Can cash transfers help households escape poverty traps?

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INTRODUCTION AND MOTIVATION

Cash transfers and poverty traps

- Dozens of countries in the developing world have cash transfer programs (conditional, labeled, unconditional)
- In Latin America, these programs cover millions of households and cost 0.2-0.7 GDP points
- Cash transfers have been shown to have substantial effects on short-term consumption and income poverty, and on the utilization of education and health services
- Can they also help households escape poverty traps?
  - This is largely a question about the long-term effects of cash transfers

Cash transfers and poverty traps

- Households may be **liquidity- or bandwidth-constrained**
- Transfers could have positive effects:
  1. Investments in child human capital help break inter-generational poverty trap
  2. Investments in productive assets that yield stream of income or help households cope with shocks
- Transfers could have negative effects if they discourage work

Cash transfers and poverty traps: The evidence to date

- On long-term effects of children exposed in utero and at young ages (as reviewed in Molina-Millan et al. 2016):
  - Mexico: Fernald et al. (2009) estimate PROGRESA effects on children in utero or <13 months of age when program started, 10 years later
    - Experimental effects indicate that 18 months differential exposure led to reduction in behavioral problems, but no impacts on any measure of child growth, cognition or language
    - Non-experimental estimates find higher cash transfers are significantly associated with height-for-age and higher verbal and cognitive test scores, but identification critiqued by Attanasio et al. (2010)
  - Mexico: Behrman et al. (2009) examine children aged 0-8 at the start of the PROGRESA program, 6 years later
    - Experimental results indicate that 18 months differential exposure had no significant effect on grade progression for children aged 9-11 in 2003
    - Difference-in-difference matching estimates that compare the original treatment group receiving six years of benefits to the 2003 non-experimental comparison group show positive and significant absolute effects in progression rates of about 15 percent for boys and 7 percent for girls
  - Nicaragua: Barham et al. (2013) analyze the impact of RPS for boys exposed in utero and during the first two years of life, as compared to boys exposed outside of this potentially critical 1,000-day window, 10 years later
    - Differential timing of exposure to the 3-year program resulted in cognitive outcomes that are on average 0.15 standard deviations higher for the early treatment group

Cash transfers and poverty traps: The evidence to date

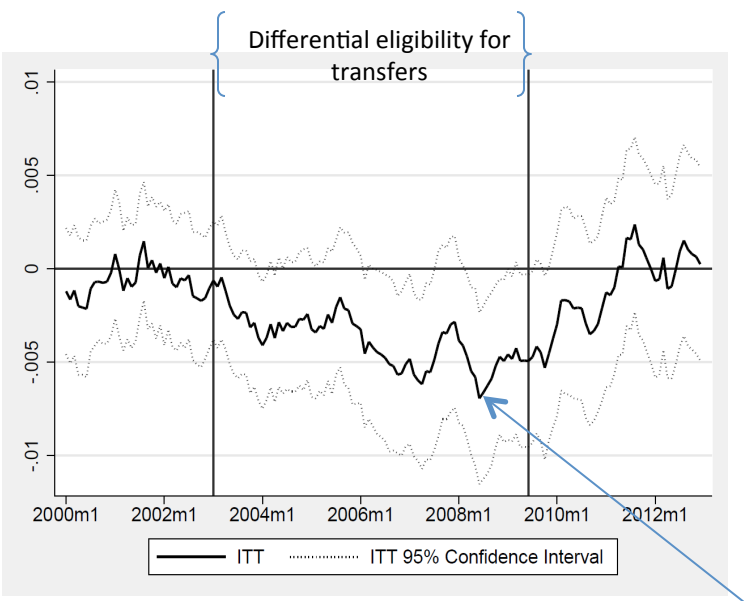
- On long-term effects of children exposed during school-age (as reviewed in Molina-Millan et al. 2016):
  - Mexico: Behrman et al. (2009, 2011) compare outcomes of children with varying degrees of exposure to PROGRESA, age 9-15 years at baseline, 6 years later
    - Comparing early versus late treatment groups, find 0.2-0.5 more grades of schooling completed but no impact on achievement tests covering reading, writing and mathematics skills
    - Non-experimental estimates suggest 0.5-1 more year of schooling completed for children with ~6 years of transfers
    - Attrition (related to migration) appears to be a serious concern
  - Nicaragua: Barham et al. (2016) analyze the effects of benefiting from RPS transfers in a period of the life cycle that is considered critical for educational investments (the age at which the probability of dropping out of school is high) versus three years later (when dropout is more likely to have already taken place), 10 years later (for boys only)
    - Impacts of one grade more schooling completed, 0.2 standard deviations higher test scores, increased off-farm migration leading to increase of 10-30 percent in monthly off-farm income
  - Colombia: Barrera-Osorio et al. (2015) estimate impact of alternative CCT treatments on enrollment in tertiary institutions, 8 years later
    - Some positive effects, although estimates somewhat imprecise
  - Cambodia: Filmer and Schady (2014) estimate impact of "scholarship" for girls, ~5 years later
    - Increased schooling completed by 0.6 grades
    - No evidence of significant effects on test scores, employment, earnings, or the probability of getting married or having a child in adolescence

