Policy Report on a Village-level RCT Testing for Direct and Interaction Effects of Cash Transfers and Psychological Interventions

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Contents

1 Overview of Design and Results of Studies

This report describes results for primary analysis on data collected for two studies using the same experiment. Both studies report on a village-level randomised controlled trial with adult women in 415 villages in Western Kenya. The trial has four arms: it cross-randomizes a psychological intervention with a USD 2,237 PPP unconditional cash transfer. This represents preliminary reuslts from the project: please contact study authors for updated results before circulation or citation.[1]

The psychological intervention consists of two ten-minute videos, viewed by participants on a tablet, a facilitated drawing exercise and discussion and distribution of a calendar. The videos narrate the life stories of two upwardly-mobile poor women similar to the audience. They describe how they came to be successful, the obstacles they faced and some lessons they drew from their experiences. For example, one woman in the video saves, takes sewing classes and opens a tailoring business. Participants then do exercises where they visualise a future desirable to them and make plans to achieve it. Women in villages not assigned to receive the aspirations intervention watch a placebo video and do placebo exercises containing the same factual information but without the individual narratives. The cash transfer intervention is the standard programme implemented by the NGO GiveDirectly, as well as the placebo video.

The first study, "Aspirations, Assets and Anti-Poverty Policies", collects household level data on economic outcomes from the adult primary female member in each sampled household in all 415 villages in Siaya and Homa Bay counties. This is authored by Kate Orkin, Robert Garlick, Mahreen Mahmud, Richard Sedlmayr, Johannes Haushofer and Stefan Dercon.

This study finds that the psychological intervention increases labour supply, monetary investment in economic activities, revenue from investment, consumption expenditure and assets 19 months later. The cash transfer with no psychological components has similar effects to the psychological intervention on labor supply, effects about twice as large on investment, revenue and consumption and effects about four times as large on non-land assets.[2] Offering people both the cash transfer and the psychological intervention (the "combined" intervention) generates similar outcomes to the cash transfer alone on most aspirations and investment behaviours, except education. The combined intervention raises education aspirations and spending more than the cash transfer alone, highlighting that the combined intervention changes the allocation of household resources in households receiving cash transfers toward children.

Women face a range of constraints within the household that the psychological intervention

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[1]This note is substantively consistent with the Pre-Analysis Plan, reporting on the pre-specified primary analysis. We present here the main primary outcomes noting any deviations in variable definitions in the appendix, and leave robustness tests and investigations of mechanisms to the full working papers being produced at present.

[2]As prespecified, consumption expenditures include spending on durable household goods but not on housing. There is a large effect of nearly USD 500 PPP on housing expenditures of the cash transfer.

may alleviate. The second study, "Economic and Psychological Constraints to Womens Empowerment", collects data on intimate partner violence and women's economic empowerment from a subset of the household study sample: married women below the age of 60 from 176 villages in Siaya county. It is authored by Mahreen Mahmud, Kate Orkin and Emma Riley.

The psychological intervention on its own has no effect on women's experience of violence, nor on control of or input into household enterprises, although it has positive effects on shares of household assets owned. Both the cash transfer and combined intervention improve women's control of household economic resources. However, they have different effects on an index of the frequency of physical and sexual intimate partner violence. The cash transfer has no effects on the violence index compared to the placebo group, while the combined intervention reduces the violence index relative to just receiving the cash transfer.

2   Sampling and Data

We study households living in rural areas near Lake Victoria in Western Kenya. The first study works in 415 villages spread over 21 contiguous rural 'sublocations' (Kenyan administrative units) in Homa Bay and 16 contiguous rural sublocations in Siaya. Each village contains an average of 76 households. Roughly half of the villages contain a primary school, one third contain a market, and one sixth contain a clinic (Table A1). Of these 415 villages, 176 are in Siaya and women in these villages are surveyed at endline twice, for both studies in this report. The remainder are in Homa Bay and are surveyed only once at endline, for the study of household outcomes.

We worked with Innovations for Poverty Action to collect data on village amenities and conduct a short census of 32,921 households in all study villages. Based on the census data, we sampled 9-24 households per village that (i) included an adult female member, (ii) were non-polygamous, and (iii) scored below a village-specific poverty threshold. These criteria were chosen to ensure that all sampled households would be eligible for both the cash transfer and the psychological intervention. Appendix A describes the sampling process in more detail.

2.1   Study 1 sample

The sampling process generated a baseline sample of 8,309 households. This is the sample used in Study 1.[3] We administered a baseline survey to the adult 'primary female' member in each sampled household. We chose this person because the upcoming cash transfer intervention was most likely to be paid to women. Although households receiving cash transfers could choose which member's mobile money account received the transfer, 85% chose the adult primary female member. The

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[3]We don't sample the same proportion of every village, so sampling probabilities vary by village. None of the descriptive or causal results in the paper change substantively when we reweight the data to account for variation in sampling probabilities.

baseline survey measured household-level investment in multiple activities (agriculture, livestock, non-farm enterprises, market labour), revenue, assets, and consumption. We also measured psychological characteristics of the respondent – aspirations, beliefs, preferences, and mental health.

We conduct an endline survey approximately 19 months after the psychological and placebo interventions. This region of Kenya has two crop cycles per year, so this timing allows all households to complete at least two cycles of planting and harvesting between treatment and endline. We attempt to survey every respondent from the baseline and successfully survey 87.2% of all baselined households. We conduct a comprehensive tracking exercise, including tracking migrants and split households and interviewing proxies from households if the woman cannot be found.[4] See Appendix A for more details. Attrition does not differ by treatment assignment and is only weakly related to baseline household characteristics (Table A3).

The survey measures six prespecified household-level economic aggregates: expenditure on intermediate inputs for production (including hired labour), household labour supply, revenue, consumption expenditure, stock of assets, and investment in education. Each core economic concept is an aggregate constructed from multiple measures in our survey instruments. For example, household labour supply is constructed by summing labour supply over all household members, and the member-level labour supply is constructed by summing over different economic activities such as crop agriculture, raising livestock, working in household non-farm enterprises, and work outside the household.[5] We also prespecified analysis of psychological mechanisms. In this report, we discuss effects on aspirations, discussed in more detail in Section 4.5, as they help explain some of the effects on economic aggregates. Data on other mechanisms are being cleaned. Our reporting here does not indicate that we view these as more important than other mechanisms specified in our analysis plan.

2.2   Study 2 sample

Study 2 focuses on a subset of the household study sample: married women below the age of 60 at baseline from all 176 household study villages in Siaya county only. Due to budgetary constraints we could only administer surveys for this study in one county. We surveyed nearly 3,000 monogamously married women in face-to-face surveys an average of 12 months after the

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[4]For psychological variables, we only use responses from the baseline female respondent. For economic variables, we use an average of values for the woman's new and old household if we find both, or values for either household if we only find one.

[5]We generally code activity-specific measures as zeroes for households that do not engage in that activity. For example, revenues from and expenditure on non-farm enterprises are coded as zero if the household does not have a non-farm enterprise. The only exceptions are education-related measures. These are only asked about household members in specific age ranges, so households with no members in the relevant age ranges are coded as missing. None of the treatments substantially change the number of school-age household members, so this coding rule does not generate a sample selection problem.

households received the first tranche of the cash transfer (US$203 PPP).[6] The Study 1 household endline survey in Siaya was conducted three to six months after the Study 2 endline survey.

All respondents are at least 18 years of age. We pre-specified that we would restrict the sample to those aged 59 and below at baseline, since intimate partner violence decreases with age and becomes rarer in women above this age.[7] The overall rate of attrition for this sample is 11 percent. Attrition is also uncorrelated with treatment in this sample (Table D).

We collect data on three prespecified primary classes of variables: enterprise ownership, asset shares and intimate partner violence.[8] For some outcomes, we rely on the Study 1 baseline and endline surveys, which are conducted with the same primary female respondent.

All surveys for this study were administered face to face. However, questions related to physical and sexual violence and any injuries sustained due to violence were elicited using Audio Computer-Assisted Self-Interview Software (ACASI), discussed in Appendix E. ACASI was used to alleviate concerns that women would not want to directly report violence to the enumerator. ACASI allowed the respondent to listen to recorded questions in the local language, Luo, and answer directly on the tablet by selecting coloured shapes corresponding to each answer choice.

3 Interventions and Experimental Integrity

We randomly divided the 415 study villages into four treatment arms using a stratified random assignment algorithm, described in Appendix A.4. Stratification was based on census data and occurred before baseline surveys. One quarter of villages were assigned to receive both cash transfers and a psychologically active intervention. A second group of villages was assigned to receive only the psychologically active intervention. The third group of villages was assigned to receive cash transfers and a placebo intervention, while a fourth group of villages was assigned to receive only the placebo intervention. Within each village, the interventions were offered to all households who completed the baseline survey. We describe each of the interventions in the next two subsections.

3.1 Psychological and Placebo Interventions

The psychological intervention has two parts. First, participants watched two ten minute videos.[9] We collected real-life stories of women whom village elders identified as having improved their

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[6]95% of the sample was surveyed during this period with the rest (5%; 140 respondents) being interviewed at different points during the Study 1 household survey.

[7]In the study placebo group, 23% of the women above 60 report an incident of physical violence in the last 12 months while 61% women report an incident below the age of 60.

[8]We have not yet completed results on consumption shares, a fourth primary class of variables.

[9]All intervention materials including all videos are available from https://www.socialscienceregistry.org/trials/996. Our thanks to Catherine Thomas and Michala Iben Riis-Vestergaard (Princeton University) and Pat Olvera and Rita Wachera (Khangarue Media) for their work on video development, scripting and production with the research team. Thanks to Carol Dweck for her comments on the script.

life through hard work and persistence and compiled them into scripts. The videos show women with aspirations for their children to achieve high levels of education such as going to university, to increase income, including by starting new businesses, and to acquire assets such as owning a house. The videos show how women improved their economic position from being very poor, the obstacles they faced and some lessons they drew from their experiences. We filmed the videos with street actors from areas near the study location, as filming real people from the stories posed ethical challenges. See Appendix C for further details on the storylines and psychological theory underlying the intervention.

