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Effects of a single cash transfer on school re-enrollment during COVID-19 among vulnerable adolescent girls in Kenya: Randomized controlled trial

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ABSTRACT

COVID-19 related school closures in Kenya were among the longest in Africa, putting older adolescent girls nearing the end of secondary school at risk of permanent dropout. Using a randomized-controlled trial we evaluated a logistically simple cash transfer intervention in urban areas designed to promote their return to school. There were no required conditions for receiving the transfer and the intervention is interpreted as a labeled cash transfer. It had substantial significant effects on re-enrollment of adolescent girls, with greater effectiveness for older girls and even for some not enrolled earlier in the school year. The program effectiveness demonstrates feasibility of the approach and underscores the potential importance of additional resources for schooling during the pandemic, when a large majority of households had suffered income losses.

1. Introduction

Economic lockdowns and associated school closures during the COVID-19 pandemic could have enduring negative implications for educational outcomes worldwide (World Bank, UNESCO & UNICEF, 2021). Consequences of the disruptions might be particularly severe in lower-income settings with poor outcomes even before the crisis (United Nations, 2020; Dang et al., 2022; Wang et al., 2023). Within those settings certain groups of vulnerable students, for example girls or those at stages in their schooling trajectory when they are at higher risk of dropout, could be affected more (Akmal et al., 2020; Andrabi et al., forthcoming).

We present a randomized controlled trial (RCT) evaluating the effects of a single cash transfer of ~US $150 made to households in densely populated informal urban settlements in Nairobi, Kenya, when schools reopened after prolonged closures. The transfer was unconditional, but households were informed that it was intended to help cover the costs of re-enrollment for a specific adolescent girl nearing completion of secondary school. Following Benhassine et al. (2015) and others, we interpret the intervention as a labeled unconditional cash transfer, although the main motivation for the chosen design was for it to be logistically feasible (and thus easily scalable), allowing quick rollout during the pandemic. Notably, while there is considerable research on the impacts of cash transfers on education, less is known about their effects during severe economic crises (Doocy & Tappis, 2017) and relatively little known about their effects during the height of the pandemic (Gentilini et al., 2021).

Following a pre-specified analysis plan (Casey et al., 2012), we find that the COVID-19 cash transfer treatment had substantial significant effects on re-enrollment of adolescent girls when schools reopened. Despite enrollment in the control group being relatively high (88%) after reopening, even higher than before the school closures, beneficiaries were 7.7 percentage points more likely to be enrolled. Effects were larger among older girls and those not actually enrolled earlier in the year. Therefore, the transfers helped stave off permanent dropout for girls at a vulnerable point in their secondary schooling. There was also some evidence, though not robust, that the gain for targeted girls may have been somewhat offset by negative spillovers to other children in the household. The results demonstrate the potential for such an emergency transfer program and at the same time underscore how lack of resources likely remains a barrier to universal secondary school completion in Kenya. Finally, while cash transfers are not generally considered highly cost-effective for school enrollment outcomes (JPAL 2017), at least compared with other cash transfer programs the

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intervention was relatively cost-effective.

Conditional, unconditional and unconditional labeled cash transfers have previously been shown to affect short-term schooling decisions in a variety of low- and middle-income settings.[1] This body of literature points to possible differences among the three approaches in terms of their underlying mechanisms and their effects (Baird et al., 2011; Baird et al., 2014; Benhassine et al., 2015; Kilburn et al., 2017). For example, research often reveals heterogenous effects along important dimensions including sex, age, grade level, prior enrollment status or household wealth. Another consideration specific to programs that also have age cut-off or other individual-level targeting approaches is whether there might be positive or negative spillovers on schooling decisions for other children in the household (Barrera-Osario et al., 2008; Ferreira et al., 2009; Lincove & Sheather, 2016; Camilo & Zuluaga, 2022).

The types of programs considered unconditional labeled cash transfers have different specific features but have in common some sort of articulation or nudge on how to spend the money. For example, the Kenyan Cash Transfer for Orphans and Vulnerable Children (CT-OVC) is a labeled program in which upon registration beneficiaries are instructed that they are expected to use the money for the health and education of the targeted child. The Lesotho Child Grants Program (CGP) included reminders about its purpose every other month when the transfers were made (Pace et al., 2019). In Indonesia, annual registration for the Tayssir Cash Transfer Program was done at schools by the headmasters at the same time they were registering and enrolling students (Benhassine et al., 2015), implicitly endorsing education. In addition, the Tayssir program was clearly labeled with flyers referring to it as a pilot program to fight school dropout. The cash transfer intervention we study was not explicitly labeled like Kenya's CT-OVC and or Lesotho's GCP but embeds nudges in line with those in other labeled transfer programs.

During the initial period of the pandemic, unconditional cash transfers were a widely implemented policy response. In most countries transfers were initiated or expanded by building on existing programs in what appears to have been the largest ever scale up of social assistance (Cejudo et al., 2020; Gentilini, 2022). Rigorous evaluation demonstrates that transfers during the initial months of the COVID-19 pandemic crisis modestly improved economic welfare (Aggarwal et al., 2020; Banerjee et al., 2020; Gentilini et al. 2022; Londoño-Vélez & Querubín, 2022). Despite more than a dozen programs mentioning schooling or education, however, there is only minimal direct evidence on schooling effects, at least in part because the assessments were done when schools were closed. One exception is for a randomized study in Colombia which found modest increased parental investment in schooling during the period (0.03 standard deviations), reflecting increased spending on remote learning activities and materials (Londoño-Vélez & Querubín, 2022). Our study contributes to the evidence base by examining the effect of cash on school re-enrollment during the pandemic in Kenya.

2. Background

The study is implemented in two urban informal settlements of Nairobi: Kibera and Huruma. Characterized by high mobility and high population density (~20,000 per square kilometer) with multiple religious and ethnic groups, the settlements have low-quality housing and lack basic services (APHRC, 2014). About half of all households only have simple pit latrines as opposed to flush toilets and only one-quarter have electricity. Similar to elsewhere in sub-Saharan Africa (Josephson et al., 2020), residents of Kibera faced substantial income loss and related economic challenges due to the pandemic and associated lockdowns. For example, the first round of a COVID-19 effects longitudinal phone survey following adolescents in urban informal settlements of Nairobi revealed that in June 2020 nearly 40% of their households had full income loss and a further 50% had partial income loss (Population Council, 2021). Correspondingly, 75% had skipped meals in the prior week. Similar patterns were observed earlier in April 2020 in a separate online rapid assessment in both rural and urban areas of Kenya (Kansiime et al., 2021).

Public primary school in Kenya has eight grades and has been free since 2003, before even the oldest girls in our study had begun schooling. Secondary school (forms 1–4 corresponding to grades 9–12), however, is tuition fee based both for public and private schools. Kibera and Huruma have considerable public and private school choice at both primary and secondary levels. Primary completion is nearly universal in the settlements although grade repetition and transfers between schools are high, with students often behind for their age (Maluccio et al., 2018). Despite the improvements in primary school completion in the country there remain significant bottlenecks in the transition to and completion of secondary school, where admittance depends on national primary school leaving tests (Lucas & Mbiti, 2012).

