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The impact of a randomized cash transfer intervention on mortality of adult household members in rural South Africa, 2011–2022

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ABSTRACT

Background: Increasing socioeconomic resources through cash transfer payments could help promote healthy longevity. However, research in this area is limited due to endogeneity in cash transfer exposures and limited geographic representation.

Methods: We leveraged the HPTN 068 randomized cash transfer trial, conducted from 2011 to 2015 in a rural setting in South Africa. We assessed long-term mortality follow-up (until March 2022) on older adult members (n = 3568) of households enrolled in the trial from the complete Agincourt Health and socio-Demographic Surveillance System census of the underlying source population. The trial intervention was a monthly cash payment of 300 Rand conditional on school enrollment of index young women. The payments were split between the young woman (1/3) and their caregiver (2/3). Young women and their households were randomized 1:1 to intervention vs. control. We used Cox PH models to compare mortality rates in older adults living in intervention vs. control households.

Findings: The cash transfer intervention did not significantly impact mortality in the full sample [HR (95% CI): 0.94 (0.80, 1.10)]. However, we observed strong protective effects of the cash transfer intervention among those with above-median household assets [HR (95% CI): 0.66 (0.50, 0.86)] and higher educational attainment [HR (95% CI): 0.37 (0.15, 0.93)].

Interpretation: Our findings indicate that short-term cash transfers can lead to reduced mortality in certain subgroups of older adults with higher baseline socioeconomic status. Future work should focus on understanding the optimal timing, structure, and targets to maximize the benefits of cash transfer programs in promoting healthy aging and longevity.

1. Introduction

Although global life expectancy has increased by more than six years since 2000 (World Health Organization, 2020), high-income countries have generally reached much higher life expectancies than their low- and middle-income country (LMIC) counterparts (Jetter et al., 2019; Preston, 1975). For example, in 2019 the United States had an estimated overall life expectancy at 78.5 years, while in South Africa, it was over a decade younger at 65.3 years (World Health Organization, 2020). At the same time, LMIC settings like those in sub-Saharan Africa are the very

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areas where aging populations are rapidly growing. The population of adults over 65 living in African countries is projected to more than triple from 2019 to 2050 (United Nations Department of Economic and Social Affairs Population Division, 2019). The collision of these aging population dynamics with underlying LMIC environments that pose challenges to healthy longevity create the conditions for public health crises. There is an urgent need to address disparities in life expectancies between high- and low-income settings, and to identify interventions that can prolong healthy lives and promote healthy aging globally.

Cash transfer interventions could plausibly help to reach this goal (Freeman et al., 2020). Cash transfers provide supplemental income to recipients through direct monetary payments. As low socioeconomic status (SES) is consistently linked to increased mortality in both high- and low-income settings (Bosma et al., 2001; Chetty et al., 2016; Kabudula et al., 2017a; Kinge et al., 2019; Steenland et al., 2004a, 2004b), increasing SES through cash transfer payments could help promote healthy longevity (Fig. 1) (Forde et al., 2012b; Sun et al., 2021). Cash transfers increase healthcare access and utilization (Ranganathan and Lagarde, 2012; Salinas-Rodríguez and Manrique-Espinoza, 2013); increase access to healthy food and clean water leading to improved nutritional status and food/water security; and increase investments in household infrastructure, such as quality sanitation, roofing, and kitchen stoves (Sun et al., 2021). These established cash transfer benefits provide plausible pathways to improved health and long-term survival. However, much of the current evidence base for cash transfer interventions is drawn from programs designed to improve child health and well-being (e.g. Brazil's *Bolsa Familia*, Mexico's *Progresa*, and South Africa's Child Support Grant). It is less clear how much of these benefits 'trickle up' to reach other adult household members.

Although much of the prior evidence supports pathways to a protective effect of cash transfers on survival, there are plausible pathways in the opposite direction as well. It is possible that increased income could facilitate access to exposures that negatively impact survival, such as high fat and calorically dense foods (Fernald et al., 2008; Forde et al., 2012a; Leroy et al., 2013), alcohol, and tobacco. However, a growing number of studies have evaluated outcomes in this area, and they tend to agree that the concern around cash transfers leading to increased consumption of these 'temptation goods' is not backed by evidence (Evans and Popova, 2014; Haushofer and Shapiro, 2013).

The existing evidence base for the relationship between cash transfer interventions and mortality is mixed and limited in scope. The bulk of the evidence that exists is weighted towards a protective effect. Findings from Latin America show significant reductions in adult mortality attributable to national cash transfer programs (Barham and Rowberry, 2013; Pescarini et al., 2020). However, preliminary evidence from Mexico evaluating a pension program (70 y Más) found a 5% increase in older age mortality attributable to the program (Feeney, 2017). A more robust set of evidence shows protective impacts of cash transfers on maternal, infant, and child mortality outcomes (Okeke and Abubakar, 2020; Ramos et al., 2021; Rasella et al., 2021), though these effects may be achieved through different pathways (e.g. childhood vaccinations, prenatal care) than more general adult mortality outcomes.

