WORKING PAPER Nº IDB-WP-1598

Can Conditional Cash Transfers Alter the Effectiveness of other Human Capital Development Policies?

Diether W. Beuermann
Andrea Ramos Bonilla
Marco Stampini

Inter-American Development Bank
Country Department Caribbean Group

April 2024

Can Conditional Cash Transfers Alter the Effectiveness of other Human Capital Development Policies?

Diether W. Beuermann
Andrea Ramos Bonilla
Marco Stampini

Inter-American Development Bank
Country Department Caribbean Group

April 2024

Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library

Beuermann, Diether.

Can conditional cash transfers alter the effectiveness of other human capital development policies? / Diether W. Beuermann, Andrea Ramos Bonilla, Marco Stampini.

p. cm. — (IDB Working Paper Series ; 1598)

Includes bibliographical references.

1. Cash transactions-Jamaica. 2. Transfer payments-Jamaica. 3. Income maintenance programs-Jamaica. 4. Education, Secondary-Jamaica. 5. School choice-Jamaica. 6. Academic achievement-Jamaica. 7.Human capital-Jamaica. I. Ramos Bonilla, Andrea. II. Stampini, Marco. III. Inter-American Development Bank. Country Department Caribbean Group. IV. Title. V. Series.

IDB-WP-1598

http://www.iadb.org

Abstract

Covering the full population of applicants to the Jamaican Conditional Cash Transfer Program (PATH), we explore whether receiving PATH since childhood altered the academic gains from attending a more preferred public secondary school. To uncover causal associations, we implement a double regression discontinuity design motivated by both the PATH eligibility criteria and the centralized allocation process to public secondary schools. Among girls, receiving PATH benefits did not influence the academic gains from attending a preferred school. However, boys exposed to PATH experienced significantly lower gains from preferred school attendance with respect to comparable peers who did not receive PATH. These results highlight the relevance of considering both the direct effects of conditional cash transfers and the potential indirect effects that such policies could convey through altering the effectiveness of other related policies. [1]

JEL Codes: H52, H75, I21, I26, I28, I38

Keywords: academic performance, conditional cash transfers, school selectivity, Jamaica.

[1] **Acknowledgements:** We are deeply grateful to Cynthia Hobbs, Sabine Rieble-Aubourg, and Orlando Patterson for their critical support in establishing the necessary contacts to assemble the data used in the study. We thank Elsa Marks-Willis from the Ministry of Labour and Social Security; Viviene Johnson, Maryah Ho-Young, Phillipa Livingston and Peter Gordon from the Ministry of Education, Youth, and Information; and Trudy Deans from the Office of the Prime Minister for facilitating access to the data needed for the study, their assistance, and their generosity. Francisco Pardo provided excellent research assistance. This paper benefited from comments by Orazio Attanasio, Cynthia Hobbs, Pablo Ibarrarán, C. Kirabo Jackson, Ofer Malamud, Ana Sofia Martinez, Costas Meghir, Miguel Sarzosa, Agustina Thailinger, and Diego Vera-Cossio. The content and findings of this paper reflect the opinions of the authors and not those of the Inter-American Development Bank, its Board of Directors, or the countries they represent.

1 Introduction

Conditional Cash Transfers (CCT) are fundamental components of social protection policies in Latin America and the Caribbean (Fiszbein and Schady, 2009; Stampini and Tornarolli, 2012; Paes-sousa et al., 2013; Ibarrarán et al., 2017). They also operate in several developing countries in Asia and Africa, as well as in some high income countries, including the United States. With their dual objective of (a) alleviating current poverty and (b) fostering demand for health and education services (through conditioning the monetary transfers to school attendance and regular medical screenings); they seek to alter households' incentives to increase human capital of children in ways that may improve long-term productivity. Therefore, by design, CCTs seek to foster the utilization of other public services related to human capital development and do not operate within a vacuum. Consequently, such overlap between CCTs and other human capital development interventions, could result in altering the effectiveness of the latter. In this study, we investigate this possibility by exploring whether benefiting from CCTs since childhood altered the academic gains from attending a more preferred secondary school in Jamaica.

An extensive body of research has analyzed the direct effects of CCTs on several short, medium and long-term outcomes. This evidence consistently documents desirable effects on poverty reduction, school enrollment and attendance to health services (Attanasio et al., 2011; Bastagli et al., 2016; Baird et al., 2013; de Walque et al., 2017; Schultz, 2004; Todd and Wolpin, 2006). Studies investigating CCT effects on school learning mostly suggest null impacts (Araujo et al., 2017; Baez and Camacho, 2011; Baird et al., 2014), with some exceptions that find modest positive impacts (Barham et al., 2013; Stampini et al., 2018). More recent studies explore CCT effects on longer-term educational attainment, labor market outcomes, and resilience against poverty among individuals who were treated during childhood (Araujo and Macours, 2021; Attanasio et al., 2021; Barham et al., 2017, 2018; Parker and Vogl, 2023; Molina Millán et al., 2020; Oconnor, 2024).[2]

A related strand of growing literature has engaged in experimenting with complementary interventions to potentially enhance the effectiveness of cash transfers. This Cash-Plus approach notes that the common conditionality of school attendance or regular healthcare visits embedded in CCTs, might not be enough to bolster early childhood development. This because many critical behaviors that mediate appropriate child development, like balanced and nutritious feeding or stimulation to promote socio-cognitive growth, are mostly dependent on (private) parental practices rather than on access to education and health services. The dominant approach in this important empirical work has been to design enhanced programs that combine cash with complementary interventions, and test their effectiveness with respect to the provision of cash alone. These complementary interventions have included nutritional supplementation, parenting programs, psychosocial stimulation on

[2] For a comprehensive review of CCT's long-term impacts, see Molina Millán et al. (2019).

cognitive development, among others (Arriagada et al., 2018, 2020; Little et al., 2021). While some of these studies have shown positive short run impacts (Behrman and Hoddinott, 2005; Macours et al., 2012), such gains have tended to dissipate over time (Attanasio et al., 2014). To potentially address this issue, more recent studies investigate the effectiveness of incorporating behavioral-based interventions aimed at promoting more sustainable behavioral changes of parents that could favor long-term human capital development of children (Akwii et al., 2018; Benhassine et al., 2012; Cohen et al., 2017; Datta et al., 2021; Sedlmayr et al., 2018).

Therefore, the focus of the previous strands of CCT related literature has been either to (a) investigate the direct effects of CCTs on several outcomes over the short, medium and long-run; or (b) experiment with complementary interventions aimed at increasing the effectiveness of cash alone within a Cash-Plus framework. We complement these important studies investigating whether CCT participation since childhood altered the effectiveness of a subsequent (and already existing) human capital development policy. Although related to the Cash-Plus literature in the sense that our study focuses on potential interaction effects between different interventions, our question does not target the design of an intervention aimed at enhancing the effectiveness of cash transfers. Rather, we ask whether CCT participation could alter the effectiveness of other, already institutionalized, human capital development policy.[3] More generally, our work is also related to studies that have examined interaction effects between different exogenous shocks or interventions (Adhvaryu et al., 2020; Bhalotra and Venkataramani, 2015; Gilraine, 2018; Goff et al., 2023; Johnson and Jackson, 2019; Rossin-Slater and Wust, 2020; Mbiti et al., 2019). Therefore, we contribute to this literature by providing the first evidence of whether CCTs can affect the effectiveness of other human capital development policy – accessibility to a more preferred secondary school.

Investigating these issues is challenging as it requires credible causal identification strategies for both participation in CCTs and preferred school attendance. We circumvent these challenges exploiting the Jamaican institutional setting regarding (a) the eligibility for the national CCT Programme of Advancement through Health and Education (PATH), and (b) the admission process to public secondary schools. Identification of PATH effects derives from the fact that household eligibility is determined by a strict cutoff on a poverty proxy means test score which we exploit within a regression discontinuity design (RDD). Identification of preferred school effects derive from the centralized assignment process to public secondary schools. Within this process, students are assigned to schools based on their performance in a national standardized examination and their school preferences. This creates a test-score cutoff for each school, above which students are admitted and below which they are not. This setting also allows the implementation of a RDD to

[3]These potential interactions between different policies are also related to the hypothesis that skills might beget skills (Cunha and Heckman, 2007). Under this hypothesis, for example, beneficiaries of CCTs during childhood who might have experienced positive effects on early learning would benefit more from later education-related interventions (i.e., positive complementarities).

identify the causal effects of attending a more preferred secondary school. Finally, we combine both RDDs within a double regression discontinuity design (DRD) to identify potential interaction effects between both policies.

To measure these effects, we assembled a unique longitudinal database tracking the outcomes of interest of the relevant Jamaican population over time. We used the PATH administrative registry covering the full population of applicants to the program between its inception (in 2001) and 2013. We then matched this data, at the individual level, with the primary level national examinations that determine secondary school placements. Subsequently, we matched these registries with end of secondary and post-secondary high stakes examinations independently administered by the Caribbean Examinations Council. Therefore, the resulting matched database traces the full population of PATH applicants over primary, secondary, and up to post-secondary studies. Coupled with the DRD causal identification strategy, our data allows investigating potential interaction effects between PATH and preferred schools on academic performance at scale.

For both girls and boys, we document null direct effects of PATH on learning at the end of secondary school or on post-secondary certifications. We also find that both girls and boys significantly benefit from attending more preferred schools. For girls, the gains from preferred school attendance are similar for those who benefited from PATH and comparable peers who did not receive PATH. However, among boys, the gains from preferred school attendance are significantly lower for those who were PATH beneficiaries with respect to comparable counterparts who did not benefit from PATH. Overall, our findings highlight the relevance of considering not only the direct effects of CCTs, but also potential indirect effects that could operate through altering the effectiveness of other interventions.

We, therefore, contribute to the body of knowledge regarding how different interventions interact to impact outcomes with the added value of investigating such question at scale exploiting nationally institutionalized policies and full-population administrative data. The remainder of the paper is organized as follows. Section 2 summarizes the Jamaican context. Section 3 presents the data and summary statistics. Section 4 lays out the empirical strategy used to isolate the causal effects of both interventions and their interactions. Section 5 presents our results and their discussion. Section 6 concludes.

2 The Jamaican Context

2.1 The Programme of Advancement through Health and Education (PATH)

PATH targets households in the bottom two quintiles of the income distribution. I encompasses a dual aim of alleviating current poverty (through income support) and developing children's human capital (through health and education conditionalities). It was launched in 2001 with a one-year pilot in the parish of St. Catherine, after which it was rolled out to the entire country.[4] To date, it has about 350,000 beneficiary households.

Enrollment is demand driven. A household representative starts the application process at a Parish Office, where s/he completes a socioeconomic form. This information is used to compute a poverty proxy means test (PMT) score with a formula that is unknown to applicants.[5] Applicant households with a score under the predetermined eligibility threshold are declared eligible. Applicants exceeding the threshold by less than five points enter an automatic appeal process. Although applicants exceeding the threshold by more than five points do not enter an automatic appeal, they can appeal on their own initiative. A social worker visits appealing households and collects updated information that is used to calculate a rectified PMT score. For all our empirical work, we use the initial formula-based score as the running variable for PATH eligibility as it is calculated homogeneously for all applicants.[6]

Once a family is declared eligible, it starts receiving transfers, which are paid every two months. The education transfer is conditioned on children attending at least 85% percent of school days. Compliance is verified through information provided by the schools to the program (Levy and Ohls, 2010).[7] The transfer is granted to each eligible child until the completion of secondary school, and the amount differs by the age of the child. The amounts have varied over time to account for inflation, and between 2012 and 2015 they also varied by the gender of the child (with boys receiving transfers 10 percent higher than girls). On average, the per-child monthly transfer has represented about 9 percent of the prevailing monthly full-time minimum wage.[8]

[4] A parish is a geopolitical area that has its own local government arrangements. Jamaica is divided into 14 parishes.

[5] The research team was granted access to the PATH applications databases and the PMT score of each applicant. However, the formula used to obtain the score was not revealed.

[6] For our empirical work, we observe PATH applications with the initial PMT score for the 2001-2013 period. However, we only observe both the initial and the rectified PMT score for the 2009-2013 period. During this period, out of the 12,819 applicant households, 808 (or 6.3% of all applicants) went through a rectification process and, of those, only 288 (or 2.25% of all applicants) ended up qualifying to receive PATH benefits.

[7] The share of eligible households that did not receive the transfers within our study period accounts for 5.8%. Administratively, the main reason for which eligible households could have not received transfers resides in not complying with the conditionality.

[8] In 2006, the value of the education transfer amounted to J$ 600 (about US$ 10) per month per child. Later, the value was differentiated by grade and sex of the beneficiary. For example, in 2012, girls in grades 1–6 (primary), 7–9 (lower secondary) and 10–13 (upper secondary) received J$ 750 (US$ 8.4), J$ 975 (US$ 11) and J$ 1150 (US$ 12.9) per month, respectively; the transfers for boys were 10% higher, at J$ 825 (US$ 9.3), J$ 1075 (US$ 12.1) and J$ 1265 (US$ 14.2) per month, respectively (in 2012, J$ 88.99 = US$ 1). In 2015, the differentiation by sex was removed; the education transfer amounted to J$ 1045 (US$ 9), J$ 1400 (US$ 12) and J$ 1600 (US$ 13.8) for primary, lower secondary and upper secondary children, respectively, irrespective of their sex (in 2015, the average exchange rate was J$ 116.28 = US$ 1). In 2017, the education transfer amounted to J$ 1350 (US$ 10.5), J$ 1800 (US$ 14) and J$ 2100 (US$ 16.4) for primary, lower secondary and upper secondary children, respectively, irrespective of their sex (in 2017, the average exchange rate was J$ 128.30 = US$ 1).

2.2 The Education System

At the end of primary school, students register to take the Grade Six Achievement Test (GSAT) and, at registration, provide a list of ranked secondary school choices to the Ministry of Education, Youth, and Information (MOEY). Between 2003 and 2004, students could rank up to three school choices. Between 2005 and 2015, students could rank up to five school choices. The GSAT is comprised of five subjects that all students take: mathematics, science, language arts, social studies, and communication tasks. Based on the GSAT performance and the school choices, the MOEY assigns students to schools using a serial dictatorship algorithm (Gale and Shapley, 1962; Abdulkadiroglu and Sonmez, 1998).[9] This algorithm ranks students by their GSAT overall score and gender. No other criteria are used (e.g., sibling preferences or geographic proximity). Individual school capacity by gender is predetermined. The algorithm assigns the highest-ranked student to her first choice. It then moves on to the second and treats her similarly. The procedure continues until it reaches a student whose first choice is full. At that point, it tries to assign the student to her second choice. If full, to the third choice and so on. Once this student has been assigned to a school, the algorithm moves on to the next person.

