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

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Abstract

Education conditional cash transfer programs may increase school attendance in part due to the information they transmit to parents about their child's attendance. This paper presents experimental evidence that the information content of an education conditional cash transfer program, when given to parents independently of any transfer, can have a substantial effect on school attendance. The effect is as large as 75 percent of the effect of a conditional cash transfer incentivizing parents, and not significantly different from it. In contrast, a conditional transfer program incentivizing children instead of parents is nearly twice as effective as an "information only" treatment providing the same information to parents about their child's attendance. Taken together, these results suggest that children have substantial agency in their schooling decisions. The paper replicates the findings from most evaluations of conditional cash transfers that gains in attendance achieved by incentivizing parents financially do not translate into gains in test scores. But it finds that both the information only treatment and the alternative intervention incentivizing children substantially improve math test scores.

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

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Introduction

Governments the world over strive to incentivize parents to ensure that their children attend school regularly through subsidies, fines, and truancy laws (BBC, 2005, Maynard et al., 2017). In developing countries, arguably the most significant innovation in social policy in the past few decades has been the introduction of conditional cash transfers (CCT) made to parents in order to incentivize a number of prescribed behaviors such as regular school attendance, and which are now implemented in over 60 countries (Parker and Todd, 2017).

When information frictions in parent-child interactions are taken into account, however, simply providing information to the parent about child attendance at school may improve attendance independently of any costly transfer, and incentivizing children themselves may be more cost-effective than incentivizing parents. While the role of children in their own schooling decisions is generally ignored by researchers and policy makers, repeated instances of peer effects in attendance decisions (Lalive and Cattaneo, 2006; Cipollone and Rosolia, 2007; Bobonis and Finan, 2009) suggest that children take part in decisions over whether they attend school (Kremer and Holla, 2009). In fact, when asked, a majority of surveyed children who have dropped out of school by age 15 in India, Ethiopia, Peru and Vietnam say that they themselves played the most important role in deciding to do so.[1]

In this paper, we provide experimental evidence from a poor, rural African country setting that children have agency in decisions regarding their schooling as early as sixth grade, and that neglecting this reality may lead to inefficiencies in education policies. We present experimental evidence of the effect of three alternative policies targeting Mozambican girls in the last two grades of primary

[1] More precisely, 40% in India, 58% in Ethiopia, 65% in Vietnam, and 80% in Peru. Authors' calculations based on Young Lives Round 3 data (Boyden, 2014).

school: (1) providing weekly information to parents about their child's attendance (information treatment); (2) providing this information and making cash transfers to parents conditional on attendance – where the maximum annual transfer is worth about 8 times the daily wages of an agricultural laborer or 7% of per capita GDP (CCT treatment); or (3) providing the same information and making transfers of the same nominal value to *children* in the form of a voucher, also conditional on attendance (child incentive treatment). We draw three main conclusions from our experiment. First, we find evidence that the information content of a conditional transfer can have a substantial effect on school attendance independently of any transfer. In our experiment, where the value of the transfer is modest but similar to other CCT programs across the world,[2] the estimated effect of the information treatment on attendance is as large as 54% of the child incentive treatment effect and 75% of the effect of the CCT treatment. Our second key conclusion is that incentivizing children is at least as effective in raising attendance as incentivizing parents – and importantly, *not* because parents were able to appropriate the transfers made to children or reallocate household expenditure to cancel out the transfer made to their children.[3] Finally, we replicate findings from most evaluations of CCTs that gains in attendance achieved by incentivizing parents financially do not translate into gains in test scores. In contrast, both the information treatment and the children's incentives treatment improve scores on the (ASER or "Annual Status of Education") math test by 8.5 to 9.4% of the control group's mean. This

[2] In their review CCT programs, Fiszbein and Schady (2009) state that transfers range from no more than 4 percent of mean household consumption in Honduras, Bangladesh, Cambodia and Pakistan to 20 percent in Mexico (p.5).

[3] Although we cannot reject the hypothesis that the parents incentives and children incentives treatments had the same effect, the estimated effect of incentivizing children is in fact 38% larger than the effect of incentivizing parents.

suggests that improved attendance is beneficial for cognitive skills, but that conditional cash transfers directed at parents may have counterproductive effects, echoing findings by Baird, de Hoop and Özler (2013) that increases in the monetary value of conditional transfers for which the household is eligible increases adolescent psychological distress.

From a policy point of view, our results provide evidence of the effectiveness of two lower cost, easily scalable, and, at least in the case of the information intervention, arguably less politically controversial alternatives to traditional CCTs: providing information to parents about their child's attendance at school through a simple report card to be taken home at the weekend, and incentivizing children with vouchers to be exchanged for a limited choice of items which are likely to "stick" to their recipient such as school uniforms, shoes, and school bags.

Our research contributes to three strands of literature. First, we augment the knowledge base on intrahousehold decision making in the presence of information asymmetry. A recent literature has emerged with the aim of understanding the respective role of men and women and asymmetric information between spouses in household decisions such as household expenditure and family planning (Ashraf, 2009; Ashraf et al., 2014). But despite recent work suggesting that children take part in household decisions (Dauphin et al., 2011), recognizing that children may have their own preferences (Dunbar et al., 2013), and finding that the investments in learning made by children age 10-14 are more important for test scores than those of their parents (Del Boca et al., 2017), there is little evidence on the respective role of parents and children in making schooling decisions – one of the key areas of decision affecting children's lives. Bursztyn and Coffman (2012) first showed that, in the presence of asymmetry of information regarding school attendance between parents and children, conditional cash transfers may be effective in increasing attendance in part because they improve the parent's ability to monitor their child – at the very least, a parent who receives a transfer conditional on 80% school attendance should be reassured that her child has attended school at least 80% of the time. Bursztyn and Coffman (2012) further find that parents in a poor urban Brazilian setting indeed value the information component of a national CCT, but they do not test empirically the effectiveness on school attendance or other outcomes of providing information only, or how it compares to the effectiveness of providing the conditional transfer as well as the information.

Several recent studies have investigated the effect of improving the information parents receive about attendance (Berlinsky et al., 2017; Rogers and Feller, 2018) and other measures of student effort at school (Bergman, 2016; Bergman and Chan, 2017; Cunha et al., 2017) in urban, middle- to high-income country settings.[4] All find significant effects on attendance and, in three out of

[4] Two of these papers explicitly compare the effect of providing information about attendance to that of an encouragement for parents to ensure that their children attend school regularly. Rogers and Feller (2018) compare the effect of a reminder of the importance of regular attendance and of the parents' role in ensuring regular attendance with the effect of providing this reminder *as well as* individual information about the child's attendance record over the ongoing academic year. They find that, while the reminder in itself reduces absences by 3.5%, the "reminder and attendance information" treatment reduces absences by 6.9%, and that the two effects are statistically different from each other. Cunha et al. (2017) compare the effect of text messages to parents about the importance of school attendance, punctuality and assignment completion, with that of text messages with information about the performance of their children during the preceding 3 weeks on these three outcomes, but without reminder of the importance of these behaviors. They find that both types of treatment 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

five cases, on test scores as well (Bergman, 2016; Berlinsky et al., 2017; and Cunha et al., 2017). The technologies used to transmit information to parents in these studies (text messaging, email, post), although low-cost in a rich country setting, would be difficult to implement if not unfeasible in many rural, developing country settings such as the one we study. More importantly, evidence of information frictions between parents and children in Santiago de Chile, São Paulo, Philadelphia and Los Angeles do not necessarily translate to rural Sub-Saharan settings – it is therefore striking for these studies and ours to reach similar conclusions.[5]

A second strand of literature which our research complements is that on the optimal design of cash transfers. Previous literature has, among others, focused on the role of the conditionality (see Baird et al., 2014 for a meta-analysis and Baird et al., 2011; Benhassine et al., 2015 and Akresh et al., 2016 for experimental comparisons of conditional and unconditional or "labeled" transfers), varied the gender of the recipient (Benhassine et al., 2015; Akresh et al., 2016; Haushofer and Shapiro, *forthcoming*), and studied the optimal timing of the transfers (Barrera-Osorio et al., 2011). But little is known about the effectiveness of incentivizing children vs. incentivizing parents. Two recent

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providing information only. In our experiment, we only provided information, and only information about the pupil's attendance.