Cash transfers and poverty traps: The evidence to date

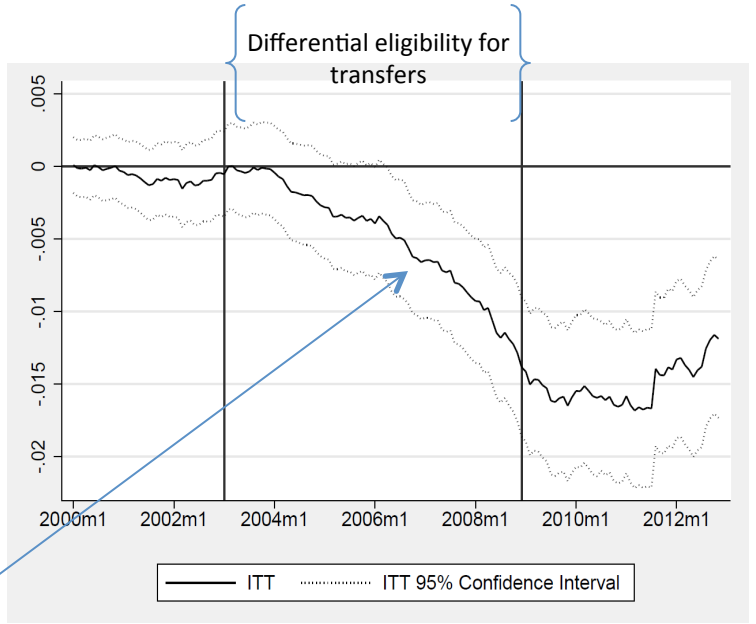
- Cash transfers may also allow households to escape poverty traps if they allow households to invest in businesses or productive durable goods
  - Mixed evidence (Gertler et al. 2012 on Mexico; Maluccio 2010 on Nicaragua)
- On the other hand, cash transfers could also ***create*** poverty traps if they disincentivize work
  - Big concern with welfare programs in developed countries
  - Recent evidence suggests that, in developing countries, households that receive transfers do not work less, but may switch from the formal to the informal sector to hide income (Banerjee et al. 2016; Araujo et al. 2016)
    - Informality measured in different ways including contributions to social security (mandatory for salaried workers) and payment of VAT and income taxes (mandatory for self-employed and firm owners)

Cash transfers and poverty traps: The evidence to date

Impact of cash transfers on Ecuador on probability of making contributions to social security



Impact of cash transfers on Ecuador on probability of making VAT and income tax payments



Differences in contribution or tax payment rates, eligible and ineligible households

Source: Araujo et al. (2016)

Summary: Existing evidence

- Mixed evidence on whether cash transfers increase human capital in the long term
  - Some studies (frequently experimental) compare children in early and late treatment households
    - Analyze whether having received transfers at a critical age improves outcomes
  - Other studies (nonexperimental) compare children in households that received or did not receive transfers over longer periods
- Mixed evidence also on whether households invest cash transfers in productive assets
- Clearer evidence on whether cash transfers affect work
  - No impact on amount of work supplied
  - Some transfer of work from the formal to the informal sectors

What we do in this paper

- We use two distinct data sets and identification strategies to look at the medium-and long-term effects of a cash transfer program in Ecuador on human capital
  - Experimental evidence (comparison of "early" and "late" treatment groups): Do children in households that received cash transfers while they were in utero or younger than 5 years of age have better schooling outcomes (enrollment rates, years of schooling completed, test scores, "strengths and difficulties") 10 years later?
  - RD evidence (comparison of just-eligible and just-ineligible households): Are children whose families received cash transfers while they were in late childhood or early adolescence more likely to be enrolled in secondary or post-secondary education 7 years later?