There are clear gaps in the current evidence base. Evaluating the causal nature of the relationship between cash transfers and mortality is challenging due to the inherent endogeneity of many cash transfer exposures, as few truly randomized cash transfer interventions exist, and due to the long-term follow-up required to observe longitudinal trends in mortality outcomes. The literature is also comprised of studies exclusively from Latin America, leaving a large research gap for other LMIC settings, especially those in sub-Saharan Africa. Finally, there is a growing body of evidence suggesting that cash transfer interventions have heterogeneous effects on health across population subgroups (e.g., sex, SES, education) (Cooper et al., 2020). It is important to formally incorporate subgroup analyses to be able to make policy recommendations about how to best design and target cash transfer programs to promote healthy longevity.

In this study, we leverage a randomized cash transfer intervention (HPTN 068), which was administered in rural South African communities from 2011 to 2015, to evaluate its association with long-term mortality outcomes of adults aged 40+ living in the same households as trial participants. We augment this unique randomized cash transfer intervention with high-quality longitudinal mortality data through March 2022 from the Agincourt Health and socio-Demographic Surveillance System (HDSS), the sampling frame underpinning the trial. Using these merged datasets, we estimate the causal effect of the cash transfer intervention on adult mortality, evaluate time trends in these effects, explore whether effects differed by duration and nature (direct vs. spillover) of transfer receipt, and evaluate whether effects differed across key population subgroups, including those defined by age, sex, SES, and education.

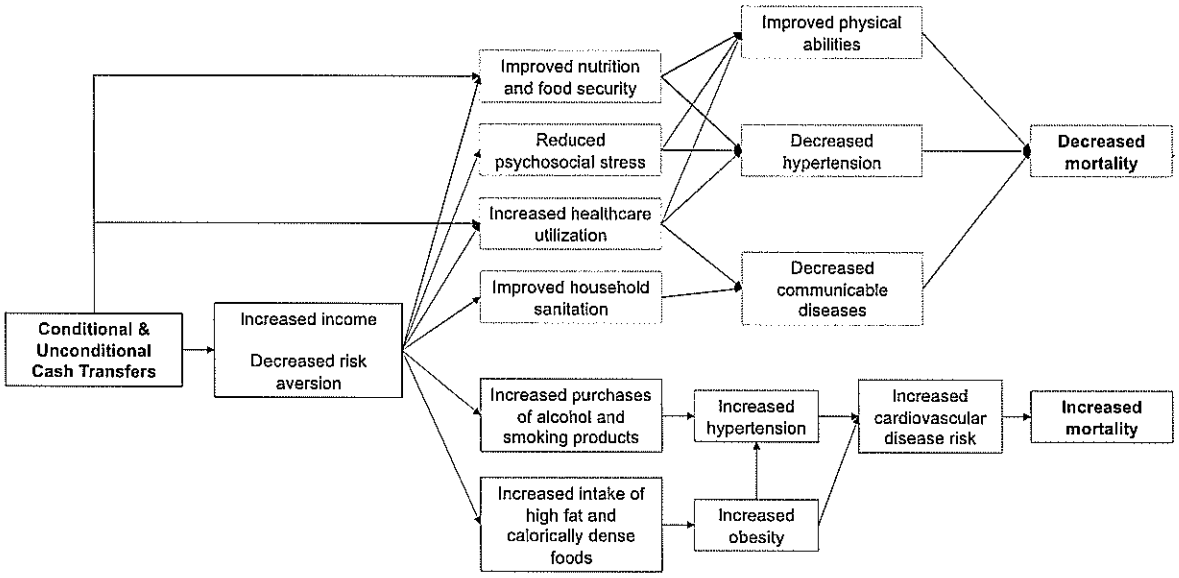

Fig. 1. Conceptual model outlining hypothesized pathways between cash transfer intervention and adult mortality.

