“Assessing the Long-term Effects of Conditional Cash Transfers on Human Capital: Evidence from Colombia”

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Washington DC
April 4, 2011
Motivation

• Conditional Cash Transfers (CCTs) have been on the rise in recent years (more than 32 programs worldwide)

• All 11 and 15 CCTs evaluated against school enrollment and attendance, respectively, have positive effects (IEG-WB, 2011)

• The underlying assumption is that more school participation is expected to lead to higher educational attainment

• Yet, very little is known about the long-term effects of CCTs on the stock of human capital (i.e. educational attainment and learning in early adulthood)
This paper

- Looks at the expected but empirically uncertain link between higher school participation due to CCTs and educational achievement

- Investigates whether various cohorts of children from poor households that benefited from the Colombian CCT *Familias en Acción* (FA) for up to nine years:
  1. attain more years of schooling
  2. perform better in academic tests at the end of high school
Preview of findings

• The program helps participant children to increase their school attainment by making them more likely to complete high school (4-8 percentage points)

• Evidence of heterogeneity in program impacts: effects on school completion are larger for girls and beneficiaries in rural areas

• Program participants who graduate from high school perform in academic tests at the same level as equally poor non-recipient graduates
Existing literature

- **Positive effects on enrollment and attendance.** Substantial evidence from impact evaluations of programs in Brazil, Cambodia, Colombia, El Salvador, Honduras, Jamaica, Malawi, Mexico, Nicaragua, Pakistan, and Turkey (Fiszbein and Schady, 2010; IEG-WB, 2011)

- A subset of these evaluations studies tracked school progression and show **positive effects on grade transition, repetition, and dropout rates**
Existing literature (cont.)

- Evidence about impacts on final (or close to final) outcomes in education much more limited
  

  - **Learning outcomes**: (1) Improvements in cognitive development found only for children in pre-school and primary education; (2) no effects for older children – even for analyses that do not condition on school attendance (Behrman et al 2000, Behrman et al 2005 and Behrman and Parker 2008 for Mexico; Filmer and Schady, 2009 for Cambodia)
The educational context in Colombia

• **Average school attainment** (WB 2005, 2008)
  – **1992**: 6.4 years
  – **2003**: 7.6 years

• **School progression** (Sanchez et al 2010). For each 100 students that start primary school:
  – 60.7% reach grade 5
  – 48.9% reach grade 9
  – **34.8% reach grade 11** (last year of high school)
The program: *Familias en Acción* (FA)

- **Started in 2001** in response to a severe economic crisis
- **Treatment**: Monthly stipends of US$8-16 + nutritional package
- **Conditionality**: 80% school attendance, regular participation in nutrition workshops
- **Treatment group defined by**:
  - **Proxy-means test (Sisben)**: 20% poorest population + internally displaced + indigenous groups
  - **Ages**: school stipends (children 7-18 years old); nutritional stipends (children 0-6 years old)
- **Program coverage/cost**: 2.8 million families, 2010 (participation rate of 64%); 0.27% of GDP in 2009
Existing evidence about FA

- Early evaluations of FA show positive effects on short-term: (1) consumption (13-15%), (2) school enrollment (2% in primary, 5-7% in secondary), (3) child labor, (4) morbidity, immunization and anthropometrics [Attanasio et al., 2005, 2006 and 2009; Attanasio and Mesnard, 2005; IFS et al 2006]

- Recent analysis on learning in primary and early secondary do not find conclusive evidence (Garcia and Hill, 2010) – but data used are problematic

- There is no robust evidence on the effects of FA on school attainment and learning
Data used in this paper

Four different sources of data:

1. **Baseline (2002) of panel household survey** (stratified random sample of eligible families in both treatment municipalities and matched control municipalities)

2. **A census of the poor** collected between 1994 and 2003 for the proxy-means test system (*Sisben*)

3. **Administrative records from the M&E system of FA** – a rich longitudinal census of all program beneficiaries

4. **Administrative records on registration and results for *Icfes*** -- a standardized national test that is administered prior to graduation from high school
Outcomes of interest

• Two measures of medium/long-term human capital for children with program exposure of up to nine years:
  
  – **High school completion** -- proxied by registration for the *Icfes* test during the period 2003-2009
  
  – **Academic achievement** – measured by the actual performance of the students on the *Icfes* standardized test in Mathematics, Language and the overall score
Possible channels between CCTs and final outcomes in education

- Net effect on children’s school completion is ambiguous:

  (+) income effect from the transfer (assuming schooling is a normal good)

  (+) substitution effect for children out of school or with low attendance induced by the conditionality

  (+/-) effects due to additional pressure on existing education supply (e.g. school congestion) or displacement effects

  (-) effects due to perverse incentives to delay children’s graduation from school to keep eligibility to the transfers
Possible channels between CCTs and final outcomes in education (cont.)

