NOVAFRICA
Working Paper Series

Conditional Cash Transfers and the Learning Crisis: Evidence from Tayssir Scale-up in Morocco

Jules Gazeaud
Universidade Nova de Lisboa and NOVAFRICA

Claire Ricard
CERDI, CNRS, Université Clermont Auvergne and FSJES-Aïn Chock, University Hassan II

ISSN 2183-0843
Working Paper No 2102
February 2021

Conditional Cash Transfers and the Learning Crisis: Evidence from Tayssir Scale-up in Morocco*

Jules Gazeaud † Claire Ricard ‡

February 2021

Abstract

We use a regression discontinuity design in rural Morocco to study whether the enrollment gains from conditional cash transfer programs translate into learning benefits. Unlike most previous studies, we estimate the effects of a sustained exposure during whole primary school. We find small and seemingly negative effects on test scores at the end-of-primary school exam. Concomitant increases in class size suggest that the program constrained learning by putting additional pressure on existing resources in beneficiary areas. These results are particularly relevant for settings where transfers are geographically targeted with no measures to absorb the extra influx of students.

JEL Classification: I21, I38, J24, O12, O15.
Keywords: Conditional cash transfers, Learning outcomes, Program scale-up, Morocco, Tayssir.

*Thanks to Abdelilah Abbaia, Martine Audibert, Cátia Batista, Hamidou Diallo, Lydie Drouard, Eric Edmonds, João Firmino, Abdelhaq El Hayani, Elhabib Kinani, Francesca Marchetta, Sandrine Mesplé-Somps, Teresa Molina-Millán, Fouzi Mourji, Olivier Santoni, Olivier Sterck and seminar participants at Observatoire National du Développement Humain in Rabat, Journées Doctorales du Développement, Agence Française du Développement, Moroccan Ministry of Education, and Nova School of Business and Economics for helpful comments. We are very grateful to the Moroccan Ministry of Education (in particular Elhabib Kinani) for providing access to the *MASSAR* dataset, and to Nourddine Bouhemaid, Mohamed Elarrassi, Abdeljaouad Ezzrari, and Younes Nail for helping us with extracting and understanding the data. Gazeaud acknowledges the support received from Fundação para a Ciência e a Tecnologia (UID/ECO/00124/2013 and Social Sciences DataLab, Project 22209), POR Lisboa (LISBOA-01-0145-FEDER-007722) and POR Norte. Ricard acknowledges the support received from the Agence Nationale de la Recherche of the French government through the program "Investissements d'avenir" (ANR-10-LABX-14-01). The usual disclaimers apply. Gazeaud: jules.gazeaud@novasbe.pt; Ricard: claire.ricard@etu.uca.fr.

†NOVAFRICA, Nova School of Business and Economics, Universidade NOVA de Lisboa.

‡CERDI, CNRS, Université Clermont Auvergne & FSJES-Aïn Chock, University Hassan II.

1 Introduction

The last decades have witnessed impressive progress in access to education in developing countries. The share of primary school-aged children out of school in low- and middle-income countries has considerably declined between 1998 and 2018, from 19 percent to 9 percent.[1] As part of the global efforts to raise basic education, targeted conditional cash transfer (CCT) programs have been proven particularly effective (see e.g. Fiszbein and Schady, 2009; Baird et al., 2011, 2014; Garcia and Saavedra, 2017). CCT programs provide regular cash transfers to poor families contingent on specific behaviors such as school enrollment and regular attendance. These programs started in the late 1990s in Mexico and quickly became the public policy of choice to fight poverty and low enrollment in the developing world. Today, at least 63 countries operate CCT programs, often at a national scale (Bastagli et al., 2016).

Despite robust evidence of positive effects on enrollment, less is known about their impacts in terms of learning achievement. This gap in the literature is problematic because the decision to implement CCT programs is generally motivated by the ultimate objective of enhancing human capital and breaking the inter-generational transmission of poverty. Positive effects on enrollment point to the potential for such longer-term benefits, but recent studies suggest that "schooling is not learning" (Pritchett, 2013), and that education systems in developing countries can produce alarmingly low test scores in international assessments (Kremer et al., 2013; Bold et al., 2017).[2] Although most children in the developing world are now enrolled in school, less than half are able to read and understand a simple text at the age of 10 (World Bank, 2019). Documenting whether enrollment gains from CCT programs translate into sustained learning benefits therefore seems of crucial importance. However, credibly estimating such effects is challenging, as successful interventions often expand to the control group after the evaluation period.

This paper aims to provide new evidence on the effects of CCT programs on learn-

[1] Source: World Bank's World Development Indicators.

[2] This situation has been characterized as a "learning crisis" by the international community (World Bank, 2018).

ing in primary school. We study this question in the context of Morocco where the average performances of students are very low compared to international standards: the country ranked 73 over 78 countries in the latest PISA assessment. Morocco's conditional cash transfer program, known as *Tayssir*, began operating in 2008 and quickly became the flagship education policy of a government strongly committed to fight school dropout. A randomized evaluation of the pilot found large gains in school participation (Benhassine et al., 2015). Following this evaluation, the program was scaled-up and targeted all municipalities with poverty rate above 30% (and all households with children aged 6-15 within these municipalities). Between 2010 and 2018, up to 800,000 children in 434 municipalities received regular cash transfers that were conditional on school enrollment and attendance. Annual transfers were equivalent to between 6% (in grades 1 and 2) and 10% (in grades 5 and 6) of the average annual spending per capita. Because transfer allocation has remained remarkably stable between 2010 and 2018, this program offers an ideal setup to study the effects of conditional cash transfers on outcomes such as learning which require cumulative investments.

We exploit the fact that eligibility was determined on the basis of the poverty rate of each municipality to conduct a fuzzy regression discontinuity analysis. Municipalities were eligible to receive *Tayssir* if they had a poverty rate above 30% according to the poverty map of 2004. We find that this 30% cut-off is highly predictive of *Tayssir* allocation. We use novel administrative data from the information system of the Ministry of Education of Morocco to explore the effects of the program on enrollment, achievement, and learning. This system was launched in 2013. It provides unique identification for all students in Morocco and is cited by Abdul-Hamid (2017) as an example of successful education management information system – although to our knowledge it has never been used by academics. To conduct this research, Morocco's Ministry of Education granted access to anonymized information for all primary school students in municipalities with poverty rates between 20% and 40% in 2004. Overall, our sample contains over 8,700 schools and 900,000 students in each year of the 2013-2018 period.

We first assess program effects on dropout rates and check for possible differences with Benhassine et al. (2015) estimates on the pilot. This exercise is important because impacts may differ as programs are taken to scale (Banerjee et al., 2017). We find that the program reduced dropout in all grades of primary school: the grade-specific dropout rate decreased by 1.3 percentage points on average (which corresponds to more than one third of the sample mean). We find larger effects for girls. These estimates are fully consistent with those of Benhassine et al. (2015) on the pilot.

We then study the impacts of the program on test scores at the graduation exam at the end of primary school. We correct for selection issues arising from differential dropout rates by imputing test scores corresponding to different degrees of selection into dropout. These estimates provide lower and upper bounds to the true effect of the program on test scores. The results indicate that continued exposure to the program during whole primary school did not lead to significant improvements in test scores and in fact reduced the scores of boys by between 0.10 and 0.18 SD.

We discuss different mechanisms that could be driving these results and we test whether the program constrained learning by putting additional pressure on existing educational resources in beneficiary areas. We identify a sizable and positive impact of the program on class size which in turn had negative effects on graduation scores. Class size in beneficiary areas increased by as much as 3.6 students in grade 6, which corresponds to 12% of the average class size in the sample (30.7 students). We use an IV strategy to examine whether this increase in class size had negative effects on test scores. More specifically, we instrument class size by the eligibility cutoff and find that the increase in class size induced by the program had negative effects on test scores within the sample of boys: increasing class size by one student led to test scores 0.03 to 0.05 SD lower for boys in beneficiary municipalities.

More reassuringly, we find that the program increased the probability of enrollment in secondary school by 4.5 percentage points (equivalent to a 7% increase relative to the sample mean of 63.8%), with stronger effects for girls (7 percentage points or 11% relative to the sample mean of 64.4%).

These results contribute to three strands of the literature. First, they provide novel insights on the longer-term effects of conditional cash transfer programs. In a recent review, Molina Millán et al. (2019) document positive effects on grades completion and school attainment,[3] but mixed evidence on learning. Evaluations in Nicaragua (Barham et al., 2017) and Colombia (Duque et al., 2019) find positive impacts on test scores, contrasting with non-significant effects reported for programs in Mexico (Behrman et al., 2005; Dustan, 2020), Colombia (Baez and Camacho, 2011), Cambodia (Filmer and Schady, 2014), Nicaragua (Barham et al., 2018) and Malawi (Baird et al., 2019).[4] Our paper adds to this literature by estimating the effects of a flagship program implemented by the government of Morocco. Unlike most previous studies, which evaluated relatively limited exposures to cash transfers, in this paper we focus on effects corresponding to an exposure during whole primary school. We find that the program did not lead to learning gains in primary school and actually had negative effects on boys test scores. In addition, we emphasize a new channel through which the program might have constrained learning: by increasing class size in municipalities targeted by the program. This channel of impact may be particularly relevant in settings where transfers are geographically targeted with no particular measures to absorb the extra influx of students.

Second, this paper contributes to our understanding of the effect of class size on students performances. Students assigned to smaller classes typically exhibit higher test scores (Krueger, 1999; De Giorgi et al., 2012) and benefit from welfare gains in the long-term (Chetty et al., 2011; Fredriksson et al., 2013). However, results vary across contexts and have been more modest in the case of rule-induced class size reductions (Urquiola, 2006; Angrist et al., 2019). They also often depend on peer characteristics (Hoxby, 2000; Dobbelsteen et al., 2002; Firmino et al., 2018). For example, in Kenya, children randomly assigned to classes with more high-performing peers achieved higher tests (Duflo et al., 2011). To the best of our knowledge, we provide the first estimate of the

[3]For example, Cahyadi et al. (2020) find a positive effect of CCT programs on high school completion rates in Indonesia – found also in Mexico (Parker and Vogl, 2018), Colombia (Barrera-Osorio et al., 2019), and Honduras for non-indigenous populations (Molina Millán et al., 2020).

[4]Evidence on unconditional transfers points to either null or negative effects on test scores (Ponce and Bedi, 2010; Akresh et al., 2013; Araujo et al., 2017; Avitabile et al., 2019).

effects of variations in class size induced in the context of a CCT program. Our results suggest that these variations can have a negative effect on children test scores.

Finally, we contribute to the emerging literature on the scale-up of development programs by analyzing whether *Tayssir* intervention in Morocco – whose pilot has been proven particularly successful to increase enrollment (Benhassine et al., 2015) – achieves similar effects once implemented at a national scale. While recent studies suggest that effects could be smaller (Banerjee et al., 2017; Muralidharan and Niehaus, 2017; Bold et al., 2018; Vivalt, 2020), we find that in this setting the effects of the program remained very stable despite the ten-fold increase in the number of beneficiaries and the expansion to numerous new locations. If anything, this evidence is reassuring regarding the ability of pilot evaluations to produce insightful estimates of at-scale impacts.

The rest of the paper structures as follows: section 2 presents the background of our study, section 3 describes the data and the main variables of interest, section 4 presents the empirical strategy, section 5 shows the results. The last section concludes.

2 Background

2.1 Education in Morocco

Public education in Morocco is completely governed by the Ministry of Education (MENFPESRS or *Ministère de l'Education Nationale, de la Formation Professionnelle, de l'Enseignement Supérieur et de la Recherche Scientifique*). Private education remains low despite a sizable expansion during the past decade (especially in urban areas).[5] In rural areas, schools are typically organized into clusters consisting of a central, relatively well-resourced school and several smaller satellites. The latter are often one-room schools with one teacher. According to Soumaya et al. (2018), the quality of ed-

[5]The share of students enrolled in private schools has doubled between 2007 and 2017, from 8.4% to 16.7% for primary education, and from 4% to 9.3% for lower secondary education (source: Atlas territorial de l'enseignement privé 2018). In 2018, 61.4% of the students enrolled in private primary education were living in the most urbanized regions of Casablanca-Settat, Rabat-Salé-Kénitra and Fès-Meknès (Ministry of Education, 2018).

ucation in these schools is particularly low. Primary schools include grades 1 through 6 (generally attended by children aged 6 to 12) and lower secondary schools include grades 7 through 9 (generally attended by children aged 13 to 15). Each cycle ends by a final graduation exam. Our article focuses on the exam at the end of primary school, which covers Arabic, Islamic education, French and Math. To access secondary education, students are required to get at least 5/10 at this exam. Half of the grade relies on continuous assessment, a quarter on a school exam at the end of the first semester, and the last quarter on a provincial exam at the end of the year. In 2014, 84% of the students who took this exam passed it (Soumaya et al., 2018).

The average performances of Moroccan students are very low compared to international standards. For example, Morocco ranked 73 over 78 countries in the latest PISA assessment. According to Soumaya et al. (2018), there are several factors that explain the low performances of Morocco in terms of learning. First, teachers often lack formal education and pedagogical skills: 40% of fourth-grade teachers have no formal post-secondary education (international average: 3%),[6] and 66% have not participated in any training during the past two years (international average: 16%) (CSEFRS, 2019). Second, learning is still largely based on memory even if a new curriculum prioritizing skills-based learning was adopted in 2002. Third, instruction is provided in unified modern Arabic whereas the mother tongue of most students is *Darija* (Moroccan Arabic) or *Amazigh* (Moroccan Berber).[7] Finally, enrollment in early childhood education remains relatively low. During school year 2015-2016, only 43% of children aged 4 to 5 were enrolled in early childhood education, of which 39.5% were enrolled in religious schools.[8]

[6] Until 2007, only a higher secondary school level was required to be a teacher. Since 2007, at least three years of post-secondary education in a Regional Center for Education and Training Professions are required.

[7] People typically communicate using Darija (90.9%) or Amazigh (26.8%) but not Modern Arabic (Ministry of Education, 2018). By 2030, Amazigh should become an instruction language (Soumaya et al., 2018).