The Ministry of Education closed all schools on March 15, 2020, near the end of the first of the three terms in the school year, which normally coincides with the calendar year. Remote learning opportunities during closures were limited. Although about half of adolescents in the June 2020 survey reported reading school or other books, only one-third participated in mobile phone or television learning and all spent far less time on learning activities compared to what they would have done under normal schooling circumstances (Population Council, 2021). In July it was announced schools would remain closed until January 2021. In mid-October, however, the Ministry adjusted the return plan and some schools reopened, but only for grades 4, 8 and form 4. The latter two classes mark the end of primary and secondary school levels when students prepare for the national exams determining completion of one and entry into the next schooling level; it was to help students prepare for the exams that the government prioritized those grades for earlier return. All schools were allowed to reopen fully in January 2021 for what was treated as the second term of the 2020 school year, making the closures among the longest in Africa and elsewhere (Reuters, 2020).[2]

After the prolonged school closures (for most students lasting from March–December) and extreme household economic stress, there was substantial risk that adolescents who had been attending school prior to the pandemic would not return. These concerns were reflected in the June 2020 survey in which although 88% of 15–19- year-old adolescents enrolled prior to school closings expected to return, 60% worried that a barrier would be school fees (Population Council, 2021). As seen elsewhere in sub-Saharan Africa (Dang et al., 2022), failure to return could lead to loss of key literacy and numeracy skills and lower final grade attainment including not completing secondary school, a prerequisite for any tertiary education and an important credential in the labor market in its own right.

The combination of household inability to pay school fees (which are typically due at the start of each of the three school terms per year), potential prioritization of male schooling by households, and increased risk of pregnancy could hinder the return of adolescent girls, even in a setting with relatively high enrolment rates. This possibility echoes initial concern and later evidence regarding possible gender differentiated effects of the pandemic (Akmal et al., 2020; Hidrobo et al., 2020; Gavrilovic et al., 2022). Akmal et al. (2020) report that many frontline organizations expected school closures would increase the risk of gender-based violence, pregnancy and early marriage. In rural Kenya,

[1] There is substantial literature on conditional and unconditional cash transfer programs, starting with Fizsbein and Schady (2009) and later reviewed by Bastagli et al. (2016). Work focused on labeled unconditional transfers includes Benhassine et al. (2015), Pace et al. (2019) and Heinrich and Knowles (2020).

[2] National exams for grade 8 and form 4 were rescheduled from November 2020 to March 2021. The third and final term of the 2020 school year was from May to July 2021. Subsequent accelerated school terms realigned the school and calendar years starting in January 2023.

Zulaika et al. (2021) found that among girls in their final year of secondary school, lockdowns led to higher dropout as well as increased sexual activity and pregnancy. In this context, after lockdowns ended and schools fully reopened, cash transfers could have potential to relax important liquidity constraints to cover the lumpy expenses associated with enrollment, in particular school fees, as well as to compensate for opportunity costs of schooling (Fiszbein & Schady, 2009).[3]

3. Study design and methodology

3.1. COVID-19 cash transfer treatment

The intervention provided a single cash transfer of 16,000 Kenyan Shillings (~$150) with the purpose of encouraging re-enrollment of girls after prolonged school closures[4]. We designed it with minimal complexity (in particular with only a single transfer) to ensure it would be logistically feasible during the uncertainty of the pandemic and be easily replicable. Fig. 1 presents the timeline of school closings and reopening, delivery of intervention components and the surveys.

On December 1, 2020, shortly after randomization to treatment and control (Section 3.2), Population Council notified treatment households by phone that they would be receiving a monetary transfer. The script (translated from Swahili) was:

> [Name of girl] has been randomly selected within the AGI-K cohort to benefit from a single education cash transfer. You will receive 16,000 Kenyan Shillings in early January [2021] to support the cost of her schooling so that she can re-enroll when schools reopen after COVID-19 closures. We wish to collect your bank account details to enable disbursement of the cash transfer in January. Population Council.

The transfer amount was calculated to cover average school fees for one term of secondary school, typically due in the first few weeks of each term. The amount was approximately 10–15% of average annual household income in Kibera prior to the pandemic (Tompsett et al., 2023). Despite the somewhat shortened term shown in Fig. 1, fees were not discounted when schools reopened. Therefore, the transfer was not intended to—nor did it necessarily—cover the full cost of schooling for each girl. Fees differ across schools and can be higher for private schools, which about one-quarter attended. Moreover, students normally have other schooling-related expenses including books and materials, uniforms and transportation.

Following the practice of Kenyan government social protection programs including the CT-OVC, money was transferred directly into the bank account designated by the parent (or guardian) with main financial responsibility for the girl. A majority of beneficiaries already had a bank account but for those without one Population Council assisted in opening it and covered any related administrative fees.[5] This approach was possibly more expensive and logistically complex than relying on a mobile money service such as widely used M-PESA, but also was chosen to provide greater security for transferring the large sum and to ensure the intended beneficiaries received the transfers. A 2019 financial access study had found that 8% of mobile money users in Kenya reported losing funds due to fraud (Blackmon et al., 2021).

From January 6–8, 2021, just after schools had been allowed to fully reopen, Population Council transferred the 16,000 Kenyan Shillings to treatment households and on January 11 sent the following SMS message:

> Happy New Year! We are delighted to inform you that you have received 16,000 Kenyan Shillings in your bank account to support education for [name of girl]. Please check your account, and if there are any problems, contact us at [phone number]. Population Council.

After this message was sent there was no other contact with the household until a follow-up survey in late February (Section 3.2).

For the research we refer to the intervention as the COVID-19 cash transfer treatment, but as evident from the messaging it was not named during exchanges with beneficiaries. Apart from the eligibility criteria assessed prior to contacting study participants (Section 3.2), there were no required conditions for receiving the transfer. However, because the messaging articulated guidance on how to use the money—including the initial phone message informing treatment households of the forthcoming transfer and the subsequent SMS message after the money was deposited—rather than a pure unconditional transfer we interpret it as an example of a labeled cash transfer (Benhassine et al., 2015; Pace et al., 2019; Heinrich & Knowles, 2020).

In addition, the timing of the SMS message that alerted beneficiaries the transfer had been made reminded them of its intended purpose just as schools were fully reopening. Consequently, with our research design it is not possible to separate the influence of that SMS reminder from the influence of the transfer itself, in contrast to a design providing cash transfers both with and without text messaging.[6] Therefore, we consider the SMS reminder as a nudge that is part of the intervention's messaging package. Arguably, this is not unlike the types of messaging and reminders in other labeled cash transfer programs such as Kenya's CT-OVC and Lesotho's CGP, or the additional program components implicitly encouraging school enrollment in Indonesia's Tayssir program.

3.2. Research design and data

The study sample is drawn from the Adolescent Girls Initiative-Kenya (AGI-K) longitudinal cohort of girls living in Kibera and Huruma, first interviewed in 2015 when they were 11–15 years old and re-interviewed in 2017 and 2019 (Austrian et al., 2021b; Kangwana et al., 2022). In 2015, AGI-K randomized individual girls residing in Kibera to one of four interventions and also included a non-experimental comparison sample of girls from nearby Huruma, yielding five distinct study arms (Table 1). Austrian et al. (2016) provide details of the AGI-K program; most relevant for the current study is that three of the study arms included an education conditional cash transfer (CCT) which lasted two years, ending in 2017.[7] Although the effects on schooling from the earlier randomized AGI-K intervention were only moderate, we consider how cross-randomization with the prior program influences interpretation and might moderate the influence of the current COVID-19 cash transfer treatment we study in Section 3.3.2.

Given the intervention objective of promoting re-enrollment in a context where relatively few go on to tertiary education, we excluded from the study girls likely to have already completed secondary school (21%) as well as those who had not attended school for the last several years and therefore were likely permanent dropouts (4%). Specifically, we screened all girls from the AGI-K cohort re-interviewed in 2019 ($N = 2558$), treating as eligible girls who had not yet enrolled in or completed the final year of secondary school (form 4), but who had been enrolled in school at some point since 2017; this yielded 1912 eligible adolescent girls. Using contact information last updated in mid-2019, in the last week of November 2020 we administered a short baseline phone survey attempting to contact and update information on all eligible girls,

[3] Another possible mechanism is that the transfer reduced economic-related parental stress, facilitating optimal decision making including for investment in child human capital (Kilburn et al., 2017).

