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

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This draft: June 2019

Abstract:

If parents do not perfectly observe whether their children attend school, (i) cash transfers conditional on an attendance target may increase school attendance in part through an information effect and (ii) incentivizing children directly could be more cost effective than incentivizing parents. We isolate experimentally and for the first time the information effect of a CCT and find that it is large. Incentivizing children is at least as effective as incentivizing parents—and importantly, not because parents were able to appropriate conditional transfers made to children. These results imply the possibility of large savings relative to traditional CCTs.

JEL Codes: I25, D82, N37

Keywords: school attendance, conditional cash transfers, moral hazard.

*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, 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, 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.

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I- Introduction

Parents and children may disagree on the optimal level of investments in the children's human capital, and children may have substantial agency in schooling decisions.[1] Parents may, for instance, act as principals who want to maximize the effort of their child, but do not perfectly observe the child's effort (Heckman and Mosso, 2014). If this is the case, there may be preferable alternatives to the very popular conditional cash transfers (CCT) program model. CCTs, which are now implemented in over 60 countries (Parker and Todd, 2017), typically incentivize parents financially to comply with conditions such as regular school attendance by the child. Since payments are only made conditional on compliance with the attendance target, receiving or not receiving the conditional transfer conveys information to parents about whether or not the child attended school regularly (Bursztyn and Coffman, 2012). This information could in itself be responsible for part of the previously documented positive effects of CCTs on attendance. If this information effect is large relative to the CCT effect, attendance gains could be achieved at a much lower cost than with a CCT. In addition, if children have private information about their school attendance, incentivizing children themselves could be more cost-effective than incentivizing parents since parents are unable to perfectly match rewards to the children's actions.[2]

For the first time, we isolate experimentally the information effect of a conditional transfer program. We do so by comparing the effect of an "information only" treatment giving parents the same information about the child's school attendance as that contained in a conditional transfers program. In addition, we compare the effect of incentivizing parents versus incentivizing children to attain an attendance target, and in doing so, provide evidence of the relative importance of children and parents' returns in the decision to attend school. 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

[1] Kremer and Holla (2009) note that repeated instances of peer effects in school attendance decisions (e.g., Lalive and Cattaneo, 2006; Cipollone and Rosolia, 2007; Bobonis and Finan, 2009) suggest that children have agency in these decisions. And when asked, a large share of children who have dropped out of school by age 15 in India (40%), Ethiopia (58%), Peru (80%) and Vietnam (65%) say that they themselves played the most important role in deciding to do so (authors' calculations based on Young Lives Round 3 data (Boyden, 2014)).

[2] As explained more formally in Section II-B.

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 ("girl voucher" or "child incentive" treatment).

We draw two main conclusions from our experiment. First, we find evidence that the information content of a conditional transfer program can have a substantial effect on school attendance independently of any conditional transfer. In our experiment, where the value of the transfer is modest (7% of per capita GDP) but similar to safety net programs found elsewhere,[3] the estimated effect of the information treatment on attendance (4.5 percentage points) is as large as 75% of the effect of the CCT treatment. Our second key finding is that children's returns to school attendance matter at least as much as parental returns to their child's attendance in the decision to attend. Indeed, incentivizing children is at least as effective in raising attendance as incentivizing parents—and importantly, we find that this is **not** because parents were able to appropriate transfers made to children.

From a policy point of view, our results provide evidence of the effectiveness of two lower cost, easily scalable alternatives to traditional CCTs: providing weekly information to parents about their child's attendance at school through a simple paper and pen report card to be taken home at the weekend, and incentivizing children with vouchers to be exchanged for a limited choice of items which their recipient is likely to be able to hold on to such as shoes, backpacks, and school uniforms. This raises the possibility of large savings: the annual cost of increasing attendance by one percent is $2.68 in our CCT arm, compared to only $1.57 for the girl voucher arm and as little as $0.33 in the information arm.[4]

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[3] 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.

[4] While these cost effectiveness figures 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 et al., 2017).

In related literature, Bursztyn and Coffman (2012) show that parents in a poor urban Brazilian setting value the information component of a national CCT since, in a lab experiment, a majority of parents only prefers an unconditional cash transfer (UCT) to a CCT of similar nominal value if text messages about their child attendance accompany the UCT. Recent studies have also found that improving the information parents receive about attendance (Berlinski et al., 2017; Rogers and Feller, 2018) and other measures of student effort at school (Bergman, forthcoming; Bergman and Chan, 2017; Cunha et al., 2017) in urban, middle- to high-income country settings increases attendance and, in three out of five cases, improves test scores as well (Bergman, forthcoming; Berlinski et al., 2017; and Cunha et al., 2017).[5,6] But our experiment is the first to compare the effect of a conditional transfer program with that of a treatment only giving parents the information contained in the conditional transfer program, and thus the first to isolate the effect of the information component of a CCT.

The second key contribution of our study is to compare the importance of parents' and children's returns in attendance decisions—a question of crucial importance for the optimal design of education policies but on which little is known.[7] Three studies compare experimentally the effect of incentivizing

[5] 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.

[6] For conciseness, we focus here on the literature interested specifically in the information asymmetry between parents and their children in the area of education. Gallego et al. (2017) 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 et al., 2017; Dizon-Ross, 2017).

[7] 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 et al. (2009), Jackson (2010), Fryer (2011), Bettinger (2012), Levitt et al. (2016a), Burgess et al. (2016), Hirshleifer (2017). For experimental evaluations of the effect of distributing free school uniforms without conditionality, see Hidalgo et al. (2013), Duflo et al. (2015) and Evans and Ngatia (2017).

parents relative to incentivizing children to achieve an attendance- (Baird et al., 2011), performance- (Berry, 2016), or combined performance and attendance target driven mainly by performance (Levitt et al., 2016b).[8] None of them find significant differences, on average, between incentivizing parents and children. However valuable, knowledge of whether it is more effective to incentivize parents or children to achieve a certain grade does not, in general, answer the question of the relative importance of parents' and children's returns to human capital investments in schooling decisions. For it to be the case, the investments of parents and children in children's human capital should contribute equally to the production of test scores, and this is unlikely to be the case.[9] For instance, Berry (2016) finds no significant difference between incentivizing parents or children on average, but 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 input such as attendance vary with the recipient of the incentive (parents or children), we provide a direct test of the agency of children in schooling decisions. Unlike the only other previous study incentivizing parents and children to achieve an **attendance** target (Baird et al., 2011), 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 in order to ensure that children are the end recipient of the incentive intended to them.[10] We do so by incentivizing children not with cash but with vouchers redeemable against a 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

