Do Conditional Cash Transfers Lead to Better Secondary Schools?

Evidence from Jamaica's PATH

Marco Stampini
Sofia Martinez-Cordova
Sebastian Insfran
Donna Harris

Social Protection and Health Division

TECHNICAL NOTE Nº IDB-TN-1125

October 2016

Do Conditional Cash Transfers Lead to Better Secondary Schools?

Evidence from Jamaica's PATH

Marco Stampini
Sofia Martinez-Cordova
Sebastian Insfran
Donna Harris

October 2016

Inter-American Development Bank

Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library
Do conditional cash transfers lead to better secondary schools?: evidence from Jamaica's PATH / Marco Stampini, Sofia Martinez-Cordova, Sebastian Insfran, Donna Harris.
p. cm. — (IDB Technical Note ; 1125)
Includes bibliographic references.
1. Transfer payments-Jamaica. 2. Economic assistance, Domestic-Jamaica. 3. Education, Secondary-Jamaica-Evaluation. 4. Poverty-Government policy-Jamaica. I. Stampini, Marco. II. Martinez-Cordova, Sofia. III. Insfran, Sebastian. IV. Harris, Donna. V. Inter-American Development Bank. Social Protection and Health Division. VI. Series.
IDB-TN-1125

scl-sph@iadb.org

www.iadb.org/SocialProtection

Do Conditional Cash Transfers Lead to Better Secondary Schools? Evidence from Jamaica's PATH[1]

Marco Stampini, Sofia Martinez-Cordova, Sebastian Insfran, Donna Harris

Abstract

We explored the hypothesis that the Programme of Advancement through Health and Education (PATH), Jamaica's conditional cash transfer program, contributes to breaking the inter-generational poverty cycle by placing its urban beneficiaries on a higher educational trajectory. Using a regression discontinuity design, we found that PATH urban male beneficiaries who sat the Grade Six Achievement Test (GSAT) over the period 2010–2014 performed better on the test (scoring 16.03 points, or 3.6%, higher than non-beneficiaries); consequently, they were placed in better secondary schools (1.5 percentiles higher in a national school ranking based on placed students' GSAT scores). In contrast, we found no significant impact for urban girls.

JEL Classification: I25, I38, O15

Keywords: conditional cash transfers (CCTs), educational aspiration, school performance, school placement, regression discontinuity design, Jamaica, Programme of Advancement through Health and Education (PATH).

Resumen – Exploramos la hipótesis de que el Programa de Transferencia Monetaria Condicionada de Jamaica, Programa de Avance a través de la Salud y Educación (PATH, por sus siglas en inglés), contribuye a romper el ciclo intergeneracional de la pobreza al ubicar a sus beneficiarios en una mejor trayectoria educativa. Utilizando un diseño de regresión discontinua, encontramos que los beneficiarios hombres del PATH que habitan en áreas urbanas y que tomaron el Examen de Aprovechamiento de Sexto Grado (GSAT, por sus siglas en inglés) durante el periodo 2010–2014: (i) obtuvieron mejores resultados en el GSAT (alcanzando 16.03 puntos, o 3.6%, más que los no beneficiarios); y, (ii) fueron consecuentemente ubicados en mejores escuelas secundarias (1.5 percentiles más en un ranking nacional de escuelas calculado en base a los resultados del GSAT de los estudiantes ubicados en ellas). En contraste, no encontramos resultados significativos para niñas que habitan en áreas urbanas.

[1] Social Protection and Health Division, Inter-American Development Bank. Emails: mstampini@iadb.org; anmartinez@iadb.org; sebastiani@iadb.org; donnah@iadb.org. We wish to thank the Government of Jamaica for providing the data used in this study. Many government and program officers went beyond the call of duty to provide us with information. In particular, we would like to thank Dunstan Bryan, Elsa Marks-Willis, Debayo Graham, Sasha Fraser, Grace-Ann Scarlett and Petula Manboard from the Ministry of Labour and Social Security; and Barbara Allen, Sonia Mullings and Gregory Henry from the Ministry of Education. We would also like to thank Matias Cattaneo, Diether Beuermann, Ciro Avitabile, Patrick McEwan, Ferdinando Regalia, Pablo Ibarrarán, Norbert Schady, Colette Robinson and Leticia Juarez for their useful comments. Katie Metz provided professional editing. Remaining errors are ours only. The content and findings of this paper reflect the opinions of the authors and not those of the IDB, its Board of Directors or the countries they represent.

1. Introduction

Conditional cash transfer (CCT) programs have become the main anti-poverty program in many Latin American and Caribbean countries (Fiszbein and Schady, 2009; Stampini and Tornarolli, 2012; Paes-Sousa et al., 2013). Numerous rigorous impact evaluations have shown that CCTs have been successful in increasing "school enrollment and attendance (with rates that vary from 0.5 percentage points (pp) in Jamaica to 12.8 pp in Nicaragua). […] CCTs also increased school attainment. For example, in Mexico after 3–5 years of participation in the program *Oportunidades*, the beneficiaries accumulated between half and one year of additional schooling.

The evidence on learning achievement is mixed (Fiszbein and Schady, 2009; García et al., 2012; Saavedra and García, 2012). Barham et al. (2013) find that, in Nicaragua, receiving the CCT for three years had significant impacts on years of schooling and on mathematics and language learning for young men 10 years after participating in the program. Learning increased by one quarter of a standard deviation, which loosely corresponds to half a year of learning. On the other hand, Behrman et al. (2009) find that higher enrollment levels have not resulted in better performance on achievement tests in Mexico. Evidence from outside the region is also mixed. Baird et al. (2011) report positive impacts on learning for a pilot CCT in Malawi, while Filmer and Schady (2014) and Benhassine et al. (2015) find no effect of a CCT on learning outcomes in Cambodia and Morocco, respectively" (IDB, 2014, p. 2).[2]

More recently, the literature has started to look at CCTs' impacts on educational aspirations.[3] CCTs may help beneficiaries break out of the "aspirational trap" through the provision of a steady income flow, which can lead to adopting a longer-term perspective.[4] Macours and Vakis (2008) find that an asset transfer program in Nicaragua had a positive effect on attitudes toward the future and, consequently, toward investments in human capital. Chiapa et al. (2012) show that Mexico's *Progresa* increased beneficiary parents' educational aspirations for their children. Finally, Avitabile et al. (2015) find that, in Mexico City, *Oportunidades* increased students' probability of choosing a vocational track, which is associated with higher labor market returns when entering the labor market after completing secondary education.

In this paper, we explore the hypothesis that Jamaica's CCT Programme of Advancement through Health and Education (PATH) contributes to breaking the inter-

[2] For a recent critical literature review of the evidence on CCT long-term impacts, see Molina-Millan et al. (2016).