The *Bono de Desarrollo Humano* (BDH) program in Ecuador

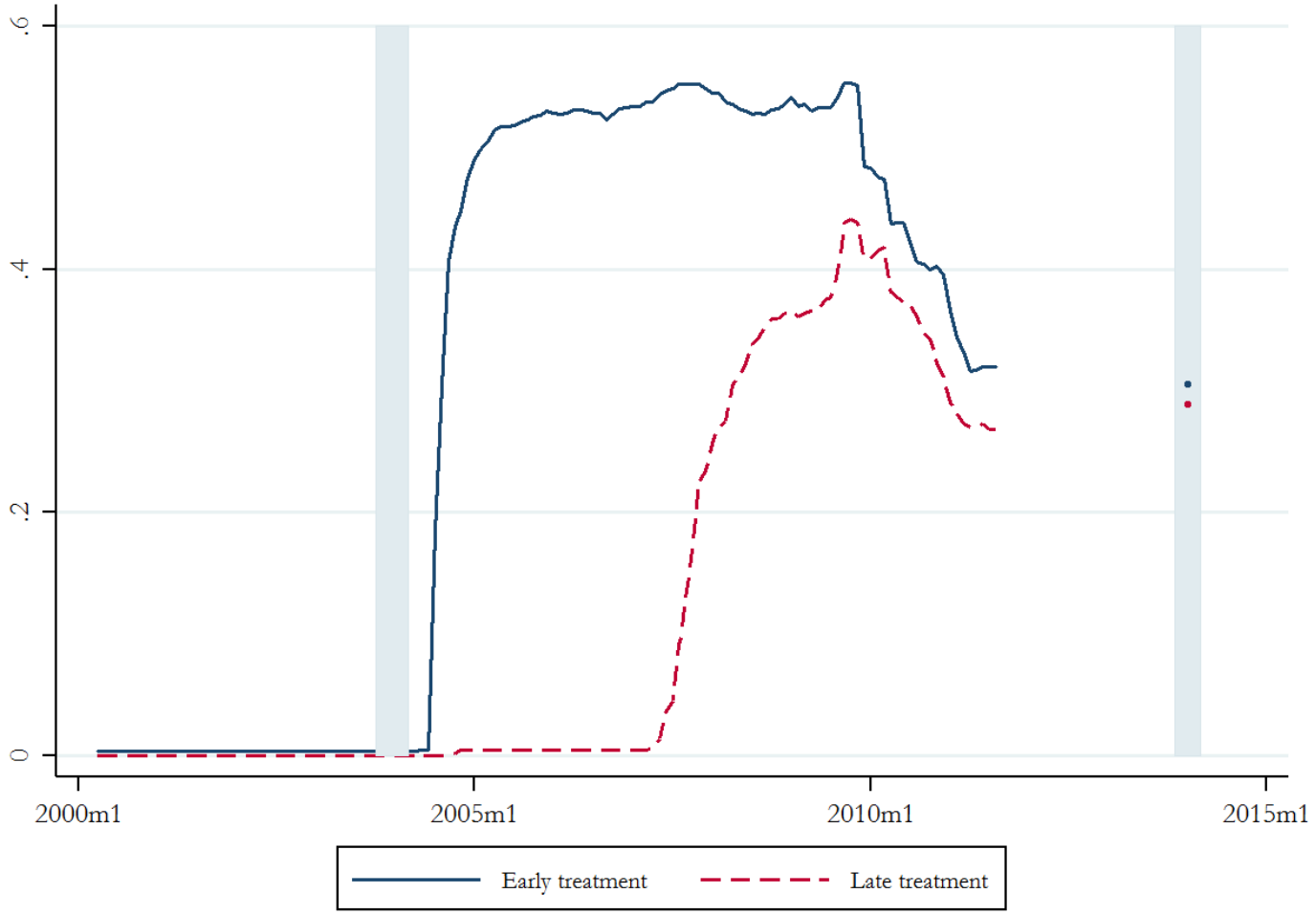
- Created in 1999, during banking crisis that led to 32 percent contraction in GDP in a single year
- During the period we analyze it was the largest cash transfer program (in proportional terms) in Latin America
  - Covered as much as 40 percent of the population, cost 0.7 percent of GDP, in period we study
    - In the US, the Earned Income Tax Credit, Food Stamps, and cash welfare (TANF) jointly cost about US $100 billion (Hoynes et al. 2016), which is 0.69 percent of GDP
  - Transfers accounted for ~15-20 percent of pre-transfer income of average recipient household
- Transfers not explicitly conditional, although BDH perhaps best understood as a "labeled" cash transfer program
  - Some evidence that, at least in early phases, confusion about whether transfers were conditional or not (Schady and Araujo 2008)

EXPERIMENTAL SAMPLE

Identification

- Randomized evaluation of the impact of cash transfers on child health and development (Paxson and Schady 2010)
- Random assignment took place at the parish level
  - 51 parishes in early treatment group: made eligible for payments in June 2004
  - 26 parishes in late treatment group: made eligible for payments in March 2007
- Baseline survey collected between October 2003 and March 2004
  - Random assignment "worked": balanced baseline characteristics of early and late treatment groups

Payments received, early and late treatment groups



Baseline Characteristics, Experimental Sample

	Early Treatment		Late Treatment		
	Mean	SD	Mean	SD	pvalue
Child and family baseline characteristics					
Child is male (proportion)	0.48	0.50	0.53	0.50	0.011
Age in months	35.64	13.34	34.97	13.05	0.433
TVIP	82.98	13.50	84.80	14.38	0.219
Mother completed primary or less (proportion)	0.35	0.48	0.32	0.47	0.666
Household size	4.81	2.12	4.79	2.06	0.942
Number of household assets	3.88	2.41	4.05	2.65	0.656
Log (Predicted per capita expenditure)	3.35	0.22	3.38	0.26	0.258
N	1157		550		

Estimation and outcomes

- Paxson and Schady (2010) use 2005 survey to estimate that, after ~18 months receiving transfers, children randomly assigned to early treatment did not have better outcomes than those assigned to control, on average
- However, significant program effects on child development among households in poorest quartile
  - 0.18 SDs on cognitive and behavioral measures
  - 0.16 SDs on physical measures
- We use a survey carried out in 2014 to see whether these effects are sustained 10 years after the early treatment group began to receive transfers
- Outcomes include whether child is enrolled in school, years of schooling completed, test scores