2. Methods

2.1. Study setting and data sources

Data for this study are from two major linked data sources: the Agincourt HDSS (Kahn et al., 2012) and the HIV Prevention Trial Network (HPTN) 068 study (Pettifor et al., 2016a, 2016b). HPTN 068 is the source of the randomized cash transfer exposure for this study, while the Agincourt HDSS is the source for the adult mortality outcome. The Agincourt HDSS is a surveillance system, established in 1992, in the rural Mpumalanga province of northeastern South Africa, near the border with Mozambique. The Agincourt HDSS regularly collects vital statistics, health, and sociodemographic data on the full community cohort of ~117,000 people living within a geographically well-defined area covering 31 villages in the rural Agincourt sub-district. Vital events data (births, deaths, migrations) were collected during annual household visits until 2020. Since 2020, the data collection has expanded to include one household visit and two telephone surveys per year. In these visits, trained fieldworkers conduct interviews and update information for all members of the household. Other data collection modules (e.g., SES, employment, education) are collected through modules embedded within the household visit interview, but at less frequent intervals.

The Agincourt HDSS covers a rural, Black South African population in an area of the country to which Black South Africans were forcibly moved to during the Apartheid period (1948–1994) of legislated racial segregation (Christie and Gaganakis, 1989). The area is characterized by a history of poor-quality education and high unemployment. As with the broader South African population, residents in the Agincourt HDSS study have access to a broad social safety net. Two of the largest national cash transfer programs are designed to offset the costs of raising children to age 18 [Child Support Grant (Triegaardt, 2005)], and to support older adults age 60 and above [Old Age Pension (Case and Deaton, 1998)].

The HPTN 068 study was a Phase 3 randomized control trial designed to test the effect of a conditional cash transfer on HIV incidence in young South African women. The study was embedded within the Agincourt HDSS study site and used the Agincourt HDSS as a random sampling frame. In 2011, n = 2533 young women from the Agincourt HDSS were enrolled in the trial. Eligibility was restricted to young women aged 13–20 who were enrolled in school (grade 8–11), and not married or pregnant at enrollment (Pettifor et al., 2016b). Participants were also required to have the documentation needed to open a bank or post office account, and co-reside with a parent/caregiver, also with documentation for a bank or post office account. The refusal rate was 17.3% (Pettifor et al., 2016a). Young women were randomized with their parent/caregiver using sequentially numbered sealed envelopes with block randomized assignments 1:1 to the cash transfer intervention or control arm. The cash transfer intervention was paid out monthly, conditional on the young woman attending 80% of school days in the past month. The monthly cash value of the transfer was 300 Rand (approximately ~$36USD at the time). The payment was split so that 1/3 went directly to the young woman, and 2/3 to the parent/caregiver. The amount of the cash transfer was chosen to be similar to the monthly payment value of the South African Child Support Grant (R280 in 2012) (Pettifor et al., 2016a). In 2011, the average monthly income in Mpumalanga Province was R6467 (Mpumalanga Provincial Treasury, 2015). The R300 transfer represented a 5% increase to this monthly average. Young women participants and their parent/caregivers who were assigned to the control arm did not receive payments.

HPTN 068 participants were enrolled in the trial and received their intervention or control conditions for up to three years. Participants were no longer eligible for participation after completing grade 12. Thus, participants enrolled in grades 8, 9, or 10 at baseline could contribute a full three years (R10,800 possible), while participants enrolled in grade 11 could contribute up to two years (R7200 possible). The amount of the cash transfer was also determined by school attendance patterns, as monthly payment was conditional on 80% school attendance. However, most participants received their payments each month, with the average participant attending 95% of monthly school days. The study protocol was reviewed and approved by the University of North Carolina-Chapel Hill Institutional Review Board and the University of the Witwatersrand Human Research Ethics Committee (Medical).

2.2. Cohort construction

To understand the impact of the cash transfer intervention on the mortality patterns of older adults living in households that did and did not receive the cash transfer, we constructed a cohort using the Agincourt HDSS census platform of all members of the HPTN 068 households who were aged 40 years or older in 2011 at the time of trial enrollment. Members began contributing person-time at the date of the baseline HPTN 068 interview with the index young woman, which occurred between March 5th, 2011 and December 17th, 2012. Members accumulated person-time until they died (primary outcome), out-migrated from the study area (n = 352), or otherwise were administrative censored on March 4, 2022. The maximum amount of time contributed by a participant was thus ~11 years. People were excluded from analysis if their end outcome in the Agincourt HDSS was coded as a death or migration, but no accompanying end date was available (n = 4).

2.3. Key measures

The primary outcome we assessed was mortality, as recorded in the complete annual Agincourt HDSS census. Any death that occurred in the interval between two census rounds was recorded in the subsequent census with the date of death. Survival time was then calculated from the date of the baseline HPTN 068 interview to the date of death (or date of censoring in cases where a death did not occur). We assessed whether effects of the cash transfer program on mortality differed across short-, medium-, and longer-term time horizons by categorizing the timing of mortality post-enrollment date in categories of up to 3-, 5-, and 7-years, and across the full follow-up period.

