Committed to Education: School Fees, Vouchers, and the Timing of Cash Transfers

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Abstract

Primary-school dropout remains widespread in Sub-Saharan Africa, and school fees are seen as a major barrier to completion. In an experiment with 2,000 parents and children in Uganda, we study whether time inconsistency contributes to unpaid school fees and lower school participation. We first elicit parents' willingness to pay for two restrictions on a cash transfer: (1) delaying it until fees are due and (2) earmarking it as a voucher paid directly to the school, holding timing fixed. Demand for both restrictions is substantial: 62 percent of parents are willing to pay for delay and 69 percent for earmarking. We then randomly assign parents to three treatment arms and a control group. The three arms provide the same amount as cash six weeks before fees are due, as cash when fees are due, or as a voucher, and the control group receives no transfer. The voucher more than doubles fees paid, increases school attendance, and cuts within-term dropout by more than half. Cash has smaller effects on all three outcomes, with no additional effect from delayed cash over early cash. Perhaps surprisingly, the voucher's effects on fee payment and attendance are largest among parents with low willingness to pay for it, in part because they substantially underestimate how much it will raise their fee payment relative to cash. In a model of quasi-hyperbolic discounting, these findings are consistent with partial naïveté, in which parents anticipate their own time inconsistency but underestimate its severity.

JEL Codes: C93, D91, I21, I25, O15
Keywords: commitment devices, present focus, school fees, cash transfers, in-kind transfers, education, Uganda

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1 Introduction

Although private returns to schooling are higher in low-income countries than in high-income ones, children in these countries complete far fewer years of school.[1] Despite progress in expanding access to primary education, including the abolition of tuition in many low- and middle-income countries, about one in three children in Sub-Saharan Africa still do not complete primary school (United Nations, 2023; World Bank, 2024). One possible explanation is that abolishing tuition has not eliminated the costs of schooling. Schools continue to levy fees for expenses not fully covered by government funding, and these fees remain an important barrier to participation.[2] Many of these fees recur at predictable points in the school year, yet they often go unpaid, putting children at risk of suspension until their balances are cleared.

Why do families fail to make these payments? One explanation is that parents with limited resources rationally choose to spend their money on other needs, weighing the value of current consumption against the future benefits of schooling (Björkman-Nyqvist, 2013; Dillon, 2021). But paying school fees is also an intertemporal problem. A parent may plan to set aside money for fees but spend it before the deadline, or redirect it to other needs when the fees come due. Time-inconsistent preferences can lead to these reversals (Strotz, 1955; Laibson, 1997; O'Donoghue and Rabin, 1999), because spending on current needs offers an immediate benefit while the benefits of paying school fees lie in the future. Parents who anticipate failing to follow through may demand commitment devices that restrict their future saving or spending decisions. Yet, little is known about the role of self-control problems in parental investments in education.

In this paper, we ask three interrelated questions. First, do parents anticipate their own self-control problems in paying school fees, and are they willing to pay to restrict their future use of money? Second, are these self-control problems a barrier to fee payment and children's schooling? And third, does parents' demand for commitment predict how much it affects their children's schooling? We study these questions in a field experiment with 2,000 parents and children in 30 government primary schools in eastern Uganda, where schools charge fees each term for children to attend.[3] The consequences of unpaid fees are substantial: in our sample, 87 percent of parents report that their child had been suspended from school over late or unpaid fees in the previous year.

[1] On returns to schooling, see Montenegro and Patrinos (2014) and Psacharopoulos and Patrinos (2018); on educational attainment, see Barro and Lee (2013).

[2] On fees following tuition abolition, see Deininger (2003); Nishimura et al. (2008); on schooling costs and participation, see Baird et al. (2011); Duflo et al. (2021).

[3] Uganda abolished tuition in government primary schools under Universal Primary Education in 1997, but schools continue to charge fees for other expenses (Deininger, 2003).

In the experiment, we introduced a transfer of UGX 35,000 (USD $26 PPP), approximately 85 percent of the average term fee, which parents could receive as cash within a day of the baseline interview (the *Early Cash* arm), cash six weeks later when fees were due (the *Fee-Day Cash* arm), or a voucher paid directly to the school (the *Voucher* arm). Before random assignment, parents chose between Early Cash and varying amounts of Fee-Day Cash, and between the Voucher and varying amounts of Fee-Day Cash. These choices measure parents' willingness to pay for two forms of commitment: *delaying* the transfer prevents parents from spending it before fees are due, while *earmarking* it also prevents them from redirecting it to other needs when fees come due. Importantly, we hold transaction costs fixed across transfers, and a randomly selected 2 percent of parents received the option implied by their choices at a randomly drawn price, making the choices incentive compatible.[4]

We then randomly assigned the remaining 1,960 parents to Early Cash, Fee-Day Cash, the Voucher, or no transfer. This allows us to estimate the causal effects of providing financial assistance on children's schooling and examine how the design of the transfer itself affects parents' fee-payment decisions. Because assignment did not depend on parents' choices, we can also test whether allowing parents to choose between cash and the Voucher would direct the Voucher to the children whose schooling it raises most. We measure outcomes using school administrative records, unannounced attendance visits, and an endline survey of children and parents, in which we also re-elicit demand for a future voucher.

Our first finding is that many parents are willing to sacrifice substantial amounts of money to restrict their own choices. Sixty-nine percent of parents give up some cash to have the transfer paid directly to the school rather than receive the same amount in cash on the same day, and 62 percent give up cash to receive it when fees are due rather than immediately. Mean willingness to pay is 35 percent of the transfer for earmarking and 24 percent for delay.[5] The earmarking premium, about UGX 12,000 (USD $9 PPP), is close to a day of median household consumption in the region. These choices are difficult to reconcile with time-consistent preferences: cash weakly dominates an in-kind transfer of the same amount, since households can make the same purchase themselves while retaining the option to spend the money elsewhere (Moffitt, 1989; Currie and Gahvari, 2008). Similarly, absent constraints on saving, parents should prefer receiving cash earlier to paying to receive it later (Frederick et al., 2002).

Parents' explanations for their choices in the baseline survey are consistent with anticipated self-control problems. Many describe the temptation to spend cash on other things

[4]The Voucher required a trip to the school, and each cash transfer included a small top-up to cover mobile-money withdrawal fees.

[5]These means account for censoring at the bounds of the elicitation grid (Section 4.1).

before paying school fees. As one parent explains, "I wouldn't want to be tempted to use the money to buy nice food like fish instead of paying it for school." We find less support for alternative explanations: demand is not systematically related to household disagreement over school fees, redistributive pressures from kin, concern about theft, expected inflation, or social-desirability scores. Together, these patterns suggest that parents pay for the restrictions to change their own spending, rather than to protect the money from other household members or outside claims.

Our second finding is that, although parents are willing to pay for both restrictions, only earmarking substantially increases fee payments. Early Cash and Fee-Day Cash raise the share of the term fee paid by 11 to 12 percentage points, from a control mean of 37 percent, and their effects are statistically indistinguishable ($p = 0.86$). Thus, delivering cash when fees are due does not lead to greater fee payment than providing it six weeks earlier. The Voucher, in contrast, more than doubles total fees paid and raises the share of the term fee paid by 60 percentage points ($p < 0.001$). The large difference between Fee-Day Cash and the Voucher suggests that having money available when fees are due is not enough to ensure that parents use it to pay them.

These differences in fee payment have consequences for children's schooling. By the end of the term, the Voucher reduces dropout by more than half, from 5.1 percent in the control group to 2.4 percent ($p < 0.05$). Early Cash and Fee-Day Cash reduce dropout by 1.1 and 1.5 percentage points, respectively. Neither effect is statistically significant, although we cannot rule out effects as large as the Voucher's.

The transfers also improve school attendance. Weekly unannounced school visits show that Fee-Day Cash and the Voucher raise attendance by 3 and 5 percentage points, respectively, from a control mean of 77 percent ($p < 0.05$ and $p < 0.01$). The effect of Early Cash is smaller and not statistically significant. Most of these gains occur among children who remain enrolled throughout the term, consistent with reductions in sanctions for unpaid fees. All three transfers reduce the frequency with which children are suspended from school or denied examinations, school meals, or report cards ($p < 0.05$ for each). We find no detectable gains in child literacy or numeracy over the term.

The Voucher's larger effects on fee payment and attendance could nevertheless come at a cost. Restricting how parents spend the transfer reduces their flexibility, a standard argument for cash over in-kind transfers (Moffitt, 1989; Currie and Gahvari, 2008) and a cost of commitment in models of self-control (Amador et al., 2006). Yet this does not appear to be a leading concern in this setting. On the outcomes we observe, Voucher households fare no worse than cash households on average: food insecurity and disagreement over school fees fall by similar amounts across the transfer arms, and we find no detectable changes in household

assets. Both cash transfers increase borrowing, whereas the Voucher does not, although our one-term follow-up cannot capture the longer-run consequences of this borrowing.

If the Voucher had left more pressing needs unmet, parents who received it might be willing to pay less for it after experiencing it. Instead, experience with the Voucher raises demand for it: at endline, after one term with their assigned transfer, we again asked parents to choose between a Voucher and Fee-Day Cash for a subsequent term. Seventy-seven percent of Voucher recipients are willing to pay at least as much for the Voucher as at baseline. Relative to control parents, assignment to the Voucher raises the endline premium by UGX 4,000 ($p < 0.001$).[6] On average, parents who received the Voucher show little sign of regretting the restriction.

Our third finding is that, perhaps surprisingly, greater demand for commitment does not predict larger effects on children's schooling. Because in-kind transfers are often seen as paternalistic (Currie and Gahvari, 2008), a natural compromise is to let recipients choose between cash and in-kind support. Allowing parents to choose could also improve targeting if parents who prefer the Voucher are those for whom it would increase fee payments and schooling most (Roy, 1951; Alatas et al., 2016; Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2019). Because we elicited demand before random assignment, we can test this directly. We find the opposite for fee payments: the Voucher has significantly larger effects among parents who prefer cash to a Voucher of the same amount. Relative to Fee-Day Cash, it raises their fee payments by UGX 23,900, compared with UGX 15,100 among parents who prefer the Voucher ($p = 0.001$). We find a similar, though less precise, pattern for attendance: the Voucher raises attendance by 5.3 percentage points among parents who prefer cash, compared with 0.1 percentage points among those who prefer the Voucher ($p = 0.12$ for the difference). Letting parents choose would therefore direct the Voucher away from some of the children whose schooling it improves most.

Why does demand fail to predict treatment effects? One possibility is that parents misjudge how much commitment would change their behavior. To examine this, we compare parents' beliefs about the effects of each transfer, elicited before random assignment, with the effects we observe experimentally. Parents' willingness to pay is related to their expectations: those who expect earmarking to have a larger effect on fee payment are willing to pay more for it. Yet parents substantially underestimate how much earmarking actually changes their fee payments. Before random assignment, parents expected to clear their child's fees in full with a probability of about 74 percent under both Fee-Day Cash and the Voucher.[7] In practice,

[6] Because many Voucher recipients reach the upper bound of the elicitation grid, observed premiums understate the increase in demand. Accounting for censoring raises the estimated effect to UGX 10,800 (Section 6.3).

[7] Forecasts concern full fee clearance, but the Voucher covers only part of the fee. Parents may therefore

only 15 percent of Fee-Day Cash recipients clear their fees, compared with 50 percent of Voucher recipients. Parents thus underestimate the effect of earmarking on clearing fees in full by 34 percentage points ($p < 0.001$).[8] Among parents who prefer cash, only 10 percent clear their fees when assigned Fee-Day Cash, although they forecast clearing fees as often as parents who prefer the Voucher.

To interpret these findings, we develop a model of school-fee payment with quasi-hyperbolic discounting and uncertain income (Laibson, 1997; O'Donoghue and Rabin, 1999).[9] In the model, a parent makes two decisions: how much to set aside before school fees are due, and whether to pay when they come due. Even a parent who initially judges that schooling is worth the fee may depart from this plan at either point because of present focus. Fee-Day Cash prevents parents from spending the transfer before fees are due, while the Voucher also removes the decision of whether to use it to pay the school. Willingness to pay for a restriction thus implies that parents anticipate difficulty following through on their plans. Following O'Donoghue and Rabin (2001), we distinguish the parent's *actual* present focus, which governs behavior, from *perceived* present focus, which governs forecasts of that behavior and hence willingness to pay. Demand for commitment and its effects can therefore diverge when parents misjudge their own present focus. Our findings are consistent with partial naïveté: parents anticipate self-control problems, but underestimate their severity (O'Donoghue and Rabin, 2001).

This paper contributes to three literatures. First, we contribute to the literature on schooling costs and participation. Previous studies show that in-kind and conditional transfers can increase school participation (Kremer et al., 2009; Baird et al., 2011; Barrera-Osorio et al., 2011; Benhassine et al., 2015; Evans and Ngatia, 2021), but typically assign subsidies without eliciting recipients' preferences over how they are provided. We instead elicit parents' willingness to pay for different forms of the same transfer, varying whether it is earmarked for school fees and when it arrives, before randomly assigning the transfers. In-kind transfers are often viewed as restrictions imposed on households to direct spending toward policymakers' objectives (Moffitt, 1989; Currie and Gahvari, 2008; Cunha et al., 2019; Lieber and Lockwood, 2019). Our results suggest that most parents choose this restriction themselves, and that paying the school directly can help them carry out educational investments they already plan to make.

value increased partial payment, which can prevent sanctions, even if they expect no change in full clearance (Section 5).

[8]Both forecasts concern equal transfers delivered on the same day, so common errors about future income or expenses cannot explain the expected difference (Augenblick et al., 2026). The Voucher's advantage remains about 30 percentage points among households reporting no negative shock.

[9]The baseline model assumes parents cannot borrow against future income. Appendix B.5 relaxes this assumption by allowing borrowing at a high interest rate.

Second, we contribute to the literature on self-control and commitment, which documents demand for commitment devices for savings, smoking, work effort, farm inputs, and borrowing (Ashraf et al., 2006; Giné et al., 2010; Duflo et al., 2011; Dupas and Robinson, 2013; Kaur et al., 2015; Schilbach, 2019; Allcott et al., 2022; Carrera et al., 2022). We provide new evidence on parents' willingness to pay for commitment in education. In a related study, Bursztyn and Coffman (2012) find that parents pay to retain attendance conditions that discipline their children, which reflects demand for control over the child's behavior rather than the parent's own. In Uganda, Karlan and Linden (2025) find that children save less in accounts that restrict withdrawals to educational purchases than in unrestricted accounts, so the restriction affects how much is saved as well as how it is spent. Because our transfers have a fixed size, parents' choices measure demand for the restriction itself. Our results suggest that time inconsistency contributes to unpaid school fees and lower school participation, even among parents who anticipate the problem. Because parents make these investments on behalf of their children, the consequences of their self-control problems may extend to the next generation.

Third, we contribute to work on self-selection and targeting, which asks whether individuals who take up or pay for a program are those whose behavior responds most to it (Ashraf et al., 2010; Cohen and Dupas, 2010; Jack, 2013; Alatas et al., 2016; Walters, 2018; Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2019; Shapiro, 2020; Berry et al., 2020; Campos et al., 2025). The closest studies elicit willingness to pay and then randomize allocation to ask whether demand predicts use (Ashraf et al., 2010; Berry et al., 2020). We ask whether demand predicts treatment effects and use elicited beliefs to study why the two diverge. Within the commitment literature, Kaur et al. (2015) find that workers with the largest payday effects choose commitment contracts more often, whereas Carrera et al. (2022) find little association between take-up and actual or perceived time inconsistency (see also John, 2020; Bai et al., 2021). Because we assign the restriction independently of demand, we observe its effects among parents who would have declined it, and find that greater demand for earmarking predicts smaller effects. Our results suggest that misprediction of one's own behavior can break the link between demand and effects, limiting what voluntary take-up can accomplish as a targeting device.

The remainder of the paper proceeds as follows. Section 2 describes the setting and experimental design. Section 3 presents a conceptual framework. Section 4 examines demand for earmarking and delay, and Section 5 reports the effects of the transfers on fee payment, schooling, and the household. Section 6 asks whether demand identifies the children whose schooling responds most to the Voucher, compares anticipated with realized effects, and studies how experience changes demand. The final section concludes.

2 Setting and Experimental Design

2.1 School Fees in Uganda

Uganda introduced Universal Primary Education (UPE) in 1997, abolishing tuition fees in government (public) primary schools. Primary enrollment rose from about 3.1 million pupils in 1996 to 5.3 million in 1997 (UNESCO, 2026), and gender and income gaps in access narrowed (Deininger, 2003; Grogan, 2009). By 2025, Uganda had 9.1 million primary pupils, 57 percent of them in government schools (Uganda Bureau of Statistics, 2026). Yet many children still leave school before completing primary education. Of the nearly 1.9 million children who entered Primary 1 in 2016, only about 832,000 (44 percent) went on to sit the Primary Leaving Exam at the end of Primary 7 (Etukuri and Kwesiga, 2023).

One reason is that schooling remains costly despite the abolition of official tuition. The capitation grants that replaced tuition cover only part of schools' operating expenses, and schools commonly charge informal term fees for meals, examinations, and support staff (Nishimura et al., 2008), on top of the scholastic materials and uniforms that parents must also buy. Schools charge fees at the start of each of the three terms, usually as a lump sum paid in cash.[10] In our study schools, the average term fee is UGX 48,000 and fees rise with grade (Supplementary Appendix Figure A1). In practice, many households pay late or do not pay at all. In the 2019/20 Uganda National Household Survey, lack of funding was the most common reason given for leaving school among people aged 6 to 24, cited by 67 percent of boys and 62 percent of girls (UNICEF Uganda, 2024).

Unpaid fees have immediate consequences for children. Headteachers collect the fees, and schools may exclude children from classes or school services until their parents pay. At baseline, 87 percent of parents reported that their child had been suspended over unpaid fees in the previous 12 months. Sixty percent reported that their child had been denied school meals, 55 percent that the child had been required to start the term late or sit outside class, 39 percent that the child had been denied a report card, and 32 percent that the child had been barred from an examination (Figure 1).

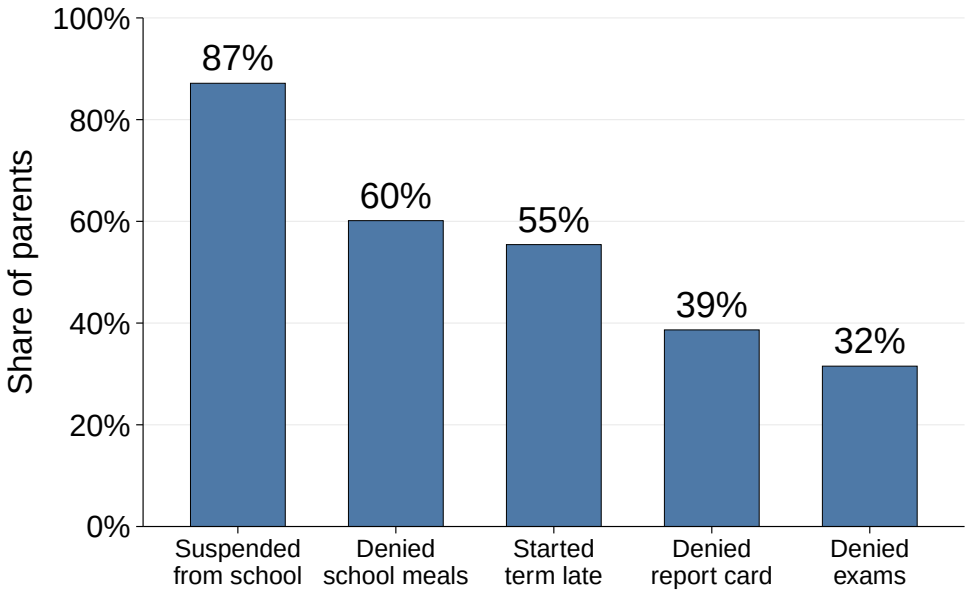
2.2 Sample

We conducted the study in Jinja District, a predominantly rural part of the Busoga sub-region in eastern Uganda.[11] We randomly selected 30 schools from the district's 50 eligible

[10] Parents can sometimes provide food, typically maize or beans valued at market prices, instead of paying the feeding component of the fees in cash.

[11] In 2020 the urban core of the area was carved out as the separate Jinja City; the surrounding Jinja District, where this study takes place, remains predominantly rural, with about 281,000 residents across six

Figure 1: Consequences of Late or Unpaid Fees at Baseline



Notes: This figure shows the share of parents who report that their child experienced each consequence of late or unpaid fees in the 12 months before the baseline survey. Shares exclude "don't know" responses (N = 1,997–2,000 per item).

government primary schools.[12] Within each school, we sampled pupils enrolled in Primary 4 or Primary 6 in Term 3 of 2025, stratifying by school, grade, and gender and selecting at most one child per household.[13] Our final sample includes 2,000 children and their primary guardians. We call these guardians "parents" throughout the paper, although some are grandparents or other relatives.

We interviewed both the child and the parent, beginning with the child. When we could not reach a selected household, we used the next household on a pre-randomized replacement list, as specified in the pre-analysis plan.[14]

2.3 Timeline and Transfers

Figure 2 summarizes the study timeline. We conducted baseline interviews between December 13, 2025, and January 12, 2026, during the school holiday before Term 1. The survey followed the November–December harvest; the region's other main harvest occurs in June–

sub-counties at the 2024 census (Uganda Bureau of Statistics, 2024).

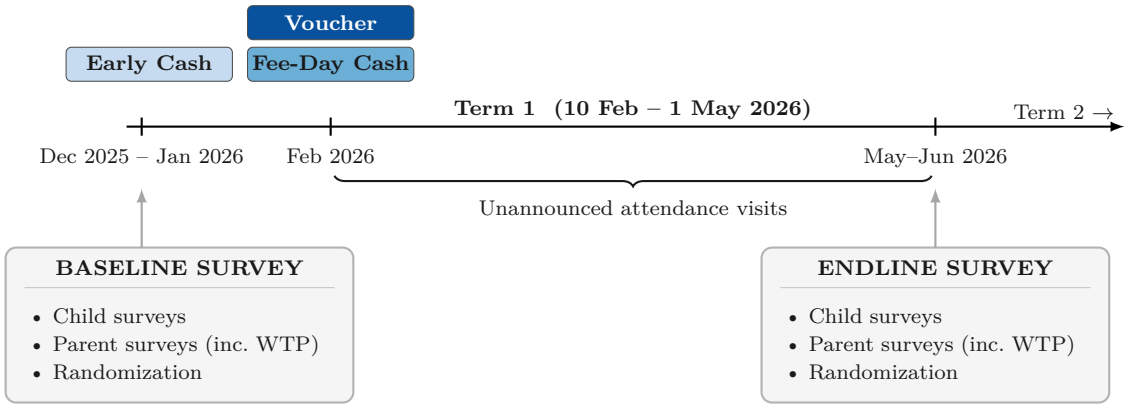
[12]One school was excluded because it is a boarding school, and eight others were excluded for administrative reasons.

[13]We oversampled girls, with a target of 60 percent girls in each school and grade. Some schools did not have enough girls in a grade to meet the target, so girls make up 57 percent of the final sample.

[14]Replacements were triggered when a household could not be located, the child had moved away, or the primary guardian declined to participate. Each replacement was the next pre-randomized name in the same school–grade–gender cell.

July. Parents were interviewed an average of five weeks before the scheduled fee date, with the interval ranging from three to seven weeks. Term 1 was originally scheduled to begin on February 2, although the government later postponed the start of term by eight days (Section 2.5).

Figure 2: Study Timeline



Notes: The start of Term 1 was postponed from February 2 to February 10, 2026, after baseline data collection. Fee-Day Cash was paid on February 2, except in the timing sub-experiment, and the Voucher on February 10 (Section 2.5).

We offered three transfers of UGX 35,000 that varied in timing and whether the parent could spend the money outside school fees. The amount is equivalent to about US$26 at purchasing-power parity, 85 percent of a child's term fee, and 8 percent of median monthly household consumption in Busoga in 2019/20 (Uganda Bureau of Statistics, 2020). We randomly assigned 1,960 parents to one of four groups. Parents in the *Control* group ($N = 495$) received no transfer. Parents assigned to *Early Cash* ($N = 489$) received an unconditional mobile-money transfer within 24 hours of the baseline interview. Those assigned to *Fee-Day Cash* ($N = 490$) were scheduled to receive the same amount on February 2, the original first day of Term 1. The *Voucher* ($N = 486$) paid UGX 35,000 directly to the child's 2025 school at the start of term, provided the child re-enrolled there.[15]

The three transfers allow us to compare two restrictions on the same amount of money. Early Cash and Fee-Day Cash were both unrestricted, so comparing them measures the effect of delaying the transfer. Fee-Day Cash and the Voucher were scheduled for the same day, but only the Voucher had to be spent on school fees. Their comparison was therefore designed to measure the effect of earmarking. Comparing the Voucher with Early Cash gives the combined effect of delay and earmarking. We also compare each transfer with the Control group to estimate the effects of receiving that form of support rather than no transfer.

[15]We confirmed re-enrollment with the school before making the payment.

The remaining 40 parents (2 percent of the sample) formed the *Choice group*. Rather than receive a randomly assigned transfer, they received an option determined by their answers to the willingness-to-pay questions described below. Every parent had a chance of entering this group, making those choices consequential (Section 2.4).

2.4 Eliciting Willingness to Pay and Beliefs

Before revealing treatment assignments, we measured parents' willingness to pay for each restriction. Enumerators presented two choice tasks in random order. One measured demand for *earmarking* by comparing the Voucher with Fee-Day Cash. The other measured demand for *delay* by comparing Early Cash with Fee-Day Cash. Enumerators described the Voucher as a school-fee subsidy paid directly to the school.

Both tasks began with a choice between equal amounts. The earmarking task asked:

> "Would you prefer to receive a 35,000 USh subsidy for Term 1 or a 35,000 USh cash transfer delivered on February 2?"

The timing task asked:

> "Would you prefer to receive 35,000 USh in cash delivered today or 35,000 USh in cash delivered on February 2?"

After the first choice, each parent answered three more questions in which we varied the amount of Fee-Day Cash. The Voucher or Early Cash remained fixed at UGX 35,000. When a parent chose that fixed option, we increased the next offer of Fee-Day Cash; when the parent chose Fee-Day Cash, we lowered it. The adjustments became progressively smaller, at UGX 10,000, 5,000, and 2,500 (Supplementary Appendix Figure A2). The four choices locate the parent's indifference point to within UGX 2,500, over a range of UGX 17,500 to 52,500 in Fee-Day Cash. We also asked parents to explain their initial choices in their own words.[16]

We define the *earmarking premium* as the amount of Fee-Day Cash at the indifference point minus UGX 35,000. It measures how much cash a parent would forgo to have UGX 35,000 paid directly to the school on the same day. We define the *delay premium* as UGX 35,000 minus the indifference point in the timing task. These premiums measure how much cash parents are willing to forgo to have the transfer earmarked for school fees

[16]Each parent then completed one of two additional exploratory timing tasks, assigned at random: Early Cash versus cash in one week, or Early Cash versus cash three weeks after the start of term. We do not use these tasks in this paper.

or delayed until fees are due. A negative premium indicates a preference for unrestricted or earlier cash at equal amounts.