Participants then completed exercises. In the video, characters do a "best possible selves" exercise, envisioning themselves in a desirable future (King, 2001; Peters et al., 2010). Characters contrast their aspiration with aspects of their reality that impede the realisation of this future. After the video, respondents did an hour-long exercise practising the behaviours modelled by the characters. The participants were asked to imagine their lives in five years "after everything has gone as well as it possibly could." They drew what they imagined and explained it to the group or the facilitator, including discussing how it was different to their lives at present.

Characters were shown identifying smaller, more immediate goals that would lead toward their long-term aspirations and delineated specific strategies for achieving these goals. These smaller steps were linked directly to achieving the larger long term goal. Similarly, participants worked with the fieldworker to make goals clear and specific and then to rank goals according to what they thought was both achievable and possible. They listed steps to work towards their highest ranked goal. Characters were shown making specific plans for how they would adjust plans when things went wrong, using the if-then approach. They were also shown facing substantial obstacles and planning how to overcome them. Similarly, in the exercises, after making plans towards their goals, participants worked out potential obstacles to achieving their goal and strategies they could adopt to overcome these obstacles. This has elements of Mental Contrasting and Implementation Intentions (MCII) interventions (Duckworth et al., 2013; Oettingen and Gollwitzer, 2010) and personal goal-setting interventions (Morisano et al., 2010; Stadler, Oettingen, and Gollwitzer, 2009).

All participants from villages not assigned to the psychologically active intervention were assigned to a placebo intervention. The placebo intervention is designed to contain the same factual information as the psychologically active intervention and to deliver the same experiences of watching a video on a tablet and interacting with a facilitator from outside the village. The placebo video included at least one shot of every scene and character from the psychologically active video but excluded elements that are likely to manipulate critical psychological variables, such as characters, insightful narratives, shots of people conveying obvious emotion, or music. The placebo video described the same activities as the psychologically active video. For example, one character in the latter video describes starting to raise poultry and learning not to overfeed her chickens so

that they do not lay eggs; the former video shows the same shots of a poultry house, chickens and eggs while a narrator notes that people farm chickens and that if they are overfed they will not lay eggs. The placebo group also participated in exercises where they were reminded of the content of the video, discussed the video and facts presented, and drew the scenes that were most memorable.

The psychological and placebo treatments were administered either individually or in groups of three to four people. At the conclusion of the exercise, participants received a single-page calendar depicting the two role models from the videos, and sayings that described the core spirit of each video. Participants also received a set of stickers they could choose to place on the calendar. Participants in the psychologically active intervention were encouraged to choose stickers that reminded them of the exercise, while participants in the placebo intervention were not.

3.2   Cash Transfers

The cash transfer intervention is the standard lump-sum, unconditional transfer implemented by our partner, GiveDirectly. The transfer is large: 2,237 USD PPP (1,000 USD nominal), compared to our sample's mean annual consumption expenditure and mean non-land asset value at baseline of respectively 4,331 and 1,230 USD PPP. GiveDirectly emphasizes that the transfer is entirely unconditional and that there are no restrictions on how it is spent. At endline, only 3% of households report thinking that there were any conditions attached to the transfer. Similar cash transfer programmes have been shown to have positive effects on recipient households across a wide variety of economic measures, such as expenditure, asset holdings and food security, (e.g. Evans and Popova, 2014; Banerjee et al., 2017; Bastagli et al., 2016).

In each cash-treated village in this study area, GiveDirectly gave transfers to all households in the village fulfilling the eligibility criteria.[10] Households were sent three mobile money transfers, made in intervals of approximately two months: a small transfer ("token") of approximately 203 USD PPP and two large transfers of approximately 1,107 USD PPP. Transfers were sent using the mobile money system MPesa. If an eligible household did not have a mobile phone to access MPesa, GiveDirectly provided one and subtracted its cost from the value of the final transfer. Transfers were typically sent at one time per month to all households scheduled to receive transfers. Transfers were offered to the household as a whole, although whichever household member was at home usually signed up to receive the transfer via M-Pesa. In 85% of households in our sample, the woman was the recipient. Those trial participants who were not assigned to the cash transfer received no component of the cash transfer intervention.

The cash transfer and both the psychological and placebo interventions were delivered at similar times, but by entirely different teams. All enrolment and delivery of the cash transfer

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[10]GiveDirectly's exact eligibility criteria for ongoing enrolment have since changed and they now give transfers to most or all households in a village. The details of the rollout of transfers are described further in Appendix B.

was implemented by GiveDirectly. The psychological and placebo interventions, as well as the baseline and endline surveys, were implemented by Innovations for Poverty Action. It is possible that recipients saw a link between the two organisations, but neither organisation announced a link to the other. Both the psychological and placebo interventions were timed to occur in the same month that (most of) the villages in an area received the first lump sum payment. Details of the intervention timing relative to the cash transfer and video administration are in Appendix C.

3.3 Balance and Compliance

The treatment assignments are balanced on a wide range of pre-treatment village-level characteristics (Table A1). Household-, and respondent-level characteristics are also balanced (Table A1) for Study 1; Table A2 for Study 2).

We show compliance in the Study 1 sample (Table A5). In both studies, we use intention-to-treat estimates, capturing the effect of the policy were it implemented in practice. Compliance is similar in the placebo and psychological intervention groups, the first main comparison we use, with 89–90 percent of women watching the video to which they are invited. It is also similar in the cash and combined groups, the second main comparison we use, with 77–78% of women receiving both interventions to which they were assigned. In the Study 2 sample, compliance rates are similarly balanced across these comparisons.

4 Study 1 Results: Household Economic Outcomes

4.1 Summary of Results

We collect data on six prespecified household-level economic aggregates: expenditure on intermediate inputs for production (including hired labour), household labour supply, revenue, consumption expenditure, stock of assets, and investment in education, a median of 19 months after intervention. Deviations to variable definitions from the PAP are given in Appendix F.

The psychological intervention, relative to the placebo, increases multiple types of investment relative to the placebo – expenditure on productive inputs (by 27%) and labour supply (by 5%). It does not increase overall education spending, the prespecified outcome, although it does increase spending for young children (by 11%). Households increase revenue (by 12%) and accumulate 6% more assets, mainly through cash saving. The intervention has positive effects of 0.11 standard deviations on an index of all six pre-specified outcomes, significant at the 1% level.

The cash transfer by itself increases investment, labour supply, and revenue, in line with other studies (Bastagli et al., 2016; Banerjee et al., 2017). Effects on labor supply are similar to effects of the psychological intervention. Compared to the psychological intervention, effects are twice as large on investment, revenue and consumption and about four times as large on non-land assets.

The combined cash transfer and psychological intervention has similar effects to the cash transfer on most economic outcomes. However, the combined intervention and cash transfer differ in their effects on education. The combined intervention increases education spending significantly more than the small effect of the cash transfer alone. To finance the higher education spending, households in the combined intervention group spend slightly less than households in the cash transfer group on food and non-durable consumption and purchase fewer livestock.

In this report, we discuss effects on one mechanism, aspirations, which may offer one explanation for differences in effects between the cash and combined arms[11]. The psychological intervention raises women's aspirations 19 months later, with aspirations measured as an index of variables capturing the level of income, wealth and children's education they want to achieve in 10 years time. Households that are offered cash transfers without the psychological intervention raise their aspirations for future assets and income by respectively the value of the transfer and by 11%. However, there is no effect on aspirations for children's education, in line with there being no effect on education spending. In contrast, the combined intervention substantially increases both aspirations for children's education and education spending.

4.2   Descriptive Statistics

The average untreated respondent in this sample is 41 years old and lives in a household with 1.8 other adult members and 2.9 children, yielding a dependency ratio of approximately 1.35 (Appendix Table A1). 58% of the respondents are married; most of the rest are widows. Total annual household consumption at baseline is 4,331 USD PPP. This is 11.86 USD PPP per household per day, or 2.2 USD PPP per household member. Households own non-land assets worth 1,230 USD PPP on average. They spend 707 USD PPP per year on inputs for their economic activities and earn revenue of 2,194 USD PPP from these activities. Most households engage in a mixture of income-generating activities. On average, households get 21% of their revenue from casual/salaried work outside the household, 30% from raising crops, 25% from raising livestock, and 24% from non-farm enterprises (mostly small retailers).

4.3   Specification

Our estimation and inference methods follow directly from the structure of the experiment. We estimate models of the form

$$Y_{iv} = \text{Cash}_v \cdot \beta_C + \text{Psych}_v \cdot \beta_P + \text{Both}_v \cdot \beta_B + \mathbf{X_{iv}} \cdot \mathbf{\Gamma} + \epsilon_{iv}, \tag{1}$$

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[11]We pre-specified more mechanism outcomes but we do not include them in this report.

where i and v index individuals and villages. Y_{iv} denotes the post-treatment outcome of interest measured in the follow-up. Cash_v, Psych_v, and Both_v are indicators for villages assigned to respectively cash transfers, psychological interventions, and both treatments. Hence β_B measures the combined effect of both interventions relative to neither, not the interaction effect of the two variables. $\mathbf{X_{iv}}$ is a vector of prespecified covariates and fixed effects for sublocation (a geographic unit containing roughly 10 villages)[12].

We conduct inference using heteroskedasticity-robust standard errors clustered by village, the unit of treatment assignment. We also report p-values for tests of the hypotheses that the cash and psychological interventions have the same effect ($\beta_C = \beta_P$), the cash and combined interventions have the same effect ($\beta_C = \beta_B$), the psychological and combined interventions have the same effect ($\beta_P = \beta_B$), and the combined intervention has the same effect as the sum of the cash and psychological interventions ($\beta_B = \beta_C + \beta_P$).

Papers increasingly show one omnibus index to provide one summary measure of the economic consequences of a treatment. We thus also report treatment effects on an index that averages the six economic aggregates using inverse covariance weighting, following Anderson (2008). This is not prespecified, as we prespecified that we view each economic aggregate as testing a theoretically distinct hypothesis. However, we noted we would adjust strategies for multiple test adjustment as conventions evolve.

