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

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Abstract:

If parents do not perfectly observe whether their children attend school, (i) increasing parental information may increase attendance, (ii) cash transfers conditional on an attendance target may increase school attendance in part through an information effect and (iii) incentivizing children directly could be more cost effective than incentivizing parents. We isolate experimentally the information effect of a conditional cash transfer and find that it is large. Providing information without transfers increases attendance and improves parental monitoring. Incentivizing children is at least as effective as incentivizing parents—importantly, not because parents were able to appropriate conditional transfers made to children.

JEL Codes: I25, D82, N37

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I- **Introduction**

Regular school attendance supports sustained learning and is associated with a reduced risk of dropping out in both developed- (Aucejo and Foy Romano, 2016; Robinson et al., 2018) and developing countries (Bedi and Marshall, 1999; Manacorda 2012; Glewwe et al. 2014; Bassi et al. 2020). The design of effective policies to reduce pupil absenteeism however depends on who has agency over the attendance decision. While there are settings where children, and girls in particular, are unlikely to have much say in the decision to attend school (e.g., in very socially conservative contexts), in many cases 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 (Bursztyn and Coffman, 2012; Heckman and Mosso, 2014). This would have important implications for education policies. First, simply improving parental information about the child's effort could be effective in raising human capital investments. Second, the design of conditional cash transfers (CCT), now implemented in over 60 countries (Parker and Todd, 2017), could be improved by increasing the attendance information fed back to parents. Third, 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]

We show, through a randomized controlled trial, that the Mozambican teenage girls in our study have agency over school attendance decisions due to imperfect monitoring by parents. In addition, we test side-by-side the effectiveness of two alternatives to traditional CCTs, which are suitable to such settings, and find them to be effective in increasing attendance at low cost.

More precisely, we present experimental evidence of the effect of three alternative policies targeting Mozambican girls in the last two grades of primary school: (1) providing information to parents about their child's attendance through a weekly attendance report card ("information only" treatment); (2)

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[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.

providing this information and making cash transfers to parents conditional on regular attendance (CCT or "parent incentive" treatment); or (3) providing the same weekly attendance information and making transfers to children of the same nominal value as in (2) in the form of a voucher, also conditional on regular attendance ("child incentive" treatment).

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

Our second key finding is that the effect of the information treatment is large relative to the overall effect of a conditional transfer program providing the same information. In our experiment, where the value of the transfer is modest but similar to safety net programs found elsewhere (7% of per capita GDP),[3] the estimated effect of the information treatment on attendance is as large as 75% of the effect of the CCT treatment (4.5 and 6 percentage points, respectively). It also compares favorably with the effect of other non-monetary interventions on pupil absenteeism.[4] If the information- and financial incentive effects of the CCT are additive, this implies a small effect of financial incentives. To test whether these effects are additive, one would need an additional treatment arm in which parents are financially incentivized for the child to meet the attendance

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

target but do not learn anything about the child's attendance. This treatment is however not feasible. Unless the conditionality is not enforced—in which case the CCT is *de facto* an un-conditional cash transfer, there is no such thing as a CCT conditional on school attendance in which parents are not fed back information about the child's attendance. Receiving or not receiving the conditional transfer indeed conveys information to parents about whether the child attended school regularly (Bursztyn and Coffman, 2012).[5]

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

Taken together, these findings raise the possibility of increasing attendance at low cost: the annual cost of increasing attendance by one percent is $2.68 in our parent incentive arm, compared to only $1.57 for the child incentive arm and as little as $0.33 in the information arm.[6]

We also find that both the information treatment and the children's incentives treatment significantly improve scores on the measure of learning that we collected (the ASER or "Annual Status of Education" math test) by 8.5 to 9.4% of the control group's mean. Consistent with most evaluations of CCTs, the effect of the parent incentives treatment is not statistically significant, although its confidence interval includes meaningful gains in learning.[7] Further investigation (Section VI.C) indicates that the learning gains in the parent incentive arm are more concentrated at the top of the distribution than in the other arms, which might be explained by suggestive evidence that the population of children induced to attend school more as a result of the treatment (compliers) differs across treatment arms.

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

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

[7] 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).

We make three main contributions to the literature. For the first time, we compare experimentally the effect of an attendance information treatment with that of a CCT (containing the same information). Bursztyn and Coffman (2012) show that parents in a poor urban Brazilian setting value the information component of a national CCT. In a lab experiment, they find that most parents only prefer an unconditional cash transfer (UCT) to a CCT of similar nominal value if, before being asked to choose between UCT and CCT, they are first offered free text messages notifying them of their child's absences. This groundbreaking study however leaves unanswered the important question of how much of the overall effect of a CCT can be obtained simply by giving parents the information contained in this CCT, which we answer here.

Second, we provide novel evidence that, in a poor, predominantly rural country setting, providing more information to parents about child attendance increases attendance *even in the absence of financial transfers*. Recent field experiments abstracting from the question of social transfers have found that improving the information parents receive about attendance (Berlinski et al., 2021; Rogers and Feller, 2018) and other measures of student effort at school (Bergman, 2021; Bergman and Chan, 2021; Cunha et al., 2017) in urban, middle- to high-income country settings increases attendance.[8,9] Our work is however the first to estimate the effect of only giving parents information on attendance in a rural, low-income setting, where the need to improve education levels is the most pressing, children may be expected to have less agency (Galiani et al. 2017), and the financial ability of parents to incentivize children to attend school in the absence of cash transfers is the most limited.

[8] 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.