[8] While the combined targeted in Levitt et al. (2016b) also comprised targets on attendance and behavior at school, 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.
[9] See Del Boca et al. (2017) for evidence of differential returns to children and parental inputs and, in particular, that investments in learning made by children age 10-14 are more important for test scores than those of their parents.
[10] Baird et al. (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.

did not appropriate indirectly the transfers to the child by reducing the child's non-food private consumption.[11]

Intriguingly, we replicate the puzzling finding from most evaluations of CCTs that gains in attendance achieved by incentivizing parents financially do not translate into significant gains in test scores.[12] In contrast, both the information treatment and the children's incentives treatment improve scores on the (ASER or "Annual Status of Education") math test by 8.5 to 9.4% of the control group's mean, and statistical tests allow us to reject the hypothesis that the CCT treatment has the same effect on learning as the other two treatments. This suggests that improved attendance is beneficial for cognitive skills, but that financial incentives directed at parents specifically cancel out at least part of the gains from the information component of the CCT. Our experimental design and results give us the unprecedented opportunity to show that CCTs can exhibit their commonly observed "attendance without significant learning" pattern for reasons other than test sample selection and school quality concerns, and probably too for reasons other than intrinsic motivation crowding out and multitasking incentive problems. Part of the difference in the effect of our treatments on learning may be due to children less willing or able to learn being induced to attend school due to the introduction of parental financial incentives. We also find some support in our data for a new mechanism for the poor performance of CCTs on learning working through changes in parenting strategies, suggesting a fruitful avenue for future work.

In the remainder of the paper, we present the study context, theoretical motivation for, and design of our experiment (Section II), then turn to a description of the data and randomization process (Section III), before reporting our main results (Section IV) and various robustness checks (Section V). Section VI discusses our finding that increases in attendance translate into larger test scores gains in the information only and children incentives arms than in the CCT arm. Section VII concludes.

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[11] An alternative would have been to give cash to the children, and then check whether the pattern of household and individual consumption was the same whether the cash was targeted at parents or children. While interesting in itself, this exercise would have been unlikely to uncover a pattern of consumption which could not have been rationalized as the result of parents appropriating the cash intended to their daughter and them deciding how to spend it (potentially in part on goods consumed by the girl).

[12] 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).

II- 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 despite a doubling of real GDP per capita between 2001 and 2016 (from $615.3 to $1,128.3 PPP, World Bank 2017). 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. As of 2014, the net enrollment rate in primary education was 87.6%, up from 54.8% in 2002. But the survival rate to the last grade of primary education was only 33.2% in 2013, compared to a Sub-Saharan average of 57% (World Bank Education Statistics Data Bank, 2017).

While the net intake at Grade 1 of primary schooling is high for both boys (74.5%) and girls (73.1%), and secondary schooling is still restricted to an elite (17.9% net enrollment for both boys and girls), most children in Mozambique, and girls in particular, experience difficulties in completing primary school.[13] For upper primary schooling (Grades 6 and 7 or Ensino Primário de Segundo Grau "EP2", which the present study focuses on), the official completion rate is abysmal, especially in rural areas where even at age 19 it is only about 14% for males and 8% for females (Fox et al., 2012). In this context, a policy priority is to find ways to increase the school attachment of pupils, and girls in particular, in the higher grades of primary school.

We focused on 173 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 and is home to 7.5% of the country's population. It is close to the national average on a number of indicators, from 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%), to annual drop-out rates in primary schooling (6.8% in

[13] 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. All figures are taken from World Bank Education Statistics Data Bank (2017).

Manica and countrywide for EP1, 9.9% versus 8.8% nationwide for EP2) (INE, 2015; MPD-DNEAP, 2016). 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), 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 is, as explained below, motivated by the hypothesis that there may be imperfect monitoring of the children's actions by parents, which is plausibly more likely when population density is low and the school is located further away from the child's home.

B. Theoretical Motivation

One key policy tool used to improve school enrollment and attendance rates in today's developing world is cash transfers, which are often conditional on attendance and other prescribed behaviors. While CCTs have been implemented in over 60 countries (Parker and Todd, 2017), there are several unanswered questions about this type of social transfers.

One highly debated question is that of the role of conditionality. If the only reason why individuals do not invest more in human capital is that they face credit constraints, then cash transfers should have a positive effect on human capital investments, whether conditional or unconditional. On the other hand, conditionality may lead to larger increases in school enrollment, e.g. if individuals underestimate returns to education or if the conditionality helps parents monitor their children's behavior, since they can infer whether their child attended school regularly from the transfers they receive or do not receive. The first argument in favor of conditionality (underestimation of returns to education) is well-known. The second, however, has appeared only recently in the literature, and has been shown to be very relevant in the Brazilian urban context, where parents have been found to value the monitoring of their children's attendance at school (Bursztyn and Coffman, 2012).

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.[14] Bursztyn

[14] 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

and Coffman (2012) propose a simple principal-agent model to illustrate the effect of asymmetric information in attendance decisions. Their model can be summarized as follows. Consider the parent-child pair indexed by n for whom adult utility is:[15]

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

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:

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

A parent can only condition transfers to the child based on signal s_n, which is correct with probability π.[16] 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:[17]

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be found in Doepke et al. (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.

[15] 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.

[16] 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.

[17] 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 $e_n = 0$.

$$V_n^a \geq \frac{\pi}{2\pi-1}(c_n - V_n^c) \qquad (3)$$

Where the probability of inequality (3) holding increases with signal quality (higher π). As a consequence, under imperfect information, simply providing information to the parent may induce higher attendance. Since enforced CCTs give the parent, at a minimum, a binary signal as to whether or not the child met the attendance requirement upon which payments are conditioned, the conditionality may 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 first-time 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 $\frac{\pi}{2\pi-1}$. Indeed, increasing V_n^c by some transfer τ makes inequality (3) 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 $\frac{\pi}{2\pi-1}$. 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

In order 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

Condition (3) 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 $w_n \geq 0$.

