Evaluating Alternative Cash Transfer Designs in Kenya Using Behavioral Economics

Carolina Kansikas
University of Warwick

Anandi Mani
University of Oxford

Paul Niehaus
UC San Diego

January 24, 2023*

*Kansikas: Unversity of Warwick. Mani: University of Oxford. Niehaus: University of California, San Diego and NBER. Niehaus is a co-founder, former president (2012-2017), and current director of GiveDirectly. We are grateful to Sendhil Mullainathan and Anuj Shah for their partnership in the early stages of this project. Thanks to Muqing Zhou and Federica Esu for outstanding research assistance with field work and to Samuel Krumholz and David Emes for help with data analysis. We gratefully acknowledge financial support from the Wellspring Foundation.

Introduction

This document reports findings of our study in collaboration with the NGO GiveDirectly on the effects of its various cash transfer designs. A related paper reproduces some of these results along with results based on modified specifications and additional results on recipient preferences. The specifications here are those defined in pre-analysis plan (PAP) AEARCTR-0000541 as registered on the AEA RCT registry, with any changes made out of necessity noted below. For ease of reference we reproduce the original PAP text in its entirety in black, with new discussion and commentary on the results in blue.

2 Average effects of treatment

2.1 Effects of tranche structure

We seek to estimate two quantities: the effect of receiving funds in 2 tranches as opposed to 1, and the effect of being assigned one's preferred tranching structure. To calculate these we estimate

$$Y_i = \alpha + \beta_1 L_i(\tilde{L}_i) + \beta_2 L_i(1 - \tilde{L}_i) + \gamma \tilde{L}_i + \epsilon_i \tag{1}$$

Here L_i ($\tilde{L}_i$) indicates whether recipient i was assigned to (preferred to) receive payment in a single lump sum, as opposed to two installments. Note that this design accounts for the fact that randomization was stratified on preferences, so that we do not cluster standard errors by preference. We estimate this model using the the 90% of subjects who were assigned to a tranche structure chosen at random (and thus were *not* assigned to have their preferred tranche structure implemented for sure). We then test

$$\rho^1 \beta_1 + \rho^2 \beta_2 = 0 \tag{2}$$

$$\rho^1 \beta_1 - \rho^2 \beta_2 = 0 \tag{3}$$

where ρ^p is the fraction of subjects preferring option p. The first test measures the average impact of getting a single lump sum transfer as opposed to two; the second measures the average impact of getting your preferred tranche structure as opposed to not. We also estimate models that fully interact the right-hand side of (1) with the (discretized) baseline value of the dependent variable, and then calculate the average treatment effect as the weighted sum of the category-specific effects weighted by the size of each category for net assets, income and expenditures.

When examining the impact of receiving a single tranche as opposed to two, our focus is on whether these structures lead to different uses of money and thus different aggregate patterns in household income statements and balance sheets, and also whether experiencing a given transfer structure affects recipients preferences. We prioritize the following outcomes:

- Net assets (and its subcategories)
- Net income (and its subcategories)
- Annualized expenditure including net transfers (and its subcategories)

Table 1: Effect of Tranche Structure

	(1) Net assets	(2) Income	(3) Expenditure	(4) Retrosp. Pref 1	(5) RVP
$\rho^1\beta_1 + \rho^2\beta_2$	-17405.6	-7426.9	-3085.2	-0.0402	70706.3
	(45594.7)	(15285.8)	(18863.0)	(0.0567)	(88741.1)
p-value	0.908	0.724	0.805	0.84	0.159
FDR q-value	1	1	1	1	1
N	426	422	426	426	420
Mean	230543	107318	159672	.75	207903

Note: The table presents results on the effect of receiving a single lump-sum transfer relative to two tranches, estimated using equation 2. Standard errors clustered by are reported in parentheses. They are not clustered by preferences, since randomization was stratified by tranche preferences. Rows 3 report p-values for the cofficients in each column, followed by sharpened q-values adjusted for multiple hypothesis testing following Benjamini et al. (2006). Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

- Retrospective transfer preferences
- Retrospective valuation of things purchased

Table (1) reports the results. Across all five outcomes we see no significant differences between those who received one as opposed to two tranches. We also cannot rule out large differences for several outcomes, so would not view these results as establishing tight equivalence between the two structures. At the same time, we note that recipients overwhelming preferred one *or* two tranches to four or twelve tranches, which suggests that differences in impacts between less similar tranching structures might be more pronounced. This study was not optimized to detect such effects in that GD intentionally to chose to implement only the two most popular tranching options. For the outcomes we also observe at baseline, results are substantively unchanged when we estimate models interacted with the discretized baseline value (Table A.9).