The primary exposure was the randomly allocated HPTN 068 cash transfer arm. To create this variable, HPTN 068 trial data were merged with Agincourt HDSS census data and the HPTN 068 arm assignment was applied to all household members of enrolled households.

We examined the effect of the cash transfer intervention by two key differences in its distribution. First, we categorized older adults by primary caregiver status to track those most likely to be direct vs. indirect recipients of the cash transfer. The R200 household portion of the HPTN cash transfer was paid directly to primary caregivers of the index young woman. Other household members did not directly receive the intervention but would have received any benefit through household spillover. This primary caregiver variable is maintained in Agincourt HDSS records. However, these data are only available for household members who were related to the young woman as parents or grandparents. Members with other household relationships were coded as non-primary caregivers if a primary caregiver was otherwise identified. They were coded as missing if a primary caregiver was not otherwise identified. Second, we categorized the amount of time each household received the intervention into durations of <1, 1–2, and 2–3+ years (max 3.3 years). The duration of the HPTN 068 transfer was primarily determined based on the school grade in which the young women were enrolled at baseline. Trial participants had to be enrolled in school to contribute to the trial, so those who enrolled in later grades were eligible to remain in the trial for shorter follow-up periods. To create an appropriate comparison group in the control arm for these duration categories, we assigned control households to duration categories based on grade of the index young woman at baseline. Young women in Grade 8–9 were assigned to 2–3+ years, Grade 10 to 1–2 years, and Grade 11 to <1 year.

We used several covariates to contextualize the cohort and to

identify the effectiveness of the cash transfer intervention across certain subgroups. Most of these covariates were collected in the HPTN 068 baseline survey: age (in years, at time of HPTN 068 enrollment), gender (man/woman), and household SES (measured with an asset index categorized in quartiles). Two of the covariates were collected in the Agincourt HDSS census: former Mozambican refugee status (yes/no) (Polzer, 2004) and highest educational attainment (none, some primary or secondary education, secondary education or above). Covariates were assessed at the time of baseline HPTN 068 enrollment, or from the Agincourt census year closest in time to the date of enrollment in the HPTN 068 study. The categorical educational attainment variable was derived from the number of years of education reported in the Agincourt HDSS in 2012. In the case of missing educational data for 2012, we used the most recent report of education between age 25 and the year 2012, under the assumption that educational attainment is relatively static after age 25. For observations with no educational attainment data in this range from which to draw (15% of our sample), we used multiple imputation to fill in the remaining missing observations. Twenty datasets were imputed using a Markov Chain Monte Carlo algorithm with multiple chains. Missing data on educational attainment were imputed using gender, former refugee status, age at baseline, household SES (quartile), and educational attainment from years 1999–2019.

2.4. Statistical analysis

To evaluate the association between the cash transfer intervention and mortality, we specified Cox proportional hazards models. We conducted an intention-to-treat analysis. Because the cash transfer intervention was almost universally paid out to intervention arm participants, we expect this estimate to approximate a per-protocol analysis (Pettifor et al., 2016b). Time origin was the HPTN 068 baseline interview, with follow-up until the date of death, outmigration, or March 4, 2022. We calculated unadjusted hazard ratios for mortality outcomes after the full follow-up time, and at 3-, 5-, and 7- years. Adjustment for covariates was not necessary as socio-demographic variables were balanced through randomization. Stratified analyses comparing treatment to control arms were conducted by the key covariates defined above. Intervention duration was not incorporated into these stratified models. The subgroup analysis by education used imputed datasets, and Cox proportional hazard regression was conducted on and then pooled across all 20 imputed datasets. To account for clustering at the household level, we constructed Cox mixed effects models, specifying household as a random effect.

2.5. Role of the funding source

The study sponsors had no role in the study design, data analysis, interpretation of results, manuscript drafting, or in the decision to submit the manuscript for publication.

3. Results

Overall, we identified n = 3568 (n = 1785 intervention vs. n = 1783 control) adult members of households with HPTN 068 enrollment contributing person-time to this analysis, living across 2116 households (Table 1, Fig. 2). The duration of follow-up time ranged from 1 to 132 months (median 126 months). This cohort was majority women (60%), and over a third (35%) identified as a former refugee from Mozambique, reflective of the underlying Agincourt population of this age range. The mean age of cohort members at baseline was 54 years (range 40–108 years), with nearly half (43%) between the ages of 40–50 years. Consistent with the limited educational opportunities experienced under Apartheid, the cohort reported low levels of educational attainment, with about a quarter (28%) reporting no formal education. After using multiple imputation to impute the missing education data (15%), the mean (SE) years of education was 5.65 (0.08), compared to 5.98 (0.08)

Table 1
Sociodemographic characteristics of n = 3568 adult household members of HPTN 068 trial participants, by cash transfer intervention arm.