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

The third key contribution of our study is to provide new evidence on the relative 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.[10] Three studies compare experimentally the effect of incentivizing parents relative to incentivizing children to achieve an attendance- (Baird et al., 2011), grade- (Berry, 2015), or combined grade and attendance target driven mainly by grades (Levitt et al., 2016b).[11] None of them finds significant differences, on average, between incentivizing parents and children. Parents can have a direct input into the production of grades, so that comparing the effectiveness of incentivizing parents or children to achieve a certain *grade* target combines their relative influence on schooling decisions (agency) and their relative efficiency in producing grades. For instance, Berry (2015) finds heterogeneous effects consistent with a model in which it is more effective to incentivize parents (children) when the parents' (children's) input is relatively more productive. By comparing how responses to incentivizing a simple child *input* such as attendance vary with the recipient of the incentive (parents or children), we speak more directly to the agency of children in decisions regarding their schooling.[12] Unlike the only other previous study incentivizing parents and children to achieve an *attendance*- rather than a grade 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 to ensure that children are the end recipient of the incentive intended to them.[13] 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

[10] See Appendix C2 for literature concerned with incentivizing children specifically.

[11] 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.

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

[13] 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.

else, and that parents did not appropriate indirectly the transfers by reducing the child's non-food private consumption.[14]

The amount of information embedded in CCTs found around the world varies through differences both in payment frequency and enforcement of conditions (Fiszbein and Schady, 2009; Baird et al., 2014).[15] Our aim is not to identify the effect of the information component of an elusive "average" CCT. Instead, we identify the effect of providing only high-frequency information using an easily scalable technology relative to that of implementing a well-enforced CCT incorporating the same information.

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 explores the mechanisms behind our results. Section VII concludes.

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. The country's recent history has been marked by a 15-year civil war following independence in the 1970s, and occasional clashes between armed forces and RENAMO's armed militias in the center of the country.

Despite large increases in enrollment rates in lower primary school grades, most children are still not completing primary education. The net intake at Grade 1 of primary schooling is high for both boys (74.5%) and girls (73.1%). But most children in Mozambique, and girls in particular, experience difficulties

[14] 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).

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

in completing primary school.[16] As of 2017, the survival rate to the last grade of primary school was only 41% for boys and 37% for girls (UNESCO Institute for Statistics), justifying our focus on girls in upper primary schooling (Grades 6 and 7 or Ensino Primário de Segundo Grau "EP2").

While high teacher absenteeism is a well-known issue across the developing world in general and in Mozambique in particular, the latest World Bank's Service Delivery Indicators for Mozambique indicate that pupil absenteeism is much higher (45.8% pupil absenteeism versus 29.8% teacher absenteeism in 2018) and that pupil absenteeism is the strongest predictor of student test scores among a wide range of school, teacher and pupil-level characteristics (Bassi et al. 2020).

We focused on 173 co-educational schools comprising over 16,000 EP2 female students in one province of Mozambique where our implementation partner—the development NGO Magariro—is active and well-known: Manica. Manica Province is located in the Center Region of Mozambique, it is home to 7.5% of the country's population, and is close to the national average on a number of indicators.[17]

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

B. Theoretical Motivation

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

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

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

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. Conditional transfers may lead to larger increases in school attendance than unconditional ones if individuals underestimate returns to education. In addition, conditional transfers may have larger effects because they help parents monitor their children's behavior (Bursztyn and Coffman, 2012).[18]

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.[19] Bursztyn 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:[20]

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

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

[18] For a more comprehensive discussion of arguments in favor of *conditional* transfers, see Martinelli and Parker (2003) and Fiszbein and Schady (2009).

[19] A number of models of parent-children interactions in which children do not have private information about their schooling effort have been proposed. An excellent recent review can be found in Doepke 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.

[20] 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.

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.[21] 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:[22]

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

Where the probability of inequality (3) holding increases with signal quality (higher π). Therefore, under imperfect information, simply providing information to the parent may induce higher attendance. Enforced CCTs give the parent, at a minimum, a binary signal as to whether the child met the attendance requirement upon which payments are conditioned. The conditionality may therefore in itself lead to higher attendance, as pointed out by Bursztyn and Coffman (2012). This motivates our test of whether giving parents information about their child's attendance has an effect on attendance. This also motivates our novel test of the extent to which the effect of giving this information and nothing else differs from that of a CCT program providing the parents with the same information as part of the program.

In addition, we make the new observation that, under imperfect information, incentivizing the child should be more cost-effective than incentivizing the parent because of the informational wedge $\frac{\pi}{2\pi-1}$. 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

[21] 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.

[22] Denote w_n the transfer made by the parent to the child if $e_n = 1$ and $\overline{w_n}$ the minimum payment such that the child's expected payoff is at least as large when $e_n = 1$ than when $e_n = 0$. Condition (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 $w_n \geq 0$.

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.

In this model, we consider the child's agency in deciding the level of a child's educational input (here, attendance) when pay-offs are attached directly to whether the child supplies this input. The parent's role is simply to incentivize or not the child to attend. If instead we considered incentives to achieve a certain outcome (e.g., test scores), then differences between incentivizing parents vs. children would combine the relative influence of parents and children on input decisions and their relative efficiency in producing grades (see Appendix D for a formal illustration).

C. Study Design and Motivating Evidence

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

(i) both parents and girls of this age have an influence on school attendance decisions and
(ii) children have private information on their school attendance.

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

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

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

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

Experimental Groups. Given these insights, we defined the following four experimental groups (as summarized in Panel A of Table 1). In two of the experimental groups, we introduced transfers conditional on achieving at least 90% attendance during the school trimester. In a “child incentives” treatment arm, we gave money-equivalent vouchers at the end of each trimester to girls in Grades 6 and 7 who could then use the vouchers to buy a selected number of items such as: school uniforms, shoes, backpack, smaller materials (soap, toothpaste, pens, notebooks, etc...). These items were delivered at the school by the research team and could be purchased during the research team visit. The choice of items made available was based on the preliminary focus group interviews. Importantly, school uniforms are *de facto* not compulsory in Mozambican primary schools and are only used by a minority of students. The same applies to backpacks, so that the provision of these items does not simply equate to a school expenditure subsidy. Indeed we do not find that parents substitute these items to expenditures on the girls, as discussed in Section VI.B.