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

papers compare experimentally the effect of incentivizing parents relative to incentivizing children to achieve an attendance (Baird et al., 2011) or performance target (Berry, 2016). Baird et al. (2011) vary experimentally the amount of cash given to parents (from \$4 to \$10) and that given to adolescent girls (from \$1 to \$5) in Malawi and find similar effects for each extra dollar irrespective of the nominal recipient of the cash transfer. It is however possible that the cash given to the adolescent girl with the full knowledge of her parents had to be, at least in part, devolved to the parent. In addition, the authors find that increasing the value of the conditional transfer offer has no effect on the outcomes studied over and above the minimum value of the conditional transfer. In a context where increasing the amount of the transfer does not increase the treatment effect, it is perhaps not surprising to find that the identity of the recipient of the extra dollar does not matter. Our experiment does not split the transfer amount between parent and children but varies who receives the transfer altogether, and in order to ensure that our child incentive treatment indeed incentivized the child, we chose to provide transfers to children in the form of vouchers to be redeemed against a limited number of items that are both attractive to the children and unlikely to be appropriated by others. Berry (2016) compares the effect of a range of treatments – randomized across individuals – incentivizing the performance of Indian children in Grades 1 to 3 on a literacy test, varying the type of transfer (cash, voucher to buy toys, toy) and recipient (parent or child). He finds no evidence that the identity of the recipient matters on average, but sheds light on how the recipient of the performance incentive scheme may interact with the relative productivity of parents and children in producing learning. Our paper instead focuses on the effect of incentivizing children who are older (and thus possibly more likely to have agency in schooling decisions) simply to attend school, and hence abstracts from issues of relative parent/child efficiencies in learning production in order to provide a direct test of the importance of parents' and children's returns to education in attendance decisions.

Finally, we offer new light on two puzzles arising from experiments evaluating the effect of education subsidies in developing countries. The first of these puzzles is that most studies evaluating the effect of CCTs (to parents) on test scores estimate positive effects on enrollment and/or attendance but find no evidence of gains in test scores (with the notable exceptions of Barham, Macours and Maluccio, 2016 and Baird et al., 2011). While this lack of evidence of cognitive skills gains can be rationalized in part by sample selection issues – in studies using school-based tests, children induced to attend by the CCT may have lower skill levels– and/or school quality being negatively affected by increased school participation, we replicate this finding using data from a sample of eligible children who are not selected based on school attendance and largely ruling out the worsening school quality mechanism. Indeed, we find that our other two interventions have effects on attendance of the same order of magnitude as the CCT, but have positive effects on test scores an order of magnitude larger than the CCT. This finding points to the conclusion that introducing parental incentives on attendance produces specific negative spillovers on learning. The second well-known puzzle arising from previous research on which we shed new light is that simply distributing free school uniforms has had large effects on attendance, enrollment, and pregnancy outcomes in Kenya (Duflo et al., 2015 and Evans and Ngatia, 2017), which, as noted by Kremer and Holla (2009), is difficult to reconcile with simple models of human capital investment where decisions are solely made by parents.[6,7]

[6] In urban Ecuador, Hidalgo et al. (2013) find that a program of free school uniforms *decreases* attendance, which they argue may be due to a combination of factors including the fact that only 63% of schools where children were told that they would receive free uniforms actually did.

[7] Examples of ITT (LATE) effects reported in these studies are: 3.8 (7.0) %-points or 20 (37)% decrease in absenteeism in Evans and Ngatia (2017), and

Even if not conditional on an attendance target, it is reasonable to think of the receipt of free, new, school uniforms as increasing *children*'s returns to school attendance in addition to decreasing out-of-pocket costs for parents. If children's returns to education matter for schooling decisions independently of the value parents attach to this education – as we find supportive evidence for here, then the increase in children's benefits to school attendance could explain part of the large effects observed.

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

I- Institutional Context, Theoretical Motivation and Study Design

A. Institutional Context

Mozambique is a predominantly rural country in South-Eastern Africa (68.4% of the population lives in rural areas, INE 2015) and, with a Human Development Index ranking 181 out of 188 (Kenya, for instance, is ranked 146), is one of the poorest countries in the world despite a doubling of real GDP per capita between 2001 and 2016 (from \$615.3 to \$1,128.3 PPP, World Bank 2017). The country's recent history has been marked by a 15-year civil war following independence in the 1970s, and occasional clashes between armed forces and RENAMO's armed militias in the center of the country. Despite large increases in enrollment rates in lower primary school grades, most children are still not completing primary education. As of 2014, the net enrollment rate in primary education was 87.6%, up from 54.8% in 2002. But the survival rate to

16.5 (18.9)% decrease in female (male) primary school drop out after three years in Duflo et al. (2015).

the last grade of primary education was only 33.2% in 2013, compared to a Sub-Saharan average of 57% (World Bank Education Statistics Data Bank, 2017).

While the net intake at Grade 1 of primary schooling is high for both boys (74.5%) and girls (73.1%), and secondary schooling is still restricted to an elite (17.9% net enrollment for both boys and girls), most children in Mozambique, and girls in particular, experience difficulties in completing primary school.[8] For upper primary schooling (Grades 6 and 7 or Ensino Primário de Segundo Grau "EP2", which the present study focuses on), the official completion rate is abysmal, especially in rural areas where even at age 19 it is only about 14% for males and 8% for females (according to Figure 3.10 in Fox et al., 2012). In this context, a policy priority is to find ways to increase the school attachment of pupils, and girls in particular, in the higher grades of primary school.

We focused on one province of Mozambique where our implementation partner – the development NGO Magariro – is active and well-known: Manica. Manica Province is located in the Center Region of Mozambique and is home to 7.5% of the country's population. It is close to the national average on a number of indicators, from population density (30.3 people per square meter compared to a national average of 31.3), poverty rate (41% in 2014 compared to a national average of 46.1%), to annual drop-out rates in primary schooling (6.8% in Manica and countrywide for EP1, 9.9% versus 8.8% nationwide for EP2) (INE, 2015; MPD-DNEAP, 2016). Mozambique in general, and Manica Province in particular, have low population density even for Sub-Saharan Africa standards (where the average was 42.6 in 2015), but not dissimilar to other countries in Eastern Africa (36.7% in Kenya, for instance). This may matter in our context because our study design is, as explained below, motivated by the hypothesis that there may be imperfect monitoring of the children's actions by

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

parents, which is plausibly more likely when population density is low and the school is located further away from the child's home.

B. Theoretical Motivation

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

One highly debated question is that of the role of conditionality. If the only reason why individuals do not invest more in human capital is that they face credit constraints, then unconditional and conditional cash transfers should have a positive effect on human capital investments irrespective of the conditions attached to the transfer. On the other hand, conditionality may lead to larger increases in school enrollment, e.g. if individuals underestimate returns to education or if the conditionality helps parents monitor their children's behavior, since they can infer whether their child attended school regularly from the transfers they receive or do not receive. The first argument in favor of conditionality (underestimation of returns to education) is well-known. The second, however, has appeared only recently in the literature, and has been shown to be very relevant in the Brazilian urban context, where parents have been found to value the monitoring of their children's attendance at school (Bursztyn and Coffman, 2012). The idea is simple, and is rooted in the well-known critique to Becker's (1974) "Rotten Kid Theorem". 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. Bursztyn and Coffman (2012) show, both theoretically in a simple principal-agent model with moral hazard, and through a lab experiment in the case of the Brazilian education CCT program *Bolsa-Escola*, that the conditionality may reduce information asymmetry and thus reestablish the conditions for the theorem to hold.