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. 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, and, if they answered that it would, a follow-up, open-ended question then asked why they thought such a report would be useful. Eighty percent of parents responded "yes" to the first question, and among those, when asked (the open-ended question of) why they thought it would be useful, 98% responded that it would allow them to monitor their child's school attendance.

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. The main conclusions were 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, that if they did receive cash they would give it to their parents (or be expected to), but that there were 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.

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 "girl vouchers" treatment arm, we gave money-equivalent vouchers (400 meticais[18] at the end of each trimester with a maximum of 1,200 meticais over the 2016 school year) 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 (pens, notebooks, etc...), which 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 carried out in villages outside the study area. Importantly, school uniforms are *de facto* not compulsory in Mozambican

[18] 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.

primary schools and are only used by a minority of students. The same applies to school bags, so that the provision of these items does not simply equate to a school expenditure subsidy (and indeed we do not find that parents systematically substitute these items to expenditures on the girls, as discussed in Section IV-A). In a CCT treatment arm, we instead gave the same value (400 meticais per trimester) in cash to the parents and made the same items as in the "girl vouchers" arm available for **optional** purchase at the school. 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. In addition to matching the value of the vouchers given to girls to the cash received by parents, the price of items in vouchers matched the price in Mozambican meticais to reinforce comparability. By using vouchers rather than cash, we increased social acceptability of the program and, as discussed in Section IV, succeeded in ensuring that the end recipient of the conditional transfer was the recipient intended by the study design.[19]

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 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 the intervention. Parents, either through direct attendance at this initial meeting, or through learning about the report card system from other parents, teachers, or

[19] For previous experimental literature studying how to optimize the design of conditional transfers see: Baird et al. (2011), Benhassine et al. (2015) and Akresh et al. (2016) for comparisons of conditional and unconditional or "labeled" transfers; Benhassine et al. (2015), Akresh et al. (2016) and Haushofer and Shapiro (2016) for variation in the gender of the recipient; Barrera-Osorio et al. (2011) on the optimal timing of the transfers; and Skoufias et al. (2008) and Cunha (2014) on cash vs. in kind transfers.

pupils, could draw their own conclusions if a child decided not to show the parent the report card. 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.

In order 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" or "class directors") to fill out those cards, we introduced a small compensation scheme. The scheme worked as follows: in the three treatment groups, the class directors 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 airtime at the end of the trimester. 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 to thank them for their assistance and cover the cost of communications with the research team.

III- **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. 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 II-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 girl in the household who had completed, at least, 5^{th} Grade, and, at most, 6^{th} Grade, as of the end of 2015 (and was therefore potentially eligible for our treatments implemented in Grades 6 and 7 in 2016): 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).

Household survey sample. The household data used in the analysis is 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), and who still lived with their parent or guardian at baseline (given our analytical framework based on asymmetric information between parents and children). The target was to interview 20 potentially eligible girls per school, sampling those enrolled in 2015 (the last academic year before the experiment) and recent drop outs 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 drop outs") in the school. During fieldwork, however, there were difficulties locating the girls listed in the school records, and most of the recent drop outs 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 in order to help preserve power. All in all, the median number of girls surveyed per school in the baseline household survey is 18, and recent drop outs were under-represented in the household survey sample (3% of the baseline sample compared to 13% of all girls last enrolled in Grades 5 or 6 at some point during 2013-2015 across all 173 schools). There was no difference in the total number of girls interviewed in the baseline household survey, or the share of recent drop outs in the household survey sample, across treatment arms, however.[20]

[20] 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 drop outs (p-value: 0.21).

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 obtaining all the necessary administrative authorizations and assigning randomly each school to an experimental arm, the school director of each school was contacted by the implementation NGO Magariro to obtain the consent of the director for the school to take part in the research—all directors consented. 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. School 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). [21] 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.

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

[21] 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.

announced the treatments.[22] 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 the control group than in the treated groups although 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 above 0.10 (0.153). Robustness checks reported in Section V show that attrition is unlikely to be driving our conclusions. 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), and on average 5.6 times during the school year, and the number of times each school was surveyed is independent of experimental arm (the p-value of a joint F-test of no treatment effect in a regression of the number of attendance spot checks on the three binary treatment indicators and district fixed effects is 0.55).

B. Randomization and Experimental Balance

We first stratified our sample of 173 schools by district to avoid randomly occurring imbalances across experimental arms in district characteristics, in fieldwork operations (since these were organized district by district), as well as to gain power. 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 number generator.[23] At the time of the

[22] 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.

[23] 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

announcement of the treatments, a human error led to two schools in the Vanduzi district being swapped (one in the information treatment and one assigned to the parent cash treatment). 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.[24]

Table A-1 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, by treatment arm, as well as whether p-values of t-tests of differences between each treatment arm relative to the control group indicate that those differences are statistically significant.[25] Table A-1 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-1 are jointly orthogonal to treatment status.[26] Some individual differences are, however, statistically significant, and thus we provide robustness checks controlling for these baseline characteristics. Note that, other than for the many language and religion categories, for which there are some differences across experimental groups, the only other variables with some significant baseline differences between experimental arm pairs 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 parent-reported absences prior to the 2016 school year, but we can compare parent-reported absences for October 2016 to our

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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 “girl voucher” schools, 5 control schools and 21-17=4 “information” schools.

[24] Full results available on request.

[25] These p-values are those associated with 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.

[26] More specifically, the p-value associated with an F-test that the set of characteristics listed in Table 2 does not explain the experimental arm classification within district is between 0.17 (Information v. Parents) and 0.52 (Control v. Girls) for all 6 experimental arm pairs, and thus the null of joint orthogonality cannot be rejected. 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.

attendance spot check data for the same period in the control group. As we show in Section V, 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 in the CCT and girl vouchers arms relative to the control group. This is confirmed in our analysis, where we show that our conclusions are robust to controlling for baseline characteristics including parent-reported absences.

