Incentivizing School Attendance
in the Presence of Parent-Child Information Frictions*

Damien de Walque and Christine Valente*

January 2022

Abstract:

Many countries use CCTs targeted to parents to promote schooling. Attendance conditions may work through two channels: incentivization and information. If children have private information, (i) providing attendance information to parents may increase attendance inexpensively relative to CCTs and (ii) it may be more effective to incentivize children, who have full information, than parents. Tackling both questions in a unified experimental setting, we find that information alone improves parental monitoring and has a large effect relative to our CCT. Incentivizing children is at least as effective as incentivizing parents—importantly, not because parents were able to appropriate transfers to children.

JEL Codes: I25, D82, N37
Keywords: school attendance, conditional cash transfers, moral hazard.

* de Walque: Development Research Group, The World Bank. Valente (corresponding author): Department of Economics, University of Bristol and IZA. Address: Department of Economics, University of Bristol, Priory Road Complex, Priory Road, Bristol, BS8 1TU, U.K. Email address: christine.valente@bristol.ac.uk. Telephone: +441179289091. The authors' names are listed alphabetically.
Acknowledgements: We are extremely grateful to the Ministry of Education and Human Development in Mozambique and in particular to Director Ivaldo Quincardete and all provincial and district authorities in Manica Province. This study is funded by the Results in Education for All Children (REACH), Strategic Research Program (SRP) Trust Funds, Research Support Budget (RSB) at the World Bank, as well as the International Growth Center (IGC). The data were collected by Intercampus, Lda with special thanks to Yolanda Chongo, Ana Lopes, Duelo Macia and Vitor Silva. The interventions were implemented by Magariro, with special thanks to Cecilia João, Celia Macuacua, Raul Maharate, and Mateus Mapinde. Nicola Tissi and Vicente Parruque provided expert field coordination assistance. We are grateful to Marina Bassi, Bruno Besbas, Fadila Caillaud, Peter Holland, Sophie Naudeau, Ana Ruth Menezes at the World Bank and Alberto da Cruz, Claudio Frischtak, Novella Maugeri, Jorrit Oppewal and Sandra Sequeira at IGC for their support and guidance. We are indebted to Orazio Attanasio, Abhijit Banerjee, Felipe Barrera-Osorio, Erlend Berg, Leonardo Bursztyn, Stefano Caria, Deon Filmer, Stephan Heblich, Michael Kremer, Marco Manacorda, Owen Ozier, Berk Özler, Zahra Siddique, Hans Sievertsen, Yanos Zylberberg and presentation audiences at the University of Bristol, University of Reading, University of Sheffield, Navarra Center for International Development (Pamplona), EDePo (Institute for Fiscal Studies, London), Universitat Autonoma Barcelona, World Bank DIME seminar, Paris School of Economics, Université Paris-Dauphine, University of Illinois at Urbana-Champaign, Royal Economic Society Conference 2018, European Economic Association Congress 2018, 3rd Workshop on Labour and Family Economics (York, 2018), LACEA-LAMES Conference 2018, SOLE Conference 2019, DIAL Conference 2019, and 2nd IZA/World Bank/NJD Conference for their useful comments and suggestions. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank, its Executive Directors, or the countries they represent. This research was approved by the University of Bristol's School of Economics, Finance, and Management Ethics Committee on 14th March 2015. AEA registry trial ID number: AEARCTR-0001069. Initial registration date: February 29, 2016.

Pupil absenteeism is a pervasive issue which hinders learning and is associated with an increased risk of dropping out in both developed- (Aucejo and Foy Romano, 2016; Robinson et al., 2018) and developing countries (Bedi and Marshall, 1999; Manacorda, 2012; Glewwe et al. 2013; Bassi, Medina and Nhampossa, 2020). One popular policy tool to improve school attendance in developing countries is the use of conditional cash transfers (CCT), which are now implemented in over 60 countries (Parker and Todd, 2017) and typically incentivize parents to meet a set of conditions including children's regular school attendance.

The conditionality attached to CCTs may work through both a financial incentive channel and, when parents do not perfectly observe whether their children attend school, an information channel (Bursztyn and Coffman, 2012). If information frictions exist, then (i) sizeable attendance gains could be achieved (at a much lower cost than with a CCT) simply by providing attendance information to parents and (ii) incentivizing children themselves could be more cost-effective than incentivizing parents since children have perfect information about their attendance. In this paper, we consider both issues in a unified, randomized setting. Prior work has shed light on the effect of providing attendance information to parents without financial incentives (Rogers and Feller, 2018; Bergman, 2021; Bergman and Chan, 2021; Berlinski et al., 2021) and partially addressed the question of the relative effect of transfers to parents and children (Baird, McIntosh and Özler, 2011; Berry, 2015; Levitt, List and Sadoff, 2016). But to the best of our knowledge, we are the first to examine the relative effect of providing information with- and without financial transfers. We are also the first to compare the effect of incentivizing only parents or only children to achieve an attendance target. In doing so, we shed new light on the optimal design of CCT programs and on the agency of children in attendance decisions, with wider implications for policies targeting pupil absenteeism.

More precisely, we present experimental evidence of the effect of three alternative policies targeting Mozambican girls in the last two grades of primary school: (1) providing information to parents about their child's attendance through a weekly attendance report card ("information only" treatment); (2) providing this information and making cash transfers to parents conditional on regular attendance (CCT or "parent incentive" treatment); or (3) providing the same weekly attendance information and making transfers to children of the same nominal value as in (2) in the form of a voucher, also conditional on regular attendance ("child incentive" treatment) and redeemable against a choice of goods such as school uniforms, backpacks and shoes.

We draw three main conclusions from our experiment. First, we find that, even in a poor, rural Sub-Saharan African setting, school attendance can be increased by improving parental information on their child's absenteeism without any accompanying social transfer. Importantly, we find evidence that strongly suggests that our information treatment worked, as intended, through an improvement in parental information. In the control group, absences reported by parents in a household survey are not significantly correlated with actual absences observed during unannounced attendance checks. In contrast, in the information treatment, absences reported by parents are nearly half as strongly correlated with actual absences as what would be observed in a perfect monitoring benchmark.

Our second key finding is that the effect of the information treatment is large relative to the overall effect of a conditional transfer program providing the same information. In our experiment, where the value of the transfer is modest but similar to safety net programs found elsewhere (7% of per capita GDP),[1] the estimated effect of the information treatment on attendance is as large as 75% of the effect of the CCT treatment (4.5 and 6 percentage points, respectively). It also compares favorably with the effect of other non-monetary interventions on pupil absenteeism.[2] If the information- and financial incentive effects of the CCT are additive, this implies a small effect of financial incentives. To test whether these effects are additive, one would need an additional treatment arm in which parents are financially incentivized for the child to meet the attendance target but do not learn anything about the child's attendance. This treatment is however not feasible. Unless the conditionality is not enforced—in which case

[1] As a point of reference, in their review of CCT programs Fiszbein and Schady (2009) report that total household transfers range from no more than 4% of mean household consumption in Honduras, Bangladesh, Cambodia and Pakistan to 20% in Mexico (p.5). More recently, a safety net program funded by World Bank loans in Guinea piloted transfers worth (over a comparable time period to our experiment) between 3% and 7% of per capita GDP per child.

[2] The impact of our "information only" intervention is larger than the most effective non-monetary intervention in the literature reporting effects on pupil absenteeism for developing countries (Cunha et al. 2017, 2.1 percentage points), while the impacts of our two transfer interventions (6 percentage points for "parent incentive" arm and 8.3 percentage points for the "child incentive" arm) are similar to the 8 percentage points reported by Baird, McIntosh and Özler (2011) for conditional cash transfers targeting adolescent girls in Malawi. These relatively large effects might, in part, be explained by the low attendance rate in our control group (65%).

the CCT is *de facto* an un-conditional cash transfer, there is no such thing as a CCT conditional on school attendance in which parents are not fed back information about the child's attendance. Receiving or not receiving the conditional transfer indeed conveys information to parents about whether the child attended school regularly (Bursztyn and Coffman, 2012).[3]

Our third key finding is that incentivizing children is at least as effective in raising attendance as incentivizing parents. In other words, children's returns to school attendance matter at least as much as parental returns to their child's attendance in the decision to attend. Importantly, we rule out an alternative mechanism for our child- and parent-incentive arms to result in similar effects, namely that parents were able to appropriate transfers to children.

Taken together, these findings raise the possibility of increasing attendance at low cost relative to our CCT benchmark: the annual cost of increasing attendance by one percent is $2.68 in our parent incentive arm, compared to only $1.57 for the child incentive arm and as little as $0.33 in the information arm.[4] They also suggest that attendance gains could be achieved by sharing with parents more of the information collected as part of CCTs without the cost of enforcing further conditionality. More generally, our findings show that children have substantial agency in attendance decisions, which can be leveraged by policy.

We also find that both the information treatment and the children's incentives treatment significantly improve scores on the measure of learning that we collected (the ASER or "Annual Status of Education" math test) by 8.5 to 9.4% of the control group's mean. Consistent with most evaluations of CCTs, the effect of the parent incentives treatment is not statistically significant, although its confidence interval includes meaningful gains in learning.[5] Further investigation (Section V.C) indicates that the learning gains in the parent incentive arm are more concentrated at the top of the distribution than in the other arms, which might be explained by suggestive evidence that the

[3] Alternative designs, such as delaying conditional payments until the end of the school year, would create other issues given the independent effect of the timing of payments (Barrera-Osorio et al. 2014).

[4] While the cost effectiveness figures presented here are useful to fix ideas, a full welfare analysis would need to take into account not only the effect of each treatment on human capital accumulation but also their effect—or absence thereof in the case of the information treatment––on the reduction of current poverty (Alderman, Behrman and Tasneem, 2019).

[5] A systematic review by Snilstveit et al. (2015) estimates the average effect of CCTs on learning to be essentially zero (between -0.01 and 0.01 depending on subject).

population of children induced to attend school more as a result of the treatment (compliers) differs across treatment arms.

Prior literature on CCTs is vast and has been extensively reviewed (Fiszbein and Schady 2009; Snilstveit et al. 2015, Ganimian and Murnane, 2016; Glewwe and Muralidharan, 2016 and Millán et al., 2019). Taken together, reviews conclude that CCTs increase time spent in school nearly everywhere where they have been evaluated. Effect sizes vary, however, and the effect of CCTs on learning is often disappointing – although many estimates of learning effects suffer from selection issues. A range of studies have investigated how to optimize the design of CCTs but several gaps remain.[6]

One highly debated question is that of the role of the conditionality. When conditions are enforced and well understood by parents, CCTs have larger effects than unconditional cash transfers (UCTs) (Baird et al., 2014). But the extent to which the information component of conditional transfers contributes to their effectiveness is still unclear.[7] Bursztyn and Coffman (2012) show that parents in a poor urban Brazilian setting value the information component of a national CCT. In a lab experiment, they find that most parents only prefer an unconditional cash transfer (UCT) to a CCT of similar nominal value if, before being asked to choose between UCT and CCT, they are first offered free text messages notifying them of their child's absences. This groundbreaking study however leaves unanswered the important question of how much of the overall effect of a CCT can be obtained simply by giving parents the information contained in this CCT – which would come at a much lower cost. The first contribution of our paper is to answer this question.

Another question in the CCT literature is that of whether parents or children themselves should receive the transfers. Most prior literature has documented the effect of incentivizing parents *or* children.[8] Two studies compare

[6] For previous experimental literature studying how to optimize the design of conditional transfers see: Baird, McIntosh and Özler (2011), Baird et al. (2014), Benhassine et al. (2015) and Akresh, de Walque and Kazianga (2016) for comparisons of conditional and unconditional or "labeled" transfers; Benhassine et al. (2015), Akresh, de Walque and Kazianga (2016) and Haushofer and Shapiro (2016) for variation in the gender of the (parent) recipient; Barrera-Osorio et al. (2011) on the optimal timing of the transfers; and Skoufias, Unar and Gonzalez-Cossio. (2008) and Cunha (2014) on cash vs. in kind transfers.
[7] For a more comprehensive discussion of arguments in favor of *conditional* transfers, see Martinelli and Parker (2003) and Fiszbein and Schady (2009).
[8] Most of the CCT studies reviewed in Fiszbein and Schady (2009), Snilstveit et al. (2015), Ganimian and Murnane (2016), Glewwe and Muralidharan (2016) and Millán et al. (2019)

experimentally the effect of incentivizing parents relative to incentivizing children to achieve a grade target (Berry, 2015, Levitt, List and Sadoff, 2016).[9] Neither finds significant differences, on average, between incentivizing parents and children. Parents can have a direct input into the production of grades, however, so that the relative effectiveness of incentivizing parents or children to achieve a certain *grade* target combines their relative influence on the level of schooling inputs and their relative returns to these inputs. For instance, Berry (2015) finds heterogeneous effects consistent with a model in which it is more effective to incentivize parents (children) when the parents' (children's) input is relatively more productive. By comparing how responses to incentivizing a simple child *input* such as attendance vary with the recipient of the incentive (parents or children), we speak more directly to the agency of children in decisions regarding their schooling.[10] The only other previous study incentivizing parents and children to achieve an *attendance*- rather than a grade target (Baird, McIntosh and Özler, 2011) varies the recipient of the transfer at the *intensive* margin.[11] The authors find significant effects of transfers on schooling outcomes at the extensive margin but that further incentives beyond the minimum transfer have no effect, whether targeted to children- or parents. An open question is thus whether varying the recipient of the transfer at the margin found to matter in Baird, McIntosh and Özler's (2011) study (namely, the extensive margin) would lead to different effects. Answering this question is the second contribution of our paper. More specifically, we vary the recipient at the extensive- rather than intensive margin, equalize the nominal value of transfers to parents and children and, crucially, design the experiment to ensure that children are the end recipient of the incentive intended to them. We do so by incentivizing children not with cash but with vouchers redeemable against a

incentivize parents. See Appendix C2 for literature concerned with incentivizing children specifically.

[9] Conditions in Levitt, List and Sadoff. (2016) also include targets on attendance and behavior at school, but only 3 percent of students meeting the grade target failed to meet the overall target, and the treatments had a significant effect on grades but not on the other individual target components.

[10] Hirshleifer (2017) provides evidence that the choice of incentivizing a child input- or output matters, and in her case that incentivizing an input is more cost-effective.

[11] Baird, McIntosh and Özler (2011) experimentally vary whether a conditional transfer is unconditional or conditional on 80% school attendance, as well as the amount of cash given to parents (from $4 to $10) and that given to adolescent girls (from $1 to $5) in Malawi. In the CCT arm, they find that increasing the minimum conditional transfer amount has no effect on any outcome, irrespective of the recipient of the extra dollar.

number of items which prior qualitative work indicated as being valued by children in the research area and unlikely to be appropriated by others. Data collected at the end of the experiment confirms that children did not have to hand in these items to anyone else, and that parents did not appropriate indirectly the transfers by reducing the child's non-food private consumption.

In other related literature, recent field experiments abstracting from the question of social transfers have found that improving the information parents receive about attendance (Berlinski et al., 2021; Rogers and Feller, 2018) and other measures of student effort at school (Bergman, 2021; Bergman and Chan, 2021) in urban, middle- to high-income country settings increases attendance.[12,13] Our work is however the first to estimate the effect of only giving parents information on attendance in a rural, low-income setting, where the need to improve education levels is the most pressing, children may be expected to have less agency (Galiani, Staiger and Torrens, 2017), and the financial ability of parents to incentivize children to attend school in the absence of cash transfers is the most limited.

The amount of information embedded in CCTs found around the world varies through differences both in payment frequency and enforcement of conditions (Fiszbein and Schady, 2009; Baird et al., 2014).[14] Our aim is not to identify the effect of the information component of an elusive "average" CCT. Instead, we identify the effect of providing only high-frequency information using an easily scalable technology relative to that of implementing a well-enforced CCT incorporating the same information.

---

[12] Rogers and Feller (2018) compare the effect of a reminder of the importance of regular attendance with the effect of providing this reminder *as well as* individual information about the child's attendance record. While the reminder in itself reduces absences by 3.5%, the "reminder and attendance information" treatment reduces absences by 6.9%. Cunha et al. (2017) compare the effect of text messages to parents *either* about the importance of school attendance, punctuality and assignment completion *or* containing information about the performance of their children on these three outcomes and find that both treatments have similar effects. Taken together, these two studies suggest that reminding parents of the importance of attendance and other behaviors can have an effect in itself, and that the magnitude of this "salience" effect can be roughly similar to that of providing information only. In our experiment, we only provided information, not reminders of the parents' responsibilities, and only provided information on the child's attendance.

[13] For conciseness, we focus here on the literature interested specifically in the information asymmetry between parents and their children in the area of education. See Appendix C1 for an overview of other related work.

[14] Out of forty CCTs for which Fiszbein and Schady (2009, Table A.3) report payment schedules, twelve are monthly, fourteen bimonthly, nine quarterly and five less frequent.

In the remainder of the paper, we present the study context, theoretical motivation for, and design of our experiment (Section I), then turn to a description of the data and randomization process (Section II), before reporting our main results (Section III) and various robustness checks (Section IV). Section V explores the mechanisms behind our results. Section VI concludes.

I- Institutional Context, Theoretical Motivation and Study Design

A. Institutional Context

Mozambique is a predominantly rural country in South-Eastern Africa (68.4% of the population lives in rural areas, INE 2015) and, with a Human Development Index ranking 181 out of 188, is one of the poorest countries in the world. The country's recent history has been marked by a 15-year civil war following independence in the 1970s, and occasional clashes between armed forces and RENAMO's armed militias in the center of the country.

Despite large increases in enrollment rates in lower primary school grades, most children are still not completing primary education. The net intake at Grade 1 of primary schooling is high for both boys (74.5%) and girls (73.1%). But most children in Mozambique, and girls in particular, experience difficulties in completing primary school.[15] As of 2017, the survival rate to the last grade of primary school was only 41% for boys and 37% for girls (UNESCO Institute for Statistics), justifying our focus on girls in upper primary schooling (Grades 6 and 7 or Ensino Primário de Segundo Grau "EP2"). Twenty two percent of children aged 5-14 engage in child labor nationwide and this share is, if anything, slightly higher among the children in this age group who are also enrolled in school (25%, National Statistics Institute 2009). Engaging in child labor does not appear to hinder enrolment, but it may contribute to pupil absenteeism.