Bursztyn and Coffman's (2012) point can be summarized as follows. Consider the parent-child pair indexed by *n* for whom adult utility is:[9]

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

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.

If $V_n^c > c_n$, the child attends school even without further incentives, irrespective of the parent's ability to monitor her attendance. If $V_n^a < c_n - V_n^c$, then it is optimal for the child not to go to school from the point of view of maximizing the sum of the payoffs of the parent and child, irrespective of the parent's ability to monitor her attendance. If, however, $V_n^c \leq c_n$ but $V_n^a \geq c_n - V_n^c$, then whether or not it is optimal for the parent to incentivize the child to go to school depends on the quality of the parent's monitoring technology. Define the signal technology as:

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

[9] 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.

A parent can only condition transfers to the child based on signal s_n, which is correct with probability π.[10] 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:[11]

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

Where the probability of inequality (3) holding increases with signal quality (higher π). As a consequence, under imperfect information, simply providing information to the parent may induce higher attendance. Since CCTs give the parent, at a minimum, a binary signal as to whether or not the child met the attendance requirement upon which payments are conditioned, the conditionality may in itself lead to higher attendance. This motivates our test of whether giving parents information about their child's attendance has an effect on attendance. In addition, it motivates our 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 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 t makes inequality (3) more likely to hold than increasing V_n^a by the

[10] 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.

[11] 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 $w_n \geq 0$.

same amount (since $\frac{\pi}{2\pi-1} > 1$).[12] This motivates our comparison of the additional effect (relative to improving information only) of conditional transfers aimed at parents to that of conditional transfers aimed at children.

C. Study Design

In order to assess the relevance of our analytical framework, as well as to help define the design of the Randomized Controlled Trial (RCT) described below, we first undertook a qualitative analysis in the province where the RCT took place but 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. In addition, data collected in the baseline household survey in the experimental sample asked parents (both in treatment and control areas) whether they thought it would be useful to see a weekly report showing whether their daughter had attended school regularly, and, if they answered that it would, a follow-up, open-ended question then asked why they thought such report would be useful. Eighty percent of parents responded "yes" to the first question, and among those, when asked (the open-ended question of) why they thought it would be useful, 98% responded that it would allow them to monitor their child's school attendance.

Other than providing a first pass confirmation of the relevance of our analytical framework to the study area, the preliminary focus groups aimed to establish how to incentivize girls effectively and in a manner that would be acceptable to the local population. The main conclusions were that giving cash

[12] And the effect of incentivizing children should be all the larger than that of incentivizing parents the larger the informational wedge. A CCT which improves parental information such as ours should therefore lead to a reduction in the additional effectiveness of incentivizing children relative to parents.

to girls would make both the girls and their parents uncomfortable, that if they did receive cash they would give it to their parents (or be expected to), but that there were a number of items which, if given to them to reward school attendance, would be welcome by the girls and likely to "stick" to them.

Given these insights, we designed the following four experimental groups. In two of the experimental groups, we introduced transfers conditional on achieving at least 90% attendance during the school trimester. In a "girl vouchers" treatment arm, we gave money-equivalent vouchers (400 meticais[13] at the end of each trimester with a maximum of 1,200 meticais over the 2016 school year) to girls in Grades 6 and 7 who could then use the vouchers to buy a selected number of items such as: school uniforms, shoes, school bag, smaller materials (pens, notebooks, etc...), which were delivered at the school by the research team and could be purchased during the research team visit. The choice of items made available was based on the preliminary focus group interviews carried out in villages outside the study area. The qualitative evidence collected indeed suggested that these items met two important criteria: (i) they were consistently cited when children (parents) were asked what gifts could incentivize them (their daughters) to attend school regularly and (ii) both parents and children seemed confident that a girl who was given these items would be able to keep them for herself and would not be expected to share with anyone else. In a "parents cash" treatment arm, we instead gave the same value (400 meticais per trimester) in cash to the parents and made the same items as in the "girl vouchers" arm available for *optional* purchase at the school. It was clearly explained that there was no expectation as to how the parents would spend the

[13] 400 Mozambican meticais was worth US$8.36 on January 1, 2016 but only US$5.62 on December 31, 2016, as the exchange rate deteriorated substantially over the course of the (school) year. The maximum annual transfer is worth about 8 times the daily wages of an agricultural laborer in the study area.

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.

Note that, in addition to matching the value of the vouchers given to girls to the cash received by parents, the price of items in vouchers matched the price in Mozambican meticais to reinforce comparability. In both conditional transfers arms, the conditionality was enforced by the implementing NGO based on the information contained in attendance report cards distributed at the start of the trimester. These simple report cards (a sheet of paper inside a plastic pocket) had a coding easily understood by parents: the teacher drew a circle for a given day if the girl attended school that day, or the teacher marked a cross for each day missed. The report cards were given to the girls at the end of each week to show their parents and brought back to school at the start of the next week. The report card system was explained by the implementing NGO during a visit to the school community before the intervention. Parents, either through direct attendance at this initial meeting, or through learning about the report card system from other parents, teachers, or pupils, could draw their own conclusions if a child decided not to show the parent the report card. All girls enrolled in EP2 in the conditional transfer schools were eligible for transfers and thus given attendance report cards.

In a third treatment arm, we applied an "information" treatment, in which we introduced the report card system described above, but where attendance was not incentivized by vouchers or cash transfers. A fourth experimental group constituted the control group.

A comparison of school attendance rates between the information treatment group and the control group answers the question of the effect of reducing the information asymmetry between children and parents. The inclusion of the information treatment group allows us to disentangle the effect of providing information to parents on their child's attendance from that of increasing parental (in the "parent cash" arm) or children's (in the "children vouchers" arm) returns to attendance.

In order to ensure the quality of the data recorded in the attendance report cards,[14] and given the extra work required from the main teachers ("Directores de Turma") to fill in those cards, we introduced a small compensation scheme. The scheme worked as follows: in the three intervention groups, the main teachers in charge of a class who, at every spot check by the independent surveyor, were found to have thoroughly filled in all their (female) pupils' report cards for the current trimester until the day of the spot check, received 250 Meticais' worth of airtime at the end of the trimester. The value of 250 meticais corresponds to the opportunity cost of about 5 minutes per day, evaluated at the hourly salary equivalent of the average teacher. The school directors of all schools, including the control group, received 250 meticais in airtime at the end of each trimester without conditions to thank them for their assistance and cover small costs due to necessary communications with the research team.

II- Data, Randomization and Experimental Balance

A. Data

Independent, unannounced, attendance checks ("spot checks"). The main outcome of interest for the evaluation is whether a girl enrolled in school was present during independent attendance "spot checks" by the survey firm hired by the authors. Twice per trimester, an enumerator arrived unannounced at each school in the sample and recorded in person the individual attendance/absence

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[14] A systematic review by Baird et al. (2014) of conditional versus unconditional transfers shows the importance of enforcing conditionality. The review indeed concludes that CCT programs that are explicitly conditional, monitor compliance and penalize non-compliance have substantively larger effects than unconditional transfers (60% improvement in odds of enrollment), whereas the advantage of CCTs with minimal monitoring and enforcement over unconditional cash transfers is not clear.

of every child enrolled in EP2. The attendance rate triggering transfers in the conditional transfers arms was calculated by the implementing NGO solely based on the information contained in the attendance report cards described in Section I-C. No incentive was paid on the basis of the presence or absence of pupils during the attendance spot checks and therefore there is no reason to expect the data to be manipulated. In addition, the enumerators were blind to the treatment arms, so that there is no reason either to expect the data to be affected by social desirability bias.

In addition to the spot check data, a baseline household survey and an endline household survey collected basic household information as well as, for each girl in the household who had completed, at least, 5th Grade, and, at most, 6th Grade, as of the end of 2015 (and was therefore potentially eligible for the conditional transfers): 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 personal goods consumed by the eligible girl, cognitive tests (at endline only), and household expenditure data (at endline only).

