing quality and access to services, durable goods ownership, and several household characteristics such as the household head's age and education level and household size, and used the resulting coefficients to impute the logarithm of expenditure at baseline for the sample of households in the panel. We use the same measure in the analysis in this chapter.

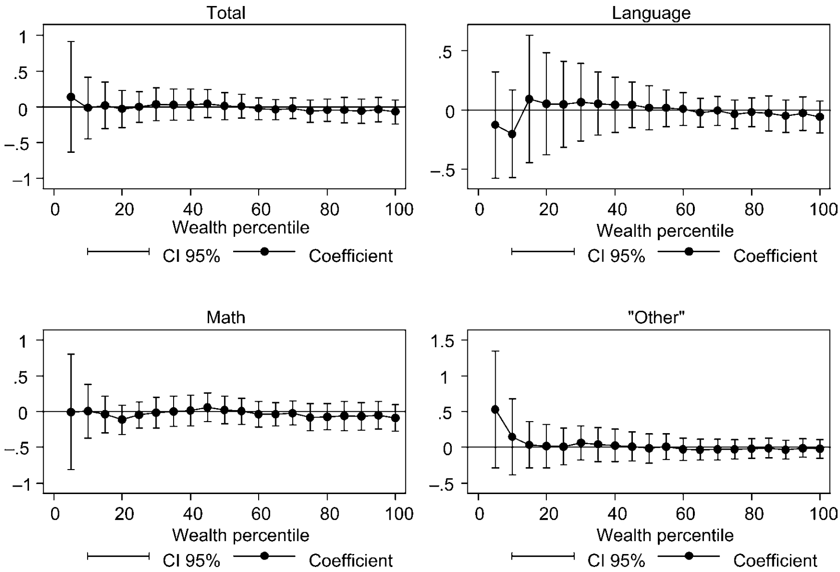

Fig. 10.5 Program effects on test score aggregates, by ventile of baseline per capita consumption, experimental sample

Note: Each figure depicts coefficients and 95 percent confidence intervals from regressions of outcomes on a dummy variable for children in the early treatment group, and by cumulative ventile of the distribution of per capita expenditures at baseline. The first (leftmost) value in each figure corresponds to coefficients and confidence intervals for regressions that limit the sample to the 5 percent poorest households, the next corresponds to the 10 percent poorest households, and so on. The rightmost value in each figure corresponds to the sample as a whole, and is equivalent to the estimates in table 10.2. All regressions include canton fixed effects and the following controls: gender of the child, age at baseline in months, maternal years of education, household size, and the number of durables owned by the household. Standard errors are clustered at the parish level.

In sum, table 10.2 and figure 10.5 indicate that, ten years after children randomly assigned to the early treatment group began to receive transfers, children in this group did not have higher scores on any of a large number of tests taken in late childhood than children in the late treatment group.[22]

22. Given an attrition rate of 19 percent, it is in principle possible that the fact that we observe BDH effects in the short run (as in Paxson and Schady 2010) but not in the long run (as in table 10.2 in this chapter) could be explained by the change in the sample. Specifically, it could be that program effects are particularly large among children who attrit from the sample between 2004 and 2014. To test whether this is the case, we went back to the sample of children in Paxson and Schady (2010) and reestimated short-run program effects, limiting the sample of children to those that could be found in the 2014 survey. For their sample of 2,069 children, Paxson and Schady (2010) report a program effect of 0.052 standard deviations for the sample as a whole (with a standard error of 0.052), and 0.170 standard deviations for children in the poorest quartile (with a standard error of 0.074). When we reestimate these regressions for the smaller sample of children who could be found in 2014 (1,734 children), we find a program effect of 0.055 standard deviations for the sample as a whole (with a standard error of 0.053) and 0.199 standard deviations for children in the poorest quartile (with a standard error of