	Total (n = 3568)	Cash Transfer (n = 1785)	No Cash Transfer (n = 1783)	*P-value*[a]
	Mean (SD)	Mean (SD)	Mean (SD)	
Household Asset Score	15.4 (7.3)	15.5 (7.4)	15.3 (7.1)	0.9
Missing	N = 7	N = 0	N = 7	0.4
Age (years)	54.4 (12.3)	54.3 (12.3)	54.6 (12.2)	
	Freq. (%)	*Freq. (%)*	*Freq. (%)*	*P-value*[b]
Gender				0.8
Male	1417 (40)	713 (40)	704 (39)	
Female	2151 (60)	1072 (60)	1079 (61)	
Former Mozambican refugee				0.4
Yes	1242 (35)	634 (36)	608 (34)	
No	2324 (65)	1149 (64)	1175 (66)	
Missing	N = 2	N = 2	N = 0	
Age at HPTN enrollment				0.3
<50 years old	1539 (43)	747 (42)	792 (44)	
50–79 years old	1522 (43)	776 (43)	746 (42)	
≥80 years old	507 (14)	262 (15)	245 (14)	
Marital Status				0.9
Divorced/ Separated	227 (7)	115 (7)	112 (7)	
Married/ Cohabiting	2117 (63)	1059 (64)	1058 (63)	
Single	453 (14)	217 (13)	236 (14)	
Widowed	539 (16)	269 (16)	270 (16)	
Missing	N = 232	N = 125	N = 107	
Household asset index				0.3
Quartile 1	690 (19)	351 (20)	339 (19)	
Quartile 2	924 (26)	458 (26)	466 (26)	
Quartile 3	947 (27)	494 (28)	453 (26)	
Quartile 4	1000 (28)	482 (27)	518 (29)	
Missing	N = 7	N = 0	N = 7	
Years of education, pre-imputation				0.07
No education	849 (28)	444 (29)	405 (27)	
<12 years	1733 (57)	867 (57)	866 (57)	
≥12 years	436 (14)	198 (13)	238 (16)	
Missing	N = 550	N = 276	N = 277	
Years of education, post-imputation				
No education	884 (25)	461 (26)	423 (24)	
<12 years	2123 (60)	1058 (59)	1065 (60)	
≥12 years	561 (16)	261 (15)	300 (17)	
Missing	N = 0	N = 0	N = 0	
Relationship to young woman				0.3
Father	947 (27)	472 (26)	475 (27)	
Mother	1149 (32)	551 (31)	598 (34)	
Grandfather	140 (4)	79 (4)	61 (3)	
Grandmother	472 (13)	244 (14)	228 (13)	
Other	860 (24)	439 (25)	421 (24)	
Primary caregiver[c]				0.9
Yes	1376 (49)	665 (49)	711 (48)	
No	1385 (51)	708 (51)	677 (52)	
Missing	N = 807	N = 412	N = 395	
Cumulative deaths				
By 3-years	177 (5)	84 (5)	93 (5)	0.5
By 5-years	303 (9)	143 (8)	160 (9)	0.3
By 7-years	423 (12)	197 (11)	226 (13)	0.1
By Study End	640 (18)	313 (17)	327 (18)	0.6
Cash Transfer Duration				

(continued on next page)

Table 1 (*continued*)

	Total (n = 3568)	Cash Transfer (n = 1785)	No Cash Transfer (n = 1783)	*P-value*[a]
	Mean (SD)	Mean (SD)	Mean (SD)	
<1 year	–	380 (23)	–	
1–2 years	–	664 (39)	–	
2+ years	–	641 (38)		
Missing	–	N = 100	–	

[a] Wilcoxon rank sum test.
[b] Pearson's Chi-squared test.
[c] Primary caregiver status assessed for participants who were parents or grandparents of the HPTN 068 young women only. Participants with other relationships were coded as missing if a primary caregiver was not otherwise noted for the young woman.

pre-imputation. As expected, due to the randomized design, there were no statistically significant differences between arms in the socio-demographic covariates we assessed (Table 1).

There was some variation in the duration and way the cohort members were exposed to the cash transfer. Most individuals in the cohort were exposed to the intervention for 1–2 years (39%) or 2–3 years (38%). Fewer were exposed to the cash transfer for <1 years (23%). About half of the sample in both arms (49%) were the primary caregiver of the index young woman.