[3] The rationale for focusing on this channel is that living in poverty hampers the capacity to aspire to a better future and thereby creates a self-sustained trap (Ray, 2002; Mullainathan and Shafir, 2013). Richer people have greater "navigational capacity" to aspire (Appadurai, 2004) and more opportunities to explore the possible outcomes of their choices and share them with one another. In contrast, the poor have fewer opportunities to use this "navigational capacity"; the options they foresee for their future are, therefore, more limited (Appadurai, 2004; Székely, 2015).

[4] In this fashion, CCTs would address the financial needs that truncate parents' aspirations (Gutman and Akerman, 2008). The literature suggests the existence of alternative channels. For example, CCTs may bring exposure to highly educated role models such as social workers and school and health care professionals. Having access to these professionals may give beneficiaries a bigger and richer stock of experiences from which to learn about the link between aspirations and outcomes (Appadurai, 2004). They could become mentors for beneficiary children and play an important role in their aspirational development. The lack of mentors faced by poor children and households hampers aspiration formation (Gutman and Akerman, 2008).

generational poverty cycle by increasing school performance and aspirations, thereby placing its beneficiaries on a higher educational trajectory. More specifically, we look at whether PATH beneficiaries achieve placement in higher quality secondary schools, relative to comparable children who do not receive the program.

To the best of our knowledge, our study is the first in the literature to take a comprehensive look at CCT impacts on educational aspirations, school performance and placement. We use a regression discontinuity design (RDD), which compares students on the two sides of the PATH eligibility threshold. Data comes from two sources. The first contains PATH applications during the period 2007–2008, including socio-economic information and the PATH eligibility score, which is essential for the RDD. The second contains the scores of the Grade Six Achievement Test (GSAT) taken between 2009 and 2014, preferences for secondary schools expressed before taking the test, and the post-test placement. Due to limitations in the data from rural areas, we focus on urban areas only.

We find evidence that PATH improved boys' GSAT score achievement and school placement. More specifically, PATH urban male beneficiaries who sat the GSAT between 2010 and 2014 performed better on the test (scoring 16.03 points, or 3.6%, higher than non-beneficiaries) and, consequently, were placed in better secondary schools (1.5 percentiles higher in a national school ranking based on placed students' GSAT scores). We find no significant impact on educational aspirations or urban female beneficiaries' performance and placement.

The remainder of the paper is organized as follows. Section 2 provides background information on Jamaica's education system and PATH. Section 3 describes data sources and the process used to merge them into a unique data set. Section 4 explains our identification strategy, based on RDD. Section 5 presents the results of the impact evaluation. Section 6 concludes the paper.

2. Background

2.1. Education in Jamaica

Schooling is compulsory in Jamaica from age 4 to 16 and typically includes preschool, elementary school, and secondary school up to grade 11. Primary education is made up of six grades, with children normally attending from age 6 to 11. Secondary education is divided into a lower cycle (grades 7 to 9) and an upper cycle (grades 10 and 11).

Education is mainly public. As of the academic year 2007–2008, around 91% of elementary students and 95% of secondary students attended public schools. There were 1,014 public schools, of which: 546 offered primary education only; 246 were all-age schools (offering grades 1 to 9) or primary and junior high schools that offered both primary and secondary education (up to grade 9); 148 were secondary high schools, and 16 were technical or agricultural schools (Ministry of Education of Jamaica, 2012).[5] The average teacher-to-pupil ratio was 1:30 for primary education and 1:19 for secondary education (UNESCO, 2009).

School attendance drops at the secondary level. In 2011, the secondary school net enrollment rate was 74%, with a significant gap for boys, whose enrollment rate was 72% versus 76% for girls.[6]

In grade 6 (i.e., age 11 or 12), all students are required to sit the GSAT.[7] The exam evaluates academic development in mathematics, science, language arts, social studies and communication tasks. During the period of our analysis, i.e., between 2010 and 2014, the subject area with the lowest score was language arts, with a mean value of 57.86%. The mean GSAT combined score was 442.44 (Table 1), which corresponds to the 58th percentile of its own distribution.

In general, girls outperform boys in all subject areas. For instance, girls' language arts and communication GSAT scores exceeded the boys' scores by about 8 percentage points (Table 1). In terms of the GSAT combined standard score, girls outperformed boys by 21.28 points (their average corresponded to the 69th percentile of the distribution, versus the 44th percentile for the boys).

Table 1 – Average GSAT Scores between 2010 and 2014, by Gender

Subject areas	Total	Female	Male	Difference (F – M)
Mathematics (%)	58.20	60.98	55.25	5.73 ***
Science (%)	61.00	63.08	58.77	4.31 ***
Social studies (%)	58.33	60.87	55.62	5.25 ***
Language arts (%)	57.86	61.66	53.80	7.85 ***
Communication task (%)	68.13	72.09	63.91	8.18 ***
Combined standard score	442.44	452.74	431.45	21.28 ***

Source: Authors' calculations based on data from Jamaica's Ministry of Education. Note: *** statistically significant at 1%, ** statistically significant at 5%, and * statistically significant at 10%.

The GSAT serves as the main mechanism to determine students' secondary school placement. Children sitting the GSAT have the possibility to indicate up to five preferred secondary schools. According to the Ministry of Education, more than 70% of students are placed in one of the five schools of their choice (Buckley, 2015). The majority of the remaining 30% are placed in schools close to their homes (Saunders, 2015). Over the period 2010–2014, 66.7% of students who sat the GSAT were placed in one of their five preferred schools, and

[5] The remaining 58 were infant schools (31), special schools (10) and other public institutions (i.e., community centers, teachers colleges, among others) (17).

[6] Data from the World Development Indicators (http://databank.worldbank.org/). Secondary school gross enrollment rates in 2011 were 89% for the total population, 91% for girls and 86% for boys. Gross and net enrollment rates with gender disaggregation are available for 2005 and 2011. Values for 2005 were higher than for 2011. For example, in 2005 the secondary school net enrollment rate was 83% for the total population, 85% for girls and 81% for boys.

[7] The GSAT was first implemented in 1999 as a replacement for the Common Entrance Examination (CEE). The GSAT is part of the National Assessment Programme (NAP), which also includes the Grade Four Literacy Test (G4LT), aimed at assessing elementary school performance (Lewis, 2010).

16.7% were placed in their first preferred school (Table 2). Normally, the first choices for students are “high achieving traditional high school[s] in Kingston, Manchester, or St. Elizabeth” (Miller, 2014, p.44).[8]

Table 2 – School Placement over the Period 2010–14

	2010	2011	2012	2013	2014	Period average
% placed in first preferred school	15.1	16.6	16.9	16.9	18.4	16.7
% placed in second preferred school	12.4	13.6	11.9	13.2	13.7	12.9
% placed in third preferred school	11.4	12.3	11.1	12.2	12.9	12.0
% placed in fourth preferred school	12.5	13.0	12.5	12.2	13.5	12.7
% placed in fifth preferred school	12.0	12.5	12.3	11.8	13.1	12.3
% placed in a non-preferred school	33.1	27.7	30.5	30.7	25.6	29.6
% with no placement	3.2	4.0	2.8	2.5	2.9	3.1
% with no school preference	0.3	0.3	1.8	0.4	0.2	0.6
Total (%)	100.0	100.0	100.0	100.0	100.0	100.0
Total (n. obs.)	48,311	43,479	45,545	43,570	40,870	44,355

Source: Authors’ calculations based on data from Jamaica’s Ministry of Education.