While high teacher absenteeism is a well-known issue across the developing world in general and in Mozambique in particular, the latest World Bank's Service Delivery Indicators for Mozambique indicate that pupil absenteeism is much higher (45.8% pupil absenteeism versus 29.8% teacher absenteeism in 2018) and that pupil absenteeism is the strongest predictor of student test scores

[15] The net intake equals the ratio of the total number of pupils in Grade 1 of the official starting age (6) divided by the number of children age 6. Figures are taken from World Bank Education Statistics Data Bank (2017) unless stated otherwise.

among a wide range of school, teacher and pupil-level characteristics (Bassi, Medina and Nhampossa, 2020).

We focused on 173 co-educational schools comprising over 16,000 EP2 female students in one province of Mozambique where our implementation partner—the development NGO Magariro—is active and well-known: Manica. Manica Province is located in the Center Region of Mozambique, it is home to 7.5% of the country's population, and is close to the national average on a number of indicators.[16]

Mozambique in general, and Manica Province in particular, have low population density even for Sub-Saharan Africa standards (where the average was 42.6 in 2015 compared to 31.3 in Mozambique), but not dissimilar to other countries in Eastern Africa (36.7% in Kenya, for instance). This may matter in our context because our study design and findings speak to the imperfect monitoring of the children's actions by parents, which is plausibly more likely when population density is low and the school is located farther away from the child's home.

B. Theoretical Motivation

Becker (1974) shows that an altruistic parent can incentivize his/her child to do what is optimal according to the parent. Therefore, from a policy maker's point of view, it suffices to incentivize the parent to achieve a desired outcome such as school attendance. Bergstrom (1989) however demonstrates that the theorem does not necessarily hold in the presence of moral hazard.[17] Bursztyn and Coffman (2012) propose a simple principal-agent model to illustrate the effect of asymmetric information in attendance decisions. Their model can be

[16] E.g., population density (30.3 people per square meter compared to a national average of 31.3), poverty rate (41% in 2014 compared to a national average of 46.1%), annual drop-out rates in primary schooling (6.8% in Manica and countrywide for EP1, 9.9% versus 8.8% nationwide for EP2) and teacher (28.1% compared to 29.8% overall) and pupil (50.9% vs. 45.8% overall) absenteeism (INE, 2015; MPD-DNEAP, 2016; Bassi, Medina and Nhampossa, 2020).

[17] A number of models of parent-children interactions in which children do not have private information about their schooling effort have been proposed. An excellent recent review can be found in Doepke, Sorrenti and Zilibotti (2019). In keeping with our experimental focus, here we discuss a model in which the parent acts as a principal who does not fully observe whether the child attends school.

summarized as follows. Consider the parent-child pair indexed by n for whom adult utility is:[18]

$$U_n^a = \begin{cases} V_n^a \; if \; e_n = 1 \\ 0 \; if \; e_n = 0 \end{cases} \qquad (1)$$

Where e_n indicates whether the child chooses the high or low effort action (here, attending school or not), and the child's utility is:

$$U_n^c = \begin{cases} V_n^c - c_n \; if \; e_n = 1 \\ 0 \; if \; e_n = 0 \end{cases} \qquad (2)$$

Where c_n is the utility cost of effort experienced by the child. V_n^a is the benefit the adult derives from the child's education, net of costs borne by the adult (such as foregone child labor). If $V_n^c \geq c_n$, the child attends school even without further incentives, irrespective of the parent's ability to monitor her attendance. If $V_n^c < c_n$ and $V_n^a < c_n - V_n^c$, then the child does not find it privately optimal to attend school, and it is not optimal for the parent to incentivize the child to go to school irrespective of the parent's ability to monitor her attendance. If, however, $V_n^a \geq c_n - V_n^c > 0$, then whether or not the child goes to school depends on whether it is optimal for the parent to incentivize the child to attend, which depends in turn on the quality of the parent's monitoring technology. To see this, define the signal technology as:

$$\Pr(s_n = 1|e_n = 1) = \Pr(s_n = 0|e_n = 0) = \pi, \pi \in \left]\tfrac{1}{2}, 1\right] \; (3)$$

A parent can only condition transfers to the child based on signal s_n, which is correct with probability π.[19] Assuming limited liability on behalf of the child, the adult will find it optimal and feasible (i.e., incentive-compatible from the child's point of view) to incentivize the child only if:[20]

$$V_n^a \geq \frac{\pi}{2\pi - 1}(c_n - V_n^c) \qquad (4)$$

Where the probability of inequality (4) holding increases with signal quality (higher π). Therefore, under imperfect information, simply providing

---

[18] Note that the discussion extends to the case where the payoffs associated with education are only received with probability p<1 (e.g., the probability of finding a skilled job). To see this, replace V_n^i , $i = c, a$, with pv_n^i and define v_n^i as the benefit received by agent *i* if the child finds a skilled job.

[19] When π is just larger than $\frac{1}{2}$, there is close to no information contained in the signal since the parent's inference is only marginally superior to a random guess, while the case $\pi = 1$ corresponds to the full information case.

[20] Denote w_n the transfer made by the parent to the child if $e_n = 1$ and $\overline{w_n}$ the minimum payment such that the child's expected payoff is at least as large when $e_n = 1$ than when $e_n = 0$. Condition (4) is obtained by maximizing the adult's utility subject to the incentive compatibility constraint $w_n \geq \overline{w_n}$ and the limited liability constraint $w_n \geq 0$.

information to the parent may induce higher attendance. Enforced CCTs give the parent, at a minimum, a binary signal as to whether the child met the attendance requirement upon which payments are conditioned. The conditionality may therefore in itself lead to higher attendance, as pointed out by Bursztyn and Coffman (2012). This motivates our test of whether giving parents information about their child's attendance has an effect on attendance. This also motivates our novel test of the extent to which the effect of giving this information and nothing else differs from that of a CCT program providing the parents with the same information as part of the program.

In addition, we make the new observation that, under imperfect information, incentivizing the child should be more cost-effective than incentivizing the parent because of the informational wedge $\left(\frac{\pi}{2\pi-1}\right)$. (Indeed, increasing V_n^c by some transfer τ makes inequality (4) more likely to hold than increasing V_n^a by the same amount (since $\frac{\pi}{2\pi-1} > 1$). This motivates our comparison of the additional effect (relative to improving information only) of conditional transfers aimed at parents to that of conditional transfers aimed at children. We note, however, that the differential effect of incentivizing children rather than parents increases with the informational wedge $\left(\frac{\pi}{2\pi-1}\right)$. As π gets closer to one, so too does $\left(\frac{\pi}{2\pi-1}\right)$. A conditional transfer program which substantially improves parental information such as ours should therefore lead to a reduction in the additional effectiveness of incentivizing children relative to parents, and thus provide a lower bound for this additional effectiveness.

C. Study Design and Motivating Evidence

Qualitative and Survey Evidence of Imperfect Monitoring by Parents. To assess the relevance of our analytical framework, as well as to help define the design of the Randomized Controlled Trial (RCT) described below, we first undertook a qualitative analysis in the province where the RCT took place (in areas that were not included in the trial). The information gathered during focus group discussions with parents and (separately) with their daughters age 11-15 gives support to the hypotheses that:

(i) both parents and girls of this age have an influence on school attendance decisions and

(ii) children have private information on their school attendance.

For instance, several parents contrasted different daughters of theirs, with one sibling going to school regularly without any problem, and another dragging their feet, arriving late at school, making excuses not to go, or simply skipping school. E.g., the child would leave home with the appearance of going to school but then turn around and go back home while their parents are out for work. See Appendix E for further detail including quotes.

In addition, data collected in the baseline household survey in the experimental sample asked parents (both in treatment and control areas) whether they thought it would be useful to see a weekly report showing whether their daughter had attended school regularly. Eighty percent of parents responded "yes" to this question and when probed as to why they thought it would be useful, 98% responded that it would allow them to monitor their child's school attendance.

We were unable to implement attendance checks in schools prior to the experiment, but we can compare absences reported by parents in the endline survey and actual absences in the control group. In Section V.A, we show that the two are not statistically significantly correlated, which confirms that, absent intervention, monitoring is imperfect in our setting.

Choice of child incentives. Other than providing a first pass confirmation of the relevance of our analytical framework to the study area, the preliminary focus groups aimed to establish how to incentivize girls effectively and in a manner that would be acceptable to the local population. We found that, in our setting where 80% of girls are below 13 at baseline, giving cash to girls would make both the girls and their parents uncomfortable. We also concluded that, if they did receive cash, they would probably give it to their parents (or be expected to). On the other hand, we identified a number of items which, if given to them to reward regular school attendance, would be welcome by the girls and would be likely to remain in the girls' possession.

Experimental Groups. Given these insights, we defined the following four experimental groups (as summarized in Panel A of Table 1). In two of the experimental groups, we introduced transfers conditional on achieving at least 90% attendance during the school trimester. In a "child incentives" treatment arm, we gave money-equivalent vouchers at the end of each trimester to girls in Grades 6 and 7 who could then use the vouchers to buy a selected number of items such as: school uniforms, shoes, backpack, smaller materials (soap, toothpaste, pens, notebooks, etc...). These items were delivered at the school by the research team and could be purchased during the research team visit. The choice of items made available was based on the preliminary focus group interviews.

In a "parent incentives" arm, we instead gave the same value in cash to the parents and made the same items as in the "child incentive" arm available for *optional* purchase at the school. In both transfers arms, the value of the transfers was 400 meticais[21] per trimester with a maximum of 1,200 meticais over the 2016 school year or about 7% of per capita GDP. It was clearly explained that there was no expectation as to how the parents would spend the money, and the items were available for purchase at a short distance from the desk at which the cash was distributed to avoid pressurizing the parents. As in any comparison between treatments involving different recipients, it is very difficult to equate marginal utilities experimentally. But our design answers the policy question of what recipient to target with a fixed budget since the parent- and child incentives have the same nominal value. To reinforce comparability, the price of items in vouchers also matched their price in Mozambican meticais. By using vouchers rather than cash, we increased social acceptability of the program and, as discussed in Section V.B, succeeded in ensuring that the end recipient of the conditional transfer was the recipient intended by the study design.

In both conditional transfers arms, the conditionality was enforced by the implementing NGO based on the information contained in attendance report cards. These simple report cards (a sheet of paper inside a plastic pocket) had a coding easily understood by parents and clearly labelled on the report card. The teacher simply drew a circle for a given day if the girl attended school that day, or the teacher marked a cross for each day missed. The report cards were given to the girls at the end of each week to show their parents and brought back to school at the start of the next week. The ministry of education guidelines ask schools to report absences to parents once per trimester. A sizeable minority of schools in our sample routinely notified parents of *repeated* absences at baseline, but only three schools systematically reported absences on a weekly basis prior to the experiment. The report card system was explained by the implementing NGO during an initial visit to the school community publicizing

[21] 400 Mozambican meticais was worth US$8.36 on January 1, 2016 but only US$5.62 on December 31, 2016, as the exchange rate deteriorated substantially over the course of the (school) year.

the intervention. All girls enrolled in EP2 in the conditional transfer schools—irrespective of the recipient of the transfers—were eligible for transfers and thus given attendance report cards. From the point of view of parents, both conditional transfers treatment arms therefore have the same informational content.

In a third treatment arm, we applied an "information only" treatment, in which we introduced the same attendance report card system as in the CCT and girls' vouchers arms, but where attendance was not incentivized by vouchers or cash transfers. A fourth experimental group constituted the control group.

To ensure the quality of the data recorded in the attendance report cards, and given the extra work required from the teachers responsible for each class ("directores de turma") to fill out those cards, we introduced a small compensation scheme. The scheme worked as follows. In the three treatment groups, the teachers who, at every spot check by the independent surveyor, were found to have thoroughly filled in all their (female) pupils' report cards for the current trimester until the day of the spot check, received 250 meticais' worth of mobile phone credit ("airtime") at the end of the trimester.[22] The value of 250 meticais corresponds to the opportunity cost of about 5 minutes per day, evaluated at the hourly salary equivalent of the average teacher. The school directors of all schools, including the control group, received 250 meticais in airtime at the end of each trimester without conditions.

Compliance. Compliance was high, but not so high that our treatment effects are likely to only speak to the efficacy of our interventions in an ideal experimental setting rather than their effectiveness at scale. Across the three treatment groups, 66.4% (information only), 69% (CCT) and 69.2% (child incentive) of parents and 69.8% (information only), 72% (CCT) and 73.2% (child incentive) of girls interviewed in the endline household survey said that they knew about the treatment. While not perfect, this level of knowledge is reassuringly high and similar across treatment arms. Among those who said they knew about the treatment, the most common outcome was to obtain the transfer twice out of three trimesters (in about 50% of cases), while just under 8% did not receive any transfer, which indicates that the conditions for transfers were

[22] Teachers were not rewarded for the accuracy of their attendance reporting for two main reasons. First, to ensure the continued support of teachers. Second, to limit the amount of time spent by the enumerator in each class and thus prevent the possibility for manipulation of the data for one class while the enumerator checked the other.

enforced – as is the case in about half the CCT programs reviewed in Baird et al. (2014, see Figure 5). Less than 6% of enrolled girls went to a different school from that from which they were sampled, and school switches are independent of experimental arm (Appendix G). Although it took the experience of the first trimester for teachers to adjust to the high standards required to obtain the airtime reward (at least 80% of reports fully filled and without obvious errors), teachers also largely complied. At the end of the first trimester, only 57% of teachers were rewarded compared to 83% (82%) at the end of the second (third) trimester.[23]

II- Data, Randomization and Experimental Balance

A. Data

Independent, unannounced, attendance checks ("spot checks"). The main outcome of interest for the evaluation is whether a girl enrolled in school was present during independent attendance "spot checks" by the survey firm. Up to twice per trimester, an enumerator arrived unannounced at each school in the sample and recorded in person the individual attendance/absence of every child enrolled in EP2. The attendance rate triggering transfers in the conditional transfers arms was calculated by the implementing NGO solely based on the information contained in the attendance report cards described in Section I-C. No incentive was paid on the basis of the presence or absence of pupils during the attendance spot checks and therefore there is no reason to expect the data to be manipulated.

Baseline and endline household surveys. Household surveys collected basic household information as well as, for each eligible girl in the household: data on self-reported quality of attendance monitoring by the parent or guardian, degree of agreement about statements regarding returns to education for each child, self-reported girl empowerment, expenditure on 23 personal items consumed by the eligible girl, and an ASER math test (at endline only).

The ASER test was developed in 2005 in India by the Pratham network to assess the skills of children aged 5 to 16. It has been used in India and elsewhere (e.g., in Morocco by Benhassine et al., 2015) since then and has been comprehensively validated (Vagh, 2009). Test takers can achieve one of five

[23] Unfortunately, the NGO records do not allow us to match rewarded teachers to schools or treatment arms.

levels: recognition of single digit numbers (scored 1), recognition of double-digit numbers (scored 2), correct subtraction (scored 3), correct division with remainders (scored 4), or cannot even recognize single digit numbers (scored 0).[24] The official Sixth (Seventh) Grade mathematics curriculum allocates 60 hours to the recognition, addition, subtraction, multiplication and division of numbers up to 1 Million (Billion) (MINEDH 2015). The average control group score in our analytical sample is however only 2.16, which indicates that the level of difficulty of the test was appropriate (see also Figure A-1 for the distribution of scores by experimental arm).

Household survey sample. The household data used in the analysis are based on a sample drawn from the universe of girls enrolled in the 173 schools included in our study within three years of the start of the intervention based on school records, as in Benhassine et al. (2015). More specifically, this included girls who (i) had completed, at least, 5th Grade and, at most, 6th Grade (and were therefore potentially eligible for our treatments implemented in Grades 6 and 7 in 2016), and (ii) still lived with their parent or guardian at baseline (given our analytical framework based on asymmetric information between parents and children).[25] The target was to interview 20 girls per school, sampling those enrolled in 2015 (the last school year before the experiment) and recent dropouts who were not enrolled in 2015 but were enrolled in 2013 or 2014, proportionally to the size of each of the two groups ("enrolled in 2015" and "recent dropouts"). During fieldwork, however, there were difficulties locating the girls listed in the school records, and most of the recent dropouts had either moved away or were not living with their parents anymore and were thus ineligible for interview. The sampling target of 20 per school was therefore not attained in many of the smaller schools, and where possible more than 20 girls were sampled to help preserve power. All in all, the median number of girls surveyed per school in the baseline household survey is 18, and recent dropouts were under-

[24] More specifically, a score of 1 (2) is obtained if at least four out of five single (double) digit numbers are recognized correctly, a score of 3 is obtained if two out of two subtraction problems are correct, and a score of 4 is obtained if one division problem out of one is correct. The child can choose which numbers/subtraction/division to attempt out of a choice of between 4 (divisions) and 10 (two-digit numbers).

[25] Table A-1 reports summary statistics on comparable socio-economic status (SES) indicators for our sample and for the 2015 HIV/AIDS and Malaria Indicator Survey. Our focus on upper primary schooling implies that our sample has higher SES than the average household in Manica, but is still very poor (e.g., only 35% have improved forms of sanitation compared to 25% (40%) in representative samples for Manica (Mozambique as a whole).

represented in the household survey sample (3% of the baseline sample compared to 13% of the universe). There was no difference in the total number of girls interviewed in the baseline household survey, or the share of recent dropouts in the household survey sample, across treatment arms, however.[26] The analysis uses data from both those enrolled in 2015 and those not enrolled in that year. Given the very small number of recent dropouts, we do not study them separately.

Timing. The Mozambican school year runs from February to December. We collected a baseline household survey between the end of the 2015 school year and the start of the 2016 school year, and a follow-up survey one year later (See Figure 1). After assigning randomly each school to an experimental arm, our partner NGO obtained the consent of all the directors for their school to take part in the research. Each school then received an initial visit by Magariro to publicize the intervention in the school and explain the details of what participation would entail. Staff in all schools were invited to an information meeting in which they were informed that there would be unannounced visits by the survey firm to independently collect attendance data between one and three times per trimester throughout the school year. In treatment schools, the initial meeting was also open to pupils and parents of the relevant grades, and the relevant intervention was explained, attendance report cards distributed to the school, and questions answered. The intervention started at the beginning of the 2016 school year (February 5) or as soon as the treatment was announced, if announced after the start of the academic year, which was the case for the vast majority of schools.