There were 640 deaths observed over the follow-up period. About a quarter of deaths (28%) were observed in the first 3 years of follow-up, nearly half (47%) in the first 5 years of follow-up, and two-thirds (66%) in the first 7 years of follow-up. The cash transfer intervention did not have a significant effect on mortality outcomes in the full sample over the full follow-up period (Table 2). The hazard of mortality was slightly lower in the intervention arm compared to the control arm, but the effect size was small with confidence intervals spanning the null [HR (95% CI): 0.94 (0.80, 1.10)]. Similar effect sizes were observed for each of the follow-up time subdivisions, with the strongest effect estimate observed for mortality at 7 years [HR (95% CI): 0.86 (0.71, 1.05)]. Similar effect sizes were also observed across the different durations of cash transfer exposures, with the strongest effect estimate observed for durations of 1–2 years [HR (95% CI): 0.71 (0.50, 1.02)]. There were also no differences in effects observed by primary caregiver status.

The cash transfer intervention had strong protective effects in some of the socio-demographic subgroups we assessed (Table 3). There were no significant differences in effect sizes by gender, marital status or age category, although the point estimates across nearly all these categories trended on the protective side of the null. The exception to this trend was the 'divorced/separated' marital status category with a point estimate of 1.45 (95% CI: 0.64, 3.40). For household assets, we found strongly protective effects of the cash transfer intervention on mortality in those with above median assets [HR (95% CI): 0.66 (0.50, 0.86)], but not those with below median assets [HR (95% CI): 1.15 (0.87, 1.52)]. The protective effects were of similar magnitude across the highest two asset quartiles [HR_{Q3} (95% CI): 0.63 (0.43, 0.94); HR_{Q4} (95% CI): 0.68 (0.46, 1.01)]. Similarly, we found strongly protective effects for those with the highest educational attainment [HR (95% CI): 0.37 (0.15, 0.93)], but not for those with lower levels of educational attainment.

4. Discussion

We found new evidence from an extension to a rigorous, randomized trial that a cash transfer intervention led to reduced mortality in certain subgroups of a cohort of older adults in a rural South African setting. Older adults in households that received the intervention tended to live longer than those in control households, though the protective effect in the overall cohort was small in magnitude with confidence intervals overlapping the null. However, we observed strong protective effects in individuals from households with above median SES, and in individuals with higher educational attainment. It is important to contextualize these subgroups in the larger and limited socio-economic environment of the study site. The Agincourt HDSS site covers an area with uniformly low economic opportunities (Collinson, 2010; Kabudula et al., 2017b; Sartorius et al., 2013; Wilkinson et al., 2017), and it covers an older adult population who were limited from accessing educational opportunities during Apartheid (Gomez-Olivé et al., 2018). Thus, even the

Table 2
Impact of the cash transfer intervention on mortality outcome in full follow-up of full sample, and in subgroups by 3-, 5-, and 7-years of follow-up and by duration of cash transfer intervention.

	Intervention arm	Control arm	HR (95% CI)
	MR per 1000 p.m.	MR per 1000 p.m.	
Full sample	1.62	1.73	0.94 (0.80, 1.10)
Mortality timing			
3 years	1.24	1.37	0.90 (0.67, 1.21)
5 years	1.36	1.52	0.89 (0.71, 1.12)
7 years	1.40	1.62	0.86 (0.71, 1.05)
Primary caregiver[a,b]			
Yes	0.96	1.05	0.91 (0.62, 1.35)
No	1.67	1.70	0.98 (0.74, 1.31)
Duration of cash transfer[b]			
<1 year	1.54	1.61	0.96 (0.64, 1.44)
1–2 years	1.13	1.58	0.71 (0.50, 1.02)
2+ years	1.61	1.65	0.97 (0.73, 1.23)

PM = person-month HR = hazard ratio.
[a] Primary caregivers were most likely the direct recipients of the household portion of the cash transfer intervention. Non-primary caregivers likely received exposure to the cash transfer through indirect spillover mechanisms.
[b] At 7 years of follow-up.