When choices are unlimited, the assignment mechanism delivers an equilibrium of strategy-proof submission of the full preferences. That is, the top ranked choice is the most preferred option of all options, the second is the second preferred, and so on (Abdulkadiroglu and Sonmez, 1998). However, when the number of school choices is constrained (as in our setting), the submitted set of choices may be strategic such that the top-ranked school listed may not be the most preferred of all options, and the second may not be the second preferred, and so on. Indeed, students may have an incentive to exclude some desirable schools from their list if the probability of admission is too low (Haeringer and Klijn, 2009). Nonetheless, among the set of schools listed, it is a dominant strategy to list them in order of true preference (Roth and Oliveira Sotomayor, 1990). Accordingly, so long as parents make rational choices, one can infer that a higher-ranked school is preferred to a lower-ranked school. As will be shown in Section 3, parents consistently rank schools with higher average incoming GSAT scores higher. As the assignment mechanism determines that highest-achieving students are admitted to their top choices first, a preferred school is virtually synonymous with being more selective or more academically elite.

All secondary schools teach a homogeneous national curriculum. Secondary school begins in first form (the equivalent of 7th grade) and ends at fifth form (the equivalent of 11th grade) when students take the Caribbean Secondary Education Certification (CSEC) examinations. These are equivalent to the British Ordinary levels examinations and are externally graded by the Caribbean Examinations Council (CXC). The CSEC examinations are given in 37 subjects. Passing five sub-

[9]There were 468 public secondary schools to which students were assigned during our study period.

jects (including English and mathematics) is a sufficient entry requirement for community colleges, technical schools, or training schools. It can also be used for entry at some colleges in the United States. Students who complete these requirements could either continue their studies at a tertiary institution (if accepted) or pursue the Caribbean Advanced Proficiency Examination (CAPE), also externally graded by CXC.

The CAPE is a tertiary-level program. Students seeking to attend university (as opposed to a community college) take the CAPE. The CAPE is equivalent to the British Advanced levels examinations. The CAPE is a two-year program and includes two core units (Caribbean and Communication Studies) and six other units. Passing six CAPE units is a common admission requirement to British higher education institutions. The post-secondary qualification of a CAPE Associate Degree is awarded after passing seven CAPE units (including the core units).

3 The Data and Summary Statistics

We observe the full population of households who applied to the PATH between its inception in 2001 and 2013. Among these, we focus on those households with at least one member who: (i) was younger than 11 at the time of application (as students typically take the GSAT and enroll in secondary school at 11–12 years old); and (ii) belongs to year of birth cohorts that allow sufficient time to reach the age of CSEC/CAPE taking by 2020 (which is the most recent data available). This because we seek to study potential effects of PATH and subsequent attendance to preferred secondary schools on CSEC/CAPE outcomes. As students enroll in secondary school at 11–12 years old, our relevant sample includes those who were below this age threshold at PATH application and with enough age to observe the outcomes of interest within our data. This delivers 280,888 individual-level observations.

We then merged the PATH data with the official administrative GSAT data from 2003 until 2015. In the absence of individual identifiers, the data were linked by full name, gender, and date of birth. We matched 78.4% of PATH applicants to the GSAT records. This closely mimics the 78.7% official statistic of school age children enrolled in primary school.[10] This suggests that our match rate is not an artifact of our methodology but reflects the true primary school enrollment rate.[11] The matched data comprise 220,092 individual-level observations of which 113,140 are girls and 106,952 are boys.[12] These data include the parish of residence, the gender and educational attainment of the adult who filed the PATH application, household per capita income, home

[10] Source: World Development Indicators Database (https://databank.worldbank.org/source/world-development-indicators). Country: Jamaica. Year: 2013.

[11] As an additional check, we computed the ratio of the total number of individuals in the GSAT database aged 10–12 by the census date (April 4th, 2011) divided by the population aged 10–12 counted in the census. This exercise delivers an implied GSAT taking rate of 80.2% which is in line with our PATH-GSAT match rate.

[12] See Appendix Table A.1 for a sample breakdown by year of birth and PATH application year.

ownership status, household size, the PMT score, the PMT eligibility cutoff, whether the household actually received PATH benefits, the individual-level GSAT performance and the ranked list of secondary schools the student wished to attend.

To track the outcomes of interest, we collected population data on the CSEC examinations between 2005 and 2020; as well as population data on the CAPE examinations between 2009 and 2020. Both the CSEC and CAPE data contain scores for each subject examination taken. The CSEC and CAPE data were linked at the individual level to the GSAT data.[13] Notice that since the CAPE is completed seven years after the GSAT and the most recent CAPE data is 2020, then the last relevant GSAT cohort for these outcomes is 2013. By similar logic, since the CSEC is completed 5 years after GSAT and the most recent CSEC data is 2020, then the last relevant GSAT cohort for these outcomes is 2015.

Table 1 reports summary statistics. The average individual was 80 months old at the time of PATH application. About 86% of household representatives applying to the program were female; 38% of them had completed secondary education. Household weekly income per capita was about PPP US$ 25.7 (equivalent to 23% of the prevailing weekly full-time minimum wage).[14] About 42% report owning the dwelling; households had on average 5.7 members. About half of the applicants ended up receiving PATH benefits.

The average student took the GSAT at 143 months of age. Girls score about 0.46 sd higher than boys in the GSAT and attend more selective schools, with 0.27 sd higher in incoming peer GSAT scores than those attended by the average boy. We measure the selectivity of school choices by computing the average GSAT standardized score of students assigned to each school choice. While both girls and boys consistently rank more selective schools higher, the choices of girls are always relatively more selective than those of boys. About 40-45% were assigned to one of their first three school choices.

About 53% of boys took at least one CSEC subject compared to about 73% of girls. Similarly, while 27.4% of girls qualified for tertiary education based on CSEC performance (i.e., passing at least five subjects including English and math), only 14.9% of boys achieved the same. Post-secondary outcomes confirm this pattern; 16.9% of girls took the CAPE, against 9% of boys. CAPE success also favors girls, with 7% of them earning an Associate Degree, against 3.2% of boys.[15]

Households who were classified as eligible for PATH show, on average, less educated household

[13]The full population GSAT data was linked to the CSEC/CAPE data by full name, gender, and date of birth. 92% of CSEC and 96% of CAPE observations were matched to the GSAT data. The 4-8% of unmatched observations closely mimics the 6% enrollment rate in private secondary schools that would not have taken the GSAT.

[14]Monetary figures expressed in real 2019 U.S. dollars in purchasing power parity (PPP).

[15]Our analyses focus on PATH applicants. However, we possess the full population GSAT, CSEC and CAPE data. In Appendix Table A.2 we show how PATH applicants differ from non-applicants confirming that applicants constitute a relatively underprivileged segment of the population.

heads than those resulting ineligible (28% compared to 49% with at least secondary education). They also live with more members (7 compared to 4) and their kids show lower GSAT, CSEC and CAPE performance (Appendix Table A.3). With respect to school assignments, there were no average baseline household level differences between students assigned to their top choices and those assigned to less preferred schools. Nonetheless, those assigned to their top choices show better average outcomes in terms of CSEC and CAPE performance (Appendix Table A.4).

4 Empirical Strategy

4.1 The Impact of Preferred Secondary Schools

The centralized school assignment mechanism creates a test score cutoff above which applicants to each school are admitted and below which they are not. Since parents list their school choices before students sit the GSAT and the cutoffs are a function of the (unknown) national distributions of GSAT scores and school choices, cutoffs are very difficult to game. If nothing else differs among those scoring just above and just below the cutoff, any sudden change in outcomes as students' GSAT score goes from below to above the cutoff for a preferred school can be attributed to attending that preferred school (Hahn et al., 2001). Therefore, one can exploit the discontinuity in the likelihood of admission through the cutoff by estimating the following two-stage least-squares (2SLS) model:

$$Attend_{ijt} = \delta \cdot Above_{ijt} + g_1(GSAT_{ijt}) + \mathbf{X'_{it}}\omega_\mathbf{1} + C_{1,jt} + \varepsilon_{1,ijt} \tag{1}$$

$$Y_{it} = \theta \cdot \hat{Attend}_{ijt} + g_2(GSAT_{ijt}) + \mathbf{X'_{it}}\omega_\mathbf{2} + C_{2,jt} + \varepsilon_{2,ijt} \tag{2}$$

The first stage (1) predicts whether individual i who belongs to GSAT cohort t attended school j, $Attend_{ijt}$, as a function of scoring above the cutoff for preferred school j within GSAT cohort t, $Above_{ijt}$, and controls.[16] To account for latent outcomes that vary smoothly through the cutoffs, the model controls for a smooth function of the GSAT score (relative to each school cutoff j) fully interacted with the $Above_{ijt}$ indicator, $g_1(GSAT_{ijt})$. We also control for all socioeconomic characteristics collected at PATH application (included in $\mathbf{X_{it}}$).[17] Following Jackson (2010) and Pop-Eleches and Urquiola (2013), we stack the data across all schools' application pools into a single cutoff, recenter GSAT scores at each respective cutoff, and include cutoff fixed effects ($C_{1,jt}$). The cutoff fixed effects ensure that all comparisons are among students who applied to the same school in the same year.[18] In the second stage (2), the outcome of interest (Y_{it}) is a function

[16] We code the attended school as the one in which the student was enrolled in the last year (i.e., fifth year) of secondary studies. For those who leave school early, we use the MOEY administrative school assignment.

[17] These include parish of residency fixed effects, gender, education of the household member who filed the PATH application, household per capita income, home ownership status, and household size.

[18] Each student appears in all the cutoffs associated with schools to which she applied. For example, consider a

of predicted preferred school attendance and all controls from Equation (1). The second stage excluded instrument is $Above_{ijt}$. Because the same individual can enter the stacked database for more than one cutoff, the estimated standard errors are clustered at the individual level.[19] In this context, estimates of θ yield the causal effect of attending a preferred secondary school on the outcomes of interest.

The key identifying assumption of this RDD model is that conditional on GSAT scores and school choices, nothing other than the likelihood of preferred school attendance changes discontinuously at the cutoff. Given that previous literature documents important gender heterogeneity of preferred school effects (Jackson, 2010; Beuermann and Jackson, 2022), our analysis will explore effects by gender and, therefore, we also show that the identification assumptions hold for both the female and male samples. The first stage, graphically shown in Panel A of Figure 1, portraits that scoring above the cutoff sharply increases the likelihood of attending a preferred school. Panel A of Table 2 displays estimates on the $Above_{ijt}$ indicator from the first stage equation (1). Scoring above a cutoff increases the likelihood of preferred school attendance by 30-41 percentage points with a strong F-Statistic on the excluded instrument of 1,800.

We now proceed to show that other factors that might be systematically related to the outcomes of interest remain smooth through the cutoff (i.e., valid exclusion restriction of the $Above_{ijt}$ instrument). First, we show that the baseline socioeconomic composition of households remains smooth through the preferred school cutoffs. We follow Kling et al. (2007) and compute a baseline socioeconomic standardized index defined as the equally weighted average of the z-scores of all available socioeconomic variables reported at PATH application.[20] We then estimate reduced-form models as in equation (1) with the baseline socioeconomic index as dependent variable.[21] If our identification assumptions hold, we should not observe discernible relations between the excluded instrument, $Above_{ijt}$, and the socioeconomic index. That is, estimates of δ, should be indistinguishable from zero. Panel B of Table 2 displays these estimates which are small in magnitude and indistinguishable from zero. Second, we follow McCrary (2008) and test for potential gaming of the school cutoffs assessing the presence of discontinuities in densities through the school admission cutoffs.

student who submitted 4 school choices and was admitted to her 3rd choice. This student appears 3 times in the stacked database: 2 times as an unsuccessful applicant (i.e., scoring below the cutoff) for her first 2 choices and 1 time as a successful applicant (i.e., scoring above the cutoff) for her 3rd choice. The average student appears in 3 cutoffs (equivalent between females and males).

[19] In our context, this approach is equivalent to heteroskedasticity-robust estimated standard errors allowing for off-diagonal non-zero terms in the variance-covariance matrix when the same individual enters the data for more than one cutoff. Kolesár and Rothe (2018) show this to be a more conservative approach than also clustering estimated standard errors at the level of the running variable, $GSAT_{ijt}$.

[20] These include parish of residency, gender of the household head, education of the household head, household per capita income, home ownership status, and household size.

[21] In this regression, we do not control for the baseline characteristics ($\mathbf{X_{it}}$) as these are included in the socioeconomic index.

Panel C of Table 2 shows no evidence of density discontinuities through the cutoffs. All these tests suggest that the RDD identification strategy for preferred school effects is valid.

4.2 The Impact of PATH

PATH eligibility depends on whether the household's PMT score lies across a fixed threshold unknown to applicants. If nothing else differs among households scoring just above and below the eligibility threshold, any sudden change in outcomes through the threshold can be attributed to the PATH (Hahn et al., 2001). Therefore, one can exploit the discontinuity in the likelihood of being a PATH beneficiary through the threshold by estimating the following two-stage least-squares (2SLS) model:

$$PATH_{it} = \lambda \cdot AboveP_{it} + f_1(Score_{it}) + \mathbf{X'_{it}}\vartheta_\mathbf{1} + C_{1,jt} + \varepsilon_{1,ijt} \tag{3}$$

$$Y_{it} = \beta \cdot PA\hat{T}H_{it} + f_2(Score_{it}) + \mathbf{X'_{it}}\vartheta_\mathbf{2} + C_{2,jt} + \varepsilon_{2,ijt} \tag{4}$$

In the first stage (3) the model predicts whether individual i who applied for PATH benefits in year t actually received them, $PATH_{it}$, as a function of scoring above the PATH eligibility threshold, $AboveP_{it}$, and controls.[22] To account for latent outcomes that vary smoothly through the thresholds, the model controls for a smooth function of the PATH score (net of the threshold) fully interacted with the $AboveP_{it}$ indicator, $f_1(Score_{it})$. Subsequently, the model controls for the baseline socioeconomic characteristics ($\mathbf{X_{it}}$) and the cutoff fixed effects ($C_{1,jt}$) as previously defined in Section 4.1.[23] In the second stage (4), the outcome of interest (Y_{it}) is a function of the predicted reception of PATH benefits and all controls from Equation (3). The second stage excluded instrument is $AboveP_{it}$. In this context, estimates of β yield the causal effect of receiving PATH benefits on the outcomes of interest.

The key identifying assumption in this RDD model is that nothing other than the likelihood of PATH reception changes in a discontinuous manner through the eligibility threshold. We test this assumption in several ways. It is first shown that the likelihood of receiving PATH benefits discontinuously changes through the PATH eligibility threshold (Panel B of Figure 1). The first stage estimates on the $AboveP_{it}$ indicator from equation (3) are reported in Panel A of Table 3. Being just above the eligibility threshold increases the likelihood of receiving PATH benefits by 82-84 percentage points with a strong F-Statistic on the excluded instrument of 27,340. By contrast, the socioeconomic composition of households remains smooth through the PATH cutoff. Indeed, when estimating a reduced-form model as in (3) with the baseline socioeconomic index as the

[22]Notice that we use the negative of the PATH PMT score and thresholds in all our specifications.

