SSM - Population Health 26 (2024) 101663

Contents lists available at ScienceDirect

SSM - Population Health

journal homepage: www.elsevier.com/locate/ssmph

Long-term impacts of a cash plus program on marriage, fertility, and education after six years in pastoralist Kenya: A cluster randomized trial

Karen Austrian [a,*], John A. Maluccio [b], Erica Soler-Hampejsek [c], Eva Muluve [d], Abdullahi Aden [e], Yohannes D. Wado [f], Benta Abuya [f], Beth Kangwana [d]

[a] GIRL Center, Population Council, PO Box 17643-00500, Nairobi, Kenya
[b] Department of Economics, Middlebury College, Middlebury, VT, USA
[c] Independent Consultant, Barcelona, Spain
[d] Population Council – Kenya, Nairobi, Kenya
[e] Save the Children, Nairobi, Kenya
[f] African Population and Health Research Center, Nairobi, Kenya

ARTICLE INFO

Keywords:
Adolescent girls
Cash transfers
Girls education
Child marriage
Randomized controlled trial
Kenya

ABSTRACT

Background: Preventing early marriage by increasing girls education has shown promise. We assessed the effects of a two-year cash plus program on marriage and fertility in a pastoralist setting in Northeastern Kenya, six years after it began.

Methods: A prospective 80-cluster randomized trial followed 2,147 girls 11–14 years old starting in 2015, re-interviewing 94.2% in 2021. Interventions included community dialogues (violence prevention), a conditional cash transfer (education), health and life skills training (health), and financial literacy (wealth creation). Villages were randomized to one of four study arms: (1) violence prevention only (V-only); (2) + education (VE); (3) + health (VEH); or (4) + wealth creation (VEHW). We used analysis of covariance to estimate intent-to-treat impacts of each study arm with an education component, as well as a pooled (weighted average) study arm combining VE, VEH and VEHW, in reference to V-only, four years after the intervention ended when girls were 17–20 years old.

Findings: Base specification estimates show reductions in the primary outcomes, though none statistically significant in the full sample. Estimates with extended controls are larger and the pooled study arm had significantly lower marriage and pregnancy. There are considerably larger statistically significant effects for the baseline outofschool subsample. Pooled estimates indicate 18.2 percentage point lower marriage compared to V-only and 15.1 percentage point lower pregnancy. For the same group pooled estimates indicate a 27.9 percentage point increase in current enrollment (compared to 7.1% in V-only) and a 1.8 grades increase (compared to 1.2 in V-only).

Conclusion: This study shows the potential for interventions in early adolescence with an education component to delay marriage and fertility into late adolescence and early adulthood in a marginalized and socially conservative setting with low education and high rates of child marriage.

1. Introduction

Although there has been a promising reduction over the past decade from 1 in 4, to 1 in 5 girls married as children (<18 years old) worldwide, the current rate of decline is too slow to achieve the Sustainable Development Goal of ending child marriage by 2030 (United Nations Children's Fund, 2021). Moreover, progress differs substantially across different contexts, with child marriage twice as high in areas of greater economic, social and institutional fragility (United Nations Children's Fund, 2021). Complicating the picture further, advances may have stalled or even reversed during the COVID-19 pandemic. When the pandemic began, global projections were that the combination of extended school closures, economic shocks and adolescent pregnancies could result in as many as 13 million additional child marriages over the following decade (Yukich et al., 2021).

In line with global trends, early marriage in Kenya also has been

* Corresponding author.
E-mail address: kaustrian@popcouncil.org (K. Austrian).

https://doi.org/10.1016/j.ssmph.2024.101663
Received 8 December 2023; Received in revised form 29 February 2024; Accepted 18 March 2024
Available online 23 March 2024

falling. Between 2014 and 2022, the percentage of girls 15–19 years old ever married fell from 13.2% to 8.4% (Kenya National Bureau of Statistics et al., 2015; KNBS and and ICF, 2023). There was a corresponding, though smaller drop in the percent ever pregnant, 18.3%–14.8% (Kenya National Bureau of Statistics et al., 2015; KNBS and and ICF, 2023). It is possible these declines might have been larger without the pandemic. As with global averages, national level trends mask regional variation, which are large in Kenya; across 47 counties in 2022, the percent of 15–19 years olds ever married ranged from 2% to 47% and ever pregnant from 5% to 50% (Kenya National Bureau of Statistics et al., 2015; KNBS and and ICF, 2023).

Substantial literature demonstrates a strong association between increased education and later marriage, (Bates et al., 2007; Bongaarts et al., 2017) but direct interventions aimed at delaying marriage by improving educational outcomes have had mixed results (Handa et al., 2015; Prakash et al., 2019). A 2016 review identified several mechanisms in addition to education that could help delay marriage including community engagement, economic support, and life skills training for girls, although no one approach was identified as being the most effective (Kalamar et al., 2016).

A possible explanation for the failure to single out a dominant approach is that child marriage has multiple drivers which operate to different degrees across different contexts. Psaki and colleagues develop a framework that identifies poverty and economic factors, social norms, lack of agency, lack of opportunity and fear of girls' sexuality and pregnancy as key determinants underlying early marriage that can be addressed through programs and policies adapted to specific contexts (Psaki et al., 2021). In pastoralist settings in Ethiopia and Kenya, for example, poverty and lack of access to education shape norms and support for child marriage, factors that should be considered during program design (Jones et al., 2019; Lowe et al., 2022).

Correspondingly, there is a growing literature on multisectoral approaches that combine different sets of interventions based on contextually relevant drivers (Chandra-Mouli et al., 2018; Chandra-Mouli and Plesons, 2021; Malhotra & Elnakib, 2021). Evidence to date on such locally tailored multisectoral programs is mixed, however, and few studies are able to assess longer-term outcomes when girls reach young adulthood. In Ethiopia, a girls empowerment program, when combined with strong community support, resulted in better outcomes where the intervention did not impact community norms (Hamory et al., 2023). In Bangladesh, a program that combined community engagement with girls empowerment groups reduced child marriage ~25% after 18 months years (Amin et al., 2018). Program effects can also differ for different types of girls facing different constraints. For example the Kenyan multisectoral program aiming to delay child marriage we examine in this paper had significant impacts in the shorter-term, but only for girls who were not enrolled in school when the program started (Austrian et al., 2022).

The timing of interventions also may be important. Early adolescence has been proposed as a potentially critical window for interventions aiming to prevent early school leaving, marriage and childbirth (Blum et al., 2014). For such interventions it is of course to necessary to follow individuals for a sufficiently long period, for example until young adulthood, to gauge their ultimate effects. There is only limited evidence assessing either the durability of initial effects on early adolescents or the extent to which they translate into further impacts later in life. The evidence that does exist relates mostly to cash transfer programs. It also hints at possible heterogeneous effects. Assessment of the effects of a five-year education conditional cash transfer (CCT) program in Honduras found an increase of nearly 0.5 grades for females exposed starting when they were 11–13 years old and measured 13 years later when 24–26 years old. Effects on schooling were slightly larger for (on average poorer) indigenous females who, although no less likely to be married were 4 percentage points less likely to have a child (Millán et al., 2020). For an ongoing CCT in Mexico, females 9–15 years old at the start and exposed to the program two years earlier had higher completed grades after 20 years when 29–35 years old. Although there were no (longer) differences in the percent married or with children by those ages, the findings indicate that girls exposed earlier to the CCT tended to marry and have their children about a half year later (Araujo & Macours, 2021). In rural Malawi, one year of conditional cash transfers for girls in mid/late adolescence who were not in school at the start of the program led to positive, sustained effects on enrollment and reductions in marriage two years after program end (Baird et al., 2019). Assessment of a 2008 Kenyan policy that reduced the cost of secondary school found it not only increased secondary school enrollment, but significantly lowered the likelihood of being married by ages 16 or 18, suggesting that catalyzing school enrollment in early adolescence resulted in delayed marriage in later adolescence (Brudevold-Newman, 2021). Similarly, free primary school education policy in Ghana led to increased grades attained, resulting in delays in marriage, particularly for girls from poor and rural households (Adu Boahen and Yamauchi, 2018). Also in Ghana, scholarships for secondary schooling led to both higher educational attainment and reduced fertility for young women after a decade (Duflo et al., 2021).