[4] The study protocol is registered at ISRCTN (12792822).

[5] The 2015–17 AGI-K intervention upon which the study builds (Section 3.2) also used bank accounts, partly explaining why a majority already had them.

[6] Bursztyn and Coffman (2012) find that the addition of text reminders increased the effectiveness of cash transfers in raising adolescent schooling in urban informal settlements in Brazil.

[7] The annual value of transfers for the CCT was approximately equivalent to the one-time COVID-19 cash transfer.

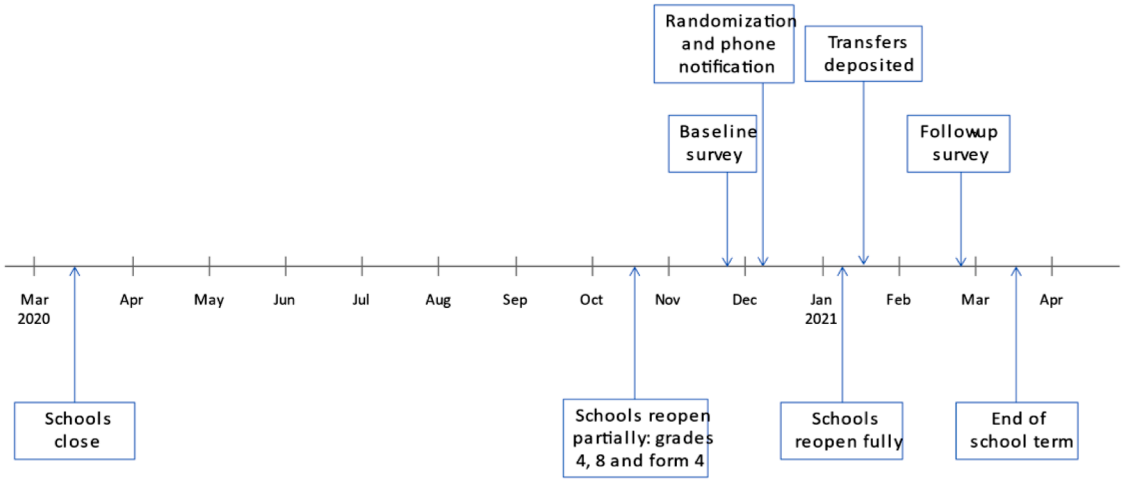

Fig. 1. Study timeline.

Table 1
AGI-K study arms.

AGI-K Study arms and associated intervention package	Abbreviation	Location
Violence Prevention Only	V-only	Kibera
Violence Prevention + Education CCT	VE	Kibera
Violence Prevention + Education CCT + Health	VEH	Kibera
Violence Prevention + Education CCT + Health + Wealth Creation	VEHW	Kibera
Non-experimental comparison group	Huruma	Huruma

Notes: AGI-K interventions were implemented from 2015 to 17.

successfully interviewing 1620 (85%).

After the baseline survey was completed, the girls were stratified by the five AGI-K study arms and half of each arm randomized into the COVID-19 cash transfer treatment and half into the control. Messages and transfers to the treatment group were then delivered as described above. Finally, we administered a short follow-up phone survey in the last week of February, approximately six weeks after the start of the school term. Interviewers were not informed of respondent treatment status. Control households received no treatment, i.e., no cash transfer and no contact or messaging at any point other than the two short phone surveys.

The November 2020 baseline survey indicated that 90% of girls had been enrolled when schools closed earlier that year in March. To reduce the possibility of influencing decisions in either treatment group, intentions to return to school were not asked about in the baseline survey. Intentions were measured earlier for a similar sample, however, in the separate COVID-19 effects longitudinal phone survey described above. In June 2020, 87% of girls 15–19 years old in that survey expected to return to school (Population Council, 2021). Therefore, in the absence of any intervention we hypothesized that 78% (90% × 87%) of girls in our sample would re-enroll in 2021. Given the sample size of 1620, a minimum detectable effects approach was used to conduct power analysis (StataCorp, 2017). With power of 0.8 and significance level of 0.05, the study is able to detect a minimum difference of 5.5 percentage points in school enrollment between girls randomized to receive the cash transfer and girls in the control.

3.3. Methodology

We evaluate the COVID-19 cash transfer treatment using the individual-level randomized controlled trial with treatment and control groups of equal size. Except where otherwise indicated, analyses follow the pre-specified statistical analysis plan (SAP) developed following Hiemstra et al. (2019) and presented in Austrian et al. (2021a). The primary hypothesis is that the single cash transfer made to the household when schools fully reopened would increase re-enrollment, measured as current enrollment for targeted girls six weeks after the start of the January 2021 term. Secondary outcomes include: 1) ever enrollment in 2021, i.e., enrollment at any point during the first six weeks of that term (regardless of current enrollment measured at six weeks); 2) high expectation of finishing secondary school, i.e., whether the parent thought the probability the girl would complete secondary school was greater than 50%;[8] and 3) the fraction of the girl's siblings 6–20 years old currently enrolled. Ever enrollment in 2021 is important since some girls could initially re-enroll but then drop out, for example if they are unable to fully cover their school fees and therefore are not allowed to remain in school. Expectations for finishing secondary school reflect potential persistence of intervention effects beyond what can be observed in our 3-month study. The fraction of siblings enrolled captures potential positive or negative spillover effects within the household resulting from the intervention.

3.3.1. Main analyses

We estimate the main experimental intent-to-treat (ITT) effect of the cash transfer using:

$$Y_{is} = \beta_0 + \beta_1 T_i + \boldsymbol{\alpha}_s + \boldsymbol{X}_i \boldsymbol{\beta}_2 + \varepsilon_{is} \tag{1}$$

where Y_{is} is the outcome for girl i in AGI-K study arm s; T_i equals 1 if the household of the girl is assigned to the COVID-19 cash transfer treatment and 0 otherwise; and $\boldsymbol{\alpha_s}$ are stratification fixed effects for the AGI-K study arms per the COVID-19 transfer stratified randomized design. For the main specification for enrollment and expectations outcomes for individual girls, $\boldsymbol{X_i}$ includes binary indicators for age at the start of the AGI-K longitudinal cohort in 2015, and the 2019 highest grade completed. For the fraction of siblings enrolled outcomes, $\boldsymbol{X_i}$ includes the number of female and male siblings. ε_{is} is an assumed idiosyncratic error term. β_1 in (1) yields the weighted average ITT effect of the COVID-19 cash transfer treatment on outcome Y_{is}.[9] To increase power and to control for other possible differences due to potential imbalance after randomization or resulting from attrition, we also estimate the models with an extended set of controls measured in 2015 and listed in the tables.

[8] A binary indicator is constructed from the parental response to the question: What are the chances that [name of girl] will finish secondary school? Response choices included high, about 50-50 or low.

[9] Weighted over the prior AGI-K treatment study arms as described below in Section 3.3.2.

3.3.2. Heterogeneity analyses

Heterogeneity analyses explore whether subgroups are affected differently, which can shed light on possible underlying mechanisms or be used to improve targeting. We implement two distinct pre-specified subgroup analyses.