To reduce concerns about repercussions from school administrators, enumerators assured parents that their responses would remain confidential and would not be shared with anyone outside the research team, including teachers or headteachers. They also emphasized that parents could spend cash transfers however they wished, including on expenses unrelated to schooling.

We also made transaction costs comparable across transfers. Each cash transfer included an additional UGX 1,000 to cover mobile-money withdrawal fees. Because parents ordinarily visit the school to pay fees, we required Voucher recipients to return to the school on a separate day to complete enrollment, so that the Voucher did not save them a trip.

The choices were incentive compatible. Enumerators explained that parents' answers could determine which transfer they received. For each parent selected into the Choice group, the tablet randomly drew one task and one amount of Fee-Day Cash, then implemented the option implied by the parent's choices at that amount, following a Becker–DeGroot–Marschak procedure (similar to Allcott et al. (2022)).[17]

Before assignment, parents also reported the probability, from 0 (impossible) to 100 (certain), that they would clear their child's Term 1 fees in full under each transfer and without a transfer. Differences between their forecasts under the Voucher and Fee-Day Cash measure the expected effect of earmarking, while differences between Fee-Day Cash and Early Cash measure the expected effect of delay.

At endline, we elicited demand for the Voucher again to study whether experience changed parents' preferences. Parents completed the same earmarking task for a future term, with the same return-trip requirement. Parents of children in their final year of primary school made choices for Term 3 of 2026, while the remaining parents were randomly assigned to Term 3 of 2026 or Term 1 of 2027. Five percent of households were randomly selected to receive a future transfer, and for half of these households, the transfer was determined by their choices. (Section 6.3).

2.5 Randomization and Delivery

We randomized treatment at the individual level within strata defined by school, grade, and gender. A computer generated assignments in advance, but enumerators learned a parent's assignment only after completing the choice tasks and fee-payment forecasts. The four treatment arms contain 1,960 parents; we exclude the 40 parents in the Choice group from

[17]The amount was drawn from the full grid, UGX 17,500 to 52,500 in steps of 2,500, so it could differ from the amounts offered during the task. The payout was always consistent with the parent's answers.

treatment-effect analyses. School staff knew which children received the Voucher because we paid their fees directly. They did not know which children were assigned to Control, Early Cash, or Fee-Day Cash.

We delivered both cash transfers by mobile money. For the Voucher, we paid the school only after confirming that the child had re-enrolled. Parents who received cash could keep it even if their child did not re-enroll; the Voucher was not paid unless the child returned to school.

On January 16, 2026, after baseline data collection, the government postponed the start of Term 1 from February 2 to February 10 because of election-related safety concerns. We kept Fee-Day Cash on its original February 2 schedule and paid the Voucher when schools reopened. The postponement also allowed us to elicit demand for delay through a second, binding choice. We randomly selected 90 parents assigned to Fee-Day Cash and offered them the choice of receiving their transfer on February 2 or February 10. Each received the transfer on their chosen date, while the remaining Fee-Day Cash recipients were paid on February 2 as planned. This pre-registered sub-experiment provides a second measure of demand for delay (Section 4.1) and, through the randomized offer, an instrument for the date of receipt (Supplementary Appendix Table A6).

2.6 Data

We surveyed parents and children at baseline and endline, with the endline survey taking place between May 2 and June 7, 2026, after Term 1 ended. Both surveys collected information on household demographics, economic outcomes, and children's schooling. At endline, we also measured whether children remained enrolled and whether they had faced sanctions for unpaid fees during the term.

We measure fee payment using administrative records from the 30 study schools. Headteachers maintain ledgers of payments toward each child's term fees, including cash and in-kind contributions. We value in-kind payments at the prices posted by each school and combine total payments with the fees charged for the child's grade to calculate the share of the term fee paid.

We measure attendance among study children using weekly unannounced school visits. Enumerators visited each school 10 to 12 times during Term 1, on randomly selected days and times, and recorded which study children were present. We also collected teachers' daily attendance registers, which cover all pupils in the study children's classes. We use these records to examine attendance among classmates outside the study sample. Because the registers sometimes have missing entries and continue to record children who have left

school as absent, we rely on the unannounced visits for our main attendance measure.

We assessed children's literacy and numeracy at baseline and endline using the Uwezo and Beyond Basics instruments (Uwezo, 2021; Jones et al., 2014). Uwezo tests basic reading and arithmetic skills, while Beyond Basics includes more advanced tasks. We administered these assessments independently because children with unpaid fees may be barred from school examinations. This allows us to measure learning even among children who were excluded from examinations or left school during the term.

2.7 Sample Characteristics and Balance

Table 1 describes the sample at baseline. Half of the guardians we interviewed are the child's mother and 17 percent are the father; the others are mostly grandparents or other relatives. Seventy-seven percent are women, and their average age is 44. Households have 6.7 members on average, and 64 percent report agriculture as their main income source. Children are 13 years old on average. Fifty-seven percent are girls, reflecting our oversampling, and 53 percent were enrolled in Primary 4 in 2025, with the remainder in Primary 6.

The treatment arms are balanced on observed baseline characteristics. Parents, households, and children look similar across arms, as do the baseline willingness-to-pay measures. The joint orthogonality tests do not reject balance, and only one of the 20 individual tests, for the food-security index, rejects at the five-percent level. Every study household completed the endline survey, so we have no endline survey attrition. School fee records and attendance visits, however, cover only the 1,776 children who did not transfer to another school during the term. We report Lee (2009) bounds for these outcomes (Supplementary Appendix Table A4).

Table 1: Baseline Balance Across Treatment Arms

	(1) **Control Mean**	(2) **Early Cash (T1)**	(3) **Fee-Day Cash (T2)**	(4) **Voucher (T3)**	(5) **Joint** p-value
Child characteristics					
Child in Primary 4 (2025)	0.525				
Child is female	0.578				
Child age (years)	13.251	−0.062	−0.048	0.045	0.706
		(0.095)	(0.092)	(0.099)	
Learning index (z-score)	0.000	0.024	0.092	0.081	0.356
		(0.061)	(0.060)	(0.061)	
Literacy score (z-score)	0.000	0.049	0.068	0.046	0.696
		(0.060)	(0.059)	(0.060)	
Numeracy score (z-score)	0.000	−0.006	0.094	0.098	0.167
		(0.062)	(0.062)	(0.063)	
Commute time to school (minutes)	34.8	0.1	0.0	0.9	0.896
		(1.4)	(1.4)	(1.5)	
Parent characteristics					
Parent is female	0.792	−0.032	−0.045*	−0.012	0.344
		(0.027)	(0.027)	(0.026)	
Parent age (years)	43.670	−0.293	0.668	0.760	0.511
		(0.815)	(0.837)	(0.854)	
Parent is the child's mother or father	0.671	0.036	0.010	−0.024	0.250
		(0.029)	(0.030)	(0.030)	
Parent completed primary (P7+)	0.426	0.032	0.032	0.041	0.589
		(0.031)	(0.032)	(0.032)	
Parent is the household head	0.503	−0.005	0.004	−0.043	0.430
		(0.032)	(0.032)	(0.032)	
Household characteristics					
Adults in household	2.626	0.117	0.062	0.078	0.610
		(0.088)	(0.088)	(0.088)	
Children in household (<18)	3.984	0.107	0.075	−0.010	0.658
		(0.111)	(0.108)	(0.108)	
Single-adult household	0.188	−0.040*	−0.045*	−0.034	0.230
		(0.024)	(0.024)	(0.024)	
Asset index (z-score)	0.000	0.075	0.080	0.085	0.469
		(0.063)	(0.063)	(0.062)	
Household income mainly agricultural	0.636	−0.007	−0.001	−0.003	0.996
		(0.029)	(0.029)	(0.029)	
Per capita household income (past 14 days, UGX)	12,754	−186	−491	1,732	0.254
		(1,096)	(1,092)	(1,178)	
Any household savings (past year)	0.771	−0.023	−0.015	0.020	0.386
		(0.027)	(0.027)	(0.026)	
Food insecurity index (z-score)	0.000	0.106*	−0.045	−0.043	0.035
		(0.060)	(0.062)	(0.062)	
Preference measures					
Earmarking premium (UGX)	5,907	377	−606	492	0.532
		(808)	(818)	(799)	
Delay premium (UGX)	4,152	87	1,025	824	0.498
		(824)	(815)	(808)	
Joint orthogonality test (p-value)		0.836	0.808	0.779	
Observations	495	489	490	486	

Notes: This table reports baseline characteristics of the 1,960 children and parents in the four treatment arms. Column 1 reports control-group means. Columns 2–4 report each arm's difference from the control group from regressions with randomization-strata fixed effects, and column 5 reports the p-value from a joint test that the three differences are zero. The orthogonality row reports, for each arm, the p-value from a joint test that all characteristics are unrelated to assignment. School, grade, and gender are stratification variables. Literacy and numeracy scores combine the Uwezo and Beyond Basics assessments and are standardized within grade to the control group, and the learning index is their average, standardized the same way. The asset index is the first principal component of indicators for dwelling characteristics, water and sanitation, land, durable goods, and livestock. The food insecurity index is the number of affirmative answers to the eight items of the Food Insecurity Experience Scale, so higher values indicate more food insecurity. Both indices are standardized to the control group. Per capita income is winsorized at the 99th percentile. Premia are censored at ±UGX 17,500. Robust standard errors are in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

3 A Conceptual Framework for School-Fee Payment and Self-Control

In this section we present a model of school-fee payment with quasi-hyperbolic discounting (Laibson, 1997; O'Donoghue and Rabin, 1999) and uncertain income on fee day. We use the model to derive willingness to pay for each restriction, to generate predictions about the effects of each transfer, and to show how the comparison between the two identifies whether parents correctly forecast their own present focus. Formal statements of the decision rules, the derivation of the value functions, all proofs, and four extensions are collected in Appendix B.

3.1 Setup

Timing. There are four dates, $t = 0, 1, 2, 3$. Date $t = 0$ is the baseline interview, at which the parent chooses among transfers and we elicit willingness to pay. Date $t = 1$ is the period before the term begins: the parent receives income y_1 and chooses how much to save, $s \geq 0$. Date $t = 2$ is *fee day*, the first day of term: the parent receives income y_2 and either pays the fee F or does not. At $t = 3$ the parent receives the return to schooling, R, if and only if the fee was paid. In practice a parent can pay part of the fee or pay it late (Section 2.1). We treat payment as a single decision on fee day, which corresponds to clearing the fee in full, the outcome that parents forecast at baseline (Section 2.4).

Fee-day income. For simplicity, income on fee day takes two values: $y_2 = y_H \geq F$ with probability $1 - q$, so income alone covers the fee, and $y_2 = y_L < F$ with probability q, a fee-day shortfall. The low-income state represents a term in which the household has too little cash on hand to pay the fee when it falls due.

Transfers. The program delivers a transfer of common cost $C = F$ through one of three channels, against a no-transfer control ($d = 0$): fungible *Early Cash* ($d = 1$, paid at $t = 1$); fungible *Fee-Day Cash* ($d = 2$, paid at $t = 2$); and a *Voucher* ($d = V$) paid directly to the school toward the fee at $t = 2$, contingent on enrollment. All three deliver the same amount F and differ only in when it arrives and in whether it can be spent on anything else.

Choices and payoffs. The parent makes three choices: the transfer d at $t = 0$, saving $s \geq 0$ at $t = 1$, and whether to pay the fee, $a \in \{0, 1\}$, on fee day. Consumption at $t = 1$ is income plus any Early Cash, less saving: $c_1 = y_1 + C\,\mathbf{1}\{d = 1\} - s$. Without a transfer and under the cash transfers, the parent arrives at fee day with cash on hand $\ell = s + y_2 + C\,\mathbf{1}\{d = 2\}$, which is what they saved, plus fee-day income, plus Fee-Day Cash if they receive it. Paying the fee uses F of this cash and yields the return, so $c_2 = \ell - aF$ and $c_3 = aR$. Under

the Voucher the school receives F directly on fee day, so the fee is paid without any of the parent's cash, $c_2 = s + y_2$, and $c_3 = R$.

Preferences and beliefs. The parent is a quasi-hyperbolic discounter with $\delta = 1$ and linear utility, so each self discounts any future period by $\beta \leq 1$. Writing c_t for consumption, the self active at each date maximizes

$$U_2 = c_2 + \beta c_3, \qquad U_1 = c_1 + \beta\,\mathbb{E}[c_2 + c_3], \qquad U_0 = \beta\,\mathbb{E}[c_1 + c_2 + c_3],$$

the last because the $t = 0$ self holds no consumption of their own. A parent with $\beta = 1$ is the time-consistent benchmark, for whom the design of the transfer is irrelevant. Following O'Donoghue and Rabin (1999, 2001), we set $\delta = 1$, because the period between the baseline interview and fee day is short, about five weeks on average.[18]

When $\beta < 1$, the parent may not be fully aware of it. Following O'Donoghue and Rabin (2001), their true parameter is β but they believe their later selves will act with $\tilde{\beta}$, so that $\tilde{\beta} = \beta$ is *sophisticated*, $\tilde{\beta} = 1$ fully *naive*, and $\beta < \tilde{\beta} < 1$ partial naïveté. We also allow $\tilde{\beta} < \beta$, in which case the parent overestimates their own present focus. Proposition 2 shows how the data distinguish this case from underestimation. The belief $\tilde{\beta}$ governs their forecast of their own saving and payment, and hence the perceived probabilities of paying the fee, $\tilde{\mu}_d(\tilde{\beta})$, and what they pay for commitment; the true β governs what their later selves do, and hence the treatment effects. At $t = 0$ the parent chooses the transfer that maximizes $V_0^{(d)}$, the expected value of U_0 under transfer d given this forecast. The difference between $\tilde{\beta}$ and β is the parent's misperception of their own present focus.

Assumption 1. (i) Paying the fee is worthwhile when the return is not discounted, $R > F > 0$. (ii) For simplicity, parents cannot borrow, so $s \geq 0$ and the fee can be paid only out of cash on hand. (iii) The transfer covers the fee, $C = F$.

3.2 Saving and Payment Decisions

We solve the model backward from fee day. Holding cash ℓ at $t = 2$, the fee-day self compares paying with not paying: paying yields $(\ell - F) + \tilde{\beta}R$ and not paying yields ℓ, so the fee-day self pays if and only if paying is feasible, $\ell \geq F$, and worthwhile, $\tilde{\beta}R \geq F$. The first condition requires enough cash on hand; the second, the *payment condition*, is met when present focus is mild enough that the discounted return exceeds the fee. The $t = 0$ self weighs the same payment differently. For the $t = 0$ self the fee and the return both lie in the future, so the

[18] The longer delay between fee payment and the schooling return is common across all three transfers, and we interpret R as the value of that return as of fee day, thereby folding longer-run discounting into its level.

fee is worth paying whenever cash allows, because $R > F$. The fee-day self bears the fee today and discounts the return, and may therefore leave the fee unpaid with cash in hand. Under the Voucher neither condition applies: the fee is paid directly to the school.

Saving matters only under Early Cash, since Fee-Day Cash and the Voucher deliver F on fee day. With linear utility the period-1 self saves either nothing or $F - y_L$, the amount needed to pay the fee when fee-day income is low (Lemma B.2). Saving reduces consumption at $t = 1$ for certain and allows the fee to be paid only if income is low. A parent who meets the payment condition therefore saves $F - y_L$ if and only if

$$\tilde{\beta}\, q\,(R - F) \;\geq\; (1 - \tilde{\beta})\,(F - y_L) \quad \Longleftrightarrow \quad \tilde{\beta} \geq \bar{\beta}_S \equiv \frac{F - y_L}{(F - y_L) + q(R - F)}.$$

The right side is the cost of saving as the period-1 self sees it. They give up $F - y_L$ today and get it back on fee day, which they discount by $\tilde{\beta}$. The left side is the benefit, the discounted net return from paying the fee in the low-income state, which occurs with probability q and is the only state in which saving changes payment. The cutoff $\bar{\beta}_S$ is higher when the shortfall $F - y_L$ is larger, and lower when a shortfall is more likely or the net return $R - F$ is larger. Combining the payment and saving rules gives the probability, as forecast at $t = 0$, that the parent pays the fee under each transfer:

$$\tilde{\mu}_V \equiv 1, \qquad \tilde{\mu}_2 = \mathbf{1}\{\tilde{\beta}R \geq F\}, \qquad \tilde{\mu}_1 = \mathbf{1}\{\tilde{\beta}R \geq F\}\big(1 - q\,\mathbf{1}\{\tilde{\beta} < \bar{\beta}_S\}\big). \tag{1}$$

The fee is paid with certainty under the Voucher because the transfer covers it. In the experiment the transfer covered 85 percent of the term fee on average. When it covers only part of the fee, the fee is paid in full under the Voucher only if the parent pays the balance, and the most present-focused parents value the Voucher below cash (Appendix B.7).

These probabilities sort parents, by how present-focused they believe themselves to be, into three contiguous regions. When present focus is severe ($\tilde{\beta}R < F$, the *under-payment* region) the parent expects not to pay the fee under either cash transfer, even with the cash on hand, and expects the fee to be paid only under the Voucher. For intermediate present focus ($F/R \leq \tilde{\beta} < \bar{\beta}_S$, the *under-saving* region) the parent would pay but will not save, so they expect to pay the fee with certainty under Fee-Day Cash and the Voucher, and under Early Cash only when fee-day income is high. When present focus is mild ($\tilde{\beta} \geq \bar{\beta}_S$) the parent saves and pays on their own, and expects to pay the fee under every transfer. Early Cash leaves both decisions with the parent: the cash must be saved until fee day and then paid toward the fee.

Saving is the only way to pay the fee after a shortfall because parents cannot borrow. If

parents can instead borrow at interest on fee day, a parent who did not save can still pay the fee under Early Cash, at a cost that rises with the interest rate. The earmarking premium is then unchanged, and the delay premium falls with the cost of credit and is zero when credit is free (Appendix B.5).

3.3 Willingness to Pay for Commitment

We define willingness to pay for one transfer over another as the amount of Fee-Day Cash that leaves the $t = 0$ self indifferent between them, as in the elicitation (Section 2.4). With linear utility these amounts are (Appendix B.2)

$$\begin{aligned} \text{WTP}_{\text{voucher}} &= (R - F)\,(\tilde{\mu}_V - \tilde{\mu}_2), \\ \text{WTP}_{\text{delay}} &= (R - F)\,(\tilde{\mu}_2 - \tilde{\mu}_1). \end{aligned} \tag{2}$$

Each premium equals the net return to paying the fee, $R - F$, multiplied by the increase in the probability of paying that the parent expects from the restriction. This holds for any distribution of fee-day income (Appendix B.2). Because the three transfers deliver the same amount, a parent who pays for a restriction pays for a change in their own later behavior, as in Carrera et al. (2022) and Allcott et al. (2022). The premium therefore measures the change in fee payment that the parent expects, not $\tilde{\beta}$ itself. The two-value income distribution serves only to map $\tilde{\beta}$ into the premia in Proposition 1. Strictly, the premia require only that the parent expects cash to be spent before it becomes fees; their own present focus is the leading interpretation of that expectation, and Section 4.3 weighs it against claims on their cash by others. Substituting the probabilities in (1) into (2) gives Proposition 1.

Proposition 1 (Demand for commitment)**.** *For every* $\tilde{\beta} \in (0, 1]$,

$$\text{WTP}_{\text{voucher}}(\tilde{\beta}) = (R - F)\,\mathbf{1}\{\tilde{\beta}R < F\}, \qquad \text{WTP}_{\text{delay}}(\tilde{\beta}) = q\,(R - F)\,\mathbf{1}\{F/R \le \tilde{\beta} < \bar{\beta}_S\}.$$

A parent pays for earmarking if they expect not to pay the fee out of cash on fee day, and pays for delay if they expect to pay on fee day but not to save beforehand. A parent who expects to save and pay values neither restriction. The range of $\tilde{\beta}$ *in which delay is valued is non-empty when* $y_L < F(1 - q)$.

The earmarking premium equals the full net return, $R - F$, because a parent in the under-payment region expects to leave the fee unpaid under cash, and the Voucher has it paid for certain. The delay premium is smaller, $q(R - F)$, because delay changes payment only when fee-day income is low. When income is high, a parent who did not save can still

pay out of income. A parent in the under-saving region expects to spend Early Cash before fee day, and Fee-Day Cash holds the transfer until the date on which they would pay.

Demand for delay also depends on the household's circumstances, not only on $\tilde{\beta}$. A parent who never faces a shortfall can pay out of fee-day income whether or not they saved, so the timing of the transfer does not affect their payment. A larger shortfall requires the parent to set aside more at $t = 1$, which raises the cutoff $\bar{\beta}_S$, so a wider range of parents expect not to save.

Supplementary Appendix Figure A3 displays the partition. A parent who expects to pay the fee in any case values the Voucher exactly as they value cash, which is the usual result for an in-kind transfer of a good the recipient would have bought anyway (Moffitt, 1989; Currie and Gahvari, 2008). The Voucher is worth more than cash only to a parent who expects that they would otherwise leave the fee unpaid. Paying for either restriction requires $\tilde{\beta} < 1$: a parent restricts their own transfer only if they perceive a self-control problem to begin with (Bryan et al., 2010).

Two features of Proposition 1 are specific to linear utility and $\delta = 1$. First, no parent pays for both restrictions. With concave utility the same parent can pay for both (Appendix B.6), as many parents in our data do (Section 4.1). Second, neither premium is negative, because a restriction costs the parent nothing. With $\delta < 1$ the characterization is unchanged provided $\delta R > F$, but a parent who values neither restriction strictly prefers earlier cash, so their delay premium is negative (Appendix B.4). With concave utility a parent with little cash on fee day may prefer cash to a paid fee, so the restrictions carry the cost of lost flexibility studied by Amador et al. (2006) and Laibson (2015). In both extensions a parent who believes they are time consistent pays for neither restriction.

Prediction 1 (Demand for restrictions). A parent pays for a restriction only if they believe they are present-focused. Each premium is increasing in the parent's own perceived behavior change: $\text{WTP}_{\text{voucher}}$ in $\tilde{\mu}_V - \tilde{\mu}_2$ and $\text{WTP}_{\text{delay}}$ in $\tilde{\mu}_2 - \tilde{\mu}_1$.

3.4 Treatment Effects and Naivete

Willingness to pay is governed by the forecast $\tilde{\beta}$; realized behavior is governed by the true β. Replacing $\tilde{\beta}$ with β in equation (1), the fee-day self pays if and only if $\beta R \geq F$, and the period-1 self saves if and only if $\beta \geq \bar{\beta}_S$ and they expect the fee-day self to pay. Under the Voucher the fee is paid regardless of β, because the fee-day self makes no payment decision. Under either cash transfer the fee is paid only if that self in fact chooses to pay, and, under Early Cash, only if the period-1 self in fact saved. The transfers therefore differ in how many of the two decisions they leave to the parent. Early Cash leaves both saving and payment,

Fee-Day Cash leaves only payment, and the Voucher leaves neither. A parent who would fail at either decision pays the fee only under a transfer that removes it.

Prediction 2 (Effects). Fee payment is weakly ordered across transfers: it is highest under the Voucher, followed by Fee-Day Cash and then Early Cash. The Voucher's advantage over Fee-Day Cash is the share of parents with $\beta R < F$. Fee-Day Cash raises payment over Early Cash only among parents who pay on fee day but do not save beforehand, so the two cash transfers have similar effects if few parents are of this kind. Because schools send children home over unpaid fees (Section 2.1), we expect the same ranking for dropout.

Comparing forecasts with realized behavior identifies whether parents anticipate their own present focus (Acland and Levy, 2015; Augenblick and Rabin, 2019; Allcott et al., 2022; Carrera et al., 2022). Let μ_d denote the realized probability that the parent pays the fee under transfer d, and for each cash transfer $d \in \{1, 2\}$ let $e_d \equiv (\tilde{\mu}_V - \tilde{\mu}_d) - (\mu_V - \mu_d)$ denote the forecast error in the gain in that probability from the Voucher over that transfer, the forecast gain minus the realized gain.

Proposition 2 (Forecast errors and naïveté)**.** *For $d \in \{1, 2\}$, $e_d = \mu_d - \tilde{\mu}_d$. A sophisticated parent ($\tilde{\beta} = \beta$) has $e_d = 0$. The error is negative only if the parent underestimates their present focus ($\beta < \tilde{\beta}$) and positive only if they overestimate it ($\tilde{\beta} < \beta$). The corresponding error for timing, $(\tilde{\mu}_2 - \tilde{\mu}_1) - (\mu_2 - \mu_1) = e_1 - e_2$, can take either sign in both cases.*

Under the Voucher the fee is paid regardless of the present-focus parameter, so an error in the forecast gain from the Voucher over cash is an error about the parent's own behavior under cash; forecast errors that do not depend on the transfer, such as errors about income or fees, difference out. The probability of paying the fee with cash is weakly increasing in the present-focus parameter. A parent who underestimates their present focus overestimates the probability that they pay the fee with cash, and therefore underestimates the gain from the Voucher. A parent who overestimates their present focus makes the opposite error. The sign of e_d reveals the direction of the misperception, and sophistication implies $e_d = 0$, which the data can reject (Section 6.2). The timing error carries no such restriction, because the under-saving region lies in the middle of the parameter range: a parent can move into it or out of it as β moves away from $\tilde{\beta}$.

The distinction between $\tilde{\beta}$ and β also determines whether the parents who pay for a restriction are the parents it affects. The Voucher raises a parent's fee payment over Fee-Day Cash if and only if $\beta R < F$, and the parent pays for earmarking if and only if $\tilde{\beta} R < F$. For sophisticated parents the two conditions coincide, so the parents who pay for earmarking are those whose payment it changes, as under selection on gains (Roy, 1951). A parent who

underestimates their present focus may pay nothing for the Voucher although it would raise their payment, and a fully naive parent pays for neither restriction whatever their β. A parent who overestimates their present focus may pay for the Voucher and yet pay the fee when given cash.

Prediction 3 (Demand and treatment effects). If no parent overestimates their present focus, the Voucher's effect on fee payment is at least as large among parents who pay for earmarking as among parents who do not. A smaller effect among parents who pay for earmarking implies that some of them overestimate their present focus.

In the same way, a parent can pay for delay ($\tilde{\mu}_2 > \tilde{\mu}_1$) yet find their payment unchanged ($\mu_2 = \mu_1$). We examine Prediction 3 in Section 6.1 and return to these points when parents re-value the transfers after experiencing them (Section 6.3).