[8] Source: Ministry of Education, 2015-2016 statistical yearbook.

2.2 The Tayssir conditional cash transfer program

Tayssir began operating in 2008 and quickly became the flagship education program of a government strongly committed to fight school dropout. *Tayssir* provides bi-monthly cash transfers to the parents of children aged 6-15 conditional on school enrollment and regular attendance. The monthly transfer per child is increasing with grade: from US$8 in grades 1 and 2, to US$10 in grades 3 and 4, to US$13 in grades 5 and 6, and to US$18 in grades 7 to 9.[9] Transfers are perceived for all school months (10 months) in which an eligible child attend school regularly (i.e. at most 4 absences in primary school, and 6 absences in lower secondary school). Annual transfers for children complying with this condition correspond to between 6% (in grades 1 and 2) and 13% (in grades 7 to 9) of the average annual spending per capita in rural areas.[10] Transfers are restricted to a maximum of three children per household. Parents can withdraw the cash transfers at the local post office, or, in remote areas, upon the visit of mobile cashiers. During school years 2015/16 and 2016/17, the program was plagued by liquidity issues, causing important delays in payments (L'Economiste, 2017).

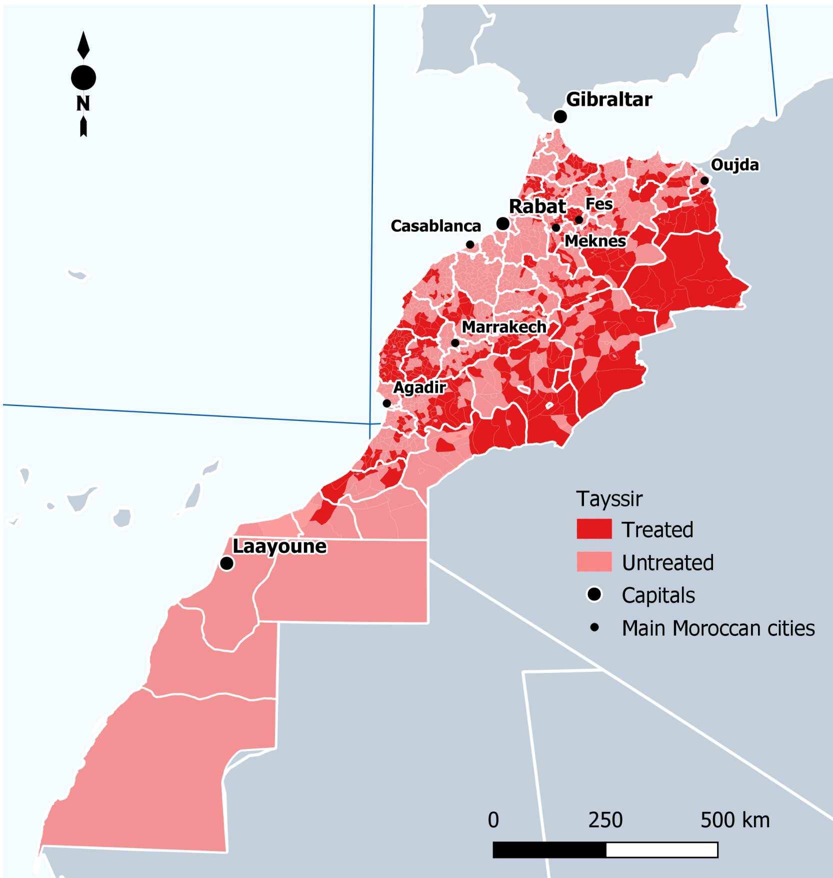
Started as a pilot in 2008, *Tayssir* was then rolled-out to 434 poor, rural municipalities in 2010. By the end of 2010, it had already expanded to cover 609,000 children. Municipalities were eligible to receive *Tayssir* if they had a poverty rate above 30 percent according to the poverty map of Morocco's *Haut Commissariat au Plan*.[11] The treatment status of Morocco's 1,687 municipalities is shown in Figure 1. Initially all primary school children within treated municipalities were eligible for the transfers. In subsequent years, in order to follow the various cohorts of beneficiaries, the program was gradually expanded to lower secondary school (i.e. grades 7 to 9), ultimately reaching around 800,000 children annually. Most recently, during the 2018/19 school year, an important reform enacted a fourfold increase in budget and the expansion to benefi-

[9]We use the 2008 exchange rate of 7.75 Moroccan dirhams for one US dollar (but this rate has remained relatively stable over time).

[10]Authors' calculations using data from the *Enquête Nationale sur la Consommation et les Dépenses des Ménages 2013/14*. Because the program was targeted towards the poorest municipalities, these estimates should be interpreted as lower bounds.

[11]Eligible municipalities were also supposed to have school dropout rates above 8 percent, but in practice this condition was never enforced (according to our discussions with program officials).

Figure 1: Municipalities receiving *Tayssir*



Notes: This map depicts the treatment status of the different municipalities. Plain white lines correspond to provinces' administrative boarders. Dotted white lines correspond to municipalities' administrative boarders.
Source: Authors' elaboration using treatment data from the *Haut-Commissariat au Plan*.

ciaries in new municipalities using proxy means testing (Médias24, 2018). The present paper focuses on the period prior to this reform.

Benhassine et al. (2015) evaluate the impacts of *Tayssir* pilot with a special focus on the importance of conditions and recipient gender. The authors compare households assigned to four variants of *Tayssir* and to a control group: (i) "labeled" cash transfers (LCT) to mothers, (ii) "labeled" cash transfers to fathers, (iii) conditional cash transfers to mothers, and (iv) conditional cash transfers to fathers. "Labeled" cash transfers were not conditional on school attendance but explicitly labeled as assistance for education costs.[12] Outcomes were measured two years after the start of the program. The authors find large and similar impacts on school participation under all versions of the program. The dropout rate decreased by 62 percent over the 2-year period for children in households receiving CCT to fathers. The reduction in dropout was somewhat larger for girls (8.3 p.p. from a base of 12 percent) than for boys (4.8 p.p. from a base of 8.3 percent). Interestingly, the authors document that the program increased parents' beliefs about the returns to education, suggesting that the decrease in dropout may operate through an information effect whereby transfers are interpreted by parents as a signal of the value of education. The authors also administered an ASER arithmetic test to a random subset of children at endline (i.e. after two years of transfers).[13] They find small, non-significant effects on standardized test scores (0.04 SD for CCT to fathers), and no evidence of heterogeneous effects by gender. Because the test was administered at home, these estimates do not suffer from selection issues due to differential dropout rates across experimental groups. However, measuring effects within the first two years of the program may be insufficient to capture effects on outcomes such as learning which typically require cumulative investments over an extended period. The main objective of this paper is to analyze whether program effects on learning emerge for children exposed to six years of cash transfers (i.e. during all their primary educa-

[12] In what follows, we discuss only the effects of CCT to fathers but results are very similar for LCTs and CCT to mothers. The version that was ultimately rolled-out consists in a CCT given to one of the parents.

[13] ASER is a mathematics test measuring the ability of children to perform basic arithmetic such as recognizing a one-digit or two-digit number, performing a subtraction, and performing a division.

tion). In addition, we assess whether positive effects on enrollment persist at scale.

3 Data

This study collates data from three primary sources. First, we rely on the poverty rate in the 2004 poverty map built by the Morocco's *Haut-Commissariat au Plan* (this rate was used to determine eligibility to the program). Second, we rely on *Aiddata* geoquery tool to compile pre-program data on nighttime lights, population density, land occupation, and distances. These data are used to conduct balance checks. Third, we use data from *MASSAR*, the information system of Morocco's Ministry of Education, to construct the outcomes of interest. The rest of this section gives more details on *MASSAR* and on the outcomes of interest.

3.1 The *MASSAR* database

MASSAR was officially launched in 2013 with the aim of providing unique identification for all students in Morocco. It is cited by Abdul-Hamid (2017) as an example of successful education management information system. In *MASSAR*, teachers and school directors enter information on students (age, gender, performances) using a dedicated website or mobile application. Every student receives a unique ID number for the entire duration of her education which allows to track them throughout their schooling experience even if they migrate or move to a different school. *MASSAR* also includes information at the school level, such as the municipality of the school, the number of classes per level, the number of teachers per class, and the number of rooms.

To conduct this research, Morocco's Ministry of Education granted access to a subset of *MASSAR*. In particular, we obtained anonymized information for all primary school students in municipalities with poverty rates in the range [20%, 40%] in 2004.[14]

[14] We took into account both practical and methodological considerations. On the practical side, it would have been particularly challenging for our partners at the Ministry of Education to extract data for more students. On the methodological side, given our regression discontinuity design, it seemed preferable to sample within a relatively narrow range around the targeting cut-off in order to maximize

ID numbers were used to follow students even if they had migrated or moved to schools outside the 20-40% interval within the 2013-2018 period (this is crucial to avoid considering as dropouts the students who moved to schools outside the sample). Map A1 shows that the set of municipalities with poverty rates inside the 20-40% interval is relatively well scattered across Morocco. The only provinces with no sampled municipalities are those in Western Sahara and in the greater Casablanca-Rabat area.

According to Abdul-Hamid (2017) and to our own discussions with Moroccan officials, *MASSAR* covers the universe of students. Table 1 further shows that the number of students and schools in our sample has remained remarkably stable over time. This is consistent with the fact that *MASSAR* was immediately operational at scale. Overall, our sample contains about 8,700 schools and 900,000 students each year. Looking at test scores at the graduation exam, we see many missing values in the first two years of *MASSAR* (40.1% in 2013/14; 31.1% in 2014/15). More reassuringly, the share of missing test scores in the following years was much lower: one percent in both 2015/16 and 2016/17. There are several reasons why scores are missing, including absence on the day of the exam, or teachers/directors not entering information in *MASSAR*.[15] To limit the issue of missing scores, we restrict the sample to years 2015/16 and 2016/17.

3.2 Outcome variables

Using data from *MASSAR*, we construct outcome variables related to three main domains: dropout, learning, and attainment. This subsection describes in details the construction of these variables. Table A1 and Maps A2a to A2d provide descriptive statistics.

Dropout We consider a student as a dropout in schooling year *t* if he or she was enrolled in year *t-1* but not in year *t*. This means that a student who stopped attending

statistical power (Cattaneo et al., 2019b).

[15]The higher prevalence of missing scores in 2013/14 and 2014/15 may reflect potential issues in the first years of *MASSAR*, including teachers and school directors struggling to get acquainted with the new system, or initial protests from students and teachers (Médias24, 2014). Qualitative research by Ennaji (2018) highlights that many students protested against *MASSAR* because they believed it would change the grading system and might therefore increase school failure.

Table 1: Coverage of *MASSAR*

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) ALL
Number of schools							
2013/14	8,482	8,456	8,303	8,248	8,111	8,040	8,742
2014/15	8,476	8,464	8,445	8,285	8,234	8,159	8,765
2015/16	8,449	8,492	8,460	8,445	8,276	8,261	8,654
2016/17	8,479	8,508	8,534	8,492	8,462	8,374	8,721
Number of children							
2013/14	159,980	148,544	152,138	149,999	144,752	139,617	895,030
2014/15	157,756	150,538	150,134	146,220	145,950	152,451	903,049
2015/16	162,794	150,647	151,999	146,252	143,268	156,686	911,646
2016/17	170,431	155,083	152,359	147,736	143,009	154,135	922,753
Share of missing test scores (excluding dropouts)							
2013/14	.	.	.	.	.	.401	.
2014/15	.	.	.	.	.	.311	.
2015/16	.	.	.	.	.	.010	.
2016/17	.	.	.	.	.	.010	.

Notes: Authors' elaboration using data from *MASSAR*. Sample restricted to primary education in municipalities with poverty rates between 20% and 40%.

school during a given year counts as a dropout only if he or she is not re-enrolled in the following year. Table A1 reports the dropout rates by schooling year and grade. Considering all years and grades, we estimate an average annual dropout rate of 3.3%.[16] The dropout rate has remained very stable over the period, consistent with the fact that Morocco did not experience major shocks during any of these years. Looking at grade-specific dropout rates, we see that higher grades are associated with more dropout, and that this relationship holds for all years. Taken together, these patterns provide some reassurance about the consistency of *MASSAR* over time. If anything, data from 2014/15 produce larger dropout rates, especially in the first grades. Because dropouts in 2014/15 are derived using enrollment data from 2013/14 (the first year of service of *MASSAR*), this could be a symptom of an imperfect roll-out of *MASSAR*. As mentioned in Section 3.1, in order to limit data quality concerns, we do not use data from 2014/15 to investigate the effects of *Tayssir*. Analysis are restricted to the years 2015/16 and 2016/17, for which data produce patterns of dropout rates that are hardly distinguishable. Overall, we are confident that this data capture an accurate account of the

[16]This corresponds to the annual dropout rate in primary school. In order to approximate the overall dropout rate in primary school, one should multiply this rate by the average number of years spent in primary school before graduation (a quantity that depends on the prevalence of grade repetition and grade skipping).

reality.