The official enrollment period ended on January 6, 2016, i.e. at least a week before the initial "announcement" visit by the implementing NGO (which started on January 14 and ended on March 3).[27] The communities included in the experiment would therefore have had no prior knowledge of it until after enrollment decisions were made, especially considering the likely delay in spreading information to the parents of marginal enrollees.

[26] The maximum difference between any two experimental arms is 0.8 girl (p-value: 0.47) for the number of girls interviewed and 1.4 percentage points difference in the share of recent dropouts (p-value: 0.21).

[27] There is no statistically significant difference in the timing of the treatment announcements across treatment arms relative to the start of the academic year. More precisely, when regressing the number of days between treatment announcement and the start of the academic year on two treatment indicators (where the third treatment is the omitted category) and a set of district fixed effects, the p-value of a joint F-test of significance of the two treatment coefficients is 0.72.

Initial visits by Magariro to announce the treatments took place after the start of baseline survey collection in all but one school. But given the delays in completing the baseline survey caused by political tensions between RENAMO and government forces and by heavy rains, in just under 22% of schools, the baseline survey was completed after the initial visit in which the NGO announced the treatments.[28] There is however little reason to believe that it should have affected data on the baseline socioeconomic indicators for which we test balance at baseline, since the interventions were not means-tested, and more generally there was no room to manipulate the eligibility criteria (gender and grade).

Panel B of Table 1 presents the allocation of schools and girls across the four study groups as well as the attrition rate for girls sampled for the household survey. Attrition of girls taking part in the household survey was limited at 5.3% overall. It was slightly larger in magnitude in the control group than in the treated groups although, but the p-value of a joint F-test of no treatment effect in a regression of the share attrited on the three binary treatment indicators and district fixed effects is indeed above 0.10 (0.153). Robustness checks show that attrition is unlikely to be driving our conclusions (see Section IV and Appendix G). Our main outcome of interest (independently verified attendance rate at school) is available for *all* schools between one (for 3 schools) and 6 times (for 132 schools). On average, it was measured 5.6 times during the school year, and the number of times each school was surveyed is independent of experimental arm (p-value: 0.55).

B. Randomization and Experimental Balance

We first stratified our sample of 173 schools by district. We then split the schools included in our study, within each district, randomly between the four experimental arms (one control and three treatment arms) using a random

[28] There is no statistically significant difference in the timing of the treatment announcements across treatment arms relative to the average baseline household interview date. More precisely, when regressing the number of days between treatment announcement and average household interview dates on two treatment indicators (where the third treatment is the omitted category) and a set of district fixed effects, the p-value of a joint F-test of significance of the two treatment coefficients is 0.54. More generally, there are no statistically significant differences in the timing of the baseline survey across the four experimental arms, be it in terms of start date, end date, average date or duration.

number generator.[29] At the time of the announcement of the treatments, a human error led to two schools in the Vanduzi district being swapped. Throughout this paper, we classify each school based on their randomly assigned treatment arm, but our findings are robust to assigning treatment based on actual treatment status instead of intended treatment status.[30]

Table A-2 presents summary statistics for all the socioeconomic indicators measured in the baseline survey and characteristics of the eligible girls and self-reported monitoring technology relevant to our research framework. Stars indicate statistically significant differences between each treatment arm relative to the control group.[31] Table A-2 suggests that the randomization of experimental arms worked well in practice. For each pair of experimental arms, an F-test cannot reject that the baseline characteristics listed in Table A-2 are jointly orthogonal to treatment status. More specifically, the p-value associated with an F-test that the set of characteristics listed in Table A-2 does not explain the experimental arm classification within district is between 0.17 (Information v. Parent) and 0.52 (Control v. Child) for all 6 experimental arm pairs, and thus the null of joint orthogonality cannot be rejected.[32] Some individual differences are, however, statistically significant, and thus we provide robustness checks controlling for these baseline characteristics.[33] Other than for the many language and religion categories, the only other variables with some significant baseline differences are school absences reported by parents (for October 2015) and, to a marginal extent, self-reported quality of child attendance monitoring by the parents. We do not have reliable attendance data with which to compare

[29] In districts where the number of schools was not a multiple of four, one of two rounding rules was first selected at random to determine the number of schools to assign to each experimental group before assigning schools randomly to experimental arms. Rounding rule 1 stated that the number of schools in the control group should be rounded up, and that in both conditional transfers arms be rounded down. Rounding rule 2 stated that the number of schools in the control group should be rounded down, and that in both conditional transfers arms be rounded up. The residual experimental arm was the information treatment arm, which explains that slightly fewer schools fall in this experimental arm (41 compared to 44 in all the other arms). For instance, in the Vanduzi district, where there are 21 schools, the randomly selected rounding rule was rule 2, resulting in 6 "parent cash", 6 "child incentive" schools, 5 control schools and 21-17=4 "information" schools.

[30] Full results available on request.

[31] Based on a t-test of $\beta_g = 0$, $\beta_p = 0$ and $\beta_i = 0$ respectively, obtained from estimating Equation (4) with each baseline characteristic, in turn, on the left-hand side.

[32] The other four p-values are as follow: Control v. Parents: 0.35; Control v. Information: 0.18; Girls v. Information: 0.42; Girls v. Parents: 0.25.

[33] We also report results obtained when controlling for baseline variables that are predictive of the outcomes studied, as suggested in Bruhn and McKenzie (Table A-11).

parent-reported absences prior to the 2016 school year, but we can compare parent-reported absences for October 2016 to our attendance spot check data for the same period in the control group. As we show in Section V.A, the number of absences reported by parents does *not* predict actual absences in the control group. In addition, the coefficient of correlation between the number of parent-reported absences at baseline and endline is only 0.038 (in the control group). This suggests that the differences in parent-reported absences at baseline do not reflect a genuine difference in baseline absenteeism. This is confirmed in our analysis, where we show that our conclusions are robust to controlling for baseline characteristics including parent-reported absences.

III- **Main Results**

In this section we report and discuss cluster-level estimates based on Equation (4):

$$Y_c = \beta_0 + \beta_g T_{gc} + \beta_p T_{pc} + \beta_i T_{ic} + \boldsymbol{D}_{\boldsymbol{c}}' \boldsymbol{\beta}_{\boldsymbol{d}} + \varepsilon_c \qquad (4)$$

Where Y_c is the cluster (i.e., school) average for outcome Y; T_{gc}, T_{pc} and T_{ic} are indicator variables for the girls' incentives, parents' incentives, and information only treatment arms, respectively; $\boldsymbol{D}_{\boldsymbol{c}}'$ is a row vector of 10 district (i.e., strata) fixed effects (as there are 11 districts), and ε_c is an iid error term. β_g, β_p, and β_i can be interpreted as average treatment effects for our sample of 173 schools, giving each school an equal weight, or unweighted average treatment effects. We follow the advice in Athey and Imbens (2017, p.111), and analyze the data at the cluster level given our cluster-randomized design, but also report sample-weighted estimates in a robustness check. Our setting matches what Athey and Imbens (2017) describe as the type of experiments where this is particularly appropriate. First, our main substantive questions are whether our innovative treatments ("information only" and "girls' incentives") have any effect and how significant the additional effect of incentivizing parents or girls is relative to only providing information. In that sense, the unweighted average treatment effect is as valid as the population-weighted average treatment effect. Second, while we have many small schools, we also have a few very large schools—50% of schools have no more than 62 girls in EP2, but 5% of schools have between 311 and 622 EP2 girls, so that inferences for the unweighted average treatment effect are likely to be more precise than for the population average treatment effect. Furthermore, Young (2019) shows that the alternative approach of estimating treatment effects at the individual data level and clustering standard errors at the cluster level may lead to over-rejection of the null of no treatment effect. For completeness, in Table A7 we report estimates at the individual level and find similar results (see Section IV).

A. Effects on School Attendance and Self-Reported Enrollment

Table 2 presents estimates of the impact of the different interventions on pre-specified schooling outcomes. In Columns (1) and (4), we report findings for our primary study outcome, i.e., school attendance. This is measured as the share of girls in the targeted grades who were found in their classroom by the independent surveyor during unannounced school visits. The two columns present estimates of Equation (4) with and without controlling for the (school average) baseline characteristics listed in Table A-2 for which a t-test rejects equal means at baseline for at least one treatment arm.

Compared to the control group, all three interventions significantly and substantially increased school attendance. Compared to a control group mean of .65, the information only treatment increased attendance by 4.5 percentage points (6.9%), the parent cash treatment increased attendance by 6 percentage points (9.2%), and the child incentive treatment increased attendance by 8.3 percentage points (12.8%). The p-values reported at the bottom of the table show whether the coefficients for each of the three interventions are statistically different from each other. The first row of p-values indicates no significant difference in impacts between the "information only" and "parents incentives" interventions. This leads to the conclusion that providing attendance information to parents can have a substantial effect on school attendance independently of any transfer even in very poor settings. In our experiment, where the value of the transfer is equivalent to 7% of the national GDP per capita—a non-negligible sum for poor households—the estimated effect of the information treatment on attendance is as large as 75% of the effect of a CCT providing the same information. We take this finding as evidence of the powerful role of information even when provided on its own. We are, however, agnostic as to whether the effects of providing information and financial incentives are additive. This may, for instance, not be the case if there are ceiling effects limiting the total potential increase in attendance.

Incentivizing girls directly is nearly twice as effective as simply providing information, and in our baseline specification (Column (1)), this difference is statistically significant at the 10% level. We cannot reject the hypothesis that incentivizing parents and children has the same effect, although the point estimate associated with the child incentive treatment is consistently larger than that associated with the parent incentive treatment—in short, incentivizing children is at least as effective as incentivizing parents. In Section V.B, we provide supportive evidence in favor of this interpretation rather than the competing mechanism that parents were able to appropriate (directly or indirectly) the transfer intended for the child.

In Column (4) of Table 2, we present estimates of the effect of our treatments on attendance obtained when controlling for the baseline characteristics for which there was at least one statistically significant difference between experimental arms and confirm that results are virtually unchanged.

Columns (2) and (5) report results on the effect of our treatments on school enrollment as reported by parents in the household survey. Starting from a high enrollment rate (95% in the control group) and given the fact that the intervention was announced after the end of the official enrollment period (and, in most cases too, after the start of the school year), it is not surprising to confirm that our interventions had no effect on enrollment decisions. The CCT seems to have had a small impact, increasing enrollment by 2.7 percentage points in the baseline specification, but when controlling for baseline characteristics (Column (5)), the point estimate decreases and a t-test cannot reject the null of no effect (p-value: 0.21). Based on this and further tests confirming that the effect on enrollment is not robust (Section IV), we conclude that the effect on enrollment was negligible and therefore that our results on attendance are not driven by selection into being enrolled in school. [34, 35]

[34] Funding limitations prevented us from collecting further data at the school level. Our only post-treatment attainment data therefore comes from the endline household survey — namely, the eligible girl's highest completed grade, which we did not pre-specify as an outcome. None of our treatments has a statistically significant effect on either highest completed grade or on an indicator for completing primary education defined as having completed at least 7 grades (Table A-3 Panel A).

[35] We do not have attendance data for the eligible girls' siblings, so it is not possible to test for substitution/complementarity effects in attendance between siblings. Enrolment data for 2016 as reported by parents suggest that enrolment of siblings, and especially that of sisters, significantly increased (in the case of sisters, by 8-11 percentage points from a base of 65% in the control group, Table A-3 Panel B). Caution should prevail in interpreting these unverified self-reported data since parents in treatment arms may erroneously expect that enrolment of

B. Effects on Test Scores

Rigorous evidence of the effect of conditional cash transfers (to parents) on test scores is limited. This evidence is often based on school data and thus potentially affected by selection into being present at school at the time of the test. This might contribute to the effect of cash transfers on test scores being generally insignificant. Indeed, a systematic review by Snilstveit et al. (2015) estimates their average effect to be essentially zero (between -0.01 and 0.01 depending on subject).[36]

In Columns (3) and (6), we report treatment effects based on test scores at a math (ASER) test administered to eligible girls in our endline household survey. Our findings are therefore not affected by the type of selection bias which may undermine test scores effect estimates from CCTs based on school tests. Given the small effects on learning observed elsewhere and survey time constraints, however, we only included one cognitive test in our survey, which limits what conclusions we can draw regarding the effect of our treatments on learning.

Both the information treatment and the girls' incentives treatment improve math scores by 8.5 to 9.4% of the control group's mean or .17 and .19 of a standard deviation when considering the distribution of scores across the girls tested in the control group.[37] For a mean attendance of 65% and 190 days of instruction per year, our control group receives an average of 123 days of instruction. The observed increase of 4.88 (8.41) percentage points in daily attendance in the information (child incentive) arm translates into 9 (16) more days of instruction, or an 8% (13%) increase, to be compared with 8-9% increases in average test scores relative to the control mean. We lack continuous measures of learning to test this hypothesis, but a possible explanation for these large learning effects is that, absent treatment, a sizeable share of girls are not far from achieving the next ASER threshold (e.g., they are able to recognize three two-digit number out of five correctly but not four, or are able to execute correctly one- but not two subtractions) and that the interventions allow a substantial fraction of them to reach that next level

siblings will make them eligible for some future intervention. But the size and direction of estimates suggest that large substitution effects are unlikely.

[36] See Millán et al. (2019) for examples of cases in which a CCT was found to increase learning.

[37] To ease comparisons with previous work, here we refer to the standard deviations in the distribution of tests scores at the individual level (mean 2.19 and SD 1.083) rather than the distribution at the school average level (mean 2.16 and SD 0.567).

Gains in attendance from incentives to parents do not translate into significant gains in test scores. Importantly, however, we cannot reject the possibility that the parent incentives treatment has a modest but positive effect on math scores since the p-value of a test that the effect is equal to 0.1 (or about 0.09 of a standard deviation) is above 0.25. In Section V.C, we summarize our exploratory analysis of the causes behind the heterogeneity in learning effects at the mean across treatment arms.

C. Effects on Pre-specified Non-Schooling Outcomes

Table 3 reports estimates of the impacts of our interventions on all the pre-specified non-schooling outcomes (see Appendix F for details about the outcomes specified at the time of the registration of the trial).

In Columns (1) (without controls for baseline characteristics) and (5) (with controls), we test whether our treatments had any effect on teacher absenteeism. The rationale for pre-specifying this outcome was to shed light on the mechanisms behind the effect of our treatments on attendance. As discussed in more detail in Section V, we find no significant effect on teacher absenteeism, which supports our interpretation of the attendance effects as coming from a demand-side mechanism.

In Columns (2) and (6), we estimate the effect of our treatments on ever having been married. Given the young age of the targeted girls (12.65, on average, at baseline), only 2.28% (2.66%) of eligible girls in the household survey were married at baseline (endline) in the control group. For the mean and standard deviation that prevail in the control group, we lack power to detect realistic reductions in the proportion ever married (see Table A-12). The point estimates of the effect of the information only and the parents' incentives treatments on the proportion ever married are large in magnitude. But only the effect of the information treatment is statistically significant at 10% in the baseline regressions, and it becomes insignificant when controlling for baseline characteristics (Column (6)).

The remaining columns of Table 3 show tests of whether the treatments had any effect on the share of girls with an above-median predicted score based on two separate principal component analyses (PCA). The first variable measures the self-reported quality of the monitoring exercised by parents on their children's school attendance, while the second one measures the extent to which the girls say that they participate in decisions about their own lives. The set of interventions evaluated in this experiment had no impact on either measure.

There are, however, several reasons to believe that the self-reported measure of monitoring quality is a poor proxy for actual monitoring quality. First, only 3.4% (6.6%) of parents answered “neither agree nor disagree” or “disagree” when asked at baseline whether, at the end of each day, they know whether their daughter was at school (in the classroom). And only 6.2% answered that it had happened at least once that, on a particular day, they thought that the girl was at school but actually she was not. This contradicts their answers to the less stigmatizing question, asked at baseline, of whether they thought it would be useful to receive a weekly attendance report card showing if their daughter had attended school regularly. Indeed, 80% responded that it would be useful, thus suggesting that most of them think that their monitoring is not perfect. And in the confidence of the focus groups conversations between parents discussed in Section I.C, many parents did express concerns about their ability to monitor their daughters’ attendance.

Fortunately, our data allow us to compare the predictive power, across experimental arms, of the number of absences reported by the parent on whether the girl was absent at school during a spot check, thus providing a more objective measure of monitoring quality. We analyze the correlation between absences reported by parents and observed during attendance checks in Section V.A and find evidence that monitoring is poor in the control group but much improved in our treatment groups.

To summarize, we find evidence that providing high-frequency information to parents about their daughter’s school attendance increases school attendance even in the absence of any transfer, and that this effect is not statistically distinguishable from that of a CCT to parents also providing the same information. Incentivizing girls with vouchers allowing them to buy a choice of goods is at least as effective as incentivizing parents with the cash-equivalent of these vouchers. In terms of learning, the attendance gains from the information only and girl incentives treatments translated into significant improvements in scores at the ASER math test. None of the treatments had a robust effect on enrollment (by design), teacher attendance (as intended), early marriage, self-reported quality of parental monitoring and self-reported girl autonomy. The next section explores the robustness of these findings before turning to the analysis of the mechanisms at play in Section V.

IV- **Robustness Checks**

Fisher randomization and joint testing. Our baseline treatment effect estimates are implemented through regression analysis and thus rely on asymptotic theorems. For individual coefficient tests, the two main issues highlighted by Young (2019) when relying on asymptotic theorems are related to: (i) high-leverage (which only arises with the inclusion of covariates) and (ii) clustered estimates of variance. We were therefore careful to present results that do not include covariates other than district fixed effects or rely on clustered standard errors. An additional issue which we also address using the randomization-based tests proposed by Young (2019) is that of joint testing of multiple hypotheses. In Table A-4a, we report estimates based on exact p-values for the sharp null hypothesis of no treatment effect for none of the schools in our sample. More specifically, we report p-values for individual tests of each treatment effect estimate, as well as for joint tests of, respectively, all treatment effects in each equation, all treatment effects in each table, and all treatment effects across both results tables. Differences between exact randomization p-values for individual significance tests and the estimates reported in the main analysis are very small, and the same conclusions (and levels of significance) are obtained. Joint tests confirm the robustness of our findings on schooling outcomes (positive effects on attendance and math score, but not on enrollment) and the absence of treatment effects on the other outcomes studied. In Table A-4b, we test for differences in treatment effects relative to the "information only" arm. Individual tests of differences lead to the same conclusions to those relying on asymptotic tests (Tables 2 and 3) in that the only consistently significant difference across treatment arms is that the CCT leads to smaller gains in test scores. Further confirming this, a joint test of all the differences in treatment effects across all outcomes rejects the null of zero difference in treatment effects when including test scores in the set of outcomes (p-value: 0.053), but does not when test scores are excluded (p-value: 0.191).