[23]Notice that this model is defined over the same stacked database described in Section 4.1. Therefore, it includes cutoff fixed effects and estimated standard errors are clustered at the individual level.

dependent variable, estimates on the $AboveP_{it}$ indicator are both economically and statistically indistinguishable from zero (Panel B of Table 3).[24] In addition, we follow McCrary (2008) and test for a discontinuity in density through the eligibility threshold and find no discontinuities (Panel C of Table 3). These tests suggest that our RDD strategy to estimate PATH effects is valid.

4.3 Interactions between PATH and Preferred Secondary Schools

To estimate potential interaction effects between PATH and preferred secondary schools, we combine both RDD models outlined above within a double regression discontinuity design (DRD). To pursue this approach, we exploit the fact that, at each school cutoff, we observe individuals who were marginally eligible and ineligible for PATH benefits. This allows the estimation of preferred school effects among PATH beneficiaries and also among comparable non-beneficiaries. Therefore, the DRD model will allow testing if the effectiveness of preferred schools differed between PATH beneficiaries and non-beneficiaries. To see this, consider the second stage equation of the DRD model:

$$\begin{aligned} Y_{it} = \beta_1 \cdot \widehat{PATH}_{it} + \theta_1 \cdot \widehat{Attend}_{ijt} + \tau \cdot \widehat{PATH_{it} \cdot Attend_{ijt}} + f(Score_{it}) + g(GSAT_{ijt}) \\ + f(Score_{it}) \cdot g(GSAT_{ijt}) + \mathbf{X'_{it}}\vartheta + C_{jt} + \varepsilon_{ijt} \end{aligned} \tag{5}$$

Similar to the individual RDD models, the DRD second stage equation in (5) instruments PATH reception ($PATH_{it}$) with an indicator for being above the eligibility threshold ($AboveP_{it}$), and instruments preferred school attendance ($Attend_{ijt}$) with scoring above the admission cutoff ($Above_{ijt}$). In addition, following Jackson (2021), we instrument the interaction term ($PATH_{it} \cdot Attend_{ijt}$) with the interaction of both instruments ($AboveP_{it} \cdot Above_{ijt}$). Accordingly, the DRD model possesses three first stage equations of the following form:

$$\begin{aligned} \mathbf{ENDOG_{ijt}} = \pi \cdot AboveP_{it} + \phi \cdot Above_{ijt} + \varphi \cdot AboveP_{it} \cdot Above_{ijt} + f(Score_{it}) + g(GSAT_{ijt}) \\ + f(Score_{it}) \cdot g(GSAT_{ijt}) + \mathbf{X'_{it}}\vartheta + C_{jt} + \varepsilon_{ijt} \end{aligned} \tag{6}$$

where vector $\mathbf{ENDOG_{ijt}}$ includes the three instrumented variables: $PATH_{it}$, $Attend_{ijt}$, and $PATH_{it} \cdot Attend_{ijt}$.[25]

Within the framework of the second stage equation (5), estimates of β_1 denote the direct effect of PATH on the outcomes of interest. Estimates of θ_1 denote the effect of attending a preferred secondary school among PATH non-beneficiaries. Furthermore, because the DRD model includes the estimated effect of attending a preferred school for both PATH non-beneficiaries and beneficia-

[24] In this regression, we do not control for the baseline characteristics ($\mathbf{X_{it}}$) as these are included in the socioeconomic index.

[25] For all main results, we estimate the DRD model with all available observations, and consider 3rd-order polynomials for both $f(Score_{it})$ and $g(GSAT_{ijt})$. However, as we show in Section 5.4, our results are robust to alternative polynomial orders and when computing optimal bandwidths according to Calonico et al. (2017).

ries, the coefficient on the instrumented interaction, $PATH_{it} \cdot \hat{Attend}_{ijt}$, identifies the parameter of interest τ – the causal effect of the *change* in the effectiveness of preferred schools between PATH beneficiaries and comparable non-beneficiaries. That is, a positive τ coefficient would denote the additional benefit that PATH beneficiaries could expect from attending a preferred school with respect to comparable non-beneficiaries. Conversely, a negative τ coefficient would represent the diminished benefit from attending a preferred school that PATH beneficiaries could expect with respect to comparable non-beneficiaries. Consequently, the effect of attending a preferred secondary school among PATH beneficiaries is given by adding: $\theta_1 + \tau$.

Our main question is whether the effectiveness of preferred schools differs between PATH beneficiaries and comparable non-beneficiaries. This question, therefore, translates into testing the following null hypothesis:

$$\underbrace{\theta_1}_{\text{Preferred School Effect among PATH Non-Beneficiaries}} = \underbrace{\theta_1 + \tau}_{\text{Preferred School Effect among PATH Beneficiaries}} \tag{7}$$

Failing to reject this null hypothesis, would imply that both PATH non-beneficiaries and beneficiaries experienced the same academic returns from attending a preferred school. Nonetheless, a finding of $\theta_1 + \tau > \theta_1$, would imply that PATH enhanced the effectiveness of preferred schools. While a finding of $\theta_1 + \tau < \theta_1$, would imply that school effectiveness was eroded among PATH beneficiaries.

While the validities of the RDD models for the identification of PATH and preferred school effects separately have been already shown, we now show that the combined DRD model is also valid. One key threat to the validity of the DRD model follows from the possibility that qualifying for PATH might have affected the school choices considered by parents (e.g., by affecting aspirations, or what parents can afford) or the GSAT performance of students. In such a case, the exclusion restriction of the DRD model would be violated. This is because $AboveP_{it}$, which is the excluded instrument for $PATH_{it}$, would be systematically correlated with the running variable of the preferred school treatment (i.e., $GSAT_{ijt}$) and the school cutoff fixed effects (i.e., C_{jt}). That is $AboveP_{it}$ would no longer be affecting the outcomes only through the $PATH_{it}$ intervention, but would also be affecting the outcomes through other components of the equation (5). As the instrument for the interaction effect (i.e., $AboveP_{it} \cdot Above_{ijt}$) also includes $AboveP_{it}$, such occurrence would render the estimate of the key interaction effect (τ) biased in an unknown magnitude and direction. Therefore, the validity of the DRD model requires an orthogonal relation between the excluded instrument for PATH reception (i.e., $AboveP_{it}$), school choices, GSAT performance, and school placements.

Accordingly, we directly test for the orthogonality between $AboveP_{it}$ and GSAT performance, school choices, and school assignments. To this end, we estimate reduced-form models as equation (3) with GSAT performance, school choices, school assignments, and peer academic quality as dependent variables. We report the estimates on the $AboveP_{it}$ indicator in Panel A of Table 4. Estimates show no relation between $AboveP_{it}$ and GSAT performance, the selectivity of school choices (as measured by the average GSAT score of students assigned to each school), the final school placements, and the peer academic quality of the attended school. Following this evidence, we also show that PATH reception had no effect on these measures. To do so, we estimate the full PATH discontinuity model of equations (3)-(4) with GSAT performance, school choices, and school assignments as the outcomes of interest. Panel B of Table 4 shows estimated PATH 2SLS effects (i.e., estimates of β from equation (4)) evidencing null impacts on GSAT performance, the selectivity of school choices, the school placements, and the peer academic quality of the attended school.[26] Overall, the evidence shows that the DRD model is not biased due to the possibility of PATH affecting GSAT performance or school choices.

We now proceed to evidence the robustness of the DRD model's first stages and the joint validity of the excluded instruments. Panel A of Table 5 displays the first stage estimates resulting from the estimating equation (6) for each of the instrumented variables. For each instrumented variable, we show the estimated coefficient on its excluded instrument. Each excluded instrument is strongly related to its instrumented variable with a strong F-Statistic on the excluded instruments of 2,100. Finally, to show that the combined exclusion restriction of the instruments likely holds, we estimate equation (6) with the baseline socioeconomic index as the dependent variable.[27] Panel B of Table 5 displays the estimated coefficients on the two instruments and their interaction suggesting no relation with baseline characteristics. The economically and statistically insignificant estimated coefficients on the interaction of both instruments ($AboveP_{it} \cdot Above_{ijt}$) rule out the possibility that PATH changed the type of people that scored above the preferred school cutoff. This shows that the DRD strategy identifies valid counterfactual groups to estimate causal and comparable preferred school effects for both PATH beneficiaries and non-beneficiaries. Overall, all these tests provide evidence that the proposed DRD model is valid to estimate potential interaction effects between PATH and preferred schools.

[26] Focusing on a reduced sample of PATH applicants during the period 2007-08 within urban areas of 10 (out of 14) parishes, Stampini et al. (2018) finds that PATH increased GSAT performance of boys by 5.1%. In our case, when focusing on the full population of PATH applicants between 2001 and 2013, we find no discernible relation between PATH and GSAT performance. Consistent with Stampini et al. (2018), we also find no relation between PATH and educational aspirations as measured by the selectivity of school choices.

[27] In this regression, we do not control for the baseline characteristics ($\mathbf{X_{it}}$) as these are included in the socioeconomic index.

5 Results and Discussion

5.1 Characteristics of Preferred Secondary Schools

We begin by documenting the consequences of attending a preferred secondary school on the learning environment to which pupils are exposed. We estimate the 2SLS model (1)-(2) with available characteristics of attended schools as dependent variables. Table 6 reports estimates of the θ parameter from equation (2) for both girls and boys.

Attending a preferred school increases peer GSAT quality by 0.54 (0.41) sd for boys (girls). This is roughly the difference in average school selectivity between the top and the fourth school choice. Preferred school attendance also leads to more academically homogeneous cohorts (as evidenced by the reduced incoming GSAT score gap among admitted students to each school). More academically able peers within more homogeneous groups have been shown to favor learning (Duflo et al., 2011; Jackson, 2010; Pop-Eleches and Urquiola, 2013). Preferred schools appear to be more diverse, as evidenced by the reduced Herfindahl–Hirschman Index (HHI) computed using the shares of parishes of origin among students within each school.

Using the 2009 Teacher Census, we computed the proportion of teachers who hold a university degree within each school. Attending a preferred school increases the exposure of students to teachers with university degrees. We also extracted school-level information from the School Inspection Reports conducted by the National Education Inspectorate which covered 364 secondary schools between 2010 and 2015.[28] These reports provide information on pupil-teacher ratios and yearly average student attendance rates. They also deliver school ratings based on several dimensions of school management, as well as academic and nonacademic performance of students.[29] Attending a preferred school is significantly associated with lower pupil-teacher ratios, higher attendance rates, and improved overall school ratings.[30] These characteristics are also consistent with environments that favor learning outcomes (Glewwe et al., 2021).

5.2 Does PATH influence the Effectiveness of Preferred Secondary Schools?

We now explore our main question: whether participating in PATH affected the causal effects of preferred secondary school attendance. We investigate this question focusing on two main CSEC

[28] These reports can be accessed at: https://www.nei.org.jm/Inspection-Findings/School-Reports

[29] These dimensions include: (1) leadership and management; (2) teaching in support of student learning; (3) students' performance in English and math; (4) students' personal and social development; (5) use of human and material resources; (6) curriculum and enhancement programs; and (7) provisions for safety, security, health, and well-being. Each of these dimensions were rated on a scale ranging from 1 (extremely poor) to 5 (exceptionally high). The overall effectiveness rating is a composite of all the measured dimensions which also ranges between 1 and 5. We, therefore, compute a normalized effectiveness index for each school j which ranges between 0 and 1 as follows: $(EffectivenessRating_j$ - 1)/(5 - 1).

[30] Appendix Table A.5 reports preferred school effects on each individually rated dimension evidencing positive impacts on all of them.

outcomes and two main CAPE outcomes.[31] We begin assessing the effects on an indicator for whether the student took at least one CSEC subject. This proxies for secondary school completion. We then evaluate effects on an indicator for whether the student qualified for tertiary education based on CSEC performance (i.e., passing at least five subjects including the core examinations of math and English).[32] We then proceed to evaluate the effects on an indicator for whether the student took at least one CAPE unit (which proxies for post-secondary school attendance), and on an indicator for whether a CAPE associate's degree was earned.[33]

We begin exploring the direct effects of PATH. These are captured by estimates of β_1 from equation (5). Table 7 (column 1) displays these estimates. Overall, we observe null direct PATH effects among girls and boys. One exception is a positive effect on the likelihood of taking at least one CSEC subject among boys equivalent to 2.54 percentage points ($p-value$<0.05). This reflects a 4.75% increase with respect to the average CSEC taking rate of 53.4% among boys. While this outcome does not measure a learning effect, it serves as a proxy for secondary school completion and is consistent with previous evidence suggesting positive effects of CCTs on secondary graduation rates (Baez and Camacho, 2011; Araujo et al., 2017; Attanasio et al., 2021).

Estimated effects of attending a preferred school among those who did not benefit from PATH (i.e., estimates of θ_1 from equation (5)) are shown in Table 7 (column 3). In terms of taking the CSEC, no discernable effects are found among boys. However, we observe a negative effect of 3.53 percentage points among girls. While this effect is modest relative to the average CSEC taking rate of 72.8% among girls, it might suggest that more selective schools either increase dropout among girls or discourage marginal students from taking the CSEC to avoid potential worsening of the school average CSEC performance.[34] However, preferred school attendance conveys significant benefits on individual-level CSEC performance. Both girls and boys experience an increase of 8–9 percentage points in the likelihood of qualifying for tertiary education based on CSEC performance. The magnitudes of these effects are relatively large with respect to the average CSEC passing rate of 27.4% for girls and 14.9% for boys. Post-secondary CAPE outcomes are also positively affected for both girls and boys. The likelihood of taking the CAPE increases by 8.4 (5.3) percentage

[31] Nonetheless, when exploring potential mechanisms in Section 5.3, we show that our main results extend to several other learning outcomes.

[32] Following Jackson (2010), Jackson (2021), Beuermann and Jackson (2022), and Beuermann et al. (2023), we define this indicator as a measure of secondary school success without censoring the data. Therefore, the indicator takes the value of unity for those who achieved the certification, while zero otherwise (which includes not taking the CSEC, as well as taking the CSEC without achieving the certification.)

[33] Following Beuermann and Jackson (2022), and Beuermann et al. (2023), we define this indicator as a measure of post-secondary academic success without censoring the data. Therefore, the indicator takes the value of unity for those who achieved the associate's degree, while zero otherwise (which includes not taking the CAPE, as well as taking the CAPE without achieving the degree.)

[34] Jackson (2010) also finds negative effects of selective school attendance on the likelihood of taking the CSEC when using a similar discontinuity model in Trinidad and Tobago.

points among girls (boys); while the likelihood of earning an Associate Degree goes up by 9 (5.4) percentage points among girls (boys). These effects are substantial with respect to the average CAPE taking rates of 16.7% (9.1%) among girls (boys), and the proportion of girls (boys) with an Associate Degree of 7% (3.2%).