The Adolescent Girls Initiative-Kenya (AGI-K) was a randomized trial designed to test the short-and longer-term effects of a two-year multisectoral, cash plus program targeting girls 11–14 years old. One implementation site was Wajir, a pastoralist rural county where girls face many of the challenges described above (Austrian et al., 2016). The intervention increased school enrollment after two years (in 2017) and reduced child marriage and pregnancy after four years (in 2019) (Austrian et al., 2021, 2022). Effects were substantially larger for girls not enrolled in school at baseline, pointing to important heterogeneity. To understand whether these effects were sustained as the girls aged, this paper assesses the effects in 2021, six years after the start or, equivalently, four years after the end of the two-year program—when the young women were ~17–20 years old. We estimate effects on the primary outcomes of marriage, pregnancy and birth, as well as on education-related secondary outcomes. The study adds to the limited rigorous research on the longer-term effects of cash plus interventions during early adolescence on education and marriage, and does this in an impoverished, remote and socially conservative setting. It also explores the evolution and persistence of effects, including on schooling, over the period of COVID-19 school closures that occurred in between the 2019 and 2021 follow-ups.

2. Methods

2.1. Study context and design

AGI-K operated in rural Wajir, one of the poorest counties in Kenya. Wajir is semi-arid with little arable land and characterized by low population density (<30 people per square kilometer), high food insecurity and minimal infrastructure; for example, only 14% of households have electricity and less than half have access to sanitation facilities (Kenya National Bureau of Statistics, 2019). Due to the vast terrain and sparse paved road network, most villages are isolated and transportation between them is difficult. Parts of the county experience periodic insecurity related to local clan disputes and to instability in bordering Somalia. Muslim Somalis form the predominant religious and ethnic group (County Government of Wajir, 2018). Characteristics of the study sample are similar. Virtually 100% are Muslim Somalis, <5% have electricity and only 15% have a pit latrine or other improved sanitation facility, often shared with other households. In 2014, prior to the intervention, one third of girls 15–19 years old had never attended school, 23.4% had married and 17.4% had been pregnant. The total fertility rate in 2014 was 7.8, the highest in the country (Kenya National Bureau of Statistics et al., 2015).

The theory of change underlying AGI-K posits that a package of violence prevention, education, health and wealth creation interventions for girls in early adolescence (starting when they were

11–14 years old) would delay eventual fertility-related outcomes (Appendix Fig. A1). (Muthengi & Austrian, 2018, pp. 41–57) Prior to the program, median maternal age at first birth (19.4 years) in Wajir was more than a year higher than median female age at marriage (18 years), consistent with low rates of premarital sex in the Muslim Somali population. In this context, we hypothesized that the program would delay marriage which in turn would lead to later childbearing (Kenya National Bureau of Statistics et al., 2015; Muthengi & Austrian, 2018, pp. 41–57).

AGI-K intervention packages included nested combinations of four single-sector interventions implemented for two years from August 2015 to July 2017. We examined the effectiveness of three combinations of interventions, each compared with a base single-sector community-level intervention (Table 1).

The study was approved by the Population Council IRB (p661) and AMREF Ethics and Scientific Review Committee (p143-2014, p1036-2021) prior to implementation.

2.2. Randomization

We carried out prospective cluster-level randomization of the four different packages, or study arms, with the cluster defined as a rural village with a single public primary school. Eighty clusters were identified in three of the four districts in Wajir: East (28), West (20) and South (32). Random assignment to study arm was stratified by district and determined at a public meeting with local leaders in each district (Austrian et al., 2016).

2.3. Procedures and participants

Each cluster was assigned to one of the four study arms (Table 1). For approval by local authorities to carry out the research, all four study arms included the community-level violence prevention intervention ensuring that all participating communities received a direct benefit. In each cluster a locally formed key stakeholder committee discussed inequitable gender norms and factors contributing to violence against girls in their community, and then formulated and implemented an action plan funded by AGI-K to address them. Per the theory of change, this component promotes an enabling environment supporting the additional interventions layered on top of it in the different study arms, even if this mechanism cannot be directly tested given the study design. The remaining study arms sequentially nested the three other interventions. Although examining the full factorial design of intervention components would have been conceptually appealing, with 14 different combinations (apart from a control group) such an approach was infeasible both in terms of budget and the number of clusters needed to ensure adequate statistical power. It was determined that up to four study arms was possible. Consequently, in addition to the community-level violence intervention, three study arms also included an education intervention that provided cash transfers to households and school supplies to girls, conditional on enrollment and regular attendance. Two transfers of ~ $15 per transfer were made to the household head in each of three school terms per year; the first after verification of enrollment at the start of the term and the second after verified continued attendance about one month later. At the start of each term girls also received an in-kind school supplies kit valued at ~$6. Finally, the program covered school fees up to ~$7 for primary and ~$60 for secondary school per term, paid directly to the schools along with a $5 incentive for each AGI-K girl enrolled. For primary school beneficiaries the transfers represented approximately 10% of monthly household expenditures for an average household. Two study arms also had a health intervention including girls empowerment groups with ~20 girls each that met weekly and covered a specially designed health and life-skills curricula with 36 sessions delivered by a trained adult female mentor. Finally, one study arm also included a financial literacy curriculum in the group sessions, along with a home savings bank and small annual cash transfer provided as a savings incentive.

Table 1
Study arms.

Intervention Package/Study Arm	Abbreviation
Violence Prevention	V-only
Violence Prevention + Education	VE
Violence Prevention + Education + Health	VEH
Violence Prevention + Education + Health + Wealth Creation	VEHW

Program fidelity and take-up were high. All clusters completed their community action plans by 2018 at the latest, with most making improvements in village primary schools. Nearly 90% of girls in study arms VE, VEH or VEHW received at least one cash transfer (and associated school fee payments and school kits) and on average girls received 9 of 12 possible payments. On the other hand, no girls in V-only received any cash transfers. Eligible girls attended 33 empowerment meetings on average, with about three-quarters having attended at least 12. Additional measures of program fidelity and take-up, along with further program details, are shown in Appendix Fig. A2 and described elsewhere (Austrian et al., 2016, 2021).

All community members in study clusters were exposed to the violence prevention intervention and a household listing was conducted in each cluster to identify all girls 11–14 years old eligible for the other interventions implemented in the cluster. A baseline survey was conducted prior to cluster randomization (March–May 2015); it targeted for interview a random sample of 40 girls per cluster based on the listing, though some clusters had fewer girls. Longitudinal surveys targeting all girls interviewed at baseline were conducted every two years until the September–October 2021 follow-up survey we introduce in this paper (Austrian et al., 2021, 2022).

Appendix Figure C1 presents the sample flow to 2021. The 2015 baseline target sample was 2297 girls (across 80 clusters), of which 2147 (93.5%) were interviewed. For security reasons, after the baseline survey one cluster in VE was not followed. 2023 (94.2%) girls were re-interviewed in the 2021 follow-up, with success rates differing little across study arms (92.4–95.5%). We estimated the probability of re-interview in 2021 and examined whether its correlates differed by study arm. The probability of re-interview was modestly higher for girls in school at baseline, girls whose fathers had ever attended school and girls living in Wajir East (Appendix Table C1). There were few statistically significant differences in the associations between the covariates and the probability of re-interview in 2021 across study arms.