4 Demand for Commitment

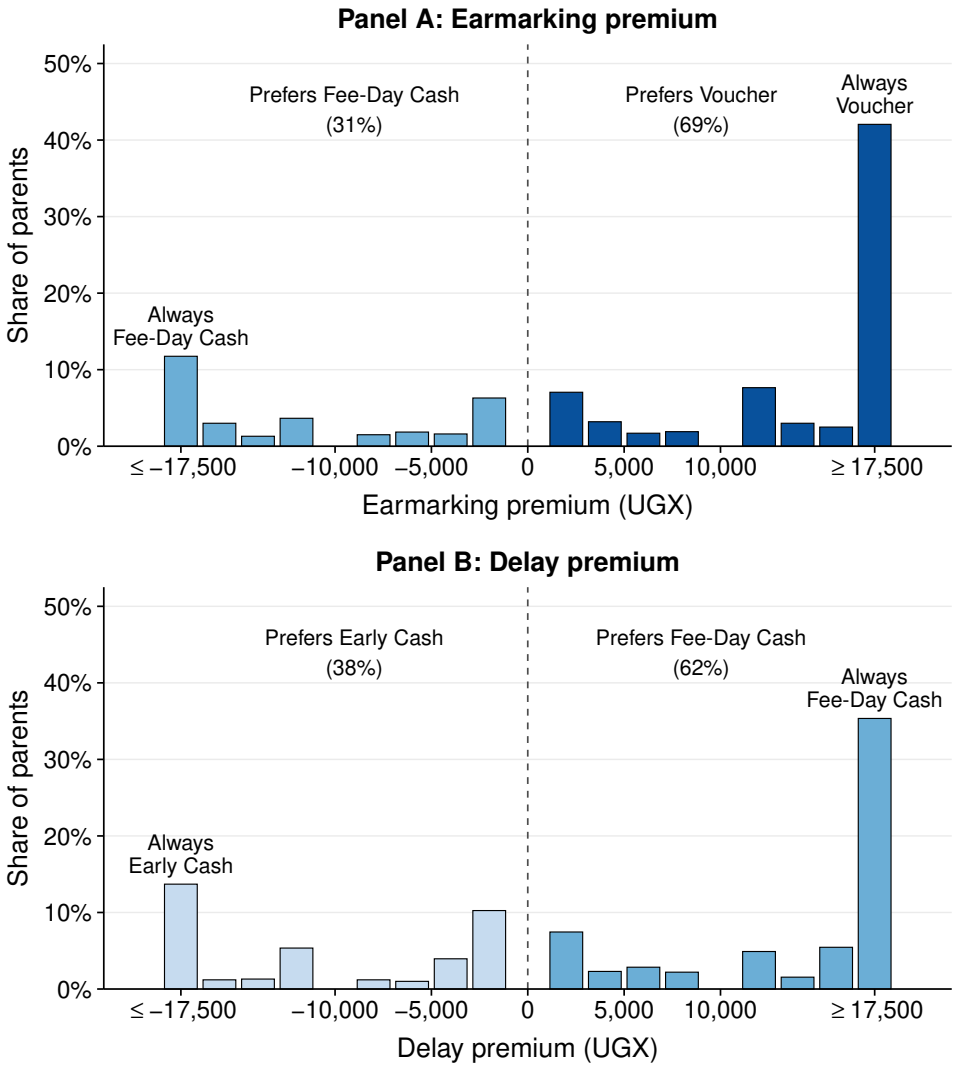
We study how much parents are willing to pay for earmarking and delay, and why they value these restrictions. We first describe their choices, then examine their explanations and forecasts of fee payment. We also consider other reasons for demanding commitment and ask which parents pay more.

4.1 Willingness to Pay for Earmarking and Delay

Most parents are willing to give up cash to restrict their own use of a transfer. Figure 3 shows that 69 percent prefer to have UGX 35,000 paid directly to the school rather than receive the same amount in cash on the same day. Another 62 percent prefer to receive cash when fees are due rather than several weeks earlier, even at a lower amount. These choices are difficult to reconcile with time-consistent preferences. As shown in Section 3, a time-consistent parent can use cash to pay the fee or save an early transfer until fees are due, so they have no reason to pay for either restriction (Proposition 1). This benchmark does not require the functional forms of our model, provided that saving and paying the fee in cash carry no additional costs (Moffitt, 1989; Currie and Gahvari, 2008; Frederick et al., 2002; Andreoni and Sprenger, 2012). Positive premia therefore suggest that parents anticipate difficulty following through on their plans, although they may also value the restrictions for other reasons, which we discuss in Section 4.3.[19]

[19]Indifference points are recorded at midpoints of the UGX 2,500 grid, so no parent has a premium of exactly zero. Choosing the restriction at equal amounts produces a recorded premium of at least UGX 1,250, and choosing the unrestricted option produces a premium of at most −UGX 1,250. Parents close to indifferent can therefore fall on either side of zero.

Figure 3: Distribution of the Earmarking and Delay Premia



Notes: This figure shows the distribution of the earmarking premium (Panel A) and the delay premium (Panel B) for all 2,000 parents. Premia are defined in Section 2.4.

Many parents are willing to give up substantial amounts. Forty-two percent choose the Voucher even when offered UGX 52,500 in Fee-Day Cash, and 35 percent choose UGX 17,500 in Fee-Day Cash over UGX 35,000 in Early Cash. Both groups would forgo at least UGX 17,500, half the transfer, to keep the restriction. Across all parents, the median premium is UGX 11,250 for earmarking and UGX 6,250 for delay (Table 2). Because so many parents choose a restriction at every price, the premia are censored at the bounds of the elicitation grid. An intercept-only two-limit Tobit model, which extrapolates beyond these bounds, gives mean premiums of UGX 12,245 for earmarking and UGX 8,517 for delay, or 35 and 24 percent of the transfer. The mean earmarking premium is close to a day of median household consumption in the region. The reported medians and shares choosing a restriction at every price do not rely on the Tobit model.

Table 2: Willingness to Pay for Earmarking and Delay

	Earmarking premium *(Voucher vs. Fee-Day Cash)*	**Delay premium** *(Fee-Day Cash vs. Early Cash)*
Mean (Tobit)	12,245	8,517
Mean	6,000	4,569
Standard deviation	13,064	13,062
25th percentile	−1,250	−3,750
Median	11,250	6,250
75th percentile	17,500	17,500
Share with positive premium	69.0%	62.0%

Notes: This table reports the distribution of the earmarking and delay premia, defined in Section 2.4, for all 2,000 parents. Premia are recorded at the midpoints of the UGX 2,500 grid and censored at ±UGX 17,500. The first row reports the latent mean from an intercept-only two-limit Tobit model (Appendix C), and the remaining rows describe the observed distribution.

Parents often value both restrictions. Eighty-five percent pay for at least one, and 46 percent pay for both.[20] At equal amounts, however, 31 percent prefer cash to the Voucher and 38 percent prefer Early Cash to Fee-Day Cash. Some of these parents may be nearly indifferent, while others may value the option to use the money for unexpected needs (Amador et al., 2006).

A second, binding choice provides further evidence of demand for delay. After the government postponed the start of term from February 2 to February 10, we offered 90 randomly selected Fee-Day Cash recipients a choice between receiving their transfer on the two dates.

[20] In the baseline model with linear utility, a parent pays for earmarking or for delay but not both. With concave utility, a parent can pay for both (Appendix B.6). Both choice tasks were incentivized. Their randomized order does not affect either premium ($p = 0.22$ for earmarking and $p = 0.27$ for delay), and the adaptive procedure does not permit inconsistent choices within a task.

We paid each parent on the date they chose (Section 2.5). Standard discounting predicts a weak preference for the earlier payment. Yet 63 percent chose to wait until fees were due (Figure A4).

The extent of demand is unusual relative to much of the commitment literature, where take-up often falls sharply when commitment carries a price (Ashraf et al., 2006; Giné et al., 2010; Bryan et al., 2010; Kaur et al., 2015; Augenblick et al., 2015; Laibson, 2015; John, 2020; Bai et al., 2021). In Augenblick et al. (2015), for example, 59 percent of participants choose commitment when it is free, but only 9 percent do so when it costs \$0.25. Some studies, however, document substantial willingness to pay. In Casaburi and Macchiavello (2019), 86 percent of Kenyan dairy farmers accept a 15 percent lower price in exchange for monthly rather than daily payments. About a third of the rickshaw drivers in Schilbach (2019) choose sobriety incentives over a strictly higher unconditional payment, and payday borrowers in Allcott et al. (2022) value incentives to avoid future borrowing. Unlike many of these studies, ours draws parents from randomly selected government schools, rather than recruiting people interested in a particular savings or commitment product.[21]

Several features of school fees and our design may help explain why demand is so high. Fees are lumpy, recurring payments due at predictable times, and children whose fees remain unpaid may be sent home. Paying also involves a trade-off across time, since spending on current needs offers an immediate benefit while much of the return to schooling arrives later (O'Donoghue and Rabin, 1999). The stakes may be high as well. In the model, willingness to pay increases with the net return to paying the fee, $R - F$, so even a modest expected change in payment can justify a substantial premium.[22] Finally, the restrictions apply to a transfer that parents have not yet received. Unlike commitment savings products, which require users to deposit money they already hold, parents can restrict this transfer without first setting money aside (Casaburi and Macchiavello, 2019). Their other income remains available for unexpected expenses, limiting the cost of lost flexibility (Laibson, 2015).

[21]Related arrangements also help households set aside resources for planned expenses. Labeled transfers and savings devices can increase educational investments and health savings (Benhassine et al., 2015; Dupas and Robinson, 2013); workers may voluntarily delay wage payments (Brune et al., 2021), and offering fertilizer shortly after harvest increases adoption (Duflo et al., 2011).

[22]Returns to schooling in Uganda are high by international standards. Using the Uganda National Panel Survey 2019/20, a regression of log hourly wages on schooling and standard demographic controls gives an association of 16.3 percent per additional year (s.e. 1.6, clustered by district). Montenegro and Patrinos (2014) report 15.9 percent for Uganda in 2010, compared with 9.7 percent across 139 economies. These associations need not be causal.

4.2 Why Do Parents Demand Commitment?

Parents' explanations for their choices suggest that many anticipate spending money intended for school fees on other household needs. Asked why they preferred the Voucher, one parent explained that "if it is sent on the phone, it might get other expenses, and I fail to pay fees." Another said, "I have a lot of problems, so I might use it for other business, and at the end fail to clear school fees when the term resumes."[23] Among parents who preferred the Voucher to the same amount in cash, 54 percent said they might otherwise spend the money on immediate household needs such as food, medical care, and other essentials. Fewer than 8 percent said they would save it, and only 0.6 percent mentioned alcohol.

These explanations concern ordinary household expenses rather than particular temptation goods. Spending on food or medical care need not be wasteful. But a time-consistent parent who expects these expenses to take priority over school fees would prefer to retain the flexibility of cash, not pay to give it up (Amador et al., 2006). Choosing the Voucher is instead consistent with parents anticipating that they may spend cash on immediate needs even though they would prefer, in advance, to pay school fees.

Parents who chose to wait in the February timing experiment described a similar concern. One explained, "I don't want to stay with money because money is so tempting," and another said, "when I get money before the term starts I may misuse it for other purposes." Parents who preferred the earlier date often planned to buy uniforms or scholastic materials before school reopened. Their answers show why flexibility can also be valuable.

These explanations are consistent with the two decisions in our model. A parent may spend money before the fee is due rather than save it, or use money available on fee day for other expenses rather than pay the school. Fee-Day Cash removes the first opportunity, while the Voucher also removes the second. The model further predicts that a parent pays more for a restriction when they expect it to increase their fee payment by more. Specifically, each premium equals the expected increase in the probability of paying the fee multiplied by the net return to payment, $R - F$ (equation (2); Prediction 1). We test this prediction using parents' forecasts of their own fee payment.

Parents reported the probability that they would clear their child's fees in full without a transfer and under each of the three transfers (Section 2.4).[24] On average, they expected to clear their fees with a probability of 43 percent without a transfer, 60 percent with Early Cash, and 73 percent under both Fee-Day Cash and the Voucher. Thus, parents expected

[23] Open-ended explanations were recorded by enumerators in English, sometimes in the third person; quotations are lightly edited for spelling and punctuation.

[24] Enumerators elicited each probability on a scale from 0 (impossible) to 100 (certain). Parents practiced with the probability of rain the following day, and enumerators explained the scale again until the parent could use it.

delay to improve their chances of paying but, on average, did not expect earmarking to increase the probability of clearing the full balance.

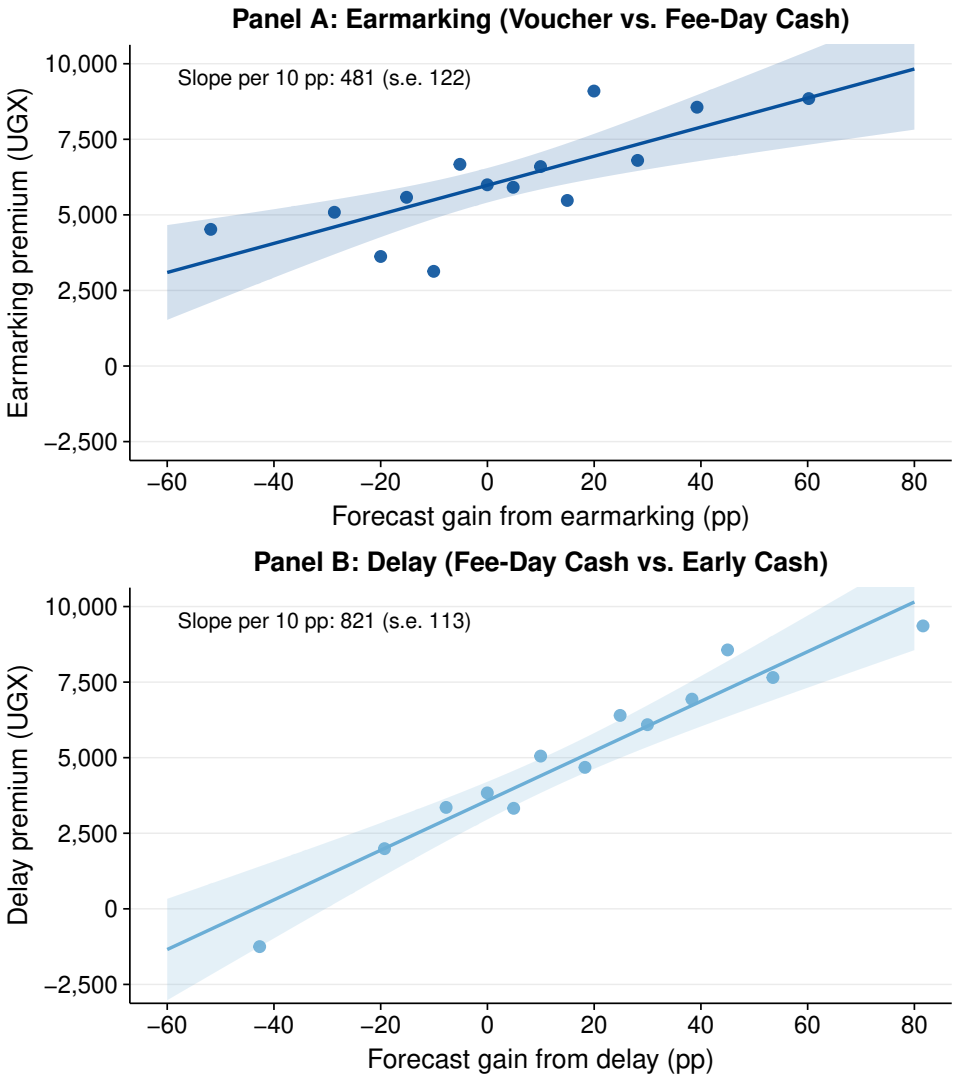
Parents who expect a larger increase in fee payment are willing to pay more for the corresponding restriction, as the model predicts. Figure 4 plots each premium against the parent's corresponding forecast gain. A 10-percentage-point larger expected increase in full fee payment is associated with a UGX 481 higher earmarking premium and a UGX 821 higher delay premium ($p < 0.001$ for both). The relationship could also arise if parents who forecast larger gains simply have more at stake, a larger $R-F$. The estimates remain similar, however, when we control for the child's grade and achievement, household wealth, parents' expected returns to schooling, the fee charged, and randomization strata, at UGX 425 for earmarking and UGX 796 for delay ($p < 0.001$ for both; Supplementary Appendix Table A1).[25] Proxies for the value of schooling also do not predict either forecast gain (Supplementary Appendix Table A2), so the forecasts appear to capture the change in behavior parents expect rather than how much they value schooling. The positive relationship between expected gains and premia resembles findings for gym attendance incentives in Carrera et al. (2022) and borrowing incentives in Allcott et al. (2022).

These forecasts explain some of the variation in the earmarking premium across parents, but they do not readily explain why most parents pay for the Voucher. Parents expect to clear their fees equally often under Fee-Day Cash and the Voucher, yet most are willing to pay for the Voucher. One reason may be that the forecasts concern clearing the fee *in full*, whereas the Voucher covers about 85 percent of the term fee on average. Parents may value having most of the fee paid even if they do not expect to clear the remaining balance, particularly because partial payments can prevent sanctions such as being sent home (Section 5.3).[26] We return to these forecasts in Section 6, where we compare them with the effects of the transfers on actual fee payment.

[25]The forecast gain from delay also predicts the earmarking premium, conditional on the forecast gain from earmarking. A 10-percentage-point increase in the former is associated with a UGX 424 higher earmarking premium ($p < 0.001$), whereas the gain from earmarking does not predict the delay premium ($p = 0.87$; Supplementary Appendix Table A1, column 4). This asymmetry could reflect an element of anticipated present focus common to both restrictions, although the forecasts are noisy.

[26]More than a third of parents report a negative forecast gain from earmarking, which the model rules out because the Voucher leaves only the balance for the parent to pay. Reported probabilities are heavily rounded, with 77 percent multiples of 10 and 32 percent exactly 0, 50, or 100 (Manski, 2004; Delavande et al., 2011; Allcott et al., 2022). Noise of this kind produces negative forecast gains even for parents who expect the Voucher to help and, if it is classical, attenuates the relationship between forecasts and premia.

Figure 4: Willingness to Pay and Forecast Gains



Notes: This figure plots each premium against the parent's forecast gain from the corresponding restriction. The forecast gain is the difference between the parent's baseline forecasts of fully clearing Term 1 fees under the Voucher and Fee-Day Cash (Panel A) and under Fee-Day Cash and Early Cash (Panel B). Points are means within 20 quantile bins of the forecast gain, with parents who report the same gain kept in one bin. Lines and shaded areas are OLS fits and 95% confidence intervals estimated on the 2,000 parent-level observations, and each panel reports the slope with its robust standard error.

Table 3: Alternative Explanations for Willingness to Pay

		Earmarking Premium *(Voucher vs. Fee-Day Cash)*		**Delay Premium** *(Fee-Day Cash vs. Early Cash)*	
	Mean	Marg. effect	(s.e.)	Marg. effect	(s.e.)
Panel A: Claims within the Household					
Disagreement over fees in past year (0/1)	0.25	140	(652)	707	(660)
Parent is female (0/1)	0.77	−386	(669)	76	(686)
Parent is household head (0/1)	0.49	−695	(569)	−678	(578)
Single-adult household (0/1)	0.16	663	(773)	−633	(801)
Panel B: Claims from Kin					
Kin pressure index (z-score)	0.00	270	(285)	−421	(290)
Net transfers out of household (1,000 UGX)	4.44	−1.81	(1.80)	−0.43	(1.81)
Panel C: Theft, Inflation, and Social Desirability					
Concern about theft (z-score)	0.00	−145	(284)	−563*	(288)
Expected inflation, maize (10 pp)	2.30	155	(152)	123	(154)
Expected inflation, soap (10 pp)	1.01	−101	(219)	156	(222)
Social desirability score (z-score)	0.00	249	(285)	−102	(290)

Notes: This table reports average marginal effects, in UGX, of each row variable on the earmarking and delay premia, each from a separate two-limit Tobit model censored at ±UGX 17,500 (Appendix C), for all 2,000 parents. For binary variables, the effect is the difference between one and zero. The kin pressure index combines how often the parent is asked for money, how difficult they find it to refuse, and whether they lent or gave money outside the household in the past year. Concern about theft refers to cash kept at home. Expected inflation is the expected percent change in the price between the interview and February, in units of 10 percentage points. The social desirability score is the 13-item short form of the Marlowe–Crowne scale (Reynolds, 1982). Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.3 Alternative Explanations

Parents may value earmarking or delay for reasons other than their own self-control. The restrictions could protect money from claims by household members or relatives, reduce the risk of theft, or make it easier to earmark money mentally for school fees. Parents might also choose the restricted transfers because they appear to be the more responsible options. We examine these explanations using household characteristics and parents' reported concerns (Table 3).[27]

Claims by others. Parents may prefer the Voucher because other people cannot claim money paid directly to a school, or prefer delay because it reduces the time during which relatives can request money. Previous studies show that individuals sometimes limit access to resources to protect them from spouses or kin (Anderson and Baland, 2002; Ashraf, 2009; Jakiela and Ozier, 2016). If such pressures drive demand here, parents who disagree with household members over school fees or face greater pressure from relatives should pay more for the restrictions.

We find little evidence of these patterns. Parents who report household disagreement over school fees pay no more for either restriction ($p = 0.83$ for earmarking and $p = 0.28$ for delay). Neither premium varies systematically with whether the parent is female or heads

[27] Unlike attendance conditions designed to discipline children (Bursztyn and Coffman, 2012; De Walque and Valente, 2023), these restrictions operate primarily on decisions about the parent's own money. The Voucher could nonetheless affect parents' dealings with schools, which we examine in Sections 5.3 and 5.4.

the household, with an index of pressure from relatives, or with transfers made outside the household (Table 3, Panels A and B). Demand is also high among parents living without another adult. Among these 312 households, 72 percent pay for earmarking and 63 percent pay for delay, and their median premia match those in the full sample. Only 0.2 percent of parents mention giving money to people outside the household when asked how they would otherwise use cash. Claims by others may matter for some households, but they do not appear to explain the widespread demand in our sample.

Mental accounting. Parents may value the Voucher because its school-fee label helps them keep money separate from other household resources. Labels can affect spending even without an enforceable restriction (Shefrin and Thaler, 1988; Thaler, 1990; Benhassine et al., 2015; Hastings and Shapiro, 2018). The Voucher differs from Fee-Day Cash in both its label and its restriction, so we cannot isolate the value of each. Mental accounting may also help parents follow through on their plans and therefore be part of the mechanism rather than an entirely separate explanation. Labeling alone, however, has difficulty explaining demand for delay. Early Cash and Fee-Day Cash were described identically, and the February choice compared the same unrestricted transfer on two dates. Nevertheless, most parents chose to wait.

Risks of holding cash. Keeping cash until the fee date may expose it to loss or theft, which would make delay attractive (Karlan et al., 2014). We find no supporting pattern. Concern about theft is associated with a *lower* delay premium ($p = 0.05$; Table 3, Panel C). Parents who expect prices to rise might instead prefer earlier cash, but expected inflation in the prices of maize and soap predicts neither premium.

Social desirability and experimenter demand. Parents might choose the restricted transfers because they believe paying school fees is the more responsible response or that researchers want them to choose commitment. We cannot rule out such effects, but several features of the study mitigate the concern. First, the choices had real consequences. A randomly selected 2 percent of parents received the option implied by their choices, and every parent offered the February choice was paid on the date they chose, so choosing a restriction to please the enumerator risked giving up money. Across a range of incentivized experimental tasks, mild signals about what the researcher expects shift choices only modestly (**?**). Second, enumerators assured parents that their responses would not be shared with teachers or headteachers. This reduces concerns about repercussions from school administrators, although it does not remove a desire to please the enumerator. Third, scores on the 13-item short form of the Marlowe–Crowne Social Desirability Scale, which measures the tendency to present oneself favorably, predict neither premium (Crowne and Marlowe, 1960; Reynolds,

1982).[28].

4.4 Who Pays More for Commitment?

The model provides two reasons why a parent may pay more for commitment. They may expect a restriction to increase their fee payment by more, or they may place greater value on having the fee paid. Equation (2) captures these two sources of demand through the expected gain in fee payment and the net return to paying, $R - F$. We examine how premia vary with household resources, access to saving and borrowing, and children's achievement, and whether these characteristics also predict the gains parents expect.

Parents who already save pay less for earmarking, but not for delay. Those who saved in the previous month have an earmarking premium UGX 2,584 lower than other parents ($p < 0.001$), and parents with a bank account or membership in a savings group pay UGX 1,944 less ($p < 0.001$; Supplementary Appendix Table A3). Neither measure predicts the delay premium ($p = 0.42$ and $p = 0.44$). Existing savings arrangements may reduce the value of having the transfer earmarked for fees, although they do not necessarily eliminate the perceived benefit of receiving money later.

Access to credit shows the opposite pattern. A one-standard-deviation increase in the reported ease of borrowing UGX 50,000 to 200,000 is associated with a UGX 726 lower delay premium ($p = 0.01$), but does not predict the earmarking premium ($p = 0.23$). This is consistent with the model's borrowing extension (Appendix B.5). A parent who can borrow to cover a shortfall when fees are due has less reason to pay to delay the transfer. Borrowing does not, however, address the problem of choosing to spend available cash rather than pay the fee.

Both premia also increase with children's baseline numeracy, which may reflect differences in the value parents place on keeping their children in school. Controlling for grade and household assets, a one-standard-deviation increase in numeracy is associated with a UGX 571 higher earmarking premium and a UGX 810 higher delay premium ($p = 0.04$ and $p = 0.005$; Table 4, Panel B). Literacy predicts neither premium. Numeracy does not predict either forecast gain ($p = 0.84$ for earmarking and $p = 0.19$ for delay; Supplementary Appendix Table A2). The pattern is consistent with the model's distinction between expected behavioral gains and the returns to paying fees, but numeracy is only an indirect proxy for those returns. Parents' own forecasts of the earnings return to schooling do not

[28]The scale includes statements such as "I'm always willing to admit it when I make a mistake" and "There have been occasions when I took advantage of someone." Higher scores indicate a greater tendency to present oneself favorably.

predict either premium.[29]

Table 4: Correlates of Willingness to Pay for Earmarking and Delay

		Earmarking Premium *(Voucher vs. Fee-Day Cash)*		**Delay Premium** *(Fee-Day Cash vs. Early Cash)*	
	Mean	Marg. effect	(s.e.)	Marg. effect	(s.e.)
Panel A: Wealth and Liquidity					
Asset index (z-score)	0.06	−816***	(272)	−685**	(277)
Liquidity index (z-score)	0.00	−834***	(283)	−675**	(288)
Forecast: clears fees with no transfer (10 pp)	4.30	−266**	(115)	−239**	(117)
Panel B: Child Achievement and Expected Returns					
Learning index (z-score)	0.05	443	(281)	521*	(288)
Numeracy score (z-score)	0.05	571**	(281)	810***	(287)
Literacy score (z-score)	0.04	240	(281)	107	(288)
Expected return to schooling (z-score)	0.00	9	(283)	48	(293)

Notes: This table reports average marginal effects estimated as in Table 3. The asset and learning indices and the literacy and numeracy scores are defined in Table 1. The liquidity index averages five standardized measures of the ease of obtaining a small emergency loan, whether the household could raise UGX 50,000 within 24 hours, crops in storage, any savings contribution in the past year, and savings-group membership. The forecast is the parent's reported probability of fully clearing Term 1 fees with no transfer, in units of 10 percentage points. The learning, literacy, and numeracy rows control for grade and the asset index. The expected return to schooling is the standardized difference in expected log earnings at age 25 between completing Senior 4 and Primary 7 (Section 4.4). Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Poorer and less liquid households pay more for both restrictions (Table 4, Panel A). A one-standard-deviation increase in the asset index is associated with a UGX 816 lower earmarking premium and a UGX 685 lower delay premium ($p = 0.003$ and $p = 0.01$). The corresponding reductions for a one-standard-deviation increase in the liquidity index are UGX 834 and UGX 675. Parents who expect to be less likely to clear fees without a transfer also pay more for both restrictions.