• There is also ambiguity about the net effect on children’s learning outcomes:

  (+) effects of higher school attendance and more time devoted to school work

  (+) effects through increased investments in learning enhancing inputs (books, more nutritious food, parental time, and reduction in child work)

  (+/-) effects due to additional pressure on existing supply (e.g. school congestion) and peer effects

  (-) CCTs often targeted to the neediest areas where school quality may be low
Empirical approach: PSM

Main identification issue: (1) program not randomly allocated (welfare and geographic targeting), (2) voluntary participation

Strategy 1: Propensity score matching (PSM)

- Match children surveyed at baseline that could have finished high school between 2003-2009
- Location, age and grade at baseline determine treatment status and length of exposure
- Main caveat: lack of baseline measures of outcome variables + identifying assumption of PSM
- Three different participation models to estimate p-scores (differences in distribution of covariates at baseline)
Data merging for PSM

Icfes tests for students in grade 11 (T&C)

First round of the panel survey (T&C)

Second round (T&C)

Third round (T&C)
Empirical approach: RDD

**Strategy 2: Regression Discontinuity Design (RDD)**

- Exploits variation in assignment to treatment arising from the discontinuous rule that determines eligibility
- Identifying assumption: households just below and above threshold are statistically comparable except for their participation in the program
- Any discontinuity on the conditional distribution of the outcome variables at the cutoff is interpreted as the effect of the program
Data merging for RDD

First round of the panel survey (T & C)

Icfes tests for students in grade 11 (T & C)

Administrative systems of FA (census of participants, only T)

Sisben I (T & C) [94-03]

Sisben II (T & C) [03-07]
RDD’s first stage

- *Sisben* predicts substantial but not perfect changes in the probability of treatment $\rightarrow$ fuzzy RDD
## RDD’s first stage

### Panel A: Same functional form above and below threshold

<table>
<thead>
<tr>
<th>Eligibility</th>
<th>0.726**</th>
<th>0.719***</th>
<th>0.714**</th>
<th>0.715***</th>
<th>0.693**</th>
<th>0.705***</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>[0.002]</td>
<td>[0.001]</td>
<td>[0.002]</td>
<td>[0.001]</td>
<td>[0.002]</td>
<td>[0.002]</td>
</tr>
<tr>
<td>Observations</td>
<td>583,037</td>
<td>583,037</td>
<td>583,037</td>
<td>583,037</td>
<td>583,037</td>
<td>583,037</td>
</tr>
<tr>
<td>R2</td>
<td>0.67</td>
<td>0.73</td>
<td>0.67</td>
<td>0.73</td>
<td>0.67</td>
<td>0.73</td>
</tr>
</tbody>
</table>

### Panel B: Different functional form above and below threshold

<table>
<thead>
<tr>
<th>Eligibility</th>
<th>0.693**</th>
<th>0.703**</th>
<th>0.695**</th>
<th>0.699**</th>
<th>0.685**</th>
<th>0.692**</th>
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<tr>
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<td>[0.002]</td>
<td>[0.002]</td>
<td>[0.002]</td>
<td>[0.002]</td>
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<td>583,037</td>
<td>583,037</td>
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<td>583,037</td>
</tr>
<tr>
<td>R2</td>
<td>0.67</td>
<td>0.73</td>
<td>0.67</td>
<td>0.73</td>
<td>0.67</td>
<td>0.73</td>
</tr>
</tbody>
</table>

- Quadratic: Yes
- Cubic: Yes
- Quartic: Yes
- Municipality fixed effects: Yes

**Note:** Heteroskedasticity-robust standard errors clustered at the municipality level reported in square bracket. Models include linear \([Si - S^*]\), quadratic \([Si - S^*]^2\), cubic \([Si - S^*]^3\), and quartic \([Si - S^*]^4\) specifications of the control function below and above the cutoff of eligibility \(S^*\).
Impacts on school completion