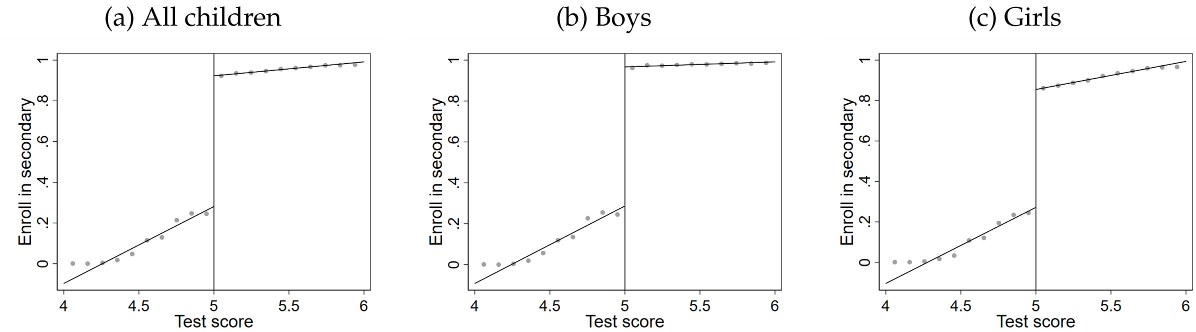
Learning We measure learning using the score obtained at a high-stake exam (called *Certificat d'études primaires*) administered to all children in grade 6. This exam assesses student knowledge in four areas: Arabic, Islamic education, French, and Mathematics. We focus on the part of the exam administered at the end of the year because it is the only part that is common at the provincial level and therefore allows for comparisons between students from different schools and municipalities.[17] In each province, questions to assess students' learning are determined by a local committee composed of school inspectors and teachers designated by the regional authorities. Tests are graded anonymously and centrally by provincial teachers. We define our outcome as the average test score standardized relative to the control group within each province. In theory, students should get an overall score of at least 5 (over 10) to access secondary school. To explore the extent to which this rule is enforced in practice, we relate the overall score to enrollment in secondary school the following year (Figures 2a to2c). While the figures indicate a clear discontinuity in access to secondary schools for scores just above or below 5, it appears that a non-negligible share of students with scores below 5 access secondary school. In addition, despite scores above 5, some students (girls in particular) do not access secondary school – a likely consequence of the high dropout rates observed in grade 6 for girls (Table A2).

Educational attainment We measure educational attainment using two dummies indicating (i) graduation from primary school, and (ii) enrollment in secondary school. *MASSAR* includes no explicit data on graduation, however, as shown in Figure 2, the score obtained at the exam in grade 6 provides a relatively good proxy. We define graduation in a theoretical sense, that is students with test scores above 5. Enrollment in secondary school is derived from the changes in *MASSAR* over time, by observing

[17]Note, however, that the bulk of the exam (i.e. three quarters) is composed of the grades obtained during the school year through continuous assessment. This part of the exam is poorly suited to estimate the effect of *Tayssir* because of the absence of standardization. Different teachers are likely to use different tests (and to grade differently). Moreover, there is no variation in treatment condition within the group of students assigned to a teacher.

whether students in grade 6 are enrolled in secondary school the following year.

Figure 2: Selectivity of grade 6 examination



Notes: Each figure represents the probability of enrolling in secondary school in year t+1 depending on the score obtained at grade 6 final exam in year t. Authors' elaboration using data from *MASSAR*. Sample restricted to primary education in municipalities with poverty rates between 20% and 40%.

4 Empirical strategy

As mentioned in Section 2.2, only children in municipalities with poverty rates above 30 percent in 2004 were eligible to *Tayssir*. We use a regression discontinuity (RD) design to compare students in municipalities above and below this eligibility threshold. A key assumption is that students in municipalities that just qualify for *Tayssir* (i.e. municipalities whose poverty rate is just above 30%) are sufficiently similar to those in municipalities that just miss out *Tayssir* (i.e. municipalities whose poverty rate is just below 30%). We test for the presence of discontinuities around the 30% poverty rate threshold using predetermined variables that are measured at the municipality level.[18] Coefficients are small and non-significant at conventional levels, providing some reassurance on the validity of our design (Table A3).

Figure 3a illustrates the discontinuity in treatment allocation in the neighborhood of the cut-off using treatment data and poverty rates at the municipality level. The 30 percent cut-off is highly predictive of *Tayssir* allocation, although the discontinuity is not perfect: some municipalities with scores below the cut-off receive *Tayssir* and some municipalities with scores above the cut-off do not receive *Tayssir*. This means that our

[18]We lack pre-program data at the student level because *MASSAR* was only launched in 2013.

RD design is fuzzy. Map A3 distinguishes '*complying*' municipalities from the '*never takers*' (i.e. eligible municipalities not receiving *Tayssir*) and the '*always takers*' (i.e. non-eligible municipalities receiving *Tayssir*). Compliance is somewhat lower in the North of the country (in the Rif region) where the share of always takers is particularly high.

We use the following simple linear model to estimate treatment effects:

$$Y_{it} = \lambda_0 + \lambda_1 Tayssir_i + \lambda_2 Z_i + \lambda_3 Tayssir_i \times Z_i + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome of children i in school year t, $Tayssir_i$ is an indicator variable for whether student i is in a treated municipality, Z_i is the continuous poverty rate of the student's municipality, and ε_{it} is the error term. Because the cut-off is fuzzy, we instrument $Tayssir_i$ with $Above_i$, that is an indicator for being above the eligibility cut-off of 30%. The parameter of interest, λ_1, corresponds to the local average treatment effect (LATE) of *Tayssir*. It can be interpreted as the causal effect of receiving *Tayssir* in complying municipalities near the cut-off.[19]

Our baseline strategy for estimating equation (1) makes use of the full sample of students in primary school described in Section 3.1. We do not rely on Imbens and Kalyanaraman (2012)'s or Calonico et al. (2014)'s data-driven bandwidth selection methods because our sample is already restricted to students in municipalities close to the cut-off, and because such methods have poor behaviors when the underlying bias is close to zero (Cattaneo et al., 2019a).[20] In line with Cattaneo and Vazquez-Bare (2016) and Gelman and Imbens (2019), we chose a simple linear model because higher order polynomials can lead to erratic behavior of the estimator at the cut-off. Finally, following Cattaneo et al. (2019a), we weight observations using a triangular kernel function, and account for within-cluster data dependence by clustering standard errors at the mu-

[19] To the best of our knowledge, there were no other programs or government interventions in which eligibility was determined using the municipal poverty rate. Eligibility to RAMed – Morocco's nationwide medical assistance program – has been targeted at the household level through proxy means testing (Cottin, 2019). Other demand side education programs do not depend on the municipal poverty rate. In particular, the initiative *Un million de cartables* – which provides school bags and other school supplies – is universal. Boarding schools and school feeding programs depend on students' characteristics (remoteness, number of siblings, academic performances, etc.).

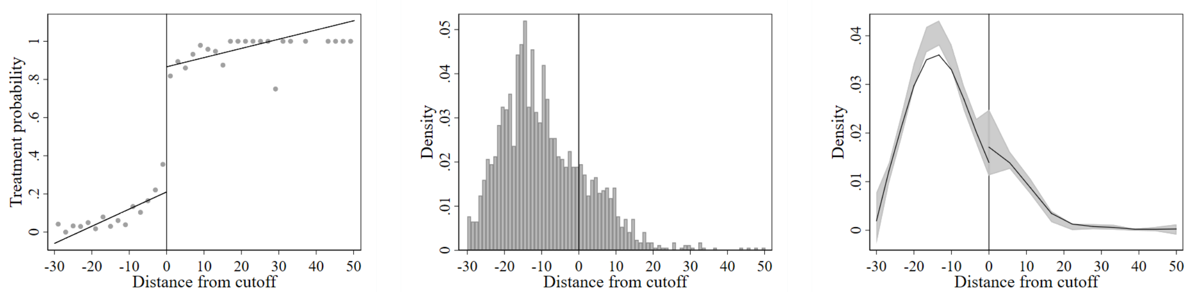
[20] As mentioned in Section 3.1, our sample is restricted to students in municipalities with poverty rates in the range [20%, 40%].

nicipality year level using the nearest-neighbor estimator. We show that results are largely robust to (i) weighting observations using a uniform kernel function, (ii) adding municipality controls accounting for baseline characteristics (nighttime lights, population density, share of cropland, distance to road, land occupation, and travel time to the closest city), (iii) using a quadratic polynomial approximation, (iv) narrowing the bandwidth to 7.5 p.p. on either side of the cut-off, and (v) narrowing the bandwidth to 5 p.p. on either side of the cut-off (see Tables A4,A5, A6 and A7).

A key assumption for RD designs to provide unbiased causal estimates is that of no sorting around the eligibility threshold. In our setting, precise manipulation around the threshold is unlikely because poverty rates were determined prior to *Tayssir* implementation (and prior to the announce of the elegibility threshold). To confirm this, we check the density of municipalities across poverty rates (Figure 3b) and test for the presence of a discontinuity at the cut-off using McCrary (2008)'s test (Figure 3c). Overall, we see no jump in the density at the cut-off and McCrary test does not reject the null of no discontinuity (p-value=0.33).

Figure 3: First stage and distribution of the running variable

(a) First stage (b) Histogram (c) McCrary test



Notes: Figure 3a shows a linear prediction of *Tayssir* receipt on municipality poverty scores, run separately on each side of the normalized eligibility cut-off. Municipalities are sorted into bins of width of 2 percentage points. Figure 3b displays the density of the running variable across municipalities. Figure 3c features a graphical representation of the McCrary (2008) test of no discontinuity in the running variable at the cut-off (p-value=0.33). The vertical lines indicate the normalized eligibility cut-off.
Source: Authors' calculation using poverty data from Morocco's *Haut-Commissariat au Plan* and treatment data from *MASSAR*.

5 Results

We start by presenting program effects on school dropout. Then, we present program effects on test scores, discussing potential issues arising from differential dropout rates and our proposed solution. Finally, we report effects on educational attainment. We are interested in the effect of receiving *Tayssir* and therefore focus the discussion on LATE estimates (λ_1 in Eq. 1). However, following Cattaneo et al. (2019a), we also report graphical presentations of reduced-form estimates to provide more transparency on our RD design.

5.1 Impacts on school dropout

Table 2 shows the results from fuzzy RD regressions. Columns 1 to 6 report the results for each grade. Column 7 displays the results for all grades. Panel A shows estimates for all children, while panels B and C show estimates for boys and girls respectively. Figures 4a-4c show reduced-form estimates.

The estimates reveal that the program led to a significant reduction in dropout rates. Point estimates imply that the average grade-specific dropout rate in primary school decreased by 1.3 p.p. (a 41% reduction relative to the average dropout rate in the sample). As expected, we see a positive correlation between school drop out and the poverty rate on each side of the eligibility cut-off (Figure 4a). Looking at effects by grade, we find small and non-significant effects in grade 1, but negative and statistically significant effects in higher grades. Higher grades are consistently associated with larger absolute reductions in dropout rates: from 0.6 p.p. in grade 2, to roughly 1 p.p. in grades 3 and 4, to 1.8 p.p. in grade 5, and to 3.1 p.p. in grade 6. Regressions by gender outline that negative effects on dropout are stronger for girls. Girls benefiting from *Tayssir* are on average 1.8 p.p. less likely to drop out from schools (a 50% reduction compared to the sample mean). Boys benefiting from *Tayssir* are on average 1 p.p (or 36%) less likely to drop out from schools. The largest absolute reductions are found for boys and girls in the highest grades.

Table 2: Fuzzy RD analysis of dropout effects around *Tayssir* eligibility threshold

	Dep Var: Drop-out						
	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) All Grades
Panel A. All students							
LATE	0.000	-0.006***	-0.010***	-0.011***	-0.018***	-0.031*	-0.013***
	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)	(0.017)	(0.004)
Mean drop-out	0.005	0.011	0.013	0.022	0.040	0.101	0.032
Observations	333,225	305,730	304,358	293,988	286,277	310,821	1,834,399
Panel B. Boys							
LATE	0.001	-0.006**	-0.008**	-0.011***	-0.017***	-0.014	-0.010***
	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.015)	(0.004)
Mean drop-out	0.006	0.011	0.013	0.021	0.036	0.084	0.028
Observations	174,878	160,229	160,590	155,123	151,247	164,642	966,709
Panel C. Girls							
LATE	-0.001	-0.007**	-0.011***	-0.011**	-0.020***	-0.049**	-0.018***
	(0.002)	(0.003)	(0.004)	(0.005)	(0.007)	(0.021)	(0.004)
Mean drop-out	0.005	0.011	0.014	0.024	0.043	0.120	0.036
Observations	158,347	145,501	143,768	138,865	135,030	146,179	867,690

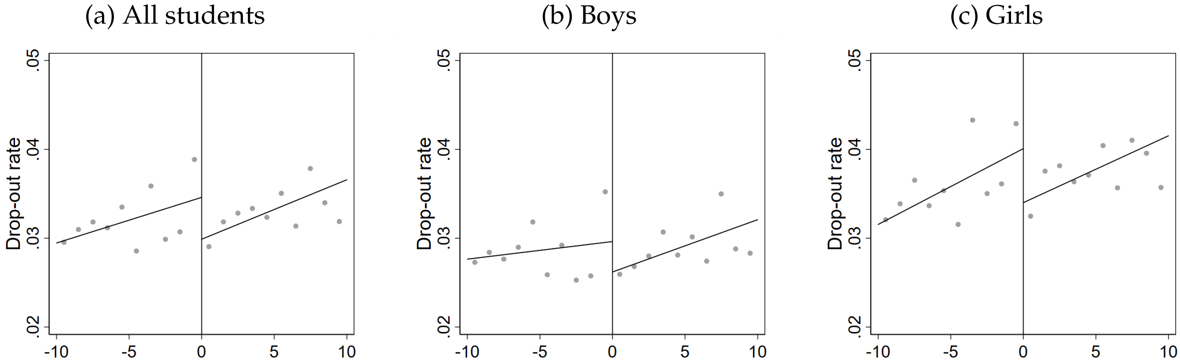
Notes: This table reports local average treatment effects of *Tayssir* cash transfers (Eq. 1 in the text). The dependent variable is a dummy equals one if a student dropout from school. Columns 1 to 6 report results for grade 1 to 6 respectively. Column 7 reports results for all grades. The unit of observation is a student-year for the schooling years 2015/16 and 2016/17. Sample restricted to students in primary schools in municipalities with 2004 poverty rates in the range [20%, 40%]. Observations are weighted using a triangular kernel function. Robust standard errors in parenthesis are clustered at the municipality year level. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculation using data from *MASSAR* and *Haut-Commissariat au Plan*.