Greater demand among poorer households could reflect more difficulty saving or paying school fees, or a higher value of ensuring that a fee is paid. In the model, the risk of a shortfall on fee day raises the potential value of delay. Yet poorer parents do not forecast larger gains in full fee clearing from either restriction ($p = 0.77$ for earmarking and $p = 0.13$ for delay; Supplementary Appendix Table A2), and they do not expect higher earnings returns to schooling (Supplementary Appendix Figure A5). Their higher premia may instead reflect the value of partial fee payment or the difficulty of clearing an unpaid balance later, neither of which is fully captured by the forecast of clearing fees in full. These results establish which parents value commitment more, but not whether the restrictions change their behavior more.

[29]Following Attanasio and Kaufmann (2014), parents estimated the child's earnings at age 25 after completing Primary 7 or Senior 4. They expected lower secondary schooling to roughly double earnings, equivalent to an annualized return of 26–30 percent (Supplementary Appendix Figure A5). These forecasts capture only part of the value parents may attach to schooling, and distant earnings are difficult to predict (Jensen, 2010; Attanasio and Kaufmann, 2014).

Taken together, the evidence is consistent with many parents anticipating self-control problems in paying school fees. They give up substantial amounts to restrict their choices, pay more when they expect a restriction to raise fee payment by more, and describe the risk of spending money intended for school fees on other needs. We find little evidence that claims from others, risks of holding cash, or social desirability explain the widespread demand, although we cannot rule out these motives for individual parents or separate the Voucher's label from its restriction. In the model, demand depends on perceived present focus, $\tilde{\beta}$, while the effects of the transfers depend on actual present focus, β. We next estimate those effects and ask whether parents who value commitment most are also those whose behavior responds most.

5 Effects of the Transfers

This section presents the effects of the three transfers on fee payment, children's schooling, and household outcomes. Because the transfers have the same value, the comparison of Fee-Day Cash with Early Cash reflects timing, the comparison of the Voucher with Fee-Day Cash mainly reflects earmarking, and the comparison of the Voucher with Early Cash reflects both restrictions together.

5.1 Empirical Specification

We estimate intention-to-treat effects with the specification

$$Y_{is} = \beta_1 T_{1,is} + \beta_2 T_{2,is} + \beta_3 T_{3,is} + X'_{is}\gamma + \delta_s + \varepsilon_{is}, \tag{3}$$

where $T_{1,is}$, $T_{2,is}$, and $T_{3,is}$ indicate assignment to Early Cash, Fee-Day Cash, and the Voucher, and δ_s are randomization-strata fixed effects. The vector X_{is} contains the baseline value of the outcome where one exists (ANCOVA) and, for enrollment outcomes, the gender of the responding parent. Standard errors are heteroskedasticity-robust. We estimate attendance at the child-week level with week fixed effects and standard errors clustered by child. The difference $\beta_2 - \beta_1$ measures the effect of timing for unrestricted cash. The difference $\beta_3 - \beta_2$ measures the effect of earmarking, together with the Voucher's re-enrollment requirement and the eight-day gap between the two payment dates, and $\beta_3 - \beta_1$ measures the effect of both restrictions together.

The five pre-specified primary outcomes are the share of the term fee paid, dropout, moving to another school, attendance, and the learning index, reported in Tables 5 and 7.[30]

[30] Dropout and moving to another school together make up the pre-analysis plan's outcome of enrollment

We report Anderson (2008) sharpened q-values across these five outcomes and the three arms, and we do not adjust the estimates for other outcomes. Because fee records and attendance checks are collected at the baseline school, fee payment is observed only for the 1,776 children who did not move to another school during the term. Since the transfers reduce such moves, we also report Lee (2009) bounds for fee payment and attendance (Supplementary Appendix Table A4).

5.2 Fee Payment

The Voucher more than doubles the fees paid toward the term fee. It raises total fees paid by UGX 22,800, an increase of 113 percent over the control mean of UGX 20,123, and the share of the term fee paid by 60 percentage points, from a control mean of 37 percent (Table 5, columns 1 and 2; $q = 0.001$). Much of this effect is mechanical, because the Voucher is paid to the school once the child re-enrolls. Half of Voucher children clear the fee, excluding the food charge, compared with 12 percent of control children (Supplementary Appendix Table A5, column 4).

Cash raises fee payment by much less, and by the same amount whether it arrives six weeks before fees are due or when they are due. Early Cash and Fee-Day Cash raise fees paid by UGX 4,649 and 4,762, increases of 23 and 24 percent, and each raises the share of the fee paid by 11 to 12 percentage points (columns 1 and 2). The two effects are statistically indistinguishable ($p = 0.94$ for the amount and $p = 0.86$ for the share). Even parents who receive Fee-Day Cash, and therefore hold UGX 35,000 shortly before fees are due, raise their fee payment by only about a seventh of the transfer.[31] The Lee (2009) bounds on the Voucher's effect are close to the point estimate, whereas the lower bounds for both cash transfers include zero (Supplementary Appendix Table A4).

Together, the two restrictions raise fees paid by UGX 18,151 more than Early Cash does ($p < 0.001$), and almost all of this difference comes from earmarking. Delivering the cash when fees are due rather than six weeks earlier accounts for only UGX 113 of it ($p = 0.94$), and earmarking accounts for the remaining UGX 18,038 (column 1). This is the ordering in Prediction 2. In the model, delivering cash on fee day rather than earlier raises payment only for parents who would pay on fee day but would not keep the cash until then, whereas earmarking raises payment for parents who hold the cash on fee day but do not use it for the

at the baseline school.

[31] An eight-day postponement of the term provides a second test of timing within the Fee-Day Cash arm (Section 2.5). We instrument the date on which a parent received the transfer with the randomized offer to choose it. The estimated effect on fees paid of receiving the transfer on February 10 rather than February 2 is −UGX 3,607, with a standard error of 3,786 (Supplementary Appendix Table A6). The compliers are parents who chose the later date when offered it, so the estimate applies to parents who want the delay.

Table 5: Effects on Fee Payment, Enrollment, and Attendance

	Fee Payments		Enrollment			Attendance	
	(1) Total Paid (1,000 UGX)	(2) Share Paid	(3) Enrolled (Any School)	(4) Dropped Out	(5) Transferred Schools	(6) All	(7) Did Not Transfer
T1: Early Cash	4.649***	0.116***	0.002	−0.011	−0.028	0.042**	0.024
	(1.417)	(0.028)	(0.008)	(0.013)	(0.019)	(0.019)	(0.015)
		[0.001]		[0.257]	[0.145]		[0.140]
T2: Fee-Day Cash	4.762***	0.111***	0.004	−0.015	−0.031*	0.052***	0.032**
	(1.428)	(0.028)	(0.007)	(0.013)	(0.019)	(0.018)	(0.015)
		[0.001]		[0.190]	[0.126]		[0.063]
T3: Voucher	22.800***	0.595***	0.009	−0.027**	−0.035*	0.068***	0.049***
	(1.260)	(0.026)	(0.007)	(0.012)	(0.019)	(0.018)	(0.015)
		[0.001]		[0.049]	[0.089]		[0.003]
Control Mean	20.123	0.374	0.984	0.051	0.119	0.696	0.772
Observations	1,776	1,776	1,960	1,960	1,960	22,251	20,180
p-value: T1 = T2	0.936	0.859	0.782	0.769	0.853	0.577	0.555
p-value: T2 = T3	0.000	0.000	0.384	0.248	0.819	0.348	0.209
p-value: T1 = T3	0.000	0.000	0.264	0.146	0.682	0.136	0.060

Notes: This table reports intention-to-treat estimates from equation (3) for the 1,960 children in the four treatment arms. Fees paid (columns 1–2) are from school fee records for Term 1, with missing records coded as zero. Enrollment outcomes (columns 3–5) are from the endline parent survey and control for the gender of the responding parent, and dropping out includes never enrolling during the term. Attendance (columns 6–7) uses child-week observations from the unannounced visits, includes week fixed effects, and clusters standard errors by child. Column 6 counts children who transferred to another school as absent. Columns 1, 2, and 7 are restricted to the 1,776 children who, according to the endline parent survey, did not transfer to another school during the term. The bottom rows report p-values from tests of equality between arms. Anderson (2008) sharpened q-values, computed across the five primary outcomes and three arms, are in brackets. Robust standard errors are in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

fee. The large effect of earmarking and the absence of a timing effect are therefore consistent with parents having more difficulty using cash for the fee once they have it than keeping it until fees are due.

These effects do not line up with parents' demand for the two restrictions. Sixty-two percent of parents were willing to pay for delay, which does not raise fee payment, and 69 percent were willing to pay for earmarking, which raises it substantially (Section 4.1). On their own, these comparisons do not show that the difference between the transfers reflects time inconsistency rather than other reasons parents spend cash on things other than the fee. Section 6 returns to this question by comparing the effects with parents' own forecasts and with their willingness to pay.

These results differ from evidence in Bogotá, where paying a third of a monthly conditional transfer as a lump sum when students had to re-enroll raised re-enrollment by 4 percentage points, whereas the standard monthly transfer did not (Barrera-Osorio et al., 2011). That design turned part of a monthly stream into a lump sum available at the time of the decision, whereas our two cash transfers are lump sums of the same size that differ only in when the money arrives.

5.3 Schooling Outcomes

Sanctions. Fee payment matters for schooling because schools sanction children whose fees are unpaid, by sending them home and by withholding exams, meals, and report cards. In the control group, 62 percent of children were sent home over unpaid fees during the term, and 32 percent were sent home for five days or more (Table 6, columns 2 and 3). All three transfers reduce these sanctions substantially. The share of children sent home falls by 19 percentage points under Early Cash, 28 under Fee-Day Cash, and 33 under the Voucher (column 2), and an index of the five sanctions documented at baseline (Section 2.1) falls by 0.4 to 0.6 standard deviations (column 1).

Fee-Day Cash achieves most of the Voucher's reduction in sanctions, even though it raises fees paid by only about a fifth as much. It reduces the share of children sent home by 28 percentage points, compared with 33 points under the Voucher ($p = 0.06$ for the difference), and achieves about three-quarters of the Voucher's reduction in the shares barred from exams and denied a report card (columns 2, 5, and 7). The two transfers have almost identical effects on the share of children sent home for five days or more (column 3; $p = 0.71$), even though the Voucher raises the share who clear the fee by a further 35 percentage points.

One explanation is that schools respond to partial payment. The cash transfers raise the share of children with any recorded payment by only 8 to 9 percentage points (Supplementary

Table 6: Effects on School Sanctions for Late or Unpaid Fees

	(1) Sanctions Index (SD)	(2) Suspended from School	(3) Suspended 5+ Days	(4) Started Term Late	(5) Denied Exams	(6) Denied School Meals	(7) Denied Report Card
T1: Early Cash	−0.407***	−0.193***	−0.119***	−0.074***	−0.042**	−0.113***	−0.092***
	(0.061)	(0.031)	(0.028)	(0.023)	(0.018)	(0.030)	(0.026)
T2: Fee-Day Cash	−0.506***	−0.276***	−0.192***	−0.084***	−0.052***	−0.121***	−0.106***
	(0.060)	(0.030)	(0.026)	(0.023)	(0.017)	(0.030)	(0.026)
T3: Voucher	−0.604***	−0.330***	−0.200***	−0.073***	−0.069***	−0.153***	−0.137***
	(0.059)	(0.029)	(0.026)	(0.023)	(0.016)	(0.030)	(0.025)
Control Mean	0.000	0.620	0.323	0.193	0.107	0.503	0.281
Observations	1,936	1,936	1,934	1,936	1,936	1,936	1,936
p-value: T1 = T2	0.085	0.005	0.002	0.642	0.490	0.777	0.552
p-value: T2 = T3	0.076	0.056	0.710	0.589	0.208	0.280	0.166
p-value: T1 = T3	0.000	0.000	0.000	0.942	0.053	0.171	0.049

Notes: This table reports intention-to-treat estimates as in Table 5 for the 1,936 children enrolled in any school during the term. Outcomes are parent-reported sanctions for late or unpaid fees during Term 1. The sanctions index sums the five binary sanctions in columns 2 and 4–7 and is standardized to the control group. Column 3 excludes two children with invalid reports of the number of days suspended. Robust standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Table A5, column 3) but reduce the share sent home by 19 to 28 points. Parents who would have paid part of the fee anyway may have paid somewhat more or somewhat earlier, which may be enough to keep the child in school without clearing the fee. The additional payment the Voucher makes beyond Fee-Day Cash would then avert few further sanctions. The model in Section 3 abstracts from this by treating the fee as either paid or unpaid.[32]

Parents also report fewer children sent home under Fee-Day Cash than under Early Cash, by 8 percentage points (column 2; $p = 0.005$), even though the two transfers raise fees paid by the same amount. Attendance, which enumerators observed directly, does not differ significantly between the two cash transfers (Table 5, column 7; $p = 0.56$), so this difference should be interpreted with some caution. Fee-Day Cash parents, who still held the cash when the term began, may have paid somewhat sooner. Eleven of the 30 study schools record the date of each fee payment. In these schools, the two cash arms had the same share of children with a recorded payment on the first day of term, but by March 10 the share was 65 percent

[32]These comparisons are approximate. Payment is measured among children who stayed and sanctions among all enrolled children, and the cash effects on payment are less robust to selection than the Voucher's. The comparison between Fee-Day Cash and the Voucher also attributes their differences to their effects on payment, although the two arms differ in when the money arrives and in the Voucher's re-enrollment requirement. Because the sanctions are reported by parents, the comparisons could also be affected by reporting. Attendance, which enumerators observed directly, shows the same pattern in weaker form.

under Fee-Day Cash and 58 percent under Early Cash, although this difference is imprecise ($p = 0.19$; Supplementary Appendix Figure A7).

The transfers also reduce the pressure that headteachers put on parents, by more under Fee-Day Cash and the Voucher than under Early Cash (Supplementary Appendix Table **??**, column 1). In the control group, about half of children report fee-related stigma from classmates and from teachers or other adults. Early Cash and the Voucher reduce stigma from classmates by 5 to 6 percentage points, and the Voucher reduces stigma from teachers and other adults by 8 points, although an index of stigma does not change significantly in any arm (Supplementary Appendix Table A8, columns 1 to 3).

Enrollment. Nearly every child is enrolled in school during the term, since enrolling requires no payment. In the control group, 98.4 percent of children were enrolled in some school during the term, and no transfer changes this share (Table 5, column 3). Children instead leave school during the term, often after being sent home over unpaid fees. Consistent with its effect on sanctions, the Voucher halves dropout during the term, from 5.1 to 2.4 percent, a decline of 54 percent (column 4; $q = 0.049$).[33] The cash transfers reduce dropout by about half as much as the Voucher, by 1.1 and 1.5 percentage points. These estimates are not statistically significant, but neither can be distinguished from the Voucher's ($p = 0.15$ and $p = 0.25$), so the point estimates follow the ranking in Prediction 2. Part of the Voucher's effect may also reflect its re-enrollment requirement.

Moving to another school is also common, with 11.9 percent of control children moving during the term (column 5). Children who are sent home over unpaid fees may enroll elsewhere rather than return. All three transfers reduce moves, by 2.8 to 3.5 percentage points (q-values between 0.089 and 0.145). The cash transfers, which carry no re-enrollment requirement, have effects similar to the Voucher's, so the requirement is not the only reason children are less likely to move.

Attendance. Consistent with the reductions in sanctions, the transfers also raise attendance. Among children who stayed at their baseline school, the Voucher raises attendance by 4.9 percentage points from a control mean of 77 percent, which amounts to almost three additional days of school over the term (Table 5, column 7; $q = 0.003$). Fee-Day Cash raises attendance by 3.2 points ($q = 0.063$), two-thirds of the Voucher's effect and statistically indistinguishable from it ($p = 0.21$). Like its effect on sanctions, this is much larger than its small effect on fees paid would suggest. The effect of Early Cash, 2.4 points, is not significant.

Because the transfers also reduce moves to other schools, the effects are larger when

[33] For comparison, free school uniforms in Kenya reduced girls' dropout by 3.1 percentage points over three years, from 18.8 percent (Duflo et al., 2015).

children who moved are counted as absent from their baseline school, at 4.2, 5.2, and 6.8 percentage points under Early Cash, Fee-Day Cash, and the Voucher, from a control mean of 70 percent (column 6). Among children who stayed, the Lee bounds on the Voucher's effect exclude zero (Supplementary Appendix Table A4). Most of the gain comes from children who remain enrolled through the term attending more often, rather than from fewer dropouts, which is consistent with fewer children being sent home (Supplementary Appendix Figure A6).[34]

The effect of Fee-Day Cash is similar to the average effect of conditional cash transfers on primary-school attendance, 2.7 percentage points (García and Saavedra, 2017), and relative to the amount transferred, the Voucher's effects are at the high end of those found in randomized evaluations of other programs that reduce the cost of schooling (Supplementary Appendix Figure A8).[35]

Learning. The attendance gains do not translate into detectable learning gains after one term (Table 7). The estimates on the learning index range from 0.000 to 0.059 standard deviations, and the 95 percent confidence intervals exclude effects larger than 0.15 standard deviations in every arm (column 1). Because enumerators administered the Uwezo and Beyond Basics assessments at home (Section 2.6), the scores include children who were barred from school examinations or who left school (Jones et al., 2014). The English and math components show the same pattern (columns 2 and 3), with the only individually significant estimate, a 0.096 standard-deviation effect of Early Cash on math, significant at the 10 percent level before any adjustment for multiple testing. School examinations also show no effects, although children with unpaid fees can be barred from sitting them, so treatment changes who is tested (Supplementary Appendix Table A10).

A null after one term is consistent with the short exposure, fewer than three additional

[34] We find no evidence that the transfers changed the attendance of control children in the same classrooms, as a merit scholarship for girls in Kenya did for teachers (Kremer et al., 2009). In the daily class registers, the attendance of control children is statistically indistinguishable from that of classmates who were never in the study, with a difference of 1.5 percentage points (standard error 1.3) on a classmate mean of 92 percent (Supplementary Appendix Table A7). This comparison would not detect effects common to the whole class.

[35] Following García and Saavedra (2017), we divide each effect by the program's average monthly transfer per child, spreading our one-time transfer over the term (Table A9). By this measure, the Voucher raises attendance by about 2.0 percentage points per dollar in the full sample and 1.5 among children who stayed, compared with 1.6 and 2.2 for a conditional cash transfer to parents and a voucher offered to girls in Mozambique (De Walque and Valente, 2023) and 1.1 for the conditional transfer in Malawi (Baird et al., 2011). For enrollment, the Voucher's ratio of 0.8 is similar to those of the more effective conditional cash transfer programs, although the confidence intervals overlap widely. Ratios of this kind favor small transfers when effects are concave in transfer size, and our estimates cover a single term, whereas the comparison programs ran for longer. Free school uniforms in Kenya reduced absenteeism by 37 percent, but eight years later there is no evidence of effects on primary school completion (Evans and Ngatia, 2021). The ratios also exclude administrative costs, which range from 4 to 63 cents per dollar transferred across conditional cash transfer programs (García and Saavedra, 2017).

Table 7: Effects on Learning

	(1) Learning Index	(2) Literacy Index	(3) Numeracy Index
T1: Early Cash	0.059	0.003	0.096*
	(0.044)	(0.043)	(0.052)
	[0.158]		
T2: Fee-Day Cash	0.015	0.009	0.030
	(0.045)	(0.044)	(0.051)
	[0.329]		
T3: Voucher	0.000	−0.006	0.020
	(0.044)	(0.044)	(0.051)
	[0.362]		
Control Mean	0.000	0.000	0.000
Observations	1,956	1,956	1,959
p-value: T1 = T2	0.327	0.903	0.209
p-value: T2 = T3	0.743	0.734	0.846
p-value: T1 = T3	0.180	0.818	0.140

Notes: This table reports intention-to-treat estimates as in Table 5, controlling for the child's baseline score. Outcomes are endline Uwezo and Beyond Basics scores, standardized within grade to the endline control group, and the learning index is the average of the literacy and numeracy scores. Sample sizes differ because a few endline assessments are incomplete. Sharpened q-values are in brackets. Robust standard errors are in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

days of school in a setting where teacher absence is high (Bold et al., 2017). Comparable programs raised test scores, if at all, over longer horizons.[36]

5.4 Household Outcomes

The Voucher prevents parents from using the transfer for anything other than the fee, so its larger effects on schooling could come at the expense of other needs. This is the standard argument for cash over in-kind transfers (Currie and Gahvari, 2008), and in models of self-control, the loss of flexibility is the cost of commitment (Amador et al., 2006). In Malawi, for example, the conditional transfer raised schooling by more than the unconditional transfer, but only the unconditional transfer reduced teenage marriage and pregnancy, entirely among girls who left school (Baird et al., 2011). Table 8 compares the three transfers on food insecurity, disagreement within the household over school expenses, changes in livestock and durable assets, transfers to other households, and borrowing, and Supplementary Appendix Tables **??** and **??** report additional household outcomes.

On these outcomes, we find no evidence that Voucher households fare worse than cash households. Food insecurity falls by 0.12 to 0.17 standard deviations in every arm, and the Voucher's effect is statistically indistinguishable from those of Early Cash and Fee-Day Cash (column 1; $p = 0.91$ and $p = 0.47$). Part of this decline may reflect children eating at school, since every transfer reduces the share of children denied school meals, by 15 percentage points under the Voucher and 11 to 12 points under cash (Table 6, column 6). The share of households that disagree over school expenses falls by about a quarter in every arm (column 2), and no arm detectably reduces livestock or durable assets (columns 3 and 4).

One reason is that the Voucher replaces fee payments that many parents would otherwise have made themselves. In the control group, 56 percent of children have a recorded payment toward the fee (Supplementary Appendix Table A5, column 3), and for their parents a transfer paid to the school frees money that would otherwise have gone to the fee. In the model, such a parent values the Voucher exactly as they value cash, as with any in-kind transfer of a good the household would have bought anyway (Section 3.3; Moffitt, 1989).

Cash, in contrast, leaves parents free to spend on other things, and some of it reached the child's siblings. Among siblings aged 6 to 17, 96 percent attend school in the control group, and no arm changes this share, so the Voucher does not reduce the schooling of the child's siblings (Supplementary Appendix Table A11). Fee-Day Cash, however, reduces the

[36] In a review of cash transfer programs, effects on test scores are "small at best" (Baird et al., 2014), and in Cambodia, scholarships that raised completed schooling by 0.6 grades had no significant effect on test scores (Filmer and Schady, 2014). The conditional transfer in Malawi raised English reading comprehension by 0.14 standard deviations after two years (Baird et al., 2011), whereas the labeled transfer in Morocco had a modest and statistically insignificant effect on math scores over the same period (Benhassine et al., 2015).

Table 8: Effects on Household Outcomes

	(1) Food Insecurity (Index)	(2) Fee Disagreement	(3) Δ Livestock (Index)	(4) Δ Durables (Index)	(5) Net Transfers Out	(6) Took Any Loan	(7) Borrowed for School
T1: Early Cash	−0.115*	−0.080***	0.133*	0.049	10.1**	0.083***	0.087***
	(0.062)	(0.028)	(0.079)	(0.067)	(4.9)	(0.032)	(0.031)
T2: Fee-Day Cash	−0.172***	−0.084***	0.035	0.024	2.6	0.089***	0.051*
	(0.065)	(0.028)	(0.069)	(0.068)	(4.9)	(0.032)	(0.030)
T3: Voucher	−0.123*	−0.078***	0.121*	−0.002	1.2	0.025	0.011
	(0.066)	(0.028)	(0.069)	(0.064)	(5.5)	(0.032)	(0.031)
Control Mean	0.000	0.327	0.000	0.000	−6.8	0.491	0.343
Observations	1,960	1,957	1,960	1,960	1,939	1,942	1,955
p-value: T1 = T2	0.380	0.901	0.246	0.718	0.130	0.832	0.234
p-value: T2 = T3	0.471	0.831	0.260	0.700	0.794	0.042	0.192
p-value: T1 = T3	0.911	0.929	0.886	0.439	0.101	0.068	0.013

Notes: This table reports intention-to-treat estimates as in Table 5. Outcomes are from the endline parent survey. Food insecurity is the Food Insecurity Experience Scale score, standardized to the control group, so higher values indicate more food insecurity. Fee disagreement indicates any disagreement within the household over school expenses. Columns 3 and 4 are indices of the change since baseline in holdings of seven livestock species and five durable assets. Each item's change is winsorized at the 1st and 99th percentiles of nonzero changes and standardized, and the average is divided by its control-group standard deviation. Net transfers out are money given to other households minus money received since January 2026, in thousands of UGX. Columns 1, 2, and 5 control for the baseline value of the outcome, and column 6 for having an active loan at baseline. Robust standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

share of siblings sent home over fees by 7 percentage points, and both cash transfers raise the share of siblings who receive school meals, whereas the Voucher does neither. Parents thus appear to have used part of the cash for the schooling costs of other children, which the Voucher does not allow.

Early Cash, which parents held for several weeks before fees were due, also turns households from net recipients into net givers, raising net transfers to other households by about UGX 10,000 (Table 8, column 5). Fee-Day Cash and the Voucher produce no comparable outflow, although their differences from Early Cash are not statistically significant ($p = 0.13$ and $p = 0.10$). The outflow is consistent with claims on cash that is held for weeks before fees are due (Jakiela and Ozier, 2016), a motive that does not appear to drive demand for the restrictions (Section 4.3).

Both cash transfers also raise borrowing, much of it for schooling, whereas the Voucher does not. The share of households that took a loan rises by 8 to 9 percentage points under cash, from a control mean of 49 percent (Table 8, column 6), and the share that borrowed for school expenses rises by 9 points under Early Cash and 5 under Fee-Day Cash, compared with 1 point under the Voucher (column 7). Cash recipients therefore appear to have financed part of their schooling costs with credit. The cash may have made it easier to borrow, or part of it may have gone to other uses before school costs fell due.[37] Our follow-up covers a single term, so we cannot detect costs or benefits that arise later.

6 Anticipated and Realized Effects of Commitment

In this section we ask whether parents' demand for each restriction predicts its effect on their child's schooling. Parents may pay for a restriction because they expect it to change their behavior, and those expectations may be wrong. We therefore compare parents' baseline forecasts of their own fee payment with realized fee payment, and we examine how one term of experience with each transfer changes demand.

6.1 Does Demand Predict Treatment Effects?

Many programs let recipients choose among programs or allocate programs by willingness to pay, on the premise that the people who choose a program are those whose behavior responds most to it (Roy, 1951; Alatas et al., 2016; Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2019). For restricted transfers, choice has the further appeal that a restriction

[37] Enumerators reported that village savings groups lend more readily to members who have money saved in the group, which suggests that the amount households could borrow was tied to their savings and that the cash transfers relaxed this credit constraint.

recipients choose for themselves does not override their own judgment about how to spend the money (Currie and Gahvari, 2008). The first rationale requires that the parents who pay for a restriction are those whose behavior it changes most. Because we elicited demand before random assignment, we can test this directly.