We now explore whether the effectiveness of preferred schools differs between comparable PATH recipients and non-recipients. Estimates of τ from equation (5) capture the differential benefits that PATH recipients experienced from attending a preferred school with respect to those experienced by comparable non-recipients. These estimates are shown in Table 7 (column 2) and suggest no discernible interactions among girls but negative interactions among boys. Accordingly, we compute the estimated preferred school effects among PATH beneficiaries (i.e., $\theta_1 + \tau$) in Table 7 (column 4). For both, girls and boys, these estimates are positive and significant. This shows that PATH beneficiaries also experienced significant learning gains from attending preferred schools. When comparing the school effects shown in columns 3 and 4, they do not differ among girls. Nonetheless, estimates suggest that boys who benefited from PATH display lower returns to preferred school attendance with respect to those who did not benefit from PATH. We test this formally in Table 7 (column 5) which displays the *p-values* of testing the null hypothesis of equality of school effects between PATH beneficiaries and comparable non-beneficiaries (i.e., $\theta_1 + \tau = \theta_1$). The results reject the null hypothesis among boys. This confirms that boys who were exposed to PATH before secondary school attendance experienced significantly lower academic returns from attending a preferred secondary school with respect to comparable boys who did not benefit from PATH.[35]

Overall, we document that: (a) for both girls and boys, PATH reception had no direct effects on learning; (b) for girls, the returns to preferred school attendance were unaltered by PATH reception; and (c) for boys, the returns to preferred school attendance were significantly lower among PATH recipients with respect to comparable counterparts who did not receive PATH benefits.

5.3 Potential Mechanisms

Our main outcomes were measured without limiting the time window for CSEC/CAPE taking. As we have many rounds of data, we treated similarly those who obtained CSEC/CAPE certifications on time (i.e., within 5 years of GSAT taking for CSEC and within 7 years of GSAT taking for CAPE) and those who achieved so with delay. Since PATH requires a minimum school attendance rate of 85%, the program may have affected on time taking which could lead us to different conclusions. Appendix Table A.7 reports effects on CSEC and CAPE certifications achieved on

[35] When restricting the sample to those who took the examinations, our findings remain qualitative the same. No differential preferred school effects between PATH recipients and non-recipients among girls. Significantly lower preferred school effects on CSEC success among boys who received PATH with respect to comparable peers who did not receive PATH (Appendix Table A.6).

time. Estimates are very similar to our main results, suggesting that this possibility is not driving our findings.

The categorical outcomes that we measure may mask potentially different effects at the intensive margin. It might be that school effectiveness by PATH status could be different when looking at the number of subjects taken. As PATH requires school attendance but enforcement of academic effort is not possible, students might reduce academic effort on the core (and more demanding) subjects and take other subjects that could be perceived as more useful for their lives. Since taking the core subjects is mandatory, such potential dynamic would lead PATH recipients to take relatively more subjects without achieving certifications (which requires passing the core subjects). Appendix Table A.8 reports estimates for the number of CSEC and CAPE subjects taken and passed; while Appendix Table A.9 does so for the number of CSEC an CAPE subjects taken and passed on time. These results mimic our main findings, suggesting that this potential mechanism is not driving our main conclusions.

Our evidence points to within-school dynamics that reduce their academic effectiveness among boys who are PATH beneficiaries. One possibility might be the stigmatization of PATH beneficiaries within preferred schools such that potential socio-emotional harm partly undoes the academic benefits of preferred school attendance. Indeed, the available evidence suggests that the overall incidence of bullying within Jamaican schools is high with 30% of students manifesting fear of going to school because of bullying (PSearch-Associates, 2015). Consistent with this, qualitative evidence suggests that bullying constitutes a daily occurrence within Jamaican schools (Hudson-Davis et al., 2015). Furthermore, Castle (2015) documents that, as PATH beneficiaries are entitled to free school meals, this reveals their beneficiary status to other students triggering stigma and marginalization within schools.[36] The study also suggests that such dynamics are more prevalent among boys, which is consistent with evidence suggesting that bullying is more prevalent among boys than among girls (Currie et al., 2008; Sarzosa and Urzúa, 2021; Sarzosa, 2021). This evidence suggests that stigmatization of PATH beneficiaries is a likely mechanism operating behind the reduced effectiveness of preferred schools among boys.

Due to data availability, we focus on school examinations and post-secondary certifications. While these outcomes are highly relevant, a complete picture would also need to assess the effects on a wider set of academic and nonacademic longer run outcomes. Existing evidence shows that school effects on test scores could differ from effects on other important outcomes like crime, teen pregnancy, and adult employment (Deming, 2011; Beuermann et al., 2023; Beuermann and

[36]This also aligns with expressed concerns of policymakers and parents in media outlets. For example, the Minister of Labour and Social Security stated in June 2013: “The stigma is normally based on negative perception, so we have actually been having programmes that actually speak to the positive effects of being on PATH.” (Jamaica Observer, 2013). Parents also expressed in March 2023: “The (PATH) school feeding programme carries with it shame and a stigma, so much so that many children prefer to go hungry.” (Jamaica Observer, 2023).

Jackson, 2022). Therefore, the documented negative interactions between PATH and preferred schools on academic outcomes may not necessarily translate into similar results on other important longer-run outcomes.

5.4 Robustness

To assuage concerns that our results are driven by modelling choices, we show that our estimated effects are similar when computing optimal bandwidths according to Calonico et al. (2017) and to alternative polynomial specifications of the running variables (Appendix Tables A.10 - A.11).

6 Conclusions and Policy Implications

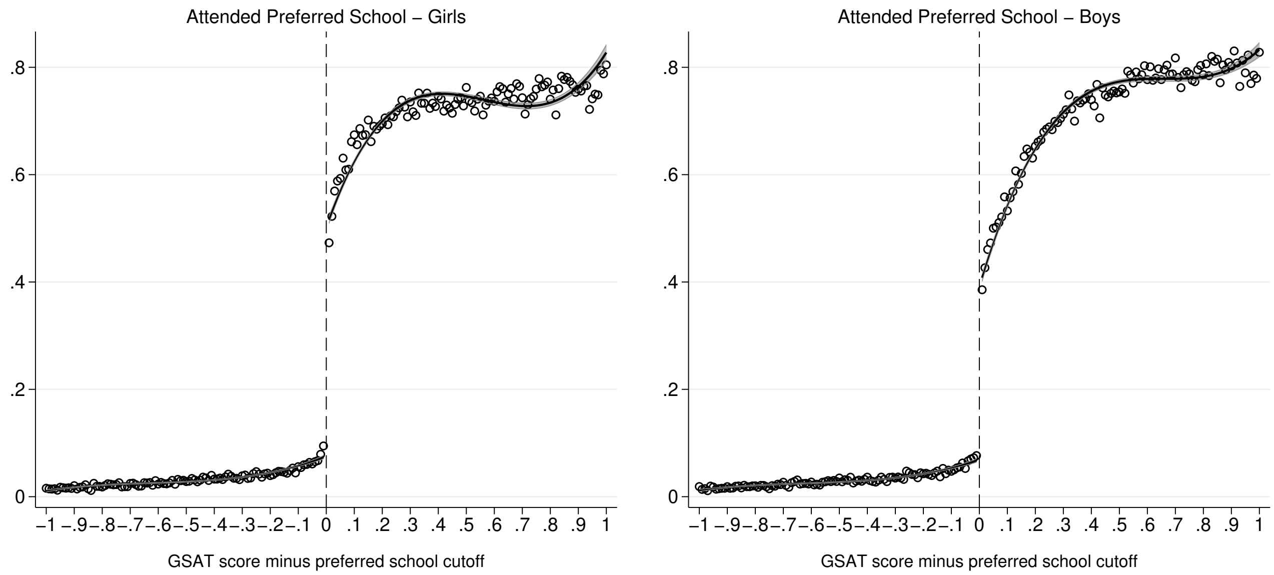
Potential interactions between different human capital interventions are highly relevant for policy design as different policies are not implemented in isolation and, therefore, the effectiveness of one policy could be affected by another. In this paper we investigate whether two institutionalized policies in Jamaica interact to affect educational outcomes at scale. These are the Programme of Advancement through Health and Education (Jamaica's Conditional Cash Transfer Program or PATH) and the subsequent attendance to a preferred (or more selective) public secondary school.

Administrative data covering the full population of students delivers three main results. First, for both girls and boys, benefiting from PATH had no direct effects on secondary and post-secondary learning. Second, the gains from preferred school attendance were unaltered by PATH participation among girls. Third, for boys, the gains from preferred school attendance were significantly lower among PATH beneficiaries when compared to equivalent counterparts who did not receive PATH. This implies that, among boys, PATH reception is partly undoing the potential benefits to attending a more selective secondary school.

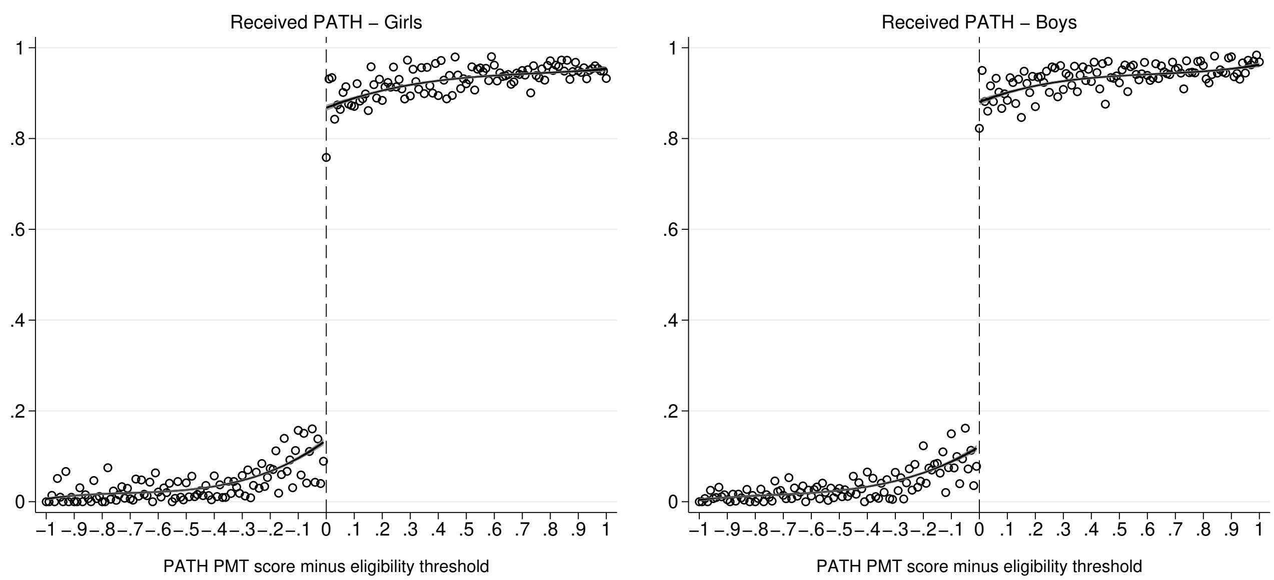
Overall, our evidence highlights the importance of understanding and measuring potential interaction effects between different public interventions. As Conditional Cash Transfers (CCT) have proliferated across many nations, our findings portray the need to evaluate whether these programs are also altering the effectiveness of other human capital development interventions. The fact that a number of CCT programs across different nations included impact evaluation designs, could facilitate the exploration of interaction effects with other policies that either included impact evaluation designs or that convey sources of exogenous variation within their targeting processes. As potential interactions across programs convey important implications for cost-benefit analyses, our findings suggest that this is a relevant issue that could be further explored in future research across different contexts.

Figure 1: First Stage

Panel A: Attended preferred school



Panel B: Received PATH



Notes: Panel A: The Y-axis represents the likelihood of having attended a preferred school. The X-axis is the GSAT standardized score relative to the preferred school admission cutoff. The circles are means corresponding to 0.25-point bins of the standardized relative score. The solid lines are generated by fitting a third degree polynomial of the relative score fully interacted with the $Above_{ijt}$ indicator. Panel B: The Y-axis represents the likelihood of having received PATH benefits. The X-axis is the (minus) PATH PMT standardized score relative to the eligibility threshold. The circles are means corresponding to 0.25-point bins of the standardized relative PMT score. The solid lines are generated by fitting a third degree polynomial of the PMT score fully interacted with the $AboveP_{it}$ indicator. All panels: The 95 percent confidence interval of the fitted polynomials are presented in light gray.

Table 1: Summary Statistics

	Girls			Boys		
	mean	sd	N	mean	sd	N
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline indicators at PATH application (PATH applicants 2001 - 2013)						
Age at PATH application (in months)	79.8970	26.9368	113,140	80.4292	26.5825	106,952
PATH applicant is female	0.8633	0.3435	113,140	0.8580	0.3491	106,952
PATH applicant completed secondary	0.3788	0.4851	113,140	0.3831	0.4861	106,952
Weekly income per capita	25.6181	18.6474	113,140	25.7866	20.0508	106,952
Own dwelling	0.4220	0.4939	113,140	0.4251	0.4944	106,952
Household size	5.7674	2.7307	113,140	5.7471	2.7176	106,952
Received PATH	0.5025	0.5000	113,140	0.4946	0.5000	106,952
Panel B: Academic indicators						
Age at GSAT date (in months)	142.8496	5.2366	113,140	143.7216	5.4064	106,952
GSAT standardized score	0.2245	0.9406	113,140	-0.2374	1.0060	106,952
Peer GSAT score	0.1456	0.8441	113,140	-0.1259	0.8432	106,952
Selectivity of school choice 1	1.3898	0.6463	112,837	1.1373	0.7605	106,506
Selectivity of school choice 2	1.2118	0.6850	112,802	0.9547	0.7719	106,452
Selectivity of school choice 3	1.0697	0.7477	112,711	0.7885	0.8094	106,340
Selectivity of school choices 4+	0.7667	0.6971	104,884	0.5586	0.7135	99,160
Assigned to school choice 1	0.1496	0.3566	113,140	0.1595	0.3662	106,952
Assigned to school choice 2	0.1316	0.3380	113,140	0.1401	0.3471	106,952
Assigned to school choice 3	0.1213	0.3264	113,140	0.1418	0.3488	106,952
Assigned to school choice 4+	0.5976	0.4904	113,140	0.5586	0.4966	106,952
Took CSEC	0.7283	0.4449	113,140	0.5344	0.4988	106,952
CSEC qualification for tertiary education	0.2736	0.4458	113,140	0.1490	0.3561	106,952
Took CAPE	0.1686	0.3744	100,975	0.0907	0.2872	96,171
CAPE Associate Degree	0.0700	0.2552	100,975	0.0316	0.1748	96,171

Notes: This table displays means (columns 1 and 4), standard deviations (columns 2 and 5), and number of individual observations (columns 3 and 6) differentiated by gender. Weekly income per capita is expressed in real 2019 PPP US$. For CSEC outcomes, we use GSAT cohorts up to 2015 because CSEC is taken five years after GSAT and the latest CSEC data available is for the year 2020. The number of observations for CAPE outcomes is lower as these are restricted up to GSAT cohort 2013 given that the CAPE is fully taken seven years after GSAT and the latest CAPE data available is for 2020.