2.2. PATH

PATH was launched in 2002 as part of a reform of Jamaica’s social safety net. Like other CCTs, it has the double objective of alleviating current poverty while fostering human capital development among beneficiary households’ children. In contrast to many other CCT programs, it also targets the elderly and individuals with disabilities.

Eligibility is determined through a proxy means test (PMT) that estimates applicants’ living conditions. Data from the application form is entered in the program management information system and combined to calculate the PMT score. If this score is lower than the eligibility threshold, the applicant household is immediately declared eligible. In certain cases, the application is followed by a home interview aimed at collecting further information that will be used to determine eligibility.

Transfers are bimonthly and made mostly through the postal system (Levy and Ohls, 2007). During the period of our analysis, the transfer conditioned on school attendance varied based on gender and grade. For example, in 2012, boys in grades 1–6, 7–9 and 10–13 received J$ 1,650, J$ 2,150 and J$ 2,530, respectively, every two months. The transfers for girls were lower, at J$ 1,500, J$ 1,950 and J$ 2,300, respectively, every two months (MLSS, 2012).[9]

[8] According to Miller (1999), traditional high schools were the only type of high schools before 1953. After the creation of other types of high schools, traditional high schools became elitist schools focusing on the middle and upper classes. Since 1957, enrollment in traditional high schools has been based on academic merit. In 1973, the government abolished tuition fees, thereby making traditional high schools free of charge.

[9] In 2012, J$ 88.99 = USD 1 (http://www.boj.org.jm/foreign_exchange/fx_rates_annual.php). So, for example, J$ 1,500 = USD 16.86. On the generosity of PATH transfers relative to other CCTs, see Figure 2 in Stampini and Tornarolli (2012).

For their household to receive the education transfer, children must attend at least 85% of school days. Compliance is verified through information provided by the schools to the program every two months (Levy and Ohls, 2007). Households that are not in compliance continue receiving other components of the transfer, including a minimum social protection floor of J$ 800 every two months (MLSS, 2012).

3. Data Sources and Preparation

We use two sources of data. The first is from the Ministry of Labour and Social Security (MLSS), more specifically, from the PATH management information system. It provides a snapshot of the demographic characteristics and socio-economic conditions of the households (including individual members) that applied to PATH during the period 2007–2008. It includes information on the following: (i) households' socio-economic and demographic characteristics, such as income, type of dwelling, parish, and members' age, gender, and marital status; and (ii) the household's proxy means test score that determines eligibility for PATH.[10] Due to limitations in the data from rural areas, we focus on urban areas only. Participation is identified through the household's "approved" or "provisionally approved" application status. This is consistent with information on the date of entry into the program. Most eligible applicants entered the program between 2007 and 2009.

The second source of data is from the Ministry of Education. It contains the results of the GSAT taken during the period 2009–2014, including the following: (i) secondary school preferences (up to five preferred schools);[11] (ii) GSAT scores (scores for the subjects of mathematics, science, language arts, social studies and communication tasks, as well as a combined score); and (iii) the school in which the student was consequently placed (after taking the GSAT). For each year between 2010 and 2014, we build an indicator of school quality, which is equal to the average GSAT combined score of the students placed in that school the previous year. This proxy is calculated for 341 secondary schools, identified through a school identifier code. We use this indicator to measure the quality of both school preference and placement.[12]

[10] Two scores are available. The first is assigned based on the data provided during the application. The second includes some changes following appeals and home visits by social workers in charge of producing a final assessment and determining eligibility in case of doubt. We use the former, which is determined homogeneously for every applicant (while the latter may include selected shifts across the eligibility cutoff resulting from the appeal process).

[11] We talk of preferred school, without any attribution of the preference to the child or his/her parents. Although it is reasonable to assume that school choices are the result of a family decision-making process or influenced by teachers, we have no information that allows us to disentangle the relative importance of alternative views and to determine who is the ultimate decision maker.

[12] The rationale for using data from the previous year is that recent information is more likely to be available to students taking the GSAT.

Our indicator measures quality at entry rather than students' improvement during secondary education. Unfortunately, at the time of writing this paper, we had no access to indicators of performance at or after graduation. Nonetheless, it is reasonable to assume that students with better performance will attempt to enter better schools, so that the average GSAT of enrolled students can be used as a proxy for school quality.

The PATH database contains 140,131 individuals belonging to 42,417 urban households. We use a sample of 15,509 urban children born between 1998 and 2001.[13] These children were likely to have taken the GSAT between 2010 —the first year after the process of inclusion of applicants was completed— and 2014. We merged this sample with GSAT data from the Ministry of Education based on children's first name, last name and date of birth.[14] The merge was successful in 10,999 cases (which represent 70.9% of the sample).

Individuals for whom we are able to merge PATH and GSAT data (i.e., those in the sample we will use for the impact evaluation) have observable characteristics that are similar to those of the attriters (i.e., those with PATH data only, for whom GSAT data could not be retrieved). Table 3 compares the sample means of selected characteristics. It shows that the only differences that are statistically significant are for gender and age, with girls and older children more likely to have GSAT information. The gender difference is consistent with the fact that girls have better school progression, which increases the likelihood for them to have taken the GSAT within our period of analysis. The age difference may be explained by the fact that older children had more time to complete primary school and take the GSAT.

[13] We restrict the sample to children born between 1998 and 2001. Children in our sample who were born in 1997 entered PATH between October 2008 and February 2009, and they typically sat the GSAT in March 2009. Due to the short length of program exposure, we decided to focus on children born in 1998 or later; however, only a portion of the children in our sample who were born in 2002 were likely to have taken the GSAT in 2014 (the GSAT is typically taken at age 11 or 12), the most recent year for which we have available data. This is consistent with a sudden drop in the percentage of children for whom we can retrieve information on the GSAT from the data sets provided by the Ministry of Education. This percentage drops from 69.6% for children born in 2001 to 51.3% for children born in 2002. For this reason, we focus on children born before 2002.

The dataset contained 16,601 urban observations with birth year between 1998 and 2001. We carefully cleaned the data to eliminate duplicates (keeping one of two or more identical observations) and cases in which we could not tell apart two individuals with the same name (in these cases, eliminating both observations that had the same name and birth date but different information on socio-economic characteristics). This process left us with 15,569 observations. We further eliminated observations that represented outliers of the assignment variable, i.e., the proxy means test score. These had a score of 100 or less (above 100, the next smallest value was 927.94). After this step, the size of the sample dropped to 15,509.