The first examines whether effects differ by two variables strongly associated with continued schooling: prior completed grades and socioeconomic status. Because the two variables are likely correlated, following Baird et al. (2020) we estimate using a single equation:

$$Y_{is} = \beta_0 + \beta_1 T_i + \boldsymbol{\alpha}_s + \boldsymbol{age_i} + \beta_2 Form2_i + \beta_3 T_i \times Form2_i + \beta_4 HiSES_i + \beta_5 T_i \times HiSES_i + \varepsilon_{is} \quad (2)$$

$Form2_i$ equals 1 if the girl had completed form 2 by mid-2019 and 0 otherwise. $HiSES_i$ equals 1 if a household-level wealth index is in the top three quintiles and 0 otherwise. The index is the first component from a principal components analysis of assets and housing characteristics constructed from variables measured in the November 2020 baseline survey and presented in the SAP (Austrian et al., 2021a).

The second heterogeneity analysis examines whether prior exposure to the randomized AGI-K education CCT moderated treatment effects. For example, effects might differ in the former CCT arms if because of the earlier AGI-K program households had invested more in the girl's schooling (for example, possibly increasing the return to later investment if there are dynamic complementarities in production [Cunha & Heckman, 2007]), valued schooling differently (for example if the CCT implicitly or otherwise provided important information on schooling or its returns), or were more prone to misinterpret the COVID-19 cash transfer as a conditional, rather than unconditional transfer. We estimate:

$$Y_{is} = \beta_0 + \beta_1 T_i + \beta_2 T_i \times CCT_i + \boldsymbol{\alpha}_s + \boldsymbol{X_i \beta}_3 + \varepsilon_{is} \quad (3)$$

similar to Eq. (1) but including an interaction between T_i and CCT_i, a variable equal to 1 if the individual was exposed to the AGI-K education CCT that operated from 2015 to 17 (Table 1) and 0 otherwise. β_1 in (3) yields the ITT effect of the COVID-19 cash transfer treatment on Y_{is} for girls not exposed to the earlier AGI-K CCT and β_2 yields the differential effect of the treatment on Y_{is} for girls who had been previously exposed.[10] The model is estimated excluding the Huruma non-experimental comparison group study arm. Because of the cross-randomization, Eq. (3) represents the long-form version (with interactions) for a factorial design as described in Muralidharan et al. (2023), instead of the short-form (without interactions) estimated in Eq. (1). This heterogeneity analysis makes clear that because of cross-randomization with a prior intervention (i.e., a factorial design), β_1 in the main specification in Eq. (1) is the weighted average ITT effect over the different study arms from the previous AGI-K experiment.

3.3.3. Attrition sensitivity analyses

Based on contemporaneously emerging guidance on phone surveys during the pandemic and practical learning from the separate COVID-19 effects longitudinal survey carried out by Population Council described above, we implemented various protocols to minimize non-response and attrition, including asking for alternate phone numbers and multiple contact persons. The relatively short 3-month time frame between the November baseline and February follow-up facilitated tracing compared to over a longer period. Nevertheless, given the high baseline mobility in the population and uncertainty during the pandemic (when many Kenyans in urban areas relocated to rural natal communities), we expected some attrition. Attrition reduces sample size and potentially threatens the internal validity of the randomized experiment if it differs across treatment and control (Molina-Millán & Macours, 2017).

Therefore, we report differences in levels of attrition, as well as compare means for treatment and control in the baseline and follow-up surveys, to explore whether there are observable differences related to attrition. In addition, for both the primary and secondary outcomes we implement three approaches to examine sensitivity of the estimates to attrition. First, we exclude the Huruma study arm where previous attrition in the cohort had been highest. Second, we use a large set of additional controls to model attrition and calculate inverse probability weights (IPW; see Appendix for details) for estimation of attrition-corrected results. Third, we estimate Lee attrition bounds, tightening by AGI-K study arm (Lee, 2009).

We report robust standard errors for all results except Lee bounds for which standard errors are bootstrapped with 1000 repetitions (StataCorp, 2017). Significance is set at the 5% level.

3.3.4. Additional analyses not pre-specified

Because of the eligibility criteria and the randomization strategy, when developing the SAP we anticipated no or minimal variation in outcomes at baseline, for example expecting that nearly all girls would be enrolled in March 2020 and there would be no difference between treatment and control. In Section 4.1 below, however, we document that not all girls in the sample were enrolled earlier in the year and that there were small differences in outcome variables at baseline across groups. Consequently, we implement another recommended specification, analysis of covariance models, including the baseline outcome measure in Eq. (1) (Banerjee et al., 2007).

Last, to further explore possible underlying mechanisms we consider additional heterogeneity analyses not pre-specified in the SAP using the approach in Eq. (3). Among others, these include whether the girl was enrolled in March 2020 prior to school closings and different age groups. We divide the sample approximately in half by age, ≤12 years old versus >12 years in 2015 (when the AGI-K longitudinal cohort survey first began); prior enrollment was higher for younger girls suggesting reduced potential for intervention effects for them.

4. Results

4.1. Sample flow, summary statistics and balance

As reported in Section 3.2, for the baseline survey we attempted to interview all 1912 eligible girls and completed interviews with 1620 (84.7%),[11] all of whom were included in the study (Table 2). On December 1, 2020, stratified randomization assigned 813 girls to the COVID-19 cash transfer treatment and 807 to control.[12] Between January 6–8, 2021, electronic transfers of 16,000 Kenyan Shillings were made to bank accounts of the parents of girls assigned to treatment. For 812 of the assigned 813 treatment households, electronic delivery to the designated bank account was verified. Population Council made no transfers to households of control girls.

In late February we attempted to re-interview all those interviewed

[10] There were only modest effects of the AGI-K education CCT on primary school completion, transition to secondary school and completed grades in 2019 (Kangwana et al. 2022). Nevertheless, we use as a control completed grades measured in 2015 in equation (3) to ensure it is uncorrelated with CCT_i.

[11] Of 292 (15.3%) not interviewed, 57 were refusals. We were unable to contact the remaining eligible girls. There are only minor average differences between girls interviewed or not in terms of prior education, parental characteristics and the household wealth index, with interviewed households appearing to be slightly better off.

[12] Randomization was done in Stata using the sample command with by option for stratification by AGI-K study arm (Statacorp 2017). In two households with two co-resident eligible girls each, the randomization assigned one to treatment and one to control. We reassigned the two girls initially assigned as control to treatment to ensure the same treatment status for both girls in the household, and their households received a double transfer.

Table 2
Sample flow by COVID-19 cash transfer treatment group.

Eligible			1912
Baseline survey (November 24–29, 2020)			1620
(% of eligible)			(84.7%)
	Treatment	Control	Total
Randomization (December 1, 2020)	813	807	1620
Transfer delivery (January 6–8, 2021)[1]	812	0	812
Follow-up survey (February 20–March 4, 2021)[2]	787	730	1517
(% of baseline)	(96.8%)	(90.5%)	(93.6%)
Loss to follow-up	26	77	103
(% of baseline)	(3.2%)	(9.5%)	(6.4%)
Reasons for loss to follow-up (% of loss to follow-up)			
Refusal	7 (26.9%)	25 (32.5%)	32 (31.0%)
Failed to contact	18 (69.2%)	48 (62.3%)	66 (64.1%)
Other	1 (3.9%)	4 (5.2%)	5 (4.9%)

[1] 796 transfers made January 6–8. Due to bank account errors, 11 were made January 21st and 6 in early February.
[2] All interviews but one completed by February 26.

at baseline, completing the follow-up survey for 1517 (93.6%). *Re*-interview rates are high in both groups, but higher in treatment (96.8%) compared to control (90.5%). Reasons for loss to follow-up are distributed similarly within treatment and control groups; approximately one-third refusals and two-thirds failure to make contact, most commonly because available contact phone numbers were no longer in service. *Re*-interview rates are similar between treatment and control for girls who had been exposed to the AGI-K education CCT from 2015 to 17, but somewhat lower in the control for girls in the V-only and Huruma study arms (Appendix Table 1).