In a "parent incentives" arm, we instead gave the same value in cash to the parents and made the same items as in the "child incentive" arm available for *optional* purchase at the school. In both transfers arms, the value of the transfers was 400 meticais[23] per trimester with a maximum of 1,200 meticais over the 2016 school year or about 7% of per capita GDP. It was clearly explained that there was no expectation as to how the parents would spend the money, and the items were available for purchase at a short distance from the desk at which the cash was distributed to avoid pressurizing the parents. As in any comparison between treatments involving different recipients, it is very difficult to equate marginal utilities experimentally. But our design answers the policy question of what recipient to target with a fixed budget since the parent- and child incentives have the same nominal value. To reinforce comparability, the price of items in vouchers also matched their price in Mozambican meticais. By using vouchers rather than cash, we increased social acceptability of the program and, as discussed in Section VI.B, succeeded in ensuring that the end recipient of the conditional transfer was the recipient intended by the study design.[24]

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

[23] 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.

[24] 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.

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

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

To ensure the quality of the data recorded in the attendance report cards, and given the extra work required from the teachers responsible for each class ("directores de turma") to fill out those cards, we introduced a small compensation scheme. The scheme worked as follows. In the three treatment groups, the teachers who, at every spot check by the independent surveyor, were found to have thoroughly filled in all their (female) pupils' report cards for the current trimester until the day of the spot check, received 250 meticais' worth of mobile phone credit ("airtime") at the end of the trimester.[25] The value of 250 meticais corresponds to the opportunity cost of about 5 minutes per day, evaluated at the hourly salary equivalent of the average teacher. The school directors of all schools, including the control group, received 250 meticais in airtime at the end of each trimester without conditions.

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

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

enforced – as is the case in about half the CCT programs reviewed in Baird et al. (2014, see Figure 5). Less than 6% of enrolled girls went to a different school from that from which they were sampled, and school switches are independent of experimental arm (Appendix G). Although it took the experience of the first trimester for teachers to adjust to the high standards required to obtain the airtime reward (at least 80% of reports fully filled and without obvious errors), teachers also largely complied. At the end of the first trimester, only 57% of teachers were rewarded compared to 83% (82%) at the end of the second (third) trimester.[26]

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. Up to twice per trimester, an enumerator arrived unannounced at each school in the sample and recorded in person the individual attendance/absence of every child enrolled in EP2. The attendance rate triggering transfers in the conditional transfers arms was calculated by the implementing NGO solely based on the information contained in the attendance report cards described in Section 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 eligible girl in the household: data on self-reported quality of attendance monitoring by the parent or guardian, degree of agreement about statements regarding returns to education for each child, self-reported girl empowerment, expenditure on 23 personal items consumed by the eligible girl, and an ASER math test (at endline only).

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

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

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

Household survey sample. The household data used in the analysis are based on a sample drawn from the universe of girls enrolled in the 173 schools included in our study within three years of the start of the intervention based on school records, as in Benhassine et al. (2015). More specifically, this included girls who (i) had completed, at least, 5th Grade and, at most, 6th Grade (and were therefore potentially eligible for our treatments implemented in Grades 6 and 7 in 2016), and (ii) still lived with their parent or guardian at baseline (given our analytical framework based on asymmetric information between parents and children).[27] The target was to interview 20 girls per school, sampling those enrolled in 2015 (the last school year before the experiment) and recent dropouts who were not enrolled in 2015 but were enrolled in 2013 or 2014, proportionally to the size of each of the two groups ("enrolled in 2015" and "recent dropouts"). During fieldwork, however, there were difficulties locating the girls listed in the school records, and most of the recent dropouts had either moved away or were not living with their parents anymore and were thus ineligible for interview. The sampling target of 20 per school was therefore not attained in many of the smaller schools, and where possible more than 20 girls were sampled to help preserve power. All in all, the median number of girls surveyed per school in the baseline household survey is 18, and recent dropouts were under-represented in the household survey sample (3% of the baseline sample compared to 13% of the universe). There was no difference in the total number

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

of girls interviewed in the baseline household survey, or the share of recent dropouts in the household survey sample, across treatment arms, however.[28]

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

The official enrollment period ended on January 6, 2016, i.e. at least a week before the initial "announcement" visit by the implementing NGO (which started on January 14 and ended on March 3).[29] 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

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

[29] 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.[30] There is however little reason to believe that it should have affected data on the baseline socioeconomic indicators for which we test balance at baseline, since the interventions were not means-tested, and more generally there was no room to manipulate the eligibility criteria (gender and grade).

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

B. Randomization and Experimental Balance

We first stratified our sample of 173 schools by district. We then split the schools included in our study, within each district, randomly between the four experimental arms (one control and three treatment arms) using a random number generator.[31] At the time of the announcement of the treatments, a human

[30] 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.

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

error led to two schools in the Vanduzi district being swapped. Throughout this paper, we classify each school based on their randomly assigned treatment arm, but our findings are robust to assigning treatment based on actual treatment status instead of intended treatment status.[32]