In Appendix G, we report in detail on further robustness checks, which are summarized here for ease of reference. We confirm that our conclusions are not driven by differential attrition. There is no attrition for our main outcome of interest (independently verified attendance rate at school) since we have spot checks data for all schools. The rate of attrition in girls taking part in the endline household survey is also not jointly significantly different across experimental groups, although the attrition rate is slightly higher for the control group. Reassuringly, reweighting observations by the inverse of the probability that they attrit does not change our conclusions (Table A-5).

We then present ANCOVA estimates. When the outcome was measured at baseline, we control for the baseline value of the outcome variable. Alternatively, we control for an available proxy of the outcome at baseline when a reasonable proxy exists. Results in Table A-6 show that all our conclusions are robust to the inclusion of these pre-treatment outcomes.

We also repeat the analysis at the individual level (clustering the standard errors at the school level), and thus weighting each school by the size of the school sample. Results—shown in Table A-7—are largely unchanged.

We additionally show that our results are not driven by selection of girls observed in attendance spot checks through school switches (Table A-8), that they are robust to trimming the school sample of the 5% largest and smallest schools (Table A-9), to excluding spot check data where conflict caused substantial disruptions to data collection (Table A-10) and to selecting control variables based on their predictive power as suggested in Altman (1985) and Bruhn and McKenzie (2009) (Table A-11). Ex-post power calculations indicate that the experiment is well-powered for our three schooling outcomes, teacher absenteeism and self-reported monitoring quality, but not for early marriage and self-reported empowerment. This confirms the inconclusiveness of our findings for early marriage and self-reported empowerment (Table A-12).

V- **Mechanisms**

In this section we discuss the mechanisms through which our treatments resulted in increased attendance and explore possible reasons for the smaller effect of the parent incentive treatment on average test scores.

A- Effect of our Treatments on Parental Monitoring

We start by documenting the effect of our treatments on parental information about their child's attendance. For the girls surveyed in their households who were both (i) enrolled in school in 2016 and (ii) could be matched to our independent school attendance records,[38] we can evaluate the quality of the

[38] 77% of girls whose parents said were enrolled in Grade 6 or 7 could be matched to names in official school records.

parental monitoring technology by comparing the number of absences reported by parents in the household survey to our spot checks data. More precisely, we can estimate the predictive power of the number of child absences during October 2016 reported by the parent in the household survey on attendance at the last spot check carried out in schools, which took place between October 10 and November 3, 2016. Table 4 reports estimates from a regression of an indicator for whether the girl was absent at the last independent attendance check on the reported number of days absent during October 2016 and district fixed effects, experimental arm by experimental arm (Columns (1) to (4)).

On the basis of 22 days of school, if the probability of being absent was the same in any given day, then an additional day absent during the month should increase the probability of being absent on the day of the spot check by 1/22=0.045. In the control group, however, the estimated increase is positive but small at only 0.009 and it is statistically insignificant, indicating that the quality of attendance monitoring by parents is low.

In all the treatment arms, the estimated increase in the probability of being absent during the spot check more than doubles and is statistically significant. [39] In particular, simply introducing the attendance report card without financial incentives more than doubles this probability, which reaches 46% of the coefficient expected in the case of perfect monitoring (0.045).

The number of absences reported by the parents may not be exogenous, but much of the heterogeneity which may lead to omitted variable bias is likely to be captured by the number of days absent in October of the previous year reported by parents at baseline. Columns (5) to (8) repeat the same analysis controlling for absences in October 2015 reported by parents at baseline, showing that results are robust. While we cannot rule out that salience played some role, the results reported in Table 4 show that the attendance report cards did substantially improve parental information about the child's attendance.

B- Alternative Explanations

[39] By showing that parental reports of missed school days are improved in all treatment arms, Table 4 also addresses the potential concern that there might be collusion between teachers and parents (in the parents' incentives arm) or between teachers and girls (in the information only- or child incentives arms) inducing teachers to omit to report absences on report cards. Furthermore, we note that if the report cards were manipulated to the extent of being devoid of information content, we would not observe the increases in independently verified attendance reported in Table 2.

We interpret the effect of the information treatment on attendance as the effect of improved parental monitoring, as supported by the evidence of the previous subsection. And we interpret the effect of the other treatments as the combined effect of improved parental attendance monitoring and material rewards for the parents (children) in the "parent incentive" ("child incentive") arm. In this subsection we investigate whether alternative explanations could explain our findings on attendance.

One concern may have been that child attendance increases in the information treatment not because of improved monitoring of child attendance by parents but because of more parental scrutiny into what goes on at school in general, including teacher absenteeism. Or teachers may have been more motivated (or put off) by higher pupil attendance. Unfortunately, our only measure of teacher engagement is teacher attendance. But reassuringly, in Section III.C we find no statistically significant effect on teacher attendance. This suggests that changes in teacher attendance do not mediate the effects we find on child attendance and test scores, which is consistent with our interpretation of these effects as resulting from a demand-side response. These findings also give us reassurance that the instrument we set up to ensure that teachers were compensated for the time spent filling in the report cards was well-calibrated (at 250 meticais' worth of airtime) since it neither significantly increased- nor decreased teacher attendance.

A candidate explanation for the large effect on attendance of information relative to the conditional transfers arms is that attendance spot checks taking place later in the term may be driving estimates down in the case of conditional transfers, either because the attendance target has already been missed or, on the contrary, the pupil can "afford" to miss a day after having had perfect or near-perfect attendance. Two rounds of spot checks were carried out in each trimester, with one "early" and one "late" round in each term (see Figure A-2 for a visual depiction of the timing of spot checks, by treatment arm). We find very similar results when computing school averages using only the early- or late checks data (Table A-13, columns 1 and 2), which suggests that strategic considerations due to the attendance conditionality cut-off are not driving our findings.[40]

[40] A separate timing consideration is that of whether treatment effects vary when the opportunity cost of children's time is higher. Ninety-two percent of small and medium farms in Manica Province grow maize (Ministério da Agricultura e Segurança Alimentar 2015),

Another concern in interpreting the effect of the child incentive- relative to that of the parent incentive treatment is the question of whether the transfers were indeed received by the targeted individuals. If girls were unable to retain the transfers intended for them, or if parents systematically passed on the transfers they received to their daughters, then there would be no practical difference between nominally incentivizing the parent or the child.

Our finding that incentivizing children is at least as effective as incentivizing parents is therefore particularly interesting in the light of evidence that our transfers remained with their intended beneficiaries. First, when asked at endline, no surveyed girl from the child incentive arm responded that she had given away her reward or had had to sell it to give the money to someone else. Second, it could have been the case that parents substituted away from expenditure on private goods consumed by the girl, thus neutralizing the transfer to their daughter. To test for this, we collected detailed information on the girls' consumption of 23 non-food private goods such as clothes, bags, soap, books, etc., excluding any item purchased through a voucher. We find a statistically insignificant effect on consumption of personal items (other than those purchased with the voucher) in the child incentive arm compared to the control group. Unsurprisingly given the substitutability between these goods and those available for purchase through vouchers, the coefficient is negative. But it is insignificant and small relative to the amounts transferred (no more than 89 meticais compared to an average of 469 meticais received in vouchers, see Table A-14).

We cannot rule out that the goods purchased with the vouchers may have made attendance easier and/or more beneficial. While indirect, the available evidence does not, however, point in this direction. First, school uniforms are *de facto* not compulsory in Mozambican primary schools and are only used by a minority of students at baseline. The same applies to backpacks, so that the provision of these items does not simply equate to a subsidy for necessary school expenditure, as confirmed by the weak substitution effect found in Table A-14. Second, if vouchers worked through relieving budget constraints on the

---

which is harvested between late March and end of May (FAO 2021). This allows us to estimate treatment effects separately for attendance spot checks carried out during- and outside harvest time (Table A-13, columns 3 and 4). Treatment effects are remarkably similar across treatment arms at harvest time (ranging between 0.052 and 0.057), which may be due to financial incentives being insufficient to compensate for the child's opportunity cost of time at times of high agricultural labor demand.

purchase of schooling inputs, we would expect poorer girls to benefit more. But in Table A-15, where we test whether the effect of our treatments differed in the poorest school tercile, we find suggestive evidence of the contrary–namely, a statistically insignificant but large *negative* interaction between child incentives and being in the poorest school tercile.

A symmetrical concern is that parents may simply have passed on to their daughters the transfers they received in the parent incentive arm. But only 4.3% of parents receiving cash transfers purchased any goods from the research team. In addition, the (statistically insignificant) effect of the parents' incentives treatment on consumption of personal items by their girls is negative, which strongly suggests that parents did not simply pass on their transfers to the girls.

C- Effects on Learning

We find that the effects on attendance of the information and girls' incentive treatments are roughly similar in magnitude to that of the CCT, but that only the information and girls' incentive treatments have a statistically significant effect on average test scores.

These learning effects should be interpreted with caution since we only have data for one coarse test and the confidence interval for the CCT effect includes non-negligible effects on learning. For completeness, we provide here some discussion of factors that may contribute to the smaller effect of the CCT on mean ASER math scores.

We can largely rule out an argument that school quality is so poor that increased attendance through the CCT does not result in improved learning because we observe significant gains in learning in the two other treatment arms. We can also largely rule out an explanation based on the idea that parents are multitasking agents who, when incentivized for attendance, improve attendance at the expense of their other educational tasks. Indeed, the CCT does not produce substantially larger effects on attendance than the information arm. For an increased effort of parents to generate attendance gains at the cost of learning in the CCT arm specifically, it would therefore have to be the case that the marginal return of parental effort on attendance is small beyond that induced by improved information, and that parents are willing to incur sizeable learning losses to achieve these marginal attendance gains, which is possible but seems unlikely. A further candidate explanation would be that material incentives crowd out intrinsic motivation. But we incentivize attendance, not test scores, so it is not clear why crowding out should occur mostly for the outcome we do not incentivize rather than the outcome we do incentivize.

Our CCT treatment may induce children who differ in observable and/or unobservable characteristics to attend school more, and these compliers may also have different returns to attendance in terms of learning as captured by the math ASER test. We do find suggestive evidence of heterogeneity in a direction consistent with expectations (Table A-15). In particular, the point estimate for the effect of the CCT treatment on school attendance is about twice the size in schools in the poorest tercile (Column 1), while the point estimate of the effect of the child incentive treatment on attendance is 70% larger in schools in the top tercile for school share of older girls (Column 3). Similarly, the effect of the information treatment on attendance is two-thirds larger in the top tercile for school share of girls with long journeys to school (Column 5).

We also estimated the effect of our treatments on the ASER score for each of 63 subgroups defined by baseline individual and household characteristics.[41] The parent incentive treatment does not have a statistically significant effect on average ASER scores in any of these subgroups (whereas significant effects are obtained in 35 (47) subgroups for the "information only" ("child incentive") treatment). The parent incentive does, however, significantly increase the probability of obtaining the top score in 30 out of 63 of these subgroups (compared to 45 and 44, respectively, for the two other treatment arms). Figure A-1 confirms graphically that the effects of the three treatments on test scores are more similar at the top of the distribution. While the information only- and child incentive treatments tend to shift the whole distribution to the right, the parent incentive treatment shifts the distribution towards both tails (significantly so in the case of the upper tail, as shown in Table A-16).

All in all, our analysis suggests that heterogeneity in compliers across treatment groups and more concentrated effects on learning at the top of the distribution in the parent incentive arm may contribute to differences in treatment effects on average performance at the ASER math test.

[41] More specifically, we interact the treatment indicators, in turn, with each of the binary variables for which we test balance at baseline. For non-binary variables (individual consumption, age at baseline, absences reported by parents), we interact the treatment with each of the variables in turn and test for treatment effects at the mean, 25% percentile and 75% percentiles.

VI- **Conclusion**

We find evidence that providing high-frequency information to parents about their daughter's school attendance increases school attendance in a low-income Sub-Saharan Africa setting even in the absence of transfers, and that this effect is not statistically distinguishable from that of conditional transfers to parents providing the same information. Incentivizing girls is at least as effective as incentivizing parents.

We also find evidence supporting the interpretation of the information treatment as improving the parental monitoring technology. While in control schools, parental self-reported knowledge of their daughter's school absences has no predictive power on the probability that their daughter was absent at a random attendance check, in treatment schools the coefficient associated with parent-reported absences is significant and more than doubles.

These findings have important policy implications and raise the possibility of increasing attendance at low cost. The cost of increasing attendance by one percent is roughly eight times (twice) lower in the information (child incentive) arm than in the CCT arm, although a full welfare analysis is beyond the scope of our study.

More generally, our results give support to the hypothesis that children have agency in decisions concerning their education. Taken together with recent work by Bergman (2021), Bergman and Chan (2021), Berlinski et al. (2021), Bursztyn and Coffman (2012), and Rogers and Feller (2016) from middle- to high-income country urban study areas, they provide compelling evidence that information asymmetries exist in a varied range of settings and can be leveraged to improve educational outcomes at comparatively low cost. Finding evidence that children's preferences matter in schooling decisions is also particularly good news considering recent work showing that non-cognitive traits relevant to schooling decisions, such as patience, can be altered through targeted interventions during childhood (Alan and Ertac, 2018).

References

Akresh, Richard, Damien de Walque, and Harounan Kazianga. 2016. “Evidence from a Randomized Evaluation of the Household Welfare Impacts of Conditional and Unconditional Cash Transfers Given to Mothers or Fathers.” World Bank Policy Research Working Paper 7730.

Alan, Sule., and Seda Ertac. 2018. “Fostering patience in the classroom: Results from a randomized educational intervention.” *Journal of Political Economy* 126(5), 1865-1911.

Alderman, Harold, Jere Behrman, and Afia Tasneem. 2019. “The Contribution of Increased Equity to the Estimated Social Benefits from a Transfer Program: An Illustration from PROGRESA/Oportunidades.” *The World Bank Economic Review* 33(3): 535-50.

Altman, Douglas G. 1985. “Comparability of Randomized Groups.” *Statistician* 34: 125–36.

Athey, Susan, and Guido W. Imbens. 2017. The Econometrics of Randomized Experiments. Chapter 3 in *Handbook of Economic Field Experiments* (Vol. 1, pp. 73-140), edited by Abhijit Banerjee and Esther Duflo. North-Holland.

Aucejo, Esteban M., and Teresa Foy Romano. 2016. Assessing the Effect of School Days and Absences on Test Score Performance. *Economics of Education Review* 55: 70-87.

Baird, Sarah, Francisco H. Ferreira, Berk Özler, and Michael Woolcock. 2014. Conditional, unconditional and everything in between: a systematic review of the effects of cash transfer programmes on schooling outcomes. *Journal of Development Effectiveness* 6(1), 1-43.

Baird, Sarah, Craig McIntosh, and Berk Özler. 2011. “Cash or Condition? Evidence from a cash transfer experiment.” *The Quarterly Journal of Economics*, 126(4), 1709-1753.

Bassi, Marina, Octavio Medina, and Lúcia Nhampossa. 2020. “Education Service Delivery in Mozambique: A Second Round of the Service Delivery Indicators Survey”. Unpublished report.

Barrera-Osorio, Felipe, Marianne Bertrand, Leigh L. Linden, and Francisco Perez-Calle. 2011. “Improving the design of conditional transfer programs: Evidence from a randomized education experiment in Colombia.” *American Economic Journal: Applied Economics* 3(2), 167-95.

Becker, Gary S. (1974). A theory of social interactions. *Journal of Political Economy*, 82(6), 1063-1093.

Bedi, Arjun, and Jeffery H. Marshall. 1999. “School attendance and student achievement: Evidence from rural Honduras.” *Economic Development and Cultural Change* 47(3), 657-682.

Benhassine, Najy, Florencia Devoto, Esther Duflo, Pascaline Dupas, and Victor Pouliquen. 2015. “Turning a Shove into a Nudge? A" Labeled Cash Transfer" for Education.” *American Economic Journal: Economic Policy 7*(3), 86-125.

Bergman, Peter. 2021. “Parent-Child Information Frictions and Human Capital Investment: Evidence from a field experiment investment.” *Journal of Political Economy* 129(1): 286-322.

Bergman, Peter and Eric W. Chan. 2021. “Leveraging Parents Through Technology: The impact of high-frequency information on student achievement.” *Journal of Human Resources* 561(1): 125-158.

Bergstrom, Theodore C. 1989. “A Fresh Look at The Rotten Kid Theorem and Other Household Mysteries.” *Journal of Political Economy 97*(5), 1138-1159.

Berlinski, Samuel, Matias Busso, Taryn Dinkelman, and Claudia. Martinez A. 2021. “Reducing Parent-School Information Gaps and Improving School Outcomes: Evidence from high frequency text messaging in Chile.” NBER Working Paper 28581.

Berry, James 2015. “Child Control in Education Decisions: An Evaluation of Targeted Incentives to Learn in India.” *Journal of Human Resources 50*(4), 1051-1080.

Bruhn, Miriam and David McKenzie. 2009. “In Pursuit of Balance: Randomization in Practice in Development Field Experiments.” *American Economic Journal: Applied Economics 1*(4), 200-232.

Bursztyn, Leonardo, and Lucas Coffman. 2012. “The Schooling Decision: Family preferences, intergenerational conflict, and moral hazard in the Brazilian favelas.” *Journal of Political Economy* 120(3), 359-397.

Cunha, Jesse. 2014. “Testing Paternalism: Cash vs. In-kind Transfers.” *American Economic Journal: Applied Economics*, 6(2), 195–230.