Household survey sample. The household data used in the analysis is based on a sample drawn from the universe of girls enrolled in the 173 schools included in our study within three years of the data collection (based on school records), as in Benhassine et al. (2015), and who still lived with their parent or guardian at baseline (given our analytical framework based on asymmetric information between parents and children). The target was to interview 20 potentially eligible girls per school, sampling those enrolled in 2015 (the last academic year before the trial) and recent drop outs who were not enrolled in 2015 but were enrolled in 2013 or 2014, proportionally to the size of each of the two groups ("enrolled in 2015" and "recent drop outs") in the school.[15] During

[15] Ahead of the baseline survey, the survey team visited each school and, based on school records, drew the list of all girls who (i) were enrolled in grade 5 or

fieldwork, however, there were difficulties locating the girls listed in the school records, and most of the recent drop outs had either moved away or were not living with their parents anymore and were thus ineligible for interview. The sampling target of 20 per school was therefore not attained in many of the smaller schools, and where possible more than 20 girls were sampled in order to help preserve power. All in all, the median number of girls surveyed per school in the baseline household survey is 18, and recent drop outs were under-represented in the household survey sample (3% of the baseline sample compared to 13% of all girls last enrolled in Grades 5 or 6 at some point during 2013-2015 across all 173 schools). There was no difference in the total number of girls interviewed in the baseline household survey, or the share of recent drop outs in the household survey sample, across treatment arms, however.[16] For transparency, the main analysis reported in this paper is carried out at the school level (i.e., averaging variables at the school level), and does not apply any sampling weights so that each school is weighted equally whatever the number of girls interviewed or observed during the spot checks. In robustness checks reported in Section IV, we also repeat the analyses giving each school a weight proportional to the size of its potential EP2 intake for 2016 (based on school enrollment data for 2013, 2014 and 2015 and thus prior to our experiment).

Timing. The Mozambican school year runs from February to December. We collected a baseline 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). Each school received an initial visit by the implementation NGO, a locally well-known development organization called Magariro. School staff in all

grade 6 in 2015 ("enrolled in 2015" list) or (ii) were enrolled last in grade 5 or 6 either in 2013 or 2014 ("recent drop-outs" list).

[16] The maximum difference between any two experimental arms is 0.8 girl (t-stat: 0.72) for the number of girls interviewed and 1.4%-points difference in the share of recent drop outs (t-stat: -1.26).

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. Re-enrollment is automatic for grades such as Grade 7 at which admission is not conditional on passing an exam. In the case of Grade 6, in which enrollment requires students to have passed the end of Grade 5 exam, re-enrollment is not automatic but the official enrollment period ended on January 6, 2016. The treatment announcement visits by the implementing NGO started on January 14 and ended on March 3. Initial visits by Magariro to announce the treatments took place after the start of baseline survey collection in all but one school. But given the delays in completing the baseline survey caused by political tensions between RENAMO and government forces and by heavy rains, in just under 22% of schools, the baseline survey was completed after the initial visit in which the NGO announced the treatments.[17] This may have affected baseline self-reported attendance and monitoring quality, but there is little reason to believe that it should have affected data on baseline socioeconomic indicators or any

[17] There is, however, 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.

other variable for which we test balance at baseline. The interventions were not means-tested, and more generally there was no room to manipulate the eligibility criteria (gender and grade). In all schools, the treatments were announced after the official enrollment period and, in three quarters of schools (corresponding to 7 out of 11 districts), well after the start of the new school year, so these announcements were unlikely to affect enrollment decisions, especially considering the likely delay in spreading information to the parents of marginal enrollees.[18]

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. While attrition of girls taking part in the household sample was limited at 5.3% overall, it was slightly larger in the control group than in the treated groups. Robustness checks reported in Section IV show that this differential attrition is unlikely to be driving our conclusions. Our main outcome of interest (independently verified attendance rate at school), however, is available for all schools between one (for 3 schools) and 6 times (for 132 schools), and on average 5.6 times during the school year - corresponding to 5.6 spot checks on average, and the number of times each school was surveyed is independent of experimental arm (the p-value of a joint FF-test is 0.55).

[18] 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.

B. Randomization and Experimental Balance

We first stratified our sample of 173 schools by district to avoid randomly occurring imbalances across experimental arms in district characteristics, in fieldwork operations (since these were organized district by district), as well as to gain power, since educational outcomes vary across the 11 districts of Manica province (see, e.g., p.24 in MINEDH 2017). 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.[19] At the time of the announcement of the treatments, a human error led to two schools in the Vanduzi district being swapped (one in the information treatment and one assigned to the parent cash treatment). Throughout this paper, we classify each school based on their randomly assigned treatment arm, but our findings are robust to assigning treatment based on actual treatment status instead of intended treatment status.[20]

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

[20] Full results available on request.

Table 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, by treatment arm, as well as whether p-values of t-tests of differences between each treatment arm relative to the control group indicate that those differences are statistically significant.[21] Table 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 2 are jointly orthogonal to treatment status.[22] Some individual differences are, however, statistically significant, and thus we provide robustness checks controlling for these baseline characteristics. Note that other than for the many language and religion categories, for which there are some differences across experimental groups, the only other variables with some significant baseline differences between experimental arm pairs are self-reported absences and, to a marginal extent, self-reported quality of child attendance monitoring by the parents. Given the direction of the differences (parents in conditional transfers arms reporting fewer child absences), this may well be due to the fact that, in about one fifth of the schools, the baseline survey could not be concluded before the treatments were announced, so that parents in conditional transfers arms may have been tempted to underreport child absences.

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

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

III- Main Results

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

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

Where Y_c is the cluster (i.e., school) average for outcome Y; T_{gc}, T_{pc} and T_{ic} are indicator variables for the girls, parents, and information only treatment arms, respectively; $\boldsymbol{D_c'}$ is a row vector of 10 district (i.e., strata) fixed effects (as there are 11 districts), and ε_c is an iid error term.

β_g, β_p, and β_i can be interpreted as average treatment effects for our sample of 173 schools, giving each school an equal weight, or unweighted average treatment effects. As noted by Athey and Imbens (2017), analyzing the data at the cluster level in cluster-randomized experiments is both transparent and appealing because all the estimation formulas obtained for simple (as opposed to cluster-) randomization apply directly. In Section IV, we report estimates giving each school a weight proportional to its relative size, as predicted by pre-treatment enrollment in Grades 5 and 6, to speak to the population average treatment effect.

Table 3 presents estimates of the impact of the different interventions on schooling outcomes. In Columns (1) and (4), we report findings for our primary study outcome, i.e., school attendance measured as the share of girls in the targeted grades who were found in their classroom by the independent surveyor during unannounced school visits. The two columns present the cluster level analysis (Equation (4)) with and without controlling for the (school average) baseline characteristics listed in Table 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 report card treatment increased attendance by 4.5 percentage points

(6.9%), the parent cash treatment increased attendance by 6 percentage points (9.2%), and the girl voucher treatment increased attendance by 8.3 percentage points (12.8%). The p-values reported at the bottom of the table show whether the coefficients for each of the three interventions are statistically different from each other. The first row of p-values indicates no significant difference in impacts between the information only (report card) and the CCT (to parents) interventions. This leads to the conclusion that the information content of a conditional transfer program can have a substantial effect on school attendance independently of any transfer – in our experiment, where the value of the transfer is relatively small at 7% of GDP (but comparable to a number of existing CCTs), the estimated effect of the information treatment on attendance is as large as 75% of the effect of the CCT. In addition, the estimated effect of the information treatment on attendance is as large as 54% of the effect of the child incentive program. Incentivizing girls directly is nearly twice as effective as simply providing information, and in our baseline specification (Column (1)), this difference is statistically significant at the 10% level. The experiment lacks power to detect a genuine difference between the parents and child incentive treatments, but the estimated effect of incentivizing children is as much as 38% larger than the effect of incentivizing parents.