2.4. Outcomes

The three primary outcomes include binary 0/1 indicators measured in the 2021 follow-up equal to one if the girl had ever: 1) been married; 2) been pregnant; or 3) given birth. We also examined two secondary outcomes related to education, a key mechanism in the theory of change: a binary 0/1 indicator for current enrollment and the number of grades attained. Other secondary domains examined in earlier follow-ups and found not to have been substantially affected were not considered (Austrian et al., 2022). Appendix Table A1 presents the key indicators from the study protocol and Appendix F all variable definitions.

2.5. Statistical analysis

A minimum detectable effect (MDE) approach was used to conduct power analysis based on the number of potential beneficiaries and clusters that could be covered by the program and research budgets. MDEs comparing each of the VE, VEH and VEHW study arms to the V-only study arm were estimated for prevalence of first birth in 2019, the original trial endline. Setting statistical power at 80% and significance level at 5%, power analysis was conducted for two-sample proportions tests using Optimal Design Plus Software Version 3.0 for a cluster randomized trial with individual-level outcomes. Under the budgetary resource constraints, the most efficient design was 20 clusters per study arm with 32 girls per cluster. Positing a 20% loss to follow-up over four years, a target number of 40 girls per cluster for interview at baseline was established. Based on the 2014 KDHS, (Kenya National Bureau of Statistics et al., 2015) we assumed that 17.6% of the girls in V-only would have given birth by 2019, allowing detection of a 5.9 percentage point difference in birth rates (Austrian et al., 2016).

We estimated the intent-to-treat (ITT) effect of each study arm relative to the V-only study arm in 2021, six years after the start of the program or, equivalently, four years after the end of the two-year program. ITT was defined as residing at baseline in a cluster assigned to a specific study arm, irrespective of actual participation in the program or eventual 2021 residential location. Because the community-level violence prevention intervention was included in all four study arms, there was no pure control group. With this research design the estimated ITT effect for each package of interventions captures both the direct effect of the specific included interventions as well as any interactive effects among the various interventions, including with violence prevention. Because the interventions were nested, an alternative equivalent model specification is to include binary 0/1 variables for each single-sector intervention type rather than for each study arm; using that approach the resulting estimates for the education intervention are identical to those we present below for the VE study arm. We also constructed a binary 0/1 indicator combining all three study arms that had the education component (VE, VEH and VEHW) into a pooled study arm to estimate the weighted average effect of the three packages relative to V-only.

All regressions included controls for age, stratification and when available, the baseline value of the outcome measure yielding analysis of covariance (ANCOVA) models. As outlined in the study protocol, we also report results including additional extended controls—in particular baseline schooling in the primary outcome specifications—to improve precision and to control for possible initial imbalance (Austrian et al., 2016).

We accounted for multiple hypotheses testing related to evaluating three primary outcomes by: 1) grouping them into a composite summary fertility measure; and 2) recalculating statistical significance. First, for each primary outcome we calculated a z-score based on the mean and standard deviation (SD) in the V-only in 2021. We constructed an inverse covariance weighted index using the three z-scores, restandardizing to be mean 0 and SD 1 in V-only. We then estimated the same ITT model on the summary z-score. Second, we recalculated statistical significance using false discovery rates (FDR), reporting the adjusted q-values (Benjamini & Hochberg, 1995).

Education plays a central role in the theory of change and cluster average school enrollment was moderately imbalanced across study arms at baseline. Moreover, results for the 2017 and 2019 follow-up survey rounds of the trial revealed substantially heterogenous effects by baseline schooling status. Therefore, we carried out post-hoc subgroup analysis for girls not enrolled in school at baseline (baseline out-of-school subsample) and those enrolled (baseline in-school subsample), as done for related interventions in similar contexts elsewhere (Baird et al., 2011; Handa et al., 2015).

To explore potential bias from nonrandom attrition, we assessed balance on baseline characteristics across the randomized study arms for the sample re-interviewed in the 2021 follow-up. We also examined the sensitivity of the main results using inverse probability weights constructed from an analysis of attrition (Appendix C) to estimate attrition-weighted results.

Apart from accounting for multiple hypotheses testing, the same methodologies were used to examine the educational outcomes. Finally, we present selected results for the same outcomes from the earlier follow-up surveys (2017 and 2019) to examine the evolution and persistence of program effects over time.

All models were estimated using ordinary (or weighted in the case of the attrition sensitivity analyses) least squares with robust standard errors accounting for clustering at the village level. We set statistical significance at 5%. Analysis was conducted using Stata 15.1. The trial was registered retrospectively in December 2015, shortly after program implementation began, in ISCRTN registry (ISCRTN77455458).

Role of funding source

The funders had no role in study design, analysis or interpretation of the manuscript. Authors had full access to all data in the study. All authors approved the final version of the manuscript before submission.

3. Results

Table 2, Panel A presents the 2015 baseline means for the full sample of girls re-interviewed in 2021 by study arm. The girls were 11.9 years old on average when AGI-K began in 2015. Three-quarters lived with their parents, few of whom had any formal education. About three-quarters of the girls were enrolled in school, and nearly all of those not currently enrolled had never attended school. Consequently, grade attainment was low, 2.9 years. Less than 1% had ever been married, pregnant or given birth (Appendix Table B1). The full 2021 follow-up sample was balanced across study arms on most baseline characteristics, though somewhat different for pre-intervention school enrollment, which was highest in V-only (78.9%) and lowest in VE (68.5%). The baseline out-of-school (Table 2, Panel B) and baseline in-school (Appendix Table B2) subsamples were similarly well balanced.

Table 3, Panel A presents the 2021 follow-up survey means for the full sample by study arm. In 2021, the girls were just over 18 years old on average. Focusing on the V-only study arm that did not receive cash transfers, although less common than at baseline, two-thirds still lived with their parents. Average grades attained had increased more than four years, reflected in a near doubling of literacy; enrollment at these older ages, however, was lower than at baseline but still above 50%. About one-quarter in V-only had married, with three-quarters of those having been pregnant. Education also improved for the subsample of baseline out-of-school girls (Panel B) in V-only between 2015 and 2021, though not by as much. By 2021 two-thirds had married and half had been pregnant. Means for the baseline in-school subsample are reported in Appendix B.

Table 4, Panel A presents the full-sample ITT effects on primary outcomes after six years, i.e., four years after the program ended. In 2021 in the V-only study arm, 24.9% of girls had ever married, 18.1% had ever been pregnant and 15.6% had given birth. All point estimates in columns 2–5 are negative, consistent with reductions in the primary outcomes, though none are statistically significant in the full sample. Estimates with extended controls (including baseline school enrollment, columns 6–9) are larger in magnitude and ever marriage and pregnancy were significantly lower in the VE and pooled study arms, and ever birth lower in VE. For example, the pooled estimate indicates a weighted average 6.7 percentage point reduction in marriage compared to V-only and 4.5 percentage point reduction in pregnancy. The pooled estimate indicates a reduction of 0.13 SD in the fertility summary measure. Estimated effects are larger for all outcomes in the VE study arm (or, equivalently, for the education intervention when the model is specified by intervention type rather than by study arm). Significant results are robust to corrections for multiple hypotheses testing (Table 4) and to reweighting for attrition bias (Appendix Table E1).