Finally, for every hypothesis test, we also report a sharpened q-value that controls the false discovery rate across all six economic outcomes tested, following Benjamini, Krieger, and Yekutieli (2006). Again, this is not prespecified: we only prespecified reporting q-values across variables which make up an aggregate, where we report them. All results reported survive correction for multiple testing.

4.4   Effects on Economic Aggregates

The psychological intervention increases multiple types of investment (Table 1)[13]. The inverse-covariance weighted average of the six economic outcomes shifts by 0.11 standard deviations (Table 1, column 1). Households increase total days of work by 27 days per year (5% of the placebo mean). Households also increase total expenditure on hired labour and intermediate inputs to home production by 231 USD PPP (27% of the placebo mean). The psychological intervention also increases revenue and asset value, potentially due to returns from the higher input expenditure

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[12]The prespecified covariates we use are month-of-endline fixed effects (to account for seasonality); the baseline values of household size, assets, a self-belief index, respondent education, and respondent age; and an indicator for the endline being answered by a proxy respondent. We also pre-specified fixed effects for randomization stratum but instead use sublocation fixed effects. There are a number of sublocations within a stratum. The two types of fixed effects are highly correlated.

[13]We make small deviations from our pre-analysis plan with the definition of certain variables. These are documented in Appendix Section F.

and labour supply (Table 1, columns 4 and 6). Total annualised revenue and the value of non-land assets increase by respectively 262 and 97 USD PPP (12% and 6% of the placebo group means): the former effect is significant at the 10% level. The psychological intervention also increases consumption expenditure by 142 USD PPP, 4% of the placebo mean (Table 1, column 7). This is significant at the 10% level. Finally, we note the psychological intervention raises some measures of education investment. Annual household-level education spending increases by a statistically insignificant 22 USD PPP, or 3.5% of the placebo mean (Table 1, column 6). This is driven by a statistically significant increase of 9 USD PPP per primary school-aged child, equal to 11% of the placebo mean.The treatment effect on post-primary school-aged children is close to zero, potentially because education expenses for older children – secondary school fees and boarding – are often larger and more lumpy (Brudevold-Newman, 2016; Habyarimana and Jack, 2020; Lucas and Mbiti, 2012).

The cash transfer generates similar effects on *most* types of economic behaviour when offered by itself or combined with the psychological intervention (Table 1). Both treatments increase expenditure on inputs and hired labour, revenue, non-land asset value, and consumption expenditure. Both have positive effects on labour supply, although only the effects of the cash arm are significant.

However, there are differences in the education dimension. The cash transfer alone has positive but modest and statistically insignificant effects on education expenditure. The combined intervention generates both higher education expenditure than the cash transfer alone. The combined group spent 127 USD PPP more per year on education than the placebo group, equal to 20% of the placebo group mean (Table 1, column 5). Households in the combined group finance the higher education expenditure with small reductions in several other dimensions. The combined group have somewhat lower spending on non-land assets: 352 USD PPP, compared to 407 USD PPP in the cash group (respectively 23% and 27% of the placebo mean), although the difference is not statistically significant. The cash group hold significantly more livestock than the combined group. The treatment effect on annual consumption expenditure is slightly larger, though not significantly different, between the two groups (322 USD PPP for cash and 232 for combined). This is due to cash effects being larger than the combined effects on food and non-food non-durables. The cash and combined groups have similar spending on durables and social expenditures.

4.5   Effects on Aspirations

The psychological intervention increases aspirations for the future (Table 2). The intervention increases aspirations for a child's years of education by 0.21, driven by a 5 percentage point increase in the probability of aspiring to get some post-secondary education. It has positive but not statistically significant effects on aspirations for income and assets (of respectively 2.3% and 3.2% of the placebo mean). These effects lead to a 0.09 standard deviation increase in the inverse covariance-weighted average of the three measures (following Anderson 2008). This is consistent with the psychological

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Table 1: Treatment Effects on Economic Behaviour

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Index Components			
	Economic Index	Labour Supplied (Days)	Inputs & Hired Labour	Revenue	Education Expenditure	Non-Land Assets	Consumption Expenditure
Psych	0.113***	26.8**	231**	262*	22.4	97**	142*
	(0.036)	(11.6)	(100)	(155)	(27.8)	(46)	(74)
	[.]	[0.070]	[0.070]	[0.071]	[0.125]	[0.070]	[0.070]
Cash	0.234***	27.2**	451***	463***	44.9	407***	322***
	(0.039)	(12.4)	(103)	(159)	(30.5)	(50)	(78)
	[.]	[0.012]	[0.001]	[0.003]	[0.036]	[0.001]	[0.001]
Combined	0.258***	9.0	653***	545*	126.6***	352***	232**
	(0.063)	(11.5)	(213)	(303)	(31.5)	(47)	(96)
	[.]	[0.096]	[0.004]	[0.031]	[0.001]	[0.001]	[0.012]
P: cash	0.003	0.973	0.041	0.236	0.503	0.000	0.033
= psych	[.]	[0.550]	[0.074]	[0.216]	[0.433]	[0.001]	[0.074]
P: cash	0.718	0.128	0.356	0.798	0.022	0.288	0.370
= combined	[.]	[0.469]	[0.587]	[0.797]	[0.150]	[0.587]	[0.587]
P: psych	0.004	0.118	0.007	0.220	0.001	0.000	0.330
= combined	[.]	[0.097]	[0.010]	[0.153]	[0.003]	[0.001]	[0.198]
P: cash	0.163	0.007	0.878	0.536	0.168	0.025	0.049
+ psych	[.]	[0.047]	[0.414]	[0.336]	[0.144]	[0.067]	[0.081]
= combined							
Placebo mean	-0.000	525	857	2,101	640	1,529	3,796
# clusters	413	413	413	413	412	413	412
# obs	7,243	7,240	7,243	7,243	6,273	7,242	7,224

Notes: All variables are at the household level and scaled to annual figures except for education expenditure. All currency values are measured in constant 2018 USD PPP. We value output and expenditure on inputs following conventions from the Living Standards Measurement Surveys (Grosh and Glewwe, 2000) and on production from self-employment in the UN System of National Accounts (FAO, 1996). Labour supply is days of work on farm and non-farm household enterprises or supplied to the market, for all household members over 16. Revenue captures production in farm and non-farm household enterprises, including crops, livestock produce, sales of live animals, and sales from all non-agricultural enterprises owned or operated by household members. Production is valued at farm-gate prices. Input expenditure includes purchase of inputs, stocks and inventory, rental, maintenance, and expenditure on hired labour. It does not include fixed asset purchases. Hired labour costs are the number of days of labour hired in, multiplied by average wage rates for the village for the activity. Education expenditure is all fee and non-fee expenditure for all household members aged 6-20 in the current and preceding school years and is shown here at the household level. Non-land assets are the estimated value, if sold, of durable assets, livestock, and stocks of dried maize, as well as savings. Consumption expenditure captures spending on food, non-durable and durable household goods, and social expenditures (Deaton and Zaidi, 2002). Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

intervention raising aspirations, inducing higher labour supply and input expenditure, generating higher revenue and asset values, and allowing slightly higher consumption 19 months later.

The cash transfer increases aspirations, but only in some domains. Asset and income aspirations are respectively 2,005 and 610 USD PPP higher in the cash group than the placebo-only group (Table 2). Adding the psychological intervention to the cash transfer slightly increases asset aspirations and slightly decreases income aspirations, but neither difference is close to statistically significant. This is consistent with the cash and combined interventions having similar effects on aspirations for future assets and income.

The cash transfer alone has a negligible effect of 0.06 on aspirations for a child's years of education. This is consistent with the cash transfer having limited effects on education expenditure. Adding the psychological intervention more than doubles the size of this effect, though the difference between the two effects is not statistically significant. This is consistent with much larger effects on education spending in the combined group than the cash group.

Table 2: Treatment Effects on Aspirations

	(1)	(2)	(3)	(4)
		Index Components		
	Aspirations Index	Assets	Income	Education
Psych	0.089**	266	127	0.214***
	(0.035)	(567)	(216)	(0.067)
	[.]	[0.743]	[0.743]	[0.005]
Cash	0.128***	2,005***	610***	0.057
	(0.036)	(626)	(205)	(0.076)
	[.]	[0.005]	[0.005]	[0.177]
Combined	0.177***	2,508***	569**	0.133*
	(0.040)	(606)	(242)	(0.071)
	[.]	[0.001]	[0.020]	[0.030]
P: cash = psych	0.312	0.007	0.030	0.029
	[.]	[0.023]	[0.023]	[0.023]
P: cash = combined	0.247	0.458	0.875	0.267
	[.]	[1.000]	[1.000]	[1.000]
P: psych = combined	0.023	0.000	0.063	0.208
	[.]	[0.002]	[0.067]	[0.104]
P: cash + psych	0.447	0.789	0.601	0.142
= combined	[.]	[1.000]	[1.000]	[0.743]
Placebo mean	-0.000	8,499	5,357	15.5
# clusters	413	413	413	410
# obs	7,232	7,204	7,185	6,142

Notes: Column (1) is an Anderson (2008) index consisting of variables in columns (2), (3) and (4). Income aspirations are the level of monthly income that a household would like to reach at the end of the next ten years. Income is defined as all sources of cash income for the household, including earnings from production and transfers from any NGO or government programmes. This value has been scaled to annual income. Asset aspirations are the level of assets that the household would like to reach at the end of the next ten years, including their house, furniture, consumer goods and a transport vehicles. Income and assets are measured in constant 2018 USD PPP. Education aspirations are the aspirations for years of education attained by a randomly selected child, set to missing for households without children. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

5 Study 2 Results: Female Empowerment

5.1 Summary of Results

The psychological intervention significantly improves the share of household assets owned by the woman but does not change her experience of intimate partner violence. The cash transfer alone improves her control of household economic resources but also does not significantly change the

frequency of physical and sexual intimate partner violence. The combined intervention has similar impacts on women's control of household resources as of cash alone but results in a marginal (insignificant) decline in the incidence and frequency of physical and sexual violence as compared to the placebo group. Further, women assigned to the combined intervention report a significantly lower frequency of sexual and physical violence as compared to the cash transfer alone. The cash transfer also leads to an increase in the incidence of violence leading to injury, an effect mitigated by the combined intervention.