One concern in interpreting any difference in the effect of the "girl voucher" and "parent cash" treatments is that of whether the transfers were indeed received by the targeted individuals: if girls were unable to retain the transfers aimed at them, then there would be no practical difference between nominally incentivizing the parent or the child. Our finding that incentivizing children is at least as effective as incentivizing parents is particularly interesting in the light of evidence that our children transfers "stuck" to the targeted child. First, when asked at endline, no surveyed girl from the girl voucher arm responded that she had given away her reward or had had to sell it to give the money to someone else.[23] Second, it could have been the case that parents substituted away from expenditure on the type of goods obtained by the girl through the voucher system, thus neutralizing the transfer to the girl. We collected detailed information on the girls' consumption of (23) personal items such as clothes, bags, soap, books, etc…, excluding any item purchased through a voucher, to test for this. While we found – unsurprisingly given substitutability – a negative effect on consumption of personal items other than those purchased with the voucher in the girl voucher treatment compared to the control group, this effect was statistically insignificant and small relative to the amounts transferred (89 Meticais compared to an average of 469 meticais received in vouchers, on average, see Table A-1).

In Column (4) of Table 3, we present results 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.

Column (2) reports 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 in 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 showing that the effect on enrollment is not robust (Section IV), we conclude that the effect on enrollment was negligible.

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

Rigorous evidence of the effect of conditional cash transfers (to parents) on test scores is limited. This evidence is generally based on school data and thus potentially affected by selection into school attendance at the time of the test (Saavedra, 2016). Most studies find no evidence of test scores gains. The two exceptions that we are aware of are Barham, Macours and Maluccio (2016), who find positive effects for boys in Nicaragua, and Baird et al. (2011) who find positive effects for girls in Malawi. In columns (3) and (6), we provide estimates based on test scores at a math (ASER) test administered to eligible girls in our endline household survey, irrespective of attendance at school, which are therefore not affected by the type of selection bias which may undermine test scores effect estimates from CCTs based on school tests. Similar to most previous evidence, we find that gains in attendance from cash incentives to parents do not translate into gains in test scores. On the contrary, both the information treatment and the girls' incentives treatment improve math scores by 8.5 to 9.4% of the control group’s mean.

While the girls’ incentive treatment has a larger effect on attendance than the information treatment, the effect of both treatments on test scores is of similar magnitude and statistically significantly larger than in the parent CCT arm. It could be that part of the effect on test scores of the information treatment stems from something else than more regular school attendance. Prompted by the realization of their imperfect attendance monitoring technology, parents in the information treatment may plausibly increase their monitoring of other aspects of schooling effort such as homework, and do so more in the information only treatment where imperfect monitoring may at first be more salient than in the transfers treatments.[24] Or the similar magnitude of the effects on math scores

[24] It may indeed take parents more time to realize the informational content of the attendance report in the transfers arms, where it fullfils the additional function of allowing verification of the condition, than in the information only arm. Consistent with this, the effect of the information treatment has a flatter

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

More importantly, while the effect on attendance of the girls' incentives treatment was larger in magnitude but not statistically different from that of the CCT, the effect of the girls' treatment on test scores is 10 times larger (and statistically significantly so) than the effect of the CCT. Taken together, this evidence suggests that CCTs directed at parents may have counterproductive effects. Baird, de Hoop and Özler (2013) find that, while eligibility for cash transfers (be it conditional or unconditional) reduces adolescent psychological distress at the extensive margin, as the monetary value of the conditional transfers increases, psychological distress increases. They find that this effect is not observed for unconditional transfers, and disappears after the CCT program has ended, suggesting that transfers to parents that are conditional on the child's behavior may negatively affect the child's wellbeing – although in the case of Baird et al. (2013), increases in the value of conditional transfers did not result in reductions in test scores gains relative to the minimum level of transfers (Baird et al., 2011). Parents may, for instance, use coercive methods to increase attendance, but only do so as the value of the transfers becomes sufficiently large. Similarly, here we find no evidence that simply providing parents with information on the child's attendance affects test scores negatively, it is only when parents stand to lose cash transfers from the child's low attendance that this leads to ostensibly counterproductive effects on the production of test scores. This suggests that the introduction of cash incentives rewarding attendance but not learning distorts parental behavior in such a way as to favor investments in attendance relative to investments in other learning inputs, at

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trend across the school year than that of the transfer treatments (results available on request).

least while the intervention lasts. One way in which parents could induce an increase in attendance in both the information treatment and the parents' incentive treatment through strategies leading to differential consequences on test scores would be for them to be more likely to use a "carrot" (which increases attendance without adverse consequences on learning) in the information treatment, and more likely to use a "stick" (which increases attendance but has negative spillovers on learning) in the parents' incentive treatment. Although statistically insignificant, the signs of the estimated effects of the treatments on consumption of personal goods by the girls are consistent with this theory (Table A-1), since the effect of the information treatment corresponds to a 6% increase in consumption of personal goods, but that of the parent treatment corresponds to a 5.3% decrease in consumption of personal goods.[25,26]

Table 4 reports impacts of the intervention on a set of pre-specified non-schooling outcomes (see Appendix A for details about the outcomes specified at the time of the registration of the trial). In Columns 1 and 5, we test whether

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

[26] An alternative explanation would be that different parents respond to the information and financial incentive components, with increased attendance due to improved information having a positive effect on test scores, but increased attendance due to the financial incentive component having no effect or even a negative effect on test scores. Given the small (statistically insignificant) difference in the effect of the CCT on attendance relative to the information-only treatment, the negative effect on test scores coming from the parents responding only to financial incentives would have to be implausibly large to fully offset the positive effect of attendance on test scores for parents responding to additional information.

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

In Columns (2) and (6), we estimate the effect of our treatments on ever having been married. Given the young age of the targeted girls (12.65, on average, at baseline), only 2.28% (2.66%) of eligible girls in the household survey were married at baseline (endline) in the control group. Given the mean and standard deviation that prevail in the control group, we lack power to detect realistic reductions in the proportion ever married. The minimum detectable effect for which we have 80% power is indeed 2.88%-points, which would require, for instance, the control group to see more than a doubling of the share ever married compared to baseline while no new girl would form a union in the treated group. While the point estimates of the effect of the information and the parents' treatment on the proportion ever married are large in magnitude, only the effect of the information treatment is statistically significant at 10% in the baseline regressions, and it becomes insignificant when controlling for baseline characteristics (Column (6)).

The remaining columns of Table 4 show tests of whether the treatments had any effect on the share of girls with an above-median predicted score based on two separate principal component analyses (PCA). The first variable measures the self-reported quality of the monitoring exercised by parents on their children's school attendance, while the second one measures the extent to which the girls say that they participate in decisions about their own lives. The set of interventions evaluated in this experiment had no impact on either measure.

If the self-reported measure of monitoring quality offered a reliable proxy of actual monitoring quality, then we should see that our treatments increase self-reported monitoring quality. There are, however, several reasons to believe that this self-reported variable is a poor proxy. First, there is hardly any variation in answers to the questions on which the PCA scores are based. At baseline, only 3.4% (6.6%) of parents answered "neither agree nor disagree" or "disagree" when asked whether, at the end of each day, they know whether their daughter was at school (in the classroom), and only 6.2% answered that it had happened at least once that, on a particular day, they thought that the girl was at school but actually she was not. This could be due to nearly all parents genuinely believing that they are perfectly well informed about their child's school attendance, but the lack of variation is likely due instead to parents not wanting to acknowledge openly their lack of control. Indeed, when asked, at baseline, whether they thought it would be useful to receive a weekly attendance report card, 80% responded that it would be useful, thus suggesting that most of them think that their monitoring is not perfect, which contradicts their answers to direct questions about knowledge of their daughter's daily attendance.[27] We return to this point in Section IV, where we provide evidence of better knowledge about daughters' absences in treatment arms (and especially so in the parents incentive arm) from comparing the predictive power, across experimental arms, of the number of absences self-reported by the parent on whether the girl was absent at school during a spot check.