These results appear very consistent with those of Benhassine et al. (2015) on *Tayssir* pilot. Two years after the start of the program, they found a 62% reduction in dropout rate (equivalent to an annual reduction of about 31%).[21] We should be cautious in comparing their results with those presented in this paper because of the different empirical strategies. Benhassine et al. (2015) use a randomized controlled trial while in this paper we rely on a regression discontinuity design. This means that estimates in Benhassine et al. (2015) correspond to average treatment effects whereas our estimates correspond to local average treatment effects (i.e. effects that apply for children in municipalities near the eligibility cut-off but not necessarily in municipalities further away). That said, the 41% reduction we estimate is of a similar sign and magnitude, and the heterogeneous patterns with respect to gender are fully consistent across studies. These similar results are remarkable given the expansion of the program to numerous new

[21]Some children may have dropped out of school temporarily so 31% should be interpreted as a lower bound.

locations and the ten-fold increase in the number of beneficiaries. If anything, they provide some reassurance regarding the ability of pilot evaluations to produce insightful estimates of at-scale impacts.

Figure 4: Reduced form effect on school dropout



Notes: Figures show a linear prediction plot of dropout rate on municipality poverty rate, run separately on each side of the normalized eligibility cut-off. The unit of observation is a student year for the schooling years 2015/16 and 2016/17. Sample restricted to students in primary school in municipalities with 2004 poverty rates in the range [20%, 40%]. Observations are sorted into bins of width of one percentage point and weighted using a triangular kernel function. The vertical lines indicate the normalized eligibility cut-off.
Source: Authors' calculation using poverty rate from Morocco's *Haut-Commissariat au Plan* and dropout data from *MASSAR*.

5.2 Impacts on test scores

The selection problem. Because the scores of children who drop out from school before the exam are not observed, the reduction in dropout rate documented in the previous section is likely to induce selection issues for estimates of program effects on exam scores. If dropouts are negatively selected, estimates would be biased downward (more children with low test scores would take the exam in beneficiary municipalities). In Table 3, we compare the characteristics of children who drop out with those of children who do not drop out. Columns 1 and 2 present the mean and standard deviation (in parentheses) of dropouts and non-dropouts. Column 3 reports the standardized difference between the two groups for the whole sample, while columns 5 to 10 show the differences for each grade. The two groups are very different and negatively selected. Children who drop out from schools have, on average, a lower GPA than children who stay in school (standardized differences of -0.42 and -0.87 in *t-1* and *t-2* respectively),[22] especially in grades where the effect of *Tayssir* on dropout is the largest. Dropouts are also older and more likely to have repeated grades. For example, 54.5% of the students who drop out in *t* repeated a grade in *t-1* against only 14% for students who do not drop out. This negative selection into dropout entails that an analysis of the effects of *Tayssir* on exam scores using the sample of test-takers is likely to introduce a downward bias in the estimates.

Table 3: Descriptive statistics: selective dropout

	All grades				Std. diff. by grade					
	(1) Drop-out = 1	(2) Drop-out = 0	(3) Std. diff	(4) Observations	(5) Grade 1	(6) Grade 2	(7) Grade 3	(8) Grade 4	(9) Grade 5	(10) Grade 6
Age	11.833	8.605	1.081	1,849,552	0.295	0.664	0.739	1.272	0.881	0.651
	(2.948)	(3.022)								
Boy	0.474	0.529	-0.111	1,849,553	0.021	-0.012	-0.003	-0.068	-0.088	-0.198
	(0.499)	(0.499)								
GPA ($t-1$)	5.128	5.835	-0.417	1,783,261	-0.129	-0.140	-0.157	-0.384	-0.749	-0.624
	(2.074)	(1.205)								
GPA ($t-2$)	4.529	5.730	-0.869	1,547,325	-0.797	-0.522	-0.678	-0.845	-0.906	-0.962
	(1.553)	(1.187)								
Repeat grade ($t-1$)	0.545	0.141	0.938	1,833,840	0.299	0.453	0.751	0.921	0.956	1.137
	(0.498)	(0.348)								
Repeat grade ($t-2$)	0.273	0.127	0.373	1,536,446	0.427	0.156	0.462	0.481	0.449	0.444
	(0.446)	(0.333)								

Notes: The unit of observation is a student-year for the schooling years 2015/16 and 2016/17. Sample restricted to students in primary schools in municipalities with 2004 poverty rates in the range [20%, 40%]. Standard deviations are in parenthesis.
Source: Authors' calculation using data from *MASSAR*.

To correct for this bias, we impute exam scores corresponding to different degrees of selection into dropout, which in turn can be used to provide lower and upper bounds to the true effect of *Tayssir* on exam scores.[23] Because the overall pattern of selection documented in Table 3 is negative, we conjecture that children who drop out from school would have obtained scores on the lower end of the distribution (that is anywhere below the median score), and estimate the effect of *Tayssir* on exam scores considering two extreme scenarios. First, we assume that dropouts would have obtained a score of zero. Second, we assume that dropouts would have obtained the median

[22] Estimates in *t-2* are somewhat cleaner than in *t-1* because most students drop out from school during *t-1* (as opposed to between the end of *t-1* and the beginning *t*) and as a result have no GPA. Among those that drop out in *t*, only 14% have a GPA in *t-1* against 98% in *t-2*.

[23] Due to data availability, we do not observe students in grade 6 during their whole primary education and therefore do not know for each cohort the exact number of students who drop out prior to grade 6. We proxy this quantity by relying on cross-sectional dropouts, that is the total number of dropouts observed in a given year in grade 1 to 6. Because grade-specific dropout rates have been very stable over time (see Table A1), we are confident that this quantity provides a good proxy of the number of students who dropped out prior to grade 6.

score.[24] Since dropouts are negatively selected, their (unobserved) exam scores should stand somewhere between these two values, and, as a consequence, estimates using these imputed values should provide lower and upper bounds to the true effect of *Tayssir* on exam scores.[25] More specifically, because Tayssir decreased the number of dropouts, and because dropouts are negatively selected, imputing the median score bounds the effect of the program from below while imputing the score of zero bounds the effect of the program from above. In addition, to provide more transparency on our method, and to illustrate the effects associated with more intermediate patterns of selection, we estimate effects imputing scores corresponding to the 10th and 25th percentiles of the score distribution.

Results. The results on test scores with and without corrections for selective dropout are shown in Table 4. We focus here on the score obtained at the graduation exam administered to all students at the end of grade 6. This exam is managed at the provincial level and therefore allows to compare children from different schools and different municipalities (see Section 3.2 for more details).

Estimates with no correction for differential dropout rates suggest that receiving cash transfers is associated with a decrease in exam scores of 0.16 SD (column 1, panel A).[26] However, because of the downward bias documented above, it is not clear whether this estimate is due to a selection bias or to a genuine effect of cash transfers. Looking at impacts by gender, we find that negative effects are concentrated within the sample of boys (column 1, panels B and C). This result is interesting because boys are *less* affected by the reduction in dropout rate induced by Tayssir (see Section 5.1) and should therefore be less affected by the selection bias highlighted above.[27] If anything, this suggests that negative estimates in column 1 are not entirely driven by selection issues

[24]Given that girls tend to have higher scores (Table A2), we compute median scores separately for boys and girls.

[25]Although we believe that positive selection is unlikely, note that our results are qualitatively unchanged (and in fact reinforced) for scores imputed from the higher end of the distribution.

[26]The reduced-form estimate is shown in Figure A4a.

[27]In Table A8, we show that the patterns of negative selection into dropout are consistent across gender. Standardized differences between dropouts and non-dropouts are similar for boys and girls (column 3). In particular, children who eventually drop out have lower GPA and are more likely to have repeated a grade prior to dropout.

Table 4: Effect of *Tayssir* on test scores

	Dep. Var. standardized graduation score				
	(1)	(2)	(3)	(4)	(5)
diff. drop-out:	No correction	Correction			
imputed score:		0	p10	p25	p50
Panel A. All children					
LATE	-0.163**	-0.025	-0.059	-0.106	-0.141**
	(0.078)	(0.073)	(0.069)	(0.066)	(0.064)
Mean	-0.000	-0.353	-0.235	-0.097	0.016
Observations	279,880	335,419	335,419	335,419	335,419
Panel B. Boys					
LATE	-0.210***	-0.100	-0.132*	-0.155**	-0.180***
	(0.079)	(0.073)	(0.070)	(0.068)	(0.067)
Mean	-0.069	-0.368	-0.247	-0.155	-0.062
Observations	151,024	176,734	176,734	176,734	176,734
Panel C. Girls					
LATE	-0.097	0.066	0.027	-0.039	-0.086
	(0.085)	(0.079)	(0.075)	(0.070)	(0.070)
Mean	0.080	-0.335	-0.223	-0.032	0.102
Observations	128,856	158,685	158,685	158,685	158,685

Notes: This table reports local average treatment effects of *Tayssir* cash transfers (Eq. 1 in the text). Sample restricted to grade 6 students. The dependent variable is the standardized graduation score obtained at the end of grade 6. Column 1: results with no correction for differential dropout rates. Columns 2 to 5: results imputing scores corresponding to different degrees of selection into dropout (column 2: upper bound of the true effect; column 5: lower bound of the true effect). See notes to Table 2 for other details. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculation using data from *MASSAR* and *Haut-Commissariat au Plan*.

and must in part reflect a genuine negative effect of cash transfers.

Estimates using the imputation method described above are presented in columns 2 to 5 of Table 4. Among the full sample of children, we estimate an effect somewhere between -0.03 SD and -0.14 SD (only significant at the lower end of the interval).[28] The results also suggest interesting gender dynamics. For boys, we estimate negative effects between 0.10 SD and 0.18 SD (panel B). These effects are generally significant at conventional levels, except for the most severe correction which imputes the score of zero to all dropouts. For girls, we estimate bounds that are well distributed around zero: we find a lower bound of -0.09 SD, an upper bound of 0.07 SD, and coefficients that are never statistically different from zero (panel C). Overall, the estimates in Table 4 suggest that Tayssir cash transfers did not lead to learning gains in primary school

[28] Reduced-form estimates are presented in Figures A4b and A4c.

and actually had negative effects on the test scores of boys. The next paragraphs discuss why these disappointing effects may have emerged.

Discussion. Few studies of conditional cash transfers have measured effects on learning, but when they did, they typically found effects that are not statistically different from zero (Behrman et al., 2005; Baez and Camacho, 2011; Filmer and Schady, 2014; Barham et al., 2018; Baird et al., 2019; Dustan, 2020).[29] However, given the methodological challenges to analyze cumulative processes such as learning outcomes, it is not obvious whether these non-significant estimates should be interpreted as a lack of effect of CCT programs. In these studies, the estimates correspond to the effects of relatively limited exposure to transfers since the programs were rapidly expanded to the control group or were discontinued after the evaluation period (for example, the differential exposure in Mexico's *PROGRESA* randomized evaluation was 18 months). Our results add to the literature by showing that continued exposure during whole primary school in Morocco did not lead to significant improvements in test scores at the primary school graduation exam and in fact reduced the scores of boys by between 0.10 and 0.18 SD.

Different mechanisms could be driving these results.[30] First, the higher number of children attending school may increase class size and put additional pressure on existing educational resources. This channel may be particularly relevant in our setting because *Tayssir* was targeted towards poor rural municipalities. Such targeting strategy entails that the extra influx of students was confined to limited geographical areas where school quality and resources to absorb the additional students were likely modest. Second, transfers could also have affected class composition, retaining lower

[29]There are two notable exceptions. Barham et al. (2017) in Nicaragua compare groups that randomly received transfers for a 3-year period at different points in time and find that males in the early treatment group experienced learning gains of about 0.20 SD on mathematic and Spanish tests. Duque et al. (2019) in Colombia find that children eligible to the national CCT program *Familias en Acción* scored 0.13 SD higher at the secondary school graduation test.

[30]We focus here on the mechanisms that could drive negative effects. Nonetheless, there may be other effects running in the opposite direction. For example, transfers could improve learning through increased investments in complementary inputs such as food, school supplies, and parental time. These effects could emerge either directly using the transfers (Fiszbein and Schady, 2009) or indirectly through an information effect signaling the value of education (Benhassine et al., 2015). The conditionality on attendance could also lead to more learning for children who do not attend school regularly.

ability students. This could have had negative effects on learning through peer effects and less effective teaching practices. In particular, higher heterogeneity in class composition may have led to instruction that was less tailored to the needs of students.[31] Third, there is growing evidence from the broader literature on the evaluation of social programs that transfers are very often used to make productive investments and can thereby increase economic activities in beneficiary households (for a review, see Baird et al., 2018). While this could reduce child labor through an income effect, in practice, several recent studies document sizable increases in child labor (Avitabile et al., 2019; de Hoop et al., 2020; Edmonds and Theoharides, 2020). Because of the strong norms against the use of girls labor in rural Morocco, this channel may help to explain why negative effects were concentrated within the sample of boys. However, we find this explanation somewhat unlikely as transfers remained relatively small compared to household spending: transfers in primary school were equivalent to between 6 and 10% of the average spending per capita. In addition, there is evidence that cash transfers that are conditional on schooling typically decrease child labor (De Janvry et al., 2006; Attanasio et al., 2010; De Hoop and Rosati, 2014).

One channel of impact that we can test using our data is whether the transfers affected learning through an increase in class size in beneficiary areas. To the best of our knowledge, this channel has never been studied in the context of cash transfer programs. We first look at the effect of the program on class size and then use an IV strategy to estimate the effect of the variation in class size induced by the program on test scores. Table 5 shows that the program had a positive impact on class size (panel A).[32] In particular, *Tayssir* increased class size by as much as 3.6 students in grade 6 (column 6), which corresponds to 12 percent of the average class size in the sample (30.7 students). Looking at effects by grade, we find that higher grades were associated with larger increases, with the exception of grade 1. Large effects in grade

[31] A related literature has shown that tracking students into separate classes by prior achievement typically benefit to children at all levels of the distribution (see e.g. Duflo et al., 2011).

[32] In contrast, the program had small and non-significant effects on the probability to repeat grades and to change school, suggesting that these other intermediary outcomes are unlikely to explain effects on test scores. See figures A5 for reduced-form estimates.