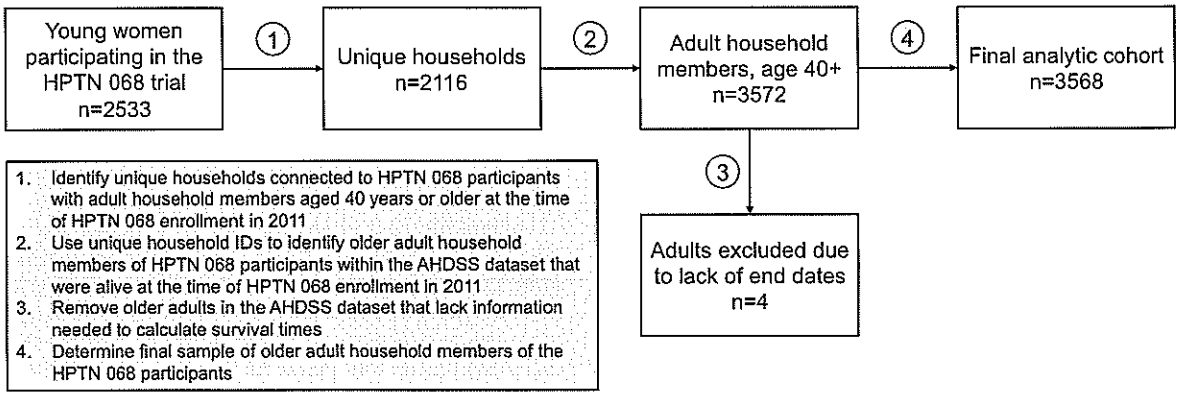

Fig. 2. Flow chart illustrating the steps to arrive at the final analytic cohort n = 3568.

Table 3
Impact of the cash transfer intervention[a] on 7-year mortality outcomes, in subgroups by gender, age, household assets, and education.

	Intervention arm	Control arm	HR (95% CI)
	MR per 1000 p.m.	MR per 1000 p.m.	
Gender			
Male	1.54	1.91	0.81 (0.60, 1.08)
Female	1.30	1.43	0.91 (0.70, 1.18)
Age			
40–49 years	0.64	0.82	0.78 (0.50, 1.19)
50–79 years	1.18	1.43	0.82 (0.60, 1.12)
80+ years	4.67	5.24	0.89 (0.65, 1.22)
Marital Status			
Divorced/ Separated	1.54	1.03	1.45 (0.64, 3.40)
Married/ Cohabiting	1.23	1.38	0.89 (0.68, 1.16)
Single	1.83	2.25	0.81 (0.50, 1.32)
Widowed	2.18	2.52	0.86 (0.58, 1.27)
Household asset quartile			
Q1	1.72	1.84	0.93 (0.62, 1.39)
Q2	1.69	1.23	1.37 (0.94, 2.02)
Q3	1.10	1.72	0.63 (0.43, 0.94)
Q4	1.19	1.74	0.68 (0.46, 1.01)
Below median	1.71	1.48	1.15 (0.87, 1.52)
Above median	1.14	1.73	0.66 (0.50, 0.86)
Education (years)			
No education	2.26	2.56	0.81 (0.59, 1.12)
<12 years	1.23	1.26	0.93 (0.71, 1.22)
≥12 years	0.47	1.21	0.37 (0.15, 0.93)

PM = person-month HR = hazard ratio.
[a] Intervention vs. control arm assignment.

upper ends of the SES and educational spectrum are living in a low-income and limited educational environment. That we observe strong protective effects on mortality in these subgroups with a modestly sized, 2–3 year cash transfer intervention is promising evidence of the utility of cash transfers to promote healthy aging outcomes.

Our findings generally align with the benefits of cash transfers on adult mortality found across Latin America (Barham and Rowberry, 2013; Pescarini et al., 2020). We extend the generalizability of this prior work to sub-Saharan Africa with our rural South African setting. Our findings are consistent with pathways from cash transfers to delayed mortality that run through some of the established benefits of cash transfers: reductions in high blood pressure prevalence (Behrman and Parker, 2013; Fernald et al., 2008), increases in vaccinations (Salinas-Rodríguez and Manrique-Espinoza, 2013), and increased healthcare utilization in older adults (Riumallo-Herl and Aguila, 2019). Our findings are in contrast to the single study we identified that found increased mortality from a pension cash transfer program in Mexico (Feeney, 2017). These differences may be explained by underlying differences in the South African versus Mexican study populations, and by differences in the income investment decisions that could arise due to age differences in the target beneficiaries, and gender profile of the direct recipients.

Importantly, the strong protective effects we observed of the cash transfer intervention on mortality were restricted to subgroups who generally exhibited higher indicators of SES. There are several plausible explanations for why we did not observe a more robust effect for all household members. First, the cumulative value of the grant could have been insufficient to benefit the poorest and least educated households. Anchored at ~5% of the average monthly income in the area, the cash transfer may have been too small to lift the poorest households out of poverty and to a place where the extra income could be allocated towards longer-term investment in health and well-being. Second, the most educated members of the cohort may have had higher levels of financial and/or health literacy. Their educational backgrounds could plausibly provide them with the knowledge base to effectively use the additional income to maximize health and well-being benefits. Finally, the direct recipients of the household portion of the grant were primarily women caregivers, often mothers or grandmothers of the trial participants. Women tend to invest household money in children and other household members instead of directly in themselves (Burns et al., 2005; Duflo, 2003). Thus, it is possible that we do not observe strong effects in the direct recipients because they were more likely to be women who invested the money outside of themselves. At the same time, it is possible that the spillover effects from this investment were too diluted to significantly benefit other older household members. Spillover effects are likely stronger in households with fewer members. If higher SES households trended towards lower household size, then this would provide another potential explanation for our findings.