Language Tests
TVIP Spanish version of the Peabody Picture Vocabulary Test that measures receptive language
Verbal comprehension Based on the subscale with the same name from the Woodcock Muñoz cognitive battery III (Tests 1A, 1B, 1C), evaluates knowledge of synonyms, antonyms and analogies
Reading comprehension The child is offered two short texts to read. It is not necessary that they are read out loud. After reading each of the texts, the child is asked five questions about their contents.
Math Tests
Numeric series Based on the subscale with the same name from the Woodcock Muñoz achievement battery III (Quantitative concepts, Test 18B), the child is asked to complete a series of numbers where one of them is missing. It measures knowledge of mathematical concepts and reasoning.
Math fluency Based on the subscale with the same name from the Woodcock Muñoz achievement battery III (Test 6), it assesses the ability to rapidly solve basic addition, subtraction and multiplication). Children are given a list of computations and three minutes to solve as many of them as they can.
Calculations Based on the subscale with the same name from the Woodcock Muñoz achievement battery III (Test 9), it assesses the ability to solve addition, subtraction multiplication, division, and other more complex mathematical and geometric calculations.
Applied problems Based on the subscale with the same name from the Woodcock Muñoz achievement battery III (Test 10), it asks the child to analyze and solve increasingly difficult mathematical problems.

Other Tests
Pair cancellation Based on the subscale with the same name from the Woodcock Muñoz cognitive battery III (Test 20), it is a measure of executive processing, attention and concentration.
Digit span The child has to repeat short sequences of digits, first in the same order then in the reverse one. It measures working memory.
Fluency of recovery Based on the subscale with the same name from the Woodcock Muñoz cognitive battery III (Test 12), it measures the capacity to recover cumulative knowledge.
Strengths and difficulties questionnaire All five scales were applied: emotional symptoms, conduct problems, hyperactivity and inattention, peer relationship problems, and prosocial behavior

Estimation and outcomes

- Estimate $Y_{ihpt+1} = \alpha_c + Z_{ihp}\beta_1 + \mathbf{X}_{ihpt}\beta_2 + \varepsilon_{ihpt+1}$
- Z is a dummy variable that indicates whether child in household in early or late treatment group
- Also do separately by gender, age, and predicted per capita expenditures at baseline
- Attrition:
  - 14.0 percent of children at baseline not found 10 years later
  - Attrition uncorrelated with treatment status

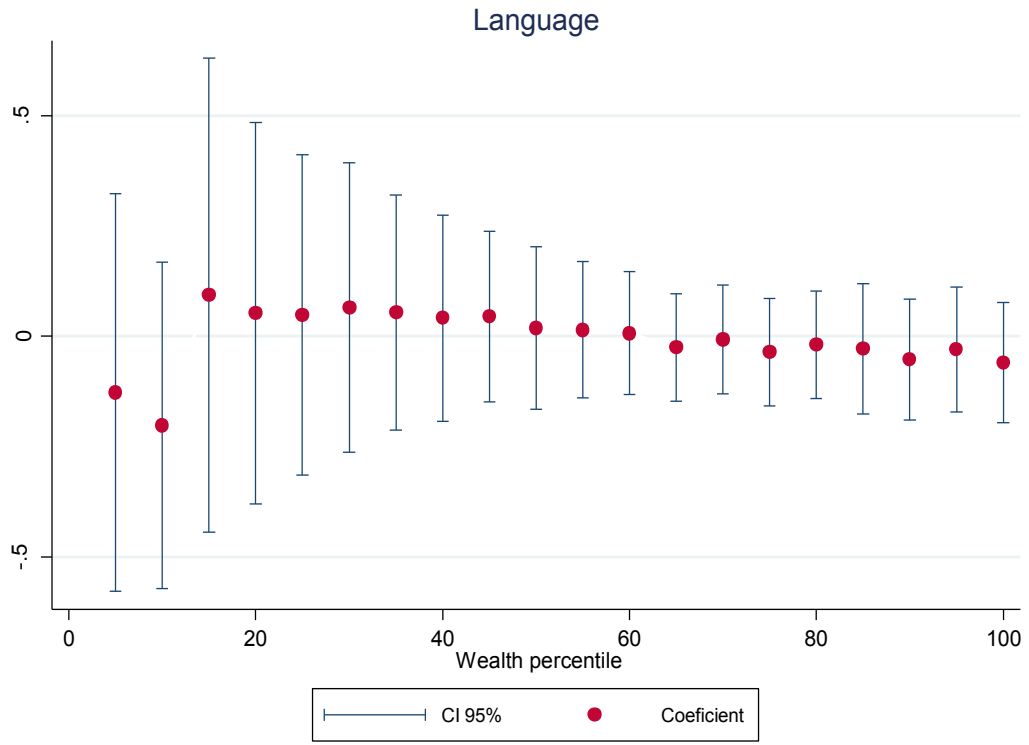
Main results

The Impact of Cash Transfers in Early Childhood on Schooling Outcomes in Late Childhood, Experimental Sample

	All n=1707	**Young** n=612	**Old** n=1095	**Girls** n=858	**Boys** n=849
Currently enrolled	0.008	0.008	0.010	-0.006	0.023*
	(0.012)	(0.008)	(0.019)	(0.014)	(0.013)
	[0.95]	[0.97]	[0.94]	[0.96]	[0.95]
Highest grade completed	0.027	-0.026	0.038	-0.137	0.205
	(0.097)	(0.098)	(0.140)	(0.130)	(0.139)
	[8.0]	[7.3]	[8.4]	[8.2]	[7.9]
Language	-0.060	-0.170*	-0.001	-0.009	-0.094
	(0.068)	(0.088)	(0.084)	(0.061)	(0.102)
Math	-0.090	-0.039	-0.125	-0.110	-0.052
	(0.094)	(0.087)	(0.119)	(0.093)	(0.118)
Other	-0.023	-0.017	-0.022	0.014	-0.041
	(0.064)	(0.065)	(0.089)	(0.082)	(0.079)