[14] We modified a STATA code —kindly provided by Diether Beuermann— to fit our needs. The code first merged individuals with a perfect match on first name, last name, year of birth, month of birth, and day of birth. Then, it progressively relaxed the requirement for a perfect match on first and last name, taking into consideration special characters, typographical errors and/or misspellings. Finally, the code also considered the possible inversion of day and month in the date of birth.

Table 3 – Relationship between Attrition and Observable Characteristics

Variable	Total		Attriters (without GSAT)		Impact evaluation sample (with GSAT)		Difference of means and p-value of the t-test	
	Obs.	Mean	Obs.	Mean	Obs.	Mean	Mean	p-value
Gender (female=1)	6,867	0.49	1,969	0.45	4,898	0.512	-0.06	**0.00**
Age on December 31, 2009	6,850	9.95	1,952	9.86	4,898	9.987	-0.12	**0.00**
PATH eligibility status (eligible=1)	6,867	0.36	1,969	0.37	4,898	0.359	0.01	0.55
Family is PATH beneficiary (yes=1)	6,867	0.52	1,969	0.53	4,898	0.521	0.01	0.57
Family head gender (female=1)	6,867	0.93	1,969	0.93	4,898	0.935	-0.01	0.17
Family head completed secondary (yes=1)	6,867	0.43	1,969	0.42	4,898	0.436	-0.02	0.13

Source: Authors' calculations. Note: t-test on the difference of means calculated for observations within the optimal bandwidth of school performance (±22.50).

4. Empirical Approach – Fuzzy Regression Discontinuity Design

Our impact evaluation adopts a regression discontinuity design (RDD). Eligibility for PATH depends on whether the household's proxy means test score (the assignment variable) is below a fixed eligibility cutoff, which is unknown to potential beneficiaries;[15] therefore, we compare the outcomes of children who were just below the cutoff (the intent-to-treat group), with those of children who were just above it (the control group). This identification strategy exploits the fact that the two groups, in a sufficiently narrow interval around the cutoff, cannot be very different from each other except for the possibility to benefit from the program.

Table 4 shows a sudden drop in program participation, from 70.6% to 18% at the 1046 threshold. This is the cutoff we employ for the regression discontinuity analysis. Table 5 shows that, in our sample, non-compliance is limited to 159 children below and 368 children above this cutoff.

[15] Our estimations are based on STATA's *rdrobust* command, which considers as treated the observations with the assignment variable above a certain cutoff; therefore, to comply with this assumption, we multiply both the assignment variable and the cutoff score by -1. All discussion in the text and graphic representations of the results, however, are based on the original values of the assignment variable and the cutoff, with the treated to the left of (or below) the eligibility cutoff.

Table 4 – Participation in PATH for Applicants with Eligibility Scores around the Cutoff

Eligibility score (range)	PATH participation rate
1036 – 1037	95.4%
1037 – 1038	92.6%
1038 – 1039	94.7%
1039 – 1040	100.0%
1040 – 1041	92.7%
1041 – 1042	93.3%
1042 – 1043	89.0%
1043 – 1044	90.4%
1044 – 1045	95.7%
1045 – 1046	70.6%
1046 – 1047	18.0%
1047 – 1048	22.8%
1048 – 1049	13.2%
1049 – 1050	19.7%
1050 – 1120	5.6%

Source: Authors' calculations.

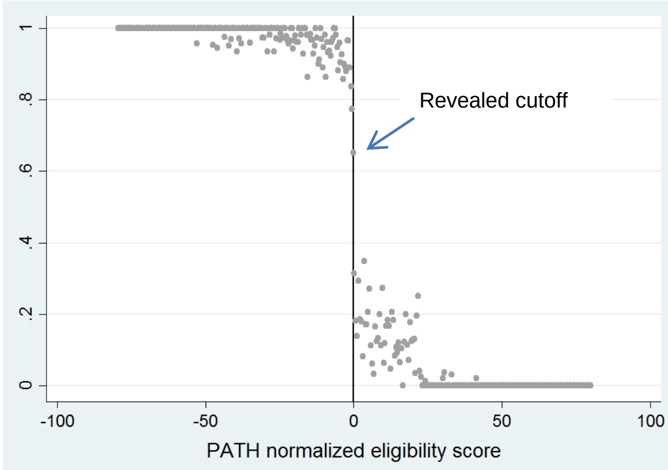
Table 5 – Compliance with PATH Revealed Eligibility Rule

	Control (Not eligible)	Treatment (Eligible)	Total
Non-participant	5,376	159	5,535
Participant	368	5,096	5,464
Total	5,744	5,255	10,999

Source: Authors' calculations.

We normalize the proxy means test score to set the eligibility cutoff to zero (so that values below zero identify the intent-to-treat group). Since the observed probability of treatment does not fall from 1 to 0 when the cutoff is crossed (Figure 1), our estimates are based on a fuzzy RDD, which employs the proxy means test score as the instrumental variable. The RDD is referred to as "fuzzy" when participation does not comply perfectly with the eligibility/treatment rule, yet the probability of participation is discontinuous at the eligibility cutoff.

Figure 1 – Rate of Participation in PATH



Source: Authors' calculations.

In a fuzzy RDD, the local average treatment effect (LATE) is calculated by dividing the variation in the outcome by the variation in the probability of take-up (Lee and Lemieux, 2009), as expressed in equation (1):

$$\tau_F = \frac{lim_{\varepsilon \downarrow 0} E[Y|X=c+\varepsilon] - lim_{\varepsilon \uparrow 0} E[Y|X=c+\varepsilon]}{lim_{\varepsilon \downarrow 0} E[D|X=c+\varepsilon] - lim_{\varepsilon \uparrow 0} E[D|X=c+\varepsilon]} \quad (1)$$

where:

Y is the outcome variable: educational aspiration, school performance, and school placement;
X is the PATH proxy means test score (assignment variable);
D is a dummy indicating participation in PATH;
c is the revealed cutoff;
ε is the error term.

Educational aspiration is measured by the average of the quality of the five preferred schools indicated by the student at the time of GSAT testing, with the quality of each school proxied by the average GSAT combined score of the students placed in that school the previous year. School performance is measured by the GSAT combined score of the student. Finally, school placement is measured by the quality of the school in which the student is placed, proxied by the average GSAT combined score of the students placed in that school the previous year.

The validity of our identification strategy fundamentally relies on the assumption of continuity of the assignment variable X_i (PATH proxy means test score).[16] Figure 2 represents the distribution of X_i; it graphically presents the clustering of applicants on either side of the

[16] RDD estimates fundamentally rely on the assumption that individuals are unable to precisely manipulate the assignment variable. When this happens, variation in treatment near the eligibility cutoff is randomized as though from a randomized experiment (Lee and Lemieux, 2009).