For the baseline out-of-school subsample marriage, pregnancy and birth rates in 2021 in V-only are substantially higher than for the full sample and estimates of the ITT effects four years after the program ended considerably larger (Table 4, Panel B). In 2021 in V-only, 68.0% of girls had married, 52.0% had been pregnant and 47.0% had given birth. Similar to estimates without extended controls, estimates with extended controls (columns 6–9) indicate that ever marriage and pregnancy were substantially lower in the VE and pooled study arms, and also ever birth in VE. For example, the pooled estimate indicates a

Table 2
Baseline means for 2021 follow-up full and baseline out-of-school samples, by study arm.

	(1) V-Only	(2) VE	(3) VEH	(4) VEHW	(5) p-value	(6) VE, VEH & VEHW
Panel A: Full sample [n = 2023]						
Age, mean (sd)	11.9 (1.3)	12.0 (1.3)	11.8 (1.2)†	11.9 (1.3)	0.070	11.9 (1.3)
Cognitive score (0–16), mean (sd) [n = 1986]	5.2 (2.9)	4.9 (3.0)	4.9 (3.2)	5.3 (3.0)	0.413	5.1 (3.1)
Lives with both parents, % [n = 2000]	74.4	73.8	77.0	74.0	0.696	74.9
Mother ever attended school, % [n = 1999]	1.3	1.4	0.6	0.4	0.240	0.8
Father ever attended school, % [n = 1997]	5.8	2.8*	3.3	3.5	0.243	3.2†
Grade attainment, mean (sd)	2.9 (2.2)	2.7 (2.3)	2.7 (2.2)	2.5 (2.1)	0.618	2.6 (2.2)
Enrolled in current school year, %	78.9	68.5†	76.4	71.9	0.230	72.2
Literate in Swahili and English, % [n = 1986]	38.5	32.7	38.7	33.4	0.454	34.9
Household wealth quintile (1–5), mean (sd) [n = 1995]	3.0 (1.5)	2.7 (1.5)	2.9 (1.5)	2.9 (1.4)	0.793	2.8 (1.5)
Sample by arm when n = 2023	474	511	512	526		1549
Panel B: Baseline out-of-school sample [n = 530, 26.2% of full sample]						
Age, mean (sd)	12.0 (1.3)	11.9 (1.3)	11.7 (1.3)*	12.1 (1.3)	0.027	11.9 (1.3)
Cognitive score (0–16), mean (sd) [n = 522]	4.1 (2.7)	4.2 (2.9)	3.1 (2.8)*	4.4 (3.4)	0.085	4.0 (3.1)
Lives with both parents, % [n = 524]	76.8	75.0	83.2	74.7	0.358	77.2
Mother ever attended school, % [n = 523]	0.0	0.6	0.0	0.0	0.716	0.2
Father ever attended school, % [n = 524]	5.1	2.5	1.7	3.4	0.615	2.6
Grade attainment, mean (sd)	0.4 (1.0)	0.3 (0.9) **	0.1 (0.5)	0.3 (1.0)	0.001	0.3 (0.8)
Literate in Swahili and English, % [n = 522]	1.0	0.6	0.0	0.7	0.392	0.5
Household wealth quintile (1–5), mean (sd) [n = 523]	2.7 (1.6)	2.7 (1.6)	3.1 (1.5)	2.7 (1.6)	0.608	2.8 (1.6)
Sample by arm when n = 530	100	161	121	148		430

Notes: N = 2023 (unless otherwise indicated) re-interviewed at 2021 follow-up. sd = standard deviation. Asterisks in columns 2–4 indicate statistically different from V-only. P-values in column 5 are from an F test for joint differences across study arms. Column 6 reports the average for VE, VEH and VEHW study arms combined and asterisks indicate statistically different from V-only. All statistical tests are based on regressions that control for district per the stratified design with robust standard errors calculated accounting for clustering at the village level. Baseline means for in-school subsample re-interviewed in 2021, and baseline means for the full, out-of-school and in-school baseline subsamples in 2015 are shown in Appendix B. $^{**}p < 0.01$, $^{*}p < 0.05$, †$p < 0.1$.

Table 3
Means for 2021 follow-up full and baseline out-of-school samples, by study arm.

	(1) V-Only	(2) VE	(3) VEH	(4) VEHW	(5) p-value	(6) VE, VEH & VEHW
Panel A: Full sample [n = 2023]						
Age, mean (sd)	18.1 (1.3)	18.2 (1.3)	18.0 (1.2)*	18.1 (1.3)	0.032	18.1 (1.3)
Lives with both parents, % [n = 1992]	67.9	66.8	64.4	68.8	0.640	66.7
Grade attainment, mean (sd) [n = 2021]	7.0 (3.9)	7.1 (3.5)	7.3 (3.7)	6.9 (3.6)	0.936	7.1 (3.6)
Enrolled in current school year, % [n = 2021]	56.9	60.4	62.5	62.2	0.694	61.7
Literate in Swahili and English, % [n = 2022]	66.4	67.9	68.2	69.8	0.938	68.6
Household wealth quintile (1–5), mean (sd) [n = 2019]	3.3 (1.0)	3.2 (1.1)	3.6 (1.0)	3.2 (1.1)	0.147	3.3 (1.1)
Ever married, % [n = 2022]	24.9	19.4	19.9	22.8	0.506	20.7
Ever pregnant, %	18.1	15.9	14.3	16.5	0.758	15.6
Ever given birth, %	15.6	13.5	12.7	15.0	0.761	13.8
Sample by arm when n = 2023	474	511	512	526		1549
Panel B: Baseline out-of-school sample [n = 530, 26.2% of full sample]						
Age, mean (sd)	18.2 (1.2)	18.1 (1.2)	17.8 (1.3)*	18.3 (1.3)	0.008	18.1 (1.3)
Lives with both parents, % [n = 524]	63.0	65.2	55.0	58.9	0.419	60.1
Grade attainment, mean (sd) [n = 528]	1.2 (2.2)	3.7 (2.8)**	2.1 (2.7)†	2.8 (2.9)**	0.000	3.0 (2.9)**
Enrolled in current school year, % [n = 528]	7.1	46.9**	29.8**	29.1**	0.000	35.9**
Literate in Swahili and English, %	8.0	41.6**	19.0	26.4**	0.000	30.0**
Household wealth quintile (1–5), mean (sd)	3.5 (1.0)	3.1 (1.1)*	3.4 (1.0)	3.1 (1.0)*	0.046	3.2 (1.0)
Ever married, %	68.0	34.8**	61.2	52.7†	0.000	48.4**
Ever pregnant, %	52.0	27.3**	43.0	39.2	0.006	35.8*
Ever given birth, %	47.0*	25.5	38.8	37.8	0.017	33.5†
Sample by arm when n = 530	100	161	121	148		430

Notes: N = 2023 (unless otherwise indicated) re-interviewed at 2021 follow-up. sd = standard deviation. Asterisks in columns 2–4 indicate statistically different from V-only. P-values in column 5 are from an F test for joint differences across study arms. Column 6 reports the average for VE, VEH and VEHW study arms combined, and asterisks indicate statistically different from V-only. All statistical tests are based on regressions that control for district per the stratified design with robust standard errors calculated accounting for clustering at the village level. Means for 2021 follow-up for the baseline in-school subsample are shown in Appendix B. $^{**}p < 0.01$, $^{*}p < 0.05$, †$p < 0.1$.

weighted average 18.2 percentage point reduction in marriage compared to V-only and 15.1 percentage point reduction in pregnancy. The pooled estimate indicates a reduction of 0.36 SD in the fertility summary measure. Estimated effects are 50% larger or more for all outcomes for the VE study arm. For the baseline in-school subsample on the other hand, marriage, pregnancy and birth rates in 2021 in V-only are lower than for the full sample and estimates of the ITT effects smaller

Table 4
Estimated intent-to-treat effects on primary outcomes after six years (at 2021 follow-up), by study arm.