IV- **Main Results**

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

$$Y_c = \beta_0 + \beta_g T_{gc} + \beta_p T_{pc} + \beta_i T_{ic} + \boldsymbol{D_c'\beta_d} + \varepsilon_c \qquad (4)$$

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, parents, and information only treatment arms, respectively; $\boldsymbol{D_c'}$ 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, as in cluster-randomized experiments such as ours, this is both transparent and appealing because all the estimation formulas obtained for simple (as opposed to cluster-) randomization apply directly. In addition, 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 (2016) 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 Section V we report estimate at the individual level, and find similar results.

A. Effects on School Attendance and Self-Reported Enrollment

Table 2 presents estimates of the impact of the different interventions on schooling outcomes. In Columns (1) and (4), we report findings for our primary study outcome, i.e., school attendance 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-1 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 girl voucher 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 (report card) and the CCT (to parents) interventions. This leads to the conclusion that the information content of a conditional transfer program can have a substantial effect on school attendance independently of any transfer—in our experiment, where the value of the transfer is equivalent to 7% of GDP per capita, the estimated effect of the information treatment on attendance is as large as 75% of the effect of the CCT. In addition, the estimated effect of the information treatment on attendance is as large as 54% of the effect of the child incentive program. 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.

One concern in interpreting the effect of the "girl voucher" relative to that of the "parent cash" treatment is that 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 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 girl voucher arm responded that she had given away her reward or had had to sell it to give the money to someone else.[27] 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. 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, to test for this. While we found—unsurprisingly given substitutability—a negative effect on consumption of personal items (other than those purchased with the voucher) in the girl voucher arm compared to the control group, this effect was statistically 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-2). A symmetrical concern is that parents may simply have passed on to their daughters the transfers they received in the CCT arm. Only 4.3% of parents receiving cash transfers purchased any goods from the research team. In addition, the (statistically insignificant) effect of the parent cash treatment on consumption of personal items is negative, which strongly suggests that parents did not simply pass on their transfers to the girls.

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

[27] Of 101 girls chosen at random among the "girl voucher" survey sample to answer this question.

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 V), we conclude that the effect on enrollment was negligible.

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 school attendance at the time of the test. But the effect of cash transfers on test scores are generally insignificant, and 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). 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, irrespective of attendance at school, which are therefore not affected by the type of selection bias which may undermine test scores effect estimates from CCTs based on school tests. Similar to most previous evidence, we find that gains in attendance from cash incentives to parents do not translate into significant gains in test scores. On the contrary, 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.[28] We cannot rule out the possibility that the CCT has a small but positive effect on math scores since the p-value of a test that the CCT effect is equal to 0.1 (or about 0.09 of a standard deviation) is above 0.25. But we can reject that the effect of the CCT on learning is equal to that of the other two treatments (with p-values of between 0.034 and 0.077 depending on specifications). We return to this point in Section VI.

While the girls' incentive treatment tends to have a larger effect on attendance than the information treatment, the effect of both treatments on test scores is of similar magnitude and statistically significantly larger than in the parent CCT arm. The similar magnitude of the effects on math scores of the information

[28] 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).

only and girl voucher treatments may be due to the coarseness of the math scores, which can only take five possible values, from 0 for girls who cannot even correctly identify single-digit numbers to 4 for girls who can correctly perform divisions with remainders.

C. Effects on Pre-specified Non-Schooling Outcomes

Table 3 reports estimates of the impacts of our interventions on a set of pre-specified non-schooling outcomes (see Appendix C 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. There is no evidence that changes in teacher attendance may mediate the effects we find on child attendance and test scores, which gives support to the interpretation of these effects as resulting from a demand-side response. This also gives reassurance that the mechanism we set up to ensure that teachers were compensated for the time spent filling in the report cards (giving 250 meticais' worth of airtime to teachers who had filled in the report cards completely at the time of each spot check) was well-calibrated.

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. Given the mean and standard deviation that prevail in the control group, we lack power to detect realistic reductions in the proportion ever married. The minimum detectable effect for which we have 80% power is 2.88 percentage points, which would require, for instance, the control group to see more than a doubling of the share ever married compared to baseline while no new girl would form a union in the treated group. While the point estimates of the effect of the information and the parents' treatment on the proportion ever married are large in magnitude, 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.

If the self-reported measure of monitoring quality offered a reliable proxy of actual monitoring quality, then we would expect to see that our treatments increase self-reported monitoring quality. There are, however, several reasons to believe that this self-reported variable is a poor proxy. First, there is hardly any variation in answers to the questions on which the PCA scores are based. At baseline, only 3.4% (6.6%) of parents answered "neither agree nor disagree" or "disagree" when asked 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 could be due to nearly all parents genuinely believing that they are perfectly well informed about their child's school attendance, but the lack of variation is likely due instead to parents not wanting to acknowledge openly their lack of control. Indeed, when asked, at baseline, whether they thought it would be useful to receive a weekly attendance report card showing if their daughter had attended school regularly, 80% responded that it would be useful, thus suggesting that most of them think that their monitoring is not perfect, which contradicts their answers to direct questions about knowledge of their daughter's daily attendance. And in the confidence of the focus groups conversations between parents discussed in Section II-C, parents did express concerns about their ability to monitor their daughters' attendance. We return to this point in Section V, where we provide evidence of better knowledge about daughters' absences in treatment arms from comparing 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.

To summarize, we find evidence that providing high-frequency information to parents about their daughter's school attendance increases school attendance, and that this effect is not statistically distinguishable from that of a traditional 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—and importantly, **not** because parents were able to appropriate the vouchers directly or indirectly through intra-household reallocation of expenditure. In terms of learning, the attendance gains from the information only and girl incentives treatments translated into significant improvements in scores at a math test. None of the treatments had a robust effect on enrollment (as would be expected given the timing of the treatment announcements), teacher attendance, early marriage, self-reported quality of parental monitoring and self-reported girl autonomy. The next section explores the robustness of these findings.

V- **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 (2016) 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 (2016) is that of joint testing of multiple hypotheses. In Table A-3, 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.

Testing the information mechanism. Here we present further evidence in support of our interpretation of the effect of the information treatment as being due to an increase in the quality of parental monitoring rather than simply due to some generic "salience" effect of the treatment. For the girls surveyed in their households who were both (i) (reported by parents as being) enrolled in school in 2016 and (ii) could be matched to our independent school attendance records for the 173 schools included in the experiment,[29] we can evaluate the quality of the 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. 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. In the parents' incentive arm, this probability more than trebles and reaches 72% of the expected 0.045 coefficient. While the number of absences reported by the parents may not be exogenous, 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.[30]

Correcting for attrition for outcomes measured through the household survey. As reported earlier, 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. While 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, below we

[29] 77% of girls whose parents said were enrolled in Grade 6 or 7 could be matched to names in official school records.