Table 2: Validity of Preferred School Effects

	Girls	Boys
	(1)	(2)
Panel A: First Stage		
Dependent Variable: $Attend_{ijt}$		
$Above_{ijt}$	0.4089***	0.3034***
	(0.0044)	(0.0051)
First Stage F-Statistic	1,800.86	
Panel B: Exclusion Restriction		
Dependent Variable: Socioeconomic index		
$Above_{ijt}$	0.0008	-0.0013
	(0.0013)	(0.0013)
Panel C: Gaming of the Cutoff		
Differential density: School admission cutoff	-0.2301	-0.4794
[p-value]	[0.8180]	[0.6317]
Observations	346,136	317,901

Notes: Panel A reports first stage estimated coefficients on $Above_{ijt}$ from equation (1) and the first stage Kleibergen-Paap F-statistic. Panel B displays estimated coefficients on $Above_{ijt}$, having the baseline socioeconomic index as a dependent variable within a reduced-form model with the same structure as equation (1). Panel C reports the results of the McCrary (2008) cutoff manipulation test around the preferred school admission cutoff. Estimated standard errors clustered at the individual level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 3: Validity of PATH Effects

	Girls	Boys
	(1)	(2)
Panel A: First Stage		
Dependent Variable: $PATH_{it}$		
$AboveP_{it}$	0.8236***	0.8402***
	(0.0035)	(0.0034)
First Stage F-Statistic	27,339.52	
Panel B: Exclusion Restriction		
Dependent Variable: Socioeconomic index		
$AboveP_{it}$	-0.0003	-0.0004
	(0.0005)	(0.0005)
Panel C: Gaming of the Cutoff		
Differential density at PATH eligibility cutoff	0.1703	0.2890
[p-value]	[0.8648]	[0.7726]
Observations	346,136	317,901

Notes: Panel A reports first stage estimated coefficients on $AboveP_{it}$ from equation (3) and the first stage Kleibergen-Paap F-statistic. Panel B displays estimated coefficients on $AboveP_{it}$, having the baseline socioeconomic index as a dependent variable within a reduced-form model with the same structure as equation (3). Panel C reports the results of the McCrary (2008) cutoff manipulation test around the PATH eligibility cutoff. Estimated standard errors clustered at the individual level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 4: PATH Effects on GSAT performance, School Choices, and School Assignments

	Girls		Boys	
	Effects	N	Effects	N
	(1)	(2)	(3)	(4)
Panel A: Reduced-Form Effects				
GSAT standardized score	0.0042	346,136	0.0054	317,901
	(0.0084)		(0.0092)	
Selectivity of school choice 1	0.0007	346,042	0.0021	317,829
	(0.0014)		(0.0015)	
Selectivity of school choice 2	-0.0004	346,019	-0.0002	317,825
	(0.0014)		(0.0015)	
Selectivity of school choice 3	-0.0003	345,855	-0.0001	317,627
	(0.0015)		(0.0017)	
Selectivity of school choices 4+	0.0010	330,601	-0.0012	303,210
	(0.0014)		(0.0015)	
Assigned to school choice 1 vs choices 2+	-0.0005	346,136	0.0018	317,901
	(0.0015)		(0.0017)	
Assigned to school choice 2 vs choices 3+	-0.0004	331,511	0.0012	302,840
	(0.0027)		(0.0030)	
Assigned to school choice 3 vs choices 4+	0.0023	304,612	0.0023	275,546
	(0.0039)		(0.0046)	
Peer GSAT score	0.0128	346,136	0.0056	317,901
	(0.0082)		(0.0085)	
Panel B: 2SLS Effects				
GSAT standardized score	0.0075	346,136	0.0097	317,901
	(0.0103)		(0.0110)	
Selectivity of school choice 1	0.0005	346,042	0.0028	317,829
	(0.0017)		(0.0019)	
Selectivity of school choice 2	-0.0010	346,019	-0.0002	317,825
	(0.0017)		(0.0018)	
Selectivity of school choice 3	-0.0007	345,855	0.0001	317,627
	(0.0019)		(0.0020)	
Selectivity of school choices 4+	0.0004	330,601	-0.0016	303,210
	(0.0018)		(0.0018)	
Assigned to school choice 1 vs choices 2+	0.0000	346,136	0.0027	317,901
	(0.0019)		(0.0020)	
Assigned to school choice 2 vs choices 3+	0.0000	331,511	0.0018	302,840
	(0.0033)		(0.0036)	
Assigned to school choice 3 vs choices 4+	0.0036	304,612	0.0034	275,546
	(0.0048)		(0.0055)	
Peer GSAT score	0.0150	346,136	0.0073	317,901
	(0.0100)		(0.0102)	

Notes: Panel A displays reduced-form estimated coefficients on $AboveP_{it}$ from a model with the same structure as equation (3). Panel B displays 2SLS estimated coefficients on $\widehat{PATH}_{it}$ using $AboveP_{it}$ as the excluded instrument (resulting from equation system (3) - (4) in the text). Estimated standard errors clustered at the individual level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 5: Validity of the Double Regression Discontinuity Design

	Girls	Boys
	(1)	(2)
Panel A: First Stage		
Dependent Variable: $PATH_{it}$		
$AboveP_{it}$	0.7687***	0.7928***
	(0.0055)	(0.0054)
Dependent Variable: $Attend_{ijt}$		
$Above_{ijt}$	0.4312***	0.3111***
	(0.0049)	(0.0055)
Dependent Variable: $PATH_{it} \cdot Attend_{ijt}$		
$AboveP_{it} \cdot Above_{ijt}$	0.5010***	0.5151***
	(0.0036)	(0.0036)
First Stage F-Statistic	2,099.50	
Panel B: Exclusion Restriction		
Dependent Variable: Socioeconomic index		
$AboveP_{it}$	0.0037	0.0008
	(0.0022)	(0.0024)
$Above_{ijt}$	0.0009	-0.0002
	(0.0014)	(0.0015)
$AboveP_{it} \cdot Above_{ijt}$	-0.0003	-0.0019
	(0.0014)	(0.0014)
Observations	346,136	317,901

Notes: Panel A reports first stage estimated coefficients on the excluded instruments (i.e., $AboveP_{it}$, $Above_{ijt}$, and $AboveP_{it} \cdot Above_{ijt}$), having the instrumented variables (i.e., $PATH_{it}$, $Attend_{ijt}$, and $PATH_{it} \cdot Attend_{ijt}$) as regressors from models with the same structure as equation (6). Panel A also reports the joint first stage Kleibergen-Paap F-statistic on the excluded instruments. Panel B displays estimated coefficients on $AboveP_{it}$, $Above_{ijt}$, and $AboveP_{it} \cdot Above_{ijt}$ having the baseline socioeconomic index as regressor within a reduced-form model with the same structure as equation (6). *** p<0.01, ** p<0.05, * p<0.10.

Table 6: Preferred School 2SLS Effects on Learning Environments

	Girls		Boys	
	Effects	N	Effects	N
	(1)	(2)	(3)	(4)
Peers GSAT score	0.4075***	346,136	0.5388***	317,901
	(0.0109)		(0.0150)	
GSAT score gap (best-worst)	-0.2933***	346,136	-0.0892***	317,901
	(0.0224)		(0.0343)	
Parish HHI for attended schools	-0.0508***	346,136	-0.0590***	317,901
	(0.0044)		(0.0059)	
Teachers with a university degree (%)	3.4558***	329,903	1.5673***	305,897
	(0.2148)		(0.3267)	
Pupil-Teacher Ratio	-0.5793***	320,584	-0.7413***	298,932
	(0.0842)		(0.1376)	
Attendance Rate (%)	3.5648***	310,122	3.7731***	290,345
	(0.1671)		(0.2673)	
Overall Effectiveness Index	0.1465***	321,911	0.1032***	300,458
	(0.0047)		(0.0067)	

Notes: This table displays 2SLS estimated coefficients on $Attend_{ijt}$ using $Above_{ijt}$ as the excluded instrument (resulting from equation system (1) - (2) in the text). The proportion of teachers with university degrees was computed for each school measured in the 2009 Teacher Census. Estimated standard errors clustered at the individual level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 7: Interactions between PATH and Preferred School Attendance

	$\hat{PATH}_{it}$	$PATH_{it} \cdot \hat{Attend}_{ijt}$	Preferred School Effects		
			Among PATH Non-beneficiaries	Among PATH Beneficiaries	p-value
	(β_1)	(τ)	(θ_1)	$(\theta_1 + \tau)$	(3)=(4)
	(1)	(2)	(3)	(4)	(5)
Panel A: Girls					
CSEC Performance					
Took at least 1 subject	-0.0118	-0.0056	-0.0353***	-0.0409***	0.53
	(0.0103)	(0.0089)	(0.0077)	(0.0094)	
Qualified for tertiary education	0.0025	-0.0128	0.0892***	0.0763***	0.14
	(0.0089)	(0.0088)	(0.0086)	(0.0099)	
Observations			346,136		
CAPE Performance					
Took at least 1 unit	0.0014	-0.0030	0.0841***	0.0811***	0.72
	(0.0075)	(0.0084)	(0.0088)	(0.0098)	
Earned associate's degree	0.0075	-0.0006	0.0900***	0.0895***	0.93
	(0.0047)	(0.0061)	(0.0064)	(0.0069)	
Observations			307,522		
Panel B: Boys					
CSEC Performance					
Took at least 1 subject	0.0254**	-0.0083	-0.0057	-0.0140	0.40
	(0.0111)	(0.0098)	(0.0106)	(0.0115)	
Qualified for tertiary education	0.0033	-0.0313***	0.0823***	0.0510***	<0.01
	(0.0066)	(0.0073)	(0.0098)	(0.0101)	
Observations			317,901		
CAPE Performance					
Took at least 1 unit	-0.0005	-0.0123*	0.0529***	0.0406***	0.06
	(0.0054)	(0.0065)	(0.0094)	(0.0095)	
Earned associate's degree	-0.0001	-0.0110***	0.0540***	0.0430***	<0.01
	(0.0030)	(0.0041)	(0.0068)	(0.0068)	
Observations			285,474		

Notes: This table displays 2SLS estimates from the second stage equation (5) that result from estimating the Double Regression Discontinuity (DRD) model outlined in equations (5) - (6). Column (1) displays estimates on the instrumented $\hat{PATH}_{it}$ indicator. Column (2) displays estimates on the instrumented interaction $PATH_{it} \cdot \hat{Attend}_{ijt}$. Column (3) displays estimated preferred school effects among PATH non-beneficiaries (i.e., the estimate on the instrumented $\hat{Attend}_{ijt}$ indicator). Column (4) displays estimated preferred school effects among PATH beneficiaries (i.e., the addition of the estimates shown in columns (2) and (3)). Column (5) displays the p-value that results from testing the equality between estimates shown in columns (3) and (4). Estimated standard errors clustered at the individual level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

References

Abdulkadiroglu, A. and T. Sonmez (1998). Random Serial Dictatorship and the Core from Random Endowments in House Allocation Problems. *Econometrica 66*(3), 689–702.

Adhvaryu, A., S. Bednar, T. Molina, Q. Nguyen, and A. Nyshadham (2020). When It Rains It Pours: The Long-Run Economic Impacts of Salt Iodization in the United States. *The Review of Economics and Statistics 102*(2), 395–407.

Akwii, S., E. Kipchumba, A. S. Mohamed, and M. Sulaiman (2018). Added on Benefits of Structured Peer to Peer Support on Unconditional Cash Transfer: Short-term Evidence from a Field Experiment in Baidoa, Somalia. Mimeo, Save The Children.

Araujo, M. C., M. Bosch, and N. Schady (2017). Can Cash Transfers Help Households Escape an Intergenerational Poverty Trap? In C. B. Barrett, M. R. Carter, and J.-P. Chavas (Eds.), *The Economics of Poverty Traps*, Chapter 10, pp. 357–394. Chicago, IL: University of Chicago Press.

Araujo, M. C. and K. Macours (2021). Education, Income and Mobility: Experimental Impacts of Childhood Exposure to Progresa after 20 Years. Working Paper 1288, Inter-American Development Bank.

Arriagada, A. M., J. Perry, L. Rawlings, J. Trias, and M. Zumaeta (2018). Promoting early childhood development through combining cash transfers and parenting programs. Policy research working paper 8670, Washington, D.C.: World Bank Group.

Arriagada, A. M., L. Rawlings, J. Trias, and M. Zumaeta (2020). Combining cash transfers and parenting programs: Operational design considerations. *Global Social Policy 20*(1), 15–20.

Attanasio, O., L. Cardona Sosa, C. Medina, C. Meghir, and C. Posso (2021). Long Term Effects of Cash Transfer Programs in Colombia. Working Paper 1288, National Bureau of Economic Research.

Attanasio, O., C. Fernández, E. Fitzsimons, S. Grantham-McGregor, C. Meghir, and M. Rubio-Codina (2014). Using the infrastructure of a conditional cash transfer program to deliver a scalable integrated early child development program in Colombia: Cluster randomized controlled trial. *BMJ 349*(September), g5785.

Attanasio, O., C. Meghir, and A. Santiago (2011). Education choices in Mexico: Using a structural model and a randomized experiment to evaluate PROGRESA. *The Review of Economic Studies 79*(1), 37–66.

Baez, J. E. and A. Camacho (2011). Assessing the long-term effects of conditional cash transfers on human capital: Evidence from colombia. Working Paper 5751, The Institute for the Study of Labor (IZA).

Baird, S., F. Ferreira, B. Özler, and M. Woolcock (2014). Conditional, unconditional and everything in between: a systematic review of the effects of cash transfer programmes on schooling outcomes. *Journal of Development Effectiveness 6*(1), 1–43.

Baird, S., F. H. G. Ferreira, B. Özler, and M. Woolcock (2013). Relative Effectiveness of Conditional and Unconditional Cash Transfers for Schooling Outcomes in Developing Countries: A Systematic Review. *Campbell Systematic Reviews 9*(1), 1–124.

Barham, T., K. Macours, and J. Maluccio (2017). Are Conditional Cash Transfers Fulfilling Their Promise? Schooling, Learning, and Earnings After 10 Years. CEPR Discussion Papers 11937.

Barham, T., K. Macours, and J. Maluccio (2018). Experimental Evidence of Exposure to a Conditional Cash Transfer During Early Teenage. CEPR Discussion Papers 13165.

Barham, T., K. Macours, and J. A. Maluccio (2013). Boys' Cognitive Skill Formation and Physical Growth: Long-Term Experimental Evidence on Critical Ages for Early Childhood Interventions. *American Economic Review 103*(3), 467–471.

Bastagli, F., J. Hagen-Zanker, L. Harman, V. Barca, G. Sturge, and T. Schmidt (2016). Cash transfers: What does the evidence say? a rigorous review of programme impact and the role of design and implementation features. Technical report, London: Overseas Development Institute.

Behrman, J. R. and J. Hoddinott (2005). Program evaluation with unobserved heterogeneity and selective implementation: The Mexican Progresa impact on child nutrition. *Oxford Bulletin of Economics and Statistics 67*(2), 547–569.

Benhassine, N., F. Devoto, E. Duflo, P. Dupas, and V. Pouliquen (2012). Turning a shove into a nudge? A labeled cash transfer for education. *American Economic Journal: Economic Policy 7*(3), 86–125.