- **OLS and PSM show positive impacts**: treated children are between 4 and 8.5 percentage points more likely to finish high school (ITT impacts are very similar across models)

<table>
<thead>
<tr>
<th>Dependent Variable: School Completion</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
</tr>
<tr>
<td>OLS</td>
</tr>
<tr>
<td>(1) 0.030* [0.017]</td>
</tr>
<tr>
<td>(2) 0.050*** [0.018]</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Matching</td>
</tr>
<tr>
<td>(1) 0.0401** [0.0187]</td>
</tr>
<tr>
<td>(2) 0.0840** [0.0220]</td>
</tr>
<tr>
<td>(3) 0.0696** [0.0214]</td>
</tr>
</tbody>
</table>

Observations: 3,452 3,452 3,476 3,476 3,888

*Notes: Mean high school completion rate of the control group for the period 2003-2009 is 0.501. Units of analysis are matched on the propensity score from three different specifications of a logistic regression on participation in the program. OLS coefficients in models 1 and 2 are estimated from specifications without any covariates and with all the control variables included in Model 3 of the matching analysis, respectively.*
Impacts on school completion (cont.)

- RDD also shows positive impacts both graphically ...
**Impacts on school completion (cont.)**

- ... and econometrically using different RD polynomial regressions (also using non-parametric methods)
- RDD effects around 3.0-6.5 percentage points

<table>
<thead>
<tr>
<th>Predicted FEA</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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</thead>
<tbody>
<tr>
<td><strong>0.015</strong>*</td>
<td>0.016***</td>
<td>0.046***</td>
<td>0.037***</td>
<td>0.071***</td>
<td>0.053***</td>
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<tr>
<td>[0.004]</td>
<td>[0.003]</td>
<td>[0.004]</td>
<td>[0.004]</td>
<td>[0.005]</td>
<td>[0.005]</td>
<td></td>
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<tr>
<td>Quadratic</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cubic</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cuartic</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Municipality fixed effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Notes:** Heteroskedasticity-robust standard errors clustered at the municipality level reported in square bracket. Models include linear [(Si - S*)], quadratic [(Si - S*)2], cubic [(Si - S*)3], and quartic [(Si - S*)4] specifications of the control function below and above the cutoff of eligibility S*. **
Impacts on learning

- OLS and PSM suggest there are no impacts on Mathematics, Spanish, or the overall score in the test

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>OLS</th>
<th>Matching estimates</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(1)</td>
</tr>
<tr>
<td>Mathematics</td>
<td>-0.401</td>
<td>-0.593</td>
<td>-0.0189</td>
</tr>
<tr>
<td></td>
<td>[0.392]</td>
<td>[0.413]</td>
<td>[0.0474]</td>
</tr>
<tr>
<td>Observations</td>
<td>1,867</td>
<td>1,867</td>
<td>1,867</td>
</tr>
<tr>
<td>Spanish</td>
<td>0.398</td>
<td>0.079</td>
<td>0.0502</td>
</tr>
<tr>
<td></td>
<td>[0.350]</td>
<td>[0.368]</td>
<td>[0.0465]</td>
</tr>
<tr>
<td>Observations</td>
<td>1,867</td>
<td>1,867</td>
<td>1,867</td>
</tr>
<tr>
<td>Overall Test scores</td>
<td>0.179</td>
<td>-0.086</td>
<td>0.0292</td>
</tr>
<tr>
<td></td>
<td>[0.226]</td>
<td>[0.237]</td>
<td>[0.0327]</td>
</tr>
<tr>
<td>Observations</td>
<td>1,867</td>
<td>1,867</td>
<td>1,867</td>
</tr>
</tbody>
</table>

Notes: Test scores are normalized by the mean and the standard deviation in each subject by year. The definition of the overall test score excludes results of the foreign language test chosen by the student. Bootstrapped standard errors reported in square brackets are obtained from 200 replications. Units of analysis are matched on the propensity score from three different specifications of a logistic regression on participation in the program.
Impacts on learning (cont.)