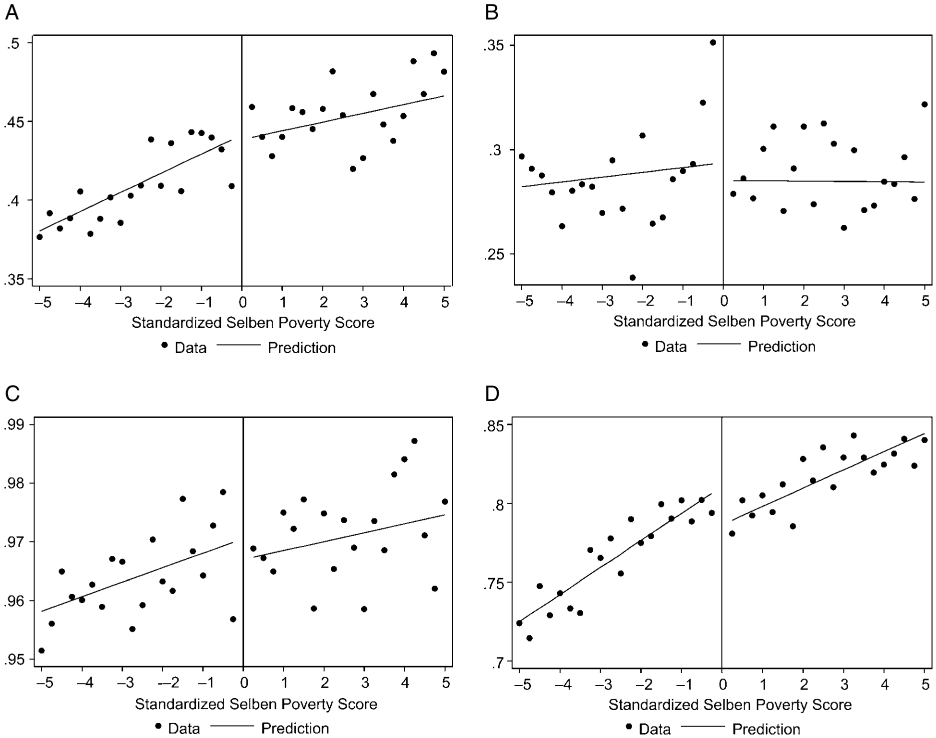

Fig. 10.6 BDH program effects, women. *A*, enrollment at any educational institution; *B*, work; *C*, elementary school completion; *D*, secondary school completion.

Note: The figure shows estimates of the effect of BDH eligibility (according to the 2000/02 poverty census) on the probability of being enrolled at any educational institution (A), working (B), having completed primary education (C), and having completed secondary education (D) in 2013/14 for women. Each panel also plots the LLR estimate, estimated separately on each side of the cutoff, with bandwidth of 5. Sample size is 34,672.

10.4.2 Results from Regression Discontinuity Analysis

We report the results from our RD estimates in figure 10.6 (for women), figure 10.7 (for men), and table 10.3. Figures 10.6 and 10.7 show no evidence of jumps at the eligibility cutoff in the probability of enrollment in an educational institution (panel A) or work (panel B), for women or men. Panels C in both figures suggest that the BDH had at most a very modest effect on the probability that young adults have completed elementary school, which is not surprising given the very high counterfactual completion rates. Finally,

0.080). These comparisons suggest that the fade-out of BDH program effects between 2004 and 2014 is unlikely to be driven by sample attrition. We thank Karen Macours for suggesting this exercise to us.