[30] 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 or girls in gaming the system by omitting to report absences. Furthermore, we note that if the report cards were manipulated to the extent to be devoid of information content, we would not observe the increases in independently verified attendance in the two incentive arms reported in Table 2.

present results for the other outcomes, correcting for differences in survey attrition. More precisely, in Table A-4, 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-1. 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 equal to either the value of the outcome at baseline, where available, or to an available proxy of the outcome at baseline, when the outcome was not measured at baseline, but 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-5 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 whatever the number of girls interviewed in the household survey or observed during the spot checks. 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 III-A. It is therefore less clear how the weighting in these individual-level estimates should be interpreted. Results—shown in Table A-6—are however largely unchanged.

In Appendix D, we report on further robustness checks in which we show that our results are not driven by selection of girls through school switches (Table

A-7), that they are robust to trimming the school sample of the 5% largest and smallest schools (Table A-8), and to excluding spot check data where conflict caused substantial disruptions to data collection (Table A-9). 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 thus confirming the inconclusiveness of our findings for early marriage and self-reported empowerment (Table A-10).

VI- **Discussion**

Intriguingly, 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 test scores. We therefore replicate, in a unified setting, the findings from two separate strands of literature, namely we replicate (1) the common finding that CCTs improve attendance but do not significantly improve learning, and (2) previous results showing that giving parents more information about child attendance at school improves both attendance and test scores. In addition, we offer the first evidence of the positive effects on both attendance and test scores of incentivizing only children for attendance (rather than incentivizing their parents or both parents and children for attendance or incentivizing children for test performance).

While the confidence interval of the CCT effect includes non-negligible effects on learning, we can reject that its effect was as large as that of the other two treatments. Put differently, we find that the information component of the CCT could have a significantly positive effect on learning were it not for the cash incentive to **parents** specifically, and in that sense the parental cash incentive attenuates the effect on learning. Figure A-1 depicts the effect of each treatment on the share of girls obtaining each possible math score. It shows that, while the information and girl vouchers treatments tend to shift the whole distribution to the right, the CCT shifts the distribution towards both tails (and leads to smaller gains in the upper tail). While the data we collected does not allow us to pinpoint exactly the mechanism(s) driving the contrast in our findings on attendance and on test scores for CCTs only, our experimental design and results allow us to largely rule out several explanations which have been suggested as plausible reasons for CCTs effective in raising attendance not having significant effects on learning. We rule out explanations based on selection into school since we carried out the math test as part of the household survey, irrespective of school attendance. We can also 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 larger gains in learning in the two other treatment arms, and in particular in the treatment arm with the smallest gains in attendance (information only). Similarly, we can rule out the argument that school quality worsens due to the increase in school attendance since we observe gains in learning in the two other treatment arms, and in particular in the treatment arm with the largest gains in attendance (girl incentives). We can also largely rule out an explanation based on the idea that parents are multitasking agents who, when incentivized for attendance, neglect their other educational tasks, since the CCT does not produce substantially larger effects on attendance than the information arm. 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 includes the information treatment—which significantly improves learning–-and does not induce many more children to attend school relative to only giving parents weekly attendance information. The "margins" through which both treatments operate should therefore largely overlap. However, we cannot reject that the CCT had a positive effect on learning—albeit of a smaller magnitude than that of the other two treatments, so that even small differences in the composition of the marginal school attendant could account for the smaller effect of the CCT on learning relative to the other treatments. While we lack power to detect differences in the response to our treatments between different population subgroups, for which our experiment was not designed, estimating Equation (4) for different subgroups provides suggestive evidence of heterogeneity in a direction consistent with expectations (Table A-11). 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 girl vouchers 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).

One of the few exceptions in the CCT evaluation literature finding positive effects on both enrollment and learning is Baird et al. (2011). Interestingly, in a companion paper, Baird et al. (2013) find that, while eligibility for cash transfers (be it conditional or unconditional) reduces adolescent psychological distress at the extensive margin, as the monetary value of the conditional transfers increases, psychological distress increases. Parents may, for instance, adopt stricter parenting approaches to increase attendance, but only do so as the value of the transfers becomes sufficiently large. Similarly, here we find that it is only when parents stand to lose cash transfers from the child's low attendance that increases in attendance do not translate into gains in test scores.

This suggests a possible additional mechanism distinct from the composition of marginal attendants to account for our results and, in particular, for the near-zero point estimate of the effect of the CCT treatment on test scores. One way in which the same parents could induce an increase in attendance in both the information treatment and the parents' incentive treatment through strategies leading to differential consequences on test scores is through a change in parenting strategies. When choosing a parenting style, parents can be thought of as not only choosing the level of their own direct investments in the child's skills production function, but more crucially as influencing the child's own investments in this production function (Doepke et al., 2019). A key insight from recent work in this area is that the costs and benefits of each parenting style are influenced by the set of constraints parents face, so that parents change their parenting style in response to changes in economic conditions and when confronted with different skill accumulation technologies (Doepke and Zilibotti, 2017, Doepke et al., 2019).

In particular, financial incentives for attendance may make it optimal for some parents to not only reward children for attendance but also start nagging or punishing their children when they receive the signal that the child did not attend school (s_n=0), despite knowing that this approach might demotivate the child and thus diminish the effect on learning. The difference in the estimated effects of the information only and CCT treatments on household expenditure on the girls' non-food private consumption is statistically insignificant (p-value: 0.174, Table A-2, Col. (2)). But the direction of this difference is consistent with this parenting mechanism, since the effect of the information treatment corresponds to a 6% increase in the consumption of non-food private goods, but that of the CCT treatment corresponds to a 5% **decrease** in the consumption of this type of goods despite the increase in household income.[31,32] The differences in the effect of the CCT and other two treatments on the distribution of the math test scores are also consistent with the parenting explanation (Figure A-1). One implication of this mechanism is indeed that some parents—those closer to indifference between using a punishment strategy or not in the absence of financial incentives—would switch to using this parenting strategy. This could account for both the smaller gains in the top half of the distribution of test scores in the CCT arm as well as the increase in the share of girls found at the bottom of the distribution.