For earmarking, we find the opposite. Figure 5 compares parents who chose the Voucher over Fee-Day Cash of the same amount at baseline, 69 percent of the sample, with parents who chose the cash. Among parents who chose cash, the Voucher raises total fees paid by UGX 23,900 (s.e. 2,100) relative to Fee-Day Cash, an amount equal to 68 percent of the transfer. Among parents who chose the Voucher, it raises fees paid by UGX 15,100 (1,500), or 43 percent of the transfer. The difference is UGX −8,900 (2,700; $p = 0.001$).

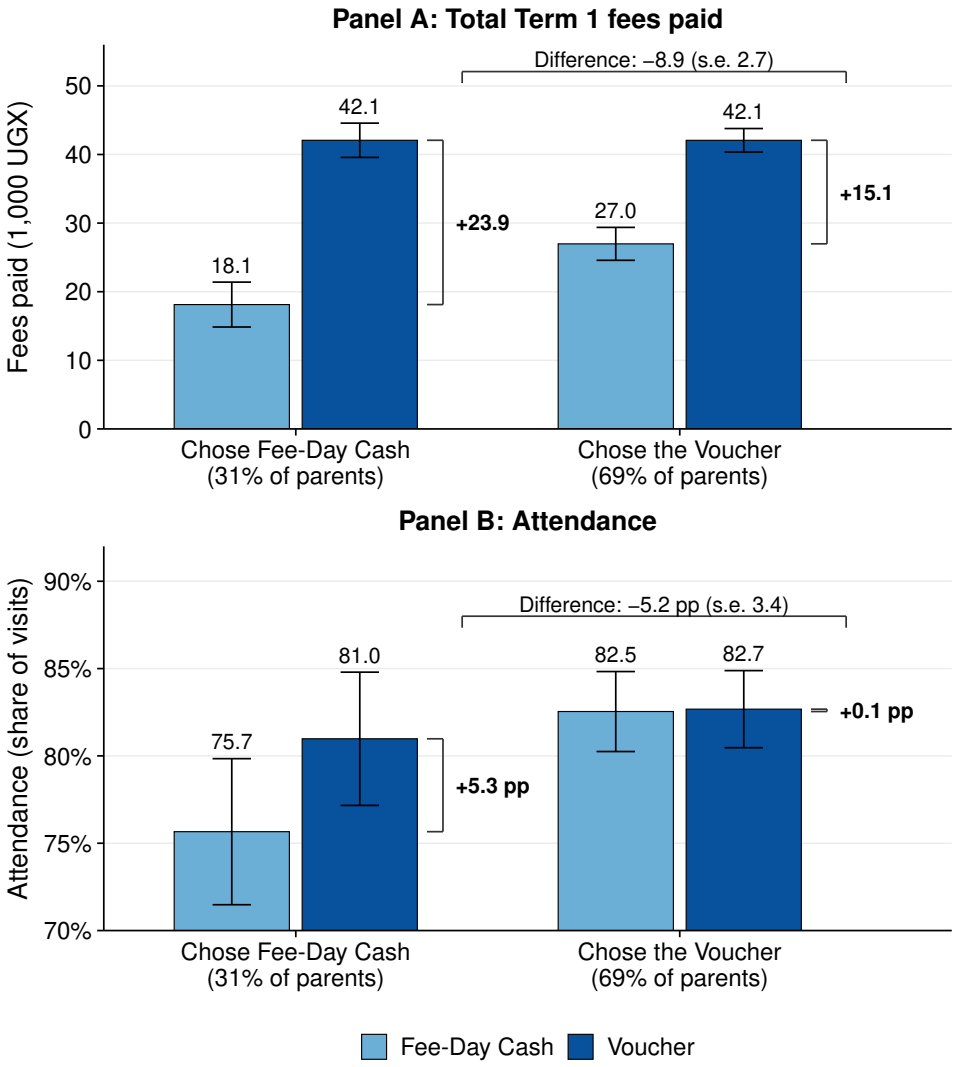
The difference comes entirely from behavior under cash. Voucher recipients pay about UGX 42,100 in both groups. Under Fee-Day Cash, parents who chose cash pay UGX 18,100, no more than control parents (difference UGX −200, s.e. 2,400). Parents who chose the Voucher pay UGX 27,000 under Fee-Day Cash, UGX 7,100 (1,800) more than control parents, so they put about one-fifth of the cash toward the fee. An earmark changes spending only to the extent that the household would not have spent the transfer on the earmarked good anyway (Moffitt, 1989; Currie and Gahvari, 2008). Parents who ask for the Voucher already direct part of an unrestricted transfer to fees, so the Voucher changes less of their spending. Parents who prefer cash use very little of it for fees, which makes the Voucher more effective for them.

Part of the Voucher's effect on fees paid is mechanical, because the Voucher pays the school directly. Attendance shows whether the difference between the groups extends to children's participation in school, and it follows the same pattern. The Voucher raises attendance by 5.3 percentage points (2.9) relative to Fee-Day Cash among parents who chose cash, and by 0.1 percentage points (1.6) among parents who chose the Voucher. The difference is not statistically significant ($p = 0.12$), but its 95 percent confidence interval rules out an attendance effect more than 1.4 percentage points larger among parents who chose the Voucher.

These findings add to evidence that demand for commitment does not always identify the people whose behavior responds most to it (Carrera et al., 2022).[38] In our setting the distinction is especially consequential because the Voucher pays the fee even when the parent

[38] In Carrera et al. (2022), a gym-attendance contract raises attendance by 3.5 visits among participants who wanted it and by 4.0 visits among those who did not, and take-up is at best weakly related to measures of present focus. In Schilbach (2019), offering a choice of sobriety incentives raises sobriety by about as much as assigning them, although only about two-thirds of participants choose them. In contrast, workers in Kaur et al. (2015) whose output rises most as payday approaches are more likely to choose dominated contracts, and the option to choose them raises output more among these workers.

Figure 5: Effects of the Voucher Relative to Fee-Day Cash, by Baseline Choice



Notes: This figure shows Term 1 fees paid (Panel A) and attendance (Panel B) under Fee-Day Cash and the Voucher, separately for parents who chose Fee-Day Cash and parents who chose the Voucher at equal amounts at baseline. The sample is the 894 children in these two arms who did not transfer to another school during the term, and outcomes are defined as in Table 5. Bars are strata-adjusted means from a regression of the outcome on group-by-arm indicators and randomization-strata fixed effects. Brackets report the Voucher's effect relative to Fee-Day Cash within each group and the difference between the two effects, with its standard error. Error bars represent 95% confidence intervals based on robust standard errors.

would not have paid it with cash. Its effect relative to cash is therefore largest for the parents who are least likely to choose it.

The results are similar under other specifications (Supplementary Appendix Table A12). Each additional UGX 10,000 of earmarking premium reduces the Voucher's effect on fees paid by UGX 2,700 (900; $p = 0.003$). Because fees and premia both rise with the child's grade, part of the variation in demand lies between randomization strata. Within strata, the difference between the two groups is UGX −6,100 (2,900; $p = 0.03$). The groups do not differ in the Voucher's effect on dropout. Demand for delay does not predict the effect of delay. Fee-Day Cash raises fees paid relative to Early Cash by the same amount among parents who chose Fee-Day Cash at equal value and among those who chose Early Cash (difference UGX 100, s.e. 3,100).

We next consider what these differences imply for allocating transfers by demand (Supplementary Appendix Table A13). Two comparisons answer different questions. First, giving Vouchers to the parents who chose them, with no transfer to the others, produces fees paid and attendance that are not detectably different from giving the same number of Vouchers at random, again with no transfer to the others (differences of UGX −200, s.e. 600, and −0.4 percentage points, s.e. 0.7). Duflo et al. (2011) similarly find that letting farmers choose the timing of a fertilizer offer raises purchases by no more than assigning it. Second, letting every parent choose between Fee-Day Cash and the Voucher produces average fees paid of UGX 35,300, compared with UGX 42,400 if every parent received the Voucher (difference UGX −7,000, s.e. 600), and attendance 1.8 percentage points lower (0.8). Letting parents choose thus achieves 69 percent of the increase in fees paid, relative to no transfer, that universal Vouchers achieve. These comparisons concern schooling outcomes, not household welfare. A parent who chooses cash may have other uses for it that matter to the household, so higher fee payment under universal Vouchers does not by itself imply that universal Vouchers are the better policy.

Demand responds to parents' forecasts, but the forecasts do not predict treatment effects. Parents who forecast a larger increase in their chance of clearing the fee under the Voucher pay more for it (Section 4.2). Yet a 10-percentage-point larger forecast gain is associated with a UGX 400 smaller effect of the Voucher on fees paid (500) (Supplementary Appendix Table A12). Parents who expect the Voucher to change their fee payment more do not see larger effects. In the model, demand and effects line up when parents correctly forecast their own present focus (Prediction 3). The next subsection asks how accurate parents' forecasts are.

6.2 Forecast Errors and Partial Naïveté

At baseline, before treatment assignment, parents reported the probability that they would fully clear their child's Term 1 fees with no transfer and under each transfer (Section 2.4). We compare these forecasts with realized full clearing in each arm. For each restriction, the *anticipated effect* is the average within-parent difference between the forecasts under the two transfers it distinguishes, and the *realized effect* is the corresponding difference in intention-to-treat effects. Following Augenblick and Rabin (2019) and Allcott et al. (2022), this comparison measures whether parents correctly predict how their own behavior responds to each transfer (see also Acland and Levy, 2015; Mahajan et al., 2026).

Parents are optimistic about clearing their fees under every transfer (Figure 6, Panel A). They forecast a 42 percent chance of clearing with no transfer, compared with 12 percent realized in the control group. Under Early Cash and Fee-Day Cash they forecast 60 and 74 percent, compared with 16 and 15 percent realized, and under the Voucher 74 percent, compared with 50 percent. These errors are large relative to other settings in which people forecast their own behavior.[39] Clearing a term's fees also depends on income over the term and on the fees the school charges, so the level errors combine errors about circumstances with errors about one's own behavior. Even among Voucher recipients, whose fees are mostly paid directly to the school, realized full clearing is 24 percentage points below the forecast.

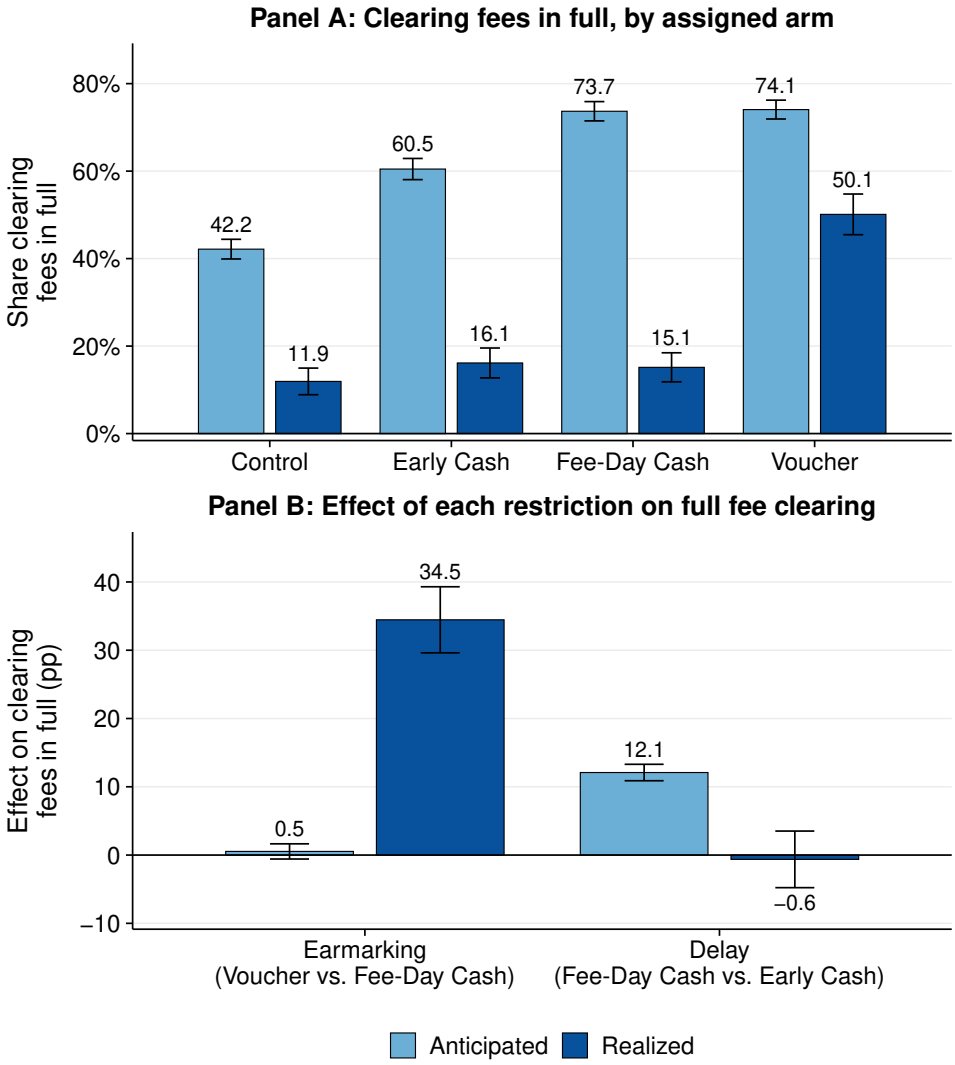
Comparing the transfers with each other removes forecast errors common to all of them. Parents expect delay to help and earmarking to do almost nothing, and the reverse occurs (Figure 6, Panel B). They expect the Voucher to raise their chance of clearing the fee by 0.5 percentage points (0.6) relative to Fee-Day Cash. The realized difference is 34.5 points (2.5), so the forecast error is −33.9 points (bootstrap s.e. 2.6), nearly the entire realized effect. They expect Fee-Day Cash to raise their chance of clearing the fee by 12.1 points (0.6) relative to Early Cash, and the realized difference is −0.6 points (2.1), a forecast error of 12.7 points (2.3). Parents expect that receiving cash when fees are due will help them pay, but the experiment shows that restricting how they use the cash matters much more. Borrowers in Allcott et al. (2022) similarly overpredict the effect of an incentive not to borrow, at about 20 percentage points against an actual 3.8.

The forecasts under the Voucher and Fee-Day Cash concern the same amount delivered on the same day, so comparing them isolates parents' forecasts of what they will do with cash in hand.[40] In the model, a sophisticated parent has a forecast error of zero, and a negative

[39] Payday borrowers in Allcott et al. (2022) forecast a 70 percent chance of borrowing again, and 74 percent do. Gym members in Carrera et al. (2022) forecast 11.5 visits without incentives and make 7.2.

[40] Forecasts were elicited before the government postponed the start of term from February 2 to February 10. Fee-Day Cash was then paid on February 2 and the Voucher on February 10, so the realized comparison also includes an eight-day difference in timing. Within the Fee-Day Cash arm, receiving the

Figure 6: Anticipated and Realized Clearing of Fees in Full



Notes: This figure compares parents' baseline forecasts of clearing Term 1 fees in full with realized full clearing for the 1,776 children who did not transfer to another school during the term. Fees are cleared in full when payments in school records are at least the fees charged, excluding the food charge. Panel A reports, by arm, the average forecast under that arm's transfer and the share of children whose fees were cleared. Panel B reports the anticipated and realized effects of earmarking and delay, defined in Section 6.2. Error bars represent 95% confidence intervals.

error arises only if the parent underestimates their present focus (Proposition 2). Together with positive willingness to pay for earmarking, which requires $\tilde{\beta} < 1$, the error is consistent with partial naïveté (O'Donoghue and Rabin, 2001). Parents anticipate difficulty using cash for fees but underestimate how much the Voucher would change their fee payment. In the model's terms, their forecasts place them in the under-saving region, where delay raises payment, while their behavior places many of them in the under-payment region, where only earmarking does. Studies that estimate both parameters find the same ordering.[41]

Parents who chose cash were no more optimistic about clearing their fees under Fee-Day Cash than parents who chose the Voucher. Among parents assigned to Fee-Day Cash, those who chose cash forecast a 74 percent chance of clearing their fees, compared with 73 percent among those who chose the Voucher. In practice, 10 and 18 percent cleared their fees, respectively.

Could parents correctly anticipate their own behavior and still underestimate the Voucher's effect? We consider three ways this could happen.

Unanticipated circumstances. The Voucher and Fee-Day Cash deliver the same amount, but unrestricted cash can also be spent on needs that arise unexpectedly. A shock on fee day raises the value of cash, and a time-consistent parent who receives Fee-Day Cash may rationally spend it on the shock, while the Voucher pays the fee regardless. If this explained the forecast error, the gap between the two transfers would be concentrated among households that experienced shocks. At endline, 49 percent of households report a negative shock, such as illness, a death, or a job loss, in January or February, when Term 1 fees fell due. These reports are balanced across arms (joint $p = 0.65$) and are not predicted by baseline forecasts ($p = 0.71$). The Voucher raises full clearing relative to Fee-Day Cash by 29.7 percentage points (3.3) among households that report no such shock and by 39.2 points (3.7) among those that report one (Supplementary Appendix Figure A9). Shocks widen the gap ($p = 0.06$), but most of it remains among households that report no shock. Households may not report every change in their needs as a shock, so these data cannot rule out this explanation entirely.[42]

Partial coverage. The forecasts concern clearing the fee in full, while the Voucher covers

transfer on February 10 rather than February 2 does not detectably raise fees paid among parents who chose the later date (Supplementary Appendix Table A6). Receiving cash at the start of term rather than six weeks earlier does not raise full clearing (Figure 6, Panel B).

[41] Bai et al. (2021) estimate $(\beta, \hat{\beta})$ of about $(0.37, 0.8)$ and Carrera et al. (2022) of $(0.55, 0.84)$.

[42] For 35 percent of children, the Voucher covers the most common fee charged in the child's randomization stratum (school, grade, and gender), so Fee-Day Cash gives the parent enough cash to clear the fee on fee day. In these cells, 18 percent of Fee-Day Cash recipients clear their fees, compared with 7 percent of control children and 99 percent of Voucher recipients. This comparison was not pre-specified, and fee levels are not randomly assigned.

85 percent of the fee on average. Optimism about paying the remaining balance would make the measured error less negative, so partial coverage does not explain why the error is so large.

Claims on cash by others. Underestimating competing demands on the parent's cash, for example from relatives, would produce the same pattern as underestimating one's own present focus. Section 4.3 finds little evidence that demand for the restrictions reflects such claims.[43]

The average forecast error can be reconciled with the negative relationship between demand and treatment effects in Section 6.1 if forecast errors vary across parents. A parent who underestimates their present focus expects to put more of the cash toward fees than they will, so they pay little for the Voucher even though it would raise their fee payment substantially. A parent who overestimates their present focus may pay for the Voucher although they would have put part of the cash toward fees anyway. With both kinds of error, demand and treatment effects can be negatively related even when parents underestimate the Voucher's effect on average.[44] Noise in measured demand would weaken the relationship (Carrera et al., 2022) but would not by itself make it negative.

6.3 Experience and Demand

At endline, we repeated the choice between the Voucher and Fee-Day Cash for a future term. Experience with the transfers changes parents' demand for earmarking. Parents who received the Voucher raise their premium, parents who received either cash transfer lower it, and control parents do not change it (Figure 7). Because the transfers were randomly assigned, these differences identify the effect of assignment to each transfer on later demand.

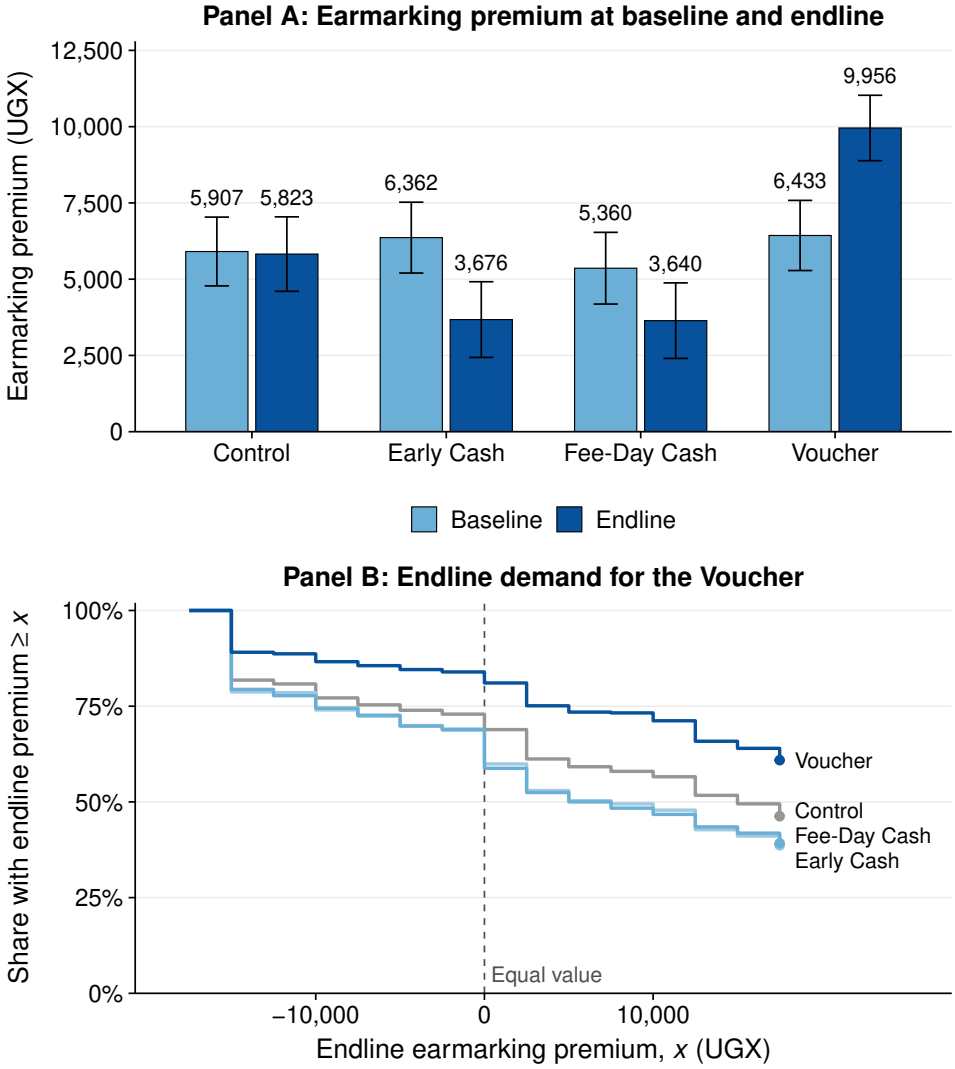
The mean premium among control parents changes from UGX 5,907 to UGX 5,823 (−83, s.e. 792). Voucher recipients raise their premium by UGX 3,524 (729), more than half of its baseline level, while Early Cash and Fee-Day Cash recipients lower it by UGX 2,687 (759) and UGX 1,719 (735). At endline, 68.9 percent of control parents choose the Voucher at equal value. Assignment to the Voucher raises this share by 12.1 percentage points, and assignment to Early Cash and Fee-Day Cash lowers it by 8.8 and 9.7 points (Supplementary Appendix Table A14).[45] The two cash arms do not differ from each other ($p = 0.77$), and

[43] Fedyk (2025) separates self-specific optimism by comparing forecasts of one's own and others' behavior. Our design identifies misprediction of the relative effect of two transfers of the same amount.

[44] In the model, a smaller effect among parents who pay for the Voucher requires that some of them overestimate their present focus (Prediction 3).

[45] In an ANCOVA Tobit specification that accounts for censoring at the bounds of the elicitation grid, assignment to the Voucher raises the endline indifference point by UGX 10,778, and Early Cash and Fee-Day Cash lower it by UGX 5,484 and UGX 4,836. The corresponding marginal effects on the observed, censored

Figure 7: Earmarking Premium at Baseline and Endline, by Treatment Arm



Notes: This figure shows the earmarking premium at baseline and at endline for the 1,960 parents in the four treatment arms. The endline premium is from the repeated earmarking task for a future term (Section 2.4). Panel A reports mean premia by arm. Panel B reports, by arm, the share of parents whose endline premium is at least the value on the horizontal axis. Error bars represent 95% confidence intervals.

each differs from the Voucher arm. Supplementary Appendix Table A15 shows similar results across alternative specifications and samples. Control parents do not revise, and a desire to stay consistent with baseline answers would attenuate rather than generate the differences. Parents may, however, report a stronger preference for whichever transfer they received, and random assignment does not separate this from other reasons for the change.

The rise in demand among Voucher recipients is consistent with other commitment settings, in which experience with a commitment device raises later demand for it.[46] The increase also gives little indication that parents found the restriction more costly once they had experienced it.

The randomized comparison identifies how experience changes demand, but not why. Voucher recipients may have learned that the Voucher raises their fee payment, but they may also have come to prefer a familiar transfer or found payment through the school convenient. The decline among cash recipients is harder to reconcile with learning about one's own present focus, since only 15 percent of Fee-Day Cash recipients cleared their fees. Cash recipients may instead have found unrestricted cash useful for other purposes, including the other schooling expenses on which they spent part of it (Section 5). Revisions within each arm are not more closely tied to realized fee payment or attendance than in the control group, although these estimates are imprecise (Supplementary Appendix Table A16).[47]

One term of experience also does not eliminate demand for earmarking. Parents in the cash arms still pay about UGX 3,600–3,700 for it on average, and 59–60 percent continue to choose it at equal value. Parents observe what they paid under the transfer they received, but not what they would have paid under the alternatives. Payday borrowers in Allcott et al. (2022) are close to sophisticated, which the authors attribute to repeated experience with clear feedback. Parents here receive no such feedback about the Voucher's effect relative to cash, so experience can change their demand for a transfer without improving their forecasts of how it compares with the others. Repeated choice would therefore not necessarily bring demand in line with treatment effects.

premium are 4,211, $-2,143$, and $-1,890$. At endline, 61 percent of Voucher recipients are at the upper bound of the elicitation, compared with 46 percent of control parents and 39 percent in either cash arm.

[46] In Schilbach (2019), participants who had received sobriety incentives were more likely than control participants to choose them later. Most workers in Brune et al. (2021) re-enroll in a deferred-wage scheme after experiencing it, and in Kaur et al. (2015) take-up of dominated contracts rises with experience among workers with the largest payday effects.

[47] Among control parents, the correlation between the baseline and endline premium is 0.13. Carrera et al. (2022) report a correlation of 0.17 across incentive levels for individual valuations of behavior change.

7 Conclusion

School fees in our setting recur at predictable times, yet parents often leave them unpaid. Our experiment shows that the form in which assistance is provided matters for whether these payments are made. Cash raises fee payment only modestly, whether delivered several weeks before the term or near its start. Paying the same amount directly to the school has much larger effects on fee payment and reduces dropout during the term by more than half. These findings suggest that difficulty holding onto money until fees are due is not the main reason cash has limited effects on fee payment.

Most parents nevertheless pay to receive cash later, and most pay to have it earmarked for school fees. They appear to recognize that their future spending decisions may conflict with what they want to do in advance. But they misjudge what the restrictions will accomplish. Parents expect delay to substantially increase their chances of clearing fees, while expecting little additional effect from earmarking. In practice, delaying cash has almost no effect on fee payment, whereas earmarking has a large one. In a model of time-inconsistent preferences, this pattern is consistent with partial naïveté: parents anticipate their own present focus but underestimate how much it will affect their fee payment.