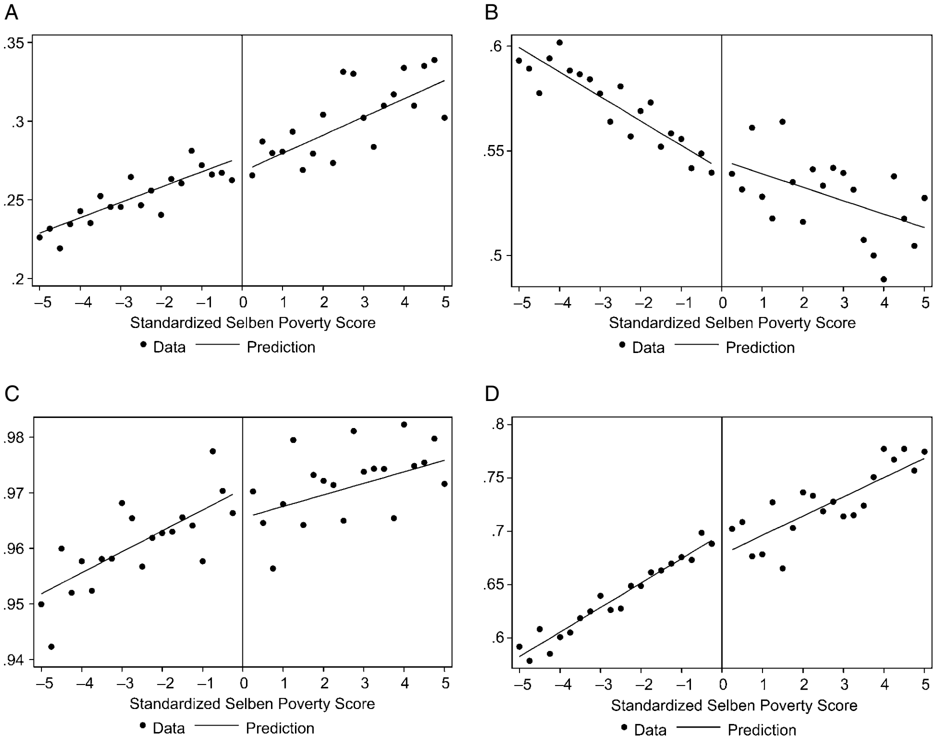

Fig. 10.7 BDH program effects, men. ***A*****, enrollment at any educational institution;** ***B*****, work;** ***C*****, elementary school completion;** ***D*****, secondary school completion.**

Note: The figure shows estimates of the effect of BDH eligibility (according to the 2000/02 poverty census) on the probability of being enrolled at any educational institution (A), working (B), having completed primary education (C), and having completed secondary education (D) in 2013/14 for men. Each panel also plots the LLR estimate, estimated separately on each side of the cutoff, with bandwidth of 5. Sample size is 53,442.

panels D suggest somewhat larger effects on the probability of completing secondary school, especially for women, where there is a jump at the cutoff in completion rates of roughly 2 percentage points.

Regression results for various samples and specifications are reported in table 10.3. The table confirms that young women in households that were eligible for transfers when they were in late childhood are 2–3 percentage points more likely to have graduated from secondary school ten years later. These results are stable across specifications. Results on secondary school completion for men are smaller in magnitude and are generally not significant. There is no evidence that young men or women who were eligible for transfers are more or less likely to be enrolled in some tertiary educational institution in 2013/14. In the case of work, some of the coefficients

Table 10.3 **Regression discontinuity estimates of BDH effects on schooling and work outcomes after ten years**

	Mean, ineligibles	(1)	(2)	(3)	(4)	(5)
Enrolled in school						
All	0.34	0.005	0.007	0.005	0.002	0.005
		(0.005)	(0.007)	(0.006)	(0.004)	(0.007)
Males	0.27	0.004	0.012	0.008	0.005	0.010
		(0.007)	(0.009)	(0.007)	(0.006)	(0.011)
Females	0.44	0.001	−0.009	−0.001	−0.002	−0.004
		(0.010)	(0.012)	(0.009)	(0.007)	(0.013)
Working						
All	0.43	−0.005	0.009	0.001	−0.006	0.004
		(0.005)	(0.008)	(0.006)	(0.005)	(0.008)
Males	0.53	−0.008	−0.009	−0.003	−0.009	−0.004
		(0.006)	(0.010)	(0.008)	(0.006)	(0.010)
Females	0.28	0.006	0.044***	0.008	−0.003	0.022*
		(0.008)	(0.012)	(0.010)	(0.008)	(0.011)
Completed elementary school						
All	0.96	0.002	0.004	0.004*	0.003	0.003
		(0.002)	(0.004)	(0.002)	(0.002)	(0.003)
Males	0.96	0.002	0.005	0.005	0.003	0.006
		(0.002)	(0.005)	(0.004)	(0.002)	(0.005)
Females	0.96	0.000	0.002	0.003	0.001	−0.001
		(0.002)	(0.005)	(0.004)	(0.002)	(0.005)
Completed secondary school						
All	0.73	0.015***	0.018**	0.016***	0.012***	0.019**
		(0.006)	(0.008)	(0.006)	(0.004)	(0.008)
Males	0.69	0.013*	0.013	0.014*	0.009	0.013
		(0.007)	(0.010)	(0.007)	(0.006)	(0.008)
Females	0.79	0.019***	0.024**	0.019**	0.017***	0.028**
		(0.007)	(0.012)	(0.009)	(0.006)	(0.013)

Note: "Mean, ineligibles" refers to the value of the outcome in question at the eligibility cutoff. Specification (1) corresponds to LLR with an optimal bandwidth; specifications (2), (3), and (4) correspond to LLRs with bandwidth = 2.5, 5, and 10, respectively; specification (5) uses the full sample and includes a quartic in the control function (the 2000/02 poverty score). All regressions include canton fixed effects. Standard errors clustered at the parish level. In the regression for "All," sample sizes are approximately 100,000 in specification (1) (with the exact number varying by outcome), 43,227 in specification (2), 88,114 in specification (3), 174,148 in specification (4), and 307,394 in specification (5). Females represent approximately 40 percent of the sample.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

for women in table 10.3 are significant, but these results are sensitive to how the control function is parametrized.