When examining the impact of receiving one's preferred transfer structure, our focus is on understanding whether giving recipients control over transfer structure leads to outcomes that they prefer. We therefore prioritize the following outcomes:

- Index of financial stresses
- Index of social input into decision-making
- Index of deliberation in decision-making
- Satisfaction with choices
- Index of regret
- Retrospective valuation of things purchased
- Index of progress towards goals made since transfer

Within the subcategories noted above we will report both the usual standard errors and also p-values corrected to control for the false discovery rate.

Table (2) reports the results. Again we see little evidence of economically meaningful effects; there is no clear pattern in the signs of the estimates, effect sizes are small, and most are insignificant. The one exception is the goal progress index, where we see a small *negative* effect

Table 2: Effect of Preferred Tranches

	(1) Financial Stress	(2) Social Input	(3) Deliberation	(4) Sat. Money Use	(5) Regret	(6) Goal Progress
$\rho^1\beta_1 - \rho^2\beta_2$	0.0215	0.0106	-0.00176	-0.0155	-0.000466	-0.0383**
	(0.0245)	(0.0170)	(0.00790)	(0.0174)	(0.0262)	(0.0175)
p-value	0.381	0.534	0.823	0.373	0.986	0.030
FDR q-value	1	1	1	1	1	0.217
N	431	427	426	427	425	427
mean	.62	.59	.53	.90	.40	.81

Note: The table presents results on the effect of receiving one's preferred tranche structure - either through random assignment to a structure, or random assignment to the 10% sample receiving their preferred tranches– estimated using equation 2. Standard errors are reported in parentheses. They are not clustered by preferences, since randomization was stratified by tranche preferences. Rows 3 report p-values for the cofficients in each column, followed by sharpened q-values adjusted for multiple hypothesis testing following Benjamini et al. (2006). Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

of receiving one's preferred tranche structure, but this result does not survive FDR control.[1] Overall, and consistent with the (absence of) clear patterns above, it appears that the one- and two-tranche structures were not sufficiently differentiated to generate detectable differences: both delivered money in tranche sizes very large relative to those recipients would otherwise typically receive, and relative to those in the 12-month design that GD ultimately did not deliver (because it was widely unpopular). How the impacts of that design would have contrast with those here remains an open question for future work.

2.2 Effects of timing

First, we examine whether receiving money earlier / later leads to different uses and outcomes. We estimate

$$Y_i = \alpha + \beta_t T_i^a + \epsilon_i \quad (4)$$

where T_i^a is the average number of months delay from the scheduled start of payments overall to the date on which recipient i was assigned to be paid. For recipients who were assigned to receive a single tranche this is simply the month on which they were scheduled to be paid; for those assigned to receive two tranches it is the average of the two months on which they were scheduled to be paid (e.g. a recipient scheduled to receive payments in months 2 and 7 would have $T_i^a = \frac{2+7}{2} = 4.5$). We estimate this using the 90% of recipients who were assigned a timing at random, excluding the 10% who were given their preferred timing. We first estimate pooling those who received transfers in one and in two tranches, but also estimate for each sub-group separately. We cluster standard errors by recipients' stated timing preferences (12 possibilities for single-tranche recipients and 6 for two-tranche recipients, for a total of 18 possible values) on which randomization was stratified. We will also estimate an analogous specification with a quadratic term in T_i^a to test for any non-linearities in the effect of timing.

Note that this model will be mis-specified if the effects of delay are non-linear (as indeed appears to be the case in the data). To see the issue, note that T_i^a measures the average delay (contingent on structure) in receiving the transfer. For the group assigned to receive

[1]This effect is also driven entirely by the effect of receiving one tranche on recipients that preferred one tranche (and not by the effect of receiving two on those who preferred two). These recipients also report experiencing more stress and deliberating less over how to receive their transfers (Table (A.10)), perhaps suggestive of the idea that they bit off a bit more than they could chew.

one tranche the average delay is also the actual amount of time elapsed before they received money. But the group assigned to receive two tranches did not in fact receive *any* money at T_i^a; their two tranches arrived at $T_i^a - 3$ and $T_i^a + 3$ months, respectively. If the effects of delay on treatment effects are non-linear then this is not equivalent to receiving both halves of the money at time T_i^a. As a result (4) is potentially misspecified, and in a way that will tend to conflate effects of timing with effects of tranching.[2] For completeness we nevertheless report results here using the original specification, and refer readers to Kansikas et al. (2022) for a less restrictive specification.