There is more variation in answers to the questions used to construct the girl self-reported empowerment index. At baseline, 84.4% of girls reported that they would be able to keep some item of clothing given to them in exchange of

[27] And in a follow-up, open-ended question about why it would be useful to receive such a report, 98% of those who answered that the weekly attendance report card would be useful spontaneously responded that it would be helpful in order to verify the attendance of the child.

work done, and the share reporting being involved in decisions concerning them is 14.6% for health care, 13.2% for visiting relatives, 28.6% for attending school, and 21.9% for work outside the house. While an increase in parental information may lead parents to restrict the child's independence, our measures of "empowerment", which mirror those traditionally used to measure the empowerment of adult women in general-purpose household surveys, seem to suffer from similar issues to those encountered in the measurement of adult female empowerment and are thus unlikely to be informative. While difficult to validate empirically in the absence of objective measures of empowerment, where such objective measures exist (Almås et al., Forthcoming) or where panel data exist (e.g., in the PROGRESA evaluation panel), commonly used measures of adult female empowerment do not perform well. In our sample, the coefficient of correlation between girls' answers at baseline and endline for the five questions used to construct the empowerment index is between -0.06 and 0.16. This is similar, for instance, to the within-household correlation in answers to a question about whether the wife or the husband is "in charge of household expenditure" in the PROGRESA evaluation panel.[28]

To summarize, we find evidence that providing high-frequency information to parents about their daughter's school attendance increases school attendance, and that this effect is not statistically distinguishable from that of a traditional CCT to parents also providing the same information. Incentivizing girls with vouchers allowing them to buy a choice of goods is at least as effective as incentivizing parents with the cash-equivalent of these vouchers – and importantly, *not* because parents were able to appropriate the vouchers or

[28] After restricting the PROGRESA evaluation sample to households composed only of mother, father, and children observed in all three survey waves between October 1998 and November 1999, the correlation coefficient between a binary indicator for whether the wife (the husband) is "in charge of household expenditure" and its 6-month lag is 0.040 (0.066) (Sokullu and Valente, 2018).

reallocated household expenditure to cancel out the transfer to their daughters. In terms of learning, the attendance gains from the information and girl incentives treatments translated into substantial improvements in scores at a math test, but not the attendance gains from a traditional CCT treatment. None of the treatments had a robust effect on enrollment (as would be expected given the timing of the treatment announcements), teacher attendance, early marriage, self-reported quality of parental monitoring and self-reported girl autonomy. The next section explores the robustness of these findings.

IV- Robustness Checks

Fisher Randomization and joint testing. Our baseline treatment effect estimates, while consistent with Neyman's randomization formulas for the average treatment effect (Athey and Imbens, 2017), are implemented through regression analysis. For individual tests, the two main issues highlighted by Young (2016) when relying on asymptotic theorems are related to: (i) high-leverage (which only arises with the inclusion of covariates) and (ii) clustered estimates of variance. We were therefore careful to present results that do not include covariates (other than district fixed effects) or rely on clustered standard errors. An additional issue which we address using the randomization-based tests proposed by Young (2016) is that of joint testing of multiple hypotheses. In Table 5, 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 individual p-values for 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.

Correcting for attrition. As reported earlier, attrition of girls taking part in the household survey was slightly larger in the control group than in the treated groups. While our main outcome of interest (independently verified attendance rate at school) and teacher attendance are not affected by this differential attrition in the household survey, below we present results for the other outcomes, correcting for differences in individual girls' probability to attrit from the household survey. More precisely, in Table 6, we ran regressions in which the school averages are obtained after weighting each girl in the endline survey sample by the inverse of the probability that she would be observed at endline, as predicted by all her individual and household baseline characteristics summarized in Table 2. Interestingly, none of the outcome variables measured at baseline predicts attrition, while older girls and girls from poorer households were more likely to attrit and girls from Manica district (of Manica province) were less likely to attrit. Reassuringly, reweighting observations by the inverse of the probability that they attrit does not change our conclusions.

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 whose parents reported 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. 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).[29] 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 7. These results are near-identical to those obtained in the main analysis, thus confirming that selection through school switches is unlikely to be biasing our results.

Ex-post power calculations. In Table 8, 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. 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 being ever married 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.

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[29] Individual coefficients (p-values) are: 0.004 (0.784), -0.013 (0.316), 0.0152 (0.237) for the information, parent cash and girl vouchers arms, respectively, and the joint F-test p-value is 0.183.

Controlling for pre-treatment outcomes. As an additional robustness check, we also present ANCOVA estimates obtained from estimating Equation (4) with an additional regressor equal to either the value of the outcome at baseline, where available, or to an available proxy of the outcome at baseline, when the outcome was not measured at baseline but a reasonable proxy exists. Using this 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 9 show that all our conclusions so far are robust to the inclusion of these pre-treatment outcomes.

Interpretation of our findings for the information treatment. Here we present further evidence in support of our interpretation of the effect of the information treatment as being due to an increase in the quality of parental monitoring rather than due to some generic "salience" effect of the treatment. For the girls surveyed in their households who were both (i) (reported by parents as being) enrolled in school in 2016 and (ii) could be matched to our independent school attendance records,[30] we can evaluate the quality of the parental monitoring technology by checking 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 10 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

[30] 77% of girls whose parents said were enrolled could be matched to names in official school records.

the spot check by 1/22=0.045. In the control group, however, the estimated increase is positive but small at only 0.009 and it is statistically insignificant. In all the treatment arms, the estimated increase in the probability of being absent during the spot check more than doubles and is statistically significant. In the parents' incentive arm, this probability more than trebles and reaches 72% of the expected 0.045 coefficient. While the number of absences reported by the parents may not be exogenous, much of the heterogeneity which may lead to omitted variable bias is likely to be captured by the number of days absent in October of the previous year reported by parents at baseline. Columns (5) to (8) repeat the same analysis controlling for absences in October 2015 reported by parents at baseline, showing that results are robust.

In addition, if the effect of the weekly attendance reports was due to improved parental information about their daughter's attendance, then one would expect the effect of this treatment to be larger where the probability of a child's absence going unreported to the parent is higher. *Ceteris paribus*, this probability should decrease with the size of the school since staff-per-pupil and social control are likely to be lower in larger schools. Indeed, we find that when schools are weighted by their share in the EP2 school population rather than having equal weight irrespective of their EP2 population, the effect of the information treatment increases from 4.5%-points to 6.15%-points (Table 11).

Population-weighted estimates. The main analysis reported in this paper is carried out at the school level (i.e., averaging variables at the school level) without applying any sampling weights, so that each school is weighted equally whatever the number of girls interviewed in the household survey or observed during the spot checks. We also repeated the analysis but weighting each school by the relative size of its potential EP2 intake, to obtain population-weighted estimates.[31]

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[31] More precisely, we apply to each school a weight equal to the school's population share divided by 1/173 (the school's sample share), where the

Results are largely unchanged, with a few small differences. First, the effect of the information treatment on attendance increases (from 4.5%-points to 6.1%-points), which, as discussed above, is consistent with the idea that the quality of monitoring may be worse in larger schools at baseline. Despite all treatment arms providing the same attendance information to parents, there is no similar increase in the effect of the transfers treatments when weighting schools proportionally to their potential (pre-treatment) enrollment. This suggests either that the effect of incentives is smaller in larger schools (e.g., because the population served by these schools is wealthier, which is the case), or that the information component of the conditional transfers treatments may be less salient than when provided on its own.[24] A second difference in results compared to the unweighted case is that, despite the larger increase in attendance in response to the information treatment compared to the unweighted estimates, the effect on the math score is slightly smaller in magnitude (0.15 instead of 0.18) and just statistically insignificant at the 10% significance level. A final difference between the weighted and unweighted estimates is that the population-weighted estimates of the negative effect of each treatment on the indicator variable for high girl empowerment increase in magnitude, although the smallest p-value remains slightly above 0.10 (0.12).