Table 5: Effect of *Tayssir* on class size, grade repetition, and school change

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) All Grades
Panel A. Effects on class size							
LATE	2.805***	1.056	1.679**	1.744**	1.803**	3.599***	2.132***
	(0.846)	(0.706)	(0.713)	(0.703)	(0.857)	(0.887)	(0.526)
Mean class size	30.076	29.136	29.689	29.440	29.319	30.682	29.738
Observations	330,981	303,193	301,795	291,416	283,863	307,908	1,819,156
Panel B. Effects on grade repetition							
LATE	0.013*	0.000	-0.005	-0.011	-0.011	0.010	0.001
	(0.007)	(0.008)	(0.010)	(0.010)	(0.010)	(0.021)	(0.006)
Mean grade repetition	0.188	0.143	0.152	0.133	0.128	0.206	0.159
Observations	333,225	305,730	304,358	293,988	286,277	310,821	1,834,399
Panel C. Effects on school changes							
LATE	0.000	0.003	0.003	0.010	0.005	0.023	0.002
	(0.004)	(0.006)	(0.006)	(0.009)	(0.006)	(0.020)	(0.006)
Mean change school	0.030	0.040	0.037	0.045	0.032	0.695	0.148
Observations	333,225	305,730	304,358	293,988	286,277	310,821	1,834,399

Notes: This table reports local average treatment effects of *Tayssir* cash transfers (Eq. 1 in the text). Each row in this table represents a separate outcome variable. Class size corresponds the average number of students by class. Grade repetition (resp. school change) is a dummy variable coded one if a student repeats a grade (resp. moved to a different school). See notes to Table 2 for more details. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculation using data from *MASSAR* and *Haut-Commissariat au Plan*.

1 could reflect the fact that children in targeted municipalities started school earlier (possibly to benefit from the transfers) and repeated grade 1 more often.[33] In Table A9, we use an IV strategy to examine whether this increase in class size had negative effects on test scores. More specifically, we estimate a fuzzy RD where we instrument class size by the eligibility cutoff.[34] Importantly, we apply the same strategy as above to correct for differential dropout rates (columns 2 to 5). We find that the increase in class size induced by *Tayssir* had negative effects on test scores, especially within the sample of boys: increasing class size by one student led to test scores 0.03 to 0.05 SD lower in beneficiary municipalities. Given the estimated first-stage increase of 3.6 students (in grade 6, i.e. when students take the exam), this result suggests that the increase in class size induced by the program led to a total reduction in test scores comprised between 0.11 and 0.18 SD for boys.

[33] There is some evidence in Table 5 that *Tayssir* increased the probability of repeating grade 1 (panel B, column 1).

[34] We replace *Tayssir* by the variable *class_size* in Eq. 1. We derive this variable using data at the school level and dividing for each grade the number of students by the number of classes. We do not have information on class size for the 15,243 students (or less than 0.83 percent of the sample) who moved to municipalities outside the sample.

In summary, the results presented in this section help to explain why the program had limited effects on test scores and why negative effects were observed within the sample of boys. They provide evidence that Tayssir constrained learning by putting additional pressure on existing educational resources in beneficiary municipalities. These findings are consistent with evidence that school resources have an important role to play in shaping educational attainment (see e.g. Holmlund et al., 2010; Gibbons et al., 2018), but contrast with those of Lucas and Mbiti (2012), Blimpo et al. (2019) and Valente (2019) who showed that increased enrollment following policies eliminating school fees in sub-Saharan Africa did not generate negative effects on test scores. This latter result may be explained by the expansion of private education which followed the removal of public school fees and by the increased socio-economic sorting of students into schools (Bold et al., 2015) – a mechanism that is less likely to operate in our setting because private education is generally absent in rural Morocco.

5.3 Impacts on educational attainment

In Table 6, we explore program effects on educational attainment by focusing on two dummy variables indicating whether a child (i) graduates from primary school, and (ii) enrolls in secondary school (see Section 3.2 for more details on the definition of these variables). We take into account the issue of selection into dropouts by imputing the value of zero to both of these variables for dropouts (who by definition do not graduate from primary school or enroll in secondary school). We present effects with and without corrections for more transparency. Figures A6 and A7 show reduced-form estimates. Without corrections, we find small and non-significant effects on both graduation and enrollment (columns 1 and 3). Correcting for differential dropout, the overall effect on graduation remains non-significant in the full sample (column 2, panel A), but looking at effects by gender we find a 5.7 p.p. increase for girls (significant at 5%). This corresponds to an increase of 9% relative to the sample mean of 65.3%. In contrast, we see no significant improvements for boys, which likely reflect the negative effects on test scores highlighted above. Regarding enrollment in secondary school,

Table 6: Effect of *Tayssir* on educational attainment

dep. var.:	Graduation from primary school		Enrollment in secondary school	
	(1)	(2)	(3)	(4)
diff. drop-out:	No correction	Correction	No correction	Correction
Panel A. All children				
LATE	-0.023	0.020	0.007	0.045**
	(0.029)	(0.026)	(0.021)	(0.021)
Mean	0.765	0.638	0.765	0.638
Observations	278,739	334,299	278,739	334,299
Panel B. Boys				
LATE	-0.044	-0.009	-0.005	0.024
	(0.035)	(0.031)	(0.026)	(0.024)
Mean	0.732	0.625	0.742	0.633
Observations	150,390	176,112	150,390	176,112
Panel C. Girls				
LATE	0.007	0.057**	0.024	0.070***
	(0.025)	(0.024)	(0.019)	(0.020)
Mean	0.805	0.653	0.793	0.644
Observations	128,349	158,187	128,349	158,187

Notes: The table reports local average treatment effects of *Tayssir* cash transfers (Eq. 1 in the text). Sample restricted to grade 6 students. Graduation from primary school: dummy variable coded one if a student in grade 6 graduates from primary school. Enrollment in secondary school: dummy variable coded one if a student in grade 6 is enrolled in secondary school the following year. See notes to Table 2 for other details. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculation using data from *MASSAR* and *Haut-Commissariat au Plan*.

we find that the program led to a significant increase once we correct for differential dropout (column 4). Overall, we estimate that the program increased enrollment by 4.5 p.p. (panel A), which is equivalent to a 7% increase relative to the sample mean of 63.8%. We estimate non-significant for boys (panel B) but sizable effects for girls (7 p.p. or 11% relative to the sample mean of 64.4%, panel C).

6 Conclusion

The invention and spread of conditional cash transfer programs in the past few decades have been described as "*perhaps the most remarkable innovation in welfare programs in developing countries*" (Cahyadi et al., 2020). Despite robust evidence on the static gains of these programs, only a few studies have analyzed their impacts on cumulative processes such as learning. In this paper, we showed that CCT in Morocco did not lead to learning gains in primary school and actually had negative effects on boys' test scores. Unlike most previous studies, which evaluated relatively limited exposures to cash transfers and found non-significant results, in this paper we focused on effects corresponding to an exposure during whole primary school. This allows us to provide new evidence on whether the enrollment gains from CCT programs are likely to eventually translate into sustained learning gains. Overall, we find that the program constrained learning by putting additional pressure on existing educational resources. We identify a sizable and positive impact of the program on class size which in turn had negative effects on graduation scores. In particular, we estimate that the test scores of boys decreased by 0.03 to 0.05 SD for each extra student in the classroom.

Naturally, these findings should in no way be interpreted as evidence that policy makers should not pursue CCT programs. Such programs, including the one studied in this paper, have been particularly effective at increasing access to basic education (a necessary first step to increase learning). However, our results, together with evidence showing that students in developing countries have alarmingly low literacy and numeracy levels (Bold et al., 2017; World Bank, 2018), suggest that the gains in attendance from CCT programs are unlikely on their own to equip students with the foundational skills they need. In fact, our results show that CCT programs can have adverse effects on learning when schools are not provided with the necessary resources to absorb the extra influx of students.

The past two decades have seen a surge in evalutions focusing on the learning effects of various interventions in developing countries (for recent reviews see McEwan, 2015; Ganimian and Murnane, 2016; Glewwe and Muralidharan, 2016; Masino and Niño-Zarazúa, 2016; Conn, 2017; Evans and Mendez Acosta, 2020). Although the evidence base does not allow to identify programs that are effective in all contexts (World Bank, 2018), some lessons have emerged over the years, and Evans and Popova (2016) in particular identified two classes of interventions that have been very consistent at improving learning: (i) pedagogical interventions that tailor teaching to student learning; (ii) individualized and sustained efforts to improve teacher's ability and practice.

An interesting question for future research – and a promising avenue for policy makers aiming to address the learning crisis – would be to explore the possible complementarities between these interventions and CCT programs. Indeed, recent evidence suggests that combining different education interventions may produce effects that are greater than the sum of their individual effects (Mbiti et al., 2019).

References

Abdul-Hamid, H. (2017). *Data for Learning: Building a Smart Education Data System*. Washington, D.C.: World Bank Publications.

Akresh, R., De Walque, D., and Kazianga, H. (2013). Cash transfers and child schooling: evidence from a randomized evaluation of the role of conditionality. World Bank Policy Research Working Paper No. 6340.

Angrist, J. D., Lavy, V., Leder-Luis, J., and Shany, A. (2019). Maimonides' rule redux. *American Economic Review: Insights*, 1(3):309–24.

Araujo, M. C., Bosch, M., and Schady, N. (2017). Can cash transfers help households escape an intergenerational poverty trap? In *The Economics of Poverty Traps*, pages 357–382. University of Chicago Press.

Attanasio, O., Fitzsimons, E., Gomez, A., Gutierrez, M. I., Meghir, C., and Mesnard, A. (2010). Children's schooling and work in the presence of a conditional cash transfer program in rural Colombia. *Economic development and cultural change*, 58(2):181–210.

Avitabile, C., Cunha, J. M., and Meilman Cohn, R. (2019). The medium term impacts of cash and in-kind food transfers on learning. World Bank Policy Research Working Paper No. 9086.

Baez, J. E. and Camacho, A. (2011). Assessing the long-term effects of conditional cash transfers on human capital: Evidence from Colombia. World Bank Policy Research Working Paper No. 5681.

Baird, S., Ferreira, F. H., Özler, B., and Woolcock, M. (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., McIntosh, C., and Özler, B. (2011). Cash or condition? Evidence from a cash transfer experiment. *The Quarterly Journal of Economics*, 126(4):1709–1753.

Baird, S., McIntosh, C., and Özler, B. (2019). When the money runs out: do cash transfers have sustained effects on human capital accumulation? *Journal of Development Economics*, 140:169–185.

Baird, S., McKenzie, D., and Özler, B. (2018). The effects of cash transfers on adult labor market outcomes. *IZA Journal of Development and Migration*, 8(1):22.

Banerjee, A., Banerji, R., Berry, J., Duflo, E., Kannan, H., Mukerji, S., Shotland, M., and Walton, M. (2017). From proof of concept to scalable policies: Challenges and solutions, with an application. *Journal of Economic Perspectives*, 31(4):73–102.

Barham, T., Macours, K., and Maluccio, J. A. (2017). Are conditional cash transfers fulfilling their promise? Schooling, learning, and earnings after 10 years. CEPR Discussion Paper No. DP11937.

Barham, T., Macours, K., and Maluccio, J. A. (2018). Experimental evidence of exposure to a conditional cash transfer during early teenage years: young women's fertility and labor market outcomes. CEPR Discussion Paper No. DP13165.

Barrera-Osorio, F., Linden, L. L., and Saavedra, J. E. (2019). Medium-and long-term educational consequences of alternative conditional cash transfer designs: Experimental evidence from Colombia. *American Economic Journal: Applied Economics*, 11(3):54–91.

Bastagli, F., Hagen-Zanker, J., Harman, L., Barca, V., Sturge, G., Schmidt, T., and Pellerano, L. (2016). Cash transfers: what does the evidence say. Overseas Development Institute Report.

Behrman, J. R., Parker, S. W., and Todd, P. E. (2005). Long-term impacts of the oportunidades conditional cash transfer program on rural youth in Mexico. Discussion Paper 122. Ibero-America Institute for Economic Research.

Benhassine, N., Devoto, F., Duflo, E., Dupas, P., and Pouliquen, V. (2015). Turning a shove into a nudge? A" labeled cash transfer" for education. *American Economic Journal: Economic Policy*, 7(3):86–125.

Blimpo, M. P., Gajigo, O., and Pugatch, T. (2019). Financial constraints and girls' secondary education: Evidence from school fee elimination in the Gambia. *The World Bank Economic Review*, 33(1):185–208.

Bold, T., Filmer, D., Martin, G., Molina, E., Stacy, B., Rockmore, C., Svensson, J., and Wane, W. (2017). Enrollment without learning: Teacher effort, knowledge, and skill in primary schools in Africa. *Journal of Economic Perspectives*, 31(4):185–204.

Bold, T., Kimenyi, M., Mwabu, G., and Sandefur, J. (2015). Can free provision reduce demand for public services? Evidence from Kenyan education. *The World Bank Economic Review*, 29(2):293–326.

Bold, T., Kimenyi, M., Mwabu, G., Sandefur, J., et al. (2018). Experimental evidence on scaling up education reforms in Kenya. *Journal of Public Economics*, 168:1–20.

Cahyadi, N., Hanna, R., Olken, B. A., Prima, R. A., Satriawan, E., and Syamsulhakim, E. (2020). Cumulative impacts of conditional cash transfer programs: Experimental evidence from Indonesia. *American Economic Journal: Economic Policy*. Forthcoming.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.

Cattaneo, M. D., Idrobo, N., and Titiunik, R. (2019a). *A practical introduction to regression discontinuity designs: Foundations*. Cambridge University Press.

Cattaneo, M. D., Titiunik, R., and Vazquez-Bare, G. (2019b). Power calculations for regression-discontinuity designs. *The Stata Journal*, 19(1):210–245.

Cattaneo, M. D. and Vazquez-Bare, G. (2016). The choice of neighborhood in regression discontinuity designs. *Observational Studies*, 2(134):A146.

Chetty, R., Friedman, J. N., Hilger, N., Saez, E., Schanzenbach, D. W., and Yagan, D. (2011). How does your kindergarten classroom affect your earnings? Evidence from Project STAR. *The Quarterly Journal of Economics*, 126(4):1593–1660.

Conn, K. M. (2017). Identifying effective education interventions in sub-saharan africa: A meta-analysis of impact evaluations. *Review of Educational Research*, 87(5):863–898.