We summarize by COVID-19 cash transfer treatment group pre-intervention characteristics measured in 2015 (Table 3a) and in the November 2020 baseline survey (Table 3b). For each variable we consider two samples, the full November 2020 baseline sample ($N = 1620$) and the February follow-up sample after attrition ($N = 1517$). Eligible girls are 12.2 years old on average in 2015 and therefore nearly 18 years old at the time of this study. In 2015 virtually all were enrolled and they had completed 5.2 grades on average. At the time, 95% of households expected them to complete secondary school or higher. More than half of their mothers and three-quarters of their fathers had completed primary school. Only half resided with both parents.

Five years later in 2020 the girls have advanced nearly one grade per year, with 9.8 grades completed (Table 3b). Less than 4% are still in primary school (with nearly all of those in the final year) and a third each are in forms 3 and 4. One-third of the girls are a grade or more behind for their age. About one-quarter of those currently enrolled attend private school, and more than half attend (private or public) boarding school. Because of the Kenyan secondary school admission process, although the sample is from two settlements in Nairobi the girls attend hundreds of different secondary schools throughout the country. At the time of school closings in March 2020, 90.4% are enrolled. Only a small fraction in the sample is married, pregnant or have a child.

To assess balance, we present standardized differences between treatment and control households for the baseline and follow-up samples and test the differences in means controlling for the strata used in randomization. Because randomization was done after the baseline survey, differences between treatment and control groups for the baseline survey are due to chance. Larger differences in the follow-up compared to the baseline sample, however, reflect the possibility of differential non-random or selective attrition between the two groups.

For the baseline sample (left panels), average characteristics measured in 2015 and 2020 across the COVID-19 cash transfer treatment and control groups are similar with all but one standardized difference $< |0.10|$ standard deviations (SD).[13] Importantly, standardized differences change little after limiting the full baseline sample to the follow-up sample (moving from the left to right panels) suggesting minimal differential selective attrition in mean characteristics.

Examining possible imbalance in pre-intervention measures of the outcomes in Table 3b, however, there are small differences in current enrollment and expectations, which are 1–2 percentage points higher for girls in treatment households. At the same time, the baseline measure for the fraction of siblings enrolled is 3 percentage points lower for treatment households.[14] Taken together, the patterns suggest good balance in the sample and limited differential selective attrition on observables but point to the potential importance of controls for baseline outcome measures as proposed in Section 3.3.4.

In Table 4 we summarize household-level experiences during the pandemic. Results underscore the severity of the health and economic crises and point to the potential importance of cash transfers in alleviating liquidity constraints. In the past year about 20% reported that a household member had a major illness (although few specified COVID-19) and 5% that a household member had died. Nearly 90% of households lost income and about 15% had relocated, often temporarily returning to their natal village. Despite the direct bank transaction information, only 79% of households in the treatment group reported receipt of cash assistance in the past year. The discrepancy from the nearly 100% confirmed deposit rate is likely due to a combination of reporting bias and some interviews being carried out with a household member who did not recall or had been unaware of the transfer. Nearly one-quarter of control households report receiving cash assistance from other institutions, consistent with the presence of various relief efforts during the pandemic (Ouma, 2021).

Conditional on reporting cash assistance, 82% of treatment households agreed it had a requirement that the girl enroll in school. This means that treatment households generally understood the purpose of the transfer but also raises the possibility that some may have considered it a formal condition for receiving the cash even though it was not. It was not possible to recall funds once transferred to the household's private bank account, which occurred at the start of the January school term. Moreover, from the outset all messaging was that this was a one-off transfer with no indication of conditions to be verified or of possible future benefits. We reflect further on participant understanding of conditionality when exploring heterogeneous effects in Section 4.4.

4.2. *Primary results for targeted girls*

Table 5 presents estimates from the randomized controlled trial for the individual level outcomes based on Eq. (1). Notably, and in line with expectations from the June 2020 survey, overall enrollment for the sample after prolonged school closures was high. In February, 87.8% of the control sample was enrolled and enrollment was even higher (93.9%) for those who had been enrolled earlier in the school year just

[13] The standardized difference for having a child is −0.10 SD, amplified because of the low percent with a child.

[14] Sibling enrollment was measured retrospectively in the follow-up so is unavailable for the full baseline sample (left panel of Table 3b). The negligible attrition-driven change across baseline and follow-up samples for all other outcomes, however, suggests the difference in sibling enrollment at follow-up provides a reliable measure of the baseline difference. The modest imbalance observed in early 2021 is also corroborated by assessment of sibling enrollment in 2019, when it was also 1–2 percentage points lower in treatment households.

Table 3a
Comparison of means by COVID-19 cash transfer treatment group: Initial 2015 characteristics.

	Baseline sample				Follow-up sample			
	T	C	Standardized difference (T-C)	P-value of difference	T	C	Standardized difference (T-C)	P-value of difference
Age, years	12.19	12.20	−0.01	[0.88]	12.20	12.18	0.02	[0.77]
(sd)	(1.13)	(1.18)			(1.13)	(1.17)		
N	813	807	1620		787	730	1517	
Cognitive test score (0–16), mean	8.16	7.94	0.07	[0.15]	8.18	7.89	0.09	[0.07]
(sd)	(3.12)	(3.08)			(3.12)	(3.05)		
N	808	802	1610		782	726	1508	
Completed schooling, grades	5.18	5.20	−0.02	[0.68]	5.19	5.20	−0.01	[0.84]
(sd)	(1.13)	(1.13)			(1.12)	(1.11)		
N	813	807	1620		787	730	1517	
Expect girl to complete secondary = 1	0.95	0.94	0.04	[0.43]	0.95	0.94	0.05	[0.30]
(sd)	(0.22)	(0.24)			(0.22)	(0.24)		
N	813	807	1620		787	730	1517	
Mother completed primary school =1	0.60	0.62	−0.04	[0.42]	0.61	0.63	−0.03	[0.57]
(sd)	(0.49)	(0.48)			(0.49)	(0.48)		
N	761	756	1517		736	685	1421	
Father completed primary school = 1	0.73	0.75	−0.03	[0.57]	0.74	0.76	−0.04	[0.58]
(sd)	(0.44)	(0.43)			(0.44)	(0.43)		
N	663	669	1332		642	607	1249	
Lived with both parents = 1	0.54	0.55	−0.02	[0.68]	0.54	0.55	−0.03	[0.54]
(sd)	(0.50)	(0.50)			(0.50)	(0.50)		
N	810	802	1612		784	725	1509	
Fraction of siblings enrolled	0.95	0.94	0.07	[0.20]	0.95	0.94	0.07	[0.18]
(sd)	0.17	0.19			(0.17)	(0.19)		
N	708	697	1405		685	629	1314	
Household wealth quintile (1–5), mean	3.00	3.06	−0.04	[0.45]	3.01	3.03	−0.01	[0.79]
(sd)	(1.43)	(1.45)			(1.43)	(1.45)		
N	810	802	1612		784	725	1509	

All data are from the AGI-K 2015 survey, summarized for the November 2020 baseline ($N = 1620$) and February 2021 follow-up ($N = 1517$) samples. Means and standard deviations (in parentheses) shown for T=treatment and C=control. Standardized difference for each variable is (mean T- mean C)/(overall standard deviation). P-value (in square brackets) of the test of significance of differences in means across treatments controlling for stratification and using robust standard errors.

prior to school closings.[15] Further limiting to those enrolled in either grade 8 or form 4, the grades that had begun returning to school part time in mid-October 2020, 97.3% in the control were enrolled. The main reasons for not enrolling in 2021 were marriage or pregnancy (42%) and being unable to cover school fees (32%).