When examining the impacts of transfer timing, our focus is on understanding (a) simple economic dynamics (e.g. does more recent receipt imply higher expenditure and lower asset accumulation), and (b) how the time lag between notification and receipt of transfer affects planning and decision-making. We therefore prioritize the following outcomes:

- Net assets (and its subcategories)
- Net income (and its subcategories)
- Annualized expenditure including net transfers (and its subcategories)
- Index of social input into decision-making
- Index of deliberation in decision-making
- Retrospective valuation of things purchased

Table 3 reports results for both linear and quadratic versions of (4). We see some evidence of positive effects from delay, particularly for the net assets and deliberation outcomes.[3] In almost all cases the point estimates from the quadratic specifications imply that these benefits are eventually reversed, as outcomes are first increasing and then decreasing in delay. That said, confidence intervals are wide, and (more importantly) the specification itself is subject to the identification concerns noted above. We therefore caution against drawing any strong conclusions.

[2] Both the interpretation of T_i^a and its support differ with respect to assigned tranching; in the one-tranche arm T_i^a can vary from 0 to 11, while in the two-tranche arm it can only vary from 2.5 to 8.5.

[3] We also see some potentially interesting patterns in asset sub-categories, with delay associated with more asset value in land and less in tools or other durable goods, which suggests that it facilitates investment in larger, lumpier assets (Table A.11). We see no clear patterns in subcategories of income (Table A.12) or expenditure (Table A.13).

Table 3: Impact of Delay

Variables	(1) Net Assets	(2) Net Assets	(3) Income	(4) Income	(5) Expenditure	(6) Expenditure	(7) Social Input	(8) Social Input	(9) Deliberation	(10) Deliberation	(11) RVP	(12) RVP
T_a	5,082*	12,784	-493.7	7,725	-2,289	-3,193	-0.000749	0.0152	0.0206***	0.0884***	805.8	19,368
	(2,494)	(9,417)	(1,005)	(6,037)	(1,467)	(4,742)	(0.00291)	(0.00871)	(0.00377)	(0.0163)	(4,967)	(42,574)
T_a^2		-697.8		-744.4		81.88		-0.00144*		-0.00614***		-1,684
		(917.6)		(585.2)		(448.6)		(0.000717)		(0.00166)		(3,396)
Constant	211,537***	195,408***	110,762***	93,585***	171,172***	173,065***	0.600***	0.566***	-0.516***	-0.658***	211,353***	172,597*
	(13,794)	(18,614)	(5,132)	(13,045)	(6,684)	(9,428)	(0.0102)	(0.0202)	(0.0415)	(0.0320)	(23,122)	(93,055)
N	424	424	420	420	424	424	424	424	422	422	417	417
Mean	230543	230543	107318	107318	158627	158627	.59	.59	.53	.53	207903	207903

Note: The table presents results from a regression of assigned delay on net assets, net income, annualized expenditure including net transfers, and index of social input into decision-making, index of deliberation in decision-making, retrospective valuation of things purchased. The regression is estimated using the specification in equation 4. Standard errors are reported in parentheses, clustered at the delay preference level. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

Second, we examine whether getting money at the time the recipient wanted it led to different subjective satisfaction with the results. Conceptually, we wish to estimate

$$Y_i = \alpha + \beta 1(|T_i^a - T_i^p| < \kappa) + \epsilon_i \tag{5}$$

As above, T_i^a is the (randomly assigned) date on which subject i was scheduled to receive their average dollar, while T_i^p the date on which they preferred to receive it. κ is a tuning parameter which determines what constitutes a "match;" we define $\kappa = 1$ initially but will also explore sensitivity to looser definitions. Note, however, that β in (5) is experimentally identified only conditional on T_i^p, which is endogenous. We therefore fully interact (5) and report the (bin-size weighted) average value of β across all values of T_i^p as the statistic of interest.

Note that while (5) does capture the effect of receiving one's preferred timing, the interpretation of this effect is nuanced given the actual distribution of preferences we observe. Specifically, most subjects preferred to receive transfers as soon as possible, or with only a short delay. As a result, one should interpret estimates of β in (5) as largely capturing the effects of receiving transfers without delay. Given that—as we have seen above—some delay appears to be beneficial, there are reasons to think that "getting exactly what you want" on this dimension need not necessarily be beneficial. (See also Kansikas et al. (2022) for related results and discussion.)

As with tranching preferences, our primary interest in exploring the effects of timing structure is to understand whether giving recipients control over transfer structure leads to outcomes that they prefer. We therefore prioritize the following outcomes:

- Index of financial stresses
- Index of social input into decision-making
- Index of deliberation in decision-making
- Satisfaction with own money use choices
- Index of regret
- Retrospective valuation of things purchased
- Index of progress towards goals made since transfer

Table 4 reports the results. Across all values of κ, the one strong and robust pattern is that recipients who received a transfer around the time they wanted it report more progress overall with respect to their goals (Column 6). For small values of κ we also see that they gathered less social input before deciding how to use the money (Column 2), though they did not deliberate less themselves (Column 3). Finally, we see some interesting but inconclusive patterns with respect to the retrospective valuation of items purchased: for low values of κ receiving transfers at the desired time is associated with lower valuations, while for higher values of κ it is associated with higher valuations. If nothing else this highlights the difference between monetary valuation and subjective goal progress as measures of success.