V- Conclusion

Regular school attendance is widely believed to be important to support sustained learning (Aucejo and Foy Romano, 2016; Robinson et al., 2017). Child absenteeism is therefore understood to be detrimental both from the pupil's point of view and from the point of view of the efficient functioning of education systems, which motivates governments around the world to spend vast amounts of tax-payer money incentivizing parents to ensure regular school

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population is defined as all girls who, based on the 173 schools' records for 2013-2015, are eligible to enroll in Grade 6 or 7 in 2016.

attendance – e.g., 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 part of the effect of CCTs may come from the information value of the conditional transfer, and (ii) incentivizing children may be more effective than incentivizing parents. We carried out a randomized controlled trial in 173 schools of Manica province, Mozambique, where we evaluate the effectiveness of providing weekly attendance reports to parents of girls in senior primary school and compare this with the effectiveness of (i) a conditional transfer program implicitly offering parents the same information as well as incentivizing a 90% and above attendance rate by giving cash to the parents of eligible girls and (ii) a conditional transfer program implicitly offering parents the same information as well as incentivizing a 90% and above attendance rate by giving vouchers to the eligible girls.

We find evidence that providing high-frequency information to parents about their daughter's school attendance increases school attendance, and that this effect is not statistically distinguishable from that of conditional transfers to parents providing the same information. Incentivizing girls with vouchers for them to buy a choice of goods is at least as effective as incentivizing parents with the cash-equivalent of these vouchers – and importantly, *not* because parents were able to appropriate the vouchers or reallocate household expenditure to cancel out the transfer to their daughters. In terms of learning, the attendance gains from the information and girls' incentives treatments translated into substantial improvements in scores on a math test, but not the attendance gains from the traditional CCT treatment. None of the treatments had a robust effect on enrollment (as would be expected given the timing of the treatment announcement), teacher attendance, early marriage, self-reported quality of parental monitoring and self-reported girl autonomy. These conclusions hold when using randomization inference, are confirmed by joint tests based on randomization inference, are robust to controlling for baseline characteristics including baseline outcomes, to weighting observations to take into account small differences in survey sample attrition between treatment arms (which might otherwise bias our estimates of the effect of the treatments on outcomes other than school attendance), are not driven by selection into treated schools in response to the treatments, and are largely unchanged when weighting each school by the size of its potential intake (although the information treatment appears to be more effective in raising attendance in larger schools).

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 self-reported absences is significant and more than doubles (and even reaches 72% of the expected coefficient under perfect information in the CCT arm).

These findings have important policy implications. First, they suggest that simply providing weekly information to parents using an easily scalable paper and pen method can be an effective intervention to reduce child absenteeism at school. Second, our findings indicate that, where budget constraints allow financial incentives for attendance to be considered, it would be beneficial to increase (and make more salient) the information component of the intervention, and, if some information frictions remain, it may be more cost-effective to incentivize children with a voucher system than incentivizing parents with cash (provided that transport and other logistical costs of the voucher system relative to cash transfers do not outweigh the benefit from incentivizing children). 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 (2016), Bergman and Chan (2017), Berlinsky et al 2017, Bursztyn and Coffman (2012), and Rogers and

Feller (2016) from middle- to high-income urban country 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.

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Tables and Figure

Figure 1: Timeline of Intervention and Data Collection

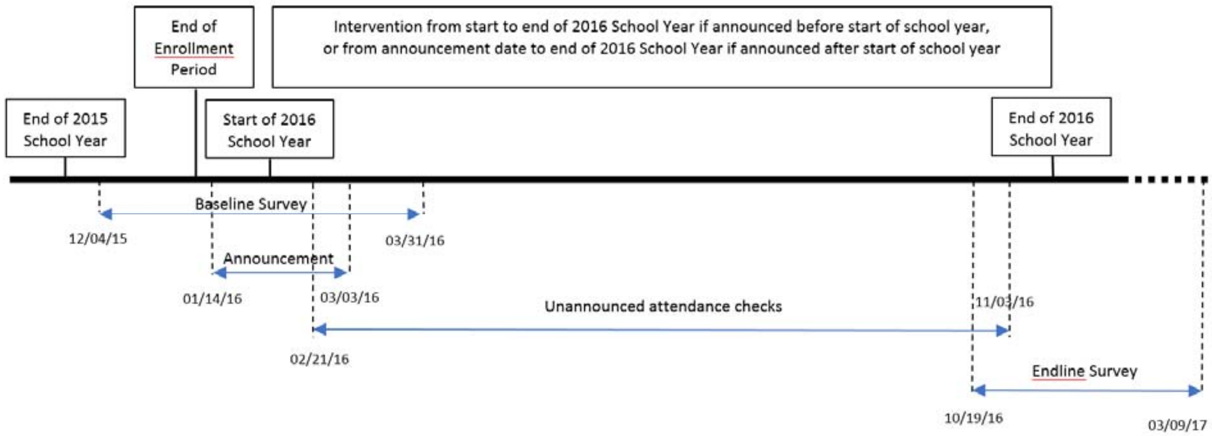

Table 1: Sample Sizes and Attrition

	Control	Girl Vouchers	Parent Cash	Information	Total
# 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: Descriptive Statistics and Balance at Baseline

	(1) Control mean	(2) Information mean	(3) Parent Cash mean	(4) Girl Voucher 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:				
Self-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 personally consumed (purchased or not) by girls who, if they were to enroll in 2016, would enroll in Grades 6 or 7, over the 12 months preceding the baseline survey. [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, self-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 3 Effect on Schooling Outcomes

	(1) Share present at attendance check	(2) Share enrolled	(3) Average ASER Math score	(4) Share present at attendance check	(5) Share enrolled	(6) Average ASER Math score
Information	0.0450**	0.00662	0.183**	0.0488**	0.00483	0.195**
	(2.00)	(0.44)	(2.01)	(2.04)	(0.30)	(2.14)
Parent Cash	0.0599***	0.0272*	0.0202	0.0588**	0.0196	-0.00233
	(2.70)	(1.84)	(0.23)	(2.49)	(1.25)	(-0.03)
Girl Voucher	0.0829***	-0.00331	0.203**	0.0841***	-0.00731	0.178*
	(3.74)	(-0.22)	(2.27)	(3.53)	(-0.46)	(1.97)
Constant and District FE	Yes	Yes	Yes	Yes	Yes	Yes
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. 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. T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table 4 Effect on Non-Schooling Outcomes

	(1) Teacher present?	(2) Ever married	(3) High self-reported monitoring quality	(4) High self-reported empowerment	(5) Teacher present?	(6) Ever married	(7) High self-reported monitoring quality	(8) High self-reported empowerment
Information	0.0305	-0.0174*	0.00937	-0.0209	0.0428	-0.0127	-0.00111	-0.0218
	(1.17)	(-1.73)	(0.36)	(-0.54)	(1.53)	(-1.19)	(-0.04)	(-0.55)
Parent Cash	0.0258	-0.00956	0.0319	0.00203	0.0241	-0.00958	0.0312	-0.00227
	(1.01)	(-0.97)	(1.25)	(0.05)	(0.88)	(-0.91)	(1.20)	(-0.06)
Girl Voucher	0.00739	-0.00401	0.00664	-0.0356	0.0168	-0.000814	-0.00405	-0.0342
	(0.29)	(-0.41)	(0.26)	(-0.94)	(0.61)	(-0.08)	(-0.15)	(-0.87)
Constant and district FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Characteristics	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). 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. T-statistics in parentheses, * $p<0.10$ ** $p<0.05$ *** $p<0.01$.