Note: Coefficients, standard errors (in parentheses), and means for the late treatment group [in square brackets]. "Young" ("Old") refers to children who were in utero or younger than 35 months of age (36-71 months of age) at the time households randomly assigned to the early treatment group first became eligible for transfers. All regressions include controls for gender, age in months, a dummy variable that takes on the value of one if the child's mother had more than completed primary education, number of household members, the number of household assets (all controls at baseline), and canton fixed effects. Standard errors correct for clustering at parish level. *Significant at 10% level.

Heterogeneity of impacts, by baseline (predicted) consumption



Note: ventiles are cumulative. For example, first regression (leftmost coefficient and confidence interval) includes only 5 percent of sample, next regression includes 10 percent of sample, final regression (corresponding to point marked "100" on x-axis) includes full sample

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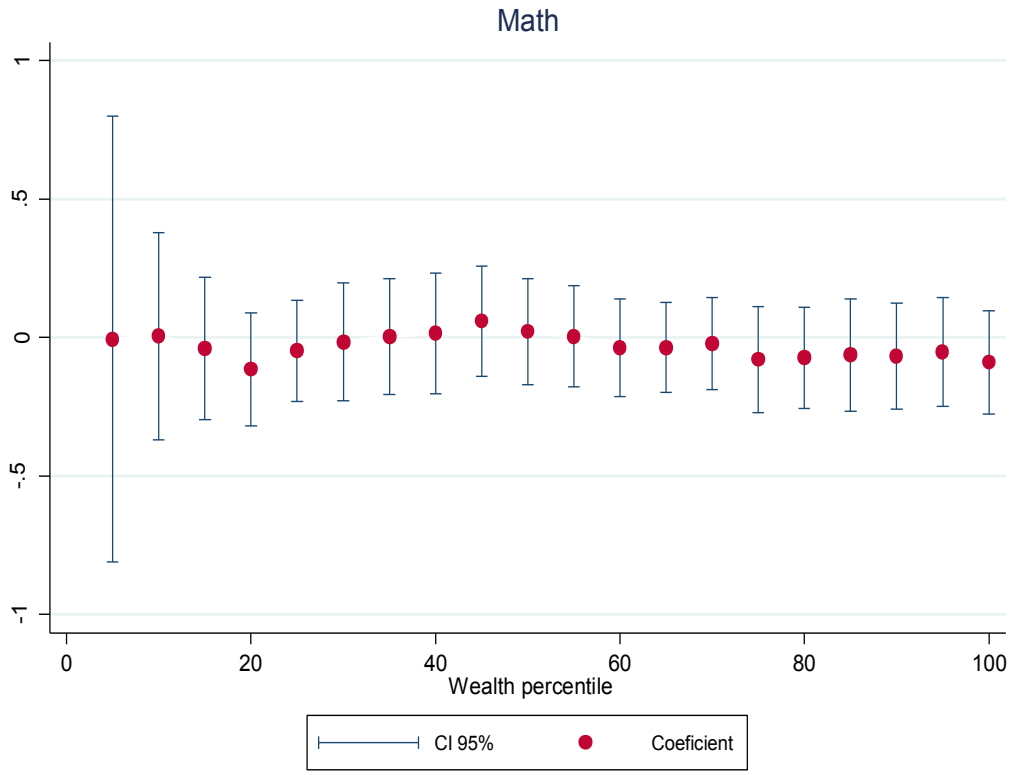

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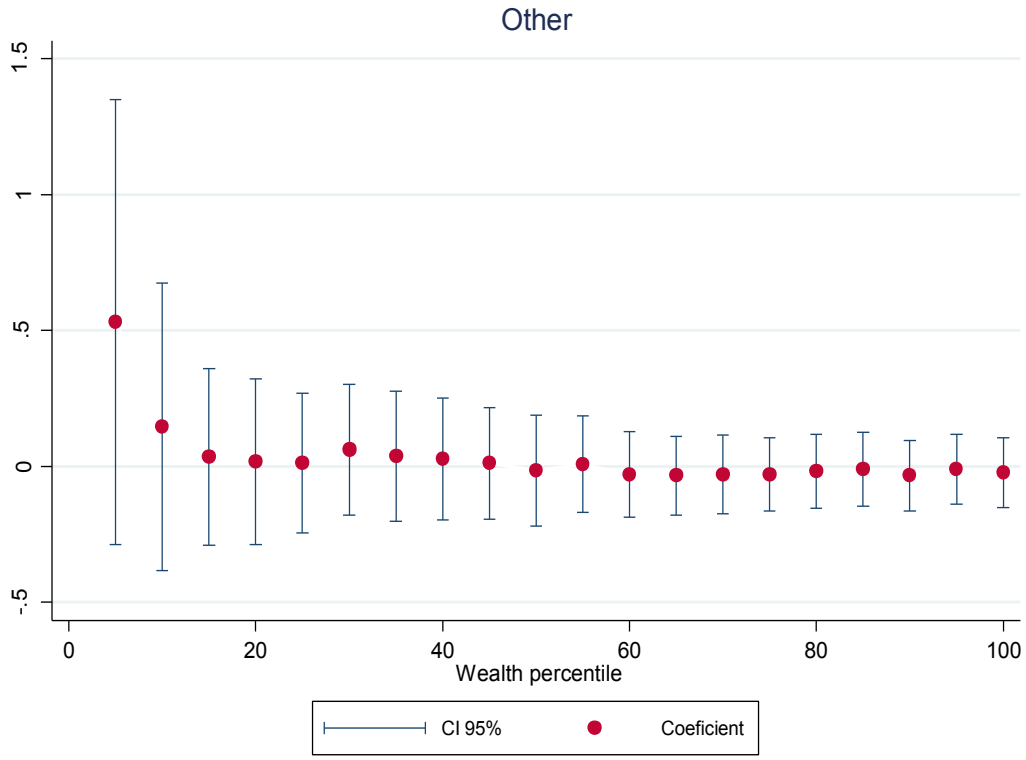