These errors also matter for who receives commitment when programs rely on choice. The Voucher raises fee payment more among parents who prefer cash than among those who prefer the Voucher. Parents who ask for earmarking already direct more of unrestricted cash toward fees, whereas those who reject it use very little of the cash for this purpose. As a result, giving Vouchers to the parents who choose them produces no more fee payment than giving the same number of Vouchers at random. This does not establish that parents would be better off without choice: unrestricted cash can finance other important needs, including other schooling expenses. But it shows that willingness to pay for a restriction does not necessarily identify the parents whose behavior it would change most.

Experience may not resolve this problem quickly. Parents who received the Voucher increase their subsequent demand for it, while those who received cash reduce theirs. Yet each parent observes what happened under the transfer they received, not what would have happened under the alternatives. Our study covers only one term, so we cannot determine whether parents eventually learn to choose more effective restrictions or whether the effects on schooling persist. The results nevertheless point to an important distinction: parents can recognize that they may have difficulty following through on their plans without correctly predicting which restrictions will help them do so.

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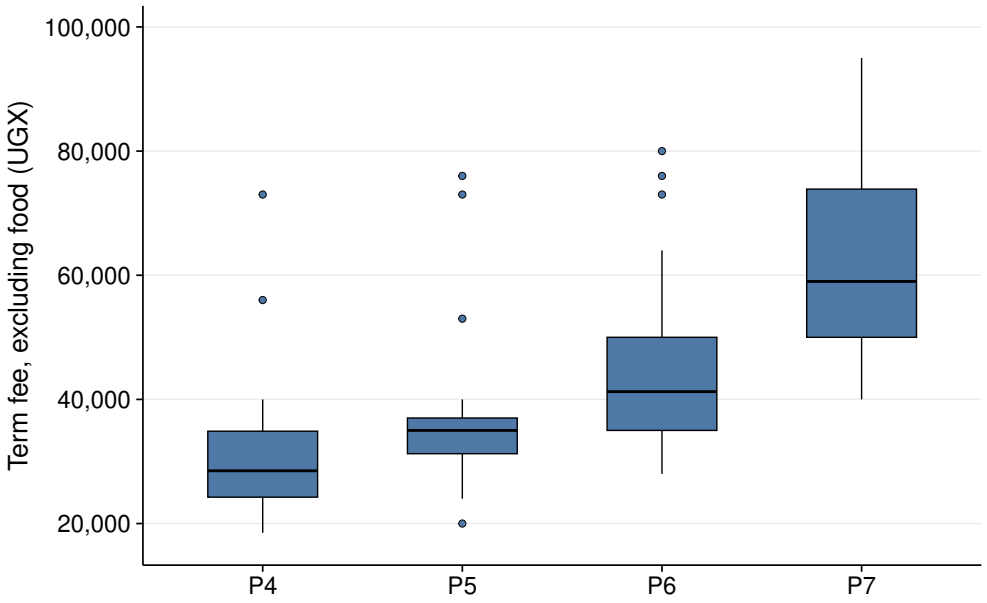
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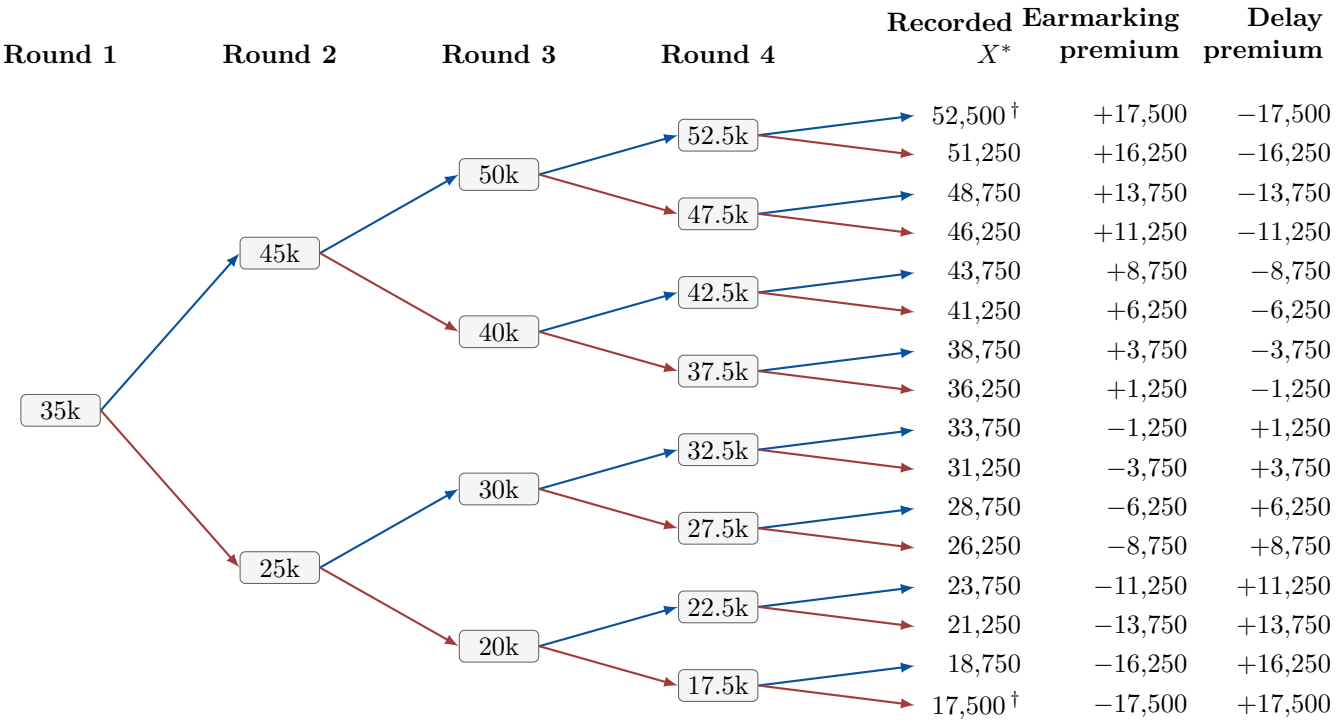
A Supplementary Tables and Figures

Figure A1: Term Fees by Grade in the Study Schools



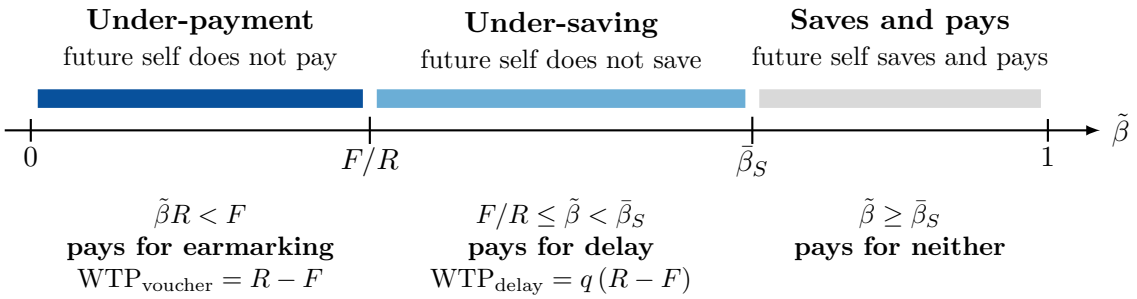
Notes: This figure shows the Term 1 2026 fee, excluding food, that each of the 30 study schools charges in each grade. Boxes show the 25th, 50th, and 75th percentiles across schools, whiskers extend to 1.5 times the interquartile range, and dots are schools outside that range.

Figure A2: The Adaptive Elicitation Procedure



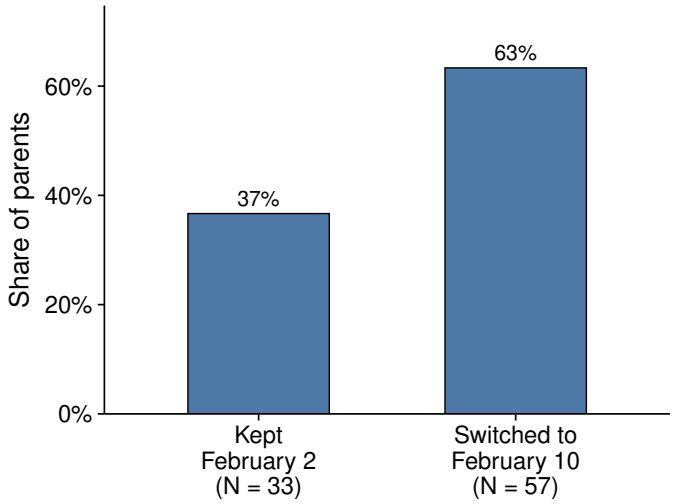
Notes: This figure shows the sequence of Fee-Day Cash offers, which is the same in the earmarking and timing tasks (Section 2.4). The fixed option is UGX 35,000 as the Voucher in the earmarking task and UGX 35,000 in Early Cash in the timing task. The recorded indifference point X^* is the midpoint of the final interval, and parents who chose the same option in all four rounds are recorded at the grid bound (†).

Figure A3: Willingness to Pay by Perceived Present Focus



Notes: This figure shows the three regions of Proposition 1 and the premium a parent pays for each restriction in each region. A lower $\tilde{\beta}$ means that the parent believes they are more present-focused. The under-saving region is non-empty when $y_L < F(1-q)$.

Figure A4: Incentivized Choice Between February 2 and February 10 Cash



Notes: This figure shows the choices of the 90 randomly selected Fee-Day Cash recipients who were offered their transfer on February 2, the original first day of term, or on February 10, the revised first day. Each parent was paid on the date they chose (Section 2.5).

Table A1: Willingness to Pay and Forecast Gains, with Controls

	(1)	(2)	(3)	(4)
Panel A: Earmarking Premium				
Forecast gain from earmarking (per 10 pp)	481***	504***	425***	579***
	(122)	(122)	(124)	(130)
Forecast gain from delay (per 10 pp)				424***
				(123)
Mean of dependent variable	6,000	5,996	5,996	5,996
Panel B: Delay Premium				
Forecast gain from delay (per 10 pp)	821***	811***	796***	789***
	(113)	(114)	(116)	(121)
Forecast gain from earmarking (per 10 pp)				−21
				(133)
Mean of dependent variable	4,569	4,569	4,569	4,569
Controls	No	Yes	Yes	Yes
Strata fixed effects	No	No	Yes	Yes
Observations	2,000	1,995	1,995	1,995

Notes: This table reports OLS regressions of the earmarking premium (Panel A) and the delay premium (Panel B), in UGX, on the parent's forecast gains, in units of 10 percentage points. Forecast gains are defined as in Figure 4, and column 1 reproduces its slopes. Controls are an indicator for Primary 4, the asset index, the child's numeracy and literacy scores, the parent's expected return to schooling, and the school's Term 1 fee. Robust standard errors are in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A2: Correlates of Forecast Gains

		Forecast Gain (pp)	
	Mean	Earmarking *(Voucher vs. Fee-Day Cash)*	Delay *(Fee-Day Cash vs. Early Cash)*
Panel A: Wealth and Liquidity			
Asset index (z-score)	0.06	0.14	−0.79
		(0.48)	(0.52)
Liquidity index (z-score)	0.00	−0.77	−1.00*
		(0.51)	(0.58)
Panel B: Child Achievement and Expected Returns			
Learning index (z-score)	0.05	0.52	0.56
		(0.52)	(0.59)
Numeracy score (z-score)	0.05	0.11	0.76
		(0.52)	(0.58)
Literacy score (z-score)	0.04	0.77	0.27
		(0.53)	(0.57)
Expected return to schooling (z-score)	0.00	0.75	−0.43
		(0.56)	(0.57)
Mean of dependent variable		0.43	12.04

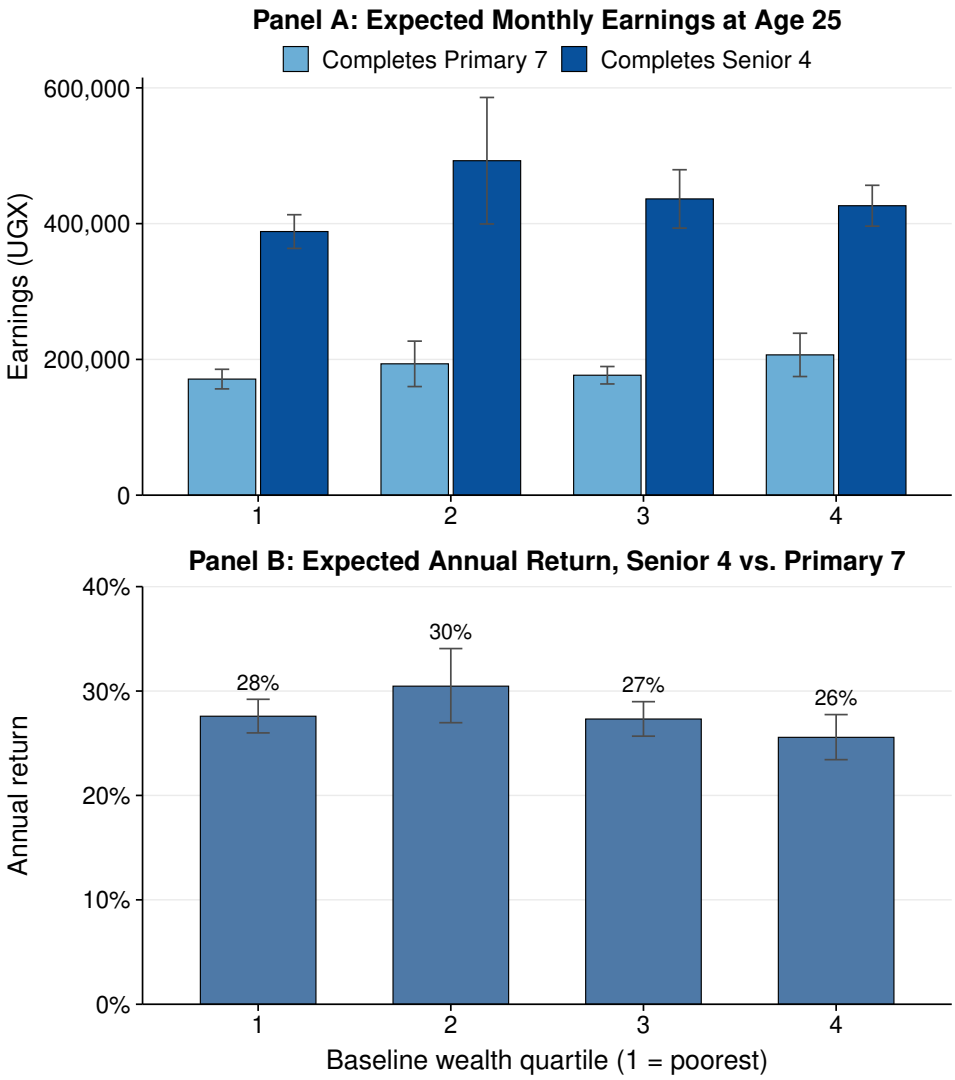
Notes: This table reports OLS regressions of the parent's forecast gain from earmarking and from delay, in percentage points and defined as in Figure 4, on each row variable separately. The learning, numeracy, and literacy rows control for grade and the asset index, as in Table 4. Robust standard errors are in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A3: Correlates of Willingness to Pay: Wealth, Credit, and Savings

		Earmarking Premium *(Voucher vs. Fee-Day Cash)*		**Delay Premium** *(Fee-Day Cash vs. Early Cash)*	
	Mean	Marg. effect	(s.e.)	Marg. effect	(s.e.)
Panel A: Wealth					
Asset index (z-score)	0.06	−816***	(272)	−685**	(277)
Income per person above median (0/1)	0.51	636	(569)	−418	(579)
Food insecurity index (z-score)	0.00	−153	(287)	313	(291)
Panel B: Access to Credit					
Has outstanding debt (0/1)	0.64	−1,012*	(589)	−117	(603)
Rejected for a loan in past 12 months (0/1)	0.26	43	(650)	−626	(667)
Could get UGX 50,000 within 24 hours (0/1)	0.21	−237	(703)	−1,037	(720)
Ease of borrowing UGX 50,000–200,000 (z-score)	0.00	−338	(283)	−726**	(288)
Panel C: Liquidity and Savings					
Liquidity index (z-score)	0.00	−834***	(283)	−675**	(288)
Saved any money in the past month (0/1)	0.76	−2,584***	(643)	−549	(682)
Has a bank account or savings group (0/1)	0.58	−1,944***	(570)	−450	(585)
Has crops in storage (0/1)	0.43	−189	(574)	−888	(584)

Notes: This table reports average marginal effects, in UGX, from separate two-limit Tobit models of each premium on the row variable alone, estimated as in Table 3. Income is household income in the past 14 days divided by household size. The liquidity index is defined in Table 4. Each model uses all parents with a non-missing row variable. Standard errors are in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Figure A5: Expected Returns to Schooling by Baseline Wealth



Notes: This figure shows parents' expectations of the child's earnings at age 25 by quartile of the baseline asset index. Panel A shows the midpoint of the parent's expected range of monthly earnings, if employed, if the child completes Primary 7 or Senior 4. Panel B shows the expected annual return, the difference in expected log earnings between the two levels divided by the four years that separate them (Attanasio and Kaufmann, 2014), converted to a percent. Error bars are 95 percent confidence intervals.

Table A4: Lee (2009) Bounds for Outcomes Observed at the Baseline School

	Fee Payments		Attendance
	(1) Total Paid (1k UGX)	(2) Share Paid	(3) Share of Weeks Present
Panel A: Treatment arms vs. Control (treatment arm trimmed)			
T1: Early Cash — trimming share 3.43%			
ITT (did-not-transfer sample)	4.649***	0.116***	0.025
	(1.417)	(0.028)	(0.016)
Lee lower bound	0.903	0.056	0.016
	(2.419)	(0.040)	(0.017)
Lee upper bound	4.825	0.129	0.051
	(1.986)	(0.035)	(0.021)
Imbens–Manski 95% CI	[-3.088, 8.102]	[-0.010, 0.187]	[-0.012, 0.085]
T2: Fee-Day Cash — trimming share 3.88%			
ITT (did-not-transfer sample)	4.762***	0.111***	0.033**
	(1.428)	(0.028)	(0.016)
Lee lower bound	0.369	0.043	0.027
	(2.411)	(0.041)	(0.016)
Lee upper bound	4.942	0.123	0.065
	(2.108)	(0.038)	(0.020)
Imbens–Manski 95% CI	[-3.601, 8.414]	[-0.024, 0.185]	[0.000, 0.098]
T3: Voucher — trimming share 3.80%			
ITT (did-not-transfer sample)	22.800***	0.595***	0.050***
	(1.260)	(0.026)	(0.015)
Lee lower bound	19.204	0.541	0.040
	(1.943)	(0.036)	(0.016)
Lee upper bound	22.904	0.620	0.077
	(1.734)	(0.034)	(0.019)
Imbens–Manski 95% CI	[16.005, 25.759]	[0.483, 0.676]	[0.014, 0.109]
Panel B: Earmarking margin (T2 trimmed)			
T3: Voucher vs. T2: Fee-Day Cash — trimming share 0.08%			
ITT (did-not-transfer sample)	18.038***	0.484***	0.017
	(1.242)	(0.027)	(0.014)
Lee lower bound	17.980	0.486	0.012
	(1.771)	(0.033)	(0.016)
Lee upper bound	18.127	0.488	0.013
	(1.838)	(0.033)	(0.015)
Imbens–Manski 95% CI	[14.577, 21.658]	[0.422, 0.552]	[-0.018, 0.043]
Share not transferring: Control 0.881; T1 0.912; T2 0.916; T3 0.916			
Observations (did not transfer)	1,776	1,776	1,776
Observations (randomized)	1,960	1,960	1,960

Notes: Fee records (columns 1–2) and spot checks at the baseline school (column 3) are compared among children who did not move to another school, and the share who move is 2.8 to 3.5 percentage points lower in every treatment arm (Table 5, column 5). Each comparison trims the arm with more children who stayed by its excess share, from the tail of its outcome distribution that minimizes the difference for the lower bound and from the tail that maximizes it for the upper bound (Lee, 2009). Panel A trims each treated arm relative to the control group and assumes that no child moves schools because of treatment. Panel B trims Fee-Day Cash relative to the Voucher and assumes that every child who stays under the Voucher would also stay under Fee-Day Cash. ITT rows use strata fixed effects, whereas the bounds compare trimmed means without them, so the bounds do not necessarily contain the ITT estimate. Column 3 is the share of weeks present, with dropouts counted as absent, estimated at the child level, so its ITT differs slightly from Table 5, column 7. Standard errors of the bounds are bootstrapped (1,000 replications, resampling within arm), and the Imbens–Manski intervals cover the effect with 95 percent probability (**?**). $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A5: Effects on Any Fee Payment and Full Clearing

	(1) Any Payment	(2) Cleared Fee
T1: Early Cash	0.086***	0.045**
	(0.030)	(0.021)
T2: Fee-Day Cash	0.079***	0.039*
	(0.030)	(0.021)
T3: Voucher	0.429***	0.384***
	(0.024)	(0.024)
Control Mean	0.557	0.119
Observations	1,776	1,776
p-value: T1 = T2	0.824	0.765
p-value: T2 = T3	0.000	0.000
p-value: T1 = T3	0.000	0.000

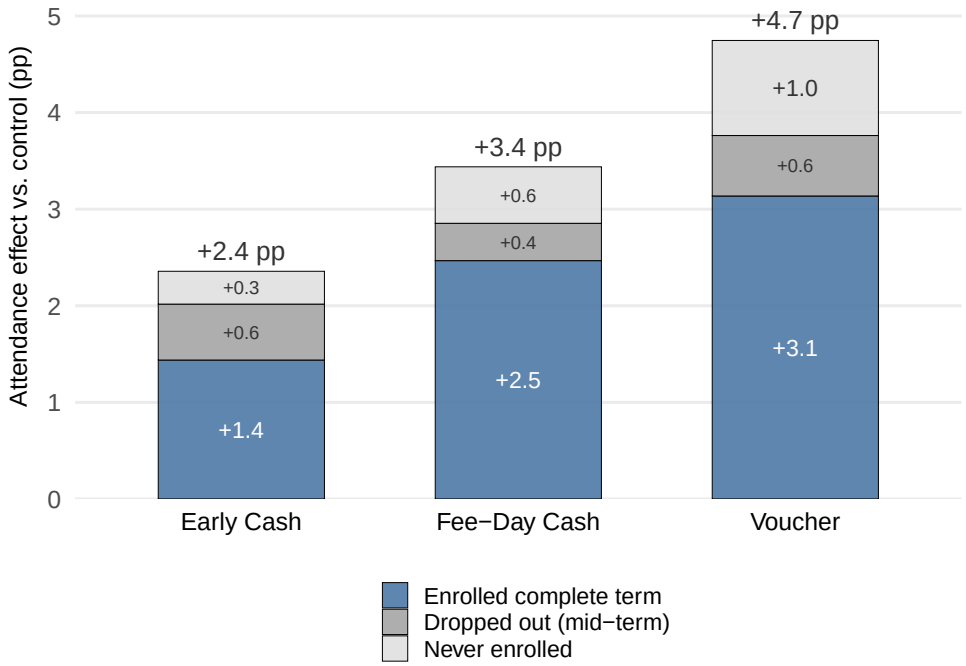
Notes: This table reports intention-to-treat estimates as in Table 5 for the 1,776 children who did not move to another school. Column 1 indicates any recorded payment toward the Term 1 fee, and column 2 that recorded payments cover the full fee excluding the food charge. Payments include in-kind contributions valued at the school's prices, and missing records are coded as zero. Robust standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6: The February Timing Sub-Experiment: IV Estimates Within Fee-Day Cash

	Fee Payments		Enrollment		Attendance
	(1) Total Paid (1k UGX)	(2) Share Paid	(3) Dropped Out	(4) Transferred Schools	(5) Did Not Transfer
Paid on February 10 (IV)	−3.607	−0.043	0.012	−0.010	0.021
	(3.786)	(0.076)	(0.034)	(0.047)	(0.035)
February-2 Mean	24.367	0.477	0.032	0.090	0.805
Observations	446	446	487	487	5,101
First stage: offered choice	0.671	0.671	0.632	0.632	0.671
First-stage F	208.0	208.0	185.5	185.5	275.2

Notes: Fee-Day Cash arm only. Being paid on February 10 is instrumented with random assignment to the binding date-choice offer (Section 2.5); two-stage least squares with strata fixed effects and robust standard errors, clustered by child in the attendance panel. The first-stage coefficient is the offer's effect on being paid February 10; the first-stage F is Kleibergen–Paap. Outcomes and samples match Table 5, except that three children who are the only Fee-Day Cash child in their randomization stratum are dropped.
$^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Figure A6: Decomposition of the Attendance Effect by Enrollment Status



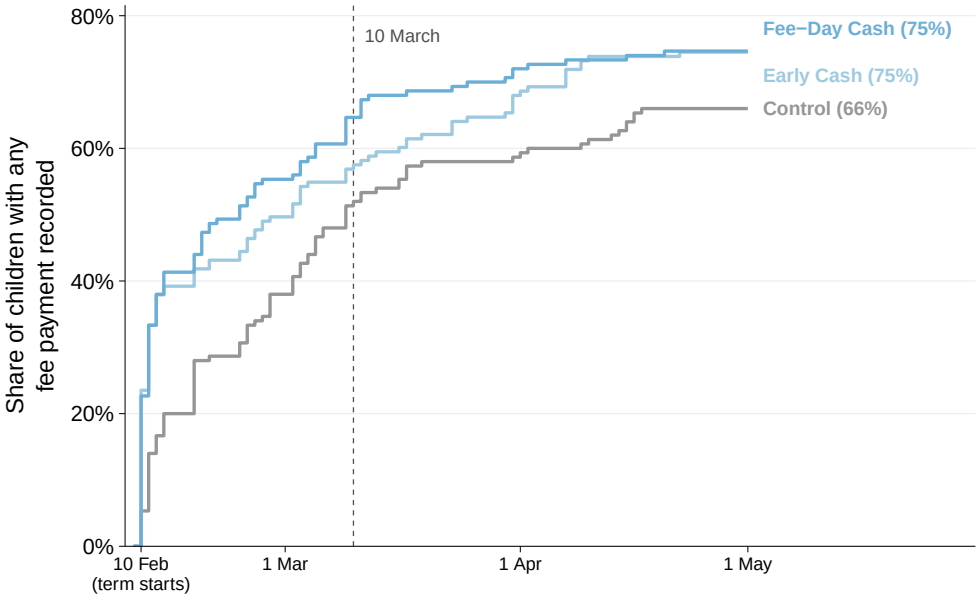
Notes: This figure splits the difference in mean attendance between each arm and the control group, for the 1,776 children who did not move to another school, into three parts with midpoint weights (Kitagawa, 1955): higher attendance among children enrolled for the full term, fewer children dropping out during the term, and fewer children who never enrolled. The parts sum to the raw difference in means, which differs slightly from Table 5, column 7. Because treatment affects enrollment status, the decomposition is descriptive.