Generally speaking, we interpret this mixed set of results as reflecting the factors discussed above: getting one's preferred timing may be helpful at times, but in this case this almost always meant less delay, where delay itself was generally helpful.

Table 4: Effect of preferred timing

	(1) Financial Stress	(2) Social Input	(3) Deliberation	(4) Satisfaction Use	(5) RVP	(6) Regret	(7) Goal Progress
$\kappa = 1$	-0.0173	-0.0744***	0.00358	-0.0717	-67825.5*	0.0106	0.0329***
	(0.0236)	(0.00840)	(0.0163)	(0.139)	(38093.4)	(0.144)	(0.00536)
Constant	0.628***	0.612***	0.528***	4.568***	243874.6***	1.409***	0.796***
	(0.0149)	(0.00643)	(0.00938)	(0.0117)	(27500.2)	(0.0140)	(0.00640)
$\kappa = 2$	0.0214*	-0.0319***	-0.00143	0.0516	-76282.8**	0.0618	0.0489***
	(0.0117)	(0.00791)	(0.00485)	(0.0657)	(33488.8)	(0.0673)	(0.00828)
Constant	0.618***	0.607***	0.529***	4.540***	254066.5***	1.393***	0.788***
	(0.0147)	(0.00699)	(0.00799)	(0.0170)	(30304.5)	(0.00806)	(0.00746)
$\kappa = 4$	0.00222	0.00899	0.000417	0.122***	35697.2***	0.0904	0.0326***
	(0.0335)	(0.0256)	(0.00367)	(0.0238)	(4676.4)	(0.0704)	(0.0106)
Constant	0.623***	0.593***	0.529***	4.488***	211427.6***	1.361***	0.784***
	(0.0301)	(0.0183)	(0.00868)	(0.0223)	(21920.3)	(0.0312)	(0.0111)
N	419	419	418	419	414	418	419
Mean	0.625	0.598	0.529	4.555	231283.4	1.411	0.802

Note: The table presents results of assignment of preferred timing on end line outcomes of participants, estimated using the specification at equation 5. Here $\kappa \leq 1$ denotes receiving the transfer in a recipient's preferred month. For t =4 and 7, $\kappa \leq t$ denotes that the transfer was received within $t-1$ months of the recipient's preferred timing. Standard errors clustered at the delay preference level are reported in parentheses. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

2.3 Effects of information treatments

Conceptually, we think of the probability that individual i invested in asset a as a function of the difference between their priors about that asset and any information they are experimentally assigned to receive:

$$Y_{ia} = \alpha_a + \delta_a I_i^p + \beta_p I_{ia}^p * (\hat{P}_a - \tilde{P}_{ia}) + \beta_r I_{ia}^r * (\hat{R}_a - \tilde{R}_{ia}) + \gamma_p \tilde{P}_{ia} + \gamma_r \tilde{R}_{ia} + \epsilon_{ia} \quad (6)$$

Here y_{ia} indicates whether individual purchased asset a; I_i^p indicates whether the individual was given information about popularity (as opposed to information about returns); I_{ia}^p (I_{ia}^r) indicates whether individual i was given information about the popularity of (returns on) investment a; $\hat{P}_a$ is the popularity score communicated to individuals who were given the popularity treatment, $\tilde{P}_{ia}$ is the individuals' self-reported prior about that score, and $\hat{R}_a - \tilde{R}_{ia}$ is analogously the difference between the individuals' priors and the message delivered about net returns. Intuitively, this models behavior as a function of the surprise contained in any information delivered (as opposed to the delivery of information per se). The remaining variables condition on priors, so that identification of the parameters (β_p, β_r) of interest is driven purely by experimental assignment.[4]

We estimate this relationship using the full sample of households. We first estimate it pooling all five investments a and multi-way clustering standard errors at the individual and stratum level (as randomization was stratified on predicted probability of being subsequently found ineligible). We then estimate it separately for each investment, in this case clustering standard errors by stratum and reporting both the usual standard errors and also p-values corrected to control the False Discovery Rate (FDR) within the group of 4 investments.

For each estimation, we estimate both (6) and a model that fully interacts this specification with a categorical variable from the baseline survey indicating the individual's prior stated likelihood of making investment a. This variables take on values 0 (no plans to invest), 1 (low priority), or 2 (high priority). We do not pre-specify plans to condition on prior ownership of these assets as ownership of any of the five is uncommon in our baseline.