All in all, our results show that CCTs can exhibit their commonly observed "attendance without significant learning" pattern for reasons other than test sample selection and school quality concerns, and probably too for reasons other than intrinsic motivation crowding out and multitasking incentive problems. Part of the difference in the effect of our treatments on learning may be due to some heterogeneity in the marginal children being induced to attend school through our different treatments, and hence the possibility that financial incentives induce children less willing or able to learn to attend. We also find some support in our data for a new mechanism for the poor performance of CCTs on learning working through changes in parenting strategies, suggesting a fruitful avenue for future work.

VII- **Conclusion**

Regular school attendance is widely believed to be important to support sustained learning (Aucejo and Foy Romano, 2016; Robinson et al., 2017). Child absenteeism is therefore understood to be detrimental both from the pupil's point of view and from the point of view of the efficient functioning of education systems, which motivates governments around the world to incentivize parents to ensure regular school attendance through fines, truancy laws, and, in many developing countries, through CCT programs. In the

[31] Contrary to what would be predicted if the cash transfer was unconditional (Weinberg 2001).

[32] It is more difficult to infer the parents' response to the information component of the girl voucher treatment from changes in the consumption of non-food private goods in this treatment arm due to the substitutability between the goods available for purchase with the vouchers and non-food private goods not purchased using the vouchers.

presence of information frictions between parents and children, however, (i) simply providing additional information to the parents about their child's attendance may increase attendance at a relatively low cost, so that part of the effect of CCTs may come from the information value of the conditional transfer, and (ii) incentivizing children may be more effective than incentivizing parents.

We find evidence that providing high-frequency information to parents about their daughter's school attendance increases school attendance, and that this effect is not statistically distinguishable from that of conditional transfers to parents providing the same information. Incentivizing girls with vouchers for them to buy a choice of goods is at least as effective as incentivizing parents with the cash-equivalent of these vouchers—and importantly, **not** because parents were able to appropriate the vouchers or reallocate household expenditure to redirect transfers from children to 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, since the cost of increasing attendance by one percent is roughly eight times (twice) lower in the information (girl vouchers) arm than in the CCT arm. 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 (forthcoming), Bergman and Chan (2017), Berlinski et al. (2017), 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 in light of 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).

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Tables

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

	Control	Girl Voucher	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

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**
	(2.00)	(0.44)	(2.01)	(2.04)	(0.30)	(2.14)
Parent Cash	0.0599***	0.0272*	0.0202	0.0588**	0.0196	-0.00233
	(2.70)	(1.84)	(0.23)	(2.49)	(1.25)	(-0.03)
Girl Voucher	0.0829***	-0.00331	0.203**	0.0841***	-0.00731	0.178*
	(3.74)	(-0.22)	(2.27)	(3.53)	(-0.46)	(1.97)
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)	0.1283	0.0870	0.5671	0.1283	0.0870	0.5671
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: 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. T-statistics in parentheses, * $p<0.10$ ** $p<0.05$ *** $p<0.01$.

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	0.0305	-0.0174*	0.00937	-0.0209	0.0428	-0.0127	-0.00111	-0.0218
	(1.17)	(-1.73)	(0.36)	(-0.54)	(1.53)	(-1.19)	(-0.04)	(-0.55)
Parent Cash	0.0258	-0.00956	0.0319	0.00203	0.0241	-0.00958	0.0312	-0.00227
	(1.01)	(-0.97)	(1.25)	(0.05)	(0.88)	(-0.91)	(1.20)	(-0.06)
Girl Voucher	0.00739	-0.00401	0.00664	-0.0356	0.0168	-0.000814	-0.00405	-0.0342
	(0.29)	(-0.41)	(0.26)	(-0.94)	(0.61)	(-0.08)	(-0.15)	(-0.87)
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)	0.1529	0.0476	0.1667	0.2283	0.1529	0.0476	0.1667	0.2283
p info=parents	0.856	0.440	0.387	0.553	0.509	0.776	0.230	0.628
p info=girls	0.377	0.187	0.916	0.703	0.355	0.271	0.912	0.756
p girls=parents	0.471	0.572	0.320	0.318	0.790	0.403	0.175	0.412

Source: 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. T-statistics in parentheses, * $p<0.10$ ** $p<0.05$ *** $p<0.01$.

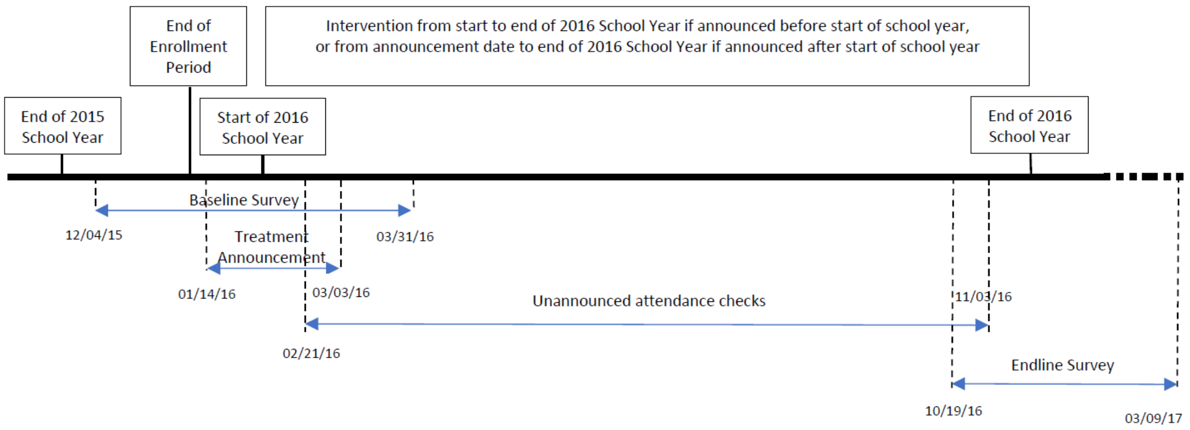
Table 4: Quality of Monitoring across Treatment Arms