Cottin, R. (2019). Le ciblage direct des ménages est-il possible pour les politiques de santé? Le cas du RAMed au Maroc. *Mondes en developpement*, (3):29–50.

CSEFRS (2019). *Résultats des élèves marocains dans l'étude internationale sur le progrès en littératie PIRLS 2016*. Rabat, Maroc: Conseil Supérieur de l'Education, de la Formation et de la Recherche Scientifique.

De Giorgi, G., Pellizzari, M., and Woolston, W. G. (2012). Class size and class heterogeneity. *Journal of the European Economic Association*, 10(4):795–830.

de Hoop, J., Groppo, V., and Handa, S. (2020). Cash transfers, microentrepreneurial activity, and child work: Evidence from Malawi and Zambia. *The World Bank Economic Review*, 34(3):670–697.

De Hoop, J. and Rosati, F. C. (2014). Cash transfers and child labor. *The World Bank Research Observer*, 29(2):202–234.

De Janvry, A., Finan, F., Sadoulet, E., and Vakis, R. (2006). Can conditional cash transfer programs serve as safety nets in keeping children at school and from working when exposed to shocks? *Journal of Development Economics*, 79(2):349–373.

Dobbelsteen, S., Levin, J., and Oosterbeek, H. (2002). The causal effect of class size on scholastic achievement: distinguishing the pure class size effect from the effect of changes in class composition. *Oxford Bulletin of Economics and statistics*, 64(1):17–38.

Duflo, E., Dupas, P., and Kremer, M. (2011). Peer effects, teacher incentives, and the impact of tracking: Evidence from a randomized evaluation in Kenya. *American Economic Review*, 101(5):1739–74.

Duque, V., Rosales-Rueda, M., Sanchez, F., et al. (2019). How do early-life shocks interact with subsequent human capital investments? evidence from administrative data. Working Pape.

Dustan, A. (2020). Can large, untargeted conditional cash transfers increase urban high school graduation rates? Evidence from Mexico city's Prepa Sí. *Journal of Development Economics*, 143:102392.

Edmonds, E. and Theoharides, C. (2020). The short term impact of a productive asset transfer in families with child labor: Experimental evidence from the Philippines. *Journal of Development Economics*, page 102486.

Ennaji, M. (2018). Morocco's experience in gender gap reduction in education. *Gender and Women's Studies*, 2(1):5.

Evans, D. and Mendez Acosta, A. (2020). Education in Africa: What are we learning? Center for Global Development Working Paper No. 542.

Evans, D. K. and Popova, A. (2016). What really works to improve learning in developing countries? An analysis of divergent findings in systematic reviews. *World Bank Research Observer*, 31(2):242–270.

Filmer, D. and Schady, N. (2014). The medium-term effects of scholarships in a low-income country. *Journal of Human Resources*, 49(3):663–694.

Firmino, J., Nunes, L. C., Reis, A. B., and Seabra, C. (2018). Class composition and student achievement: Evidence from Portugal. FEUNL Working Paper Series No. 624.

Fiszbein, A. and Schady, N. R. (2009). *Conditional cash transfers: reducing present and future poverty*. The World Bank.

Fredriksson, P., Öckert, B., and Oosterbeek, H. (2013). Long-term effects of class size. *The Quarterly Journal of Economics*, 128(1):249–285.

Ganimian, A. J. and Murnane, R. J. (2016). Improving education in developing countries: Lessons from rigorous impact evaluations. *Review of Educational Research*, 86(3):719–755.

Garcia, S. and Saavedra, J. E. (2017). Educational impacts and cost-effectiveness of conditional cash transfer programs in developing countries: A meta-analysis. *Review of Educational Research*, 87(5):921–965.

Gelman, A. and Imbens, G. (2019). Why high-order polynomials should not be used in regression discontinuity designs. *Journal of Business & Economic Statistics*, 37(3):447–456.

Gibbons, S., McNally, S., and Viarengo, M. (2018). Does additional spending help urban schools? An evaluation using boundary discontinuities. *Journal of the European Economic Association*, 16(5):1618–1668.

Glewwe, P. and Muralidharan, K. (2016). Improving education outcomes in developing countries: Evidence, knowledge gaps, and policy implications. *Handbook of the Economics of Education*, 5:653–743.

Goodman, S., BenYishay, A., Lv, Z., and Runfola, D. (2019). Geoquery: Integrating HPC systems and public web-based geospatial data tools. *Computers & geosciences*, 122:103–112.

Holmlund, H., McNally, S., and Viarengo, M. (2010). Does money matter for schools? *Economics of Education Review*, 29(6):1154–1164.

Hoxby, C. (2000). Peer effects in the classroom: Learning from gender and race variation. NBER Working Paper No. 7867.

Imbens, G. and Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of Economic Studies*, 79(3):933–959.

Kremer, M., Brannen, C., and Glennerster, R. (2013). The challenge of education and learning in the developing world. *Science*, 340(6130):297–300.

Krueger, A. B. (1999). Experimental estimates of education production functions. *The Quarterly Journal of Economics*, 114(2):497–532.

L'Economiste (2017). Abandon scolaire: Tayssir, neuf ans après. *Accessed online on 2020-09-23*.

Lucas, A. M. and Mbiti, I. M. (2012). Access, sorting, and achievement: The short-run effects of free primary education in Kenya. *American Economic Journal: Applied Economics*, 4(4):226–53.

Masino, S. and Niño-Zarazúa, M. (2016). What works to improve the quality of student learning in developing countries? *International Journal of Educational Development*, 48:53–65.

Mbiti, I., Muralidharan, K., Romero, M., Schipper, Y., Manda, C., and Rajani, R. (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.

McEwan, P. J. (2015). Improving learning in primary schools of developing countries: A meta-analysis of randomized experiments. *Review of Educational Research*, 85(3):353–394.

Médias24 (2014). Massar: Rachid Belmokhtar ne fera pas de concession. *Accessed online on 2020-09-23*.

Médias24 (2018). Programme Tayssir: le détail des nouvelles mesures de la rentrée 2018-2019. *Accessed online on 2020-09-23*.

Ministry of Education (2018). *Atlas territorial de l'enseignement privé*. Rabat, Maroc: Conseil Supérieur de l'Education, de la Formation et de la Recherche Scientifique.

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

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

Muralidharan, K. and Niehaus, P. (2017). Experimentation at scale. *Journal of Economic Perspectives*, 31(4):103–24.

Parker, S. W. and Vogl, T. (2018). Do conditional cash transfers improve economic outcomes in the next generation? Evidence from Mexico. NBER Working Paper No. 24303.

Ponce, J. and Bedi, A. S. (2010). The impact of a cash transfer program on cognitive achievement: The bono de desarrollo humano of Ecuador. *Economics of Education Review*, 29(1):116–125.

Pritchett, L. (2013). *The rebirth of education: Schooling ain't learning*. Washington, DC: Center for Global Development.

Soumaya, M., Julie, B., Marguerite, C., Elizabeth, F., Hannah, K., and Isobel, M. (2018). *Examens de l'OCDE du cadre d'évaluation de l'éducation: Maroc*. Paris, France: OECD.

Urquiola, M. (2006). Identifying class size effects in developing countries: Evidence from rural Bolivia. *The Review of Economics and Statistics*, 88(1):171–177.

Valente, C. (2019). Primary education expansion and quality of schooling. *Economics of Education Review*, 73:101913.

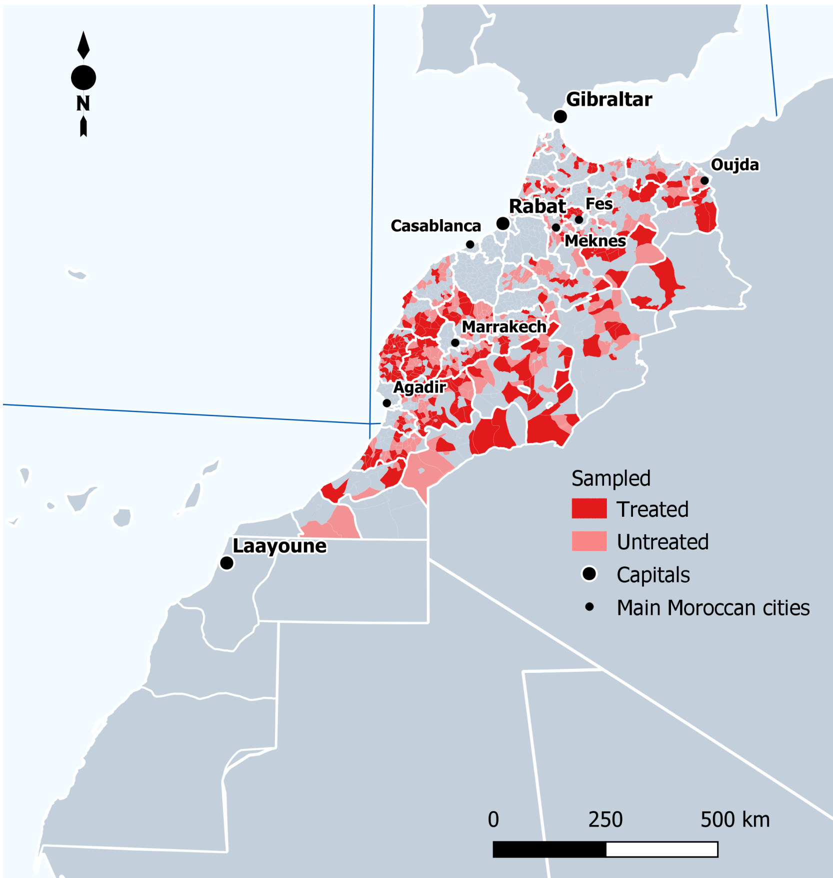
Vivalt, E. (2020). How much can we generalize from impact evaluations? *Journal of the European Economic Association*. Forthcoming.

World Bank (2018). *World Development Report 2018: Learning to realize education's promise*.

World Bank (2019). Ending learning poverty: the call of our times. *Accessed online on 2020-11-19*.

Appendix

Figure A1: Study municipalities



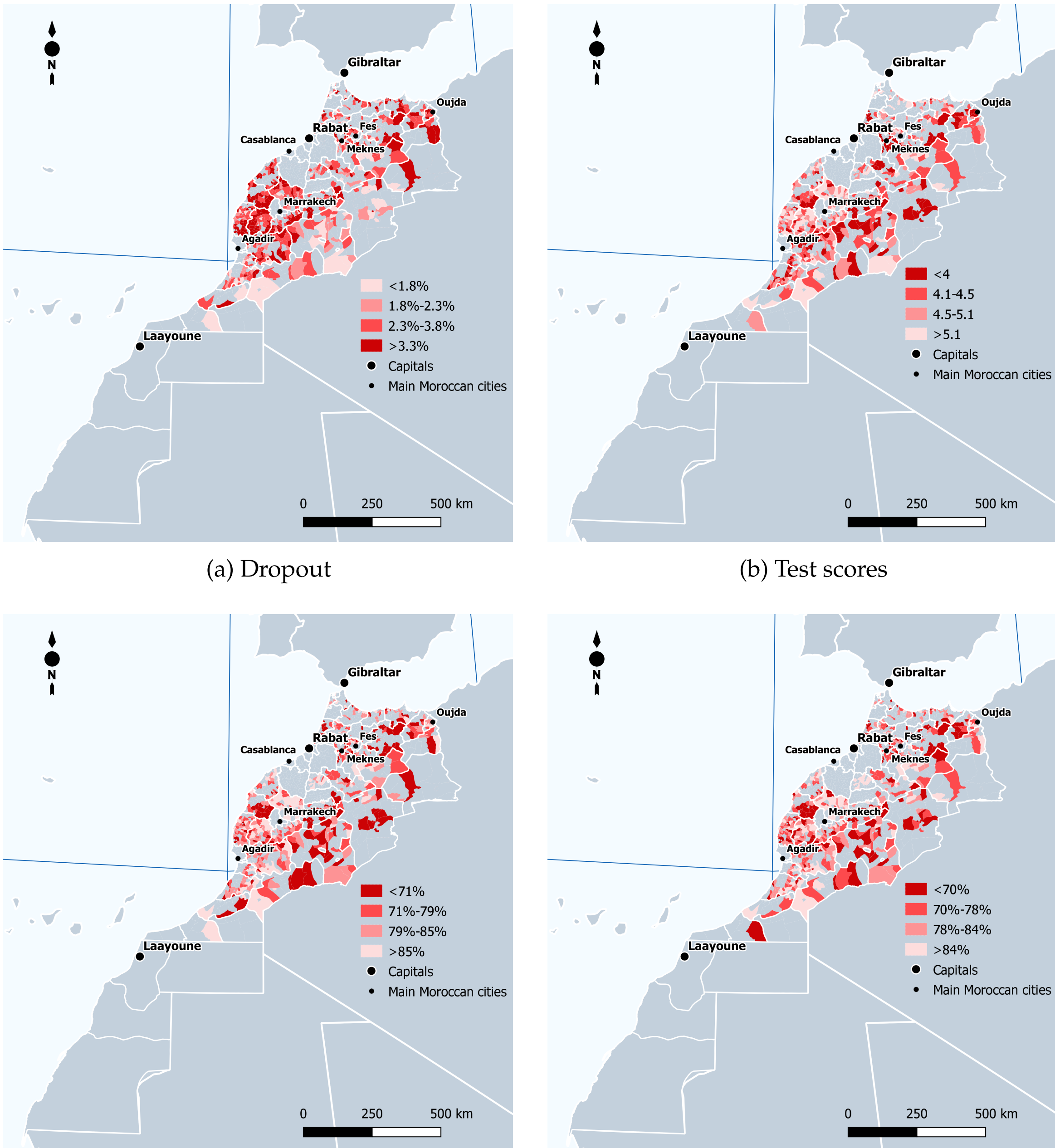
Notes: This map represents the treatment allocation for sampled municipalities (i.e. with poverty rates between 20% and 40%). See notes to Figure 1 for other details.
Source: Authors' elaboration using treatment data from the *Haut-Commissariat au Plan*.