The broad social protection landscape in South Africa is important context to consider in interpreting our results. Households in the study area had access to a family of government cash transfer programs, with the Child Support Grant (Triegaardt, 2005) and the Old Age Pension (Case and Deaton, 1998) programs having the largest reach. Both of these programs have age eligibility requirements for beneficiaries and are means tested. However, in the low-income area of our study population, most households with age-eligible members would qualify under the means test. Most recent estimates from Mpumalanga province show that 54% of households are receiving income from governmental cash transfers (Statistics South Africa, 2020), and 74% of households with age-eligible children are receiving the Child Support Grant (Statistics South Africa, 2018). In this context, it is possible that the marginal effect of the cash transfer examined in our study may not have been sufficiently large to have beneficial impacts above the governmental cash transfer income received. Future work should focus on isolating the independent and synergistic effects of cash transfer income from multiple sources.

Elements of our study design support robust causal inference about the effect of a cash transfer intervention on adult mortality over a long-term time horizon. We leveraged a randomly allocated cash transfer experiment and were thus able to address the concerns about endogeneity of cash transfer exposures that are common in observational evaluations. We were able to link this cash transfer experiment to high-quality and complete longitudinal mortality data of members of households involved in the trial because of the Agincourt HDSS platform that underpinned both the trial and an annual census on all community members in the study site. The underlying mortality data allowed us to assess longer-term follow-up (>10 years) than is usually possible in cash transfer evaluations. This unique linkage of high-quality datasets is a major strength of our work.

Though the randomized exposure is a major strength, variation in the 'dose' of cash transfer exposure received by household members influences the interpretation of our results. The older adult members of our study cohort were not the target beneficiaries of the cash transfer, so exposure would either occur through direct receipt of the cash transfer on behalf of the young woman beneficiary, or through indirect spillover mechanisms. We observed no difference in effect by primary caregiver status. However, variations in household composition and income sharing behaviors indicate that some members of our cohort will have received more direct income than others in ways we were not able to capture.

Additionally, some aspects of our study design and data structure limit the generalizability of our results. First, outmigration from the study site was a source of loss-to-follow-up as mortality outcomes were not monitored after migration. Although overall out-migration in this study was low (<10%), if migrant mortality patterns differed appreciably from those who remained in ways related to the cash transfer exposure, our results would be biased. Second, our study was conducted among older adults in a low-income, rural community in northeastern

South Africa. Future work should extend these findings to other geographic settings.

In sum, we found promising evidence that a short-term cash transfer exposure can benefit people living in households that receive it by delaying mortality. However, the protective effects were restricted to members of our cohort who were already in the upper distributions of socio-economic status and education. Our findings should prompt researchers and policymakers to investigate whether cash transfer program values need to be scaled to larger amounts for lower-SES groups. Future work should focus on understanding the optimal amount, timing, structure, and targets to maximize the benefits of cash transfer programs in promoting healthy aging and longevity.

Authorship statement

Molly Rosenberg: Conceptualization, funding acquisition, investigation, methodology, data curation, project administration, resources, writing-original draft, writing-review and editing, approval of final manuscript as submitted, Erika Beidelman: Formal analysis, data curation, writing-review and editing, approval of final manuscript as submitted, Xiwei Chen: Formal analysis, writing-review and editing, approval of final manuscript as submitted, David Canning: Funding acquisition, interpretation of study results, approval of final manuscript as submitted, Lindsay Kobayashi: Conceptualization, funding acquisition, project administration, writing-review and editing, approval of final manuscript as submitted, Kathleen Kahn: Funding acquisition, interpretation of study results, writing-review and editing, approval of final manuscript as submitted, Audrey Pettifor: Funding acquisition, data curation, interpretation of study results, writing-review and editing, approval of final manuscript as submitted, Chodziwadziwa Kabudula: Funding acquisition, data curation, interpretation of study results, writing-review and editing, approval of final manuscript as submitted.

Trial registry and protocol

The HPTN 068 parent trial is registered at clinicaltrials.gov (NCT01233531). The study protocol is archived on the HPTN website: https://www.hptn.org/research/studies/hptn068.

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Data sharing statement

The data analyzed in this paper are available upon request because they were obtained from a third party and contain sensitive health and personal information. The HPTN 068 data access is managed by FHI360. Data requests can be made by contacting Erica Hamilton at EHamilton@fhi360.org.The Agincourt Health and Demographic Surveillance System data are available upon request as described on their data overview website (http://www.agincourt.co.za/index.php/data/) and by contacting their data manager (DataManager@agincourt.co.za).

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The data that has been used is confidential.

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