Note: ventiles are cumulative. For example, first regression (leftmost coefficient and confidence interval) includes only 5 percent of sample, next regression includes 10 percent of sample, final regression (corresponding to point marked "100" on x-axis) includes full sample

Summary of results: experimental sample

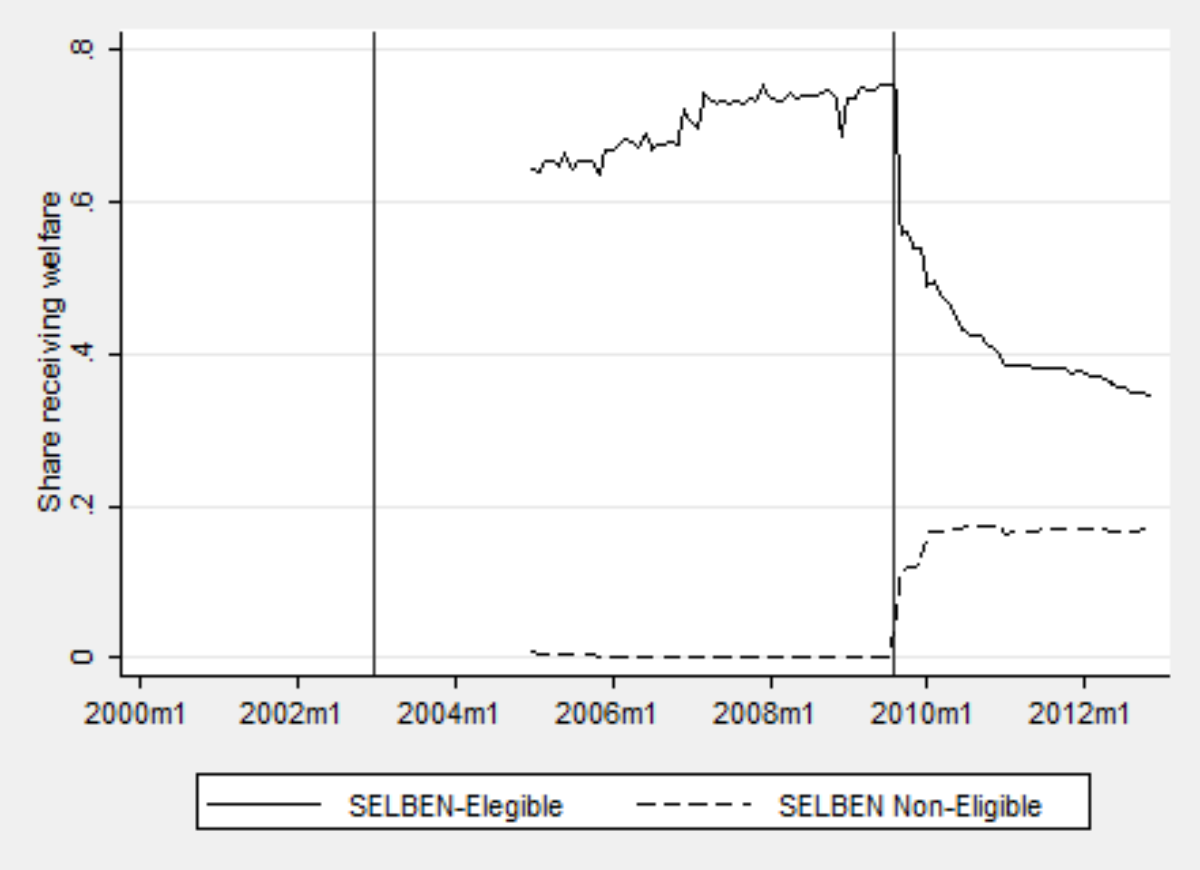
- No impacts on enrollment
  - Not surprising given high enrollment rates at this age
- No impacts on years of schooling completed
  - Not surprising given low repetition rates
- No impacts on test scores in math (number series, calculations, word problems), language (vocabulary, verbal comprehension, reading comprehension) and tests measuring attention, working memory, fluency of recovery, and "strengths and difficulties"—for the sample as a whole, or for children in households that were poorest at baseline
- Effects estimated in the short term (Paxson and Schady 2010) have fully faded out 8 years later
- Will they reappear in adulthood? See evaluations of Perry Preschool Program (Heckman et al. 2010) and Project STAR (Chetty et al. 2011)