- Overall, RDD also shows there are **no impacts on learning** (except for some negative effects on Spanish)

<table>
<thead>
<tr>
<th>Outcome: Icfes test scores</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mathematics</td>
<td>-0.009</td>
<td>0.002</td>
<td>-0.010</td>
<td>-0.007</td>
<td>0.011</td>
<td>0.023</td>
</tr>
<tr>
<td></td>
<td>[0.020]</td>
<td>[0.021]</td>
<td>[0.025]</td>
<td>[0.025]</td>
<td>[0.030]</td>
<td>[0.031]</td>
</tr>
<tr>
<td>Spanish</td>
<td>-0.036*</td>
<td>-0.035*</td>
<td>-0.025</td>
<td>-0.042*</td>
<td>-0.024</td>
<td>-0.044</td>
</tr>
<tr>
<td></td>
<td>[0.020]</td>
<td>[0.020]</td>
<td>[0.025]</td>
<td>[0.025]</td>
<td>[0.030]</td>
<td>[0.030]</td>
</tr>
<tr>
<td>Overall test score</td>
<td>-0.013</td>
<td>-0.016</td>
<td>-0.013</td>
<td>-0.029</td>
<td>-0.003</td>
<td>-0.023</td>
</tr>
<tr>
<td></td>
<td>[0.015]</td>
<td>[0.015]</td>
<td>[0.018]</td>
<td>[0.018]</td>
<td>[0.022]</td>
<td>[0.022]</td>
</tr>
</tbody>
</table>

| Quadratic                   | Yes     | Yes     |
| Cubic                       |         |         |
| Quartic                     |         |         |
| School fixed effects        | Yes     | Yes     |

**Notes**: Test scores are normalized by the mean and the standard deviation in each subject by year. The definition of the overall test score excludes results of the foreign language test chosen by the student. Heteroskedasticity-robust standard errors clustered at the municipality level reported in square bracket. Models include linear [(Si - S*)], quadratic [(Si - S*)²], cubic [(Si - S*)³], and quartic [(Si - S*)⁴] specifications of the control function below and above the cutoff of eligibility S*. 
But there is an additional problem of selection

• *lcfes* test given to children in school ... but FA influences the enrollment and attendance of children at the margin, introducing low-scorers into the treatment group

• We performed bounding procedures to symmetrically truncate the distribution of the treatment and control groups at percentile $\phi_0$ (Lee, 2002 and Angrist et al 2004):

$$\Delta_{AAT} = E(Y_i \mid D_i = 1, Y > q_0(\theta)) - E(Y_i \mid D = 0, Y > q_0(\theta))$$

• Selection-corrected non-parametric upper bounds of AAT ("always takers") do not provide an indication of effects on test scores
Impact heterogeneity and indirect effects

- Effects on high school completion are larger for girls and beneficiaries in rural areas.
- No differential effects on test scores across groups.
- No indication of indirect program effects on the school completion of ineligible older children residing in the same households as participant children.
Robustness checks

- **PSM**: there is no definitive test to rule out selection on unobservables, yet various balancing checks support the comparability of treatment and control children on observables.

- **PSM and RDD on trimmed sample**: results hold after constructing a more balanced (trimmed) sample based on the distribution of the p-score – makes estimates more precise and less sensitive to changes in specification (Crump, Hotz, Imbens and Mitnik, Biometrika 2009)
Robustness checks (cont.)

- RDD: continuity checks on baseline characteristics
Robustness checks (cont.)

- RDD: no evidence of non-random sorting around the threshold

Density of the *Sisben* poverty score

![Histogram Score](image1)

![Histogram Score Zone](image2)
Robustness checks (cont.)

• Non-random migration and crossover effects are very unlikely to happen and/or invalidate the findings of this paper

• No evidence of misspecification bias (particularly important in RDD framework)

• No evidence that the higher rates of matching survey data and test records observed for the treatment group are driven by differences in the merging procedures and quality of information rather than by the effect of the program measures
Conclusions

• FA program increases school attainment → equivalent to an increase of 8-16 percent in graduation rates

• Extrapolation of program impacts to current program size (3.5 million poor children covered): 100,000-200,000 additional high school graduates

• Analysis on learning outcomes shows no impacts, consistent with existing evidence on similar programs in Mexico and Cambodia

• Lack of effects on the test scores raises the need to further explore policy actions (e.g. supplementary supply-side interventions and changes in project design) to couple CCT’s objective of increasing human capital with enhanced learning
Our research agenda

• Try to look at other impacts further down in the causal change: (1) enrollment in college and other forms of tertiary education, (2) labor market participation, type of jobs and earnings (difficult because it requires confidential data) and (3) intergenerational effects

• Other indirect effects: teenage pregnancy, domestic violence, women empowerment, spillover effects

• Other aspects of interest: effects of FA on electoral outcomes and female participation in elections
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