As a robustness check, we will also examine whether results from probit estimation of these models are meaningfully different.

We report results in Table 5. The estimated effects of information about the popularity of and returns on investments are positive, but not significantly different from zero and also small in economic terms. (For example, the mean of $\hat{R}_a - \tilde{R}_{ia}$ is $-28,188$, i.e. most subjects had priors that were optimistic relative to our estimated actual returns, and so the implied average impact on the probability of purchase in column one is $6.4e - 07 \times -28188 = -0.018$ relative to a mean probability of 0.32.) These conclusions are generally the same across a linear model (Column 1), probit model (Column 2), or a model interacted with a baseline measure of likelihood of purchasing (Column 3), though in the last case we do see some evidence that information about returns had a significantly more positive impact on decisions for subjects who had indicated at baseline that purchasing the asset was a high priority (captured by the

[4] Note that this specification omits two control variables, $I_{ia}^p \tilde{P}_{ia}$ and $I_{ia}^r \tilde{R}_{ia}$, included in Equation (6) in the PAP, as these are not separately identified from the other controls and fixed effects and hence are redundant.

interaction with PI. Results disaggregated by assets are in Column 4; there is some evidence that information about returns was important for two of the most frequently-purchased assets (chicken and bricks). These conclusions are unchanged if we focus on sharpened q-values (Table A.14).

Table 5: Effects of information treatment

	(1) Pooled linear	(2) Pooled probit	(3) Investment prior interaction	(4) Disaggregated investment
I^p	0.054	0.038	0.16	0.065
	(0.088)	(0.073)	(0.11)	(0.11)
$(P_a - P_{ia})$	0.0032	-0.00072	0.012	
	(0.014)	(0.013)	(0.016)	
$I^p \times (P_a - P_{ia})$	0.0060	0.0048	0.0067	
	(0.016)	(0.015)	(0.022)	
$(R_a - R_{ia})$	9.6e-07*	6.5e-07	1.9e-06***	
	(5.1e-07)	(4.2e-07)	(6.8e-07)	
$I^r \times (R_a - R_{ia})$	6.4e-07	6.0e-07	-1.1e-06	
	(6.0e-07)	(5.6e-07)	(8.6e-07)	
PI			0.023	
			(0.099)	
$I^p \times PI$			-0.23*	
			(0.12)	
$(P_a - P_{ia}) \times PI$			-0.0077	
			(0.018)	
$I^p \times (P_a - P_{ia}) \times PI$			-0.0070	
			(0.023)	
$(R_a - R_{ia}) \times PI$			-7.6e-07***	
			(2.9e-07)	
$I^r \times (R_a - R_{ia}) \times PI$			2.3e-06***	
			(6.5e-07)	
$I^p \times (P_a - P_{ia})$ chicken				-0.011
				(0.023)
$I^p \times (P_a - P_{ia})$ water				-0.020
				(0.021)
$I^p \times (P_a - P_{ia})$ bricks				-0.031
				(0.021)
$I^p \times (P_a - P_{ia})$ fence				0.034*
				(0.018)
$I^r \times (R_a - R_{ia})$ chicken				1.4e-06***
				(5.0e-07)
$I^r \times (R_a - R_{ia})$ water				-3.0e-07
				(5.6e-07)
$I^r \times (R_a - R_{ia})$ bricks				2.9e-06***
				(1.0e-06)
$I^r \times (R_a - R_{ia})$ fence				8.9e-07
				(1.0e-06)
N	358	358	358	356
Mean	0.32	0.28	0.32	0.32

Note: The table reports estimation results from equation 6, comparing the effects of the popularity information treatment to the cost-benefit information treatment on pooled investment in the categories on which information was provided (chicken, water pumps, bricks, and wire fences) at endline. Column (1) presents results from a pooled linear model, column (2) presents results from a probit model, column (3) presents results from a linear model with interactions with investment priors (0= not on list, 1=low priority, 2=high priority); column (4) presents results disaggregated by asset. I^p refers to whether the individual was assigned to the popularity treatment, as opposed to the cost-benefit treatment. The surprise terms I^p_{ia} (I^r_{ia}) and $\hat{R}_a - \tilde{R}_{ia}$ reflect differences between information and individuals' priors. Robust standard errors in parentheses. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

We are also interested in the impacts of information treatments on the decision-making process itself. To examine this we estimate

$$Y_i = \alpha + \beta_p I_i^p + \epsilon_{ia} \tag{7}$$

where Y_i is an outcome characterizing the decision-making process and I_i^p indicates whether individual i received information about popularity (since all individuals received information either about popularity or about returns, the implicit "control" in this specification are the latter). In this specification, β_p measures the effect of being given popularity information as opposed to returns information. Our outcomes of interest here are