Notwithstanding the high enrollment in the control group, the cash transfer had a substantial positive effect on the primary outcome for targeted adolescent girls, increasing enrollment by 7.7 percentage points (9%) to above 95%, even higher than at baseline (Panel A, column 1). The estimated effect was similar for having ever been enrolled in 2021, 6.9 percentage points (8%).[16] Few girls (5%) had changed schools, and virtually all who re-enrolled were in the same grade as in March 2020. Alongside the effects on enrollment there was a large 9.8 percentage point (13%) increase in high expectation of finishing secondary school, relative to 75.5% in the control (Panel B, column 1). The increase in expectations points to the possibility that the effects, measured six weeks into the term after full reopening, would persist.

Effects on both enrollment and expectations are robust to the inclusion of extended controls and the baseline measure for the outcome (columns 2–3),[17] and to the three attrition sensitivity analyses (columns 4–6): 1) removal of the Huruma non-experimental comparison group study arm where attrition was highest (and therefore possibly more selective) ;[18] 2) IPW reweighting; and 3) Lee bounds.

The final columns in Table 5 present the additional assessments of the role of age and enrollment status earlier in the school year (columns 7–8). Although the effects are significant for each subgroup, there are large differences between them. Effects on enrollment are twice as large for older compared to younger girls and five times larger for those not enrolled earlier in the year (comprising ~10% of the sample) compared to those enrolled. These patterns suggest the treatment is more effective for those at higher risk of not completing secondary school since temporary dropout before completion is likely harder to reverse at higher ages. There are no significant differences in effects on expectations across the subgroups.

Because the analysis relies on parental reports rather than direct observation or administrative school attendance data, it is possible that reported enrollment suffers from response bias related to the experiment. A priori we tried to guard against this possibility during the interview process: study participants were not told there would be a follow-up survey and interviewers were not explicitly aware of

[15] The high rate of return is similar to longitudinal evidence from urban Côte d'Ivoire after much shorter (March–July) closures (Dupas et al. 2022). It is also within the range for several sub-Saharan African countries other than Kenya examined where between 1–11 % of adolescents enrolled prior to the pandemic were not enrolled in late 2021 (Wang et al. 2023).

[16] The similarity between estimates for current and ever enrollment in 2021 reflects the close correspondence between the two measures. In total only 17 girls who had enrolled prior to end-February were no longer attending by the time of the follow-up survey (6 in treatment and 11 in control). Because the two indicators are nearly identical, we report results for ever having been enrolled in 2021 only in the appendix (Appendix Table 2).

[17] They are also robust to inclusion of an indicator for re-enrollment in November 2020, possible for those in grade 8 and form 4 due to partial reopening. These households had to make both an earlier decision whether to enroll prior to notification or transfer delivery, and a subsequent decision whether to continue (or enroll) in January 2021.

[18] They are also robust to exclusion of the V-only study arm which had similarly higher attrition.

Table 3b
Comparison of means by COVID-19 cash transfer treatment group: 2020 baseline characteristics.

	Baseline sample				Follow-up sample			
	T	C	Standardized difference (T-C)	P-value of difference	T	C	Standardized difference (T-C)	P-value of difference
Completed schooling (grades), mean	9.82	9.77	0.04	[0.40]	9.84	9.79	0.04	[0.46]
(sd)	(1.19)	(1.28)			(1.19)	(1.23)		
N	813	807	1620		787	730	1517	
Enrolled in current school year (March 2020) = 1	0.92	0.89	0.09	[0.06]	0.92	0.90	0.09	[0.07]
(sd)	(0.28)	(0.31)			(0.27)	(0.30)		
N	813	807	1620		787	730	1517	
Current grade (if enrolled)	10.87	10.85	0.02	[0.66]	10.88	10.86	0.02	[0.74]
(sd)	(1.17)	(1.25)			(1.17)	(1.21)		
N	746	718	1464		726	655	1381	
Expect girl to complete secondary (2019) = 1	0.95	0.93	0.05	[0.32]	0.95	0.94	0.05	[0.92]
(sd)	(0.23)	(0.25)			(0.22)	(0.25)		
N	813	807	1620		787	730	1517	
Currently married = 1	0.01	0.02	−0.07	[0.18]	0.01	0.02	−0.08	[0.12]
(sd)	(0.09)	(0.13)			(0.09)	(0.13)		
N	813	807	1620		787	730	1517	
Has a child = 1	0.05	0.07	−0.10	[0.05]	0.05	0.07	−0.10	[0.05]
(sd)	(0.22)	(0.26)			(0.22)	(0.26)		
N	813	807	1620		787	730	1517	
Currently pregnant = 1	0.01	0.02	−0.06	[0.19]	0.01	0.02	−0.07	[0.18]
(sd)	(0.12)	(0.15)			(0.12)	(0.15)		
N	810	802	1612		784	726	1510	
Fraction of siblings enrolled			n.a.		0.88	0.91	−0.11	[0.03]
(sd)					(0.27)	(0.23)		
N					692	624	1316	
Household wealth quintile in 2020 (1–5), mean	2.99	3.00	−0.01	[0.86]	2.98	2.96	0.01	[0.87]
(sd)	(1.41)	(1.42)			(1.41)	(1.43)		
N	813	807	1620		787	730	1517	

All data measured in November 2020 baseline survey shown for baseline sample ($N = 1620$) and follow-up sample ($N = 1517$). Sibling enrollment asked retrospectively in follow-up so unavailable for full baseline sample. Means and standard deviations (in parentheses) shown for T=treatment and C=control. Standardized difference for each variable is (mean T- mean C)/(overall standard deviation). P-value (in square brackets) of the test of significance of differences in means across treatments controlling for stratification and using robust standard errors.

Table 4
Experiences of households at follow-up by COVID-19 cash transfer treatment group.

	T	C	Total
In the past year:			
Household member had a major illness = 1	0.24	0.18	0.21
N	787	730	1517
Household member had Coronavirus = 1	0.03	0.03	0.03
N	784	728	1512
Household member died = 1	0.05	0.04	0.05
N	786	730	1516
Loss of income in the household = 1	0.87	0.88	0.87
N	780	729	1509
Household moved = 1	0.15	0.14	0.14
N	787	730	1517
Household received (any) cash assistance = 1	0.79	0.23	0.52
N	773	730	1503
Cash assistance received had a "requirement" that the girl attend school = 1	0.82	0.10	0.67
N	609	165	774
Total number of female siblings 6–20 years old	1.19	1.07	1.14
(sd)	(1.13)	(1.15)	(1.14)
Total number of male siblings 6–20 years old	1.10	1.09	1.10
(sd)	(1.07)	(1.08)	(1.08)
N	786	730	1516

All data measured in February 2021 follow-up survey ($N = 1517$). Fraction responding yes shown for all variables except number of siblings which show mean and standard deviation in parentheses. T=treatment, C=control and Total=combined.

treatment status. We provide two pieces of additional evidence suggesting the reports are reliable. First, approximately 20% of the sample was also part of the separate COVID-19 effects longitudinal survey including a round administered in early February 2021, which had no messaging or benefits and therefore fewer concerns about potential response bias. This near-contemporaneous survey provides a partial check on the responses for an overlapping sample of 234 girls evenly divided between treatment and control. For 96% of the girls, school enrollment reports match across the two surveys. Moreover, the percent disagreement is similar across treatment and control, suggesting the extent of response bias is minimal. The second piece of evidence is that the reported enrollment rates for siblings were lower for treatment households. It is unclear why, and therefore seems unlikely that, respondents would overstate enrollment for some individuals while simultaneously understating it for others, further suggesting the reports are reliable.