Table A1: Descriptive statistics: children characteristics by year and grade

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) ALL
Outcome variables							
Drop-out							
2013/14	.	.	.	.	.	.	.
2014/15	0.020	0.025	0.027	0.029	0.041	0.082	0.037
2015/16	0.006	0.012	0.015	0.023	0.041	0.100	0.033
2016/17	0.005	0.012	0.014	0.023	0.039	0.099	0.032
All years	0.009	0.015	0.017	0.024	0.040	0.096	0.033
Graduation score							
2013/14	.	.	.	.	.	4.462	.
2014/15	.	.	.	.	.	4.082	.
2015/16	.	.	.	.	.	4.379	.
2016/17	.	.	.	.	.	4.510	.
All years	.	.	.	.	.	4.371	.
Graduation rate							
2013/14	.	.	.	.	.	0.729	.
2014/15	.	.	.	.	.	0.656	.
2015/16	.	.	.	.	.	0.751	.
2016/17	.	.	.	.	.	0.780	.
All years	.	.	.	.	.	0.736	.
Enrollment in secondary							
2013/14	.	.	.	.	.	0.760	.
2014/15	.	.	.	.	.	0.666	.
2015/16	.	.	.	.	.	0.760	.
2016/17	.	.	.	.	.	0.771	.
All years	.	.	.	.	.	0.743	.
Other variables							
Gender (boy = 1)							
2013/14	0.524	0.521	0.525	0.527	0.533	0.540	0.528
2014/15	0.523	0.524	0.525	0.526	0.529	0.532	0.526
2015/16	0.524	0.523	0.528	0.527	0.528	0.531	0.527
2016/17	0.525	0.525	0.528	0.528	0.529	0.528	0.527
All years	0.524	0.523	0.526	0.527	0.530	0.533	0.527
Age							
2013/14	5.836	7.088	8.357	9.533	10.689	11.879	8.819
2014/15	5.815	7.039	8.291	9.480	10.631	11.845	8.821
2015/16	5.786	7.019	8.229	9.418	10.561	11.797	8.763
2016/17	5.777	7.004	8.221	9.375	10.517	11.728	8.691
All years	5.803	7.037	8.274	9.452	10.600	11.811	8.773
Score general							
2013/14	5.680	5.772	5.672	5.663	5.702	5.772	5.708
2014/15	5.687	5.790	5.712	5.694	5.723	5.793	5.732
2015/16	5.745	5.837	5.786	5.772	5.785	5.830	5.792
2016/17	5.816	5.908	5.868	5.872	5.870	5.905	5.872
All years	5.733	5.827	5.760	5.750	5.770	5.826	5.777
Grade repetition							
2013/14	0.170	0.123	0.139	0.115	0.108	0.189	0.141
2014/15	0.179	0.132	0.141	0.122	0.114	0.209	0.150
2015/16	0.187	0.143	0.153	0.135	0.129	0.213	0.161
2016/17	0.189	0.142	0.151	0.130	0.127	0.198	0.157
All years	0.181	0.135	0.146	0.125	0.119	0.203	0.152
Change school							
2013/14	0.029	0.036	0.031	0.040	0.030	0.733	0.142
2014/15	0.032	0.037	0.033	0.042	0.029	0.697	0.146
2015/16	0.026	0.039	0.036	0.044	0.031	0.691	0.148
2016/17	0.033	0.040	0.038	0.047	0.033	0.699	0.148
All years	0.030	0.038	0.034	0.043	0.031	0.704	0.146

Notes: Authors' elaboration using data from *MASSAR*. Sample restricted to primary school children in municipalities with poverty rates between 20% and 40%. The outcomes are defined as follow. Dropout: the grade-specific dropout rate in primary school. Graduation score: the average standardized test score at the exam at the end of grade 6. Graduation rate: the share of students graduating from primary school. Enrollment in secondary: the share of students in grade 6 enrolled in secondary school the following year. See Section 3.2 for other details.

Figure A2: Outcomes in study municipalities



(a) Dropout

(b) Test scores

(c) Graduation

(d) Secondary school

Notes: Authors' elaboration using data from *MASSAR*. These maps represent the average of the main outcomes of interest in each study municipality: (a) the grade-specific dropout rate in primary school; (b) the average standardized test score at grade 6 exam; (c) the share of students graduating from primary school; (d) the share of students in grade 6 enrolled in secondary school the following year. See notes to Figure 1 and Section 3.2 for other details.

Table A2: Descriptive statistics: gender differences

	All grades				Std. diff. by grade					
	(1) Girls	(2) Boys	(3) Std. diff	(4) N	(5) Grade 1	(6) Grade 2	(7) Grade 3	(8) Grade 4	(9) Grade 5	(10) Grade 6
Age	8.614	8.829	-0.090	1,834,398	-0.038	-0.095	-0.147	-0.185	-0.229	-0.257
	(2.301)	(2.458)								
GPA	5.997	5.684	0.261	1,771,175	0.156	0.215	0.269	0.319	0.348	0.295
	(1.210)	(1.194)								
Drop-out	0.036	0.028	0.042	1,834,399	-0.008	0.003	0.001	0.020	0.034	0.119
	(0.186)	(0.166)								
Repeat grade	0.130	0.186	-0.155	1,834,399	-0.107	-0.137	-0.170	-0.198	-0.214	-0.133
	(0.336)	(0.389)								
Change school	0.148	0.148	0.001	1,834,399	-0.001	-0.003	-0.014	-0.007	-0.011	0.035
	(0.355)	(0.355)								
Graduation score	4.612	4.300	0.147	279,880	.	.	.	.	.	0.147
	(2.246)	(1.984)								

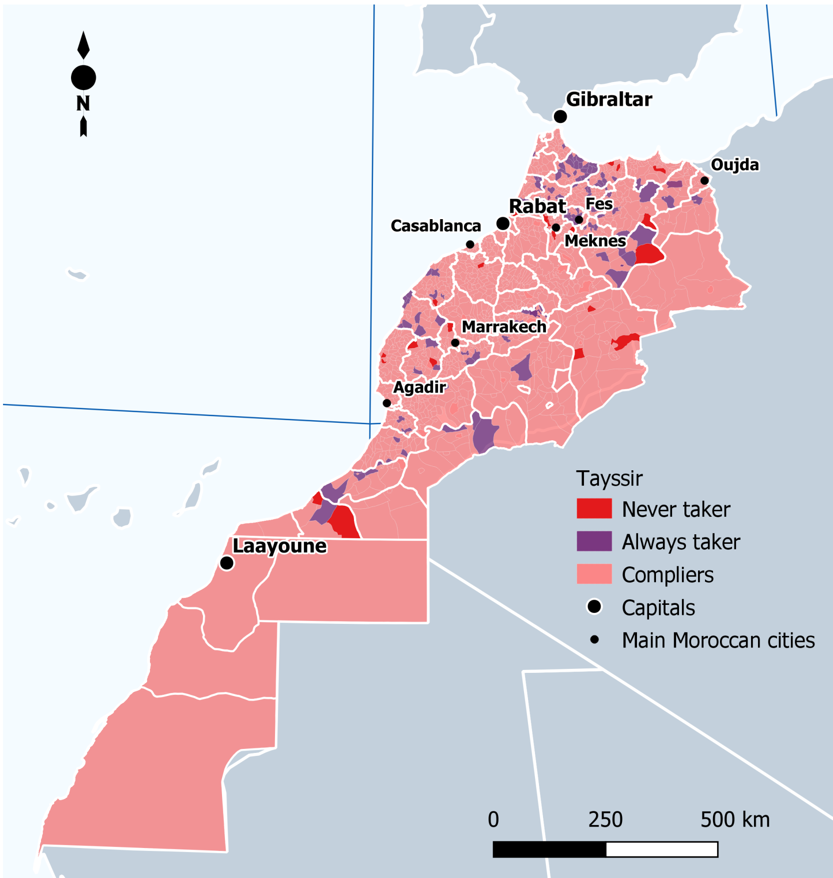
Notes: The unit of observation is a student-year for the schooling years 2015/16 and 2016/17. Sample restricted to students in primary schools in municipalities with 2004 poverty rates in the range [20%, 40%]. Standard deviations are in parenthesis.
Source: Authors' calculation using data from *MASSAR*.

Table A3: Pre-intervention balance

	(1) Nighttime light	(2) Population density	(3) Share of croplands	(4) Distance to road	(5) Travel time to city
LATE	-0.523	-12.662	-0.063	-267.926	-16.906
	(0.413)	(22.096)	(0.049)	(359.352)	(16.473)
N	670	670	670	670	670
Mean	2.164	113.869	0.385	3646.557	166.252

Notes: The table reports the results of local linear regressions using a bandwidth of 10 percentage points on each side of the cut-off. Nighttime lights, population density and the share of croplands are from 2005. Distance to road is from the 1980s to 2010 (no confirmed date). Travel time to city is from 2000. See Goodman et al. (2019) for more details. Observations are weighted using a triangular kernel function. Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Figure A3: Mapping imperfect compliance



Notes: Authors' elaboration using data from *MASSAR*. This map represents the municipalities' compliance to the *Tayssir*'s targeting rule. Source: Authors' elaboration

Table A4: Robustness checks: Effect of *Tayssir* on dropout

	Dep Var: Drop-out					
	(1) Baseline results	(2) Uniform kernel	(3) Covariates	(4) Quadratic	(5) BW= 7.5	(6) BW= 5
Panel A. All kids						
LATE	-0.013***	-0.011***	-0.015***	-0.022***	-0.017***	-0.024***
	(0.004)	(0.003)	(0.004)	(0.007)	(0.005)	(0.006)
Mean	0.032	0.032	0.032	0.032	0.032	0.032
N	1,834,399	1,834,399	1,834,399	1,834,399	1,248,413	779,486
Panel B. Boys						
LATE	-0.010***	-0.008**	-0.009**	-0.018***	-0.012**	-0.022***
	(0.004)	(0.003)	(0.004)	(0.007)	(0.005)	(0.006)
Mean	0.028	0.028	0.028	0.028	0.028	0.028
N	966,709	966,709	966,709	966,709	657,291	410,925
Panel C. Girls						
LATE	-0.018***	-0.014***	-0.018***	-0.027***	-0.022***	-0.025***
	(0.004)	(0.004)	(0.004)	(0.007)	(0.005)	(0.007)
Mean	0.036	0.036	0.036	0.036	0.036	0.037
N	867,690	867,690	867,690	867,690	591,122	368,561

Notes: This table reports local average treatment effects of *Tayssir* cash transfers (Eq. 1 in the text). The dependent variable is a dummy equals one if a student dropout from school. Column 1 reports the results from Table 2, column 7. Column 2: observations are weighted using a uniform function. Column 3 includes the following covariates: nighttime lights, population density, the share of croplands, distance to road, and travel to city (see notes to Table A3 for more details on the definition of these variables). Column 4 reports the results using a quadratic polynomial approximation run separately on each side of the cut-off. Column 5: the bandwidth is narrowed to 7.5 p.p. on either side of the cut-off. Column 6: the bandwidth is narrowed to 5 p.p. on either side of the cut-off. See notes to Table 2 for more details. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculation using data from *MASSAR* and *Haut-Commissariat au Plan*.

Table A5: Robustness checks: Effect of *Tayssir* on test scores

	Dep Var: Standardized graduation score					
	(1) Baseline results	(2) Uniform kernel	(3) Covariates	(4) Quadratic	(5) BW= 7.5	(6) BW= 5
Panel A. No correction						
LATE	-0.163**	-0.148**	-0.151**	-0.156	-0.166*	-0.171
	(0.078)	(0.072)	(0.072)	(0.119)	(0.094)	(0.109)
Mean	-0.000	-0.000	-0.000	-0.000	-0.003	-0.017
N	279,880	279,880	279,880	279,880	191,230	119,626
Panel B. Lower bound						
LATE	-0.141**	-0.127**	-0.139**	-0.138	-0.146*	-0.148
	(0.064)	(0.059)	(0.061)	(0.099)	(0.078)	(0.091)
Mean	0.016	0.016	0.016	0.016	0.014	0.002
N	335,419	335,419	335,419	335,419	229,515	143,471
Panel C. Higher bound						
LATE	-0.025	-0.015	0.006	0.019	-0.016	0.013
	(0.073)	(0.066)	(0.071)	(0.110)	(0.087)	(0.099)
Mean	-0.353	-0.353	-0.353	-0.353	-0.358	-0.368
N	335,419	335,419	335,419	335,419	229,515	143,471

Notes: This table reports local average treatment effects of *Tayssir* cash transfers (Eq. 1 in the text). The dependent variable is the standardized graduation score obtained at the end of grade 6. Column 1 reports the results from Table 4, column 1. See notes to Table 2 for more details. Panel A: results with no correction for differential dropout rates. Panel B: results imputing the score of 0 to dropouts. Panel C: results imputing the median score to dropouts. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculation using data from *MASSAR* and *Haut-Commissariat au Plan*.

Table A6: Robustness checks: Effect of *Tayssir* on class size, grade repetition, and school change

	(1) Baseline results	(2) Uniform kernel	(3) Covariates	(4) Quadratic	(5) BW= 7.5	(6) BW= 5
Panel A. Effects on class size						
LATE	3.599***	4.546***	7.843***	6.100***	4.519***	5.207***
	(0.887)	(0.793)	(0.832)	(1.239)	(1.040)	(1.082)
Mean	30.682	30.682	30.682	30.682	30.685	30.033
N	307,908	307,908	307,908	307,908	211,117	132,588
Panel B. Effects on grade repetition						
LATE	0.001	-0.005	0.002	0.000	0.003	0.000
	(0.006)	(0.005)	(0.006)	(0.010)	(0.007)	(0.009)
Mean	0.159	0.159	0.159	0.159	0.162	0.163
N	1,834,399	1,834,399	1,834,399	1,834,399	1,248,413	779,486
Panel C. Effects on school changes						
LATE	0.002	0.001	0.005	0.002	0.005	-0.007
	(0.006)	(0.005)	(0.006)	(0.009)	(0.007)	(0.008)
Mean	0.148	0.148	0.148	0.148	0.148	0.148
N	1,834,399	1,834,399	1,834,399	1,834,399	1,248,413	779,486

Notes: This table reports local average treatment effects of *Tayssir* cash transfers (Eq. 1 in the text). Each row in this table represents a separate outcome variable. Class size corresponds the average number of students by class in grade 6. Grade repetition (resp. school change) is a dummy variable coded one if a student repeats a grade (resp. moved to a different school). Column 1 reports the results from Table 5. See notes to Table 2 for more details. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculation using data from *MASSAR* and *Haut-Commissariat au Plan*.