- Index of social input into decision-making
- Index of deliberation in decision-making

Table 6: Effects of popularity information treatment

	(1) Social Input	(2) Deliberation Index
I^p	-0.0024	-0.0064
	(0.016)	(0.0076)
N	483	481
Mean	0.59	0.53

Note: This table presents estimated effects on assignment to the popularity treatment (as opposed to the returns treatment) on measures of decision-making captured at endline: social input in decision-making and deliberation index, following equation 7. Robust standard errors in parentheses. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 6 presents results. We see that receiving information about popularity (as opposed to returns) slightly lowers the social input index (as well as overall deliberation), as one might expect if popularity measures already embed some of the information that one might otherwise obtain directly from social contacts. The effect is not large or statistically distinguishable from zero, however.

2.4 Effects of "ready cash"

Finally, we examine whether recipients' outcomes were affected by the timing of the "ready cash" they were given prior to expressing their preferences over transfer structure. We estimate

$$Y_i = \alpha + \beta_L L_i + \epsilon_i \tag{8}$$

Here L_i indicates whether the individual received their ready cash at a later date, immediately before expressing their preferences; β_L is thus the effect of having recently received a small amount of cash. We focus on whether this affected how the recipient made decisions about using the transfer as well as their ultimate satisfaction with the results:

- Index of social input into decision-making

Table 7: Effect of Recent token transfer

	(1) Deliberation	(2) Social input	(3) Satisfaction Money Use	(4) Regret	(5) RVP	(6) Goal Progress
Recent Token	-0.00130	-0.00623	-0.00622	-0.0172	61505.1	-0.0124
	(0.00570)	(0.0206)	(0.0118)	(0.0218)	(41743.4)	(0.0140)
Constant	0.531***	0.598***	0.901***	0.413***	177474.4***	0.814***
	(0.00283)	(0.0102)	(0.00585)	(0.0108)	(20652.0)	(0.00695)
N	481	483	483	481	475	483
mean	0.531	0.597	0.902	0.412	177095.8	0.815

Note: The table above presents results on the effect of late token transfer on participant outcomes, estimated using Equation 1 with village-level fixed-effects. Robust standard errors, clustered at the village level, are reported in parentheses. The first column presents treatment effects of receiving cash more recently on participants' deliberation, the second column presents results on social input in decision-making, the third column presents results on satisfaction in use of money, the fourth column presents impacts on regret in cash transfer use, the fifth column presents results of the effects of ready cash on retrospective valuation of things purchased, and the sixth column presents results on participants' goal progress, including progress on income, asset and social status goals. Please refer to the main paper for results on preference for delay and preference for tranche structures. Robust standard errors in parentheses. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

- Index of deliberation in decision-making
- Satisfaction with own money use choices
- Index of regret
- Value attached to things purchased
- Index of progress towards goals made since transfer

Table 7 presents results. We do not detect significant effects on this set of outcomes, which is (in retrospect) not surprising given that these outcomes were measured at endline, roughly 18 months after token transfers were received. Kansikas et al. (2022) show that more recent token transfer receipt did significantly impact several outcomes *at the time of the preferences survey*, days or weeks after token transfers were delivered. These include both measures of cash on hand and financial stress as well as preferences over the structure of the main transfer. Since only 10% of households were (randomly) selected to have these preferences implemented, however, there was relatively little scope for impacts on preferences to translate into impacts on subsequent outcomes.

3 Attrition

We will test whether attrition from the endline survey was differential with respect to treatment status. If so, we will additionally report Lee bounds for the parameters described above.

Table 8 reports estimated effects of receiving one's preferred timing (Column 1) and tranching (Column 2) on the rate of attrition from the endline survey. In neither case do we significant effects on either the level or the composition (with respect to preferences) of attrition.

Table 8: Attrition

	(1) Tranches	(2) Delay
Assigned preferred # of tranches	-0.00726	
	(0.0630)	
Assigned 1 Tranche	-0.00198	
	(0.111)	
Assigned 2 Tranches	0	
	(0)	
Assigned preferred months of delay		0.0185
		(0.0415)
Assigned 0 months of delay		-0.0198
		(0.0444)
Assigned 1 month of delay		0.0307
		(0.0890)
Assigned 2 months of delay		-0.00292
		(0.0654)
Assigned 3 months of delay		0.0479
		(0.103)
Assigned 4 months of delay		0.0134
		(0.0610)
Assigned 5 months of delay		-0.00471
		(0.0844)
Assigned 6 months of delay		-0.00292
		(0.0109)
Assigned 7 months of delay		-0.00292
		(0.0154)
Assigned 8 months of delay		0.0497
		(0.0531)
Assigned 9 months of delay		-0.0556
		(0.0693)
Assigned 10 months of delay		-0.00292
		(0.00453)
Assigned 11 months of delay		0
		(0)
N	512	512
F-statistic	0.000317	0.07
p-value	0.986	0.935