In sum, our RD results show that ten years after one group of households became eligible for transfers and another one did not, young women in transfer-eligible households had modestly higher secondary school completion rates than those in transfer-ineligible households. However, this did not translate into a higher probability of continuing on to university or some other tertiary institution. Moreover, there is no clear effect on the probability that these women work. The broad pattern of results suggests that cash transfers prevented a small fraction of women from dropping out of secondary school, but did not have a measurable effect on their subsequent education and work choices.

10.5 Conclusion

In this chapter, we use two different data sets and two identification strategies to assess the long-term (ten-year) effects of the BDH cash transfer program in Ecuador on various measures of human capital accumulation. We note that ours is one of only two evaluations that look at the effects of cash transfers after a decade. (Barham, Macours, and Maluccio [2013, 2016] look at the ten-year effects of a conditional cash transfer program in Nicaragua.)

Our experimental estimates show that children in households that received transfers earlier, and received substantially more in total transfers, did not have better learning outcomes in late childhood. Our regression discontinuity estimates show that cash transfers received in late childhood modestly increased the proportion of young women who completed secondary school but did not affect their education and work choices after graduation. It may be that larger program effects on educational attainment and achievement, or on labor market outcomes like employment rates or wages, will become apparent as individuals in the sample age. It is simply too early to tell. Nevertheless, based on the evidence that is available to date, we cautiously conclude that cash transfers will likely have only a modest effect on the intergenerational transmission of poverty in Ecuador.

Data Appendix

This data appendix provides additional details on the tests applied in the 2014 household survey. The survey included three language tests. The first test, the Test de Vocabulario en Imágenes Peabody (TVIP), is the Spanish-speaking version of the much-used Peabody Picture Vocabulary Test (PPVT) (Dunn et al. 1986). The TVIP has been used in a number of surveys in Ecuador

(Araujo, Bosch, and Schady 2017; Paxson and Schady 2007, 2010; Schady 2011) as well as in other countries in Latin America (Macours, Schady, and Vakis 2012; Schady et al. 2015). The test has been shown to be highly predictive of future outcomes, including in the United States (Case and Paxson 2008; Cunha and Heckman 2007) and in Ecuador (Schady 2012b). The other two language tests are a test of verbal comprehension, which evaluates knowledge of synonyms, antonyms, and analogies, and a test of reading comprehension, in which a child is asked to read two short texts and is then asked simple questions about their contents. These tests were drawn from the Woodcock-Johnson-Muñoz battery of achievement tests (Muñoz-Sandoval et al. 2005).

The 2014 survey included four math tests, all of which are part of the Woodcock-Johnson-Muñoz battery of achievement tests. One test, numeric series, asks the child to complete a series of numbers where one is missing; the test measures mathematical content and reasoning. A second test, math fluency, assesses the ability of children to rapidly solve basic addition, subtraction, and multiplication problems. A third test, calculations, focuses on more complex mathematical problems. The final test, applied problems, asks a child to solve a number of word problems.

In addition to the language and math tests, the survey collected data on tests of attention and working memory. Attention and working memory are two domains in what is referred to as "executive function" (EF).[23] Executive function includes a set of basic self-regulatory skills that involve various parts of the brain, but in particular the prefrontal cortex. It is an important determinant of how well young children adapt to and learn in school. Low levels of EF in childhood carry over to adulthood. A longitudinal study that followed a birth cohort in New Zealand to age thirty-two years found that low levels of self-control in early childhood are associated with lower school achievement, worse health, lower incomes, and a higher likelihood of being involved in criminal activity in adulthood, even after controlling for IQ and socioeconomic status in childhood (Moffitt et al. 2011).

Finally, the 2014 survey included a test of fluency of recovery and a test of behavioral problems. The test of fluency of recovery was drawn from the Woodcock-Johnson-Muñoz battery of cognitive tests; it measures the capacity to recover cumulative knowledge. The behavioral test is the Strengths and Difficulties Questionnaire (Goodman 1997, 2001), which is based on direct report from the children who were interviewed. The test has five scales measuring emotional symptoms, conduct problems, hyperactivity, peer relationship problems, and prosocial behavior.

All tests were extensively piloted in Ecuador, and adjustments were made so they would be appropriate for the sample of children as needed.

23. The other two domains of executive function are inhibitory control and cognitive flexibility.

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