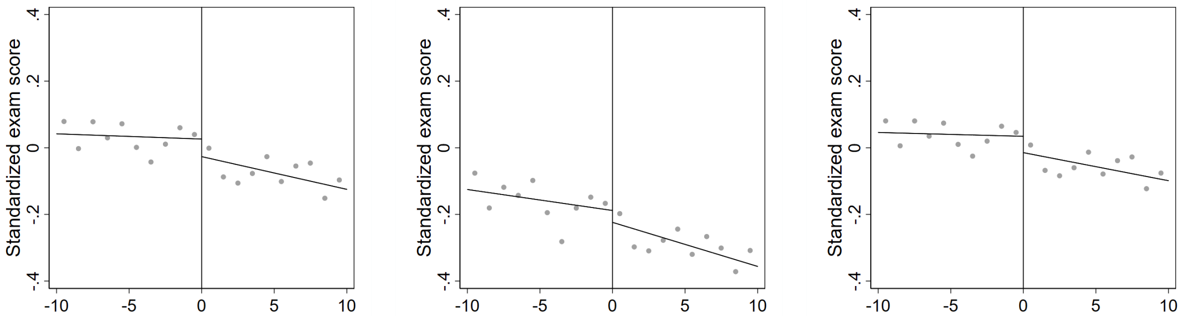
Table A7: Robustness checks: Effect of *Tayssir* on educational attainment

	(1) Baseline results	(2) Uniform kernel	(3) Covariates	(4) Quadratic	(5) BW= 7.5	(6) BW= 5
Panel A. Effects on graduation from primary school						
LATE	0.020	0.026	0.011	0.030	0.032	0.023
	(0.026)	(0.021)	(0.025)	(0.040)	(0.032)	(0.036)
Mean	0.638	0.638	0.638	0.638	0.633	0.633
N	334,299	334,299	334,299	334,299	228,987	143,212
Panel B. Effects on enrollment in secondary school						
LATE	0.045**	0.039**	0.043**	0.069**	0.059**	0.067**
	(0.021)	(0.019)	(0.020)	(0.030)	(0.024)	(0.026)
Mean	0.638	0.638	0.638	0.638	0.633	0.634
N	334,299	334,299	334,299	334,299	228,987	143,212

Notes: This table reports local average treatment effects of *Tayssir* cash transfers (Eq. 1 in the text). Sample restricted to grade 6 students. Each row in this table represents a separate outcome variable. Graduation from primary school: dummy variable coded one if a student in grade 6 graduates from primary school. Enrollment in secondary school: dummy variable coded one if a student in grade 6 is enrolled in secondary school the following year. Both outcomes are corrected for selection into dropouts. Column 1 reports the results from Table 6, column 2 and 4, Panel A. See notes to Table 2 for more details. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculation using data from *MASSAR* and *Haut-Commissariat au Plan*.

Figure A4: Reduced form effect on graduation scores

(a) No correction (b) Imputed score: zero (c) Imputed score: median



Notes: Figures show a linear prediction plot of the standardized graduation scores on municipality poverty rate, run separately on each side of the normalized eligibility cut-off. Figure A: no correction for differential dropout rates. Figure B: the score of zero is imputed to dropouts. Figure C: the median score is imputed to dropouts. See notes to Figure 4 for more details.
Source: Authors' calculation using poverty rate from Morocco's *Haut-Commissariat au Plan* and exam score from *MASSAR*.

Table A8: Descriptive statistics: selective dropout by gender

	All grades				Std. diff. by grade					
	(1) Drop-out = 1	(2) Drop-out = 0	(3) Std. diff	(4) Observations	(5) Grade 1	(6) Grade 2	(7) Grade 3	(8) Grade 4	(9) Grade 5	(10) Grade 6
Panel A. Boys										
Age	11.999	8.716	1.043	974,823	0.302	0.664	0.628	1.236	0.961	0.881
	(3.171)	(3.126)								
GPA ($t-1$)	4.960	5.688	-0.453	943,269	-0.143	-0.158	-0.223	-0.399	-0.809	-0.661
	(1.940)	(1.188)								
GPA ($t-2$)	4.336	5.584	-0.921	818,729	-0.777	-0.519	-0.747	-0.843	-0.997	-1.015
	(1.509)	(1.180)								
Repeat grade ($t-1$)	0.573	0.167	0.926	966,281	0.274	0.422	0.778	0.904	1.034	1.118
	(0.495)	(0.373)								
Repeat grade ($t-2$)	0.335	0.148	0.448	812,875	0.405	0.136	0.484	0.506	0.506	0.537
	(0.472)	(0.355)								
Change school ($t-1$)	0.027	0.031	-0.025	966,104	0.067	0.084	0.007	-0.023	-0.023	-0.093
	(0.161)	(0.173)								
Change school ($t-2$)	0.035	0.030	0.026	812,707	0.057	0.156	0.068	0.024	0.019	-0.049
	(0.183)	(0.171)								
Panel B. Girls										
Age	11.684	8.480	1.140	874,729	0.287	0.666	1.046	1.332	0.825	0.523
	(2.724)	(2.895)								
GPA ($t-1$)	5.343	6.001	-0.369	839,992	-0.111	-0.122	-0.070	-0.365	-0.678	-0.573
	(2.215)	(1.203)								
GPA ($t-2$)	4.697	5.895	-0.864	728,596	-0.824	-0.529	-0.615	-0.871	-0.867	-0.997
	(1.571)	(1.174)								
Repeat grade ($t-1$)	0.519	0.112	0.973	867,559	0.327	0.491	0.728	0.960	0.916	1.204
	(0.500)	(0.316)								
Repeat grade ($t-2$)	0.220	0.103	0.323	723,571	0.455	0.181	0.439	0.470	0.413	0.408
	(0.414)	(0.303)								
Change school ($t-1$)	0.026	0.029	-0.021	867,442	0.069	0.092	0.024	-0.055	0.055	-0.104
	(0.159)	(0.169)								
Change school ($t-2$)	0.027	0.028	-0.007	723,458	0.075	0.059	0.059	0.009	0.001	-0.089
	(0.163)	(0.166)								

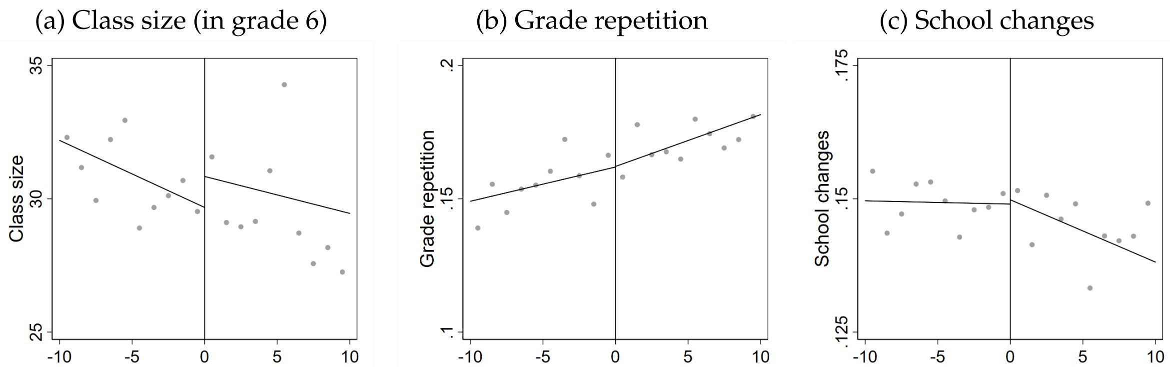
Notes: The unit of observation is a student-year for the schooling years 2015/16 and 2016/17. Sample restricted to students in primary schools in municipalities with 2004 poverty rates in the range [20%, 40%]. Standard deviations are in parenthesis.
Source: Authors' calculation using data from *MASSAR*.

Table A9: Class size and test scores

	Dep. Var. : graduation test score				
	(1)	(2)	(3)	(4)	(5)
diff. drop-out:	No correction	Correction			
imputed score:		0	p10	p25	p50
Panel A. All children					
LATE	-0.048*	-0.012	-0.022	-0.035	-0.045*
	(0.027)	(0.022)	(0.022)	(0.023)	(0.025)
Mean	-0.000	-0.353	-0.235	-0.097	0.016
Observations	279,880	335,419	335,419	335,419	335,419
Panel B. Boys					
LATE	-0.052**	-0.029	-0.037*	-0.043**	-0.049**
	(0.024)	(0.021)	(0.021)	(0.021)	(0.022)
Mean	-0.069	-0.368	-0.247	-0.155	-0.062
Observations	151,024	176,734	176,734	176,734	176,734
Panel C. Girls					
LATE	-0.038	0.016	0.003	-0.019	-0.034
	(0.036)	(0.027)	(0.026)	(0.028)	(0.030)
Mean	0.080	-0.335	-0.223	-0.032	0.102
Observations	128,856	158,685	158,685	158,685	158,685

Notes: This table reports fuzzy regression discontinuity estimates instrumenting class size by the eligibility cutoff. The dependent variable is the standardized graduation score obtained at the end of grade 6. Columns 1 to 5 report estimates from various specifications of the effect of the variation in class size induced by the program on test scores. Column 1: results with no correction for selective dropout rates. Columns 2 to 5: results imputing scores corresponding to different degrees of selection into dropout (column 2: upper bound of the true effect; column 5: lower bound of the true effect). Sample restricted to grade 6 students. See notes to Table 2 for more details. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculation using data from *MASSAR* and *Haut-Commissariat au Plan*.

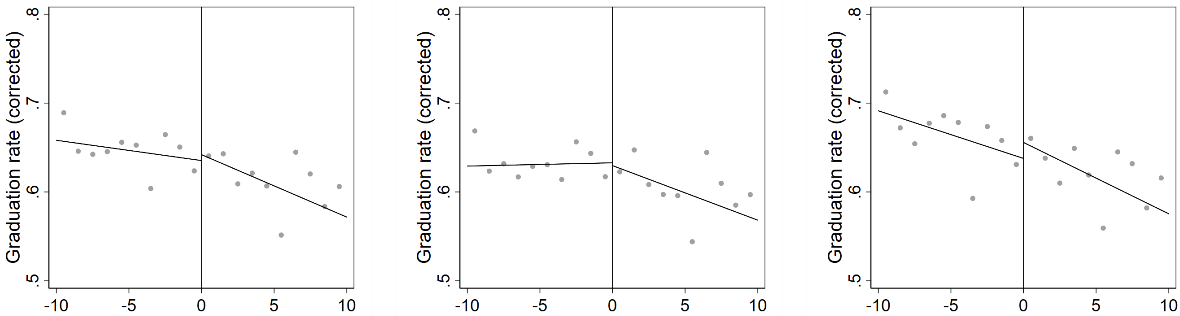
Figure A5: Reduced form effect on class size, grade repetition, and school change



Notes: Figures show a linear prediction plot of the average class size on municipality poverty rate, run separately on each side of the normalized eligibility cut-off. See notes to Figure 4 for more details.
Source: Authors' calculation using poverty rate from Morocco's *Haut-Commissariat au Plan* and exam score from *MASSAR*.

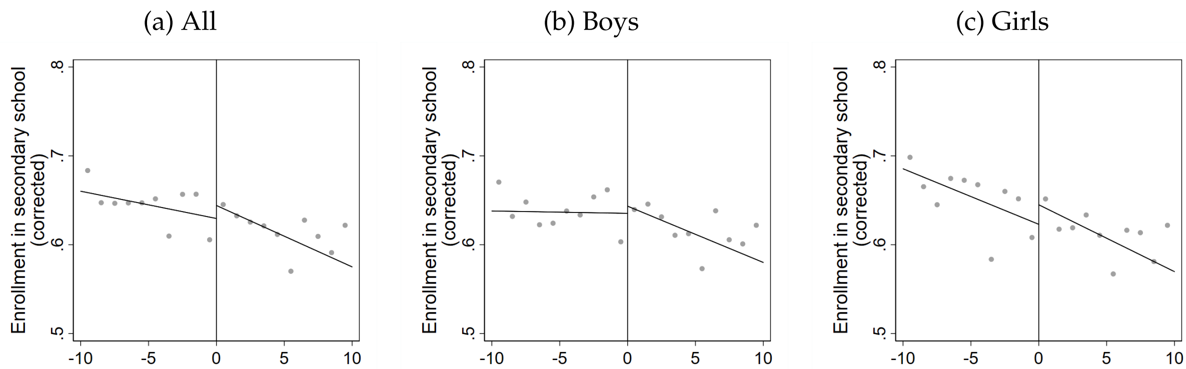
Figure A6: Reduced form effect on graduation from primary school

(a) All (b) Boys (c) Girls



Notes: Figures show a linear prediction plot of the graduation rate on municipality poverty rate, run separately on each side of the normalized eligibility cut-off. The graduation rate is defined as a dummy variable coded one if a student in grade 6 graduates from primary school. Selection into dropouts is corrected by imputing a value of zero to dropouts. See notes to Figure 4 for more details.
Source: Authors' calculation using poverty rate from Morocco's *Haut-Commissariat au Plan* and exam score from *MASSAR*.

Figure A7: Reduced form effect on enrollment in secondary school



Notes: Figures show a linear prediction plot of access to secondary school on municipality poverty rate, run separately on each side of the normalized eligibility cut-off. Access to secondary school is a dummy variable coded one if a student in grade 6 is enrolled in secondary school the following year. Selection into dropouts is corrected by imputing a value of zero to dropouts. See notes to Figure 4 for more details.
Source: Authors' calculation using poverty rate from Morocco's *Haut-Commissariat au Plan* and exam score from *MASSAR*.