Table A-2 presents summary statistics for all the socioeconomic indicators measured in the baseline survey and characteristics of the eligible girls and self-reported monitoring technology relevant to our research framework. Stars indicate statistically significant differences between each treatment arm relative to the control group.[33] Table A-2 suggests that the randomization of experimental arms worked well in practice. For each pair of experimental arms, an F-test cannot reject that the baseline characteristics listed in Table A-2 are jointly orthogonal to treatment status. More specifically, the p-value associated with an F-test that the set of characteristics listed in Table A-2 does not explain the experimental arm classification within district is between 0.17 (Information v. Parent) and 0.52 (Control v. Child) for all 6 experimental arm pairs, and thus the null of joint orthogonality cannot be rejected.[34] Some individual differences are, however, statistically significant, and thus we provide robustness checks controlling for these baseline characteristics.[35] Other than for the many language and religion categories, the only other variables with some significant baseline differences are school absences reported by parents (for October 2015) and, to a marginal extent, self-reported quality of child attendance monitoring by the parents. We do not have reliable attendance data with which to compare parent-reported absences prior to the 2016 school year, but we can compare parent-reported absences for October 2016 to our attendance spot check data for the same period in the control group. As we show in Section VI.A, the number of absences reported by parents does *not* predict actual absences in the control group. In addition, the coefficient of correlation between the number of parent-reported absences at baseline and endline is only 0.038 (in the control group). This suggests that the differences in parent-reported absences at baseline do not

2, resulting in 6 "parent cash", 6 "child incentive" schools, 5 control schools and 21-17=4 "information" schools.

[32] Full results available on request.

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

[34] 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.

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

reflect a genuine difference in baseline absenteeism. This is confirmed in our analysis, where we show that our conclusions are robust to controlling for baseline characteristics including parent-reported absences.

IV- **Main Results**

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

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

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

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. This is measured as the share of girls in the targeted grades who were found in their classroom by the independent surveyor during unannounced school visits. The two columns present estimates of Equation (4) with and without controlling for the (school average) baseline characteristics listed in Table A-2 for which a t-test rejects equal means at baseline for at least one treatment arm.

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

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

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

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

B. Effects on Test Scores

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

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

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

however, we only included one cognitive test in our survey, which limits what conclusions we can draw regarding the effect of our treatments on learning.

Both the information treatment and the girls' incentives treatment improve math scores by 8.5 to 9.4% of the control group's mean or .17 and .19 of a standard deviation when considering the distribution of scores across the girls tested in the control group.[37] Gains in attendance from incentives to parents do not translate into significant gains in test scores. Importantly, however, we cannot reject the possibility that the parent incentives treatment has a modest but positive effect on math scores since the p-value of a test that the effect is equal to 0.1 (or about 0.09 of a standard deviation) is above 0.25. In Section VI.C, we summarize our exploratory analysis of the causes behind the heterogeneity in learning effects at the mean across treatment arms.

C. Effects on Pre-specified Non-Schooling Outcomes

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

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

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

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

regressions, and it becomes insignificant when controlling for baseline characteristics (Column (6)).

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

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

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

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

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 (2019) when relying on asymptotic theorems are related to: (i) high-leverage (which only arises with the inclusion of covariates) and (ii) clustered estimates of variance. We were therefore careful to present results that do not include covariates other than district fixed effects or rely on clustered standard errors. An additional issue which we also address using the randomization-based tests proposed by Young (2019) is that of joint testing of multiple hypotheses. In Table A-4, we report estimates based on exact p-values for the sharp null hypothesis of no treatment effect for none of the schools in our sample. More specifically, we report p-values for individual tests of each treatment effect estimate, as well as for joint tests of, respectively, all treatment effects in each equation, all treatment effects in each table, and all treatment effects across both results tables. Differences between exact randomization p-values for individual significance tests and the estimates reported in the main analysis are very small, and the same conclusions (and levels of significance) are obtained. Joint tests confirm the robustness of our findings on schooling outcomes (positive effects on attendance and math score, but not on enrollment) and the absence of treatment effects on the other outcomes studied.

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

Reassuringly, reweighting observations by the inverse of the probability that they attrit does not change our conclusions (Table A-5).

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

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

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

VI- **Mechanisms**

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

A- Effect of our Treatments on Parental Monitoring

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

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

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

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

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

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

B- Alternative Explanations

We interpret the effect of the information treatment on attendance as the effect of improved parental monitoring, as supported by the evidence of the previous

[39] By showing that parental reports of missed school days are improved in all treatment arms, Table 4 also addresses the potential concern that there might be collusion between teachers and parents 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.

subsection. And we interpret the effect of the other treatments as the combined effect of improved parental attendance monitoring and material rewards for the parents (children) in the "parent incentive" ("child incentive") arm.

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

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

Our finding that incentivizing children is at least as effective as incentivizing parents is therefore particularly interesting in the light of evidence that our transfers remained with their intended beneficiaries. First, when asked at endline, no surveyed girl from the child incentive arm responded that she had given away her reward or had had to sell it to give the money to someone else. Second, it could have been the case that parents substituted away from expenditure on private goods consumed by the girl, thus neutralizing the transfer to their daughter. To test for this, we collected detailed information on the girls' consumption of 23 non-food private goods such as clothes, bags, soap, books, etc., excluding any item purchased through a voucher. We find a statistically insignificant effect on consumption of personal items (other than those purchased with the voucher) in the child incentive arm compared to the control group. Unsurprisingly given the substitutability between these goods and those available for purchase through vouchers, the coefficient is negative. But it is insignificant and small relative to the amounts transferred (no more than 89 meticais compared to an average of 469 meticais received in vouchers, see Table A-3).

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

C- Effects on Learning

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

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

We can largely rule out an argument that school quality is so poor that increased attendance through the CCT does not result in improved learning because we observe significant gains in learning in the two other treatment arms. We can also largely rule out an explanation based on the idea that parents are multitasking agents who, when incentivized for attendance, improve attendance at the expense of their other educational tasks. Indeed, the CCT does not produce substantially larger effects on attendance than the information arm. A further candidate explanation would be that material incentives crowd out intrinsic motivation. But we incentivize attendance, not test scores, so it is not clear why crowding out should occur mostly for the outcome we do not incentivize rather than the outcome we do incentivize.