Beuermann, D., C. K. Jackson, L. Navarro-Sola, and F. Pardo (2023). What is a Good School, and Can Parents Tell? Evidence on the Multidimensionality of School Output. *The Review of Economic Studies 90*(1), 65–101.

Beuermann, D. W. and C. K. Jackson (2022). The Short and Long-Run Effects of Attending The Schools that Parents Prefer. *Journal of Human Resources 57*(3), 725–746.

Bhalotra, S. R. and A. Venkataramani (2015). Shadows of the Captain of the Men of Death: Early Life Health Interventions, Human Capital Investments, and Institutions. *SSRN Electronic Journal*.

Calonico, S., M. D. Cattaneo, M. H. Farrell, and R. Titiunik (2017). rdrobust: Software for regression-discontinuity designs. *The Stata Journal 17*(2), 372–404.

Castle, M. (2015). The Administration of the PATH Programme in Two High Schools in Jamaica and its Effect on the Student Beneficiaries: A Case Study. *The Bridge: Journal of Educational Research-Informed Practice 2*(2).

Cohen, J., C. Rothschield, G. Golub, G. N. Omundi, M. E. Kruk, and M. McConnell (2017). Cash on delivery: Results of a randomized experiment to promote maternal health care in Kenya. . *Health Affairs 36*(11).

Cunha, F. and J. Heckman (2007). The technology of skill formation. *American Economic Review 97*(2), 31–47.

Currie, C., S. N. Gabhainn, E. Godeau, C. Roberts, R. Smith, D. Currie, W. Pickett, M. Richter, A. Morgan, and V. Barnekow (2008). Inequalities in young people's health: HBSC international report from the 2005/2006 Survey.

Datta, S., J. B. Martin, C. MacLeod, L. B. Rawlings, and A. Vermehren (2021). Do Behavioral Interventions Enhance the Effects of Cash on Early Childhood Development and Its Determinants ? Evidence from a Cluster-Randomized Trial in Madagascar. Policy research working paper 9747, Washington, D.C. : World Bank Group.

de Walque, D., L. Fernald, P. Gertler, and M. Hidrobo (2017). Cash Transfers and Child and Adolescent Development". in d.a.p. bundy, n. de silva, s. horton, d.t. jamison, and g.c. patton (eds), Disease Control Priorities: Volume 8, Child and Adolescent Health and Development, Third Edition, 325–41. World Bank, Washington DC.

Deming, D. J. (2011). Better Schools, Less Crime? *The Quarterly Journal of Economics 126*(4), 2063–2115.

Duflo, E., P. Dupas, and M. Kremer (2011). Peer effects, teacher incentives, and the impact of tracking: Evidence from a randomized evaluation in Kenya. *American Economic Review 101*(5), 1739–1774.

Fiszbein, A. and N. Schady (2009). *Conditional Cash Transfers: Reducing Present and Future Poverty*. Washington, D.C.: The World Bank.

Gale, D. and L. S. Shapley (1962). College Admissions and the Stability of Marriage. *The American Mathematical Monthly 69*(September 2020), 9–15.

Gilraine, M. (2018). School Accountability and the Dynamics of Human Capital Formation. Technical report, New York University.

Glewwe, P., C. Siameh, B. Sun, and S. Wisniewski (2021). School Resources and Educational Outcomes in Developing Countries. In B. P. McCall (Ed.), *The Routledge Handbook of the Economics of Education*, Chapter 9, pp. 219–252. New York, NY: Routledge.

Goff, L., O. Malamud, C. Pop-Eleches, and M. Urquiola (2023). Interactions between family and school environments: Access to abortion and selective schools. *Journal of Human Resources*, forthcoming.

Haeringer, G. and F. Klijn (2009). Constrained school choice. *Journal of Economic Theory 144*(5), 1921–1947.

Hahn, J., P. Todd, and W. Van Der Klaauw (2001). Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design. *Econometrica 69*(1), 201–209.

Hudson-Davis, A., P. A. Bourne, C. Sharpe-Pryce, C. Francis, I. Solan, D. Lewis, O. Watson-Coleman, J.-A. Blake, and C. Donegon (2015). The Psychology of the Bullying Phenomenon in Three Jamaican Public Primary Schools: A Need for a Public Health Trust. *International Journal of Emergency Mental Health and Human Resilience 17*(2), 406–414.

Ibarrarán, P., N. Medellín, F. Regalia, and M. Stampini (2017). *How Conditional Cash Transfers Work: Good Practices after 20 Years of Implementation*. Inter-American Development Bank.

Jackson, C. K. (2010). Do Students Benefit from Attending Better Schools? Evidence from Rule-based Student Assignments in Trinidad and Tobago. *The Economic Journal 120*(549), 1399–1429.

Jackson, C. K. (2021). Can Introducing Single-Sex Education into Low-Performing Schools Improve Academics, Arrests, and Teen Motherhood? *Journal of Human Resources 56*(1), 1–39.

Jamaica Observer, T. (2013). Government working to remove PATH-related stigma (June 18, 2013). (Link: https://www.jamaicaobserver.com/2013/06/18/govt-working-to-remove-path-related-stigma/).

Jamaica Observer, T. (2023). The real problem With PATH (March 22, 2023). (Link: https://www.jamaicaobserver.com/2023/03/22/the-real-problem-with-path/).

Johnson, R. C. and C. K. Jackson (2019). Reducing inequality through dynamic complementarity: Evidence from head start and public school spending. *American Economic Journal: Economic Policy 11*(4), 310–349.

Kling, J. R., J. B. Liebman, and L. F. Katz (2007). Experimental analysis of neighborhood effects. *Econometrica 75*(1), 83–119.

Kolesár, M. and C. Rothe (2018). Inference in regression discontinuity designs with a discrete running variable. *American Economic Review 108*(8), 2277–2304.

Levy, D. and J. Ohls (2010). Evaluation of Jamaica's PATH conditional cash transfer programme. *Journal of Development Effectiveness 2*(4), 421–441.

Little, M. T., K. Roelen, B. C. Lange, J. I. Steinert, A. R. Yakubovich, L. Cluver, and D. K. Humphreys (2021). Effectiveness of cash-plus programmes on early childhood outcomes compared to cash transfers alone: A systematic review and meta-analysis in low- and middle-income countries. *PLoS Med 18*(9), e1003698.

Macours, K., N. Schady, and R. Vakis (2012). Cash transfers, behavioral changes, and cognitive development in early childhood: Evidence from a randomized experiment. *American Economic Journal: Applied Economics 4*(2), 247–73.

Mbiti, I., K. Muralidharan, M. Romero, Y. Schipper, C. Manda, and R. Rajani (2019). Inputs, Incentives, and Complementarities in Education: Experimental Evidence from Tanzania. *The Quarterly Journal of Economics 134*(3), 1627–1673.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics 142*(2), 698–714.

Molina Millán, T., T. Barham, K. Macours, J. A. Maluccio, and M. Stampini (2019). Long-term impacts of conditional cash transfers: Review of the evidence. *World Bank Research Observer 34*(1), 119–159.

Molina Millán, T., K. Macours, J. A. Maluccio, and L. Tejerina (2020). Experimental long-term effects of early-childhood and school-age exposure to a conditional cash transfer program. *Journal of Development Economics 143*, 102385.

Oconnor, C. (2024). Do conditional cash transfers create resilience against poverty? long-run evidence from jamaica. *World Development 176*(April 2024), 106513.

Paes-sousa, R., F. Regalia, and M. Stampini (2013). Conditions for Success in Implementing CCT Programs: Lessons for Asia from Latin America and the Caribbean. Technical Report 8966-090, Mathematica Policy Research.

Parker, S. W. and T. Vogl (2023). Do Conditional Cash Transfers Improve Economic Outcomes in the Next Generation? Evidence from Mexico. *The Economic Journal 133*(655), 2775–2806.

Pop-Eleches, C. and M. Urquiola (2013). Going to a better school: Effects and behavioral responses. *American Economic Review 103*(4), 1289–1324.

PSearch-Associates (2015). Investigating the Prevalence and Impact of Peer Abuse on the Development of Jamaica's Children. Technical report, Funded by: UNICEF Jamaica.

Rossin-Slater, M. and M. Wust (2020). What Is the Added Value of Preschool for Poor Children? Long-Term and Intergenerational Impacts and Interactions with an Infant Health Intervention. *American Economic Journal: Applied Economics 12*(3), 255–286.

Roth, A. E. and M. A. Oliveira Sotomayor (1990). *Two-Sided Matching: A Study in Game-Theoretic Modeling and Analysis*. Cambridge: Cambridge University Press.

Sarzosa, M. (2021). Victimization and Skill Accumulation: The Case of School Bullying. *Journal of Human Resources*, 0819–10371R2.

Sarzosa, M. and S. Urzúa (2021). Bullying among adolescents: The role of skills. *Quantitative Economics 12*(3), 945–980.

Schultz, T. P. (2004). School subsidies for the poor: Evaluating the Mexican Progresa poverty program. *Journal of Development Economics 74*(1), 199–250.

Sedlmayr, R., A. Shah, and M. Sulaiman (2018). Cash-plus: Poverty impacts of transfer-based intervention alternatives. Center for the study of african economics working paper wps/2017-15-2, Oxford, United Kingdom.

Stampini, M., S. Martinez-Cordova, S. Insfran, and D. Harris (2018). Do Conditional Cash Transfers Lead to Better Secondary Schools? Evidence from Jamaica's PATH. *World Development 101*, 104–118.

Stampini, M. and L. Tornarolli (2012). The Growth of Conditional Cash Transfers in Latin America and the Caribbean: Did They Go Too Far? Policy brief 185, Inter-American Development Bank.

Todd, P. E. and K. I. Wolpin (2006). Assessing the impact of a school subsidy program in mexico: Using a social experiment to validate a dynamic behavioral model of child schooling and fertility. *American Economic Review 96*(5), 1384–1417.

Appendix: Supplemental Tables

Table A.1: Sample Composition by Year of Birth and PATH Application Year

Year of	Year of PATH Application													Total
Birth	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	
1991	1,562	0	0	0	0	0	0	0	0	0	0	0	0	1,562
1992	1,514	16,272	0	0	0	0	0	0	0	0	0	0	0	17,786
1993	1,653	18,123	2,127	0	0	0	0	0	0	0	0	0	0	21,903
1994	1,780	19,183	2,002	807	0	0	0	0	0	0	0	0	0	23,772
1995	1,927	18,834	1,988	755	901	0	0	0	0	0	0	0	0	24,405
1996	1,830	17,833	1,751	729	804	1,524	0	0	0	0	0	0	0	24,471
1997	1,729	16,562	1,714	723	856	1,450	1,060	0	0	0	0	0	0	24,094
1998	1,505	14,945	1,500	621	734	1,332	976	1,834	0	0	0	0	0	23,447
1999	0	13,579	1,386	599	700	1,325	985	1,775	924	0	0	0	0	21,273
2000	0	0	1,373	601	748	1,424	1,094	2,024	939	891	0	0	0	9,094
2001	0	0	0	535	677	1,318	1,101	1,927	904	848	726	0	0	8,036
2002	0	0	0	0	1,166	2,194	1,454	2,820	1,339	1,163	931	674	0	11,741
2003	0	0	0	0	0	1,844	1,146	2,038	1,002	971	704	500	303	8,508
Total	13,500	135,331	13,841	5,370	6,586	12,411	7,816	12,418	5,108	3,873	2,361	1,174	303	220,092

Notes: Sample includes all households who applied for PATH benefits between 2001 and 2013, with at least one member within the age range of 3 to 10 years old, and within years of birth that allow sufficient time to reach the age of GSAT taking between 2003 and 2015.

Table A.2: Differences in Academic Indicators between PATH Applicants and Non-Applicants

	Boys							Girls						
	Non-Applicants			Applicants			p-value	Non-Applicants			Applicants			p-value
	Mean (1)	SD (2)	N (3)	Mean (4)	SD (5)	N (6)	(1)=(4) (7)	Mean (8)	SD (9)	N (10)	Mean (11)	SD (12)	N (13)	(8)=(11) (14)
Age at GSAT date (in months)	142.6544	5.6120	110,695	143.7216	5.4064	106,952	<0.01	141.6989	5.4749	110,235	142.8496	5.2366	113,140	<0.01
GSAT standardized score	0.4201	1.1510	110,695	-0.2374	1.0060	106,952	<0.01	0.7816	1.0044	110,235	0.2245	0.9406	113,140	<0.01
Peer GSAT score	0.4429	0.9931	110,695	-0.1259	0.8432	106,952	<0.01	0.6962	0.9519	110,235	0.1456	0.8441	113,140	<0.01
Selectivity of school choice 1	1.5526	0.6073	110,662	1.1373	0.7605	106,506	<0.01	1.7266	0.4848	110,208	1.3898	0.6463	112,837	<0.01
Selectivity of school choice 2	1.3719	0.7012	110,633	0.9547	0.7719	106,452	<0.01	1.5602	0.5813	110,188	1.2118	0.6850	112,802	<0.01
Selectivity of school choice 3	1.2137	0.7605	110,259	0.7885	0.8094	106,340	<0.01	1.4410	0.6435	109,972	1.0697	0.7477	112,711	<0.01
Selectivity of school choices 4+	0.9663	0.7183	92,381	0.5586	0.7135	99,160	<0.01	1.1625	0.6570	92,815	0.7667	0.6971	104,884	<0.01
Assigned to school choice 1	0.1845	0.3879	110,695	0.1595	0.3662	106,952	<0.01	0.1915	0.3935	110,235	0.1496	0.3566	113,140	<0.01
Assigned to school choice 2	0.1375	0.3444	110,695	0.1401	0.3471	106,952	0.08	0.1328	0.3394	110,235	0.1316	0.3380	113,140	0.39
Assigned to school choice 3	0.1330	0.3396	110,695	0.1418	0.3488	106,952	<0.01	0.1016	0.3021	110,235	0.1213	0.3264	113,140	<0.01
Assigned to school choice 4+	0.5450	0.4980	110,695	0.5586	0.4966	106,952	<0.01	0.5741	0.4945	110,235	0.5976	0.4904	113,140	<0.01
Took CSEC	0.6153	0.4865	110,695	0.5344	0.4988	106,952	<0.01	0.7345	0.4416	110,235	0.7283	0.4449	113,140	<0.01
CSEC qualification for tertiary education	0.2869	0.4523	110,695	0.1490	0.3561	106,952	<0.01	0.4161	0.4929	110,235	0.2736	0.4458	113,140	<0.01
Took CAPE	0.1891	0.3916	95,630	0.0907	0.2872	96,171	<0.01	0.2921	0.4547	94,563	0.1686	0.3744	100,975	<0.01
CAPE Associate Degree	0.0836	0.2768	95,630	0.0316	0.1748	96,171	<0.01	0.1511	0.3582	94,563	0.0700	0.2552	100,975	<0.01

Notes: This table presents summary statistics of academic indicators extracted from the full population matched data across the GSAT, CSEC and CAPE registries. The "Applicants" sample refers to individuals who applied to the PATH program and were subsequently observed in the GSAT registries (i.e., the PATH-GSAT matched sample analyzed in this study.). The "Non-Applicants" sample refers to individuals who are observed in the GSAT registries but did not apply to the PATH program. Column (7) displays the *p-value* of a test for the null of equality of means shown in columns (1) and (4). Column (14) displays the *p-value* of a test for the null of equality of means shown in columns (8) and (11).