Cunha, Nina, Guilherme Lichand, Ricardo Madeira, and Eric Bettinger. 2017. What Is It About Communicating with Parents? October 2017 Unpublished manuscript.

de Walque, Damien and Christine Valente. 2016. "Preventing Excess Female School Drop Out in Mozambique: Conditional Transfers and the Respective

Role of Parent and Child in Schooling Decisions." AEA RCT Registry. February 29. https://doi.org/10.1257/rct.1069-1.0

de Walque, Damien and Valente, Christine. 2022. "Data for: Incentivizing School Attendance in the Presence of Parent-Child Information Frictions." American Economic Association [publisher], Inter-university Consortium for Political and Social Research [distributor]. http://doi.org/10.3886/E154261V1.

Doepke, Mattias, Giuseppe Sorrenti, and Fabrizio Zilibotti. 2019. "The Economics of Parenting" *Annual Review of Economics* 11: 55-84.

Fiszbein, Ariel, and Norbert R. Schady. 2009. *Conditional cash transfers: reducing present and future poverty*. World Bank Publications.

Food and Agriculture Organization (FAO). 2021. Global Information and Early Warning System Country Brief on Mozambique (8 June 2021). Food and Agricultural Organization, United Nations.

Galiani, Sebastian, Matthew Staiger, and Gustavo Torrens. 2017. "When Children Rule: Parenting in Modern Families." NBER Working Paper No. 23087.

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

Glewwe, Paul, Eric A. Hanushek, Eric, Sarah D. Humpage, and Renato Ravina R. 2013. School Resources and Educational Outcomes in Developing Countries: A Review of the Literature from 1990 to 2010. In Glewwe Paul. (ed), Education Policy in Developing Countries. The University of Chicago Press, Chicago.

Glewwe, Paul, and Khartik Muralidharan. 2016. Improving education outcomes in developing countries: Evidence, knowledge gaps, and policy implications. Chapter 10 in Handbook of the Economics of Education, edited by Stephen Machin, Ludger Woessmann and Eric A. Hanushek (Vol. 5, pp. 653-743). Elsevier.

Haushofer, Johannes, and Jeremy Shapiro. 2016. "The Short-Term Impact of Unconditional Cash Transfers to the Poor: Evidence from Kenya" *Quarterly Journal of Economics* 131(4): 1973-2042.

Hirshleifer, Sarojini 2017. "Incentives for Effort or Outputs? A Field Experiment to Improve Student Performance". Unpublished manuscript.

Instituto Nacional de Estatística (INE) 2015. Estatísticas e Indicadores Sociais 2013-2014. Instituto Nacional de Estatística, Maputo, Mozambique.

Kremer, Michael. and Alaka Holla, A. 2009. "Improving Education in the Developing World: What Have We Learned from Randomized Evaluations?" *Annual Review of Economics* 1: 513-542.

Levitt, Steven, John List, and Sally Sadoff, 2016. "The Effect of Performance-Based Incentives on Educational Achievement: Evidence From a Randomized Experiment". National Bureau of Economic Research Paper No. 22107.

McKenzie, David. 2012. "Beyond Baseline and Follow-Up: The Case for More T in Experiments." *Journal of Development Economics* 99: 210-221.

Manacorda, Marco. 2012. "The Costs of Grade Retention." *The Review of Economics and Statistics* 94(2): 596-606.

Martinelli, Cesar, and SusanParker. 2003. "Should Transfers to Poor Families be Conditional on School Attendance? A Household Bargaining Perspective." *International Economic Review*, 44(2), 523-544.

Millán, Teresa M., Tania Barham, Karen Macours,, John A. Maluccio, , and Marco Stampini. 2019. "Long-Term Impacts of Conditional Cash Transfers: Review of the Evidence." *The World Bank Research Observer*, 34(1), 119-159.

Ministério da Agricultura e Segurança Alimentar. 2015. *Anuário de Estatísticas Agrárias 2015*. Maputo, Mozambique.

Ministério da Educação e Desenvolvimento Humano (MINEDH). 2015. *Programas do Ensino Primário (3º Ciclo (6ª e 7ª classes)).* Instituto Nacional de Desenvolvimento da Educação, Ministério da Educação e Desenvolvimento Humano, Maputo.

Ministério da Saúde (MISAU), Instituto Nacional de Estatística (INE), e ICF 2015. *Inquérito de Indicadores de Imunização, Malária e HIV/SIDA em Moçambique 2015*. Maputo, Mozambique. Rockville, Maryland, EUA: INS, INE, e ICF.

Ministry of Planning and Development - National Directory of Studies and Policy Analysis (MPD-DNEAP). 2016. *Poverty and Wellbeing in Mozambique: Fourth National Poverty Assessment*. Ministry of Planning and Development - National Directory of Studies and Policy Analysis-, Maputo.

National Statistics Institute 2009. *Final Report of the Multiple Indicator Cluster Survey, 2008*. Directorate of Demographic, Life and Social Statistics, Maputo.

Parker, Susan W. and Petra E. Todd, P. 2017. "Conditional Cash Transfers: The case of Progresa/Oportunidades." *Journal of Economic Literature*, 55, 866-915.

Robinson, Carly D., Monica G. Lee, Eric Dearing, and ToddRogers. 2018. “Reducing Student Absenteeism in the Early Grades by Targeting Parental Beliefs.” *American Educational Research Journal*, *55*(6), 1163-1192.

Rogers, Todd and Avi Feller. 2018. “Reducing Student Absences at Scale by Targeting Parents' Misbeliefs.” *Nature Human Behaviour* 2: 335-342.

Skoufias, Emmanuel., Mishel Unar, and Teresa Gonzalez-Cossio. 2008. “The Impacts of Cash and In-Kind Transfers on Consumption and Labor Supply: Experimental Evidence From Rural Mexico.” World Bank Policy Research Working Paper 4778.

Snilstveit, Birte, Jennifer Stevenson, Daniel Phillips, Martina Vojtkova, Emma Gallagher, Tanja Schmidt, Hannah Jobse, Maisie Geelen,, Maria Grazia Pastorello, and John Eyers. 2015. *Interventions for improving learning outcomes and access to education in low-and middle-income countries: a systematic review*. London: International Initiative for Impact Evaluation.

UNESCO Institute for Statistics. 2021. Online Database. *http://uis.unesco.org/en/country/mz?theme=education-and-literacy*.

Vagh, Shaber Banu. 2009. *Evaluating the reliability and validity of the ASER testing tools.* ASER Centre. *Available at http://linguaakshara.org/yahoo_site_admin/assets/docs/ASER-Reliability__Validity_Evaluation.11091338.pdf*.

World Bank. 2017. World Development Indicators 2017. Washington, DC. https://openknowledge.worldbank.org/handle/10986/26447 License: CC BY 3.0 IGO.

World Bank Education Statistics Data Bank.2017. Online Database. URL: http://databank.worldbank.org/data/reports.aspx?source=Education-Statistics-~-All-Indicators.

Young, Alwyn. 2019. “Channeling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results.” *The Quarterly Journal of Economics* 134(2), 557-598.

Tables

Table 1: Experimental Arms Overview, Sample Sizes, and Attrition

	Control	Child Incentive	Parent Cash	Information	Total
Panel A: Experimental Arms Overview					
Weekly attendance report cards?	No	Yes	Yes	Yes	
Transfers conditional on a 90% attendance target over the trimester?	No	Yes	Yes	No	
Nominal value of transfers (meticais per trimester)	N/A	400 (in vouchers)	400 (in cash)	N/A	
Recipient of transfers	N/A	daughters	parents	N/A	
Panel B: Sample Sizes and Attrition					
# Schools	44	44	44	41	173
# Times attendance verified in each school (mean)	5.52	5.45	5.64	5.63	5.56
# Girls Surveyed at Baseline	766	738	751	695	2950
# Girls Surveyed at Endline	711	699	715	668	2793
Attrition rate (Girls in Household Survey)	.072	.053	.048	.039	.053

Source: de Walque and Valente (2022).

Table 2: Effect on Schooling Outcomes

	(1) Share present at attendance check	(2) Share self-reported enrollment	(3) Average ASER Math score	(4) Share present at attendance check	(5) Share self-reported enrollment	(6) Average ASER Math score
Information	0.0450	0.00662	0.183	0.0488	0.00483	0.195
	(0.0225)	(0.0150)	(0.0911)	(0.0239)	(0.0159)	(0.0909)
Parent Cash	0.0599	0.0272	0.0202	0.0588	0.0196	-0.00233
	(0.0222)	(0.0148)	(0.0898)	(0.0236)	(0.0156)	(0.0897)
Child Incentive	0.0829	-0.00331	0.203	0.0841	-0.00731	0.178
	(0.0222)	(0.0148)	(0.0898)	(0.0238)	(0.0158)	(0.0904)
Baseline Characteristics	No	No	No	Yes	Yes	Yes
Observations	173	173	173	173	173	173
Mean Y (control)	0.65	0.95	2.16	0.65	0.95	2.16
SD Y (control)	0.128	0.0870	0.567	0.128	0.0870	0.567
p info=parents	0.512	0.174	0.077	0.680	0.361	0.034
p info=girls	0.097	0.511	0.828	0.145	0.447	0.856
p girls=parents	0.300	0.039	0.042	0.284	0.086	0.044

Source: de Walque and Valente (2022). Outcome variables for Columns (1) and (4): unannounced spot checks attendance data. Any pupil listed on the class roll and not present in the class at the time of the attendance check is coded as absent when computing the overall share of girls present during the spot checks. All other data: household survey (endline for outcomes, and baseline for controls). Baseline characteristics are the school sample averages for the following variables: self-reported (by parents) number of missed school days in October 2015 among girls enrolled, binary indicator for high self-reported monitoring quality, five language indicators and five religion indicators. All regressions include a constant and district fixed effects. Standard errors in parentheses. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table 3: Effect on Non-Schooling Outcomes

	(1) Class teacher presence rate	(2) Share ever married	(3) Share high self-reported monitoring quality	(4) Share high self-reported empowerment	(5) Class teacher presence rate	(6) Share ever married	(7) Share high self-reported monitoring quality	(8) Share high self-reported empowerment
Information	0.0313	-0.0174	0.00937	-0.0209	0.0430	-0.0127	-0.00111	-0.0218
	(0.0258)	(0.0100)	(0.0259)	(0.0383)	(0.0276)	(0.0107)	(0.0264)	(0.0396)
Parent Cash	0.0249	-0.00956	0.0319	0.00203	0.0222	-0.00958	0.0312	-0.00227
	(0.0254)	(0.00987)	(0.0255)	(0.0377)	(0.0272)	(0.0105)	(0.0261)	(0.0391)
Child Incentive	0.00750	-0.00401	0.00664	-0.0356	0.0171	-0.000814	-0.00405	-0.0342
	(0.0254)	(0.00987)	(0.0255)	(0.0377)	(0.0274)	(0.0106)	(0.0263)	(0.0394)
Baseline Char.	No	No	No	No	Yes	Yes	Yes	Yes
Observations	173	173	173	173	173	173	173	173
Mean Y (Control)	0.90	0.03	0.89	0.30	0.90	0.03	0.89	0.30
SD Y(Control)	0.154	0.048	0.167	0.228	0.154	0.048	0.167	0.228
p info=parents	0.805	0.440	0.387	0.553	0.458	0.776	0.230	0.628
p info=girls	0.360	0.187	0.916	0.703	0.352	0.271	0.912	0.756
p girls=parents	0.492	0.572	0.320	0.318	0.852	0.403	0.175	0.412

Source: de Walque and Valente (2022), Unannounced spot checks attendance data (for outcome variable in Columns 1 and 5) and household survey (all other variables). The class teacher presence rate is the rate of presence of the class teacher over all the unannounced spot checks. Self-reported monitoring quality index components: binary indicators for parent responding “completely agree” or “agree” to questions about whether “at the end of each day, [they] know/knew whether their daughter has (had) gone to school”, whether “at the end of each day, [they] know/knew whether their daughter has (had) been in her classroom”, and whether it has “ever happened one day that [they] thought that their daughter was at school but then [they] found out that she had not”. High empowerment index components: binary indicators for whether the girl decides (individually or jointly) about: healthcare for herself, her visiting relatives, her going to school, her working outside the house, and a binary indicator for whether she would be able to keep for herself some clothes given to her in reward for her work. Both indexes are obtained by Principal Component Analysis carried out at the individual level, then used to create a binary indicator at the individual level for above-median score. The explained variable in Columns (3), (4), (7) and (8) is the proportion with above-median score at the school level. Baseline characteristics are the school sample averages for the following variables: self-reported (by parents) number of missed school days in October 2015 among girls enrolled, binary indicator for high self-reported monitoring quality, five language indicators and five religion indicators. All regressions include a constant and district fixed effects. Standard errors in parentheses. “p arm_i=arm_j” denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

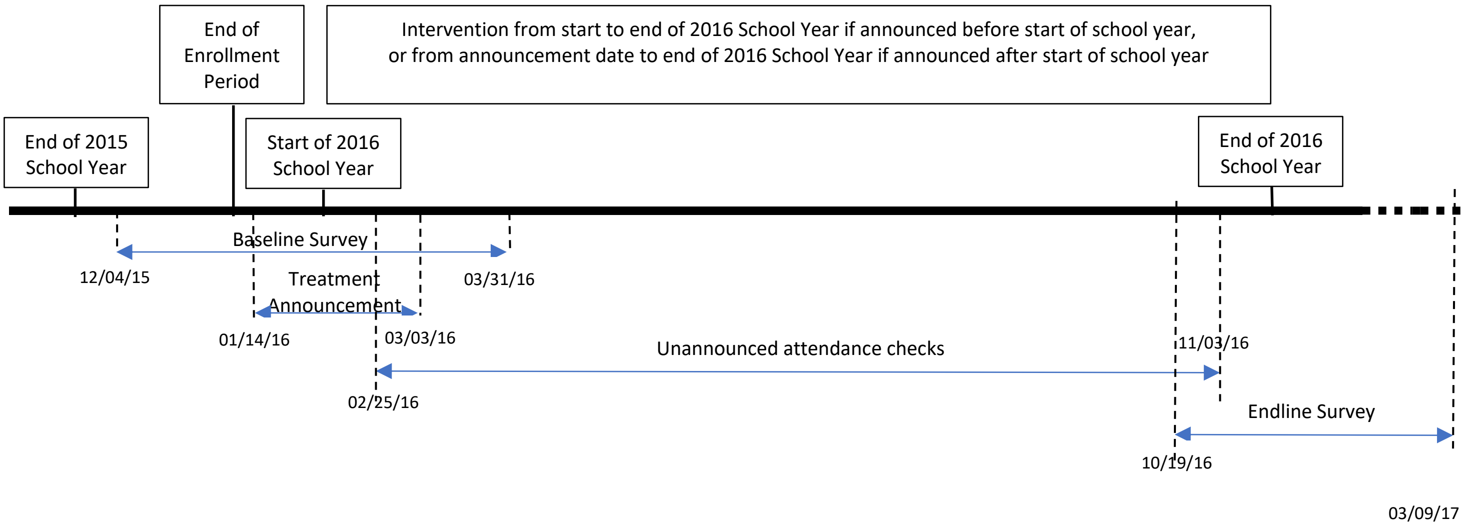
Table 4: Quality of Monitoring across Treatment Arms

	Outcome: absent at attendance check between 10 October and 3rd November 2016							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Experimental arm:	Control	Info	Girls	Parents	Control	Info	Girls	Parents
Parent-reported missed school days in October 2016	0.00868	0.0207	0.0227	0.0325	0.00839	0.0200	0.0227	0.0317
	(0.00732)	(0.00575)	(0.00626)	(0.00444)	(0.00707)	(0.00590)	(0.00611)	(0.00439)
Parent-reported missed school days in October 2015					0.0158	0.00114	-0.000743	0.0187
					(0.00987)	(0.00977)	(0.0139)	(0.00975)
Observations	473	406	427	481	458	391	415	468

Source: de Walque and Valente (2022). Household survey (number of child absences reported by the parent) and independent attendance spot checks (outcome variable). Any pupil listed on the class roll and not present in the class at the time of the attendance check is coded as absent. Sample sizes are slightly smaller in columns (5) to (8) due to some girls not being enrolled in 2015. All regressions include a constant and district fixed effects. Standard errors clustered at the school level in parentheses.

Figure

Figure 1: Timeline of Intervention and Data Collection



Online Appendix

Incentivizing School Attendance in the Presence of Parent-Child Information Frictions

Damien de Walque and Christine Valente

A. Appendix Tables

Table A-1: Sample Comparison with Representative Samples

		AIS 2015	
	Study Sample	Manica	Mozambique
Female-headed Household	0.20	0.43	0.40
Education of Head:			
No Education	0.15	0.28	0.24
Primary Education	0.56	0.46	0.54
Secondary Education or Above	0.29	0.26	0.22
Ownership of Household Assets:			
Bicycle	0.49	0.36	0.31
Motorbike	0.18	0.08	0.08
Radio	0.66	0.43	0.47
TV	0.34	0.32	0.34
Cell phone	0.80	0.59	0.64
Car	0.05	0.03	0.06
Type of Toilet Facility:			
None	0.09	0.32	0.24
Unimproved Latrine	0.56	0.43	0.37
Improved Latrine or Toilet	0.35	0.25	0.40
Number of Observations	2950	619	7169

Source: baseline household survey (de Walque and Valente, 2022) and MISAU, INE and ICF (2016). The number of observations refers to the size of the largest sample for which the variables are non-missing.