5.2   Description of Context

The region is patrilineal (inheritance is through the male line) and patrilocal (couples settle in the man's village). Payment of a bride price is the common cultural practice and women usually move to another village after marriage. In a census of study villages, 23% of marriages are polygamous and the separation/divorce rate was 3%.There were only seven (0.3%) women who reported being separated/divorced over a one year period in the study sample.

   The average woman in this study sample is 31 years of age, has around 3 years' primary education, 4 children in total and 1 child aged 0 to 5 (see Table A2). Rates of intimate partner violence are high in the area – 61% of women in the study placebo group reported that their partner had committed at least one act of physical violence in the last 12 months.[14] Nearly 50% of men and women aged 15-49 agreed that a husband is justified in hitting or beating his wife for at least one of five specified reasons (United Nations, 2015). The high rates of violence and attitudes that violence is acceptable point to unfavourable social norms for women. However, women do own some share in household assets - an average of 21% of the value of non-land household assets, primarily poultry and small durables, in the study placebo group - and there are no major restrictions on their movement outside the household. This variation highlights the importance of looking at a comprehensive range of outcomes instead of focusing only on one dimension, such as on intimate partner violence.

5.3   Specification

We estimate models of the form:

$$Y_{iv} = \beta_C \cdot \text{Cash Only}_v + \beta_P \cdot \text{Psych Only}_v + \beta_{CP} \cdot \text{Cash}_v \cdot \text{Psych}_v + \alpha_v + \epsilon_{iv}, \qquad (2)$$

where i and v index individuals and villages, Y_{iv} denotes the outcome of interest, $Cash_v$, $Psych_v$, $Cash_v \cdot Psych_v$ are indicators for assignment to the treatment arms: equal to one for villages assigned to receive cash only, psychological only or combined respectively. α_v is a stratification

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[14]Violence was measured using Audio Computer Assisted Interview. Details on the survey method is in Appendix E.

block and sub-location fixed effect (the constant term is subsumed into the vector of fixed effects) and ϵ_{iv} is a village "cluster-robust" standard error.

All outcomes are measured for the respondent or households which received one of the treatment or placebo interventions, so i indexes individuals or households. The parameters of interest are $(\beta_C, \beta_P, \beta_{CP})$, respectively the treatment effects of being assigned to only cash transfer, only psychological intervention, and both interventions together.

We are interested in three primary prespecified classes of variables: asset shares, enterprise ownership and intimate partner violence. For enterprise ownership, there is one focal outcome, if the woman is the primary owner and manager of the enterprise. We create aggregate measures for asset shares and intimate partner violence, as described below. We also display the individual variables that make up the aggregate measures. Within these classes of variables, we adjust the p-values of the coefficients of interest for multiple statistical inference by calculating sharpened q-values that control for the false discovery rate (FDR) for all variables within a class. Rather than pre-specifying a single q, we report the minimum q-value at which each hypothesis is rejected, following Anderson (2008) and Benjamini, Krieger, and Yekutieli (2006). We do not adjust across the variables in different classes because these are conceptually different hypotheses. This is consistent with the literature (Hidrobo, Peterman, and Heise, 2016; Roy et al., 2018) where these are treated as distinct concepts and studies have not corrected across these.

5.4 Effects on Control of Resources and Intimate Partner Violence

Study 1 shows that there is an overall absolute improvement in the household's economic position, in all treatment arms. The size of this improvement differs across treatment arms. Here we focus on unpacking the intra-household impacts occurring alongside this change in household economic position. We look at the effect on two key aspects of household bargaining: women's control of household resources and her experience of intimate partner violence.[15]

We operationalise control over household resources by looking at enterprise ownership and ownership share of household non-land assets. Women do not always manage to get a share in assets acquired by the household from an increase in income. For example, Roy et al. (2015) find that women retain a share of the livestock asset transferred to them but do not get a share in other assets. The increase in resources trigger intra-household bargaining over how these are to be spent, potentially shifting the relative resource control across individuals within the household.[16]

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[15]The sample is composed of married women only. We had pre-specified that we will look at the likelihood of being married at the time of the follow up survey as an outcome to check if the treatment changed the probability of widowhood or divorce/separation. There are 24 women who report being widowed and only seven who report separation/divorce at endline. These are evenly distributed across treatment arms and so none of the treatments seem to have led to a differential change in the marital status.

[16]It is possible that women only care about improvements in the household's absolute economic position and not in her control over resources. We merely seek to study what the effects are and do not make judgements

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5.5 Effects on Control of Resources

There is some indication that the psychological intervention on its own increases women's control of resources: it increases women's share of asset ownership, although not her control of non-farm enterprises. Women are already fairly empowered in their control of resources in the cash group: cash increases their ownership of enterprises and asset shares. Adding the psychological intervention to cash somewhat improves women's control of resources: the combined group control a significantly higher share of assets in non-farm enterprises than the cash group and report more input into production decisions in enterprises (although differences are not significant).

Enterprise Ownership: At the household level, the psychological intervention by itself increases investment to non-farm enterprises and the revenue from these activities (shown for this sub-sample in Column 1 in Table 3). However, all these changes are on the intensive margin: households in this arm are not significantly more likely to open new enterprises (Column 3 in Table 3), but expand existing enterprises instead. Consistent with this, married women receiving the psychological intervention in this sub-sample are not more likely to own any enterprise or own a new enterprise (Column 2 in Table 3; this is the focal outcome in this class of variables). There is also no effect on whether they have a say in decisions about production in or income from non-farm enterprises. It may be that it is difficult for women to assert ownership or decision-making power over existing enterprises, even when these are expanded.

At the household level, there is a significant increase in survival of non-farm enterprises and opening of new non-farm enterprises in the cash and combined groups, such that the likelihood a household owns an enterprise increases (Column 1 in Table 3). Consistent with this, there is a significant increase of nearly 8 percentage points in female ownership of non-farm enterprises compared to the placebo group in both the cash and combined groups (Column 2 in Table 3). This corresponds to a 17% increase on the placebo group mean of 46% and is as large as the effect on the household ownership of enterprises. Half of this increase is due to female ownership of new enterprises opened since the baseline. The rest of the increase in female non-farm enterprise ownership must therefore be coming from women taking ownership of existing enterprises or greater survival of enterprises owned by women.[17] Nearly half of the women in the study placebo group also report ownership of an enterprise suggesting women are fairly empowered in this sphere. Hence, it is not surprising that even without the psychological intervention, women are able to take ownership of some of the investments made in non-farm enterprises using the cash transfer. The effect of the combined intervention is very similar to that of cash alone. However, in columns (4)

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on women's preferences.

[17]Note that we asked at endline who in the household owns and manages non-farm enterprises but not at the baseline. Therefore, we can identify if a new enterprise, that did not exist at baseline, is owned by the woman. However, for enterprises that have survived since the baseline and are owned by the women at endline, we cannot distinguish between women taking over control from someone else in the household or if these were always owned by them.

and (5), we examine two secondary variables measuring a women's input into production decisions and income spending decisions in relation to household enterprises. We see a positive and significant effect only for the combined group on the proportion of woman who have at least some input into production decisions about enterprises and decisions about spending income from enterprises.[18]

Table 3: Impact on Non-farm Enterprise Ownership and Input in Production and Income Spending Decisions

	Household has Enterprise	Enterprise Owner	New Enterprise Owner	Input in Production Decisions	Input in Income spending Decisions
	(1)	(2)	(3)	(4)	(5)
Psych	0.011	0.041	-0.005	-0.006	-0.011
	(0.026)	(0.026)	(0.024)	(0.031)	(0.034)
				[1.000]	[1.000]
Cash	0.076***	0.086***	0.034	0.042	0.049
	(0.026)	(0.026)	(0.023)	(0.032)	(0.032)
				[1.000]	[0.652]
Both	0.079***	0.076***	0.040*	0.072**	0.072**
	(0.027)	(0.027)	(0.023)	(0.030)	(0.031)
				[0.121]	[0.141]
p: cash = both	0.905	0.733	0.791	0.355	0.479
Placebo mean	0.600	0.464	0.230	0.474	0.480
# obs	2840	2840	2840	2939	2939

Note: This table reports ITT estimates from regression of each outcome on a vector of treatment assignments (psych: video and goal-setting, cash: unconditional cash transfer, both: video and goal-setting and unconditional cash transfer), randomisation block and sub-location fixed effects. Household has enterprise is an indicator variable for if the household has an enterprise. Enterprise owner is an indicator for if a household enterprise is primarily owned and managed by the respondent. New enterprise owner is if the woman reports owning an enterprise that was opened since baseline. Production input is an indicator variable for if the woman had a say in at least some of the decisions about the activity. Income input is an indicator variable for if the woman had a say in at least some of the decisions about how to spend the income generated from the activity. Heteroskedasticiy-robust standard errors clustered by village in parentheses. Sharpened q-values are in square brackets. $***p<0.01, **p<0.05, *p<0.1$.

Asset Shares: We measure the share of ownership of an asset as the portion of the money that would come to the woman if the asset was to be sold. We ask women the portion of money they would get if assets are sold for a series of assets categories: large livestock, small livestock, poultry, non-farm enterprise assets, large durables and small durables. To calculate the average share, we take the equally weighted average of the reported shares in these six asset categories. We estimate treatment effects on the average share as well as the share in each asset category.

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[18]As pre-specified, sharpened q values correct over the two variables shown in this table and variables capturing production and income decisions in other areas. These tables are not shown for brevity.