Table A7: Attendance of Control Children Relative to Classmates Outside the Study

	(1) Attendance Rate over the Term	(2) Daily Attendance
Control child	0.015	0.016
	(0.013)	(0.014)
Fixed effects	Class	Class by day
Classmate mean	0.920	0.923
Observations	3,353	165,092

Notes: This table compares the attendance of control children with that of classmates in the same Primary 5 or Primary 7 class who were not in the study, using the daily class registers for Term 1. Control children are those in the attendance sample of Table 5, column 7, who ever enrolled. Column 1 uses each child's attendance rate over the term, and column 2 daily attendance. Standard errors clustered by class are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A7: Timing of Fee Payments



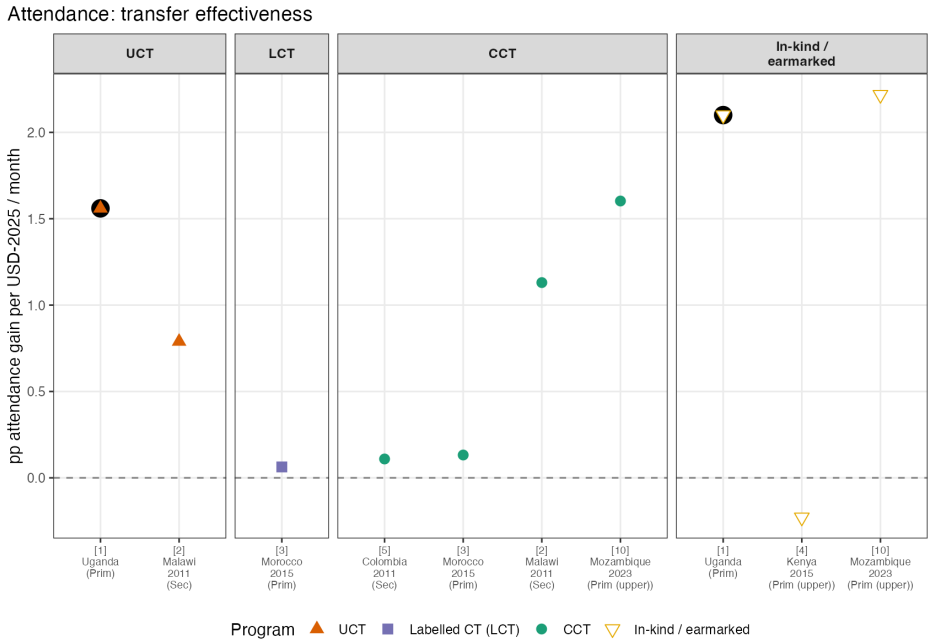
Notes: This figure shows the share of children with any recorded payment toward the Term 1 fee, in cash or in kind, by date, for the 611 children who did not move to another school in the 11 schools that record the date of each payment. Voucher payments, which schools recorded on February 10 for 95 percent of Voucher children, are not shown.

Table A8: Effects on Headteacher Pressure and Stigma

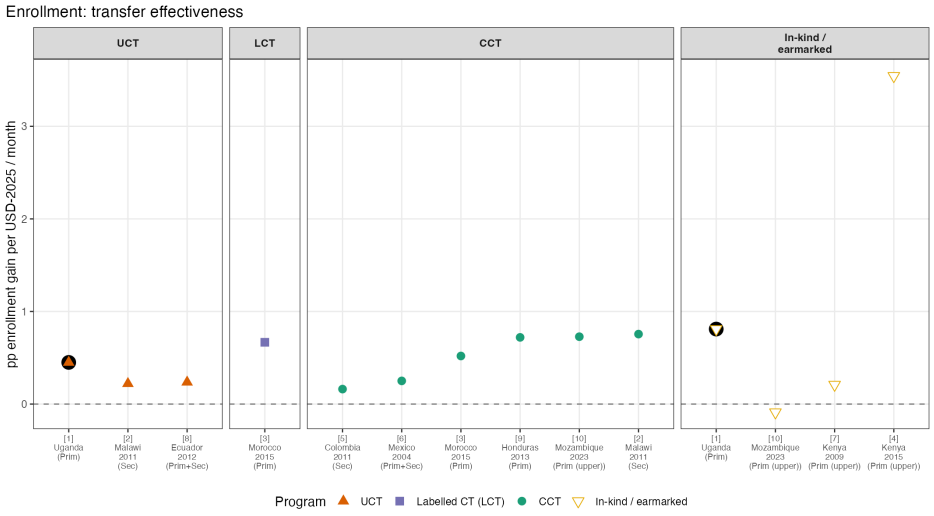
	(1) Headteacher Pressure (1–10)	(2) Stigma Index	(3) Peer Stigma	(4) Adult Stigma
T1: Early Cash	−0.804***	−0.100	−0.062**	−0.038
	(0.217)	(0.064)	(0.032)	(0.032)
T2: Fee-Day Cash	−1.275***	−0.033	−0.002	−0.028
	(0.210)	(0.064)	(0.032)	(0.032)
T3: Voucher	−1.379***	−0.094	−0.053*	−0.084***
	(0.210)	(0.065)	(0.032)	(0.032)
Control Mean	5.673	0.000	0.524	0.539
Observations	1,960	1,907	1,907	1,907
p-value: T1 = T2	0.027	0.303	0.058	0.758
p-value: T2 = T3	0.618	0.354	0.115	0.083
p-value: T1 = T3	0.007	0.924	0.759	0.153

Notes: This table reports intention-to-treat estimates as in Table 5. Column 1 is the parent's rating of pressure from the headteacher over fees, from 1 to 10. Columns 2–4 are from the endline child survey: peer and adult stigma indicate that the child reported fee-related mistreatment or shame from classmates or from teachers or other adults, and the index standardizes their sum. Stigma measures are missing for 53 children. Robust standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A8: Effects of Cost-of-Schooling Interventions per Dollar Transferred



(a) Attendance, percentage points per 2025 US dollar per month



(b) Enrollment, percentage points per 2025 US dollar per month

Notes: Each marker is one arm of a randomized evaluation of a program that reduces the cost of schooling in a low- or middle-income country. The ratio is the intention-to-treat effect divided by the program's average monthly transfer per child in 2025 US dollars, a monthly version of the transfer-effectiveness measure of García and Saavedra (2017), who divide by the annual transfer. "This study" spreads the one-time UGX 35,000 over the three-month term. Enrollment for this study is one minus dropout, the share of children enrolled at the end of the term. Our outcomes are measured after one term, whereas the comparison programs follow children for one to seven years. The ratios exclude administrative costs, which differ across delivery mechanisms, and mechanically favor smaller transfers when effects are concave in transfer size, so they are benchmarks for magnitude rather than estimates of cost-effectiveness. Underlying estimates are in Supplementary Appendix Table A9.

Table A9: Comparison Studies: Estimates Underlying Figure A8

Study	Country	Arm	Level	Follow-up (years)	Transfer ($/mo.)	Enrollment			Attendance		
						Effect (pp)	Control mean	pp per $-mo.	Effect (pp)	Control mean	pp per $-mo.
This study	Uganda	Voucher	Prim	0.25	3.33	2.7 (1.2)	95	0.81	6.8 (1.8)	70	2.04
This study	Uganda	Fee-Day Cash	Prim	0.25	3.33	1.5 (1.3)	95	0.45	5.2 (1.8)	70	1.56
This study	Uganda	Early Cash	Prim	0.25	3.33	1.1 (1.3)	95	0.33	4.2 (1.9)	70	1.26
Baird et al. (2011)	Malawi	CCT	Sec	2	14.95	11.3 (3.9)	70	0.76	16.9 (8.5)	69	1.13
Baird et al. (2011)	Malawi	UCT	Sec	2	14.95	3.3 (3.9)	70	0.22	11.8 (10.2)	69	0.79
Benhassine et al. (2015)	Morocco	Labeled cash	Prim	2	11.09	7.4 (1.6)	74	0.67	0.7 (1.0)	96	0.06
Benhassine et al. (2015)	Morocco	CCT	Prim	2	10.59	5.5 (1.2)	74	0.52	1.4 (0.7)	96	0.13
Duflo et al. (2015)	Kenya	Free uniforms	Prim	7	0.87	3.1 (1.2)	81	3.54	−0.2 (0.6)	94	−0.23
Barrera-Osorio et al. (2011)	Colombia	CCT (basic)	Sec	1	24.72	4.0 (1.1)	70	0.16	2.7 (0.7)	79	0.11
Schultz (2004)	Mexico	CCT (sec. entry)	Sec	3	44.34	11.1 (3.6)	58	0.25	–	–	–
Kremer et al. (2009)	Kenya	Merit scholarship	Prim	2	2.86	–	–	–	0.6 (1.5)	88	0.21
Edmonds and Schady (2012)	Ecuador	UCT (BDH)	Prim+Sec	2	26.24	6.2 (2.0)	49	0.24	–	–	–
Galiani and McEwan (2013)	Honduras	CCT (PRAF II)	Prim	2–3	14.02	10.1 (3.6)	65	0.72	–	–	–
De Walque and Valente (2023)	Mozambique	Parent cash CCT	Prim (upper)	1	3.74	2.7 (1.5)	95	0.73	6.0 (2.2)	65	1.60
De Walque and Valente (2023)	Mozambique	Girl voucher	Prim (upper)	1	3.74	−0.3 (1.5)	95	−0.09	8.3 (2.2)	65	2.22

Notes: This table reports the estimates in Supplementary Appendix Figure A8, one row per treatment arm: intention-to-treat effects in percentage points with standard errors in parentheses, control means in percent, and each effect divided by the program's average monthly transfer per child in 2025 US dollars at base-year market rates. For this study, UGX 35,000 is spread over the three-month term, enrollment is one minus dropout (Table 5, column 4), and attendance is the full-sample estimate (column 6). With the estimates among children who did not move (column 7), the attendance ratios are 1.47 for the Voucher, 0.96 for Fee-Day Cash, and 0.72 for Early Cash. For Kremer et al. (2009), the outcome is school participation in unannounced visits, which we treat as attendance, pooled over Busia and Teso. For Schultz (2004), the transfer is the junior-secondary first-year grant, and for Benhassine et al. (2015), the CCT standard error is that of the difference between the CCT and the labeled transfer. Some standard errors are reconstructed from published t-statistics or significance levels. Dashes indicate that the outcome is not reported in a comparable metric.

Table A10: Effects on School Examination Scores

	(1) Mid-Term Exam	(2) End-of-Term Exam
T1: Early Cash	−0.087	−0.058
	(0.078)	(0.062)
T2: Fee-Day Cash	−0.052	−0.032
	(0.079)	(0.060)
T3: Voucher	−0.078	−0.020
	(0.077)	(0.060)
Control Mean	0.000	0.000
Observations	869	1,570
p-value: T1 = T2	0.645	0.657
p-value: T2 = T3	0.727	0.837
p-value: T1 = T3	0.903	0.517

Notes: This table reports intention-to-treat estimates as in Table 5 for total marks on the school's mid-term and end-of-term examinations in Term 1, standardized to the control group. Children with unpaid fees can be barred from examinations, so treatment can change who is tested. Robust standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11: Effects on Household Schooling Spending and Siblings' Schooling

	Household	Siblings Aged 6–17		
	(1) Schooling Spending (1,000 UGX)	(2) Share Attending School	(3) Share Sent Home over Fees	(4) Share Receiving School Meals
T1: Early Cash	35.236**	−0.018	−0.017	0.049*
	(16.506)	(0.012)	(0.025)	(0.029)
T2: Fee-Day Cash	15.842	−0.014	−0.068***	0.051*
	(15.818)	(0.011)	(0.026)	(0.029)
T3: Voucher	38.095**	−0.018	−0.020	−0.012
	(16.251)	(0.011)	(0.024)	(0.029)
Control Mean	288.246	0.961	0.771	0.545
Observations	1,960	1,802	1,766	1,767
p-value: T1 = T2	0.230	0.754	0.056	0.948
p-value: T2 = T3	0.159	0.747	0.065	0.030
p-value: T1 = T3	0.863	1.000	0.907	0.033

Notes: This table reports intention-to-treat estimates as in Table 5, with one observation per household, from the endline parent survey. Column 1 is the household's spending on schooling for all children during Term 1, including fees, meals, materials, uniforms, and transport, plus UGX 35,000 for Voucher children who enrolled at the baseline school, since the Voucher was paid to the school and does not appear in parents' reports. Columns 2–4 cover the household's other children aged 6 to 17. Column 2 is the share attending school in 2026, among the 1,802 households with at least one such child. Columns 3 and 4 are the shares sent home over unpaid fees since January 2026 and receiving school meals, which were asked only about children attending school. The roster of other children was collected only at endline, so there is no baseline control. Robust standard errors are in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A12: Treatment Effects by Baseline Demand and Forecasts

	(1) Total Paid (1k UGX)	(2) Attendance (%)	(3) Dropped Out (%)
Panel A: Voucher vs. Fee-Day Cash			
Voucher × chose Voucher at equal value	−8.85***	−5.18	0.50
	(2.65)	(3.38)	(2.63)
within randomization stratum	−6.12**	−4.41	0.38
	(2.89)	(3.77)	(2.95)
Voucher × earmarking premium (10k UGX)	−2.71***	−1.94	−0.35
	(0.91)	(1.20)	(0.90)
Voucher × forecast gain from earmarking (10 pp)	−0.39	−0.38	0.18
	(0.54)	(0.62)	(0.44)
Fee-Day Cash mean	24.09	80.59	3.47
Observations	894	894	976
Panel B: Fee-Day Cash vs. Early Cash			
Fee-Day Cash × chose Fee-Day Cash at equal value	0.08	−4.27	−1.29
	(3.06)	(3.22)	(2.74)
Fee-Day Cash × delay premium (10k UGX)	0.55	−0.96	−1.16
	(1.16)	(1.20)	(1.06)
Fee-Day Cash × forecast gain from delay (10 pp)	−0.45	−0.20	−0.14
	(0.56)	(0.56)	(0.47)
Early Cash mean	24.09	79.51	3.89
Observations	895	895	979

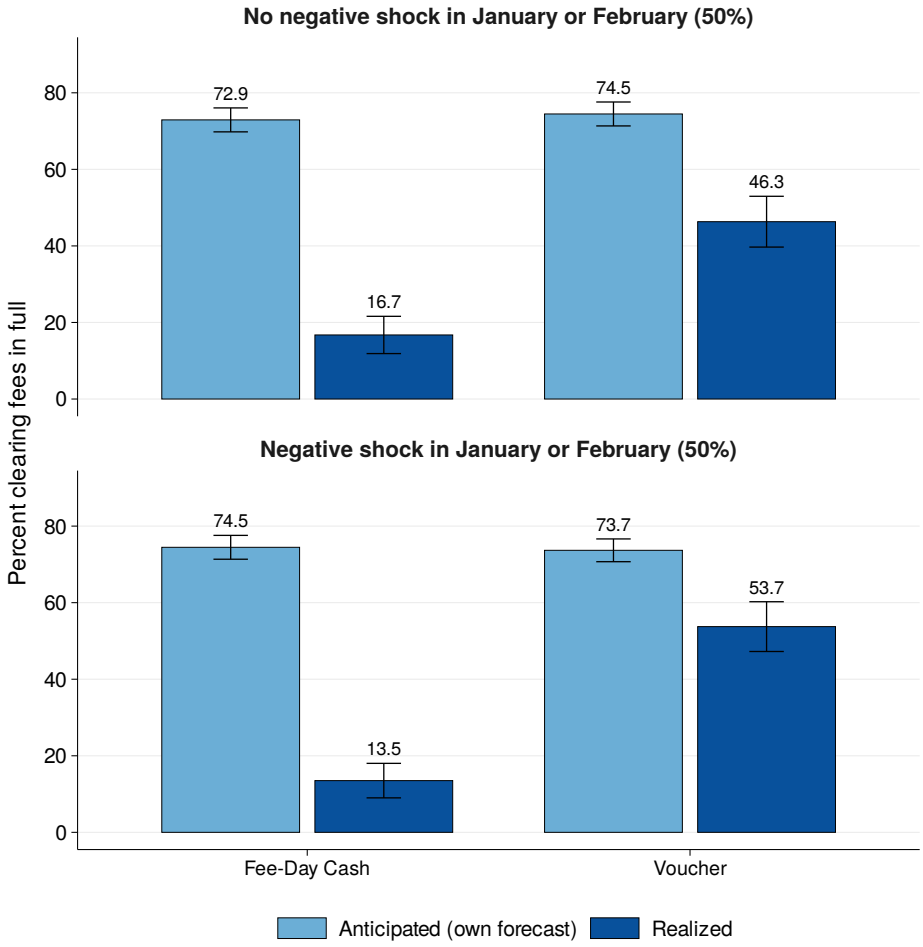
Notes: Each pair of rows reports the interaction coefficient from a separate regression of the outcome on a treatment indicator, the moderator, their interaction, and randomization-strata fixed effects, estimated on children assigned to the Voucher or Fee-Day Cash (Panel A) or to Fee-Day Cash or Early Cash (Panel B). "Chose … at equal value" indicates a positive premium. "Within randomization stratum" replaces the interaction with the interaction of the treatment indicator and the moderator's deviation from its stratum mean, controlling for the interaction with the stratum mean. Premia are in UGX 10,000. Forecast gains are within-parent differences in the baseline forecast of fully clearing Term 1 fees, Voucher minus Fee-Day Cash in Panel A and Fee-Day Cash minus Early Cash in Panel B, in 10 percentage points. Columns 1 and 2 exclude children who transferred schools. Total paid is from school records, coded as zero for children with no recorded payment. Attendance is the share of enumerator visits at which the child was present, with children who dropped out coded as absent. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A13: Schooling Outcomes under Alternative Assignment Rules

	(1) Total Paid (1k UGX)	(2) Attendance (%)	(3) Dropped Out (%)
No transfer	19.53	77.04	5.11
	(1.02)	(1.18)	(0.98)
Panel A: Every parent receives a transfer of UGX 35,000			
Fee-Day Cash for all	24.48	80.56	3.41
	(1.00)	(1.01)	(0.82)
Voucher for all	42.36	82.10	2.30
	(0.73)	(0.93)	(0.67)
Each parent receives the transfer chosen at baseline	35.33	80.33	2.83
	(0.77)	(0.98)	(0.71)
Difference: choice minus Voucher for all	-7.03^{***}	-1.78^{**}	0.52
	(0.61)	(0.84)	(0.72)
Panel B: Voucher for the share of parents who chose it, nothing for the rest			
Voucher to parents who chose it	35.37	80.20	3.79
	(0.83)	(1.02)	(0.83)
Voucher to a random sample of the same size	35.54	80.59	3.17
	(0.59)	(0.74)	(0.55)
Difference: chosen minus random	−0.16	−0.39	0.62
	(0.58)	(0.73)	(0.64)
Share choosing the Voucher	0.70	0.70	0.69
Observations	1,776	1,776	1,960

Notes: Each entry is the average outcome in the study population if transfers were assigned by the rule in the row. Entries combine strata-adjusted means from a regression of the outcome on indicators for each combination of baseline choice (Voucher or Fee-Day Cash at equal value) and assigned arm, with randomization-strata fixed effects, weighted by the share of parents in each group. "Each parent receives the transfer chosen at baseline" assigns the Voucher to parents who chose it and Fee-Day Cash to the others. Panel B holds fixed the number of Vouchers. Standard errors in parentheses are computed by the delta method from heteroskedasticity-robust standard errors and treat the group shares as fixed. Samples and outcomes are as in Supplementary Appendix Table A12, including the Control and Early Cash arms. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$ for the differences.

Figure A9: Anticipated and Realized Full Clearing, by Fee-Window Shock



Notes: Bars use the 894 children assigned to Fee-Day Cash or the Voucher in the sample of Figure 6. A household reports a fee-window shock if, at endline, it reports any of nine negative shocks (job loss, illness or medical expense, death or funeral, theft, crop failure, livestock illness, home damage, demands for money, or other) in January or February 2026. The flag is balanced across the four arms (joint $p = 0.65$). Light bars are average baseline forecasts of fully clearing Term 1 fees under the assigned transfer, and dark bars are realized full clearing, with 95% confidence intervals. In a regression of full clearing on the three treatment indicators, the shock indicator, their interactions, and randomization-strata fixed effects, estimated on all 1,776 children in the sample of Figure 6, the Voucher raises full clearing relative to Fee-Day Cash by 29.7 percentage points (s.e. 3.3) without a shock and 39.2 (3.7) with one; the difference is 9.5 (5.0, $p = 0.06$).

Table A14: Effects of Experience on Voucher Demand

	(1) Chose Voucher	(2) Indifference Point (Tobit)	(3) WTP at Ceiling	(4) WTP at Floor
T1: Early Cash	-0.088***	-5484***	-0.085***	0.035
	(0.030)	(1973)	(0.031)	(0.023)
T2: Fee-Day Cash	-0.097***	-4836**	-0.074**	0.028
	(0.030)	(1967)	(0.030)	(0.023)
T3: Voucher	0.121***	10778***	0.134***	-0.053**
	(0.027)	(2096)	(0.031)	(0.021)
Control Mean	.689	40823	.463	.145
Marg. Effect: T1 (UGX)		-2143		
Marg. Effect: T2 (UGX)		-1890		
Marg. Effect: T3 (UGX)		4211		
T1 Effect (%)	-12.8	-13.4	-18.5	24.3
T2 Effect (%)	-14.1	-11.8	-16.0	19.0
T3 Effect (%)	17.6	26.4	28.9	-36.6
p-value: T1 = T2	0.770	0.737	0.708	0.749
p-value: T2 = T3	0.000	0.000	0.000	0.000
p-value: T1 = T3	0.000	0.000	0.000	0.000
Observations	1,960	1,960	1,960	1,960

Notes: The sample excludes the Choice group ($N = 1{,}960$). Column 1 is a linear probability model of choosing the UGX 35,000 voucher over equal Fee-Day Cash in the first round of the endline task. Column 2 is a two-limit Tobit of the endline indifference point, censored at UGX 17,500 and 52,500; the Marg. Effect rows give the implied effect on the observed indifference point. Columns 3–4 are linear probability models of the endline indifference point lying at the ceiling and floor of the grid. Each column controls for its baseline analog (ANCOVA) and strata fixed effects; ITT estimates without baseline controls are similar (Supplementary Appendix Table A15), since the control-group baseline–endline correlation of the indifference point is 0.127. T Effect (%) divides each coefficient by the control mean. Robust standard errors in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A15: Preference Updating: Alternative Estimators and Samples

	(1) Endline WTP OLS, ITT	(2) Endline WTP OLS, ANCOVA	(3) Δ WTP OLS, ITT	(4) Endline WTP Tobit, Same Parent	(5) Endline WTP Tobit, Term 1
T1: Early Cash	-2163**	-2229***	-2540**	-6278***	-3718
	(868)	(858)	(1093)	(2029)	(3460)
T2: Fee-Day Cash	-2234**	-2128**	-1628	-5723***	-5694*
	(869)	(856)	(1082)	(2012)	(3341)
T3: Voucher	4105***	4019***	3613***	10033***	9277**
	(817)	(810)	(1062)	(2155)	(3739)
Control Mean	40823	40823	-83	41223	41178
p-value: T1 = T2	0.935	0.906	0.393	0.776	0.562
p-value: T2 = T3	0.000	0.000	0.000	0.000	0.000
p-value: T1 = T3	0.000	0.000	0.000	0.000	0.001
Observations	1,960	1,960	1,960	1,855	584

Notes: The dependent variable is the endline indifference point (columns 1–2, 4–5) or its change from baseline (column 3). Columns 1–3 are OLS with strata fixed effects; columns 4–5 are two-limit Tobits restricted to same-respondent households ($N = 1{,}855$) and to the Term 1 2027 version of the task ($N = 584$). With a control-group baseline–endline correlation of 0.127, ANCOVA is more efficient than the change score. Robust standard errors in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A16: Endline Demand and Realized Outcomes

	(1) Cleared Fees in Full	(2) Share of Fees Paid (10 pp)	(3) Attendance (10 pp)
Slope of the endline earmarking premium on the realized outcome, by arm			
Control	−2,543	37	103
	(2,164)	(183)	(274)
Early Cash	−1,898	−128	451
	(1,811)	(164)	(307)
Fee-Day Cash	673	53	731**
	(1,766)	(166)	(317)
Voucher	370	183	754**
	(1,250)	(316)	(304)
p-value: treated-arm slopes equal Control slope	0.456	0.765	0.331
Control mean of endline premium (UGX)	6,654	6,654	6,654
Observations	1,776	1,776	1,776

Notes: Each column reports one regression of the endline earmarking premium (UGX) on the realized outcome interacted with arm indicators, arm indicators, the baseline premium, and randomization-strata fixed effects. Rows report the slope on the realized outcome within each arm. The sample excludes the Choice group and children who transferred schools. Cleared fees in full is an indicator; the share of fees paid and attendance are in 10 percentage points. The p-value tests that the three treated-arm slopes equal the Control slope. Robust standard errors in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

B Model Derivations

This appendix collects the formal results behind Section 3: the decision rules (Lemmas B.1 and B.2), the derivation of the value functions and of the willingness-to-pay expressions in equation (2), the proofs of Propositions 1 and 2, and four extensions. Sections B.1 to B.3 maintain the assumptions of Section 3.1: $R > F > 0$, $s \geq 0$, $C = F$, $\beta, \tilde{\beta} \in (0, 1]$, and $y_L < F \leq y_H$. Decision rules are stated for the forecast parameter $\tilde{\beta}$, which the $t = 0$ self uses to predict their later selves; realized behavior obtains on replacing $\tilde{\beta}$ with the true β in each self's own trade-off (see the proof of Proposition 2).

B.1 Decision rules

Lemma B.1 (Fee-day payment)**.** *Under either cash arm, the fee-day self holding cash ℓ pays ($a = 1$) if and only if $\ell \geq F$ and $\tilde{\beta}R \geq F$. Under the Voucher, the fee is paid directly to the school regardless of ℓ and $\tilde{\beta}$.*

Proof. Paying yields $(\ell - F) + \tilde{\beta}R$ and not paying yields ℓ, so payment is preferred if and only if $\tilde{\beta}R \geq F$, and it is feasible if and only if $\ell \geq F$ given the no-borrowing constraint. Under the Voucher the program pays the school directly upon enrollment, so no fee-day choice remains. □

Lemma B.2 (Saving)**.** *Under Early Cash, optimal saving is a corner solution, $s^* \in \{0, F - y_L\}$. If $\tilde{\beta}R \geq F$, then $s^* = F - y_L$ if and only if $\tilde{\beta} \geq \bar{\beta}_S$, and $s^* = 0$ otherwise; if $\tilde{\beta}R < F$, then $s^* = 0$. Under Fee-Day Cash and the Voucher, $s^* = 0$.*

Proof. The period-1 self maximizes $U_1 = c_1 + \tilde{\beta}\,\mathbb{E}[c_2 + c_3]$. Moving one shilling from $t = 1$ to $t = 2$ costs one unit of current consumption and returns $\tilde{\beta}$ in expectation, a net loss of $1 - \tilde{\beta} \geq 0$, unless the shilling changes the fee-day payment set. Under Fee-Day Cash the low-state budget is $y_L + F \geq F$ and under the Voucher the fee is paid directly to the school, so saving never changes payment and $s^* = 0$. Under Early Cash, cash on hand on fee day is $\ell = y_2 + s$, so saving changes payment only in the low state and only if $s \geq F - y_L$; the only candidate other than $s = 0$ is therefore $s = F - y_L$, since any smaller s is a pure loss and any larger s adds one. Suppose $\tilde{\beta}R \geq F$, so the fee-day self pays whenever cash on hand covers the fee (Lemma B.1). Comparing the two candidates,

$$U_1(F - y_L) - U_1(0) = -(F - y_L) + \tilde{\beta}\big[(F - y_L) + q(R - F)\big],$$

which is nonnegative if and only if $\tilde{\beta} \geq \bar{\beta}_S \equiv (F - y_L)/[(F - y_L) + q(R - F)]$, equivalently $\tilde{\beta}q(R - F) \geq (1 - \tilde{\beta})(F - y_L)$. If instead $\tilde{\beta}R < F$, the fee-day self never pays, saving cannot

change payment, and $s^* = 0$. □

Lemmas B.1 and B.2 together deliver the payment probabilities in equation (1): under the Voucher the fee is always paid; under Fee-Day Cash it is paid if and only if the payment condition holds, since the fee-day budget always covers F; and under Early Cash it is paid if and only if the payment condition holds and either the high state realizes or the period-1 self saved.