Our CCT treatment may induce children who differ in observable and/or unobservable characteristics to attend school more, and these compliers may also have different returns to attendance in terms of learning as captured by the math ASER test. We do find suggestive evidence of heterogeneity in a direction consistent with expectations (Table A-13). In particular, the point estimate for the effect of the CCT treatment on school attendance is about twice the size in schools in the poorest tercile (Column 1), while the point estimate of the effect of the child incentive treatment on attendance is 70% larger in schools in the top tercile for school share of older girls (Column 3). Similarly, the effect of the information treatment on attendance is two-thirds larger in the top tercile for school share of girls with long journeys to school (Column 5).

We also estimated the effect of our treatments on the ASER score for each of 63 subgroups defined by baseline individual and household characteristics.[40] The parent incentive treatment does not have a statistically significant effect on average ASER scores in any of these subgroups (whereas significant effects are obtained in 35 (47) subgroups for the "information only" ("child incentive") treatment). The parent incentive does, however, significantly increase the probability of obtaining the top score in 30 out of 63 of these subgroups (compared to 45 and 44, respectively, for the two other treatment arms). Figure A-1 confirms graphically that the effects of the three treatments on test scores are more similar at the top of the distribution. While the information only- and child incentive treatments tend to shift the whole distribution to the right, the parent incentive treatment shifts the distribution towards both tails (significantly so in the case of the upper tail, as shown in Table A-14).

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

VII- **Conclusion**

Regular school attendance is widely believed to be important to support sustained learning (Bedi and Marshall, 1999; Manacorda 2012; Glewwe et al. 2014; Aucejo and Foy Romano, 2016; Robinson et al., 2018; Bassi et al. 2020). Child absenteeism is therefore detrimental both from the pupil's point of view and from the point of view of the efficient functioning of education systems.

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

This 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 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 (ii) part of the effect of CCTs may come from the information value of the conditional transfer, and (iii) 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 in a low-income Sub-Saharan Africa setting even in the absence of transfers, and that this effect is not statistically distinguishable from that of conditional transfers to parents providing the same information. Incentivizing girls is at least as effective as incentivizing parents.

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

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

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

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Tables

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

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

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)
Child Incentive	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. “p arm_i=arm_j” denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table 3: Effect on Non-Schooling Outcomes

	(1) Class teacher presence rate	(2) Share ever married	(3) Share high self-reported monitoring quality	(4) Share high self-reported empowerment	(5) Class teacher presence rate	(6) Share ever married	(7) Share high self-reported monitoring quality	(8) Share high self-reported empowerment
Information	0.0305	-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)
Child Incentive	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. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

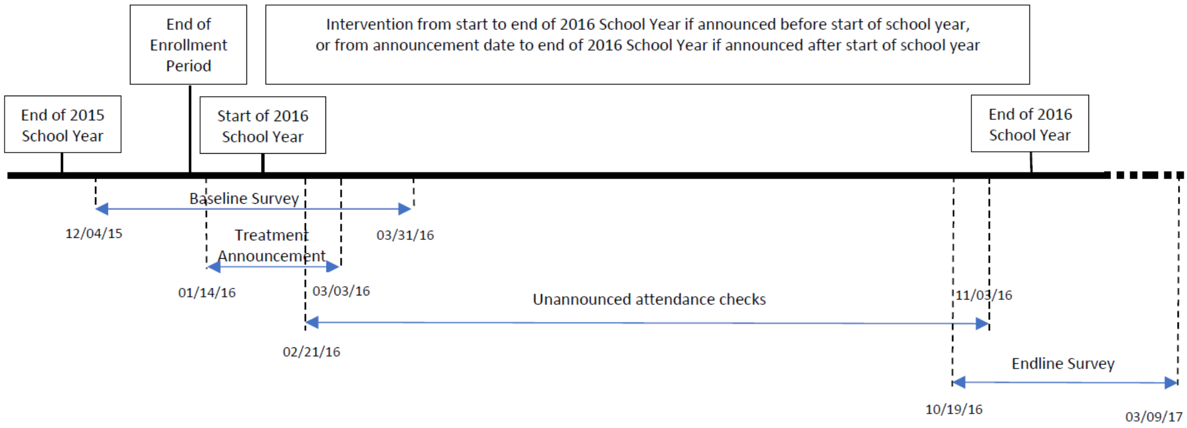
Table 4: Quality of Monitoring across Treatment Arms

	Outcome: absent at attendance check between 10 October and 3rd November 2016							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Experimental arm:	Control	Info	Girls	Parents	Control	Info	Girls	Parents
Parent-reported missed school days in October 2016	0.00868	0.0207***	0.0229***	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: Sample Comparison with Representative Samples

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

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

Table A-2: Descriptive Statistics and Balance at Baseline

	(1) Control Mean	(2) Information Mean	(3) Parent Cash Mean	(4) Child Incentive Mean
Household Head:				
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
Household wealth[1]:				
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
Language:				
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
Religion:				
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
Girl Characteristics:				
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-3: Effect of Treatments on Eligible Girls' Consumption of Personal Items

	Dependent Variable: Consumption of Personal Items Not Purchased With 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)
Child Incentive	-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…). T-statistics based on standard errors clustered at the school level in parentheses, * p<0.10 ** p<0.05 *** p<0.01. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-4: 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-5: Inverse Probability Weighting Attrition Correction

	(1)	(2)	(3)	(4)	(5)
	Average ASER math score	Share self-reported enrollment	Share ever married	Share high self-reported monitoring quality	Share high self-reported empowerment
Panel A: No controls for baseline characteristics					
Information	0.171*	0.0000403	-0.00792	-0.0146	-0.0273
	(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)
Child Incentive	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)
Child Incentive	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-2. Regressions in Panel B also include school sample averages for the following baseline characteristics: self-reported (by parents) number of missed school days in October 2015 among girls enrolled, binary indicator for high self-reported monitoring quality, five language indicators and five religion indicators. All regressions include a constant and district fixed effects. The attrition rate varies across dependent variables due to non-response at the math test and empowerment questions. The p-values reported in the last row correspond to an F-test of joint significance of the treatment variables in a regression of the school-level attrition rate on the three treatment indicators and district fixed effects. T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table A-6: ANCOVA Estimates