	(1) V-Only 2021 Mean	(2) VE Estimate	(3) VEH Estimate	(4) VEHW Estimate	(5) VE-VEH-VEHW Pooled Estimate	(6) VE Estimate: Extended Controls	(7) VEH Estimate: Extended Controls	(8) VEHW Estimate: Extended Controls	(9) Pooled Estimate: Extended Controls
Panel A: Full sample [n = 2023] rowhead									
Ever married (=1) [n = 2022]	0.249	−0.057	−0.035	−0.016	−0.036	−0.104**	−0.052	−0.046	−0.067**
95% CI		[-0.13, 0.02]	[-0.14, 0.07]	[-0.11, 0.07]	[-0.11, 0.04]	[-0.16, −0.05]	[-0.12, 0.01]	[-0.11, 0.01]	[-0.12, −0.02]
FDR adjusted q-value		0.346	0.654	0.991	0.602	0.002	0.178	0.319	0.030
Ever pregnant (=1) [n = 2023]	0.181	−0.029	−0.026	−0.008	−0.021	−0.062*	−0.039†	−0.033	−0.045*
95% CI		[-0.09, 0.03]	[-0.10, 0.05]	[-0.08, 0.06]	[-0.08, 0.04]	[-0.11, −0.01]	[-0.08, 0.01]	[-0.09, 0.02]	[-0.09, 0.00]
FDR adjusted q-value		0.346	0.654	0.991	0.602	0.017	0.178	0.319	0.064
Ever given birth (=1) [n = 2023]	0.156	−0.026	−0.015	0.000	−0.014	−0.057*	−0.025	−0.024	−0.035†
95% CI		[-0.08, 0.03]	[-0.08, 0.05]	[-0.06, 0.06]	[-0.07, 0.04]	[-0.10, −0.01]	[-0.07, 0.02]	[-0.07, 0.03]	[-0.07, 0.00]
FDR adjusted q-value		0.346	0.654	0.991	0.602	0.017	0.230	0.340	0.081
Fertility outcomes summary index z-score [n = 2023]	0.000	−0.106	−0.065	−0.015	−0.062	−0.209**	−0.102	−0.089	−0.133*
95% CI		[-0.27, 0.06]	[-0.28, 0.15]	[-0.21, 0.18]	[-0.22, 0.10]	[-0.34, −0.08]	[-0.23, 0.02]	[-0.23, 0.05]	[-0.24, −0.02]
Panel B: Baseline out-of-school sample [n = 530, 26.2% of full sample] rowhead									
Ever married (=1) [n = 530]	0.680	−0.307**	−0.028	−0.157*	−0.178**	−0.308**	−0.026	−0.159*	−0.182**
95% CI		[-0.44, −0.18]	[-0.22, 0.16]	[-0.31, −0.01]	[-0.30, −0.05]	[-0.43, −0.18]	[-0.22, 0.16]	[-0.30, −0.02]	[-0.30, −0.06]
FDR adjusted q-value		0.001	0.766	0.123	0.019	0.001	0.781	0.092	0.011
P-value test diff in-school		0.000	0.745	0.056	0.018	0.000	0.711	0.046	0.012
Ever pregnant (=1) [n = 530]	0.520	−0.225**	−0.046	−0.131	−0.143*	−0.230**	−0.055	−0.136	−0.151*
95% CI		[-0.37, −0.08]	[-0.20, 0.11]	[-0.29, 0.03]	[-0.28, 0.00]	[-0.37, −0.09]	[-0.21, 0.10]	[-0.30, 0.03]	[-0.29, −0.01]
FDR adjusted q-value		0.004	0.766	0.168	0.069	0.004	0.781	0.154	0.056
P-value test diff in-school		0.002	0.853	0.129	0.068	0.002	0.792	0.123	0.060
Ever given birth (=1) [n = 530]	0.470	−0.188*	−0.033	−0.095	−0.113	−0.194**	−0.040	−0.101	−0.121†
95% CI		[-0.33, −0.04]	[-0.19, 0.12]	[-0.26, 0.07]	[-0.26, 0.03]	[-0.34, −0.05]	[-0.19, 0.11]	[-0.27, 0.06]	[-0.27, 0.02]
FDR adjusted q-value		0.011	0.766	0.254	0.119	0.010	0.781	0.228	0.100
P-value test diff in-school		0.014	0.878	0.284	0.159	0.014	0.828	0.260	0.142
Fertility outcomes summary index z-score [n = 530]	0.000	−0.582**	−0.088	−0.295†	−0.346*	−0.590**	−0.093	−0.305†	−0.360*
95% CI		[-0.86, −0.31]	[-0.43, 0.25]	[-0.62, 0.03]	[-0.62, −0.07]	[-0.87, −0.31]	[-0.43, 0.25]	[-0.63, 0.02]	[-0.63, −0.09]
P-value test diff in-school		0.001	0.802	0.131	0.054	0.000	0.792	0.120	0.048

Notes: The 2021 follow-up was six years after the start or, equivalently, four years after the end of the program. Column 1 reports means for the V-only study arm in the 2021 follow-up and columns 2–4 the estimated ITT effects for each study arm relative to V-only. Column 5 pools all three study arms with education into a single treatment variable. Columns 6–9 report estimated ITT effects with extended controls. Numbers in square brackets indicate 95% confidence intervals (CI). Regressions estimated with robust standard errors clustered at the village level and include controls for district per the stratified design, age and outcome variable at baseline. Extended controls regressions additionally include baseline measures of in school, grade attainment, cognitive score, father ever attended school, co-residence with both parents, household wealth quintile and whether any missing baseline covariates were imputed (with the cluster median). P-value test diff in-school reports the p-value of the interaction between out-of-school at baseline and study arm using full sample. False discovery rate adjusted q-values are for the set of three primary outcomes in each sample. Results for the baseline in-school sample are shown in Appendix D. $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$.

and mostly insignificant (Appendix Table D1).

Examination of secondary outcomes related to schooling for the full sample (Table 5) reveals that the VE and pooled study arms had higher enrollment in 2021 by 7–10 percentage points (compared to 56.9% enrollment in V-only) and that each study arm (and the pooled study arm) had higher grade attainment by nearly one-half a grade (compared to an average of 7.0 in V-only). The effects on enrollment, moreover, suggest that completed grade differentials could continue to widen. Significant results are robust to reweighting for attrition bias (Appendix Table E2).

Effect sizes for the baseline out-of-school subsample were 2–5 times larger than in the full sample, and nearly all significant. For example, the pooled estimate indicates a weighted average 27.9 percentage point increase in current enrollment (compared to 7.1% enrollment in V-only) and an increase of 1.8 grades (compared to an average of 1.2 in V-only). As for the primary outcomes, effect sizes were largest for the VE study

Table 5
Estimated intent-to-treat effects on education outcomes after six years (at 2021 follow-up), by study arm.