Table 4: Impact on Asset Shares, by Asset Categories

	Average Share (1)	Large Livestock (2)	Small Livestock (3)	Poultry (4)	Non Farm Enterprises (5)	Large Durables (6)	Small Durables (7)
Psych	0.029***	0.010	0.007	0.072***	0.018	0.019	0.051*
	(0.008)	(0.011)	(0.015)	(0.026)	(0.022)	(0.014)	(0.026)
		[0.464]	[0.572]	[0.041]	[0.464]	[0.321]	[0.153]
Cash	0.027***	0.048***	0.036**	0.041**	-0.005	0.045***	-0.006
	(0.009)	(0.012)	(0.016)	(0.025)	(0.020)	(0.016)	(0.029)
		[0.001]	[0.041]	[0.084]	[0.389]	[0.014]	[0.389]
Both	0.036***	0.044***	0.034**	0.037	0.043**	0.050***	0.006
	(0.009)	(0.012)	(0.016)	(0.026)	(0.021)	(0.018)	(0.026)
		[0.002]	[0.046]	[0.070]	[0.046]	[0.013]	[0.229]
p: cash = both	0.319	0.707	0.872	0.878	0.018	0.811	0.638
Placebo mean	0.268	0.063	0.114	0.634	0.116	0.121	0.557
# obs	2939	2939	2939	2939	2939	2939	2939

Note: This table reports ITT estimates from regression of each outcome on a vector of treatment assignments, randomisation block and sub-location fixed effects. Average share is the sum of the share in the asset categories reported in (2) - (7) divided by the total number of asset categories that the household has an asset in. All assets categories are equally weighted in calculating this average share. Each asset category outcome is constructed from respondent response to what portion of money would come to them if the assets in the category were sold. Heteroskedasticiy-robust standard errors clustered by village in parentheses. Sharpened q-values are in square brackets. $***p<0.01, **p<0.05, *p<0.1$.

We find that all treatments increase women's share of household assets by about 3 percentage points on a placebo group mean of 27% (Table 4). In the psychological treatment arm, the main increase comes from the share of poultry with some increase also in the share in small durables. On the other hand, in the cash group, women are able to increase their share in larger assets such as livestock and durables. Although the average effect in the combined group is indistinguishable from that of cash alone, the share of non-farm enterprise assets is significantly higher (Table 4 column 5). This is consistent with the reported increase in say in non-farm enterprise production decisions and how the income earned is to be spent only for the combined group.

5.6 Effects on Intimate Partner Violence

The improvement in the household's and women's economic position is not accompanied by a significant reduction in violence against the woman. We find no significant effect of any treatment on the frequency of physical and sexual intimate partner violence experienced by women in the last 12 months as compared to the placebo group (Table 5). The primary summary measure for intimate partner violence, the IPV index, is a standardised index of the frequency of physical and

Table 5: Impact on Frequency and Incidence of Intimate Partner Violence

	Frequency				Incidence			
	IPV Index	Physical (0-14)	Sexual (0-8)	Injuries (0-6)	Any (0-1)	Physical (0-1)	Sexual (0-1)	Injuries (0-1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Psych	0.030	0.080	0.040	0.010	0.003	0.043	0.005	0.022
	(0.057)	(0.158)	(0.094)	(0.057)	(0.025)	(0.027)	(0.025)	(0.024)
	[.]	[1.000]	[1.000]	[1.000]	[.]	[0.544]	[1.000]	[0.561]
Cash	0.053	0.088	0.106	0.105*	-0.007	0.028	0.017	0.059**
	(0.057)	(0.157)	(0.095)	(0.059)	(0.026)	(0.027)	(0.028)	(0.023)
	[.]	[0.626]	[0.366]	[0.303]	[.]	[0.438]	[0.559]	[0.035]
Both	-0.082	-0.193	-0.128	0.006	-0.050	-0.019	-0.046	0.006
	(0.055)	(0.155)	(0.091)	(0.059)	(0.030)	(0.031)	(0.028)	(0.025)
	[.]	[0.480]	[0.480]	[0.480]	[.]	[1.000]	[0.437]	[1.000]
p: cash = both	0.029	0.095	0.019	0.086	0.144	0.137	0.021	0.028
Placebo mean	0.000	2.431	1.350	0.433	0.744	0.613	0.508	0.211
# obs	2937	2937	2926	2916	2937	2937	2926	2916

Note: This table reports ITT estimates from regression of each outcome on a vector of treatment assignments (psych: video and goal-setting, cash: unconditional cash transfer, both: video and goal-setting and unconditional cash transfer), randomisation block and sub-location fixed effects. All outcomes are measured for the last 12 months. The min-max range of values for each outcome is in brackets below the outcome name. IPV index is constructed as a standardized index (Anderson, 2008) of the frequency of physical and sexual violence. Physical, sexual and injuries frequency are the total scores for response to a series of questions that are answered on a 0 to 2 scale with 0 being never, 1 sometimes and 2 often. All incidence variables are binary indicators for at least one incident of that type of violence in the last 12 months. Any refers to an incident of either physical or sexual violence. Heteroskedasticiy-robust standard errors clustered by village in parentheses. Sharpened q-values are in square brackets. $***p<0.01, **p<0.05, *p<0.1$.

sexual violence.[19]

However, there is some indication that the combined intervention reduces violence relative to receiving cash alone. This IPV index decreases by 0.08 standard deviations in the combined group, in contrast to the cash group, where it increases by 0.05 standard deviations. Neither effect is significant compared to the placebo, but violence is significantly lower in the combined group as compared to those who received just the cash transfer by 0.14 standard deviations. The effect of the combined intervention is also significantly larger than that of the sum of the psychological and cash interventions alone (p-value of 0.048). The difference between cash and combined groups is driven by both the frequency of physical and sexual violence (columns 2 and 3 in Table 5). It appears that there are some synergies in combining the two interventions. This is in line with Roy et al. (2018) who find violence is lower after a combination intervention as compared to cash transfers alone.

Given the generally positive evidence on the impact of cash transfers on violence, the lack

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[19]We pre-specified this standardised index to be the primary outcome measure. In the pre-analysis plan we included the threat of attacking with a knife, gun or any other weapon in experience of physical violence. This has been excluded from the measure of physical violence used in this paper so that we capture actual experience of physical violence only. This does not change the results qualitatively.

of a reduction in IPV in the cash group may seem surprising (Buller et al., 2018). However, as highlighted by Baranov et al. (2019) in a review article, two-thirds of the studies on unconditional cash transfers do not find a reduction in violence. The findings are consistent with the insignificant effect on violence found in a study analysing a similar sized transfer in the same county in Kenya (Egger et al., 2019).

The pattern of incidence of any physical or sexual violence is broadly consistent with that of the frequency of violence (Table 5 columns 5 - 8). To capture the severity of violence, we also ask women about the incidence and frequency of injuries sustained as a result violence in the last 12 months. In the cash group, there is a 6 percentage points significant increase in the likelihood of an injury as compared to both the placebo and combined groups. There is also an increase in the frequency of injuries in the cash group, but this is not very large. Compared to the rate of injuries (21%) in the placebo group, there are an additional 50 women (out of a total of 743 women) in the cash group who report being injured due to violence inflicted by the husband in the last 12 months. In contrast, in the group who receive the psychological intervention as well as cash, there is no increase in injuries.

Since we measure violence using self-reported data, there may be concerns about social desirability bias resulting in under-reporting of violence. This should be balanced across treatment arms and not bias our estimates. In addition, we used Audio Computer-Assisted Self-Interview Software to measure violence so the women did not have to respond to the enumerator. It appears that this did allow women to reveal violence more truthfully in our sample. In face to face surveys with a different sample from the same villages, we find much lower rates, in response to three questions on moderate violence (see Appendix E).

There may also be concerns about treatment induced bias in reporting. In the cash arm, people might worry that if they report violence, the cash transfer may be taken away. However, the NGO partner asked recipients to report conflicts to a toll-free call centre and never stopped the transfer but rather split it in half in case of separation. In the combined arm, there are two additional potential concerns. One, the difference in reported violence between the cash and combined arms is due to perceived labelling by the partner NGO through the video. However, the video made no reference to conflict or violence in the household. Two, the videos showed women leading businesses and in positive consultative relationships with their partners. This may lead women to report lower violence. However, if this was true, the psychological intervention group should report lower violence, which does not occur.

Table 6: Costs of Each Intervention

	Cash	Psych	Combined
I: Marginal intervention costs[a]	**1,957**	**54**	**2,059**
II: Marginal program costs[b]	**2,177**	**274**	**2,279**
III: Average costs at the given scale[c]	**2,177**	**354**	**2,359**

Notes: All values are in 2018 USD PPP. (a) Marginal intervention costs are the costs of cash transfers and the roll-out of the media intervention. They assume all costs incurred prior to the intervention, including programmatic activities such as targeting, as well as the design of the media intervention, to be sunk. (b) On top of intervention costs, marginal program costs include programmatic expenses (e.g., targeting, administration, overhead) that scale roughly in proportion to the number of participants. They exclude the costs of developing the media intervention. (c) Average costs at given scale include the costs of developing the media intervention.

6 Analysis of Intervention Costs

In this section we compare the average cost of each intervention. Table 6 shows the cost estimates. The program costs are averaged over all respondents offered each intervention, rather than all respondents who receive each intervention.

Line I quantifies the marginal intervention costs per targeted participant. This includes only cash transfers, plus those personnel, travel, and material expenses that were incurred in the roll-out of the psychological intervention. This assumes that all pre-intervention costs (including the development of the media intervention, as well as all organisational infrastructure needed to reach the intervention stage - for example, all administrative, census, and targeting activities for both the cash transfer and the psychological intervention) are regarded as sunk.

Line II adds the costs of all administrative, census, and targeting activities to run the implementing agency in country, census all households in villages and register households individually for a programme targeted only at the poorest households. For the cash transfer, this is based on GiveDirectly's existing costs. For the psychological intervention, we assume the same costs (i.e. we assume GiveDirectly were running the same census and targeting but delivering the psychological intervention instead of cash). This accounts for the fact that most organisational costs will in fact scale roughly in proportion to the number of participants reached. However, it is a conservative estimate of costs for the psychological intervention, as GiveDirectly's operational model is unlikely to be the most cost-effective way to deliver the psychological intervention. For example, it might not be cost-effective to target this intervention at poor households, as the costs of a census and then revisiting poor households for registration might be more than the costs of visiting a village once and including all households in the psychological intervention.

Line III adds the average media development cost of about 80 USD PPP per participant. However, this only captures costs for the small number of participants in our study. Had the

study been scaled to all Luo-speaking households in the region who would identify with characters in the video, the cost would be much lower.