Note: The table presents attrition by treatment arm. The outcome in the regression is attrition at endline, independent variables are the treatment arms, divided into exhaustive and mutually exclusive categories. "Preferred timing" refers to allocation to receive the cash transfer with preferred delay, "Preferred tranches" refers to allocation to receive the transfer according to preferred cash transfer structure. The F-test and p-value reported at the table bottom refers to a joint orthogonality test for all treatment arms. Standard errors are reported in parentheses. Standard errors in parentheses. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

References

Benjamini, Yoav, Abba M Krieger, and Daniel Yekutieli, "Adaptive linear step-up procedures that control the false discovery rate," *Biometrika*, 2006, *93* (3), 491–507.

Kansikas, Carolina, Anandi Mani, and Paul Niehaus, "Customized cash transfers: financial lives and cash-flow preferences in rural Kenya," Technical Report, UC San Diego 2022.

A Additional results

Table A.9: Effects of Tranche Count, Aggregated Across Baseline Covariate Cells

	(1) Assets	(2) Income	(3) Expenditure
$\rho^1\beta_1 + \rho^2\beta_2$	-16578.1	-175.5	-9139.3
	(21348)	(7737)	(7673)
N	426	422	426
Mean	230543	107318	159672

Note: The table presents results on the impact of receiving a lump sum transfer, depending on whether this structure was received randomly or because of an individual's preference. $L_i(\tilde{L}_i)$ denote that individual i was assigned to receive (preferred to receive) the transfer in a single lump sum. Results are estimated based on Equation 1, but interacting the other regressors with indicators for having above-median levels of assets, income, and expenditures and then calculating the cell-size-weighted average of the estimates in each resulting cell.

Table A.10: Effects of Received Preferred Tranche Count, by Preference

	(1) Financial stress	(2) Social input	(3) Deliberation	(4) Satisfaction	(5) Regret	(6) Goal progress
$L_i(\tilde{L}_i)$	0.0831**	-0.00931	-0.0279**	-0.0299	0.00914	-0.0731**
	(0.0408)	(0.0283)	(0.0131)	(0.0290)	(0.0438)	(0.0292)
$L_i(1-\tilde{L}_i)$	0.0128	-0.0217	-0.0128	0.00756	0.00582	0.0189
	(0.0307)	(0.0212)	(0.00988)	(0.0218)	(0.0327)	(0.0219)
$\tilde{L}_i$	-0.0261	-0.0116	0.00922	0.0245	0.0625	0.0625**
	(0.0365)	(0.0252)	(0.0117)	(0.0258)	(0.0388)	(0.0260)
Constant	0.615***	0.608***	0.537***	0.890***	0.383***	0.787***
	(0.0218)	(0.0151)	(0.00706)	(0.0155)	(0.0233)	(0.0157)
Observations	431	427	426	427	425	427

Note: The table presents results on the impact of receiving a lump sum transfer, depending on whether this structure was received randomly or because of an individual's preference. $L_i(\tilde{L}_i)$ denote that individual i was assigned to receive (preferred to receive) the transfer in a single lump sum. Results are estimated based on equation 1. Standard errors are reported in parentheses. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A.11: Impact of Delay: Asset subcategories

Variables	(1) Land value	(2) Land value	(3) House value	(4) House value	(5) Tools	(6) Tools	(7) Durables	(8) Durables
T_a	6,762***	19,067***	-458.3	-1,032	134.9*	-843.5***	-921.7	-5,809**
	(2,244)	(6,246)	(751.8)	(4,059)	(64.29)	(261.4)	(1,482)	(2,162)
T_a^2		-1,118		52.00		88.64***		444.9*
		(740.0)		(303.9)		(20.87)		(249.4)
Constant	108,748***	83,030***	39,228***	40,424***	2,753***	4,802***	65,130***	75,310***
	(16,334)	(8,052)	(5,148)	(11,873)	(468.6)	(770.7)	(6,615)	(4,943)
Observations	410	410	418	418	424	424	404	404
R-squared	0.005	0.006	0.001	0.002	0.001	0.005	0.002	0.007

Note: The table presents results from a regression of assigned delay (equation 4) on asset sub-categories, which include land, housing, productive assets (labeled as "Tools"), and other durables. Standard errors, clustered at the delay preference level, are reported in parentheses. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A.12: Impact of Delay: Income subcategories