Table A-2: Descriptive Statistics and Balance at Baseline

	(1) Control Mean	(2) Information Mean	(3) Parent Cash Mean	(4) Child Incentive Mean
<u>Household Head:</u>				
Female	0.19	0.19	0.19	0.17
No Education	0.18	0.15	0.13	0.14
Primary Education	0.57	0.57	0.61	0.58
Secondary or Higher Education	0.26	0.28	0.25	0.27
Agriculture	0.53	0.48	0.55	0.50
White Collar	0.14	0.13	0.13	0.11
Other Occupation	0.33	0.39	0.31	0.39
<u>Household wealth[1]:</u>				
Lowest Tercile	0.42	0.36	0.37	0.37
Middle Tercile	0.32	0.34	0.30	0.35
Highest Tercile	0.26	0.30	0.33	0.28
<u>Language:</u>				
Portuguese	0.10	0.07	0.10	0.09
Ndau	0.21	0.21	0.26	0.28
Shona	0.11	0.13	0.13	0.14
Chiute	0.28	0.21	0.24*	0.20**
Chibarue	0.12	0.14	0.12	0.13
Other Language	0.18	0.24**	0.14	0.16
<u>Religion:</u>				
Catholic	0.12	0.07	0.11	0.12
Protestant	0.20	0.22	0.19	0.25*
Christian	0.16	0.21*	0.15	0.18
Zioni	0.20	0.21	0.28*	0.17
Atheist	0.15	0.12	0.10	0.14*
Other Religion	0.18	0.17	0.17	0.13
<u>Girl Characteristics:</u>				
Age	12.70	12.61	12.55	12.73
Consumption of Personal Goods[2]	967.08	887.45	998.58	937.30
High Empowerment[3]	0.40	0.42	0.34	0.42
Enrolled in 2015	0.97	0.98	0.98	0.96
Ever Married	0.02	0.01	0.02	0.02

Monitoring:				
Parent-Reported Absences[4]	1.12	0.93	0.76**	0.66***
High Monitoring Quality[5]	0.86	0.88	0.90*	0.88
Thinks a Weekly Attendance Report Card Would be Useful	0.84	0.82	0.81	0.80
N (Schools)	44	41	44	44

Source: baseline household survey (de Walque and Valente, 2022). *, ** and *** denote p-values significant at 10, 5 and 1% respectively obtained by estimating Equation (4). [1]Based on a principal component analysis score using information on ownership of household items and housing characteristics. [2] Value, in meticais, of non-food items purchased by any household member over the 12 months preceding the baseline survey and personally consumed by girls who, if they were to enroll in 2016, would enroll in Grades 6 or 7. [3]Share of girls with an above-median predicted score based on a principal component analysis of answers to questions about whether the girl would be able to keep some item of clothing given to her in exchange of work done, and whether she is involved in decisions concerning her healthcare, visiting relatives, attending school, and working outside the house. [4] Number of days absent from school during October 2015, if enrolled, as reported by the parent/guardian. [5]Share of girls with an above-median predicted score based on a principal component analysis of parent/guardian answers to three questions: whether they fully/partly agree that, at the end of each day, they know whether their daughter/ward was (i) at school, (ii) in the classroom; and whether it has ever happened that one day, they thought the girl was at school but actually she was not.

Table A-3: Effect on Additional Self-Reported Schooling Outcomes

Panel A: Girls' Own Outcomes				
	(1)	(2)	(3)	(4)
	Years of education	Primary education completed	Years of education	Primary education completed
Information	-0.0625	-0.0550	-0.0338	-0.0342
	(0.0589)	(0.0464)	(0.0615)	(0.0480)
Parent Cash	0.0474	0.00831	0.0425	0.000716
	(0.0580)	(0.0457)	(0.0607)	(0.0474)
Girl Voucher	0.0284	-0.00744	0.0610	0.0155
	(0.0580)	(0.0457)	(0.0611)	(0.0477)
Observations	173	173	173	173
Mean Y	6.32	0.44	6.32	0.44
SD Y	0.301	0.224	0.301	0.224
p info=parents	0.065	0.177	0.224	0.475
p info=girls	0.127	0.310	0.128	0.305
p girls=parents	0.742	0.729	0.759	0.754
Panel B: Girls' Siblings Self-Reported Enrollment				
	(1)	(2)	(3)	(4)
	Sisters	Brothers	Sisters	Brothers
Information	0.112	0.0994	0.0933	0.0767
	(0.0473)	(0.0387)	(0.0501)	(0.0390)
Parent Cash	0.0778	0.0555	0.0882	0.0272
	(0.0466)	(0.0381)	(0.0497)	(0.0387)
Girl Voucher	0.0932	0.0377	0.104	0.0202
	(0.0466)	(0.0381)	(0.0498)	(0.0387)
Observations	171	172	171	172
Mean Y	0.65	0.71	0.65	0.71
SD Y	0.211	0.197	0.211	0.197
p info=parents	0.470	0.258	0.920	0.211
p info=girls	0.693	0.113	0.828	0.150
p girls=parents	0.737	0.637	0.744	0.855
Baseline Characteristics	No	No	Yes	Yes

Source: de Walque and Valente (2022) household survey (endline for outcomes, and baseline for controls). Panel A: Dependent variables are highest completed grade in columns (1) and (3) and an indicator for completing primary education defined as having completed at least 7 grades in columns (2) and (4). Panel B: Dependent variables are enrollment of siblings of the eligible girls (sisters in columns 1 and 3; brothers in columns 2 and 4) as reported by parents. Baseline characteristics are the school sample averages for the following variables: self-reported (by parents) number of missed school days in October 2015 among girls enrolled, binary indicator for high self-reported monitoring quality, five language indicators and five religion indicators. All regressions include a constant and district fixed effects. Standard errors in parentheses. “p arm_i=arm_j” denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j. Standard errors in parentheses.

Table A-4a: Individual and Joint Tests of Treatment Effects Based on Randomization Inference

Table	Baseline Char.?	Outcome	Randomization p-values					
			(1)	(2)	(3)	(4)	(5)	(6)
			Info	Parents	Girls	Joint (equation)	Joint (table)	Joint (all 3*14=42 treatment effects)
Table 2	No	Share present at spot check	0.055	0.01	0.000	0.003		
	No	Share self-reported enrollment	0.659	0.069	0.827	0.165		
	No	Average ASER score	0.051	0.826	0.028	0.05		
	Yes	Share present at spot check	0.05	0.017	0.000	0.005		
	Yes	Share self-reported enrollment	0.762	0.204	0.643	0.356		
	Yes	Average ASER score	0.038	0.977	0.051	0.044	0.034	
Table 3	No	Class teacher presence rate	0.219	0.321	0.748	0.592		
	No	Share ever married	0.083	0.328	0.688	0.358		
	No	Share high self-reported monitoring quality	0.736	0.208	0.788	0.633		
	No	Share high self-reported empowerment	0.623	0.959	0.356	0.734		
	Yes	Class teacher presence rate	0.114	0.413	0.529	0.49		
	Yes	Share ever married	0.238	0.361	0.942	0.556		
	Yes	Share high self-reported monitoring quality	0.969	0.232	0.872	0.501		
	Yes	Share high self-reported empowerment	0.593	0.96	0.385	0.795	0.459	0.072

Source: de Walque and Valente (2022). Authors' calculations using Alwyn Young's randcmd program with 2000 randomization iterations. Randomization-t p-values in columns (1), (2), (3) and (4). Randomization-c p-values in columns (5) and (6). Baseline characteristics are the school sample averages for the following variables: self-reported (by parents) number of missed school days in October 2015 among girls enrolled, binary indicator for high self-reported monitoring quality, five language indicators and five religion indicators.

Table A-4b: Individual and Joint Tests of Treatment Effect Differences Based on Randomization Inference

Table	Baseline Char.?	Outcome	(1) Info =Parents	(2) Info =Girls	(3) Joint (equation)	(4) Joint (table) [no ASER eq.]	(5) Joint (both tables) [no ASER eq.]
Table 2	No	Share present at spot check	0.52	0.089	0.236		
	No	Share self-reported enrollment	0.167	0.499	0.105		
	No	Average ASER score	0.081	0.817	0.088		
	Yes	Share present at spot check	0.689	0.137	0.3		
	Yes	Share self-reported enrollment	0.364	0.452	0.216	0.103	
	Yes	Average ASER score	0.034	0.852	0.058	[0.215]	
Table 3	No	Class teacher presence rate	0.809	0.375	0.653		
	No	Share ever married	0.455	0.201	0.442		
	No	Share high self-reported monitoring quality	0.397	0.92	0.562		
	No	Share high self-reported empowerment	0.553	0.697	0.598		
	Yes	Class teacher presence rate	0.481	0.37	0.64		
	Yes	Share ever married	0.79	0.289	0.533		
	Yes	Share high self-reported monitoring quality	0.221	0.923	0.319		
	Yes	Share high self-reported empowerment	0.632	0.759	0.708	0.236	0.053 [0.191]

Source: de Walque and Valente (2022). Authors' calculations using Alwyn Young's randcmd program with 2000 randomization iterations. Randomization-t p-values in columns (1), (2), and (3). Randomization-c p-values in columns (4) and (5). Baseline characteristics are the school sample averages for the following variables: self-reported (by parents) number of missed school days in October 2015 among girls enrolled, binary indicator for high self-reported monitoring quality, five language indicators and five religion indicators.

Table A-5: Inverse Probability Weighting Attrition Correction

	(1)	(2)	(3)	(4)	(5)
	Average ASER math score	Share self-reported enrollment	Share ever married	Share high self-reported monitoring quality	Share high self-reported empowerment
Panel A: No controls for baseline characteristics					
Information	0.171	0.0000403	-0.00792	-0.0146	-0.0273
	(0.0958)	(0.0161)	(0.0105)	(0.0291)	(0.0418)
Parent Cash	0.0306	0.0319	-0.00221	0.0270	-0.00339
	(0.0943)	(0.0158)	(0.0104)	(0.0287)	(0.0412)
Child Incentive	0.191	-0.00797	0.0115	-0.00970	-0.0430
	(0.0943)	(0.0158)	(0.0104)	(0.0287)	(0.0412)
Panel B: Controlling for baseline characteristics					
Information	0.183	0.000310	-0.00424	-0.0297	-0.0276
	(0.0980)	(0.0171)	(0.0112)	(0.0304)	(0.0441)
Parent Cash	0.0182	0.0256	0.00179	0.0235	-0.00567
	(0.0963)	(0.0169)	(0.0110)	(0.0299)	(0.0432)
Child Incentive	0.168	-0.00792	0.0156	-0.0238	-0.0411
	(0.0955)	(0.0168)	(0.0110)	(0.0297)	(0.0430)
Panel C: Attrition					
Attrition rate in control group	.13	.072	.072	.072	.16
P-value of differences between arms	.488	.153	.153	.153	.512
Observations	173	173	173	173	173

Source: Household survey (de Walque and Valente, 2022). School averages and shares obtained after weighting each observation by the inverse of its predicted probability of being observed at endline as a function of all baseline characteristics listed in Table A-2. Regressions in Panel B also include school sample averages for the following baseline characteristics: self-reported (by parents) number of missed school days in October 2015 among girls enrolled, binary indicator for high self-reported monitoring quality, five language indicators and five religion indicators. All regressions include a constant and district fixed effects. The attrition rate varies across dependent variables due to non-response at the math test and empowerment questions. The p-values reported in the last row correspond to an F-test of joint significance of the treatment variables in a regression of the school-level attrition rate on the three treatment indicators and district fixed effects. Standard errors in parentheses.

Table A-6: ANCOVA Estimates

	(1) Share present at spot check	(2) Share self-reported enrollment	(3) Share ever married	(4) Share high self-reported monitoring quality	(5) Share high self-reported empowerment
Information	0.0431	0.00204	-0.00623	0.0121	-0.0198
	(0.0226)	(0.0144)	(0.00419)	(0.0260)	(0.0384)
Parent Cash	0.0559	0.0231	-0.000547	0.0357	-0.000994
	(0.0225)	(0.0142)	(0.00412)	(0.0258)	(0.0380)
Child Incentive	0.0778	-0.00160	-0.000183	0.00860	-0.0341
	(0.0227)	(0.0141)	(0.00410)	(0.0255)	(0.0379)
Parent-reported missed school days at baseline	-0.0101				
	(0.00994)				
Baseline outcome		0.420			
		(0.105)			
Baseline outcome			1.073		
			(0.0389)		
Baseline outcome				-0.0848	
				(0.0869)	
Baseline outcome					-0.0552
					(0.0769)
Observations	173	173	173	173	173
p info=parents	0.576	0.145	0.177	0.365	0.630
p info=girls	0.131	0.802	0.151	0.893	0.712
p girls=parents	0.321	0.082	0.929	0.287	0.386

Source: de Walque and Valente (2022). Household survey, except for the outcome variable in the first column, which comes from the attendance spot checks data. Any pupil listed on the class roll and not present in the class at the time of the attendance check is coded as absent. Parent-reported missed school days at baseline is the school average number of days parents said their daughter was absent from school during October 2015 (if enrolled in 2015). All regressions include a constant and district fixed effects. Standard errors in parentheses. “p arm_i=arm_j” denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-7: Individual-Level Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	=1 if present at spot check	=1 if self-reported enrollment	ASER score	=1 if Class teacher present	=1 if Ever married	=1 if High self-reported monitoring quality	=1 if High self-reported empowerment
Information	0.0418	0.00611	0.166	0.0210	-0.0169	-0.00664	-0.00861
	(0.0211)	(0.0143)	(0.0806)	(0.0215)	(0.00786)	(0.0196)	(0.0306)
Parent Cash	0.0513	0.0209	0.00358	0.0160	-0.0152	0.0134	-0.000725
	(0.0191)	(0.0122)	(0.0842)	(0.0190)	(0.00787)	(0.0184)	(0.0306)
Child Incentive	0.0597	-0.0110	0.210	0.0133	-0.0128	-0.0178	-0.0352
	(0.0162)	(0.0146)	(0.0730)	(0.0184)	(0.00789)	(0.0207)	(0.0322)
Observations	94746	2793	2600	96501	2793	2793	2520
No. of Clusters	173	173	173	173	173	173	173
Mean Y	0.68	0.95	2.19	0.92	0.03	0.91	0.28
SD Y	0.468	0.216	1.083	0.273	0.169	0.292	0.447
p info=parents	0.666	0.191	0.064	0.817	0.798	0.268	0.776
p info=girls	0.352	0.234	0.556	0.693	0.532	0.587	0.370
p girls=parents	0.619	0.010	0.010	0.873	0.709	0.109	0.247

Source: de Walque and Valente (2022). Outcome variables for Columns (1) and (4): unannounced spot checks attendance data. Any pupil listed on the class roll and not present in the class at the time of the attendance check is coded as absent. All other outcome variables: household survey (endline). The unit of observation in Columns (1) and (4) corresponds to one girl observed during one spot check. The unit of observation in all other columns corresponds to one girl interviewed during the endline household survey. All regressions include a constant and district fixed effects. School-level clustered standard errors in parentheses. “p arm_i=arm_j” denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-8: Effect on Attendance, Sample Restricted to Girls Registered at First Spot Check

	(1) Share present at attendance check	(2) Share present at attendance check
Information	0.0419	0.0455
	(0.0228)	(0.0243)
Parent Cash	0.0604	0.0592
	(0.0225)	(0.0240)
Child Incentive	0.0810	0.0823
	(0.0225)	(0.0242)
Baseline Characteristics	No	Yes
Observations	173	173
Mean Y	0.65	0.65
SD Y	0.128	0.128
p info=parents	0.421	0.581
p info=girls	0.090	0.135
p girls=parents	0.359	0.333

Sources: de Walque and Valente (2022). Dependent variable: attendance spot checks, sample restricted to girls with an exact name match in the class roll used in the first spot check of the year (which took place between 02/25/16 and 03/31/16). Any pupil listed on the class roll and not present in the class at the time of the attendance check is coded as absent. Baseline characteristics: household survey. Baseline characteristics are the school sample averages for the following variables: self-reported (by parents) number of missed school days in October 2015 among girls enrolled, binary indicator for high self-reported monitoring quality, five language indicators and five religion indicators. All regressions include a constant and district fixed effects. Standard errors in parentheses. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-9: Sample Trimmed of the 5% Smallest and 5% Largest School Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share present at spot check	Share self-reported enrollment	Average ASER score	Class teacher presence Rate	Share ever married	Share high self-reported monitoring quality	Share high self-reported empowerment
Information	0.0498	0.00634	0.252	0.0443	-0.0153	0.0332	-0.0437
	(0.0235)	(0.0156)	(0.0925)	(0.0280)	(0.0103)	(0.0273)	(0.0405)
Parent Cash	0.0694	0.0254	0.0377	0.0261	-0.0100	0.0354	-0.0131
	(0.0229)	(0.0152)	(0.0899)	(0.0272)	(0.0100)	(0.0265)	(0.0393)
Child Incentive	0.0860	-0.00812	0.250	0.00798	-0.000850	0.00921	-0.0363
	(0.0227)	(0.0150)	(0.0891)	(0.0270)	(0.00996)	(0.0263)	(0.0390)
Observations	157	157	157	157	157	157	157
Mean Y	0.64	0.95	2.14	0.90	0.02	0.89	0.30
SD Y	0.128	0.084	0.564	0.155	0.045	0.169	0.231
p info=parents	0.420	0.238	0.026	0.527	0.620	0.937	0.463
p info=girls	0.135	0.366	0.984	0.206	0.173	0.391	0.859
p girls=parents	0.476	0.031	0.021	0.512	0.369	0.330	0.561

Source: de Walque and Valente (2022). Outcome variables for Columns (1) and (4): unannounced spot checks attendance data. Any pupil listed on the class roll and not present in the class at the time of the attendance check is coded as absent. All other outcome variables: household survey (endline). School size defined by the number of EP2 girls recorded as enrolled as of the first attendance spot check at the school. All regressions include a constant and district fixed effects. Standard errors in parentheses. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-10: Excluding Data Affected by Conflict

	(1) Share present at spot check	(4) Class teacher presence rate
Information	0.0379	0.0300
	(0.0220)	(0.0256)
Parent Cash	0.0546	0.0332
	(0.0216)	(0.0252)
Child Incentive	0.0718	0.0117
	(0.0216)	(0.0252)
Observations	173	173
Mean Y	0.65	0.91
	0.128	
SD Y		0.161
p info=parents	0.450	0.901
p info=girls	0.127	0.479
p girls=parents	0.425	0.394

Source: Unannounced spot checks attendance data (de Walque and Valente, 2022). Any pupil listed on the class roll and not present in the class at the time of the attendance check is coded as absent. School averages obtained after dropping from the database the three spot check rounds for which attendance data could be collected for less than 70% of the district's schools. All regressions include a constant and district fixed effects. Standard errors in parentheses. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-11: Selecting Covariates Based on Their Predictive Power

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share present at spot check	Share self-reported enrollment	Average ASER score	Class teacher presence rate	Share ever married	Share high self-reported monitoring quality	Share high self-reported empowerment
Information	0.0395	-0.00799	0.202	0.0344	-0.00568	0.0101	-0.0147
	(0.0227)	(0.0143)	(0.0898)	(0.0272)	(0.00430)	(0.0264)	(0.0405)
Parent Cash	0.0647	0.0140	0.0127	0.0186	-0.00263	0.0463	0.00416
	(0.0229)	(0.0144)	(0.0906)	(0.0275)	(0.00434)	(0.0266)	(0.0408)
Child Incentive	0.0790	-0.00989	0.195	0.00302	-0.000157	0.00417	-0.0299
	(0.0227)	(0.0143)	(0.0899)	(0.0273)	(0.00431)	(0.0264)	(0.0406)
Observations	173	173	173	173	173	173	173
Mean Y	0.65	0.95	2.16	0.90	0.03	0.89	0.30
SD Y	0.128	0.087	0.567	0.154	0.048	0.167	0.228
p info=parents	0.285	0.139	0.043	0.576	0.494	0.186	0.653
p info=girls	0.089	0.896	0.940	0.259	0.209	0.826	0.712
p girls=parents	0.535	0.101	0.046	0.573	0.571	0.117	0.408

Source: de Walque and Valente (2022). Outcome variables for Columns (1) and (4): unannounced spot checks attendance data. Any pupil listed on the class roll and not present in the class at the time of the attendance check is coded as absent. All other outcome variables: household survey (endline). All regressions include a constant, district fixed effects, and any baseline variable which has a t-statistic equal to 1.96 or above when regressing the outcome on district fixed effects and the baseline characteristics reported in Table A-2 in the control group. Standard errors in parentheses. “p arm_i=arm_j” denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-12: Ex-Post Power Calculations

Outcome	Mean control group	SD control group	MDE	MDE as % of the Mean
Share present at spot check	0.65	0.128	0.078	12%
Share self-reported enrollment	0.95	0.0870	0.053	6%
Average ASER score	2.16	0.567	0.343	16%
Average teacher presence	0.9	0.153	0.092	10%
Share ever married	0.03	0.0476	0.029	96%
Share reporting high monitoring quality	0.89	0.1677	0.101	11%
Share reporting high empowerment	0.3	0.228	0.138	46%

Power calculations for a probability of type I error of 0.05 and a control and treatment group of 44 schools each (which apply to comparisons between any two of the parent cash, child incentive, and control groups). Calculations applying to comparisons between the information treatment arm (41 schools) and any of the other experimental arms have slightly larger MDEs, but differences only appear at the third decimal and are therefore omitted for conciseness.