7 Acknowledgements

Appendix

A   Details of Sampling, Data and Treatment Assignment

A.1   Eligibility Criteria and Sample of Households

In most study villages, we define a household as eligible to participate in the study if it satisfies at least one of the following criteria, imposed by GiveDirectly:

1. household's per capita housing space is less than 62,000cm^2
2. household has no telephone AND has a mud floor
3. household head is a widow AND has a mud floor
4. household has an orphan child
5. household is homeless

and none of the following criteria, imposed by the research team:

1. household is polygamous (due to difficulties associated with household definition)
2. household head is a child (for consent reasons)
3. household is homeless (due to difficulty finding them)
4. household does not contain an adult female (since the chosen psychological intervention is aimed at adult females)
5. household's GIS coordinates are judged to be incorrect[20]
6. household's per capita housing space is more than 58,000cm^2 (to maximise overlap with the GiveDirectly per capita housing criteria, accounting for measurement error)

In some parts of Siaya, we lowered the per capita housing space cutoff to reflect changes in GiveDirectly's targeting criteria.

In each village, we randomly drew two samples of eligible households from the census: the "target" and "reserve" households. Field officers were instructed to find each target household for the baseline survey. If a target household refused to participate or could not be located (e.g. due to migration), the field officers included one household on the reserve list as a replacement. We define the study sample as all households that completed the baseline survey. The idea of reserve households was used only for the baseline survey. In latter rounds of data collection, households that refused to participate or could not be located are included in the sample and treated as attriters.

We sampled up to 18 target and 6 reserve households in Homa Bay villages. In Siaya, where the villages are typically larger, we sampled up to 24 target and 18 reserves households in some villages and did not impose an upper limit on the number of target households in some other villages. Sampling probabilities vary by village because we don't sample exactly the same share of treatment-eligible households in each village. None of the descriptive or causal results in the paper change substantively when we reweight the data to account for variation in sampling probabilities.

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[20]We flagged a household as having incorrect GIS coordinates using a two-stage process. First, we calculated the median latitude and longitude within each village and flagged any household more than 3km from this joint median. Second, we calculated the mean latitude and longitude within each village excluding the flagged households, calculated the mean distance from each household to this joint mean, and flagged any household whose distance was more than 3 times the mean.

A.2 Data

A.2.1 Household Survey

Our main data source is baseline and endline household surveys. At baseline the respondent is the primary adult woman. We measure household-level economic variables and a range of psychological characteristics of the primary adult woman. The primary adult woman was invited to either the psychological intervention or the placebo intervention. For a few psychological variables, we collected manipulation checks (midline) data immediately after the psychological intervention (or placebo treatment) was administered. Baseline surveys began in April 2016 and ended in March 2017. Endline surveys began at the end of May 2018 and finished in February 2019.

A.2.2 Price Survey

Some variable construction uses price survey data. At baseline, we created a list of all markets in and adjacent to the study area, including information on which days these markets are open. We identified 31 markets in Homa Bay, which were typically open one or two days a week, with some open daily in the evenings. We identified 24 markets in Siaya, about half of which were open daily in the evenings, and the other half of which were typically open two days or evenings each week. We collected price information for the most commonly purchased goods and services at each market at baseline and endline, including food products, livestock prices, livestock input prices, non-food items, services and wages for different types of labour. Price surveys were carried out at the same time as the household surveys. At baseline, this was in August 2016 in Homa Bay and November 2016 in Siaya. At endline, this was in May-June 2018 in Homa Bay and September 2018 in Siaya.

A.3 Tracking

We surveyed village elders about the location of each household and primary female respondent before endline started. If the household or respondent had migrated, we got information on their new location. For households that had split, we surveyed both parts of the household. Those in nearby villages within the study area were included in the tracking sheets for the follow up survey. Those who had moved outside the study areas were surveyed separately between January and February 2019. They were mostly in cities or towns and were invited to central locations and given compensation for travel, or if necessary interviewed in their homes. 94 surveys were done outside the study area.

A.4 Randomisation

We stratified treatment assignment on four variables, all collected during the household census.
1. Location: This is an administrative division in Kenya containing roughly 10-50 villages. We constructed location blocks as pairs of geographically adjacent locations. The first, second, third, and fourth groups of villages contained respectively four, three, three, and two location blocks.
2. Village amenities: We calculated the first principal component of village-level indicators equal to one if the village contains a primary school, high school, market, and clinic (measured in the census). We then created an indicator variable equal to one if the village amenity index exceeded the sample median.[21]

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[21]We constructed the sample median separately for the first, second, third, and fourth groups of villages.

3. Village assets: We calculated the first principal component of household-level indicators equal to one if the household owns a solar panel, television, fridge, iron, radio, watch or clock, telephone, bicycle, motorbike, truck, or car. We then calculated village-level averages of this index and created an indicator variable equal to one if the village asset index exceeded the sample median.

4. Village size: We calculated the number of households in each village, then created an indicator variable equal to one if the village size exceeded the sample median.

This yielded 32, 24, 24, and 8 stratification blocks in the first, second, third, and fourth groups of villages respectively. We then implemented a three-stage stratified random assignment. In the first stage, we randomly assigned villages in each stratification block to the four treatment types in groups of four. If the number of villages in any stratification block was not a multiple of four, then we proceeded to the second stage of the randomization. Here we constructed "large stratification blocks" containing leftover villages that have the same values of the location, amenity, and asset variables but different values of the size variable. We randomized sets of four leftover villages within each of these large blocks. If the number of villages in any large block was not a multiple of four, we then grouped all remaining villages together and randomly assigned sets of four to treatment types. This randomization scheme prioritizes balance on location, amenities, and assets ahead of balance on size. The first group of 107 villages was randomized in April 2016. The second group of 132 villages was randomized in June 2016. The third group of 132 villages was randomized in October 2016. The fourth group of 44 villages was randomized in February 2017.

B Details of Cash Intervention

To ensure correct allocation of individual households to villages where there were no markers of village boundaries, village boundaries were mapped by the IPA survey team and these boundaries were used by both IPA and GiveDirectly. Details of individual control households were shared with GiveDirectly and removed from their system if they were enrolled accidentally. 20 such households were accidentally censused by GiveDirectly. Fewer than 10 non-cash households received cash transfers.

In cash treatment villages, GiveDirectly held a meeting open to all households in the village. The purpose of these meetings was threefold: 1) explaining their programme, 2) describing the organisation and its goals, and 3) informing village residents that GiveDirectly would be working in their village. The eligibility criteria were not disclosed, although households were told that poorer households would be targeted. GiveDirectly emphasised that the transfers were from an independent NGO, and not the result of any government programme.

One GiveDirectly team conducted a census of the village, collecting information on household names, contact information and the variables used to determine programme eligibility. GiveDirectly did not census control villages. The IPA research team conducted censuses in treatment and control villages to determine the sample of eligible households in both groups of villages. In all villages in Homa Bay county and roughly half of villages in Siaya county assigned to receive cash treatment, GiveDirectly conducted their own census after the research team census. Respondents' answers may differ across these censuses. In practice, the study team used exactly the same survey questions and criteria to define eligibility. GiveDirectly administered training to the IPA enumerators and IPA enumerators shadowed the GiveDirectly team during their training so that they knew how GiveDirectly field officers made judgements in difficult cases. In villages where

both censuses were collected, they produced nearly identical results for determining household eligibility: household eligibility status was the same for over 98% of households. But there are some cases where individual households whom study enumerators classified as eligible for treatment are not treated. In the other half of Siaya, villages in the second half of the study, GiveDirectly just used the IPA census data, so overlap is almost exact.

A second GiveDirectly team was given a list of eligible households for the village. They confirmed the household was eligible. If they were, they gave the household information on the programme (including the transfer size and timing, and explained that no conditions were attached to the transfer). This was the first time the household member(s) heard the household had been enrolled. They then registered the household for the programme, if the household consented. Households were told if there was any intra-household conflict about the transfer, they could be disqualified. Households were asked to register for M-Pesa, a mobile money transfer service used to send the transfers. Registration could be done at a network of agents in most small stores. They could receive a mobile phone if they did not have one, with the cost being subtracted from the transfer amount. All registered households were backchecked to confirm eligibility in advance of the transfers going out.[22] There was a GiveDirectly helpline that recipients could contact in case of problems.

C Details of Video Interventions

C.1 Summaries of Documentaries

The psychologically active videos tell stories about the lives of Judy and Josephine. Judy and and her partner Oyoo are smallholder farmers with few assets expecting a child. They discuss struggling to get by and what they want for the future. They decide that within 5 years, they want to put an iron roof on their house, for their children to complete school and for Judy to start a business. They draw a picture to capture these goals. They set intermediate goals to save 100 shillings each week, grow more vegetables to sell at market, purchase a plot within a year. They put money in a small container to save. Judy learns to sew, overcoming some obstacles to do so, and starts a successful business making clothes. They succeed in buying an iron roof. She adjusts her business plan to deal with competition from cheap imported clothing and her business prospers. Eventually, they send their child to university, carrying the picture of their goal.

Josephine is a teacher and farmer. She tells her life story of how she came to be successful. She begins by remembering that she used to beg for money as a child and work as a casual day labourer. Her teacher describes how she dropped out of school several times. Another woman describes teaching Josephine to weave baskets to sell at market. Her husband describes how she saved money from this to go to high school. She explains how she learned conservation farming to improve the productivity of her plot. Her husband describes how she eventually started her chicken-rearing business, despite five failed attempts, including when she overfed her chickens and they did not lay eggs. She outlines plans to build a greenhouse. She also describes her struggles to succeed at teacher training college, when she was much older than others and struggled to learn, but persisted and achieved good grades. She encourages viewers to continue learning throughout their lives.

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[22]These 'backchecks' were conducted on everyone to confirm eligibility. In addition, another audit on a sub-sample that had been flagged for checks was conducted to confirm eligibility.

C.2 Administration and Timing of Psychological and Placebo Treatments

The psychological and placebo treatments were administered by IPA enumerators. These were timed to occur in the same month that (most of) the villages in the location received the first lump sum payment of USD1,016 PPP. They always occurred after cash villages had received token payments. Within cash and placebo and cash and psychological intervention villages, some people received the first lump sum cash transfer before and some received it after participating in the psychological/placebo intervention.[23] In the villages receiving both the cash and psychological interventions, we randomly assigned individuals to receive the psychological intervention soon before or soon after the first lump sum.