RD SAMPLE

Identification

- BDH uses "poverty census" to determine eligibility
  - 2000/02 poverty census determined eligibility for transfers for 2003/09 period
    - Includes questions on whether children enrolled in school—serves as baseline
  - 2007/08 poverty census determined eligibility for transfers for 2009/14 period
    - Includes questions on whether children enrolled in school—serves as follow-up

Payments received, eligibles and ineligibles



Estimation and outcomes

- Standard RD setup

$$Y_{ihc} = \alpha_c + S_{ih}\beta_1 + I(S_{ih}<C)\beta_2 + I(S_{ih}<C)* S_{ih}\beta_3 + \mathbf{X}_{ihc}\beta_4 + \varepsilon_{ihc}$$

- S is the poverty score and C is the eligibility cutoff
- Can also instrument treatment (using administrative data on who received payments) with eligibility
- Two separate regressions
  - Children age 8-12 and enrolled in elementary school at baseline (facing transition from elementary to secondary school)
  - Children age 15-18 and enrolled in secondary school at baseline (facing transition from secondary to post-secondary education)

Estimation and outcomes

- Consider different parametrizations of the control function (bandwidth, linear or polynomial) for robustness
- No jump in observables at cutoff
- No jump in density at cutoff (McCrary density test)

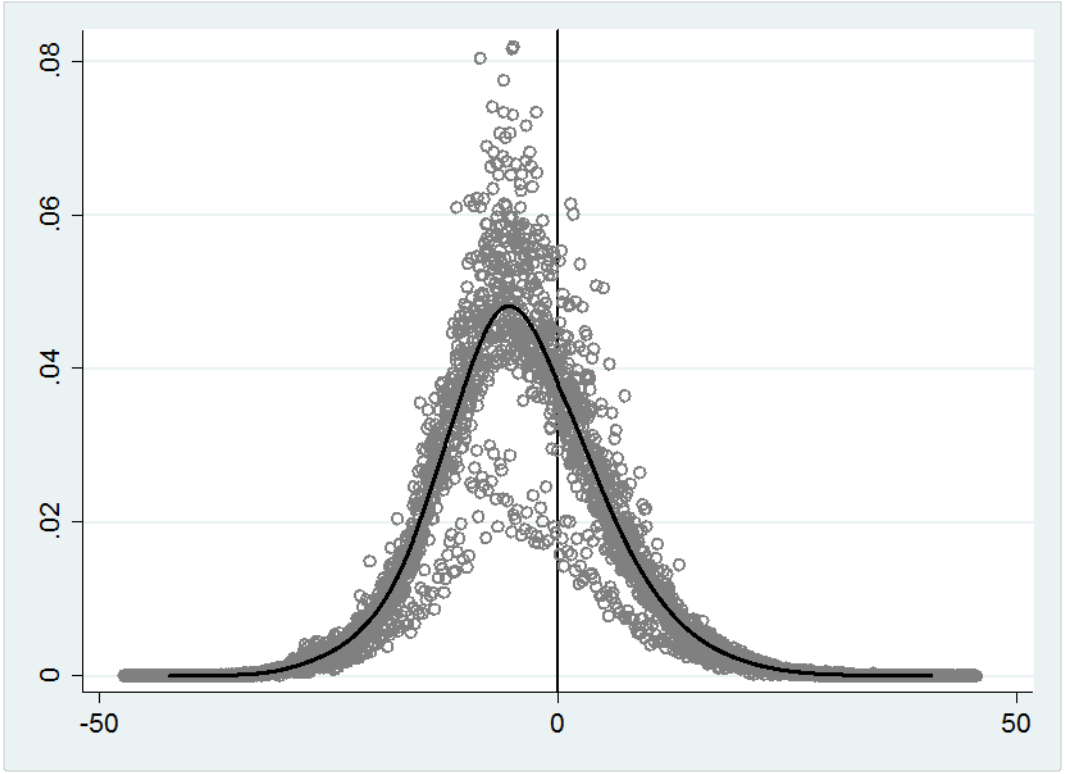
Baseline Characteristics, RDD Sample

	Children 8-12			Children 15-18		
	Control	Treatment	RDD	Control	Treatment	RDD
Household level						
Urban	0.86	0.71	0.012**	0.88	0.77	0.004
	0.35	0.45	(0.005)	0.33	0.42	(0.006)
Lives in a house	0.87	0.69	-0.018	0.90	0.74	0.014
	0.33	0.46	(0.02)	0.29	0.44	(0.010)
Has untreated floors	0.16	0.58	0.002	0.16	0.48	0.004
	0.37	0.49	(0.008)	0.37	0.50	(0.009)
Has toilet indoors	0.76	0.25	-0.014	0.75	0.33	-0.006
	0.43	0.43	(0.018)	0.43	0.47	(0.012)
Has shower indoors	0.51	0.07	0.003	0.50	0.10	-0.001
	0.50	0.26	(0.008)	0.50	0.29	(0.013)
Has gas kitchen	0.99	0.84	0.005	0.99	0.90	0.007***
	0.07	0.36	(0.004)	0.08	0.30	(0.003)
Has electricity	1.00	0.90	0.002*	1.00	0.95	0.001
	0.03	0.30	(0.001)	0.04	0.22	(0.001)
Owns lands	0.15	0.19	-0.005	0.15	0.15	0.005
	0.36	0.39	(0.006)	0.36	0.36	(0.007)
Number of rooms	2.95	1.95	0.027	3.21	2.17	0.054*
	1.25	1.03	(0.020)	1.30	1.13	(0.029)
Individual Level						
Share of males	0.56	0.56	0.002	0.53	0.50	-0.017
	0.50	0.50	(0.002)	0.50	0.50	(0.012)
Age	9.92	9.65	0.000	16.41	16.01	0.028
	1.65	1.69	(0.008)	1.44	1.50	(0.040)
Works	0.00	0.00	-0.017	0.03	0.05	0.004
	0.03	0.05	(0.030)	0.17	0.23	(0.003)

Note: The columns for "control" and "treatment" report means and standard deviations for each variable. The column for "RDD" reports the coefficient on just-eligible households from a Local Linear Regression with optimal bandwidth (chosen by the method proposed by Imbens and Kalyanaraman 2012) of characteristic on poverty score, cutoff, and interaction between poverty score and cutoff. Standard errors correct for clustering at the canton level. *significant at the 10 percent level, ** at the 5 percent level, *** at the 1 percent level

McCrary density test

Coefficient: 0.008
Standard error: 0.009



The Impact of Cash Transfers in Childhood on School Enrollment in Adolescence and Early Adulthood, Regression Discontinuity Sample