4.3. Results for siblings

In contrast to the estimated positive effect on enrollment for the targeted adolescent girl, examination of other children in the family (limiting the analysis to the selected sample of those with siblings) raises the possibility that the treatment may reduce average enrollment for her ineligible school-aged siblings. Enrollment for siblings 6–20 years old is 88.6% in control households but 3.4 percentage points lower in treatment (Table 6, Panel A, column 1). Estimated effects on enrollment are more negative for female than male siblings, although they are not statistically different from one another (Panels B and C). These findings do not hold, however, after introducing controls for baseline measures of the outcome in column 2. The results for sibling enrollment controlling for attrition (without the baseline outcome) in columns 3–5 are similar to column 1 except for the calculation for Lee bounds which encompass zero, suggesting they may not be robust to potential attrition selection

Table 5
Experimental effect of COVID-19 cash transfer on individual current enrollment and expectations.

	Base	Extended controls	Baseline outcome control	Excluding Huruma	IPW	Lee bounds	By age		By prior enrollment	
							12 or under in 2015	Additional effect for over 12	Enrolled in March 2020	Additional effect for not enrolled
	(1)	(2)	(3)	(4)	(5)	(6)	(7a)	(7b)	(8a)	(8b)
Panel A: Current enrollment=1										
T	0.077***	0.079***	0.066***	0.084***	0.078***		0.056***	0.065*	0.047***	0.213**
	(0.014)	(0.014)	(0.012)	(0.015)	(0.014)		(0.014)	(0.034)	(0.011)	(0.084)
Lower bound						0.073***				
						(0.015)				
Upper bound						0.118***				
						(0.014)				
N	1517	1517	1517	1261	1517	1620		1517		1517
Control group mean	0.878	0.878	0.878	0.879	0.877	–	0.922	0.791	0.939	0.347
Panel B: High expectation of finishing secondary school = 1										
T	0.098***	0.100***	0.099***	0.115***	0.098***		0.101***	−0.004	0.088***	0.040
	(0.020)	(0.020)	(0.020)	(0.022)	(0.020)		(0.024)	(0.044)	(0.020)	(0.087)
Lower bound						0.089***				
						(0.022)				
Upper bound						0.159***				
						(0.024)				
N	1494	1494	1494	1243	1494	1620	974	520	1363	131
Control group mean	0.755	0.755	0.755	0.750	0.756	–	0.773	0.718	0.793	0.417

Notes: T is a binary indicator for treatment in the COVID-19 cash transfer. The base model (column 1) includes binary indicators for stratification and for age in years and completed schooling in 2019. All other regressions (apart from Lee bounds) include base and extended controls; the latter are binary indicators for whether mother and father had completed primary school and whether the girl resided with both parents in 2015, and her score on a cognitive skills test in 2015. Baseline outcome control model (column 3) includes March 2020 enrollment status in Panel A and a binary indicator the girl was expected to complete secondary school or higher in 2019 in Panel B. IPW weights constructed as described in the Appendix. Robust standard errors for regressions reported in parentheses. Lee bounds calculated with tightening for stratification indicators and based on 1000 repetitions and bootstrap calculated standard errors. *** indicates $p<0.01$, ** $p<0.05$, and * $p<0.10$.

bias. Taken together, the patterns point to the possibility that the positive effects on enrollment for the targeted adolescent girl are partly offset by negative effects on other children in the household, though the finding is not robust.

4.4. Heterogeneous analyses

In this subsection we examine heterogenous effects—secondary analyses outlined in the SAP—noting the study was not explicitly powered for them. Moreover, the high enrollment rates in the control group have the implication that margins for substantial heterogeneous effects are relatively small. It is unsurprising, therefore, that we find only minimal evidence of such effects.

First, for individual-level outcomes we consider heterogeneity by completed grade level and wealth index quintile (Appendix Table 3). Having completed form 2 and being in one of the top three quintiles of the wealth index are both associated with higher probabilities of enrollment, and form 2 completion is associated with higher expectations. However, the point estimates for differential effects of the cash transfer for girls from less poor households or with higher completed grades are small, negative and insignificant. There is also no significant evidence that any effect of the transfers on sibling enrollments differs by wealth index quintile, though point estimates suggest potential net negative effects on sibling enrollment may be more muted in less poor households, consistent with the possibility that resources in the poorest households are diverted away from siblings towards the targeted girl.[19]

Second, we considered whether the effect of the cash transfers was different for girls who had previously been exposed to the AGI-K education CCT, estimating the long form of the model (with interactions) excluding the Huruma study arm. Although we pre-specified these explorations, we did not necessarily expect strong effects since the prior program had lasted only two years (2015–17), had only had small effects on schooling, and had been over for three years by the time of the COVID-19 cash transfer. There is little evidence of a differential effect on individual or sibling enrollment but some evidence that effects on high expectations for completing secondary school are concentrated among those previously exposed to the AGI-K education CCT (Appendix Table 4).[20] The subgroup we hypothesized might be more likely to have considered the transfer as formally conditional is not more likely to enroll; therefore, possible differences in understanding of conditionality do not appear to drive the enrollment results.

Finally, we examined whether there were differential effects on treatment households that directly reported receipt of the transfer (80% of households, Table 4).[21] Estimated effects on households reporting receipt were modestly larger but there was no significant additional effect for those households that indicated the transfer had a schooling requirement (logically asked only of those who reported transfer

[19] We do not explore heterogeneity with respect to the target adolescent girl's school level for sibling enrollment.

[20] Consequently, for the enrollment measures, but not for expectations, the weighted average ITT effect estimated in the short-form equation (1) without interactions is similar to the COVID-19 cash transfer main treatment effect estimated in (3). In the long-form model, the size of the effect on current enrollment is lower—0.067 versus 0.077—though still statistically significant at 10%.

[21] Transfer delivery was nearly 100% (Section 4.1), so we do not estimate actual treatment-on-the-treated analyses.

Table 6
Experimental effect of COVID-19 cash transfer on current enrollment of siblings.

	Base	Baseline outcome control	Excluding Huruma	IPW	Lee bounds
Panel A: All siblings	(1)	(2)	(3)	(4)	(5)
T	−0.034**	−0.003	−0.042**	−0.033**	
	(0.015)	(0.004)	(0.017)	(0.015)	
Lower bound					−0.045***
					(0.012)
Upper bound					0.027**
					(0.013)
N (families)	1317	1317	1101	1317	1420
N (individuals)	3383	3383	2831	3383	3689
Control group mean	0.886	0.886	0.886	0.885	–
Panel B: Female siblings					
T	−0.047**	−0.013	−0.053***	−0.047***	
	(0.018)	(0.010)	(0.020)	(0.018)	
Lower bound					−0.062***
					(0.017)
Upper bound					0.029
					(0.018)
N (families)	993	993	829	993	1093
N (individuals)	1722	1722	1434	1722	1919
Control group mean	0.904	0.904	0.905	0.904	–
Panel C: Male siblings					
T	−0.021	0.006	−0.031	−0.020	
	(0.022)	(0.010)	(0.025)	(0.022)	
Lower bound					−0.028
					(0.018)
Upper bound					0.017
					(0.019)
N (families)	1010	1010	851	1010	1077
N (individuals)	1661	1661	1397	1661	1770
Control group mean	0.867	0.867	0.867	0.867	–

Notes: T is a binary indicator for treatment in the COVID-19 cash transfer. All regressions (apart from Lee bounds) control for stratification and the numbers of female and male siblings of the targeted adolescent girl. The baseline outcome control model controls for siblng enrollment rate in March 2020. Girls without siblings in the age range are not included in the regression. We constructed a sample of individual siblings from the survey response in 2021 and estimate using that sample which implicitly weights for family size. Estimates for number of siblings for those attriting in 2021 used in the Lee bounds estimation are based on number of siblings reported in 2019. Number of families differs across columns since not all have female or male siblings. Standard errors allowing for clustering at the family level reported in parentheses. Lee bounds calculated tightening with stratification indicators and based on 1000 repetitions. *** indicates $p<0.01$, ** $p<0.05$, and * $p<0.10$.

receipt). Because virtually all treatment households received the transfer it is unclear exactly how to interpret this although it is consistent with marginally larger effects for those who misinterpreted the transfer as conditional, i.e., as having an actual schooling requirement.