Table 5: Results based on Randomization Inference

Table	Baseline Characteristics?	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 3	No	Present at spot check	0.043	0.006	0.001	0.004		
	No	Self-reported enrollment	0.633	0.067	0.824	0.161		
	No	ASER score	0.047	0.819	0.032	0.050		
	Yes	Present at check	0.037	0.014	0.001	0.005		
	Yes	Self-reported enrollment	0.744	0.215	0.652	0.365		
	Yes	ASER score	0.036	0.982	0.053	0.041	0.034	
Table 4	No	Teacher present	0.232	0.315	0.768	0.613		
	No	Ever married	0.073	0.340	0.657	0.348		
	No	High self-reported monitoring quality	0.722	0.216	0.803	0.639		
	No	High self-reported empowerment	0.571	0.963	0.353	0.709		
	Yes	Teacher present	0.134	0.385	0.545	0.513		
	Yes	Ever married	0.220	0.362	0.923	0.552		
	Yes	High self-reported monitoring quality	0.965	0.234	0.862	0.502		
	Yes	High self-reported empowerment	0.583	0.960	0.386	0.781	0.491	0.085

Authors calculations using Alwyn Young's randcmd program. 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 6: Inverse Probability Weighting Attrition Correction

	(1)	(2)	(3)	(4)	(5)
	ASER math score	Self-reported enrollment	Ever married	High self-reported monitoring quality	High self-reported empowerment
Panel A: No controls for baseline characteristics					
Information	0.174*	0.0000403	-0.00792	-0.0146	-0.0263
	(1.83)	(0.00)	(-0.75)	(-0.50)	(-0.63)
Parent Cash	0.0256	0.0319**	-0.00221	0.0270	-0.0000893
	(0.27)	(2.02)	(-0.21)	(0.94)	(-0.00)
Girl Voucher	0.188**	-0.00797	0.0115	-0.00970	-0.0400
	(2.00)	(-0.50)	(1.11)	(-0.34)	(-0.97)
Panel B: Controlling for baseline characteristics					
Information	0.184*	0.000310	-0.00424	-0.0297	-0.0249
	(1.90)	(0.02)	(-0.38)	(-0.98)	(-0.57)
Parent Cash	0.0107	0.0256	0.00179	0.0235	0.000416
	(0.11)	(1.52)	(0.16)	(0.78)	(0.01)
Girl Voucher	0.162*	-0.00792	0.0156	-0.0238	-0.0343
	(1.70)	(-0.47)	(1.42)	(-0.80)	(-0.80)
Observations	173	173	173	173	173

Source: Household survey. All regressions include a constant and district fixed effects. School averages 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 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. T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table 7: Effect on Attendance, Sample Restricted to Girls Registered at First Spot Check

	(1) Share present at attendance check	(2) Share present at attendance check
Information	0.0419*	0.0455*
	(1.84)	(1.88)
Parent Cash	0.0604***	0.0592**
	(2.69)	(2.47)
Girl Voucher	0.0810***	0.0823***
	(3.60)	(3.41)
Constant and District FE	Yes	Yes
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). 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. T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table 8: Ex-Post Power Calculations

Outcome	Mean control group	SD control group	MDE	MDE as % of the Mean
Present at spot check	0.65	0.1283	0.078	12%
Self-reported enrollment	0.95	0.087	0.053	6%
ASER score	2.16	0.5671	0.343	16%
Teacher present	0.9	0.1529	0.092	10%
Ever married	0.03	0.0476	0.029	96%
High self-reported monitoring quality	0.89	0.1667	0.101	11%
High self-reported empowerment	0.3	0.2283	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 two-by-two comparisons between the parent cash, girl vouchers, and control groups). Calculations applying to two-by-two 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 9: ANCOVA Estimates

	(1)	(2)	(3)	(4)	(5)
	Share present at spot check	Self-reported enrollment	Ever married	High self-reported monitoring quality	High self-reported empowerment
Information	0.0431*	0.00204	-0.00623	0.0121	-0.0198
	(1.91)	(0.14)	(-1.49)	(0.47)	(-0.52)
Parent Cash	0.0559**	0.0231	-0.000547	0.0357	-0.000994
	(2.48)	(1.63)	(-0.13)	(1.39)	(-0.03)
Girl Voucher	0.0778***	-0.00160	-0.000183	0.00860	-0.0341
	(3.43)	(-0.11)	(-0.04)	(0.34)	(-0.90)
Self-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)
Constant and District FE	Yes	Yes	Yes	Yes	Yes
Observations	173	173	173	173	173

Source: Household survey, except for the outcome variable in the first column, which comes from the attendance spot checks data. Self-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). T-statistics in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table 10: 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
Self-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)
Self-reported missed school days in October 2015					0.0158	0.00114	-0.000604	0.0187*
					(1.60)	(0.12)	(-0.04)	(1.92)
Constant and District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	473	406	428	482	458	391	416	469
Mean Y	0.32	0.31	0.27	0.26	0.31	0.31	0.28	0.26

Source: Household survey (number of child absences self-reported by the parent) and independent attendance spot checks (outcome variable). Sample sizes are slightly smaller in columns (5) to (8) due to some girls not being enrolled in 2015. T-statistics based on standard errors clustered at the school level in parentheses, * p<0.10 ** p<0.05 *** p<0.01.

Table 11: Population-Weighted Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Present at spot check	Self-reported enrollment	ASER score	Teacher presence	Ever married	High self-reported monitoring quality	High self-reported empowerment
Information	0.0615**	0.0102	0.150	0.0488	-0.0136	0.0268	-0.0680
	(2.26)	(0.71)	(1.51)	(1.60)	(-1.59)	(1.03)	(-1.43)
Parent Cash	0.0598**	0.0251*	0.00241	0.0346	-0.0135	0.0261	-0.0279
	(2.44)	(1.96)	(0.02)	(1.14)	(-1.53)	(1.07)	(-0.62)
Girl Voucher	0.0819***	-0.00732	0.212***	0.0264	-0.00333	0.00217	-0.0712
	(3.49)	(-0.44)	(2.75)	(0.95)	(-0.32)	(0.08)	(-1.56)
Constant and District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Characteristics	No	No	No	No	No	No	No
Observations	173	173	173	173	173	173	173
No. of clusters							
Mean Y	0.65	0.95	2.15	0.88	0.03	0.89	0.31
SD Y	0.1227	0.0687	0.5294	0.1600	0.0430	0.1112	0.2251
p info=parents	0.947	0.204	0.285	0.594	0.986	0.974	0.286
p info=girls	0.405	0.278	0.477	0.346	0.250	0.275	0.934
p girls=parents	0.320	0.031	0.064	0.725	0.294	0.283	0.248

Source: Outcome variables for Columns (1) and (4): unannounced spot checks attendance data. All other outcome variables: household survey (endline). We apply to each school a weight equal to the school's population share divided by 1/173 (the school's sample share), where the population is defined as all girls who, based on the 173 schools' records for 2013-2015, are eligible to enrol in Grade 6 or 7 in 2016, i.e., girls enrolled at some point during 2013-2015 and last enrolled in, at least, Grade 5 and, at most, Grade 6.

Appendix

A. 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. 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.

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 (77%), 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 as an indicator equal to one if the girl has a match in our 2016 class rolls and zero if not, the coefficients associated with the girls vouchers arm is 0.03 (p-value: 0.002), that associated with the parents cash arm is 0.029 (p-value: 0.003), and that associated with the information arm is -0.021 (p-value: 0.032). 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 enrollees from the household survey with those found in school records, it seems that analyzing the effect of the treatments on unconditional attendance would be a bad “cure” to solve a non-existent problem.

B. Appendix Table

Table A-1: Effect of Treatments on Eligible Girls' Consumption of Personal Items

	Dependent Variable: Consumption of Personal Items Not Purchased With Vouchers (Meticais)	
	(1) All observations	(2) Top 1% removed
Information	19.55	47.52
	(0.27)	(0.73)
Parent Cash	-50.08	-41.66
	(-0.70)	(-0.65)
Girl Voucher	-68.40	-89.18
	(-0.95)	(-1.39)
Constant and District FE	Yes	Yes
Observations	173	173
Mean Y	831.69	783.72
SD Y	517.9240	462.0591
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, school bag, travel bag/handbag, batteries, magazines/newspapers, any other good for personal use (e.g., hair extensions, etc…).