	(1) Share present at spot check	(2) Share self-reported enrollment	(3) Share ever married	(4) Share high self-reported monitoring quality	(5) Share high self-reported empowerment
Information	0.0431*	0.00204	-0.00623	0.0121	-0.0198
	(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)
Child Incentive	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
p info=parents	0.576	0.145	0.177	0.365	0.630
p info=girls	0.131	0.802	0.151	0.893	0.712
p girls=parents	0.321	0.082	0.929	0.287	0.386

Source: 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. “p arm_i=arm_j” denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-7: Individual-Level Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	=1 if present at spot check	=1 if self-reported enrollment	ASER score	=1 if Class teacher present	=1 if Ever married	=1 if High self-reported monitoring quality	=1 if High self-reported empowerment
Information	0.0418**	0.00611	0.166**	0.0210	-0.0169**	-0.00664	-0.00861
	(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)
Child Incentive	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. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

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

	(1) Share present at attendance check	(2) Share present at attendance check
Information	0.0419*	0.0455*
	(1.84)	(1.88)
Parent Cash	0.0604***	0.0592**
	(2.69)	(2.47)
Child Incentive	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. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share present at spot check	Share self-reported enrollment	Average ASER score	Class teacher presence rate	Share ever married	Share high self-reported monitoring quality	Share high self-reported empowerment
Information	0.0498**	0.00634	0.252***	0.0427	-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)
Child Incentive	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. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-10: Excluding Data Affected by Conflict

	(1) Share present at spot check	(4) Class teacher presence rate
Information	0.0379*	0.0300
	(1.73)	(1.17)
Parent Cash	0.0546**	0.0332
	(2.52)	(1.32)
Child Incentive	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. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share present at spot check	Share self-reported enrollment	Average ASER score	Class teacher presence rate	Share ever married	Share high self-reported monitoring quality	Share high self-reported empowerment
Information	0.0395*	-0.00799	0.202**	0.0350	-0.00568	0.0101	-0.0147
	(1.74)	(-0.56)	(2.25)	(1.28)	(-1.32)	(0.38)	(-0.36)
Parent Cash	0.0647***	0.0140	0.0127	0.0190	-0.00263	0.0463*	0.00416
	(2.83)	(0.97)	(0.14)	(0.69)	(-0.61)	(1.74)	(0.10)
Child Incentive	0.0790***	-0.00989	0.195**	0.00218	-0.000157	0.00417	-0.0299
	(3.47)	(-0.69)	(2.17)	(0.08)	(-0.04)	(0.16)	(-0.74)
Observations	173	173	173	173	173	173	173
Mean Y	0.65	0.95	2.16	0.90	0.03	0.89	0.30
SD Y	0.1283	0.0870	0.5671	0.1529	0.0476	0.1667	0.2283
p info=parents	0.285	0.139	0.043	0.574	0.494	0.186	0.653
p info=girls	0.089	0.896	0.940	0.242	0.209	0.826	0.712
p girls=parents	0.535	0.101	0.046	0.546	0.571	0.117	0.408

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). All regressions include a constant, district fixed effects, and any baseline variable which has a t-statistic equal to 1.96 or above when regressing the outcome on district fixed effects and the baseline characteristics reported in Table A-2 in the control group. T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01. "p arm_i=arm_j" denotes the p-value of a test of equal treatment effects between treatment arm i and treatment arm j.

Table A-12: Ex-Post Power Calculations

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

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

Table A-13: 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)
Child Incentive	0.0966***	Child Incentive	0.0766**	Child Incentive	0.0961***
	(3.35)		(2.46)		(3.65)
Child Incentive × Poorest	-0.0408	Child Incentive × Oldest	0.0541	Child Incentive × 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 "Farthest" are indicator variables equal to one if the school's share of girls surveyed at baseline that are classified as "poor", "old", and "far from school", respectively, is in the top tercile of the school distribution. "Poor" refers to girls in the lowest household wealth tercile, "old" refers to girls in the highest individual tercile for age (14 and above at baseline) and "far from school" refers to girls in the highest individual tercile for time taken to travel to school (33 minutes and above). T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