	(1) V-Only 2021 Mean	(2) VE Estimate	(3) VEH Estimate	(4) VEHW Estimate	(5) VE-VEH-VEHW Pooled Estimate	(6) VE Estimate: Extended Controls	(7) VEH Estimate: Extended Controls	(8) VEHW Estimate: Extended Controls	(9) Pooled Estimate: Extended Controls
Panel A: Full sample [n = 2023]									
Enrolled in current school year [n = 2021]	0.569	0.089*	0.049	0.072†	0.070*	0.098*	0.049	0.065	0.070*
95% CI		[0.01, 0.16]	[-0.02, 0.12]	[0.00, 0.15]	[0.01, 0.13]	[0.02, 0.17]	[-0.02, 0.12]	[-0.01, 0.14]	[0.01, 0.13]
Grade attainment [n = 2021]	7.019	0.465*	0.467*	0.394*	0.442**	0.658**	0.492**	0.440*	0.529**
95% CI		[0.10, 0.83]	[0.03, 0.90]	[0.01, 0.78]	[0.14, 0.75]	[0.35, 0.97]	[0.13, 0.85]	[0.11, 0.78]	[0.29, 0.77]
Sample size by arm	473	510	512	526	1548	510	512	526	1548
Panel B: Baseline out-of-school sample [n = 530, 26.2% of full sample]									
Enrolled in current school year [n = 528]	0.071	0.389**	0.188*	0.225**	0.277**	0.387**	0.191*	0.227**	0.279**
95% CI		[0.29, 0.49]	[0.03, 0.34]	[0.10, 0.35]	[0.18, 0.37]	[0.29, 0.49]	[0.04, 0.34]	[0.11, 0.35]	[0.19, 0.37]
P-value test diff in-school		0.000	0.055	0.001	0.000	0.000	0.033	0.000	0.000
Grade attainment [n = 528]	1.202	2.550**	0.970*	1.693**	1.822**	2.532**	0.891†	1.694**	1.809**
95% CI		[1.94, 3.16]	[0.05, 1.89]	[0.88, 2.50]	[1.21, 2.43]	[1.93, 3.13]	[-0.02, 1.80]	[0.91, 2.47]	[1.22, 2.40]
P-value test diff in-school		0.000	0.195	0.000	0.000	0.000	0.252	0.000	0.000
Sample size by arm	99	160	121	148	429	160	121	148	429

Notes: The 2021 follow-up was six years after the start or, equivalently, four years after the end of the program. Column 1 reports means for the V-only study arm in the 2021 follow-up and columns 2–4 the estimated ITT effects for each study arm relative to V-only. Column 5 pools all three study arms with education into a single treatment variable. Columns 6–9 report estimated ITT effects with extended controls. Numbers in square brackets indicate 95% confidence intervals (CI). Regressions estimated with robust standard errors clustered at the village level and include controls for district per the stratified design, age and outcome variable at baseline. Extended controls regressions additionally include baseline measures of in school, grade attainment, cognitive score, father ever attended school, co-residence with both parents, household wealth quintile and whether any missing baseline covariates were imputed (with the cluster median). P-value test diff in-school reports the p-value of the interaction between being out-of-school at baseline and study arm using full sample. Results for the baseline in-school sample are shown in Appendix D. **$p < 0.01$, *$p < 0.05$, †$p < 0.1$.

arm. There were only minimal effects, however, on the schooling indicators for the baseline in-school subsample (Appendix Table D2).

Fig. 1 summarizes the estimated ITT effects with extended controls for the full and baseline out-of-school samples in 2017, 2019 and 2021. To put the spotlight on the education CCT channel, estimates reported are for the VE study arm. Effects on current enrollment were largest just at the end of the two-year program in 2017, after which they declined by about 50% but remained significant in both 2019 and 2021 (Fig. 1, Panel A). Consistent with the persistent positive effects on enrollment, grades attained widened with each successive follow-up. In contrast to enrollment, the negative effects on the fertility-related outcomes were insignificant in 2017 when girls were still only 12–15 years old but followed the education effects with a lag as the girls aged, becoming more negative and significant in 2019 and again in 2021 (Fig. 1, Panel B).

4. Discussion

Six years after the start of the two-year AGI-K program, when the girls had reached ~17–20 years, the program had lowered early marriage by 6.7, pregnancy by 4.5 and birth by 3.5 percentage points (marginally significant), an approximate one-quarter reduction relative to V-only for each indicator. Measured four years after the program had ended, these results represent longer-term post-intervention program effects.

The effects for the full sample summarized above, however, mask substantial heterogeneity by baseline schooling status. There were much larger reductions for those not enrolled, for whom marriage was 30.8, pregnancy 23.0 and births 19.4 percentage points lower in the VE study arm, each representing a 40% or more reduction relative to V-only. There was almost no effect on girls enrolled in school at baseline. The differences in estimated ITT effects across the subsamples were not the result of differential program take-up rates, which were actually 25 percentage points lower for girls not enrolled at baseline.

Although there is minimal evidence on longer-term effects for comparable multisectoral programs, we can compare with simpler cash transfer programs in sub-Saharan Africa; effect sizes for AGI-K on fertility-related outcomes are as large or larger, and follow similar patterns related to baseline schooling. Effect sizes might be similar if the cash transfer programs mainly influence education and education is a principal mechanism underlying changes in fertility-related outcomes in both types of programs. For example, the conditional cash transfer arm of the one-year cash transfer program in Malawi, although the latter targeted an older and much wider age range, 13–22 years old. Follow-up two years after the end of that program demonstrated heterogenous impacts. For girls enrolled in school at baseline there were no sustained effects but for those not enrolled at baseline effects were substantial, with a reduction in marriage of 10.7 (27%), in pregnancy of 4.0 (8%) and in birth rates of 14.7 percentage points (29%) (Baird et al., 2019). For a national program in Kenya with on-going unconditional transfers (Cash Transfer for Orphans and Children), females measured when