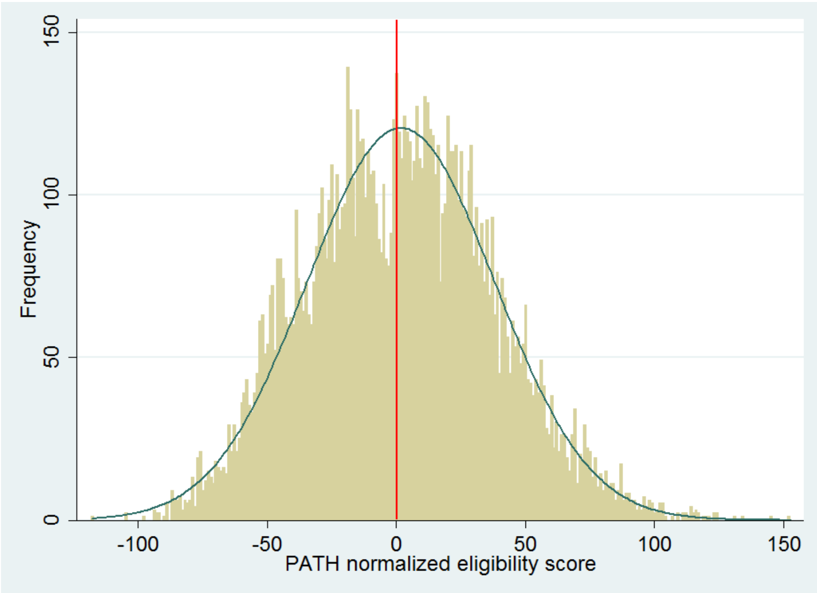
cutoff. One concern discussed in the literature on proxy means testing for the assignment of households to CCT programs is that applicants might manipulate their statements in order to move to the left of the eligibility cutoff. This would generate a spike in the density of the distribution and would create a discontinuity that hampers the validity of the RDD estimates. Results may be biased by the fact that the most entrepreneurial applicants have managed to move to the left of the cutoff. Positive estimates may be driven by these applicants' unobservable characteristics instead of program participation. Figure 2 shows that this is not our case.

Rather than relying on visual assessment, we test the assumption of continuity of the assignment variable at the eligibility cutoff using the methodology proposed by Cattaneo et al. (2016). The results, reported in Table 6, show no evidence of significant discontinuity around the cutoff (with a p-value of 0.25).

The validity of our identification strategy also relies on the assumption of continuity in the distribution of observable characteristics, which guarantees that the treatment and the control groups are comparable at the cutoff except for program participation. We test this hypothesis by estimating equation (1) with baseline covariates (gender, age at the time of GSAT testing, household head gender, and household head education) in place of the outcome variable. We find no evidence of significant discontinuous change at the cutoff (see the p-value of the take-up coefficients in Table 7).

In addition, it is worth noting that we find no evidence of significant discontinuous change in the probability of merge across data sets (or probability of attrition) at the cutoff (Table 7, last column). This reassures us that attrition does not represent a threat to the validity of our results.

Figure 2 – Distribution of the PATH Proxy Means Test Score



Source: Authors' calculations.

Table 6 – Discontinuity Test on the Assignment Variable

Discontinuity test	-1.15
p-value	(0.25)
Effective number of observations not eligible	1,471
Effective number of observations eligible	1,018
Bandwidth not eligible	12.37
Bandwidth eligible	10.63

Source: Authors' calculations. Note: STATA *rddensity* with triangular kernel and jackknife standard errors. Testing procedure based on robust bias-corrected method using MSE-optimal bandwidth choice. P-value in parentheses. *** statistically significant at 1%, ** statistically significant at 5%, and * statistically significant at 10%.

Table 7 – Discontinuity of Baseline Covariates around the Revealed Eligibility Cutoff

Covariates	Gender (female = 1)	Age at GSAT	Household head gender (female = 1)	Household head completed high school	Probability of attrition (non-attriters = 1)
Take-up	0.05	0.02	-0.01	0.03	0.04
	(0.29)	(0.46)	(0.61)	(0.41)	(0.22)
Mean covariates	0.51	11.49	0.93	0.46	0.71
Standard Deviation	0.50	0.55	0.25	0.50	0.45
Observations	10,999	10,999	10,999	10,999	15,509

Source: Authors' calculations. Note: *rdrobust* command with linear polynomial function and triangular kernel using one common MSE-optimal bandwidth selector for the RDD treatment effect estimator. Standard errors clustered at the household level. P-values in parentheses, *** statistically significant at 1%, ** statistically significant at 5%, and * statistically significant at 10%.

Consequently, we feel confident about the rigor of our RDD design. We estimate the local average treatment effect of PATH on educational outcomes and implement the robust bias-corrected confidence intervals proposed by Calonico et al. (2014, 2016). Our estimates are based on order-1 local polynomial regressions (i.e., linear regressions), using one common mean square error (MSE) optimal bandwidth selector for the RDD treatment effect estimator (Calonico et al., 2016). Annex 1 presents sensitivity analysis with quadratic polynomial regressions and shows that results are robust. Standard errors are clustered at the household level, consistent with the PATH level of intervention.

We also present the results graphically, with bin selection based on the integrated mean standard error (IMSE) optimal evenly-spaced method (Calonico et al., 2014). The graphical results are presented within the MSE-optimal bandwidth selector for the RDD treatment effect estimator.[17]

[17] We use STATA's *rdrobust* and *rdplot* commands. The *rdrobust* bandwidth selector is an upgraded version of the Imbens and Kalyanaraman (2012) and Calonico et al. (2014) implementation of the MSE-optimal bandwidth selector (Calonico et al., 2016). The *rdplot* IMSE-optimal evenly-spaced method locally approximates the underlying regression function by taking the global polynomial fit as benchmark; it provides graphical evidence of local treatment effects around the cutoff score (Calonico et al., 2014).

5. Results: Does PATH Lead to Better Secondary Schools?

We find that PATH had a significant impact on the educational trajectory of its urban male beneficiaries. For this group, participation in PATH improved the quality of the placement school. This impact appears to be driven by an increase in school performance. We discuss the results in detail below.

Consistently, for both boys and girls, we find no evidence of a significant impact on educational aspirations. The quality of the five preferred schools indicated by PATH beneficiaries at the time of GSAT testing is not statistically different from that of non-beneficiaries (Table 8).

In contrast, the analysis of school performance shows that participation in PATH led to a significant improvement in the GSAT combined score of urban male beneficiaries (Table 9 and Figure 3). The 16.03-point impact is equivalent to a 3.6% increase relative to the control group (non-beneficiaries within the impact evaluation bandwidth). In contrast, we find no evidence of impact on girls' school performance. This may be explained by the fact that girls, who had higher pre-treatment GSAT scores, had less margin for improvement when exposed to PATH.