Table A.3: Summary Statistics by PATH Eligibility Status

	Girls						Boys					
	Not eligible to PATH			Eligible to PATH			Not eligible to PATH			Eligible to PATH		
	Mean (1)	SD (2)	N (3)	Mean (4)	SD (5)	N (6)	Mean (7)	SD (8)	N (9)	Mean (10)	SD (11)	N (12)
Panel A: Baseline indicators at PATH application (PATH applicants 2001 - 2013)												
Age at PATH application (in months)	80.5626	26.8557	54,301	79.2828	26.9971	58,839	80.9458	26.6186	52,324	79.9344	26.5387	54,628
Family head is female	0.8666	0.3400	54,301	0.8603	0.3467	58,839	0.8595	0.3475	52,324	0.8564	0.3507	54,628
PATH applicant completed secondary education	0.4884	0.4999	54,301	0.2777	0.4478	58,839	0.4889	0.4999	52,324	0.2818	0.4499	54,628
Weekly income per capita	27.6380	19.7365	54,301	23.7541	17.3756	58,839	27.6780	21.1478	52,324	23.9749	18.7629	54,628
Own dwelling	0.3755	0.4843	54,301	0.4649	0.4988	58,839	0.3761	0.4844	52,324	0.4720	0.4992	54,628
Household size	4.2775	1.5799	54,301	7.1423	2.8453	58,839	4.2769	1.5686	52,324	7.1553	2.8371	54,628
Received PATH	0.0276	0.1639	54,301	0.9407	0.2362	58,839	0.0261	0.1594	52,324	0.9433	0.2312	54,628
Panel B: Academic indicators												
Age at GSAT date (in months)	142.5287	5.0722	54,301	143.1459	5.3669	58,839	143.3610	5.2418	52,324	144.0670	5.5375	54,628
GSAT standardized score	0.3706	0.9352	54,301	0.0895	0.9253	58,839	-0.0923	1.0241	52,324	-0.3765	0.9682	54,628
Peer GSAT score	0.2756	0.8551	54,301	0.0257	0.8157	58,839	-0.0058	0.8636	52,324	-0.2410	0.8067	54,628
Selectivity of school choice 1	1.4944	0.5830	54,187	1.2932	0.6857	58,650	1.2582	0.7060	52,121	1.0214	0.7921	54,385
Selectivity of school choice 2	1.3083	0.6446	54,176	1.1225	0.7086	58,626	1.0671	0.7424	52,098	0.8470	0.7842	54,354
Selectivity of school choice 3	1.1715	0.7134	54,140	0.9755	0.7661	58,571	0.8870	0.7918	52,049	0.6941	0.8148	54,291
Selectivity of school choices 4+	0.8605	0.6856	50,304	0.6804	0.6966	54,580	0.6329	0.7162	48,429	0.4878	0.7037	50,731
Assigned to school choice 1	0.1508	0.3578	54,301	0.1485	0.3556	58,839	0.1547	0.3616	52,324	0.1642	0.3704	54,628
Assigned to school choice 2	0.1316	0.3381	54,301	0.1316	0.3380	58,839	0.1366	0.3434	52,324	0.1434	0.3505	54,628
Assigned to school choice 3	0.1158	0.3199	54,301	0.1264	0.3323	58,839	0.1406	0.3477	52,324	0.1429	0.3499	54,628
Assigned to school choice 4+	0.6019	0.4895	54,301	0.5936	0.4912	58,839	0.5681	0.4954	52,324	0.5495	0.4975	54,628
Took CSEC	0.7643	0.4245	54,301	0.6951	0.4604	58,839	0.5803	0.4935	52,324	0.4904	0.4999	54,628
CSEC qualification for tertiary education	0.3160	0.4649	54,301	0.2344	0.4236	58,839	0.1755	0.3804	52,324	0.1235	0.3290	54,628
Took CAPE	0.2011	0.4008	48,803	0.1382	0.3451	52,172	0.1098	0.3126	47,282	0.0722	0.2589	48,889
CAPE Associate Degree	0.0866	0.2813	48,803	0.0545	0.2271	52,172	0.0389	0.1934	47,282	0.0244	0.1544	48,889

Notes: This table displays means (columns 1, 4, 7 and 10), standard deviations (columns 2, 5, 8 and 11), and number of individual observations (columns 3, 6, 9 amd 12). Weekly income per capita is expressed in real 2019 PPP US$. Statistics are differentiated by PATH eligibility status based on the initial PMT score. For CSEC outcomes, we use GSAT cohorts up to 2015 because CSEC is taken five years after GSAT and the latest CSEC data available is for the year 2020. The number of observations for CAPE outcomes is lower as these are restricted up to GSAT cohort 2013 given that the CAPE is fully taken seven years after GSAT and the latest CAPE data available is for 2020.

Table A.4: Summary Statistics by School Choice Assignment

	Girls						Boys					
	Assigned to school choice 1-2			Assigned to school choice 3+			Assigned to school choice 1-2			Assigned to school choice 3+		
	Mean (1)	SD (2)	N (3)	Mean (4)	SD (5)	N (6)	Mean (7)	SD (8)	N (9)	Mean (10)	SD (11)	N (12)
Panel A: Baseline indicators at PATH application (PATH applicants 2001 - 2013)												
Age at PATH application (in months)	79.3826	27.2333	31,809	80.0982	26.8174	81,331	80.5518	26.8734	32,045	80.3768	26.4571	74,907
Family head is female	0.8527	0.3544	31,809	0.8675	0.3391	81,331	0.8514	0.3557	32,045	0.8608	0.3462	74,907
PATH applicant completed secondary education	0.3907	0.4879	31,809	0.3741	0.4839	81,331	0.3889	0.4875	32,045	0.3806	0.4855	74,907
Weekly income per capita	25.3701	18.4614	31,809	25.7152	18.7189	81,331	25.2820	19.5470	32,045	26.0024	20.2588	74,907
Own dwelling	0.4549	0.4980	31,809	0.4091	0.4917	81,331	0.4512	0.4976	32,045	0.4139	0.4925	74,907
Household size	5.7287	2.7177	31,809	5.7825	2.7356	81,331	5.7518	2.7208	32,045	5.7451	2.7162	74,907
Received PATH	0.5061	0.5000	31,809	0.5011	0.5000	81,331	0.5118	0.4999	32,045	0.4872	0.4998	74,907
Panel B: Academic indicators												
Age at GSAT date (in months)	142.7731	5.3581	31,809	142.8796	5.1881	81,331	143.6258	5.4854	32,045	143.7625	5.3717	74,907
GSAT standardized score	0.8417	0.9415	31,809	-0.0170	0.8227	81,331	0.3358	1.0683	32,045	-0.4827	0.8696	74,907
Peer GSAT score	0.6938	0.8482	31,809	-0.0687	0.7392	81,331	0.3615	0.8613	32,045	-0.3344	0.7435	74,907
Selectivity of school choice 1	1.1999	0.8046	31,807	1.4643	0.5548	81,030	0.9113	0.8978	32,038	1.2345	0.6700	74,468
Selectivity of school choice 2	0.9059	0.8237	31,792	1.3318	0.5796	81,010	0.6156	0.8690	32,009	1.1005	0.6757	74,443
Selectivity of school choice 3	1.0329	0.7685	31,745	1.0841	0.7388	80,966	0.7683	0.8278	31,968	0.7972	0.8012	74,372
Selectivity of school choices 4+	0.7836	0.7289	29,074	0.7603	0.6845	75,810	0.6096	0.7273	29,624	0.5369	0.7065	69,536
Assigned to school choice 1	0.5320	0.4990	31,809	0.0000	0.0000	81,331	0.5325	0.4990	32,045	0.0000	0.0000	74,907
Assigned to school choice 2	0.4680	0.4990	31,809	0.0000	0.0000	81,331	0.4675	0.4990	32,045	0.0000	0.0000	74,907
Assigned to school choice 3	0.0000	0.0000	31,809	0.1687	0.3745	81,331	0.0000	0.0000	32,045	0.2024	0.4018	74,907
Assigned to school choice 4+	0.0000	0.0000	31,809	0.8313	0.3745	81,331	0.0000	0.0000	32,045	0.7976	0.4018	74,907
Took CSEC	0.8200	0.3842	31,809	0.6924	0.4615	81,331	0.6600	0.4737	32,045	0.4806	0.4996	74,907
CSEC qualification for tertiary education	0.4845	0.4998	31,809	0.1910	0.3931	81,331	0.2840	0.4509	32,045	0.0912	0.2879	74,907
Took CAPE	0.3085	0.4619	28,293	0.1141	0.3180	72,682	0.1742	0.3793	28,747	0.0551	0.2281	67,424
CAPE Associate Degree	0.1555	0.3624	28,293	0.0368	0.1882	72,682	0.0722	0.2589	28,747	0.0142	0.1184	67,424

Notes: This table displays means (columns 1, 4, 7 and 10), standard deviations (columns 2, 5, 8 and 11), and number of individual observations (columns 3, 6, 9 amd 12). Weekly income per capita is expressed in real 2019 PPP US$. Statistics are differentiated by school choice assignments. For CSEC outcomes, we use GSAT cohorts up to 2015 because CSEC is taken five years after GSAT and the latest CSEC data available is for the year 2020. The number of observations for CAPE outcomes is lower as these are restricted up to GSAT cohort 2013 given that the CAPE is fully taken seven years after GSAT and the latest CAPE data available is for 2020.

Table A.5: Preferred Schools 2SLS Effects on Schools' Effectiveness Dimensions

	Boys		Girls	
	Effects (1)	N (2)	Effects (3)	N (4)
Overall Effectiveness - index	0.1032*** (0.0067)	300,458	0.1465*** (0.0047)	321,911
Overall Effectiveness - exceptionally high or good	0.2466*** (0.0115)	300,458	0.3172*** (0.0092)	321,911
Leadership and Management - index	0.0775*** (0.0073)	300,458	0.0997*** (0.0051)	321,911
Leadership and Management - exceptionally high or good	0.1926*** (0.0142)	300,458	0.2292*** (0.0109)	321,911
Teaching in Support of Students learning - index	0.0941*** (0.0060)	300,458	0.1239*** (0.0042)	321,911
Teaching in Support of Students learning - exceptionally high or good	0.2147*** (0.0104)	300,458	0.2371*** (0.0083)	321,911
Students Performance in English and Math - index	0.1621*** (0.0072)	300,346	0.2312*** (0.0056)	321,709
Students Performance in English and Math - exceptionally high or good	0.2508*** (0.0091)	300,458	0.3458*** (0.0085)	321,911
Students Progress in English and Math - index	0.0600*** (0.0061)	300,458	0.1242*** (0.0048)	321,911
Students Progress in English and Math - exceptionally high or good	0.0552*** (0.0061)	300,458	0.2096*** (0.0079)	321,911
Students Personal and Social Development - index	0.0774*** (0.0061)	300,458	0.1021*** (0.0043)	321,911
Students Personal and Social Development - exceptionally high or good	0.2673*** (0.0140)	300,458	0.3054*** (0.0109)	321,911
Use of Human and Material Resources - index	0.0392*** (0.0059)	300,458	0.0602*** (0.0041)	321,911
Use of Human and Material Resources - exceptionally high or good	0.1467*** (0.0119)	300,458	0.1626*** (0.0096)	321,911
Curriculum and enhancement programs - index	0.0971*** (0.0068)	300,458	0.0978*** (0.0047)	321,911
Curriculum and enhancement programs - exceptionally high or good	0.2477*** (0.0164)	300,458	0.2196*** (0.0120)	321,911
Provisions for safety, security, health and well-being - index	0.0328*** (0.0065)	300,458	0.0506*** (0.0045)	321,911
Provisions for safety, security, health and well-being - exceptionally high or good	0.0863*** (0.0171)	300,458	0.0970*** (0.0122)	321,911

Notes: This table displays 2SLS estimated coefficients on $Attend_i jt$ using $Above_i jt$ as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors clustered at the individual level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A.6: Interactions between PATH and Preferred School Attendance (conditional on taking the CSEC)

	$\widehat{PATH}_{it}$	$PATH_{it} \cdot \widehat{Attend}_{ijt}$	Preferred School Effects		
			Among PATH Non-beneficiaries	Among PATH Beneficiaries	p-value
	(β_1)	(τ)	(θ_1)	$(\theta_1 + \tau)$	(3)=(4)
	(1)	(2)	(3)	(4)	(5)
Panel A: Girls					
CSEC Performance					
Qualified for tertiary education	0.0059	-0.0159	0.0721***	0.0562***	0.11
	(0.0117)	(0.0098)	(0.0088)	(0.0102)	
Number of CSEC subjects passed	-0.0245	-0.0309	0.0290	-0.0019	0.57
	(0.0684)	(0.0544)	(0.0466)	(0.0550)	
Observations			253,186		
CAPE Performance					
Earned Associate's degree	0.0107*	0.0009	0.0862***	0.0871***	0.90
	(0.0064)	(0.0071)	(0.0069)	(0.0073)	
Number of CAPE units passed	0.0368	-0.0297	0.7425***	0.7128***	0.62
	(0.0565)	(0.0598)	(0.0575)	(0.0628)	
Observations			224,448		
Panel B: Boys					
CSEC Performance					
Qualified for tertiary education	-0.0023	-0.0337***	0.0856***	0.0519***	<0.01
	(0.0126)	(0.0108)	(0.0105)	(0.0114)	
Number of CSEC subjects passed	0.0012	-0.1234*	0.1527**	0.0293	0.06
	(0.0803)	(0.0649)	(0.0598)	(0.0661)	
Observations			165,242		
CAPE Performance					
Earned Associate's degree	-0.0029	-0.0073	0.0631***	0.0558***	0.25
	(0.0058)	(0.0063)	(0.0071)	(0.0074)	
Number of CAPE units passed	-0.0555	-0.0291	0.5349***	0.5058***	0.61
	(0.0559)	(0.0573)	(0.0631)	(0.0654)	
Observations			146,890		

Notes: This table displays 2SLS estimates from the second stage equation (5) that result from estimating the Double Regression Discontinuity (DRD) model outlined in equations (5) - (6). The estimation sample includes individuals who at least took one CSEC subject. Column (1) displays estimates on the instrumented $\widehat{PATH}_{it}$ indicator. Column (2) displays estimates on the instrumented interaction $PATH_{it} \cdot \widehat{Attend}_{ijt}$. Column (3) displays estimated preferred school effects among PATH non-beneficiaries (i.e., the estimate on the instrumented $\widehat{Attend}_{ijt}$ indicator). Column (4) displays estimated preferred school effects among PATH beneficiaries (i.e., the addition of the estimates shown in columns (2) and (3)). Column (5) displays the p-value that results from testing the equality between estimates shown in columns (3) and (4). Estimated standard errors clustered at the individual level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A.7: Interactions between PATH and Preferred School Attendance (Examinations Taken On Time)