		Intent-to-treat					IV
	Mean, ineligibles	LLR, Optimal bandwidth	LLR, Bandwidth = 5	LLR, Bandwidth = 10	LLR, Bandwidth = 15	Fifth degree polynomial	LLR, Bandwidth = 5
Attending elementary at baseline, 8-12 years old							
All	0.79	0.006	0.014**	0.012**	0.009**	0.019***	0.020**
		(0.005)	(0.007)	(0.005)	(0.004)	(0.007)	(0.009)
Girls	0.79	0.015**	0.014	0.012*	0.009	0.027***	0.019
		(0.007)	(0.010)	(0.007)	(0.006)	(0.009)	(0.015)
Boys	0.79	0.010	0.015	0.012	0.008	0.016	0.022
		(0.007)	(0.009)	(0.007)	(0.006)	(0.012)	(0.018)
Attending secondary at baseline, 15-18 years old							
All	0.20	0.024***	0.031***	0.023***	0.011	0.033**	0.046***
		(0.009)	(0.011)	(0.008)	(0.007)	(0.015)	(0.015)
Girls	0.16	0.020	0.019	0.023*	0.006	0.024	0.026
		(0.014)	(0.016)	(0.013)	(0.011)	(0.023)	(0.021)
Boys	0.25	0.028**	0.039**	0.020*	0.014	0.037*	0.054**
		(0.014)	(0.016)	(0.011)	(0.009)	(0.021)	(0.025)

Note: "Mean, ineligibles" refers to the value of the RD regression for ineligibles at the cutoff. Intent-to-treat columns report coefficients and standard errors from RD regressions of enrollment on transfer eligibility, in IV regressions a dummy variable for whether households received transfers is instrumented with eligibility. All specifications include canton fixed effects and the following controls:. Standard errors clustered at parish level. ***, **, and *, significant at 1 percent, 5 percent, and 10 percent, respectively.

Summary of results: RD sample

- After 6 years in which one group of households received transfers and the other did not:
  - Transfers increase probability that a child age 8-12 enrolled in elementary school is still enrolled by ~1 percentage point (ITT) to ~2 percentage points (IV), from a counterfactual of 79 percent
  - Transfers increase probability that a child age 15-18 enrolled in secondary school is still enrolled by ~2-3 percentage points (ITT) to ~4-5 percentage points (IV), from a counterfactual of 20 percent

Conclusions

- We study the “long-term” effects of an unconditional (possibly “labeled”) cash transfer program in Ecuador
- We use two distinct identification strategies and two data sets
- We find weak evidence that cash transfers in Ecuador helped households escape inter-generational poverty traps

Conclusions

- For young children (in utero or <5 years of age at time when treatment began):
  - Random assignment: Comparison is between children in households that received transfers early and those that received them ~2 years later
    - Short-term effects on child physical and cognitive development (Paxson and Schady 2010)
    - No long-term effects on enrollment, grade attainment, or test scores
    - Conceivably, effects could appear again in adulthood (Project STAR, Perry Preschool Program)

Conclusions

- For older (school-aged) children:
  - RDD: Comparison is between children in just-eligible households, who received transfers for ~7 years, and just-ineligible children
    - Confirm impacts on school enrollment (as in Schady and Araujo 2008), modest for younger children (1-2 percentage points), somewhat larger for older children (3-5 percentage points)
    - Too early to assess effects on labor market outcomes, but these are likely to be modest