Higher school performance resulted in PATH urban male beneficiaries being placed in higher quality schools relative to similar children who did not participate in the program. More specifically, PATH's urban boys were placed in schools whose GSAT combined score was higher by 11.81 points (Table 10 and Figure 4). This corresponds to an increase of 1.5 percentiles in the ranking of Jamaican schools.[18] As for school performance, no statistically significant result was found for girls' school placement.

Table 8 – Impact of PATH on Educational Aspirations, Measured by Previous Year Average of the GSAT Combined Score in the Five Preferred Schools: Fuzzy RDD, Point Estimators by Gender

Sample	Total	Female	Male
Take-up	-0.23	-3.01	1.06
	(0.98)	(0.40)	(0.71)
Mean Dep. Var.	506.86	512.42	501.08
SD Dep. Var.	29.54	27.53	30.44
Observations not eligible	2,884	1,286	1,523
Observations eligible	2,643	1,230	1,358
Bandwidth	25.96	22.81	27.91

Source: Authors' calculations. Note: *rdrobust* command with linear polynomial function and triangular kernel using one common MSE-optimal bandwidth selector for the RDD treatment effect estimator. Standard errors clustered at the household level. P-values in parentheses, *** statistically significant at 1%, ** statistically significant at 5%, and * statistically significant at 10%. Results are consistent when estimated using covariates (not shown).

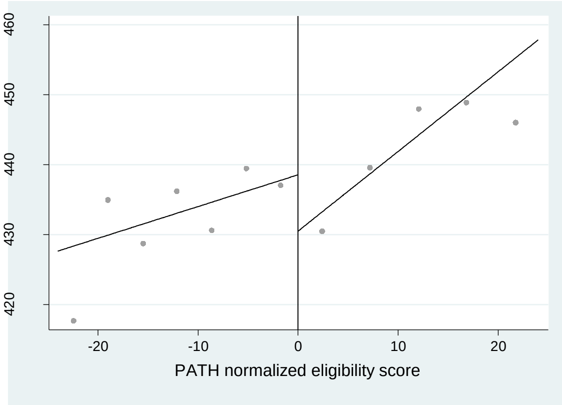
[18] The ranking is based on placed students' average GSAT combined scores over the period 2009-2013.

Table 9 – Impact of PATH on School Performance, Measured by the GSAT Combined Score: Fuzzy RDD, Point Estimators by Gender

Sample	Total	Female	Male	
Take-up	7.32	-2.34	16.03	**
	(0.11)	(0.93)	(0.03)	
Mean Dep. Var.	449.61	459.89	438.95	
SD Dep. Var.	60.9	56.93	63.01	
Observations not eligible	2,501	1,291	1,323	
Observations eligible	2,259	1,221	1,154	
Bandwidth	22.50	23.25	24.16	

Source: Authors' calculations. Note: *rdrobust* command with linear polynomial function and triangular kernel using one common MSE-optimal bandwidth selector for the RDD treatment effect estimator. Standard errors clustered at the household level. P-values in parentheses, ***statistically significant at 1%, ** statistically significant at 5%, and * statistically significant at 10%. Results are consistent when estimated using covariates (not shown).

Figure 3– Relationship between PATH Eligibility Score and Boys' GSAT Combined Score



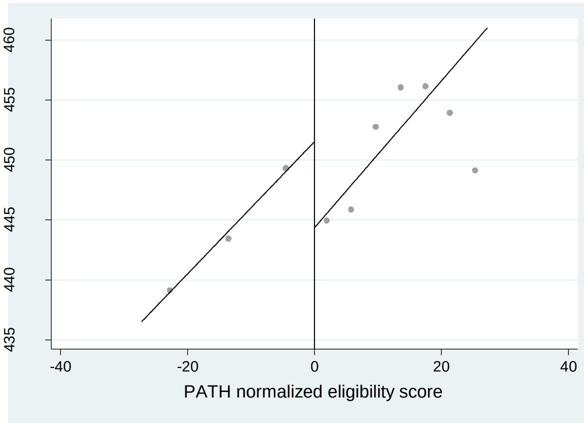
Source: Authors' calculations. Note: We use a linear regression (polynomial degree = 1) to fit the scatter of binned means within the optimal bandwidth choice. For bin selection, we adopt the IMSE-optimal evenly-spaced method using spacing estimators.

Table 10 – Impact of PATH on School Placement, Measured by Previous Year Average of the GSAT Combined Score in the School of Placement: Fuzzy RDD, Point Estimators by Gender

Sample	Total	Female	Male	
Take-up	6.62	0.15	11.81	**
	(0.12)	(0.84)	(0.03)	
Mean Dep. Var.	454.21	458.88	449.35	
SD Dep. Var.	53.84	53.43	53.84	
Observations not eligible	2,530	1,255	1,469	
Observations eligible	2,311	1,193	1,286	
Bandwidth	23.16	22.88	27.30	

Source: Authors' calculations. Note: *rdrobust* command with linear polynomial function and triangular kernel using one common MSE-optimal bandwidth selector for the RDD treatment effect estimator. Standard errors clustered at the household level. P-values in parentheses, *** statistically significant at 1%, ** statistically significant at 5%, and * statistically significant at 10%. Results are consistent when estimated using covariates (not shown).

Figure 4 – Relationship between PATH Eligibility Score and GSAT Combined Score of Boys' School of Placement



Source: Authors' calculations. Note: We use a linear regression (polynomial degree = 1) to fit the scatter of binned means within the optimal bandwidth choice. For bin selection, we adopt the IMSE-optimal evenly-spaced method using spacing estimators.

6. Conclusions

We find consistent evidence that participation in Jamaica's CCT program places urban boys on a higher educational trajectory by significantly increasing their GSAT scores and the quality of the secondary school in which they are placed. To the best of our knowledge, this is the first study to document such comprehensive impacts of a CCT program on a key transition of the educational trajectory, such as that between primary and secondary education.

One caveat is worth discussing. We cannot match all children in our sample with GSAT data. Missing information can be due either to the fact that the child did not have the opportunity to sit the GSAT (e.g., because of dropout) or to our inability to match individuals across different data sets. We are, however, confident of the validity of our results for three reasons. First, our rate of successful merge, which exceeds 70%, is high for studies based on administrative data (with no attempt to find and interview individuals with missing information), and especially so for studies in which the merge is based on name and date of birth (rather than on a unique individual identifier common to all data sets). Second, we showed that attrition was not correlated with socio-economic characteristics that may affect both the treatment status and educational outcomes on which we focused. Third, we showed that the distribution of baseline covariates (including the probability of attrition) around the eligibility cutoff is continuous. This provides comfort on the validity of our RDD estimates.

We only find impacts for boys. Although we have no means to test the hypothesis, this gender dimension of the results may be explained by (and is consistent with) the fact that girls exhibit higher levels of pre-treatment school performance (lower dropout, higher academic achievement). For example, in the academic year 2013–2014, female enrollment was 6 percentage points higher than male enrollment (UNESCO, 2014). Consequently, it is for the boys that the provision of a steady income flow is more likely to produce impacts in terms of educational trajectory. Alternatively or additionally, lack of significant impacts among girls may be due to limited statistical power.