4.5. Cost-effectiveness considerations

We carried out a limited cost-effectiveness analysis for the COVID-19 cash transfer as designed, treating possible spillover effects as negligible and ignoring any potential benefits of the transfers besides increased enrollment and consequent grade completion. In line with other research assessing the effectiveness of cash transfers we calculated cost-effectiveness with and without the transfers since the case can be made that they are direct benefits to households and therefore should not be included as program costs (Dhaliwal et al. 2013; JPAL 2017). Based on budgetary data we estimated the cost per girl to be ~$160 with transfers and ~$14 without them. Measuring cost-effectiveness based on predicted additional years of education per $100, estimates including transfers are 0.05 additional years and excluding transfers 0.55 additional years.

JPAL (2017) argues that because cash transfer programs can have other beneficial impacts, rather than treating them only as school enrollment programs, such interventions are better described as social assistance programs that can (also) increase school enrollment. The research demonstrates—at least in the longer term and outside of crisis situations—cash transfer programs are not as cost-effective for improving enrollment as various health programs such as deworming. It is unlikely that such health programs would have substantially influenced re-enrollment when schools reopened in Kenya, though it is possible that other types of health-related programs could have, for example ones directly addressing the health safety and sanitation in schools or the mental health of adolescent girls, which worsened during the pandemic (Pinchoff et al., 2021; Wang et al., 2023).

Including transfers, the cost-effectiveness of the COVID-19 cash transfer intervention (0.05 additional years per $100) was around the midpoint for unconditional and conditional cash transfers pre-pandemic (JPAL 2017). Without transfers, the intervention was relatively more cost-effective than other programs, probably in large part due to its simplicity. For both metrics important factors underlying effectiveness include inflated prices during the pandemic for many goods and the high enrollment rate in the control group which suggests the intervention affected enrollment decisions only for a fraction of girls who may have been the most difficult, and therefore costly, to influence. The heterogeneous effects suggest that additional years per $100 might have been marginally increased through more directed targeting (for example to the poorest households or older girls) or by reducing costs, for example by making transfers via mobile money instead of using bank accounts. Additional years per $100 might be lower, however, if there had not been a pre-existing registry of eligible girls or if the possible negative spillover effects to siblings were substantial.

5. Discussion and conclusions

The COVID-19 cash transfer was a logistically straightforward and scalable intervention intended to help adolescent girls return to school after prolonged school closures due to the pandemic. It had no formal conditions but did have limited direct messaging and thus can be interpreted as a labeled cash transfer. There were significant positive effects on enrollment for the targeted girls when schools fully reopened. The intervention appears to have even brought some girls not enrolled earlier in the year back into school. Therefore, the transfer helped stave off dropout during a highly uncertain period and at a vulnerable point in their secondary schooling, with possible beneficial implications for future well-being. The effectiveness of the intervention suggests an important role for additional resources for secondary schooling after the initial period of the pandemic when the vast majority of households had suffered income losses.

Effect sizes on enrollment were larger than for an earlier conditional cash transfer program available to the same girls when they were younger, mostly in primary school, and virtually all enrolled (Austrian et al., 2021b). Effects were also larger than for a conditional cash transfer program in neighboring Tanzania that also primarily influenced younger children (Evans et al., 2023). Effect sizes were more similar, however, to impacts on enrollment for adolescent girls in rural Malawi in 2008 and 2009 (Baird et al., 2011) and in 2015 (Kilburn at al., 2017), and on secondary school enrollment in 2009 for a different labeled cash transfer program in Kenya that targeted another vulnerable population, the CT-OVC (Ward et al., 2010).

There was some evidence, though not robust, that the gain for targeted girls may have been somewhat offset by negative spillovers to other children in the household. Even though more tentative, those results point to the importance of considering spillover effects of such interventions, including the possibility that in some settings or for the poorest households a different or more universal targeting scheme might be preferable. Expanding to broader targeting, however, would of course increase costs and lower cost-effectiveness.

The study has several limitations. First, it relied on self-reported measures, although reporting patterns do not suggest strong biases and for a subsample it was possible to corroborate reports using a separate overlapping survey. Second, despite clear messaging, it is possible that some beneficiaries interpreted the transfers as conditional; this may be difficult to completely avoid when providing messaging about the intent of transfers in under-resourced environments, particularly during periods of great uncertainty. While this possibility does not alter the measured effectiveness of the treatment, it does influence interpretation of the underlying reasons for the impacts. Third, the sample was drawn from a longer-term study exposed to an earlier CCT and therefore the control group may not represent a typical counterfactual. The girls were possibly in more stable situations and therefore more likely to return to school than a random sample of adolescent girls in the same age range. In addition, there may have been interactions between the COVID-19 cash transfer and the earlier AGI-K treatments. Implications for external validity and whether these aspects would strengthen or weaken treatment effects are not theoretically clear, however, and interactions with prior CCT exposure did not point to any substantial differences apart from the influence on expectations. Fourth, we only measure schooling outcomes and the intervention may have had other beneficial impacts. Finally, to reliably gauge re-enrollment the period of follow-up was relatively short, and it is not possible to determine longer term effects, for example on secondary school completion.

At the same time, the study has important strengths. First, it was a rigorous individual-level randomized controlled trial with a pre-specified analysis plan, near-perfect compliance in delivery of transfers, and no contamination of the program to control households. Second, even with precautions and adherence to regulations in place during the pandemic, it was quickly implemented underscoring its feasibility under challenging conditions. And third, despite the mobile population and widespread uncertainty at the time, there was a high follow-up survey rate so that estimated effects on re-enrollment for targeted girls are unlikely to have been affected by attrition.

Evidence from other studies demonstrates that cash transfers in the early months of the COVID-19 pandemic crisis modestly improved economic welfare (Aggarwal et al., 2020; Banerjee, Faye, Krueger, Niehaus, & Suri, 2020; Gentilini, 2022; Londoño-Vélez & Querubín, 2022). Our study indicates they were also a viable approach for mitigating the effects of the shock on schooling for female adolescents somewhat later when schools had reopened. Nevertheless, there are almost certainly other important barriers to increasing female secondary school completion in Kenya—including some that may have been exacerbated by the pandemic—that cash transfers alone cannot resolve. These barriers likely include, for example, low school quality or teen pregnancy. In the longer term, therefore, more comprehensive programming is probably necessary to achieve universal secondary schooling.

Author statement

JM, ESH, BK, KA and EM designed the intervention and developed primary and secondary hypotheses. BK, KA, EM, FM directed the intervention and data collection. JM and ESH led the statistical analyses with all participating in interpretation of results. JM wrote the first draft and then all reviewed and edited the manuscript.

Declaration of Competing Interest

None

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econedurev.2023.102429.

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