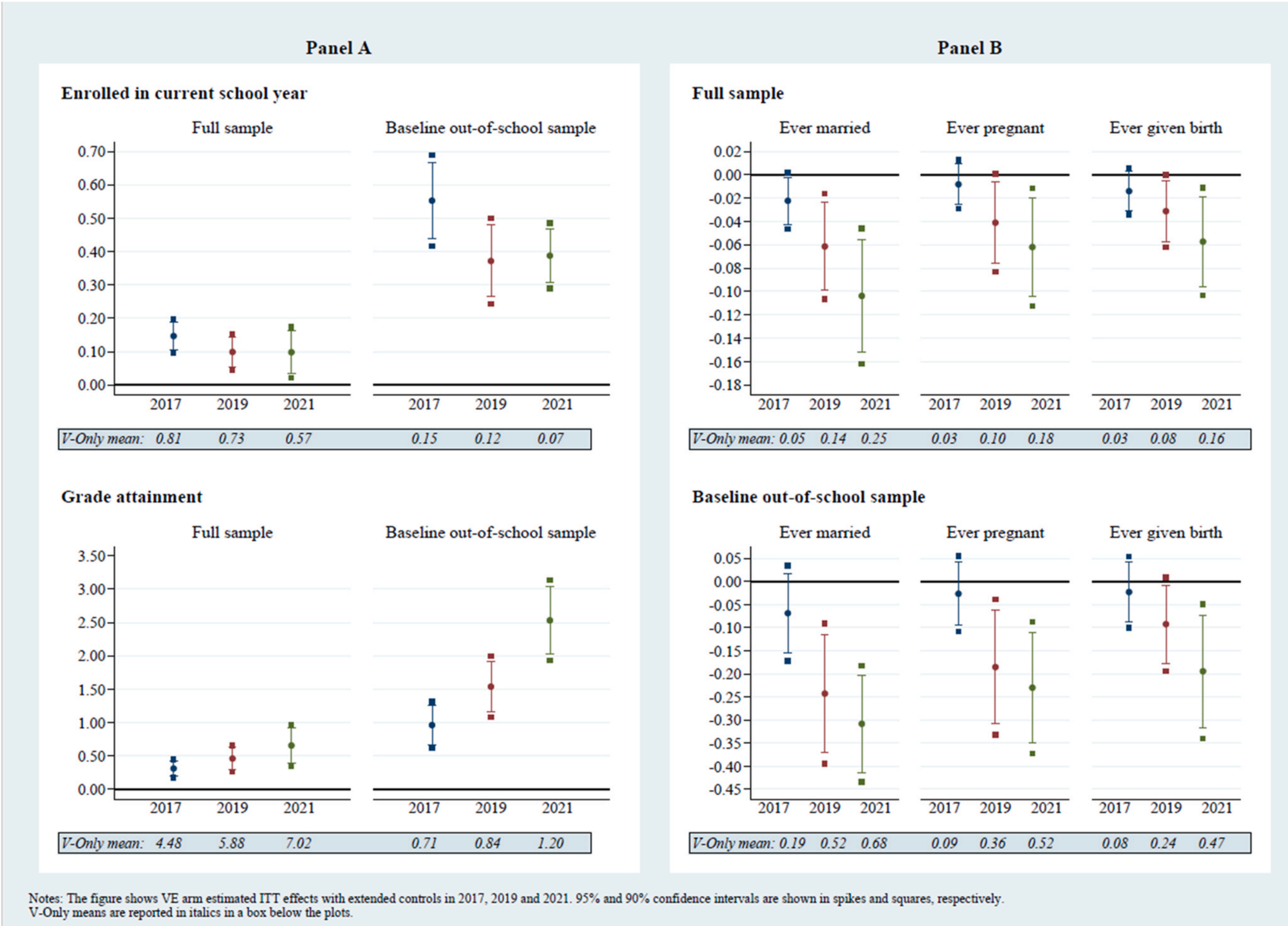

Fig. 1. Estimated ITT effects on education and fertility outcomes over time.

12–24 years old who had been eligible to receive the unconditional cash transfer for four years experienced a 4.9 percentage point (34%) reduction in pregnancy but no significant reduction in marriage (Handa et al., 2015).

AGI-K also had sustained effects on the education-related secondary outcomes, again largest for VE in which for the baseline out-of-school subsample current enrollment was five times and attained grades two times higher than for the full sample. Total effects on attained grades increased over time, resulting from persistent effects on enrollment. The effects on education, also observed in 2017 prior to any significant reductions in fertility-related measures, point to schooling as an important driver for delaying marriage and fertility with a lag in the Wajir context.

The most recent follow-up was funded and planned prior to the COVID-19 outbreak, with its main wave occurring between the 2019 and 2021 follow-up surveys. Projections made at the outset of the pandemic were that prolonged school closures—which in Kenya lasted 10 months, among the longest in Africa—would increase child marriage, particularly in settings like Wajir where the practice was already common (Yukich et al., 2021). However, whether the pandemic had substantial effects on marriage or other fertility-related measures is unclear. One non-experimental longitudinal cohort comparison study done in rural schools in Western Kenya found that lockdowns appear to have led to higher dropout as well as increased pregnancy for girls in the final year of secondary school (Zulaika et al., 2022). A parallel longitudinal cohort comparison using the V-only sample, however, does not show the same pattern for Wajir. For girls 15–16 years old in 2017, increases in marriage, pregnancy and birth rates over the subsequent two years (prior to and therefore unaffected by the pandemic beginning in 2020) were modestly lower than the two-year increases for girls 15–16 years old in 2019 (affected by the pandemic). Regardless, in the VE study arm, the magnitudes of the estimated effects on marriage and other outcomes in 2019, prior to the pandemic, were maintained or even larger in 2021. If there were pandemic related increases in marriage or fertility, therefore, AGI-K may have helped mitigate some of the possible deleterious effects.

The study has several limitations. First, it did not incorporate a pure control group or implement a full factorial design that would have enabled rigorous assessment of the absolute effects of the different packages of interventions or independent assessment of the single-sector interventions on their own or in other combinations. Second, we are unable to generalize the findings beyond similar pastoralist and socially conservative settings. Third, although carried out in rural areas, the interventions targeted villages that had a public school, and therefore the results also do not immediately generalize to even more remote areas of Wajir including to villages without local schools or to entirely nomadic communities for whom such a school-linked program would probably not be as effective. The 2019 Kenyan national census indicated that one-third of 14–17-year-old girls in Wajir were in school, compared with three-quarters in the V-only sample that same year. In 2022, KDHS indicated 25.7% of girls 17–20 years old had married, 24.9% had been pregnant and 23.3% had given birth; although the marriage rate was similar to the V-only sample, the other indicators were one-third or more higher.

Finally, because AGI-K purposively targeted young girls 11–14 years old at the start, even after six years the ultimate effects of the program may not yet have fully materialized because more than a quarter are still in school. At the same time, in a setting with high marriage rates it is unlikely that there would continue to be large differentials in the percentages ever marrying or giving birth as the women age, and rates in the other study arms would eventually catch up to V-only, although we would anticipate that the mean ages of marriage and first birth would remain higher. That was the pattern observed in the Mexican CCT for women followed for 20 years and measured when 29–35 years old (Araujo & Macours, 2021). Differentials for schooling, however, would likely persist and, along with the delays in marriage during late adolescence estimated in this paper, could also lead to improvements in child health, marriage dynamics and economic wellbeing (Baird et al., 2019; Delprato et al., 2015; Efevbera et al., 2017). Further follow-up of the cohort when all have completed their schooling would enable a more complete understanding of the time patterns of impacts of AGI-K on education, as well as on marriage and fertility.

Despite the limitations, the study has significant strengths. It employed a rigorous cluster randomized design and had minimal attrition over the six-year follow-up period. Fidelity and take-up of the AGI-K program were high and there was minimal contamination, allowing ITT analysis. And, it is one of only a few studies using a longer-term, prospective longitudinal design to examine these types of interventions for difficult to reach, marginalized and socially conservative populations. Such follow-up is essential for understanding program effects since early investment in adolescents may only pay dividends years later (Blum et al., 2014).

This study shows the potential for interventions with an education component in early adolescence to delay marriage and fertility in a marginalized and socially conservative setting with low education and high rates of child and early marriage. With effects concentrated among the most at-risk girls, AGI-K demonstrated that with early intervention it was possible to shift many girls off a path to early marriage and on to one of higher education. Because girls who were not in school when the program started benefited the most, the findings raise the possibility that the cash transfer could be reconfigured and applied differentially to improve cost-effectiveness, provided such changes did not incentivize households with enrolled girls to remove them from school. Finally, the timing of the study vis-à-vis the COVID-19 related school closures also suggests that such interventions have potential to mitigate the consequences of large external shocks that increase the risk for early marriage.

Financial disclosure statement

All authors declare no financial relationships related to the manuscript.

Ethics statement

The authors confirm that this manuscript is original work not currently published elsewhere.

The authors declare no competing interests. The study was approved by the Population Council IRB (p661) and AMREF Ethics and Scientific Review Committee (p143-2014, p1036-2021) prior to implementation. The trial was registered retrospectively in December 2015, shortly after program implementation began, in ISCRTN registry (ISCRTN77455458).

CRediT authorship contribution statement

Karen Austrian: Writing – review & editing, Writing – original draft, Supervision, Project administration, Methodology, Investigation, Funding acquisition, Conceptualization. **John A. Maluccio:** Writing – review & editing, Writing – original draft, Validation, Methodology, Formal analysis, Data curation, Conceptualization. **Erica Soler-Hampejsek:** Writing – review & editing, Writing – original draft, Validation, Supervision, Methodology, Formal analysis, Conceptualization. **Eva Muluve:** Writing – review & editing, Validation, Project administration, Data curation. **Abdullahi Aden:** Writing – review & editing, Project administration. **Yohannes D. Wado:** Writing – review & editing, Investigation. **Benta Abuya:** Writing – review & editing, Investigation, Conceptualization. **Beth Kangwana:** Writing – review & editing, Investigation, Conceptualization.