References

Appadurai, A. (2004). “The Capacity to Aspire: Culture and the Terms of Recognition.” In V. Rao and M. Walton (Eds.), *Culture and Public Action*, Washington, DC: The World Bank.

Avitabile, C., Bobba, M., and Pariguana, M. (2015). “High School Track Choice and Financial Constraints. Evidence from Urban Mexico.” Policy Research Working Paper 7427. The World Bank Group. Available at: http://www-wds.worldbank.org/servlet/WDSContentServer/WDSP/IB/2015/10/01/090224b0831183a4/2_0/Rendered/PDF/High0school0tr0ce0from0urban0Mexico.pdf

Baird, S., McIntosh, C., and Özler, B. (2011). “Cash or Condition? Evidence from a Cash Transfer Experiment.” *The Quarterly Journal of Economics*, 126(4), 1709–1753. doi:10.1093/qje/qjr032

Barham, T. (2011). “A Healthier Start: The Effect of Conditional Cash Transfers on Neonatal and Infant Mortality in Rural Mexico.” Journal of Development Economics. 94: 74- 85.

Barham, T., Macours, K., and Maluccio, J. A. (2013). “More Schooling and More Learning?: Effects of a Three-year Conditional Cash Transfer Program in Nicaragua after 10 years.” IADB Working Paper Series IDB-WP-432.

Behrman, J. R., Parker, S. W., and Todd, P. E. (2009). “Schooling Impacts of Conditional Cash Transfers on Young Children: Evidence from Mexico.” Economic Development and Cultural Change 57(3): 439–477.

Benhassine, N., Devoto, F., Duflo, E., Dupas, P., and Pouliquen, V. (2015). “Turning a Shove into a Nudge? A “Labeled Cash Transfer” for Education.” American Economic Journal: Economic Policy, 7(3): 86-125.

Bernal, R., and Peña, X. (2011). “Guía práctica para la evaluación de impacto.” Colombia: Universidad de los Andes, Facultad de Economía, Centro de Estudios sobre Desarrollo Económico.

Buckley, B. *“GSAT Statement by Hon. Rev. Ronald Thwaites, Minister of Education.”* Jamaica Information Service *17* June *2015. Available at:* http://jis.gov.jm/gsat-statement-hon-rev-ronald-thwaites-minister-education/

Calonico, S., Cattaneo, M., and Titiunik, R. (2014). “Robust Data-driven Inference in the Regression-discontinuity Design.” *The Stata Journal*. 14(4): 909-946. Available at: http://www-personal.umich.edu/~cattaneo/papers/Calonico-Cattaneo-Titiunik_2014_Stata.pdf

Calonico, S., Cattaneo, M., Titiunik, R., and Farrell, M. (2016). “rdrobust: Software for Regression Discontinuity Designs.” Unpublished manuscript available at: http://faculty.chicagobooth.edu/max.farrell/research/Calonico-Cattaneo-Farrell-Titiunik_2016_Stata.pdf

Cattaneo, M., Jansson, M., and Ma, X. (2016). “rddensity: Manipulation Testing based on Density Discontinuity.” Stata Working Paper. Available at: http://www-personal.umich.edu/~cattaneo/papers/Cattaneo-Jansson-Ma_2016_Stata.pdf

Chiapa, C., Garrido, J. L., and Prina, S. (2012). “The effect of social programs and exposure to professionals on the educational aspirations of the poor.” *Economics of Education Review*. 31(1): 778-798.

Filmer, D., and Schady, N. (2014). “The Medium-Term Effects of Scholarships in a Low-Income Country.” Journal of Human Resources, 49(3), 663–694. doi:10.1353/jhr.2014.0022

Fiszbein, A., and Schady, N. R. (2009). “Conditional Cash Transfers: Reducing Present and Future Poverty.” World Bank Policy Research Report. Washington, DC, United States: 24, World Bank.

García, A., Romero, O. L., Attanasio, O., and Pellerano, L. (2012). “Impactos de Largo Plazo del Programa Familias en Acción en Municipios de Menos de 100 mil Habitantes en los Aspectos Claves del Desarrollo del Capital Humano.” Technical report, Union Temporal Econometria S.A. SEI. con la asesoria del IFS.

Gutman, L. M., and Akerman, R. (2008). “Determinants of Aspirations.” Research Report No.27. London: Center for Research on the Wider Benefits of Learning, Institute of Education.

Inter-American Development Bank – IDB. (2014). “Conditional Cash Transfers Research Brief.” Unpublished manuscript. Washington, DC, United States: Inter-American Development Bank.

Imbens, G. W., and Kalyanaraman, K. (2012). “Optimal Bandwidth Choice for the Regression Discontinuity Estimator.” *Review of Economic Studies*. 79(3): 933-959.

Imbens, G., and Lemieux, T. (2008). “Regression Discontinuity Designs: A Guide to Practice.” *Journal of Econometrics*. 148(1): 615-635.

Lee, D., and Lemieux, T. (2009). “Regression Discontinuity Design in Economics.” Working Paper 1473. Cambridge, United States: National Bureau of Economic Research.

Levy, D., and Ohls, J. (2007). “Evaluation of Jamaica’s PATH Program: Final Report.” Washington DC, United States: Mathematica Policy Research, Inc.

Lewis, Y. (2010). “Literacy in elementary school in Jamaica: the case of the grade four literacy test.” University of Iowa. Iowa Research Online. Available at: http://ir.uiowa.edu/etd/698.

Macours, K., and Vakis, R. (2008). “Changing Households’ Investment and Aspirations through Social Interactions: Evidence from a Randomized Transfer Program”. World Bank Policy Research Working Paper No. 5137. Washington DC, United States: The World Bank.

Miller, A. (2014). “Colonial Capital Theory at Work: The Case of Jamaica.” United Kingdom: Lexington Books.

Miller, E. (1999). “Educational Reform in Independent Jamaica.” In *Education Reform in the Commonwealth Caribbean*. Edited by Errol Miller. Washington DC, United States: Organization of American States. Available at: https://www.educoas.org/Portal/bdigital/contenido/interamer/BkIACD/Interamer/Interamerhtml/Millerhtml/mil_mil.htm

Ministry of Education of Jamaica. (2012). “National Education Strategic Plan: 2011-2020.” Available at: http://www.mona.uwi.edu/cop/sites/default/files/Jamaica_NESP_2011-2020.pdf

Ministry of Labour and Social Security – MLSS. (2012). Personal communication.

Molina-Millan, T., Barham, T., Macours, K., Maluccio, J. and Stampini, M. (2016). “Long-Term Impacts of Conditional Cash Transfers in Latin America: Review of the Evidence”. IDB Technical Note No. 923. Washington DC, USA: Inter-American Development Bank. Available at: https://publications.iadb.org/handle/11319/7891

Mullainathan, S., and Shafir, E. (2013). "Scarcity: Why Having Too Little Means So Much." *Time Books*. New York, United States: Henry Holt & Company LLC.