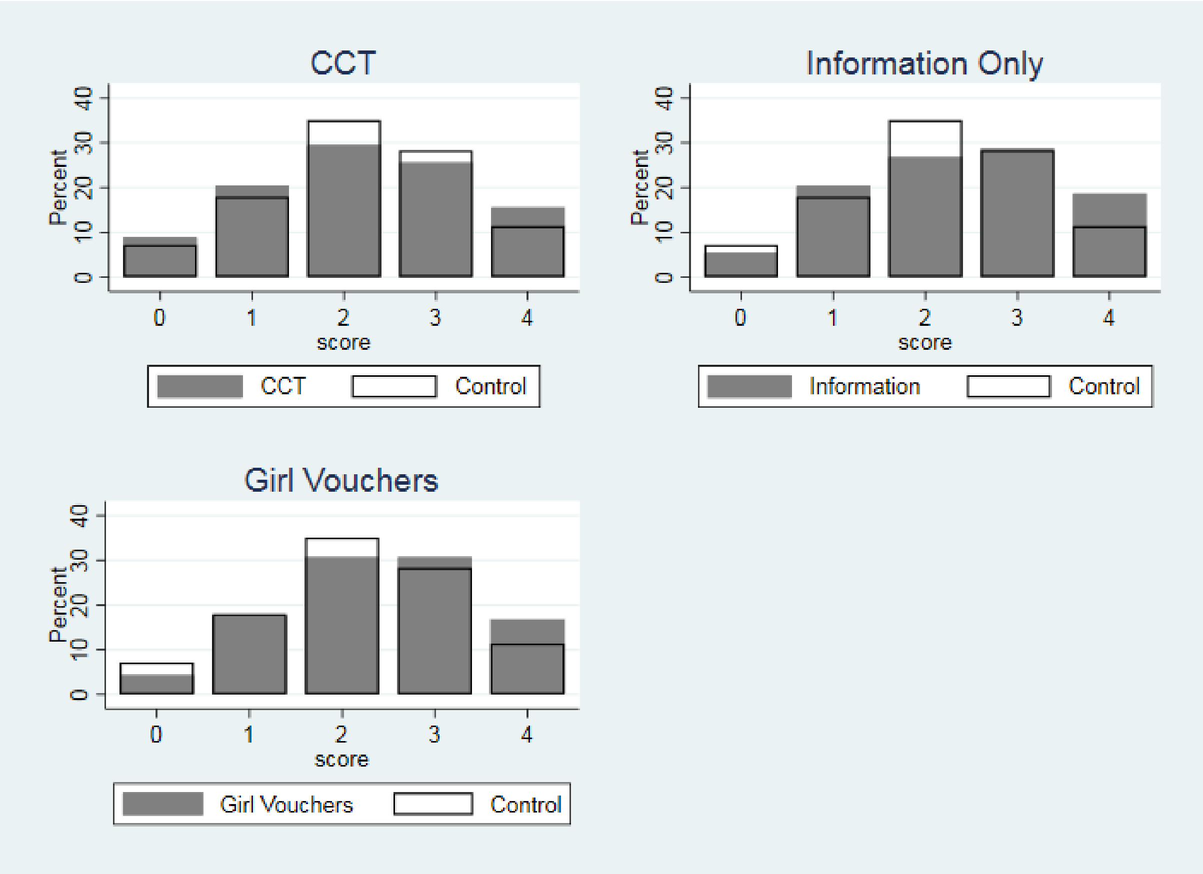
Table A-14: Learning Effects Outside the Mean

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	score	I[score=4]	I[score>2]	I[score<2]	score	I[score=4]	I[score>2]	I[score<2]
Panel A: School averages								
Information	0.183**	0.0699**	0.0804**	-0.00342	0.195**	0.0782***	0.0829**	-0.00342
	(2.01)	(2.49)	(2.09)	(-0.10)	(2.14)	(2.76)	(2.20)	(-0.10)
Parent Cash	0.0202	0.0471*	0.0228	0.0341	-0.00233	0.0427	0.0208	0.0341
	(0.23)	(1.70)	(0.60)	(1.06)	(-0.03)	(1.53)	(0.56)	(1.06)
Girl Voucher	0.203**	0.0338	0.0799**	-0.0468	0.178*	0.0256	0.0693*	-0.0468
	(2.27)	(1.22)	(2.11)	(-1.46)	(1.97)	(0.91)	(1.85)	(-1.46)
Characteristics	No	No	No	No	Yes	Yes	Yes	No
Observations	173	173	173	173	173	173	173	173
Panel B: Individual outcomes:								
Information	0.166**	0.0740***	0.0776**	0.00242	0.171**	0.0835***	0.0829**	0.0105
	(2.06)	(2.86)	(2.09)	(0.08)	(2.08)	(3.05)	(2.19)	(0.34)
Parent Cash	0.00358	0.0435**	0.0190	0.0413	-0.00673	0.0449**	0.0173	0.0494
	(0.04)	(1.99)	(0.53)	(1.28)	(-0.08)	(2.07)	(0.48)	(1.55)
Girl Voucher	0.210***	0.0571***	0.0859***	-0.0359	0.226***	0.0647***	0.0966***	-0.0356
	(2.88)	(2.75)	(2.64)	(-1.25)	(3.26)	(3.14)	(3.08)	(-1.27)
Baseline Characteristics	No	No	No	No	Yes	Yes	Yes	Yes
Observations	2600	2600	2600	2600	2464	2464	2464	2464
No. of clusters	173	173	173	173	173	173	173	173
Mean individual outcome	2.19	0.11	0.40	0.25	2.20	0.11	0.40	0.25

Source: household survey (endline). I[] denotes the indicator function. "Score" is the ASER math test score, which takes the following possible values: recognition of single digit numbers (scored 1), recognition of double-digit numbers (scored 2), correct subtraction (scored 3), correct division with remainders (scored 4), or cannot even recognize single digit numbers (scored 0). All regressions include a constant and district fixed effects. Panel B: school-level clustered t-statistics in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

B. Appendix Figure

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



Source: Endline household survey.

C. Further Related Literature

C1. Role of Information

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

C2. Children Incentives

For experiments incentivizing students, but not parents, for the student to achieve a certain standard at scholastic tests or a range of inputs in this test, see Angrist and Lavy (2009), Kremer 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 (2020).

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D. Illustration of the Case with Both Parental and Children's Inputs

In Section II-B, we consider the child's agency in deciding the level of a child's educational input (attendance) when pay-offs are attached directly to whether the child supplies this input. The parent's role is simply to incentivize or not the child to supply this input. If instead we considered the decision to invest parental and/or children's inputs in the production of an educational outcome (e.g., test scores), differences between the effects of incentivizing parents or children to achieve a certain outcome level may reflect not only the agency of the child in supplying the child input but also the relative productivity of parents and children's inputs.