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[23]For each location, we counted how many villages were getting cash from GiveDirectly out of all the villages in a location and then assigned the equivalent proportion of the placebo-only and psychological intervention-only villages in that location to receive the intervention in the same month. So, if 100% of the cash villages in a location got a cash transfer in April, all placebo-only and psychological intervention-only villages in that location would also get these interventions in April. If half the cash villages got the cash transfer in March and the other half in April, then a random half of the cash control villages would get the intervention in March and the other half in April. GiveDirectly sent out the transfer approximately around the 15th day of the month.

D Experimental Integrity Tables

Table A1: Study 1: Baseline Summary Statistics and Tests of Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Placebo	Pysch		Cash		Combined		F-test	
	Mean	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	p-value	# obs
Panel A: Village-level Characteristics from Census									
Has Primary School	0.490	-0.010	0.890	0.038	0.581	0.044	0.532	0.828	415
Has Market	0.288	-0.058	0.345	-0.029	0.643	-0.017	0.791	0.809	415
Has Clinic	0.163	-0.077	0.094	-0.048	0.319	0.011	0.829	0.182	415
Number of Households	96.3	-1.50	0.836	-6.74	0.310	2.61	0.725	0.519	415
Mean Household Asset Score	0.030	-0.018	0.723	-0.022	0.664	-0.006	0.901	0.971	415
Floor Material is Mud or Organic†	0.666	0.039	0.031	0.003	0.888	0.005	0.813	0.086	415
Roof Material is Grass, Leaves or Other†	0.054	-0.006	0.502	-0.002	0.858	0.005	0.557	0.625	415
Walls Material is Unburnt Bricks or Mud†	0.846	0.013	0.339	0.009	0.474	0.008	0.579	0.814	415
Drinking Water is Piped/Well†	0.385	0.033	0.420	-0.018	0.653	0.012	0.768	0.663	415
Lighting is Electricity†	0.284	-0.005	0.741	0.002	0.881	-0.011	0.439	0.814	415
Panel B: Eligible Respondent Characteristics from Census									
Married	0.584	0.026	0.194	0.018	0.351	0.003	0.888	0.505	8,309
Age	40.8	-0.003	0.996	-0.876	0.216	-0.816	0.302	0.409	8,302
At Least Primary Education	0.423	0.000	0.991	0.022	0.255	0.016	0.397	0.520	8,274
Household Owns a Mobile Phone	0.741	-0.016	0.320	0.014	0.382	0.003	0.860	0.315	7,743
Panel C: Eligible Household Characteristics from the Baseline									
Household Size	5.31	0.026	0.776	0.028	0.749	0.017	0.855	0.990	8,309
Dependency Ratio	1.35	-0.007	0.840	0.019	0.613	0.016	0.664	0.855	8,308
Number of Household Members Under 16	2.85	-0.005	0.942	0.043	0.521	0.025	0.726	0.859	8,309
Revenue Aggregate	1,834	-70.4	0.404	54.4	0.514	25.5	0.791	0.486	8,311
Consumption Aggregate	4,331	-29.9	0.802	-42.5	0.728	-62.6	0.636	0.970	8,296
Investment Aggregate	699	-137	0.048	-18.1	0.813	-25.4	0.807	0.091	8,311
Non-land Asset Aggregate	1,230	39.9	0.529	42.6	0.536	23.2	0.737	0.914	8,311
Total Household Labour Supply (Days)	431	-1.56	0.927	-0.868	0.958	3.52	0.827	0.993	8,283
Education Expenditure	439	-8.41	0.758	-5.43	0.858	-8.11	0.777	0.990	6,958
Index of Aspirations for Future Outcomes	-0.000	0.013	0.741	0.012	0.739	0.076	0.061	0.244	8,284

Notes: The table reports balance at village and household/respondent level for the sample observed at baseline, by using data from the census and baseline survey. Column (1) reports the placebo mean. Columns (2)-(7) report the coefficients and p-values from a regression of each covariate on assignment to each treatment arm. Column (8) reports the p-value from test of joint significance. Column (9) reports the number of observations. Panel A reports regressions at the village level. Panel B reports characteristics of eligible respondents who are the primary woman in an eligible household. Panel C reports household level characteristics. The average number of households in each village that completes the census is 75. The household asset score is constructed using principal component analysis on indicators for household ownership of a telephone, bicycle, solar panel, TV, fridge, radio, watch/clock, motorbike, truck and iron box (charcoal or electric). Economic variables in Panel C are measured in constant 2018 USD PPP annually. The dependency ratio is the number of household members under 16 divided by the number of members 16 or above. Outcomes with a † sign denote village-level proportions. For village level regressions, inference is performed using heteroskedasticity-robust standard errors while for household/respondent level regressions, inference is performed using heteroskedasticity-robust standard errors, clustered at the village level.

Table A2: Study 2: Baseline Summary Statistics and Tests of Balance

| | Sample Mean | Assigned Cash | | Assigned Psych | | Assigned Both | | F Test |
| | | Coeff | p value | Coeff | p value | Coeff | p value | p value |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	31.61	-1.17	0.03	0.96	0.11	-0.70	0.20	0.53
Own education	3.33	0.03	0.63	-0.10	0.16	0.00	0.95	0.67
Spouse education	3.61	0.02	0.76	-0.09	0.16	0.02	0.74	0.77
No. of children aged 0-5	0.94	0.05	0.30	-0.04	0.39	0.03	0.45	0.69
Total no. of children	3.61	-0.13	0.19	-0.06	0.60	-0.07	0.55	0.30
Household size	6.05	-0.13	0.19	0.05	0.64	-0.04	0.75	0.64 height

Note: The baseline sample size is 3,373. Own and spouse education is 0 if none, 1 if primary 1 to 4, 2 if primary 5 to 6, 3 if primary 7, 4 if primary 8 to secondary, 5 if secondary or higher. Asset aggregate is non-land asset stock of the household including durables, livestock, cash savings and net financial liabilities. Self-efficacy is measured using the Schwarzer and Jerusalem (1995) 10-item scale. Coefficients and p-values are obtained from the regression of the covariate on each treatment indicator and the corresponding test of the null that the coefficient equals zero. The F-test null hypothesis is that the sum of the coefficients of the treatment indicators equals zero. Standard errors are clustered by village.

Table A3: Study 1: Treatment Assignment and Baseline Correlates of Survey Attrition

	(1)	(2)	(3)	(4)	(5)	(6)
	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Psych	0.020	(0.014)			0.054	(0.056)
Cash	0.009	(0.013)			0.032	(0.050)
Combined	-0.001	(0.012)			0.065	(0.049)
Household Size			-0.011***	(0.002)	-0.007*	(0.004)
Age			-0.001*	(0.000)	-0.007	(0.005)
Self-beliefs Index			-0.012***	(0.004)	-0.006	(0.005)
Non-land Assets			0.004	(0.003)	-0.002	(0.005)
Consumption			0.000	(0.001)	-0.000	(0.001)
At Least Primary Education			0.005	(0.008)	-0.000	(0.001)
At Least Secondary Education			0.020	(0.017)	-0.000	(0.001)
Married			0.018*	(0.009)	-0.001	(0.001)
Household Size * Psych					-0.009	(0.008)
Household Size * Cash					0.001	(0.010)
Household Size * Combined					-0.016	(0.011)
Age * Psych					0.002	(0.010)
Age * Cash					-0.000	(0.006)
Age * Combined					0.009	(0.008)
Self-beliefs Index * Psych					0.003	(0.007)
Self-beliefs Index * Cash					0.004	(0.008)
Self-beliefs Index * Combined					0.001	(0.003)
Non-land Assets * Psych					-0.001	(0.004)
Non-land Assets * Cash					0.001	(0.004)
Non-land Assets * Combined					-0.002	(0.004)
Consumption * Psych					0.003	(0.015)
Consumption * Cash					-0.012	(0.021)
Consumption * Combined					0.011	(0.024)
At Least Primary Education * Psych					0.005	(0.023)
At Least Primary Education * Cash					0.045	(0.033)
At Least Primary Education * Combined					-0.025	(0.046)
At Least Secondary Education * Psych					-0.023	(0.048)
At Least Secondary Education * Cash					-0.043	(0.046)
At Least Secondary Education * Combined					0.008	(0.017)
Married * Psych					0.039	(0.028)
Married * Cash					0.000	(0.026)
Married * Combined					-0.002	(0.024)
P: All arms = 0	0.362					
P: cash = psych	0.411					
P: cash = combined	0.433					
P: psych = combined	0.114					
Placebo mean	0.122		0.122		0.122	
# obs	8,309		8,309		8,309	

Notes: Column (1) tests whether attrition differs by treatment arm by showing coefficients from a linear regression of an indicator variable for the household not being surveyed at endline on treatment arm indicators. Column (3) tests whether attrition differs by household- and respondent-level characteristics by showing coefficients from a linear regression of an indicator variable for the household not being surveyed at endline on prespecified baseline covariates. Column (5) shows coefficients from a linear regression including treatment indicators, the prespecified baseline covariates and their interactions. The consumption and asset aggregates are measured in constant 2018 USD PPP ('000s). The self-beliefs index consists of growth mindset, self-efficacy and internal locus of control scales. If a baseline covariate is missing, we replace the missing values with the sample mean and include a missing data indicator. All three regressions include sublocation fixed effects. Heteroskedasticity-robust standard errors, clustered at village level, are reported in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A4: Study 2: Attrition by Baseline Variables

	Control			Cash			Psych			Cash and Psych			Differences		
	In study	Attrited	*p* value	In study	Attrited	*p* value	In study	Attrited	*p* value	In study	Attrited	*p* value	5-2	8-2	11-2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Age	30.89	30.20	0.32	29.99	30.56	0.62	31.73	31.66	0.99	30.47	29.65	0.68	0.86	0.74	0.71
Own years of schooling	3.43	3.24	0.24	3.44	3.33	0.59	3.31	3.32	1.00	3.41	3.36	0.89	0.42	0.67	0.29
Household Size	6.22	5.34	0.00	6.04	5.45	0.04	6.37	5.64	0.01	6.19	5.28	0.00	0.83	0.47	0.82
Total number of children	3.82	3.08	0.00	3.67	2.86	0.00	3.93	3.14	0.00	3.77	2.90	0.00	0.54	0.94	0.48
Number of children aged 0 to 5	0.95	0.00	0.00	1.01	0.00	0.00	0.95	0.00	0.00	0.99	0.00	0.00	1	1	1

Note: Each variable is regressed on an attrition indicator and the p values for the coefficients are reported in columns 3, 6, 9, and 12. The variable is then regressed on all treatment indicators only for the attrited sample. The p-values of the test comparing the control group with the other treatment arms are reported in columns 13-15. Standard errors are clustered at the village level and strata fixed effects are included.