Paes-Sousa, R., Regalia, F., and Stampini, M. (2013). "Conditions for Success in Implementing CCT Programs: Lessons for Asia from Latin America and the Caribbean". IDB Policy Brief No. 192. Washington DC, USA: Inter-American Development Bank. Available at: https://publications.iadb.org/handle/11319/1487

Ray, D. (2006). "Aspirations, poverty, and economic change." In A. V. Banerjee, R. Bénabou, and D. Mookherjee (Eds.), *Understanding Poverty*. New York: Oxford University Press.

Saunders, A. *"Ministry scramble to find high school spaces for 'absent' GSAT students."* Jamaica Observer 18 June 2015. Available at: http://www.jamaicaobserver.com/news/Ministry-scramble-to-find-high-school-spaces-for--absent--GSAT-students_19150790.

Saavedra, J., and García, S. (2012). "Impacts of Conditional Cash Transfer Programs on Educational Outcomes in Developing Countries." RAND Corporation. Working Papers, (WR-921-1). Available at: http://www.rand.org/pubs/working_papers/WR921-1.html

Stampini, M., and Tornarolli, L. (2012). "The Growth of Conditional Cash Transfers in Latin America and the Caribbean: Did They Go Too Far?" IDB Policy Brief No. 185. Washington DC, USA: Inter-American Development Bank. Available at: https://publications.iadb.org/handle/11319/1448

Székely, M. (2015). "Expectativas Educativas: Una Herencia Intangible." México, El Motor Inmóvil? In R. Velez, J. Huerta, and R. Campos (Eds.), Centro de Estudios Espinosa Yglesias. 95-126. Available at: https://drive.google.com/file/d/0B6Mt1Wd2Z2kOQXFaSFkzbkVhQlU/view?pref=2&pli=1

UNESCO. (2014). "Education for All 2015 National Review Report: Jamaica." Available at: http://unesdoc.unesco.org/images/0023/002300/230020E.pdf

UNESCO. (2009). "Education Sector Plan 2009-2030." Available at: http://planipolis.iiep.unesco.org/upload/Jamaica/Jamaica_Vision_2030_Education_sector_plan.pdf

Annex 1. Sensitivity Analysis with Alternative Polynomial Specifications

Table 11 - Impact of PATH on Educational Aspirations, Measured by Previous Year Average of the GSAT Combined Score in the Five Preferred Schools: Fuzzy RDD, Point Estimators by Gender

Sample		Linear	Quadratic
Total	Take-up	-0.23	-1.37
		(0.98)	(0.60)
	Mean Dep. Var.	506.86	506.86
	SD Dep. Var.	29.54	29.54
	Observations not eligible	2,884	3,614
	Observations eligible	2,643	3,272
	Bandwidth	25.96	33.55
Female	Take-up	-3.01	-4.37
		(0.40)	(0.24)
	Mean Dep. Var.	512.42	512.42
	SD Dep. Var.	27.53	27.53
	Observations not eligible	1,286	1,821
	Observations eligible	1,230	1,671
	Bandwidth	22.81	33.26
Male	Take-up	1.06	1.30
		(0.71)	(0.71)
	Mean Dep. Var.	501.08	501.08
	SD Dep. Var.	30.44	30.44
	Observations not eligible	1,523	2,109
	Observations eligible	1,358	1,862
	Bandwidth	27.91	42.37

Source: Authors' calculations. Note: *rdrobust* command with linear and quadratic polynomial functions and triangular kernel using one common MSE-optimal bandwidth selector for the RDD treatment effect estimator. Standard errors clustered at the household level. P-values in parentheses, ** statistically significant at 5%, and * statistically significant at 10%. Results are consistent when estimated using covariates (not shown).

Table 12 – Impact of PATH on School Performance, Measured by the GSAT Combined Score: Fuzzy RDD, Point Estimators by Gender

Sample		Linear		Quadratic	
Total	Take-up	7.32		11.88	
		(0.11)		(0.11)	
	Mean Dep. Var.	449.61		449.61	
	SD Dep. Var.	60.9		60.9	
	Observations not eligible	2,501		3,411	
	Observations eligible	2,259		3,109	
	Bandwidth	22.50		32.21	
Female	Take-up	-2.34		0.67	
		(0.93)		(0.80)	
	Mean Dep. Var.	459.89		459.89	
	SD Dep. Var.	56.93		56.93	
	Observations not eligible	1,291		1,672	
	Observations eligible	1,221		1,559	
	Bandwidth	23.25		30.77	
Male	Take-up	16.03	**	23.57	**
		(0.03)		(0.02)	
	Mean Dep. Var.	438.95		438.95	
	SD Dep. Var.	63.01		63.01	
	Observations not eligible	1,323		1,864	
	Observations eligible	1,154		1,631	
	Bandwidth	24.16		36.26	

Source: Authors' calculations. Note: *rdrobust* command with linear and quadratic polynomial functions and triangular kernel using one common MSE-optimal bandwidth selector for the RDD treatment effect estimator. Standard errors clustered at the household level. P-values in parentheses, *** statistically significant at 1%, ** statistically significant at 5%, and * statistically significant at 10%. Results are consistent when estimated using covariates (not shown).

Table 13 - Impact of PATH on School Placement, Measured by Previous Year Average of the GSAT Combined Score in the Placement School: Fuzzy RDD, Point Estimators by Gender

Sample		Linear		Quadratic	
Total	Take-up	6.62		9.57	
		(0.12)		(0.17)	
	Mean Dep. Var.	454.21		454.21	
	SD Dep. Var.	53.84		53.84	
	Observations not eligible	2,530		3,432	
	Observations eligible	2,311		3,102	
	Bandwidth	23.16		32.47	
Female	Take-up	0.15		2.29	
		(0.84)		(0.74)	
	Mean Dep. Var.	458.88		458.88	
	SD Dep. Var.	53.43		53.43	
	Observations not eligible	1,255		1,664	
	Observations eligible	1,193		1,550	
	Bandwidth	22.88		30.99	
Male	Take-up	11.81	**	18.07	**
		(0.03)		(0.04)	
	Mean Dep. Var.	449.35		449.35	
	SD Dep. Var.	53.84		53.84	
	Observations not eligible	1,469		1,872	
	Observations eligible	1,286		1,644	
	Bandwidth	27.30		37.02	

Source: Authors' calculations. Note: *rdrobust* command with linear and quadratic polynomial functions and triangular kernel using one common MSE-optimal bandwidth selector for the RDD treatment effect estimator. Standard errors clustered at the household level. P-values in parentheses, *** statistically significant at 1%, ** statistically significant at 5%, and * statistically significant at 10%. Results are consistent when estimated using covariates (not shown).