	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***	0.0229***	0.0325***	0.00839	0.0200***	0.0228***	0.0317***
	(1.19)	(3.60)	(3.67)	(7.32)	(1.19)	(3.39)	(3.74)	(7.21)
Parent-reported missed school days in October 2015					0.0158	0.00114	-0.000604	0.0187*
					(1.60)	(0.12)	(-0.04)	(1.92)
Observations	473	406	428	482	458	391	416	469

Source: 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. T-statistics based on standard errors clustered at the school level in parentheses, * $p<0.10$ ** $p<0.05$ *** $p<0.01$

Figure

Figure 1: Timeline of Intervention and Data Collection



Appendix For Online Publication

A. Appendix Tables

Table A-1: Descriptive Statistics and Balance at Baseline

	(1) Control Mean	(2) Information Mean	(3) Parent Cash Mean	(4) Girl Voucher 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. *, ** 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-2: Effect of Treatments on Eligible Girls' Consumption of Personal Items

	Dependent Variable: Consumption of Personal Items Not Purchased With Girl Vouchers (meticais)	
	(1)	(2)
	All observations	Top 1% removed
Information	19.55	47.52
	(0.27)	(0.73)
Parent Cash	-50.08	-41.66
	(-0.70)	(-0.65)
Girl Voucher	-68.40	-89.18
	(-0.95)	(-1.39)
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: household survey (endline). 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…).

Table A-3: Individual and Joint Tests 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	0.043	0.006	0.001	0.004		
	No	Share self-reported enrollment	0.633	0.067	0.824	0.161		
	No	Average ASER score	0.047	0.819	0.032	0.050		
	Yes	Share present at spot check	0.037	0.014	0.001	0.005		
	Yes	Share self-reported enrollment	0.744	0.215	0.652	0.365		
	Yes	Average ASER score	0.036	0.982	0.053	0.041	0.034	
Table 3	No	Class teacher presence rate	0.232	0.314	0.768	0.612		
	No	Share ever married	0.073	0.341	0.657	0.348		
	No	Share high self-reported monitoring quality	0.722	0.216	0.803	0.639		
	No	Share high self-reported empowerment	0.571	0.963	0.352	0.709		
	Yes	Class teacher presence rate	0.134	0.385	0.545	0.513		
	Yes	Share ever married	0.221	0.362	0.923	0.551		
	Yes	Share high self-reported monitoring quality	0.965	0.234	0.862	0.502		
	Yes	Share high self-reported empowerment	0.583	0.960	0.386	0.782	0.491	0.085

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-4: 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
	(1.79)	(0.00)	(-0.75)	(-0.50)	(-0.65)
Parent Cash	0.0306	0.0319**	-0.00221	0.0270	-0.00339
	(0.32)	(2.02)	(-0.21)	(0.94)	(-0.08)
Girl Voucher	0.191**	-0.00797	0.0115	-0.00970	-0.0430
	(2.03)	(-0.50)	(1.11)	(-0.34)	(-1.04)
Panel B: Controlling for baseline characteristics					
Information	0.183*	0.000310	-0.00424	-0.0297	-0.0276
	(1.87)	(0.02)	(-0.38)	(-0.98)	(-0.63)
Parent Cash	0.0182	0.0256	0.00179	0.0235	-0.00567
	(0.19)	(1.52)	(0.16)	(0.78)	(-0.13)
Girl Voucher	0.168*	-0.00792	0.0156	-0.0238	-0.0411
	(1.76)	(-0.47)	(1.42)	(-0.80)	(-0.96)
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. 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-1. 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. T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table A-5: ANCOVA Estimates

	(1)	(2)	(3)	(4)	(5)
	Share present at spot check	Share self-reported enrollment	Share ever married	Share high self-reported monitoring quality	Share high self-reported empowerment
Information	0.0431*	0.00204	-0.00623	0.0121	-0.0198
	(1.91)	(0.14)	(-1.49)	(0.47)	(-0.52)
Parent Cash	0.0559**	0.0231	-0.000547	0.0357	-0.000994
	(2.48)	(1.63)	(-0.13)	(1.39)	(-0.03)
Girl Voucher	0.0778***	-0.00160	-0.000183	0.00860	-0.0341
	(3.43)	(-0.11)	(-0.04)	(0.34)	(-0.90)
Parent-reported missed school days at baseline	-0.0101				
	(-1.02)				
Baseline outcome		0.420***			
		(4.01)			
Baseline outcome			1.073***		
			(27.60)		
Baseline outcome				-0.0848	
				(-0.98)	
Baseline outcome					-0.0552
					(-0.72)
Observations	173	173	173	173	173

Source: 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. T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table A-6: 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
	(1.98)	(0.43)	(2.06)	(0.98)	(-2.15)	(-0.34)	(-0.28)
Parent Cash	0.0513***	0.0209*	0.00358	0.0160	-0.0152*	0.0134	-0.000725
	(2.68)	(1.72)	(0.04)	(0.84)	(-1.94)	(0.73)	(-0.02)
Girl Voucher	0.0597***	-0.0110	0.210***	0.0133	-0.0128	-0.0178	-0.0352
	(3.68)	(-0.75)	(2.88)	(0.72)	(-1.62)	(-0.86)	(-1.09)
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	0.4682	0.2165	1.0833	0.2734	0.1694	0.2924	0.4470
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: 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 t-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table A-7: 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*
	(1.84)	(1.88)
Parent Cash	0.0604***	0.0592**
	(2.69)	(2.47)
Girl Voucher	0.0810***	0.0823***
	(3.60)	(3.41)
Baseline Characteristics	No	Yes
Observations	173	173
Mean Y	0.65	0.65
SD Y	0.1281	0.1281
p info=parents	0.421	0.581
p info=girls	0.090	0.135
p girls=parents	0.359	0.333

Sources: 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. T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table A-8: 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***	0.0427	-0.0153	0.0332	-0.0437
	(2.12)	(0.41)	(2.73)	(1.51)	(-1.48)	(1.22)	(-1.08)
Parent Cash	0.0694***	0.0254*	0.0377	0.0266	-0.0100	0.0354	-0.0131
	(3.04)	(1.67)	(0.42)	(0.97)	(-1.00)	(1.34)	(-0.33)
Girl Voucher	0.0860***	-0.00812	0.250***	0.00724	-0.000850	0.00921	-0.0363
	(3.79)	(-0.54)	(2.81)	(0.27)	(-0.09)	(0.35)	(-0.93)
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	0.1280	0.0843	0.5644	0.1539	0.0446	0.1686	0.2310
p info=parents	0.420	0.238	0.026	0.580	0.620	0.937	0.463
p info=girls	0.135	0.366	0.984	0.222	0.173	0.391	0.859
p girls=parents	0.476	0.031	0.021	0.488	0.369	0.330	0.561

Source: 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. T-statistics in parentheses, * $p<0.10$ ** $p<0.05$ *** $p<0.01$.