To fix ideas, consider the parent-child pair indexed by *n* for whom, in the perfect information case, the adult and children's payoffs are represented by the matrix:

	$e_c = 0$	$e_c = 1$
$e_a = 0$	0,0	$p_c V_n^a$, $p_c V_n^c - c_n^c$
$e_a = 1$	$p_a V_n^a - c_n^a$, $p_a V_n^c$	$(p_a + p_c)V_n^a - c_n^a$, $(p_a + p_c)V_n^c - c_n^c$

Where e_c indicates whether the child supplies the child's input (associated with utility cost c_n^c) and e_a indicates whether the parent supplies the parental input (associated with utility cost c_n^a). For instance, one can think of e_c as indicating whether the child attends school and of e_a as indicating whether the adult helps the child with their homework or explains to the child what was studied at school. Provided the child's educational outcome attains a certain threshold, then the parent receives V_n^a and the child receives V_n^c. When the child (parent) supplies input $e_c = 1$ ($e_a = 1$), the probability that the educational outcome reaches the threshold is p_c (p_a). When both supply their input, the probability that the educational outcome attains the threshold is $p_a + p_c$, so we assume $1 \geq p_a + p_c \geq 0, 1 \geq p_a \geq 0, 1 \geq p_c \geq 0$.

Assuming the same monitoring technology as in Section II-B, in the case in which it is not privately optimal for the child to supply his/her effort ($p_c V_n^c - c_c < 0$) the child's incentive compatibility constraint yields the same required transfer from parents to incentivize the child to supply his/her input as in Section II-B, except for the inclusion of p_c: $\frac{1}{2\pi-1}(c_n^c - p_c V_n^c)$.

But the adults now have two instruments at their disposal to achieve a positive payoff: they can either incentivize the child to supply their input ($e_c = 1$), and/or they can invest their own input ($e_a = 1$). In this simple model, the parent will find it optimal:

- to supply the adult input as long as:

$$p_a V_n^a \geq c_n^a \qquad (5)$$

- to incentivize the child to supply the child input as long as:

$$p_c V_n^a \geq \frac{\pi}{2\pi-1}(c_n^c - p_c V_n^c) \qquad (6)$$

In the presence of imperfect monitoring, an increase in V_n^c by one dollar should increase the probability of Inequality (6) holding more than an increase in V_n^a by one dollar (since $\frac{\pi}{2\pi-1} > 1$ for any $\pi \in \left]\frac{1}{2}, 1\right]$), leading to a larger increase in the likelihood that the child invests in his/her human capital and thus increases the probability of achieving the learning threshold by p_c. But an increase in V_n^a also makes (5) more likely to hold and therefore makes parents more likely to invest the adult input. This would in turn lead to an increase in the likelihood that the child meets the learning threshold by p_a. Incentivizing the child or the adult to achieve a learning outcome will therefore have different effects on the outcome not only because children may have agency regarding his/her own investment in his/her human capital but also through the relative productivity of adult and children's inputs.[41]

[41]A similar argument would follow if we assumed that the parental input only had a positive effect on the probability of achieving the outcome threshold conditional on the child contributing their input (i.e., replacing p_a with $p_a \times e_c$ everywhere). In this case, for any positive c_n^a, the parent never invests the adult input when e_c=0, and, when e_c=1, the parent invests the adult input when Inequality (5) holds.

E. Qualitative Evidence

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

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

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

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

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

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

doesn't know how to write, she only learnt how to write her name this year, she is lazy and doesn't like to go to school. She is 11 and is in fourth grade."

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

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

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

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

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

F. Pre-Specified Outcomes

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

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

G. Detailed Description of Further Robustness Checks

Correcting for attrition for outcomes measured through the household survey. As reported in the main text, attrition of girls taking part in the household survey was slightly larger in the control group than in the treated groups, although not jointly statistically significantly so. Our main outcome of interest (independently verified attendance rate at school) and teacher attendance are not affected by any differences in attrition in the household survey. In Table A-

5, we present results for the other outcomes, correcting for differences in survey attrition. More precisely,, we ran regressions in which the school averages are obtained after weighting each individual observation in the endline survey sample by the inverse of the probability that it is included in the sample, as predicted by all the individual and household baseline characteristics summarized in Table A-2. Reassuringly, reweighting observations by the inverse of the probability that they attrit does not change our conclusions.

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

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

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

Trimming the school sample. The school-level analysis carried out in the paper is much less sensitive to outliers in terms of school size than individual-level analysis (since each school is given the same weight). Still, in Table A-9, we report results obtained when dropping the 5% largest schools and 5% smallest schools to test whether results are very different in the tails of 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 collection could not proceed. The schools for which we were able to collect attendance data at those times may therefore be selected (although, as mentioned before, there was no overall difference between treatment arms in the number of times attendance data was collected). Table A-10 reports estimates for the two outcomes based on attendance checks obtained when ignoring data from spot checks for which less than 70% of the district's schools could be surveyed.[44] Point estimates decrease slightly in magnitude—suggesting the treatments may have had larger effects at times of high absenteeism due to the conflict, but the overall picture is unchanged.

Selecting control variables based on their predictive power instead of baseline balance. Throughout the paper, we check the robustness of our findings to controlling for characteristics that were not balanced for at least one treatment arm at baseline. In Table A-11, we instead

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

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

control for covariates chosen based on their ability to predict the outcomes studied, irrespective of baseline balance. More specifically, in this robustness check we control for any baseline variable which has a t-statistic equal to 1.96 or above when regressing the outcome on district fixed effects and the baseline characteristics reported in Table A-2 in the control group. The effect of the CCT on high self-reported monitoring quality becomes statistically significant at 10% but our overall conclusions are unchanged.

Ex-post power calculations. In Table A-12, we report ex-post power calculations using the means and standard deviations of the outcomes studied in this paper in the control group, for 80% power in detecting differences between any experimental group pair and a Type 1 error of 0.05. In keeping with the main analysis, we present power calculations based on the distribution of school-level averages.[45] The last column reports the Minimum Detectable Effect (MDE) as a share of the control group's mean, showing that the experiment is well-powered for our three schooling outcomes, teacher absenteeism and self-reported monitoring quality, but not for early marriage and self-reported empowerment. This bolsters our confidence in the results for which we find consistent significant effects, while confirming the inconclusiveness of our findings for early marriage and self-reported empowerment.

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