Declaration of competing interest

All authors declare no conflict of interest.

Data availability

Data will be made available on request.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.ssmph.2024.101663.

References

Adu Boahen, E., & Yamauchi, C. (2018). The effect of female education on adolescent fertility and early marriage: Evidence from free compulsory universal basic education in Ghana. *Journal of African Economies, 27*(2), 227–248.

Amin, S., Saha, J., & Ahmed, J. (2018). Skills-building programs to reduce child marriage in Bangladesh: A randomized controlled trial. *Journal of Adolescent Health, 63*(3), 293–300.

Araujo, M. C., & Macours, K. (2021). *Education, income and mobility: Experimental impacts of childhood exposure to progresa after 20 years*.

Austrian, K., Muthengi, E., Mumah, J., et al. (2016). The adolescent girls initiative-Kenya (AGI-K): Study protocol. *BMC Public Health, 16*(1), 1–14.

Austrian, K., Soler-Hampejsek, E., Kangwana, B., et al. (2022). Impacts of multisectoral cash plus programs on marriage and fertility after 4 Years in pastoralist Kenya: A randomized trial. *Journal of Adolescent Health, 70*(6), 885–894.

Austrian, K., Soler-Hampejsek, E., Kangwana, B., Wado, Y. D., Abuya, B., & Maluccio, J. A. (2021). Impacts of two-year multisectoral cash plus programs on young adolescent girls' education, health and economic outcomes: Adolescent Girls Initiative-Kenya (AGI-K) randomized trial. *BMC Public Health, 21*(1), 1–23.

Baird, S., McIntosh, C., & Özler, B. (2011). Cash or condition? Evidence from a cash transfer experiment. *Quarterly Journal of Economics, 126*(4), 1709–1753.

Baird, S., McIntosh, C., & Özler, B. (2019). When the money runs out: Do cash transfers have sustained effects on human capital accumulation? *Journal of Development Economics, 140*, 169–185.

Bates, L. M., Maselko, J., & Schuler, S. R. (2007). Women's education and the timing of marriage and childbearing in the next generation: Evidence from rural Bangladesh. *Studies in Family Planning, 38*(2), 101–112.

Benjamini, Y., & Hochberg, Y. (1995). Controlling the false discovery rate: A practical and powerful approach to multiple testing. *Journal of the Royal Statistical Society: Series B, 57*(1), 289–300.

Blum, R. W., Astone, N. M., Decker, M. R., & Mouli, V. C. (2014). A conceptual framework for early adolescence: A platform for research. *International Journal of Adolescent Medicine and Health, 26*(3), 321–331.

Bongaarts, J., Mensch, B. S., & Blanc, A. K. (2017). Trends in the age at reproductive transitions in the developing world: The role of education. *Population Studies, 71*(2), 139–154.

Brudevold-Newman, A. (2021). Expanding access to secondary education: Evidence from a fee reduction and capacity expansion policy in Kenya. *Economics of Education Review, 83*, Article 102127.

Chandra-Mouli, V., & Plesons, M. (2021). A groundbreaking systematic review, but that alone is not enough to change the course of programming on child marriage prevention. *Journal of Adolescent Health, 68*(5), 833–835.

Chandra-Mouli, V., Plesons, M., Barua, A., Sreenath, P., & Mehra, S. (2018). How can collective action between government sectors to prevent child marriage be operationalized? Evidence from a post-hoc evaluation of an intervention in Jamui, Bihar and sawai Madhopur, Rajasthan in India. *Reproductive Health, 15*(1), 118.

County Government of Wajir. (2018). *Wajir county integrated development plan: 2018-2022*. Kenya: Government of Kenya.

Delprato, M., Akyeampong, K., Sabates, R., & Hernandez-Fernandez, J. (2015). On the impact of early marriage on schooling outcomes in Sub-Saharan Africa and South West Asia. *International Journal of Educational Development, 44*, 42–55.

Duflo, E., Dupas, P., & Kremer, M. (2021). *The impact of free secondary education: Experimental evidence from Ghana*. National Bureau of Economic Research.

Efevbera, Y., Bhabha, J., Farmer, P. E., & Fink, G. (2017). Girl child marriage as a risk factor for early childhood development and stunting. *Social Science & Medicine, 185*, 91–101.

Hamory, J., Baird, S., Das, S., Jones, N., Woldehanna, T., & Yadete, W. (2023). *Do layered adolescent-centric interventions improve girls' capabilities? Evidence from a mixed-methods cluster randomised controlled trial in Ethiopia*.

Handa, S., Peterman, A., Huang, C., Halpern, C., Pettifor, A., & Thirumurthy, H. (2015). Impact of the Kenya cash transfer for Orphans and vulnerable children on early pregnancy and marriage of adolescent girls. *Social Science & Medicine, 141*, 36–45.

Jones, N., Gebeyehu, Y., & Hamory-Hicks, J. (2019). Exploring the role of evolving gender norms in shaping adolescents' experiences of violence in pastoralist Afar, Ethiopia. *Victim, perpetrator, or what else?* Emerald Publishing Limited.

Kalamar, A. M., Lee-Rife, S., & Hindin, M. J. (2016). Interventions to prevent child marriage among young people in low-and middle-income countries: A systematic review of the published and gray literature. *Journal of Adolescent Health, 59*(3), S16–S21.

Kenya National Bureau of Statistics. (2019). *Kenya population and housing census: Volumes I-IV*. Nairobi, 2019.

Kenya National Bureau of Statistics, ICF International. (2015). *Kenya demographic and health survey 2014*. Calverton, Maryland: KNBS and ICF International.

KNBS and ICF. (2023). *Kenya demographic and health survey 2022. Key indicators report.* Nairobi, Kenya and Rockville, Maryland, USA.

Lowe, H., Kenny, L., Hassan, R., et al. (2022). 'If she gets married when she is young, she will give birth to many kids': A qualitative study of child marriage practices amongst nomadic pastoralist communities in Kenya. *Culture, Health and Sexuality, 24*(7), 886–901.

Malhotra, A., & Elnakib, S. (2021). 20 years of the evidence base on what works to prevent child marriage: A systematic review. *Journal of Adolescent Health, 68*(5), 847–862.

Millán, T. M., Macours, K., Maluccio, J. A., & Tejerina, L. (2020). Experimental long-term effects of early-childhood and school-age exposure to a conditional cash transfer program. *Journal of Development Economics, 143*, Article 102385.

Muthengi, E., & Austrian, K. (2018). The case for a multi-sectoral approach to preventing child marriage and early childbearing in sub-saharan Africa. *Global perspectives on women's sexual and reproductive health across the lifecourse*. Springer.

Prakash, R., Beattie, T. S., Javalkar, P., et al. (2019). The samata intervention to increase secondary school completion and reduce child marriage among adolescent girls: Results from a cluster-randomised control trial in India. *Journal of global health, 9*(1).

Psaki, S. R., Melnikas, A. J., Haque, E., et al. (2021). What are the drivers of child marriage? A conceptual framework to guide policies and programs. *Journal of Adolescent Health, 69*(6), S13–S22.

United Nations Children's Fund. (2021). *Towards ending child marriage: Global trends and profiles of progress*. New York.

Yukich, J., Worges, M., Gage, A. J., et al. (2021). Projecting the impact of the COVID-19 pandemic on child marriage. *Journal of Adolescent Health, 69*(6), S23–S30.

Zulaika, G., Bulbarelli, M., Nyothach, E., et al. (2022). Impact of COVID-19 lockdowns on adolescent pregnancy and school dropout among secondary schoolgirls in Kenya. *BMJ Global Health, 7*(1), Article e007666.