Table A-9: Excluding Data Affected by Conflict

	(1) Share present at spot check	(4) Class teacher presence rate
Information	0.0379*	0.0300
	(1.73)	(1.17)
Parent Cash	0.0546**	0.0332
	(2.52)	(1.32)
Girl Voucher	0.0718***	0.0117
	(3.32)	(0.47)
Observations	173	173
Mean Y	0.65	0.91
SD Y	0.1283	0.1613
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. 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. T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table A-10: 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, girl vouchers, 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.

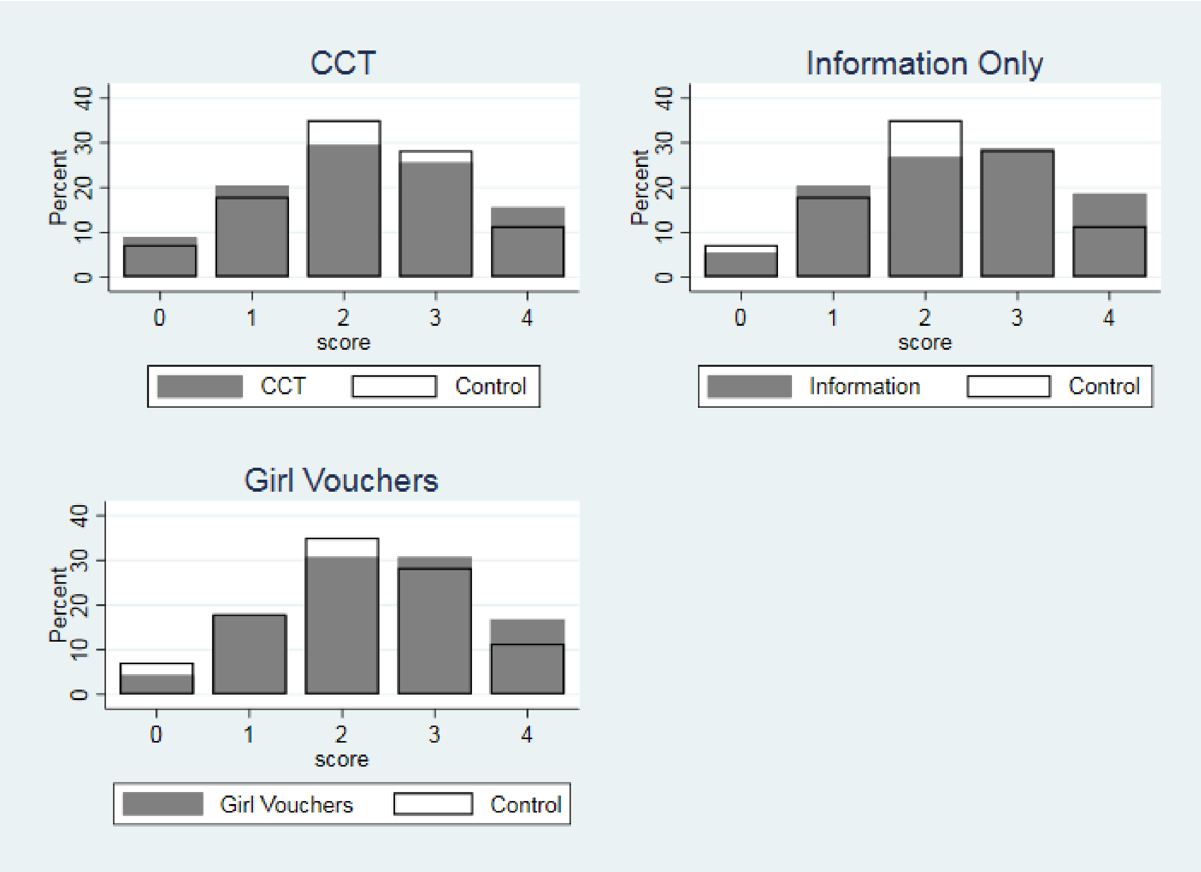
Table A-11: Effect on Attendance by Selected School Population Characteristics

	(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**
	(1.93)		(1.74)		(2.48)
Information × Poorest	-0.0121	Information × Oldest	0.0247	Information × Furthest	0.0369
	(-0.28)		(0.55)		(0.69)
Parent Cash	0.0496	Parent Cash	0.0637**	Parent Cash	0.0480**
	(1.65)		(2.42)		(2.08)
Parent Cash × Poorest	0.0435	Parent Cash × Oldest	0.00589	Parent Cash × Furthest	0.0547
	(0.96)		(0.11)		(0.89)
Girl Voucher	0.0966***	Girl Voucher	0.0766**	Girl Voucher	0.0961***
	(3.35)		(2.46)		(3.65)
Girl Voucher × Poorest	-0.0408	Girl Voucher × Oldest	0.0541	Girl Voucher × Furthest	0.0140
	(-0.76)		(0.96)		(0.22)
Poorest	0.0320	Oldest	-0.0793	Furthest	-0.0184
	(0.51)		(-1.18)		(-0.18)
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: unannounced spot checks attendance data (for outcome variable) and household survey (variables interacted with the treatment indicators). "Poorest", "Oldest" and "Furthest" 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). T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

B. Appendix Figures

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



Source: Endline household survey.

C. 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 girl voucher 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.

D. Further Robustness Checks

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).[33] 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-7. 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-8, 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 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. At peak conflict times in the most affected district (Mossurize), however, many schools were closed so that attendance data

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

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-9 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. 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.

Ex-post power calculations. In Table A-10, 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 distribution of school-level averages.[34] 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.

[34] 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.