Table A5: Study 1: Baseline and Endline Compliance Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline Sample				Endline Sample			
	Placebo	Psych	Cash	Combined	Placebo	Psych	Cash	Combined
Panel A: Cash and Video Compliance Proportions (%)								
None	16.20	16.29	14.92	11.54	10.64	9.97	7.22	5.80
Cash Only	0.05	0.00	2.59	2.83	0.06	0.00	2.65	2.80
Video Only	83.75	83.62	14.15	14.79	89.30	89.92	13.45	13.03
Cash and Video	0.00	0.10	68.35	70.84	0.00	0.11	76.68	78.38
Panel B: Cash and Video Compliance Tests								
Cash Only	-			0.730	-			0.844
Video Only	0.946			0.764	0.664			0.832
Cash and Video	-			0.485	-			0.538
All	-			0.231	-			0.722
Panel C: Cash Compliance and Reasons for Non-compliance Proportions (%)								
Cash	-	-	70.94	73.67	-	-	79.33	81.17
Refused	-	-	18.13	15.25	-	-	10.64	9.12
Not Found/Home	-	-	2.30	2.32	-	-	1.71	1.58
Moved	-	-	0.96	1.21	-	-	0.66	0.84
Ineligible	-	-	3.07	2.83	-	-	2.87	2.85
Other	-	-	4.60	4.73	-	-	4.80	4.43

Notes: Columns (1), (2), (3) and (4) are statistics for the baseline sample while columns (5), (6), (7) and (8) are for the endline sample. Panel A includes statistics for various types of compliance for each treatment group. Panel B includes the p-values from tests of equality between either the placebo and psych group proportions or the cash and combined group proportions. The p-values are centred between the columns on which the tests are run. The joint test compares equality across all compliance proportions. Panel C includes statistics on the reasons for cash non-compliance amongst the cash and combined treatment groups. Inference is performed using heteroskedasticity-robust standard errors, clustered at village level.

E   Use of ACASI for Measuring Violence

There is 61% incidence of physical violence in the last 12 months in the study placebo group. This is higher as compared to other reports for Kenya which are in the range of 26% in the last 12 months in the Demographic and Health Surveys 2014 and at 29% (last 6 months) for a similar population in Haushofer and Shapiro (2016). The higher rate in this study is likely to be driven by the use of Audio Computer Assisted Interview (ACASI). This is a method used to ask sensitive questions which allows respondents to give their answers privately. The respondents are asked the questions through audio recordings and they respond by selecting the answer choice directly on the tablet. Respondents had to go through several practice rounds to make sure they are comfortable with using tablets and understand the shapes and colours associated with answer choices.

We also asked about incidence of violence from women from ineligible to receive cash transfer households in the study villages and people in the network of the study sample at baseline. We used the same three questions on moderate violence that we used with ACASI but asked these in face to face surveys. We find much lower reports which are more in line with reported statistics,

suggesting that the reason for higher rates is the elicitation method (table A6).

There are two possible explanations for this. One, ACASI was not understood well and therefore the rate are not accurate. In order to test this we used data collected from enumerators after ACASI was administered on the difficulty the respondents had in using it. We find no differences, when we restrict the sample to those for whom the enumerators reported no issues in using ACASI. Two, ACASI allows the respondents the privacy to truthfully reveal whether they have been subjected to any violence. To test if ACASI reports are consistent with other measures in the survey we check the correlation of the IPV index with two dimensions of agency decision making and spousal control and with relationship quality. We find a strong correlation of the IPV index with spousal control (0.38) and relationship quality index (0.49) which indicates that the reports from ACASI are in fact accurate. We dont find a correlation with the decision making index (0.01), but this is consistent with Peterman et al. (2015), Almås et al. (2018) and Zegenhagen, Ranganathan, and Buller (2019) who find that decision making captures an entirely different power mechanism and is in fact negatively correlated with other measures of cooperation or empowerment.

Table A6: Comparison of ACASI and face to face survey incidence rates (%)

	Study Placebo Group	Ineligibles	Network of eligibles
push you, shake you, or throw something at you?	47.6	13.7	17.0
slap you?	43.6	15.1	15.7
twist your arm or pull your hair?	16.8	4.7	7.1
# obs	738	861	534

F   Deviations to Prespecified Variable Definitions

This study was pre-registered on the AEA Registry and includes a pre-analysis plan (PAP). We have made adjustments to the definition of certain outcomes, discussed below.

A minor modification is that we report variables using annual scaling instead of monthly scaling. We scale all variables by a factor of 12 which is equivalent to scaling all treatment effects by a factor of 12 without changing the interpretation of the results. This modification is for ease of exposition in order to make effect sizes more comparable to results in other cash transfer papers.

F.1   Assets

The PAP specifies that the asset aggregate consist of five components: durable assets, livestock, savings, net financial liabilities and stocks of dried maize. We omit the net financial liabilities component from the asset aggregate. We found it more informative to include loans taken and loans given (the components of the net financial liabilities outcome) in separate analysis with household transfers to capture flows into and out of the household.

F.2   Revenue from Economic Activity

The PAP specifies that the revenue aggregate consist of four components: agricultural revenue, livestock revenue, non-agricultural revenue and household labour earnings. The fourth component

consists of earnings from casual work for household members aged 16 and above, earnings from salaried employment for household members aged 16 and above, and remittances received from household members who are migrants. We decided not to consider migrants to be household members, so consider remittances as part of transfers, not a household's revenue and omit remittances from the household labour earnings sub-aggregate. Concerns arose during fieldwork about the data quality of proxy reports from the primary female about migrant household members living away from home. This is consistent with the PAP deviations to investment and labour supply, where migration costs and labour supply to migration are omitted respectively. Lastly, for revenues from enterprises, a component of the non-agricultural revenue sub-aggregate, we pre-specified a recall period of the last 30 days the enterprise was in operation. Our pre-specified measures of enterprise investment and labour supply use a recall period of the last 30 days so we modify enterprise revenue to use the last 30 days as well.

F.3 Investment into Economic Activity

The PAP specifies that the investment aggregate consist of three components: agricultural input expenditures, livestock input expenditures and non-agricultural expenditures. The third component consists of enterprise expenditures, spending on community group economic activities and household member's migration expenditures. We omit migration expenditures from the household's non-agricultural expenditures sub-aggregate. This is consistent with the PAP deviations to revenue and labour supply, where remittances and labour supply are omitted respectively. We have renamed "investment into economic activity" to "inputs and hired labour" in order to convey more clearly what is included in this outcome.

F.4 Investment into Human Capital

There are two deviations from the PAP. The PAP specified an index consisting of two components: education expenditure (total expenditure on education divided by the number of children in the household) and participation (total number of school days attended over the last five days school was in session divided by the number of children in the household). Firstly, instead of creating this index, we chose to instead report education expenditure as the summary measure for human capital investment. Secondly, our measure of education expenditure is not normalised to a per child basis and we instead report the total household expenditure on education. This enables us to include this figure in cost-benefit analysis.

F.5 Labour Supply

There are three deviations from the PAP. The PAP specified an index consisting of four components: labour supplied to household agricultural activities, household livestock activities, household non-farm enterprises and activities outside of the household. Labour supplied outside of the household consists of three further sub-components: casual work, salaried employment and migration. First, we omit labor supplied to migration. Therefore in the paper, labour outside of the household is defined as an aggregate of labour supplied to casual work and salaried employment. This is consistent with the PAP deviations to the revenue and investment aggregates where remittances and migration costs have also been omitted, respectively. Second, given our interest in examining the economic dynamics of various household activities, we construct a labour supply aggregate measured in days instead of an index. We scale labour supplied to each activity to annual figures.

For outcomes with a recall period of 4 weeks or 30 days, we multiply by 12. For agricultural labour supplied in the long rains, we scale up by a factor of 1.5 to represent labour supplied in both the long rains and the short rains from October-December.[24] Third, the pre-registered analysis includes labour supplied by all household members. We instead only count days of labour supplied by adults (household members aged 16 or above). This is to make the labour measure consistent with the PAP revenue aggregate, which only includes labour earnings from adults.

F.6 Consumption

There are two deviations from the pre-registered analysis. Firstly, the PAP specifies that the consumption aggregate consist of five components: food consumption, non-food non-durable consumption, expenditure on durable goods, social expenditure and education expenditure (scaled to adjust for seasonality in education expenditures). Given that we include education expenditure as a main outcome, we omit the education component from the consumption aggregate so that when comparing education and consumption expenditure aggregates, we are not double counting. Secondly, the PAP definition is scaled to 30 days; we change our scaling period such that the values are scaled to annual figures by multiplying the values by 12. This enables comparison with other cash transfer papers.

F.7 Aspirations

There are no deviations from the PAP for the asset and education aspirations outcomes. The income aspirations outcome in the PAP had a monthly scaling; we now report using annual scaling, scaling up by 12 for all households. The aspirations index, the pre-registered summary measure, is unchanged as our methodology for index creation is scale invariant.

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[24]For livestock and non-farm enterprises, we collect days of labour supplied in the last four weeks while for agriculture, we collect data for the "long rains season", between March and May. Labour supplied to casual work and salaried employment is collected over the last four weeks. Under our pre-specified index, differences across activities in the time horizon for which we collected labour information were irrelevant, given that the index is unit-less by design.

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