	$\widehat{PATH}_{it}$	$PATH_{it} \cdot \widehat{Attend}_{ijt}$	Preferred School Effects		
			Among PATH Non-beneficiaries	Among PATH Beneficiaries	p-value
	(β_1)	(τ)	(θ_1)	($\theta_1 + \tau$)	(3)=(4)
	(1)	(2)	(3)	(4)	(5)
Panel A: Girls					
CSEC Performance (After 5 Years of Secondary School)					
Took at least 1 subject	-0.0085	-0.0024	-0.0658***	-0.0682***	0.80
	(0.0108)	(0.0095)	(0.0084)	(0.0102)	
Qualified for tertiary education	-0.0023	-0.0148*	0.1566***	0.1417***	0.06
	(0.0072)	(0.0079)	(0.0080)	(0.0091)	
Observations			346,136		
CAPE Performance (After 2 Years of Postsecondary Studies)					
Took at least 1 unit	0.0016	0.0002	0.0844***	0.0845***	0.98
	(0.0074)	(0.0083)	(0.0087)	(0.0097)	
Earned associate's degree	0.0079*	-0.0014	0.0924***	0.0910***	0.82
	(0.0044)	(0.0059)	(0.0062)	(0.0067)	
Observations			307,522		
Panel B: Boys					
CSEC Performance (After 5 Years of Secondary School)					
Took at least 1 subject	0.0085	-0.0034	-0.0231**	-0.0265**	0.72
	(0.0104)	(0.0094)	(0.0108)	(0.0115)	
Qualified for tertiary education	0.0098*	-0.0344***	0.1255***	0.0911***	<0.01
	(0.0051)	(0.0063)	(0.0084)	(0.0088)	
Observations			317,901		
CAPE Performance (After 2 Years of Postsecondary Studies)					
Took at least 1 unit	0.0000	-0.0147**	0.0552***	0.0404***	0.02
	(0.0052)	(0.0064)	(0.0093)	(0.0094)	
Earned associate's degree	0.0018	-0.0130***	0.0590***	0.0460***	<0.01
	(0.0028)	(0.0039)	(0.0065)	(0.0066)	
Observations			285,474		

Notes: This table displays 2SLS estimates from the second stage equation (5) that result from estimating the Double Regression Discontinuity (DRD) model outlined in equations (5) - (6). Column (1) displays estimates on the instrumented $\widehat{PATH}_{it}$ indicator. Column (2) displays estimates on the instrumented interaction $PATH_{it} \cdot \widehat{Attend}_{ijt}$. Column (3) displays estimated preferred school effects among PATH non-beneficiaries (i.e., the estimate on the instrumented $\widehat{Attend}_{ijt}$ indicator). Column (4) displays estimated preferred school effects among PATH beneficiaries (i.e., the addition of the estimates shown in columns (2) and (3)). Column (5) displays the p-value that results from testing the equality between estimates shown in columns (3) and (4). Estimated standard errors clustered at the individual level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A.8: Interactions between PATH and Preferred School Attendance - Alternative Outcomes

	$\hat{PATH}_{it}$	$PATH_{it} \cdot \hat{Attend}_{ijt}$	Preferred School Effects		
			Among PATH Non-beneficiaries	Among PATH Beneficiaries	p-value
	(β_1)	(τ)	(θ_1)	$(\theta_1 + \tau)$	(3)=(4)
	(1)	(2)	(3)	(4)	(5)
Panel A: Girls					
CSEC Performance					
CSEC subjects taken	-0.0851	-0.0319	-0.0973	-0.1293*	0.62
	(0.0720)	(0.0648)	(0.0599)	(0.0707)	
CSEC subjects passed	-0.0501	-0.0485	0.0913*	0.0428	0.40
	(0.0615)	(0.0574)	(0.0549)	(0.0639)	
Observations			346,136		
CAPE Performance					
CAPE units taken	0.0149	-0.0320	0.7648***	0.7327***	0.56
	(0.0454)	(0.0548)	(0.0578)	(0.0634)	
CAPE units passed	0.0237	-0.0371	0.7939***	0.7568***	0.47
	(0.0419)	(0.0519)	(0.0546)	(0.0600)	
Observations			307,522		
Panel B: Boys					
CSEC Performance					
CSEC subjects taken	0.0888	-0.1478**	0.1406**	-0.0072	<0.01
	(0.0624)	(0.0590)	(0.0703)	(0.0738)	
CSEC subjects passed	0.0667	-0.1529***	0.1818***	0.0289	<0.01
	(0.0491)	(0.0499)	(0.0625)	(0.0650)	
Observations			317,901		
CAPE Performance					
CAPE units taken	-0.0227	-0.0937**	0.4784***	0.3847***	0.02
	(0.0317)	(0.0410)	(0.0640)	(0.0639)	
CAPE units passed	-0.0143	-0.0844**	0.4704***	0.3860***	0.02
	(0.0289)	(0.0375)	(0.0606)	(0.0607)	
Observations			285,474		

Notes: This table displays 2SLS estimates from the second stage equation (5) that result from estimating the Double Regression Discontinuity (DRD) model outlined in equations (5) - (6). Column (1) displays estimates on the instrumented $\hat{PATH}_{it}$ indicator. Column (2) displays estimates on the instrumented interaction $PATH_{it} \cdot \hat{Attend}_{ijt}$. Column (3) displays estimated preferred school effects among PATH non-beneficiaries (i.e., the estimate on the instrumented $\hat{Attend}_{ijt}$ indicator). Column (4) displays estimated preferred school effects among PATH beneficiaries (i.e., the addition of the estimates shown in columns (2) and (3)). Column (5) displays the p-value that results from testing the equality between estimates shown in columns (3) and (4). Estimated standard errors clustered at the individual level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A.9: Interactions between PATH and Preferred School Attendance - Alternative Outcomes measured On Time

	$\hat{PATH}_{it}$	$PATH_{it} \cdot \hat{Attend}_{ijt}$	Preferred School Effects		
			Among PATH Non-beneficiaries	Among PATH Beneficiaries	p-value
	(β_1)	(τ)	(θ_1)	$(\theta_1 + \tau)$	(3)=(4)
	(1)	(2)	(3)	(4)	(5)
Panel A: Girls					
CSEC Performance (After 5 Years of Secondary School)					
CSEC subjects taken	-0.0432	-0.0342	-0.0144	-0.0485	0.58
	(0.0658)	(0.0617)	(0.0582)	(0.0679)	
CSEC subjects passed	-0.0378	-0.0347	0.2534***	0.2187***	0.53
	(0.0558)	(0.0547)	(0.0533)	(0.0614)	
Observations			346,136		
CAPE Performance (After 2 Years of Postsecondary Studies)					
CAPE units taken	0.0231	-0.0221	0.7770***	0.7549***	0.68
	(0.0430)	(0.0533)	(0.0562)	(0.0615)	
CAPE units passed	0.0282	-0.0266	0.8062***	0.7796***	0.60
	(0.0399)	(0.0505)	(0.0533)	(0.0583)	
Observations			307,522		
Panel B: Boys					
CSEC Performance (After 5 Years of Secondary School)					
CSEC subjects taken	0.0557	-0.1516***	0.2772***	0.1256*	<0.01
	(0.0519)	(0.0518)	(0.0649)	(0.0675)	
CSEC subjects passed	0.0670	-0.1800***	0.3689***	0.1889***	<0.01
	(0.0413)	(0.0448)	(0.0582)	(0.0600)	
Observations			317,901		
CAPE Performance (After 2 Years of Postsecondary Studies)					
CAPE units taken	-0.0097	-0.1102***	0.5139***	0.4037***	<0.01
	(0.0293)	(0.0388)	(0.0617)	(0.0619)	
CAPE units passed	-0.0032	-0.0981***	0.5019***	0.4038***	<0.01
	(0.0269)	(0.0357)	(0.0587)	(0.0592)	
Observations			285,474		

Notes: This table displays 2SLS estimates from the second stage equation (5) that result from estimating the Double Regression Discontinuity (DRD) model outlined in equations (5) - (6). Column (1) displays estimates on the instrumented $\hat{PATH}_{it}$ indicator. Column (2) displays estimates on the instrumented interaction $PATH_{it} \cdot \hat{Attend}_{ijt}$. Column (3) displays estimated preferred school effects among PATH non-beneficiaries (i.e., the estimate on the instrumented $\hat{Attend}_{ijt}$ indicator). Column (4) displays estimated preferred school effects among PATH beneficiaries (i.e., the addition of the estimates shown in columns (2) and (3)). Column (5) displays the p-value that results from testing the equality between estimates shown in columns (3) and (4). Estimated standard errors clustered at the individual level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A.10: Interactions between PATH and Preferred School Attendance - Optimal Bandwidths and Linear Specification of Running Variables

	$\hat{PATH}_{it}$	$PATH_{it} \cdot \hat{Attend}_{ijt}$	Preferred School Effects		
			Among PATH Non-beneficiaries	Among PATH Beneficiaries	p-value
	(β_1)	(τ)	(θ_1)	$(\theta_1 + \tau)$	(3)=(4)
	(1)	(2)	(3)	(4)	(5)
Panel A: Girls					
CSEC Performance]					
Took at least 1 subject	0.0052	0.0176	-0.0229**	-0.0053	0.27
	(0.0081)	(0.0161)	(0.0115)	(0.0149)	
Qualified for tertiary education	0.0122	-0.0128	0.0838***	0.0710***	0.47
	(0.0096)	(0.0178)	(0.0122)	(0.0144)	
Observations			68,686		
CAPE Performance					
Took at least 1 unit	0.0046	0.0077	0.0939***	0.1016***	0.63
	(0.0090)	(0.0161)	(0.0112)	(0.0125)	
Earned associate's degree	0.0026	0.0018	0.0891***	0.0909***	0.86
	(0.0060)	(0.0101)	(0.0074)	(0.0078)	
Observations			58,013		
Panel B: Boys					
CSEC Performance					
Took at least 1 subject	0.0202**	-0.0296	0.0402**	0.0106	0.16
	(0.0096)	(0.0208)	(0.0175)	(0.0207)	
Qualified for tertiary education	0.0039	-0.0323**	0.1147***	0.0824***	0.02
	(0.0078)	(0.0143)	(0.0109)	(0.0111)	
Observations			67,661		
CAPE Performance					
Took at least 1 unit	0.0080	-0.0348***	0.0822***	0.0474***	<0.01
	(0.0059)	(0.0106)	(0.0089)	(0.0083)	
Earned associate's degree	0.0053*	-0.0183***	0.0474***	0.0291***	<0.01
	(0.0027)	(0.0050)	(0.0046)	(0.0041)	
Observations			61,860		

Notes: This table displays 2SLS estimates from the second stage equation (5) that result from estimating the Double Regression Discontinuity (DRD) model outlined in equations (5) - (6). Column (1) displays estimates on the instrumented $\hat{PATH}_{it}$ indicator. Column (2) displays estimates on the instrumented interaction $PATH_{it} \cdot \hat{Attend}_{ijt}$. Column (3) displays estimated preferred school effects among PATH non-beneficiaries (i.e., the estimate on the instrumented $\hat{Attend}_{ijt}$ indicator). Column (4) displays estimated preferred school effects among PATH beneficiaries (i.e., the addition of the estimates shown in columns (2) and (3)). Column (5) displays the p-value that results from testing the equality between estimates shown in columns (3) and (4). Estimated standard errors clustered at the individual level are shown in parentheses. For each school cutoff, optimal bandwidths of the relative GSAT score were derived following Calonico et al. (2017). The model was estimated with linear specifications for both $f(Score_{it})$ and $g(GSAT_{ijt})$. *** p<0.01, ** p<0.05, * p<0.10.

Table A.11: Interactions between PATH and Preferred School Attendance - Optimal Bandwidths and Quadratic Specification of Running Variables

	$\hat{PATH}_{it}$	$PATH_{it} \cdot \hat{Attend}_{ijt}$	Preferred School Effects		
			Among PATH Non-beneficiaries	Among PATH Beneficiaries	*p*-value
	(β_1)	(τ)	(θ_1)	($\theta_1 + \tau$)	(3)=(4)
	(1)	(2)	(3)	(4)	(5)
Panel A: Girls					
CSEC Performance]					
Took at least 1 subject	-0.0002	0.0098	-0.0079	0.0018	0.57
	(0.0105)	(0.0171)	(0.0134)	(0.0175)	
Qualified for tertiary education	0.0121	-0.0060	0.0802***	0.0743***	0.74
	(0.0123)	(0.0180)	(0.0146)	(0.0172)	
Observations			68,686		
CAPE Performance					
Took at least 1 unit	0.0039	0.0077	0.0809***	0.0885***	0.64
	(0.0114)	(0.0163)	(0.0136)	(0.0154)	
Earned associate's degree	0.0047	0.0024	0.0927***	0.0952***	0.81
	(0.0076)	(0.0101)	(0.0086)	(0.0093)	
Observations			58,013		
Panel B: Boys					
CSEC Performance					
Took at least 1 subject	0.0261**	-0.0272	0.0311	0.0039	0.21
	(0.0124)	(0.0217)	(0.0236)	(0.0270)	
Qualified for tertiary education	0.0040	-0.0346**	0.1257***	0.0911***	<0.01
	(0.0101)	(0.0142)	(0.0142)	(0.0148)	
Observations			67,661		
CAPE Performance					
Took at least 1 unit	0.0080	-0.0344***	0.1047***	0.0702***	<0.01
	(0.0076)	(0.0108)	(0.0116)	(0.0115)	
Earned associate's degree	0.0039	-0.0187***	0.0710***	0.0523***	<0.01
	(0.0036)	(0.0053)	(0.0067)	(0.0062)	
Observations			61,860		

Notes: This table displays 2SLS estimates from the second stage equation (5) that result from estimating the Double Regression Discontinuity (DRD) model outlined in equations (5) - (6). Column (1) displays estimates on the instrumented $\hat{PATH}_{it}$ indicator. Column (2) displays estimates on the instrumented interaction $PATH_{it} \cdot \hat{Attend}_{ijt}$. Column (3) displays estimated preferred school effects among PATH non-beneficiaries (i.e., the estimate on the instrumented $\hat{Attend}_{ijt}$ indicator). Column (4) displays estimated preferred school effects among PATH beneficiaries (i.e., the addition of the estimates shown in columns (2) and (3)). Column (5) displays the *p*-value that results from testing the equality between estimates shown in columns (3) and (4). Estimated standard errors clustered at the individual level are shown in parentheses. For each school cutoff, optimal bandwidths of the relative GSAT score were derived following Calonico et al. (2017). The model was estimated with quadratic specifications for both $f(Score_{it})$ and $g(GSAT_{ijt})$. *** p<0.01, ** p<0.05, * p<0.10.