Table A-13: Attendance Effects and Timing of Spot Checks

	(1)	(2)	(3)	(4)
	Early in Term	Late in Term	Not Harvest Time	Harvest Time
Information	0.0485	0.0536	0.0481	0.0532
	(0.0271)	(0.0227)	(0.0256)	(0.0250)
Parent Cash	0.0626	0.0607	0.0630	0.0521
	(0.0267)	(0.0222)	(0.0252)	(0.0246)
Girl Voucher	0.0898	0.0734	0.0913	0.0573
	(0.0267)	(0.0224)	(0.0252)	(0.0246)
Observations	172	170	173	168
Mean Y	0.64	0.65	0.61	0.78
SD Y	0.151	0.142	0.146	0.155
p info=parents	0.603	0.754	0.562	0.965
p info=girls	0.129	0.386	0.094	0.872
p girls=parents	0.305	0.566	0.260	0.832

Source: de Walque and Valente (2022). Dependent variable: attendance spot checks: any pupil listed on the class roll and not present in the class at the time of the attendance check is coded as absent. All regressions include a constant and district fixed effects. Standard errors in parentheses. “p arm_i=arm_j” denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-14: Effect of Treatments on Eligible Girls' Consumption of Personal Items

	Dependent Variable: Consumption of Personal Items Not Purchased With Vouchers (meticais)	
	(1) All observations	(2) Top 1% removed
Information	19.55	47.52
	(72.94)	(64.96)
Parent Cash	-50.08	-41.66
	(71.83)	(63.97)
Child Incentive	-68.40	-89.18
	(71.83)	(63.97)
Constant and District FE	Yes	Yes
Observations	173	173
Mean Y	831.69	783.72
SD Y	517.92	462.06
p info=parents	0.344	0.174
p info=girls	0.232	0.038
p girls=parents	0.798	0.456

Source: Endline household survey (de Walque and Valente, 2022). The dependent variable is the total value of purchases, over the 12 months preceding the survey, of the following items: trousers/skirts, shirt/t-shirt/jumper, school uniform, other ready-made garments, made-to-measure clothing, clothing repairs, shoes, sandals, trainers, other types of shoes, shoe repairs, matches, soap (detergent), soap (personal hygiene), toothpaste, teeth cleaning twig, perfume, deodorant, backpack, travel bag/handbag, batteries, magazines/newspapers, any other good for personal use (e.g., hair extensions, etc…). Standard errors clustered at the school level in parentheses. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-15: Effect on Attendance by Selected School Population Characteristics

	(1) Share present at spot check		(2) Share present at spot check		(3) Share present at spot check
Information	0.0461	Information	0.0418	Information	0.0554
	(0.0239)		(0.0241)		(0.0224)
Information × Poorest	-0.0121	Information × Oldest	0.0247	Information × Furthest	0.0369
	(0.0434)		(0.0446)		(0.0538)
Parent Cash	0.0496	Parent Cash	0.0637	Parent Cash	0.0480
	(0.0301)		(0.0264)		(0.0231)
Parent Cash × Poorest	0.0435	Parent Cash × Oldest	0.00589	Parent Cash × Furthest	0.0547
	(0.0456)		(0.0527)		(0.0615)
Child Incentive	0.0966	Child Incentive	0.0766	Child Incentive	0.0961
	(0.0289)		(0.0311)		(0.0263)
Child Incentive × Poorest	-0.0408	Child Incentive × Oldest	0.0541	Child Incentive × Furthest	0.0140
	(0.0540)		(0.0564)		(0.0640)
Poorest	0.0320	Oldest	-0.0793	Furthest	-0.0184
	(0.0631)		(0.0670)		(0.102)
District FE	Yes		Yes		Yes
Interactions District FE and Poorest or Oldest or Furthest	Yes		Yes		Yes
Observations	173		173		173
P-value 3 interactions=0	0.355		0.758		0.780

Source: de Walque and Valente (2022). Unannounced spot checks attendance data (for outcome variable) and household survey (variables interacted with the treatment indicators). “Poorest”, “Oldest” and “Farthest” are indicator variables equal to one if the school’s share of girls surveyed at baseline that are classified as “poor”, “old”, and “far from school”, respectively, is in the top tercile of the school distribution. “Poor” refers to girls in the lowest household wealth tercile, “old” refers to girls in the highest individual tercile for age (14 and above at baseline) and “far from school” refers to girls in the highest individual tercile for time taken to travel to school (33 minutes and above). Standard errors in parentheses.

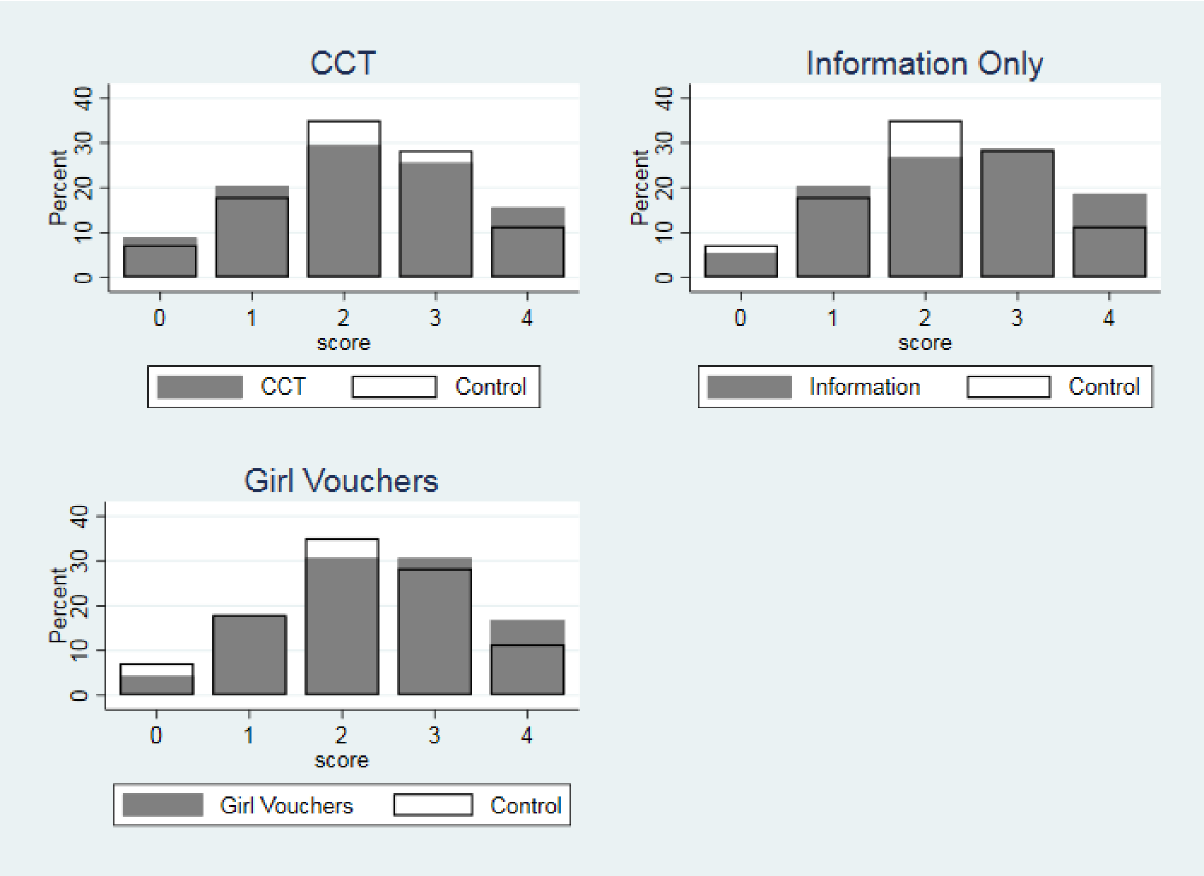
Table A-16: Learning Effects Outside the Mean

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	score	I[score=4]	I[score>2]	I[score<2]	score	I[score=4]	I[score>2]	I[score<2]
Panel A: School averages								
Information	0.183	0.0699	0.0804	-0.00342	0.195	0.0782	0.0829	-0.00342
	(0.0911)	(0.0281)	(0.0384)	(0.0326)	(0.0909)	(0.0284)	(0.0377)	(0.0326)
Parent Cash	0.0202	0.0471	0.0228	0.0341	-0.00233	0.0427	0.0208	0.0341
	(0.0898)	(0.0277)	(0.0378)	(0.0321)	(0.0897)	(0.0280)	(0.0372)	(0.0321)
Girl Voucher	0.203	0.0338	0.0799	-0.0468	0.178	0.0256	0.0693	-0.0468
	(0.0898)	(0.0277)	(0.0378)	(0.0321)	(0.0904)	(0.0282)	(0.0374)	(0.0321)
Characteristics	No	No	No	No	Yes	Yes	Yes	No
Observations	173	173	173	173	173	173	173	173
Panel B: Individual outcomes:								
Information	0.166	0.0740	0.0776	0.00242	0.171	0.0835	0.0829	0.0105
	(0.0806)	(0.0259)	(0.0371)	(0.0304)	(0.0823)	(0.0274)	(0.0379)	(0.0307)
Parent Cash	0.00358	0.0435	0.0190	0.0413	-0.00673	0.0449	0.0173	0.0494
	(0.0842)	(0.0219)	(0.0359)	(0.0323)	(0.0830)	(0.0217)	(0.0360)	(0.0318)
Girl Voucher	0.210	0.0571	0.0859	-0.0359	0.226	0.0647	0.0966	-0.0356
	(0.0730)	(0.0208)	(0.0325)	(0.0286)	(0.0693)	(0.0206)	(0.0314)	(0.0280)
Baseline Characteristics	No	No	No	No	Yes	Yes	Yes	Yes
Observations	2600	2600	2600	2600	2464	2464	2464	2464
No. of clusters	173	173	173	173	173	173	173	173
Mean individual outcome	2.19	0.11	0.40	0.25	2.20	0.11	0.40	0.25

Source: Endline household survey (de Walque and Valente, 2022). I [] denotes the indicator function. “Score” is the ASER math test score, which takes the following possible values: recognition of single digit numbers (scored 1), recognition of double-digit numbers (scored 2), correct subtraction (scored 3), correct division with remainders (scored 4), or cannot even recognize single digit numbers (scored 0). All regressions include a constant and district fixed effects. Panel A: standard errors in parentheses. Panel B: school-level clustered standard errors in parentheses.

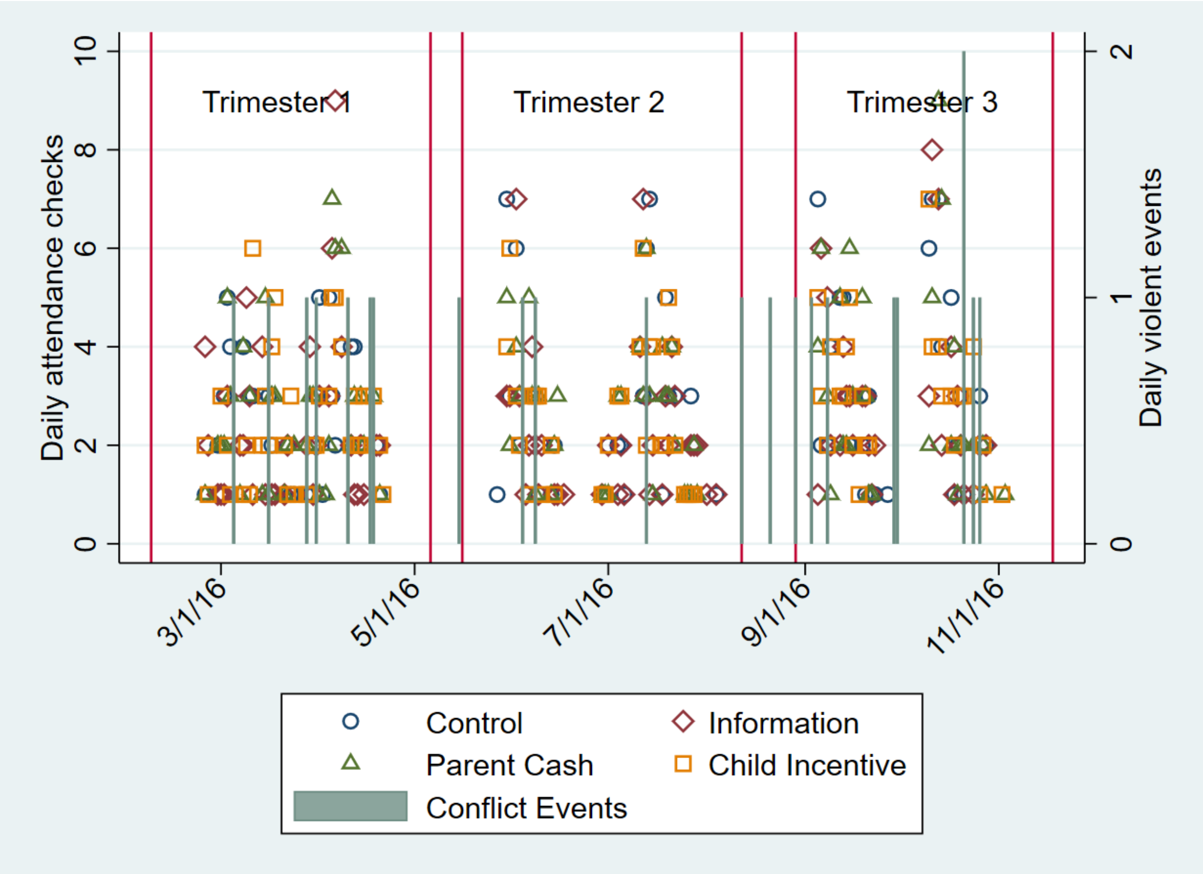
B. Appendix Figures

Figure A-1: Effect of the Treatments on the Distribution of Math Scores



Source: Endline household survey (de Walque and Valente, 2022). “Score” is the ASER math test score, which takes the following possible values: recognition of single digit numbers (scored 1), recognition of double-digit numbers (scored 2), correct subtraction (scored 3), correct division with remainders (scored 4), or cannot even recognize single digit numbers (scored 0).

Figure A-2: Timing of Attendance Spot Checks and Conflict Events



Source: Attendance spot checks (de Walque and Valente, 2022) and ACLED (2021)

C. Further Related Literature

C1. Role of Information

For conciseness, in the main text we focus on the literature interested specifically in the information asymmetry between parents and their children in the area of education. Gallego, Malamud and Pop-Eleches (2018) have documented evidence of information asymmetry between parents and children regarding internet usage, and a rich body of work has shown evidence of misinformation relevant to educational choices that goes beyond parent-children asymmetric information (e.g., Nguyen, 2008; Jensen, 2010; Bettinger et al., 2012; Hoxby and Turner, 2013; Dinkelman and Martínez, 2014; Wiswall and Zafar, 2015; Andrabi, Das and Khwaja, 2017; Dizon-Ross, 2019).

C2. Children Incentives

For experiments incentivizing students, but not parents, for the student to achieve a certain standard at scholastic tests or a range of inputs in this test, see Angrist and Lavy (2009), Kremer, Miguel and Thornton (2009), Jackson (2010), Fryer (2011), Bettinger (2012), Levitt et al. (2016), Burgess, Metcalfe and Sadoff (2016), Hirshleifer (2017). For experimental evaluations of the effect of distributing free school uniforms without conditionality, see Hidalgo et al. (2013), Duflo, Dupas and Kremer (2015) and Evans and Ngatia (2020).

D. Qualitative Evidence

Focus groups interviews took place in May 2014 to obtain a qualitative understanding of the relevance, acceptability, and feasibility of the proposed study in the Manica context. The focus groups consisted of five groups of girls age 11-15 (mostly 6th-7th graders for those currently enrolled in school) and their parents or guardians. The girls and their parents/guardians were interviewed separately to avoid girls' answers being influenced by the presence of their parents or guardians, yielding 10 focus groups in total. One of the authors was present at all the interviews, which were carried out in the local language by a member of our partner NGO, Magariro, with experience in carrying this type of interviews. The interviews were recorded, with the consent of the participants, and then translated to permit analysis by the PI.[42]

There were between 3 and 10 participants per group. The recruitment aimed to purposely include girls who attend school regularly, girls who have dropped out, and girls who miss school a lot. Two sites were chosen to cover a remote rural setting and a setting that is closer to a main town. Both schools practice double shifts (one in the morning and one in the afternoon), with shifts of about 3 hours and a half each.