B.2 Value functions and willingness to pay

Under a cash arm $d \in \{1, 2\}$, realized total consumption equals total resources plus the net return when the fee is paid, $c_1 + c_2 + c_3 = y_1 + C + y_2 + (R - F)\, a$. Saving only reallocates consumption across dates, which the $t = 0$ self values equally, so

$$V_0^{(d)} = \beta\big[y_1 + \mathbb{E}y_2 + C + (R - F)\,\tilde{\mu}_d\big], \qquad d \in \{1, 2\}. \tag{B.1}$$

Under the Voucher, the parent's own resources are untouched, the fee is paid directly to the school, and the return accrues with forecast probability $\tilde{\mu}_V$, so $V_0^{(V)} = \beta\big[y_1 + \mathbb{E}y_2 + \tilde{\mu}_V R\big]$. Using $C = F$, this can be written as $V_0^{(V)} = \beta\big[y_1 + \mathbb{E}y_2 + C + (R - F)\tilde{\mu}_V - F(1 - \tilde{\mu}_V)\big]$: relative to a cash arm, an unused Voucher forfeits the transfer, whereas unspent cash is kept.

The willingness to pay for transfer d over d' is the amount of fee-day cash x that makes the $t = 0$ self indifferent, $V_0^{(d)} = V_0^{(d')} + \beta x$, that is, $x = (V_0^{(d)} - V_0^{(d')})/\beta$. Applying this to the two elicitation tasks gives equation (2):

$$\begin{aligned}
\mathrm{WTP}_{\mathrm{voucher}} &= \frac{V_0^{(V)} - V_0^{(2)}}{\beta} = (R - F)(\tilde{\mu}_V - \tilde{\mu}_2) - F(1 - \tilde{\mu}_V), \\
\mathrm{WTP}_{\mathrm{delay}} &= \frac{V_0^{(2)} - V_0^{(1)}}{\beta} = (R - F)(\tilde{\mu}_2 - \tilde{\mu}_1),
\end{aligned}$$

where the forfeiture term in the earmarking premium vanishes because $\tilde{\mu}_V \equiv 1$. After dividing by β, willingness to pay depends on preferences only through the forecast payment probabilities.

B.3 Proofs of the propositions and comparative statics

Proof of Proposition 1. Substitute the payment probabilities (1) into (2) region by region. If $\tilde{\beta}R < F$, then $\tilde{\mu}_1 = \tilde{\mu}_2 = 0$, so $\mathrm{WTP}_{\mathrm{voucher}} = R - F > 0$ and $\mathrm{WTP}_{\mathrm{delay}} = 0$. If $F/R \le \tilde{\beta} < \bar{\beta}_S$, then $\tilde{\mu}_2 = 1$ and $\tilde{\mu}_1 = 1 - q$, so $\mathrm{WTP}_{\mathrm{voucher}} = 0$ and $\mathrm{WTP}_{\mathrm{delay}} = q(R - F) > 0$. If $\tilde{\beta} \ge \bar{\beta}_S$,

then $\tilde{\mu}_1 = \tilde{\mu}_2 = 1$ and both premia are zero. The supports are disjoint by inspection. The three regions partition $(0, 1]$ in this order provided $F/R < \bar{\beta}_S$, which holds if and only if

$$\frac{F}{R} < \frac{F - y_L}{(F - y_L) + q(R - F)} \iff qF(R - F) < (R - F)(F - y_L) \iff y_L < F(1 - q).$$

Finally, both supports lie strictly below one, since $\tilde{\beta}R < F$ implies $\tilde{\beta} < F/R < 1$ and the under-saving region requires $\tilde{\beta} < \bar{\beta}_S \leq 1$; positive willingness to pay for either restriction therefore implies $\tilde{\beta} < 1$. □

Proof of Proposition 2. In realized behavior each self applies the true β to its own trade-off and uses $\tilde{\beta}$ to forecast later selves. The fee-day self pays if and only if cash on hand covers the fee and $\beta R \geq F$, so $\mu_2 = \mathbf{1}\{\beta R \geq F\}$. Under Early Cash the period-1 self saves $F - y_L$ if and only if $\beta \geq \bar{\beta}_S$ and $\tilde{\beta}R \geq F$, so $\mu_1 = \mathbf{1}\{\beta R \geq F\}\big(1 - q\,\mathbf{1}\{\beta < \bar{\beta}_S \text{ or } \tilde{\beta}R < F\}\big)$. Since $\mu_V = \tilde{\mu}_V = 1$, $e_d = \mu_d - \tilde{\mu}_d$. Let $m_d(b)$ denote the forecast probability in equation (1) evaluated at b, so that $\tilde{\mu}_d = m_d(\tilde{\beta})$; both m_1 and m_2 are weakly increasing step functions. If $\beta \leq \tilde{\beta}$, then $\tilde{\beta}R < F$ implies $\beta R < F$, so $\mu_d = m_d(\beta)$ and $e_d = m_d(\beta) - m_d(\tilde{\beta}) \leq 0$. If $\tilde{\beta} < \beta$, then $\mu_2 = m_2(\beta) \geq m_2(\tilde{\beta})$, and $\mu_1 \geq m_1(\tilde{\beta})$: the forecast is zero when $\tilde{\beta}R < F$, and $\mu_1 = m_1(\beta) \geq m_1(\tilde{\beta})$ when $\tilde{\beta}R \geq F$. Hence $e_d \geq 0$. It follows that $e_d = 0$ when $\tilde{\beta} = \beta$, that $e_d < 0$ only if $\beta < \tilde{\beta}$, and that $e_d > 0$ only if $\tilde{\beta} < \beta$. For timing, $m_2(b) - m_1(b) = q\,\mathbf{1}\{F/R \leq b < \bar{\beta}_S\}$ is not monotone in b. With $\beta < \tilde{\beta}$, the timing error equals $-q$ when $F/R \leq \beta < \bar{\beta}_S \leq \tilde{\beta}$ and q when $\beta < F/R \leq \tilde{\beta} < \bar{\beta}_S$; with $\tilde{\beta} < \beta$, it equals $-q$ when $\tilde{\beta} < F/R \leq \beta < \bar{\beta}_S$ and q when $F/R \leq \tilde{\beta} < \bar{\beta}_S \leq \beta$. □

Demand and effects. The realized effect of earmarking on payment is $\mu_V - \mu_2 = \mathbf{1}\{\beta R < F\}$, and by Proposition 1 a parent pays for earmarking if and only if $\tilde{\beta}R < F$. If $\beta \leq \tilde{\beta}$ for every parent, then $\tilde{\beta}R < F$ implies $\beta R < F$, so the effect equals one for every parent who pays and is at most one for the others. The effect among parents who pay can be smaller than among those who do not only if $\tilde{\beta}R < F \leq \beta R$ for some parents who pay. For delay, the realized effect is $\mu_2 - \mu_1 = q\,\mathbf{1}\{\beta R \geq F\}\,\mathbf{1}\{\beta < \bar{\beta}_S \text{ or } \tilde{\beta}R < F\}$, which is weakly positive, so realized payment probabilities are ordered $\mu_V \geq \mu_2 \geq \mu_1$ for every parent.

The comparative statics used in the text follow directly. On its support, $\text{WTP}_{\text{delay}} = q(R - F)$ is strictly increasing in q. The saving cutoff $\bar{\beta}_S$ is increasing in $F - y_L$ and decreasing in q, so a lower low-state income widens the region in which delay is demanded, while a more likely shortfall raises the premium conditional on demanding but narrows the region itself. The earmarking premium involves neither q nor y_L on its support. It rises with the return R on its support, while the support itself, $\tilde{\beta} < F/R$, shrinks as R rises, so the relationship between the return and average demand for earmarking is ambiguous.

Supplementary Appendix Figure A3 displays the partition of the forecast present-focus parameter.

B.4 Long-run discounting

The main text sets $\delta = 1$, following the convention of O'Donoghue and Rabin (1999, 2001) for short-horizon problems. This subsection restores $\delta < 1$ and shows that the characterization survives, with one substitution: the maintained assumption $R > F$ becomes $\delta R > F$, so that paying the fee is worthwhile from the ex ante perspective.

Preferences. Each self now discounts exponentially as well: $U_2 = c_2 + \beta\delta c_3$, $U_1 = c_1 + \beta\delta\,\mathbb{E}c_2 + \beta\delta^2\,\mathbb{E}c_3$, and $V_0^{(d)} = \beta\big[\delta\,\mathbb{E}c_1 + \delta^2\,\mathbb{E}c_2 + \delta^3\,\mathbb{E}c_3\big]$. Beliefs concern β only; δ is known and common to forecast and behavior.

Decision rules. The fee-day self pays if and only if $\ell \geq F$ and $\tilde{\beta}\delta R \geq F$, so the payment threshold rises from F/R to $F/(\delta R)$. The saving problem remains a corner (the argument of Lemma B.2 is unchanged), and the cutoff generalizes to

$$\bar{\beta}_S(\delta) \equiv \frac{F - y_L}{\delta\big[(F - y_L) + q(\delta R - F)\big]},$$

which collapses to $\bar{\beta}_S$ at $\delta = 1$. Because $\delta < 1$ both shrinks the forecast benefit of saving and adds an exponential cost to postponing consumption, $\bar{\beta}_S(\delta) > \bar{\beta}_S$, and $\bar{\beta}_S(\delta)$ can exceed one, in which case no parent saves.

Willingness to pay. A fee-day shilling is worth $\beta\delta^2$ to the $t = 0$ self, so premia are $x = (V_0^{(d)} - V_0^{(d')})/(\beta\delta^2)$. Differencing the value functions region by region gives, for $\tilde{\beta} < F/(\delta R)$,

$$\mathrm{WTP}_{\text{voucher}} = \delta R - F > 0, \qquad \mathrm{WTP}_{\text{delay}} = -\frac{F(1-\delta)}{\delta} < 0;$$

for $F/(\delta R) \leq \tilde{\beta} < \bar{\beta}_S(\delta)$,

$$\mathrm{WTP}_{\text{voucher}} = 0, \qquad \mathrm{WTP}_{\text{delay}} = q\,(\delta R - F) - \frac{F(1-\delta)}{\delta};$$

and for $\tilde{\beta} \geq \bar{\beta}_S(\delta)$,

$$\mathrm{WTP}_{\text{voucher}} = 0, \qquad \mathrm{WTP}_{\text{delay}} = -\frac{y_L(1-\delta)}{\delta} \leq 0.$$

The structure of Proposition 1 is intact: earmarking is valued exactly in the under-payment region, and delay is most valuable in the under-saving region. Each delay premium now nets the commitment benefit against the pure cost of waiting, $F(1-\delta)/\delta$ on the amount held

back (or $y_L(1-\delta)/\delta$ on the saver's own set-aside). In the middle region the delay premium is positive if and only if $q(\delta R - F) > F(1-\delta)/\delta$, that is, if the risk of a shortfall outweighs the cost of waiting, which holds automatically at $\delta = 1$. The middle region is non-empty, $F/(\delta R) < \bar{\beta}_S(\delta)$, if and only if

$$y_L \;<\; F - \frac{qF(\delta R - F)}{R - F},$$

which nests $y_L < F(1-q)$ at $\delta = 1$ and is weaker for $\delta < 1$.

Remark 1. Positive willingness to pay for either restriction still requires $\tilde{\beta} < 1$. The earmarking premium is positive only for $\tilde{\beta} < F/(\delta R) < 1$. For delay, if $\bar{\beta}_S(\delta) \leq 1$ a parent with $\tilde{\beta} = 1$ saves on their own and $\text{WTP}_{\text{delay}} = -y_L(1-\delta)/\delta \leq 0$; if $\bar{\beta}_S(\delta) > 1$ they do not save, but $\bar{\beta}_S(\delta) > 1$ is equivalent to $q(\delta R - F) < (1-\delta)(F - y_L)/\delta \leq F(1-\delta)/\delta$, so their delay premium is again negative. Exponential discounting alone therefore cannot generate demand for either restriction.

The extension also rationalizes a feature of the data that the $\delta = 1$ model cannot: with $\delta = 1$ every premium is zero or positive, whereas with $\delta < 1$ parents who expect to save and pay on their own strictly prefer earlier cash. The negative delay premia in Section 4.1, revealed by 38 percent of parents, are consistent with this region, without changing where demand for commitment comes from. Negative earmarking premia do not arise here; Section B.7 shows how they arise when the Voucher covers only part of the fee.

B.5 Costly borrowing

The main text rules out borrowing. This subsection lets a parent cover a fee-day shortfall by borrowing at a gross interest rate $\rho > 1$. The earmarking premium is unchanged. The delay premium remains positive only for $\tilde{\beta} < 1$, equals the expected interest for parents who are willing to borrow, and converges to zero as $\rho \to 1$.

Borrowing. Borrowing is possible only on fee day. A parent who holds cash $\ell < F$ can borrow the shortfall $F - \ell$ and pay the fee. The loan is repaid with interest before the schooling return accrues, so its cost falls on the fee-day self: paying with a shortfall yields $-\rho(F - \ell) + \tilde{\beta}R$, against ℓ from not paying.[48] Let $\kappa \equiv (\rho - 1)(F - y_L)$ denote the interest on the full low-state shortfall.

[48] With linear utility only differences in consumption matter, so consumption may be negative. No self borrows to consume, because $\rho > 1$. If repayment instead fell due when the return accrues, the fee-day self would discount the cost of the loan along with the return, and borrowing would itself relax the payment condition for a parent with little cash on hand.

Decision rules. A fee-day self with $\ell \geq F$ pays if and only if $\tilde{\beta}R \geq F$, as before. A fee-day self with $\ell < F$ borrows and pays if and only if $\tilde{\beta}R \geq \ell + \rho(F - \ell)$, that is, if and only if

$$\ell \;\geq\; \ell^*(\tilde{\beta}) \equiv \frac{\rho F - \tilde{\beta}R}{\rho - 1}.$$

The threshold satisfies $\ell^* \leq F$ if and only if $\tilde{\beta}R \geq F$, and $\ell^* \leq y_L$ if and only if $\tilde{\beta}R \geq F + \kappa$. Under Fee-Day Cash and the Voucher cash on hand covers the fee, so borrowing is never used and payment is as in equation (1). Under Early Cash a parent with $\tilde{\beta}R \geq F$ chooses among three levels of saving: nothing; $\max\{\ell^* - y_L, 0\}$, which leaves a low-state shortfall that the fee-day self is just willing to borrow; and $F - y_L$, which avoids borrowing. Each shilling saved beyond $\ell^* - y_L$ replaces a shilling borrowed in the low state and is worth $\tilde{\beta}[1 + q(\rho - 1)]$ to the period-1 self, so saving the full shortfall is preferred if and only if

$$\tilde{\beta} \;\geq\; \bar{\beta}_B \equiv \frac{1}{1 + q(\rho - 1)}.$$

When $\ell^* > y_L$, saving $\ell^* - y_L$ rather than nothing changes period-1 utility by $(1 - \tilde{\beta})\big[\tilde{\beta}qR - (\ell^* - y_L)\big]$, which is nonnegative if and only if

$$\tilde{\beta} \;\geq\; \bar{\beta}_P \equiv \bar{\beta}_B \, \frac{F + \kappa}{R}.$$

Willingness to pay. Premia are differences in $t = 0$ values, as in Section B.2. The earmarking premium compares the Voucher with Fee-Day Cash, neither of which involves borrowing, so $\text{WTP}_{\text{voucher}} = (R - F)\,\mathbf{1}\{\tilde{\beta}R < F\}$, as in Proposition 1. If $\kappa \geq R - F$, the interest on the shortfall exceeds the net return, no parent borrows, and the delay premium is also as in Proposition 1. If $\kappa < R - F$,

$$\text{WTP}_{\text{delay}} = \begin{cases} q\,(R - F) & \text{if } F/R \leq \tilde{\beta} < \bar{\beta}_P, \\ q\,\min\{\tilde{\beta}R - F,\ \kappa\} & \text{if } \max\{F/R, \bar{\beta}_P\} \leq \tilde{\beta} < \bar{\beta}_B, \\ 0 & \text{otherwise.} \end{cases}$$

In the first region the parent neither saves nor borrows, and the fee goes unpaid in the low state, as in the main text. In the second the fee is paid in both states and the premium is the expected interest: $q\kappa$ if the parent borrows the full shortfall, and $q(\tilde{\beta}R - F)$ if they save part of it and borrow the rest. The first region is non-empty if and only if $y_L < F(1 - q)$, the condition in Proposition 1.

Remark 2. Both premia are zero at $\tilde{\beta} = 1$, because $\bar{\beta}_B < 1$ whenever $\rho > 1$. As $\rho \to 1$,

$\kappa \to 0$, $\bar{\beta}_P \to F/R$, and $\bar{\beta}_B \to 1$, so the delay premium converges to zero for every $\tilde{\beta}$: when credit is free, the timing of the transfer does not matter. A positive delay premium therefore requires both $\tilde{\beta} < 1$ and costly credit, whereas a positive earmarking premium requires only $\tilde{\beta} < 1$.

B.6 Concave utility

The main text assumes linear utility. This subsection shows that the central implication of Proposition 1, that a positive premium for either restriction requires $\tilde{\beta} < 1$, holds for any increasing utility function, and describes which results are specific to the linear case.

Preferences. Each self has per-period utility $u(c)$, with u strictly increasing, and discounts as in Section B.4: $U_2 = u(c_2) + \beta\delta\, u(c_3)$, $U_1 = u(c_1) + \beta\delta\, \mathbb{E}u(c_2) + \beta\delta^2\, \mathbb{E}u(c_3)$, and $V_0^{(d)} = \beta\big[\delta\, \mathbb{E}u(c_1) + \delta^2\, \mathbb{E}u(c_2) + \delta^3\, \mathbb{E}u(c_3)\big]$, where $c_3 = y_3 + aR$ and y_3 is income at $t = 3$. Fee-day income y_2 has an arbitrary distribution. Without a linear numeraire we define the premia as in the experiment. Let $V_0^{(2)}(X)$ denote the $t = 0$ value of Fee-Day Cash of amount X, and let X_E and X_D solve $V_0^{(2)}(X_E) = V_0^{(V)}$ and $V_0^{(2)}(X_D) = V_0^{(1)}$, where the Voucher and Early Cash are of amount C. Then $\text{WTP}_{\text{voucher}} \equiv X_E - C$ and $\text{WTP}_{\text{delay}} \equiv C - X_D$.

Proposition 3 (No demand without perceived present focus)**.** *Suppose $\tilde{\beta} = 1$. For any strictly increasing u, any $\delta \in (0, 1]$, and any distribution of y_2,* $\text{WTP}_{\text{voucher}} \leq 0$ *and* $\text{WTP}_{\text{delay}} \leq 0$.

Proof. When $\tilde{\beta} = 1$ the $t = 0$ self expects each later self t to maximize $\sum_{\tau \geq t} \delta^{\tau - t} u(c_\tau)$, which ranks continuation plans as V_0 does. The outcome the parent forecasts under a transfer therefore maximizes V_0 over the plans that are feasible under that transfer. Every plan that is feasible under Fee-Day Cash of amount X is feasible under Early Cash of the same amount, by saving the transfer until fee day. Every plan that is feasible under the Voucher is feasible under Fee-Day Cash of the same amount, by paying it to the school, because the two carry the same transaction costs (Section 2.4). Hence $V_0^{(1)} \geq V_0^{(2)}(C) \geq V_0^{(V)}$. Because u is strictly increasing, $V_0^{(2)}(X)$ is strictly increasing in X when $\tilde{\beta} = 1$, so $X_E \leq C \leq X_D$. □

The proposition does not use concavity, the absence of borrowing, or the two-point distribution of income. It uses only that cash can be saved at no cost and that paying the school in cash costs the same as the Voucher. A positive premium therefore implies either $\tilde{\beta} < 1$ or a failure of one of these two conditions, for example cash that cannot be held safely or that others can claim. Section 4.3 examines these alternatives.

What is specific to linear utility. When u is strictly concave, a fee-day self holding cash

$\ell \geq F$ pays if and only if

$$u(\ell) - u(\ell - F) \;\leq\; \tilde{\beta}\,\delta\,\big[u(y_3 + R) - u(y_3)\big].$$

The left-hand side falls with ℓ, so the payment condition is easier to meet when cash on hand is higher, and a parent may pay in the high state and not in the low state. Saving is interior. The closed forms of Proposition 1, and the result that no parent pays for both restrictions, then no longer hold exactly. A parent who expects to pay only in the high state under Fee-Day Cash values the Voucher, under which the fee is also paid in the low state, and can value delay at the same time. The delay premium also no longer depends on fee payment alone: delay limits consumption at $t = 1$, which raises the premium when $\tilde{\beta} < 1$, and it prevents the parent from smoothing consumption when y_1 is low, which lowers it. The earmarking premium can be negative, because in a state with little cash on hand even the $t = 0$ self may prefer cash to a paid fee. This is the cost of lost flexibility that limits demand for commitment under uncertainty (Amador et al., 2006; Laibson, 2015; Carrera et al., 2022). For example, with $u = \log$, $\delta = 1$, $F = C = 1$, $R = 10$, $q = 0.4$, $y_H = 2$, $y_L = 0.5$, $y_1 = 1.5$, and $y_3 = 1$, a parent with $\tilde{\beta} = 0.3$ has an earmarking premium of 0.12 and a delay premium of 0.48, and a parent with $\tilde{\beta} = 1$ has neither.

B.7 A Voucher that covers part of the fee

The main text assumes that the transfer covers the fee. In the experiment it covered 85 percent of the term fee on average, and the full fee for 37 percent of children. This subsection sets $C < F$. The premia in equation (2) carry over with a forfeiture term, the earmarking premium is positive on a narrower range of $\tilde{\beta}$, and the delay premium is unchanged.

Setup. All transfers are of amount $C < F$. Under the Voucher, C is paid to the school and the parent owes the balance $F - C$. The return R accrues only if the full fee is paid; if the balance goes unpaid, the amount paid by the Voucher stays with the school and yields no return. We assume $y_L \geq F - C$, so that a transfer received on fee day covers the low-state shortfall.

Decision rules. Under the Voucher the fee-day self pays the balance if and only if $\tilde{\beta}R \geq F - C$, since the parent's own cash always covers it. Under Fee-Day Cash cash on hand is at least $y_L + C \geq F$, and the fee-day self pays if and only if $\tilde{\beta}R \geq F$. Under Early Cash the period-1 self must save $F - y_L$ for the fee to be paid in the low state, and Lemma B.2 applies unchanged. Forecast payment is therefore

$$\tilde{\mu}_V = \mathbf{1}\{\tilde{\beta}R \geq F - C\},$$

with $\tilde{\mu}_1$ and $\tilde{\mu}_2$ as in equation (1).

Willingness to pay. Under a cash transfer, $V_0^{(d)} = \beta\big[y_1 + \mathbb{E}y_2 + C + (R-F)\tilde{\mu}_d\big]$, as in equation (B.1). Under the Voucher the parent pays $F - C$ and receives R when the fee is paid in full, and receives nothing otherwise, so $V_0^{(V)} = \beta\big[y_1 + \mathbb{E}y_2 + (C + R - F)\tilde{\mu}_V\big]$. Differencing,

$$\text{WTP}_{\text{voucher}} = (R-F)(\tilde{\mu}_V - \tilde{\mu}_2) - C\,(1 - \tilde{\mu}_V) = \begin{cases} -C & \text{if } \tilde{\beta} < (F-C)/R, \\ R - F & \text{if } (F-C)/R \le \tilde{\beta} < F/R, \\ 0 & \text{if } \tilde{\beta} \ge F/R, \end{cases}$$

and $\text{WTP}_{\text{delay}} = (R-F)(\tilde{\mu}_2 - \tilde{\mu}_1)$ is as in Proposition 1.

The earmarking premium is still positive where the fee-day self would not pay the fee out of cash, provided that self would pay the balance. A partial Voucher lowers the out-of-pocket cost of paying the fee from F to $F - C$, so it changes behavior for parents with $(F-C)/R \le \tilde{\beta} < F/R$. Parents below this range expect to leave the balance unpaid and the Voucher's payment to be forfeited, and they value the Voucher at C less than cash. The negative earmarking premia in Section 4.1, revealed by 31 percent of parents, are consistent with this region. As $C \to F$ the region disappears and Proposition 1 obtains.

Realized payment under the Voucher is $\mu_V = \mathbf{1}\{\beta R \ge F - C\}$, which is below one for a sufficiently present-focused parent. Proposition 2 continues to hold for parents with β and $\tilde{\beta}$ both at least $(F-C)/R$, for whom $\mu_V = \tilde{\mu}_V = 1$.

Remark 3. (i) If $y_L < F - C$, a transfer received on fee day leaves a low-state shortfall of $g \equiv F - C - y_L$, which the parent must save out of y_1. The period-1 self saves it if and only if $\tilde{\beta} \ge g/[g + q(R-F)]$ under Fee-Day Cash and $\tilde{\beta} \ge g/[g + q(R-F+C)]$ under the Voucher, where the larger gain under the Voucher reflects the payment that saving keeps from being forfeited; the cutoff under Early Cash remains $\bar{\beta}_S$. The premia are given by the same expressions in $\tilde{\mu}_d$. (ii) If the return is instead proportional to the amount paid, so that each shilling paid toward the fee yields R/F, the Voucher secures a return of RC/F and the earmarking premium is $(C/F)(R-F)\,\mathbf{1}\{\tilde{\beta}R < F\}$, with no negative region.

C Preference and Belief Elicitation

Supplementary Appendix Figure A2 shows the sequence of offers in each task.

The elicited premia are censored by construction at the bounds of the elicitation grid, so we estimate them with two-limit Tobit models. The latent premium is

$$\text{WTP}_i^* = Z_i'\pi + \varepsilon_i, \qquad \varepsilon_i \sim \mathcal{N}(0, \sigma^2), \tag{C.1}$$

observed as

$$\text{WTP}_i = \begin{cases} -17{,}500 & \text{if } \text{WTP}_i^* \leq -17{,}500, \\ \text{WTP}_i^* & \text{if } -17{,}500 < \text{WTP}_i^* < 17{,}500, \\ 17{,}500 & \text{if } \text{WTP}_i^* \geq 17{,}500, \end{cases}$$

separately for the voucher and delay premia. With only a constant in Z_i, the Tobit recovers the latent means reported in Table 2; the heterogeneity estimates in Table 4 add baseline covariates to Z_i.