[42] Ethical clearance was obtained from the University of Bristol.

The same main reasons for missing school days conditional on enrolment were cited by parents and girls, namely: illness, lack of soap to wash themselves or their clothes, lack of decent clothes (which makes some girls ashamed or sad when they go to school and see others with nicer things), some children not liking school or simply preferring doing something else like playing or hanging out with their boyfriends (from 13/14 years old according to respondents). Two parents also said that sometimes the road was not passable due to rain. Only one parent in all the groups said that sometimes they asked their daughter to stay home to help them with some work on the family plot or to look after their siblings when their mother has to travel.

It was also clear that both parents *and* children had influence on the decision to go to school. It was interesting, for instance, to note that several parents contrasted different daughters of theirs, with one sibling going to school regularly without any problem, and another dragging their feet, arriving late at school, making excuses not to go, or simply skipping schools (e.g., seemingly going to school but then turning around and going back home while their parents are out for work). Here are a few illustrative quotes, from both settings:

From a mother of two girls, one age 14 (in 6th grade) and one age 11 (in 4th grade): on her older daughter: *"Ana, she's 14 and this one likes school, she knows how to read and write, she even reads the bible for me and explains it to me. Sometimes she doesn't go to school but it's not her fault but rather because of me asking her to stay home and help with some chores at home or on our plot when there is a lot to do and sometimes if I want to travel I ask her to look after her siblings at home and she gets cross because she likes to study. Her sister Laura doesn't know how to write, she only learnt how to write her name this year, she is lazy and doesn't like to go to school. She is 11 and is in fourth grade."*

From another mother of two girls, one age 14 and one age 12: on her younger daughter: *"Yes, sometimes this one she misses school. She says she's ill, she has no soap to wash her clothes, etc… They are very different. When food is late, Luisa [the older sister], she goes to school all the same, whereas Maria, she waits and sometimes misses school because of that. Luisa only misses school if she's actually ill."*

From yet another mother of two girls, on her older daughter: *"My older daughter, Gabriela, she doesn't like going to school, she misses school a lot. I want her to go but she doesn't like it. You tell her: it's time to go, and she says: I'll go tomorrow. She finds excuses. Lucia, no, she likes going to school. Even if lunch is late, she goes to school anyway [i.e., skipping lunch]."*

And from a father of two girls, on his younger daughter: *"Veronica doesn't like school, but her older sister likes it. Sometimes she says: my clothes are dirty, or my clothes are still wet. Sometimes I'm not even sure she doesn't turn around and goes back home instead of going to school. Sometimes she says she's feeling unwell, but I'm dubious."*

All in all, the overall message seems to be that when parents want children to go to school and children do not want to, many parents' testimonies seem to imply that they had little influence on their children and that in the end children did what they wanted. Of course, some parents seemed more “in charge”. For instance, when asked whether his daughter attended school regularly, the father of one 13-year-old responded that *“Yes, she goes to school regularly, I don’t accept her not going to school”*. Several of the better educated parents said they checked their children’s notebook (“caderno diario”) to check what they had been up to (E.g., *“I check Maria’s [notebook] more because I know she doesn’t like school. She also tends to be late, so I check on her a lot.”*). And there may be other ways for illiterate parents to exert some monitoring: *“I have no way to control them on their way [to school]. I don’t know how to read, I can’t check their notebook or anything. But I hear her when she’s talking to her brothers, what she’s talking about”*. However, the monitoring technologies available to parents are certainly imperfect - when asked directly about whether they know what their children do on the way to and from school, two parents replied: *“On the way to and from school, we can’t control them” “We don’t know.”*.

When girls were asked whether they attended school regularly, the vast majority said that they did. However, when asked about the attendance of other girls in general, several girl participants report that there are girls who skip school a lot *(“It varies from person to person, there are girls who go every day but also others who miss school two or three days a week.”*), or who go to school but do not actually enter the classroom. In what highlights the limitations of the “monitoring technologies” at the disposition of parents, one girl told us that *“many stay in the street chatting to boys and then go and write in their notebook, faking corrections and go back home and show it to their parents when they never even reached the school.”*.

E. Pre-Specified Outcomes

The following main and secondary outcomes were registered on the AEA registry in February 2016. Main outcomes: school attendance conditional on enrollment, unconditional attendance, and school enrollment. Secondary outcomes: teacher absenteeism, score at ASER math test and RAVEN test, marital status, self-reported quality of monitoring of daughter's school attendance, and intra-household bargaining power. There was no further pre-analysis plan other than pre-specifying these outcomes. Here we report estimates for all the outcomes which we were able to measure satisfactorily. The two exceptions are: (i) RAVEN test, which ended up not being fielded in the endline questionnaire because pre-tests of the endline questionnaire suggested it was too long and (ii) unconditional attendance. We intended to construct this measure of unconditional attendance by setting attendance to 1 if a girl from the household survey was observed in any of our spot check class rolls and present at a check, and zero if she was matched but absent or if she could not be matched to any spot check record. If, despite being announced after the official school enrollment period, the treatments had had an impact on enrollment, this outcome variable would have allowed us to estimate the effect of the treatments on attendance independently of any selection into school enrollment, albeit on the much smaller household survey sample rather than on the universe of EP2 girls.

While, conditional on being reported by her parent as being enrolled in the endline household survey, the probability of finding a match in one of our 173 school records of 2016 enrollees is high (80%), this probability varies significantly across treatment arms. When estimating Equation (4) on the sample of girls who are reported as being enrolled in 2016 in the household survey, and defining Y_c as the share of girls with a match in our 2016 class rolls, the coefficients associated with the information only arm is -0.05, that associated with the parents cash arm is 0.02, and that associated with the child incentive arm is 0.008. In contrast, the largest absolute effect of our treatments on the share of girls self-reported as enrolled in Table 2 is 0.027, and this effect is shown not to be robust. Since evidence supports the conclusion that our treatments had no robust effect on enrollment or on school switches, while we are unequally successful across experimental arms in matching names of self-reported enrollees from the household survey with those found in school records, analyzing the effect of the treatments on unconditional attendance would be a bad cure for a non-existent ailment.

F. Detailed Description of Further Robustness Checks

Correcting for attrition for outcomes measured through the household survey. As reported in the main text, attrition of girls taking part in the household survey was slightly larger in the control group than in the treated groups, although not jointly statistically significantly so. Our main outcome of interest (independently verified attendance rate at school) and teacher attendance are not affected by any differences in attrition in the household survey. In Table A-5, we present results for the other outcomes, correcting for differences in survey attrition. More precisely, we ran regressions in which the school averages are obtained after weighting each individual observation in the endline survey sample by the inverse of the probability that it is included in the sample, as predicted by all the individual and household baseline characteristics summarized in Table A-2. Reassuringly, reweighting observations by the inverse of the probability that they attrit does not change our conclusions.

Controlling for pre-treatment outcomes. As an additional robustness check, we also present ANCOVA estimates obtained from estimating Equation (4) with an additional regressor capturing- or proxying for baseline outcomes. When the outcome was measured at baseline, we control for the baseline value of the outcome variable. Alternatively, we control for an available proxy of the outcome at baseline when a reasonable proxy exists. When the baseline outcome is available, a commonly used approach is to use a Difference-in-Differences specification. Using an ANCOVA approach is preferable to Difference-in-Differences even when the baseline outcome is available, as there is no loss of power when the correlation between pre- and post-treatment outcomes is low (McKenzie, 2012). Results in Table A-6 show that all our conclusions are robust to the inclusion of these pre-treatment outcomes.

Sample-weighted estimates. The main analysis reported in this paper is carried out at the school level (i.e., averaging variables at the school level) without applying any sampling weights, so that each school is weighted equally. We repeated the analysis at the individual level (clustering the standard errors at the school level), and thus weighting each school by the size of the school sample. For outcomes measured at spot checks, this essentially implies weighting each school by the size of its female EP2 intake. For outcomes based on the household survey, the sampling target was to interview the same number of observations per school (20), which would have led to the same weighting as in the cluster-level analysis. In practice, there was some variation in the household-survey sample size across schools—but *not* across experimental arms—due to difficulties locating the girls listed in the school records, as discussed in Section II-A. It is therefore less clear how the weighting in these individual-level estimates should be interpreted. Results—shown in Table A-7—are however largely unchanged.

No selection of girls through school switches. The treatments were announced after the official enrollment period closed, and, in most cases, after the start of the school year, so that a negligible effect on enrollment was to be expected, as confirmed in our data analysis. Another potential source of selection of girls into the school registers for which the survey firm recorded spot check attendance data is through school switches. Out of the 2,687 endline survey girls who were reported by their parents as being enrolled for the 2016 school year, only 157 (5.84%) were reported as being enrolled in a school other than the one they were sampled from at baseline. Estimating Equation (4) using, as dependent variable, a binary indicator equal to one if the girl is reported enrolled in a different school to that from which she was sampled and zero if she was reported enrolled in her original school, no treatment indicator is individually significant (nor are they jointly significant).[43] As a further robustness check, we re-estimated the effect of our treatments on attendance, but restricting the sample used to construct the share of girls present to names registered on the class roll at the first spot check. The first spot checks were carried out within the two first months of school (between February 25 and March 31), and so well before any end-of-trimester transfers were paid. The class rolls called by the independent surveyor were slightly updated between spot checks for various reasons. A few girls changed classes or schools during the year, some names were updated to match the girl's used name when it did not match that with which she was recorded in the school register, or to match the name used at home in the case of girls included in the household survey sample. Estimates obtained by restricting the spot checks data to girls with exact name matches from the first attendance check roll are presented in Table A-8. These results are near-identical to those obtained in the main analysis, thus confirming that selection through school switches is unlikely to be biasing our results.

Trimming the school sample. The school-level analysis carried out in the paper is much less sensitive to outliers in terms of school size than individual-level analysis (since each school is given the same weight). Still, in Table A-9, we report results obtained when dropping the 5% largest schools and 5% smallest schools to test whether results are very different in the tails of

[43]Individual coefficients (p-values) are: 0.018 (0.355), -0.007 (0.713), 0.017 (0.355) for the information, parent cash and child incentive arms, respectively, and the joint F-test p-value is 0.457.

the school size distribution. Trimming the school sample in this way tends to increase slightly the magnitude of all the treatment effects without altering any of the conclusions based on the baseline results.

Excluding spot check data where conflict caused substantial disruptions to data collection. Low-level conflict between government and RENAMO forces slowed down but did not prevent household data collection at baseline and endline. School closures due to the conflict are balanced across experimental arms: the p-value of a joint test of no treatment effect in a regression of an indicator for the school having closed at any point in the school year due to the conflict on the three treatment dummies and district (strata) fixed effects is equal to 0.89. Figure A-2 plots the number of daily spot checks by experimental arm against daily violent events recorded in Manica province in the ACLED dataset (Raleigh et al., 2010). It shows that both violent events and attendance spot checks were spread out across the whole school year. Within a spot-check "round", which each lasted about one month, there is also no difference across treatment arms in the probability of the enumerator successfully collecting attendance data (p-value: 0.27). At the peak time of tension in the most affected district (Mossurize), however, many schools were closed so that attendance data collection could not proceed. The schools for which we were able to collect attendance data at those times may therefore be selected (although, as mentioned before, there was no overall difference between treatment arms in the number of times attendance data was collected). Table A-10 reports estimates for the two outcomes based on attendance checks obtained when ignoring data from spot checks for which less than 70% of the district's schools could be surveyed.[44] Point estimates decrease slightly in magnitude—suggesting the treatments may have had larger effects at times of high absenteeism due to the conflict, but the overall picture is unchanged.

Selecting control variables based on their predictive power instead of baseline balance. Throughout the paper, we check the robustness of our findings to controlling for characteristics that were not balanced for at least one treatment arm at baseline. In Table A-11, we instead control for covariates chosen based on their ability to predict the outcomes studied, irrespective of baseline balance. More specifically, in this robustness check we control for any baseline variable which has a t-statistic equal to 1.96 or above when regressing the outcome on district fixed effects and the baseline characteristics reported in Table A-2 in the control group. The effect of the CCT on high self-reported monitoring quality becomes statistically significant at 10% but our overall conclusions are unchanged.

Ex-post power calculations. In Table A-12, we report ex-post power calculations using the means and standard deviations of the outcomes studied in this paper in the control group, for 80% power in detecting differences between any experimental group pair and a Type 1 error of 0.05. In keeping with the main analysis, we present power calculations based on the

[44] Note that where the robustness checks apply to spot check data only, we only report results for outcomes based on spot check data – hence not test scores, which are collected in the endline household survey. For instance, in Table A-10, we exclude spot check data collected at times of substantial conflict disruption, but this does not affect outcomes based on the household survey since the conflict slowed down but did not prevent data collection at baseline or endline.

distribution of school-level averages.[45] The last column reports the Minimum Detectable Effect (MDE) as a share of the control group's mean, showing that the experiment is well-powered for our three schooling outcomes, teacher absenteeism and self-reported monitoring quality, but not for early marriage and self-reported empowerment. This bolsters our confidence in the results for which we find consistent significant effects, while confirming the inconclusiveness of our findings for early marriage and self-reported empowerment.

[45] The standard deviation in the school-average distribution of ASER scores (0.567) is much smaller than the standard deviation in the individual-level distribution (1.083). When computing power for an analysis carried out at the individual level, and taking the mean, standard deviation, and intraclass correlation in the control group as reference parameters, the MDE for 80% power for a 0.05 Type 1 error corresponds to 0.265 of a standard deviation.

Appendix-Only References

ACLED. 2021. “The Armed Conflict Location & Event Data Project”. Conflict data for Mozambique [March 1, 2016 to November 3, 2016]. https://developer.acleddata.com/. Accessed July 8, 2021.

Andrabi, Tahir, Jishnu Das, and Asim I. Khwaja. 2017. “Report Cards: The impact of providing school and child test scores on educational markets.” *American Economic Review* 107(6): 1535-63.

Angrist, Joshua, and Victor Lavy. 2009. “The effects of high stakes high school achievement awards: Evidence from a randomized trial.” *American Economic Review* 99(4), 1384-1414.

Bettinger, Eric P. 2012. “Paying to learn: The effect of financial incentives on elementary school test scores.” *Review of Economics and Statistics*, 94(3), 686-698.

Bettinger, Eric, Bridget Long, Philip Oreopoulos, and Lisa Sanbonmatsu. 2012. “The Role of Application Assistance and Information in College Decisions: Results from the H&R Block FAFSA experiment”. *The Quarterly Journal of Economics* 127(3), 1205-1242.

Burgess, Simon, Robert Metcalfe, and Sally Sadoff. 2016. “Understanding the response to financial and non-financial incentives in education: Field experimental evidence using high-stakes assessments.” (IZA Discussion Papers, No. 10284.

Dinkelman, Taryn and Claudia Martínez A. 2014. “Investing in Schooling in Chile: The role of information about financial aid for higher education.” *The Review of Economics and Statistics* 96(2), 244-257.

Dizon-Ross, Rebecca. 2019. “Parents’ Beliefs About Their Children’s Academic Ability: Implications for Educational Investments.” *American Economic Review*. 109(8): 2728-65.

Duflo, Esther, Pascaline Dupas, and Michael Kremer. 2015. “Education, HIV, and Early Fertility: Experimental evidence from Kenya.” *The American Economic Review* 105(9), pp.2757-2797.

Evans, David. and Mũthoni Ngatia. 2020. “School Uniforms, Short-Run Participation, and Long-Run Outcomes: Evidence from Kenya.” *World Bank Economic Review* 35(3): 705-719.

Fryer, Roland G. 2011. “Financial incentives and student achievement: Evidence from randomized trials.” *The Quarterly Journal of Economics* 126(4), 1755-1798.

Hidalgo, Diana, Mercedes Onofa, Hessel Oosterbeek, and Juan Ponce. 2013. “Can Provision of Free School Uniforms Harm Attendance? Evidence from Ecuador.” *Journal of Development Economics* 103:43-51.

Gallego, Francisco, Ofer Malamud. and Cristian Pop-Eleches. 2018. “Parental Monitoring and Children’s Internet Use: The role of information, control, and cues.” NBER Working Paper No. 23982.

Hoxby, Caroline and Sarah Turner. 2013. “Expanding College Opportunities for High-Achieving, Low-Income Students.” Stanford Institute for Economic Policy Research Discussion Paper.

Jackson, C. Kirabo. 2010. “A Little Now for a Lot Later: A look at a Texas Advanced Placement Incentive Program.” *Journal of Human Resources 45*(3), 591-639.

Jensen, Robert. 2010. “The (Perceived) Returns to Education and the Demand for Schooling.” *The Quarterly Journal of Economics* 125(2), 515-548.

Kremer, Michael, Edward Miguel, and Rebecca Thornton. 2009. “Incentives to Learn.” *Review of Economics and Statistics* 91 (1): 437- 456.

Levitt, Steven D., John A. List, Susanne Neckermann, and Sally Sadoff. 2016. “The behavioralist goes to school: Leveraging behavioral economics to improve educational performance.” *American Economic Journal: Economic Policy* 8(4), 183-219.

Ministério da Saúde (MISAU), Instituto Nacional de Estatística (INE), e ICF. 2015. *Inquérito de Indicadores de Imunização, Malária e HIV/SIDA em Moçambique 2015*. Household Recode (MZHR71FL). Demographic and Health Surveys Program. https://dhsprogram.com/data/dataset/Mozambique_Standard-AIS_2015.cfm?flag=0. Accessed April 6, 2020.

Nguyen, Trang. 2008. “Information, Role Models and Perceived Returns to Education: Experimental Evidence from Madagascar.” Unpublished manuscript.

Raleigh, Clionadh, Andrew Linke, Håvard Hegre and Joakim Karlsen. 2010. “Introducing ACLED-Armed Conflict Location and Event Data.” *Journal of Peace Research* 47(5) 651-660.

Wiswall, Matthew and Basit Zafar. 2015. “Determinants of College Major Choice: Identification using an information experiment.” *The Review of Economic Studies* 82(2), 791-824.