Variables	(1) Enterprise income	(2) Enterprise income	(3) Harvest income	(4) Harvest income	(5) Wage income	(6) Wage income
T_a	-1,081	-4,113	-53.34	1,372*	-493.7	7,725
	(845.8)	(4,157)	(223.2)	(636.1)	(1,005)	(6,037)
T_a^2		273.7		-129.1**		-744.4
		(339.1)		(50.73)		(585.2)
Constant	37,157***	43,554***	10,706***	7,722***	110,762***	93,585***
	(6,270)	(12,394)	(713.9)	(1,172)	(5,132)	(13,045)
Observations	421	421	424	424	420	420
R-squared	0.002	0.004	0.000	0.006	0.000	0.005

Note: The table presents results from a regression of assigned delay on income subcategories, which include harvest, enterprise and labor income from a salaried (fixed or temporary) job. Standard errors, clustered at the delay preference level, are reported in parentheses. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A.13: Impact of Delay: Expenditure subcategories

Variables	(1) Medical	(2) Medical	(3) Food	(4) Food	(5) School	(6) School	(7) Other	(8) Other
T_a	-242.7**	-297.5	-13.13	-7.075	40.96	918.6	-1,979	-4,158
	(98.23)	(249.3)	(16.36)	(54.17)	(212.5)	(604.9)	(1,463)	(4,666)
T_a^2		4.983		-0.549		-79.51		198.1
		(25.85)		(5.213)		(47.93)		(442.0)
Constant	5,270***	5,384***	1,743***	1,731***	7,954***	6,117***	155,808***	160,355***
	(814.7)	(680.1)	(103.6)	(144.1)	(791.6)	(1,453)	(6,161)	(9,314)
Observations	423	423	424	424	424	424	423	423
R-squared	0.006	0.006	0.001	0.001	0.000	0.001	0.002	0.003

Note: The table presents results from a regression of assigned delay on expenditure subcategories. Standard errors, clustered at the delay preference level, are reported in parentheses. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A.14: Multiple hypothesis testing adjustment - information treatment

		p-values	FDR q-values
Popularity	$(P_a - P_{ia})chicken \times I^p$	0.645	0.477
	$(P_a - P_{ia})water \times I^p$	0.339	0.417
	$(P_a - P_{ia})bricks \times I^p$	0.147	0.283
	$(P_a - P_{ia})fence \times I^p$	0.051	0.257
Information	$(R_a - R_{ia})chicken \times I^r$	0.007	0.015
	$(R_a - R_{ia})water \times I^r$	0.596	0.425
	$(R_a - R_{ia})bricks \times I^r$	0.005	0.015
	$(R_a - R_{ia})fence \times I^r$	0.391	0.353

Note: The table presents multiple hypothesis testing corrections for the p-values of asset-specific prior and treatment interactions reported in Table 5, Column 4, from Row 12 onwards. We use two-stage sharpened q-values (Benjamini et al., 2006) for the correction.

Table A.15: Preferred timing, by average delay preference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	0	1	2	3	4	5	6 +
Deliberation							
$\kappa = 1$	0.0269	-0.0108	-0.0388	-0.00553	-0.0672	0.128**	0.0707**
	(0.0202)	(0.0191)	(0.00863)	(0.0128)	(0.0399)	(0.0381)	(0.0217)
Constant	0.515**	0.550**	0.528**	0.536***	0.543***	0.499***	0.529***
	(0.0202)	(0.0191)	(0.00863)	(0.00590)	(0.00902)	(0.0268)	(0.0282)
N	136	35	18	173	37	8	11
Financial stress index							
$\kappa = 1$	-0.0631**	0.118*	0.201	-0.00205	0.0827	0.0625	-0.198
	-0.00284	-0.0166	-0.0631	-0.033	-0.0877	-0.263	-0.131
Constant	0.653***	0.590**	0.580*	0.614***	0.605***	0.500***	0.656***
	-0.00284	-0.0166	-0.0631	-0.0244	-0.0436	-0.0872	-0.0998
N	137	35	18	173	37	8	11
Goal progress							
$\kappa = 1$	0.0462***	-0.0289*	-0.0556	0.0300**	0.0239	-0.0926*	-0.116
	(0.000378)	(0.00330)	(0.0164)	(0.00767)	(0.0548)	(0.0428)	(0.127)
Constant	0.787***	0.844***	0.833**	0.798***	0.828***	0.870***	0.819***
	(0.000378)	(0.00330)	(0.0164)	(0.0113)	(0.0112)	(0.0428)	(0.0404)
N	137	35	18	173	37	8	11
Satisfaction							
$\kappa = 1$	0.190**	-0.0208	-0.429*	-0.165	0.226	0.167	-0.0833
	(0.00992)	(0.155)	(0.0421)	(0.0736)	(0.123)	(0.472)	(0.379)
Constant	4.588***	4.688**	4.929***	4.533***	4.774***	4.333***	4.750***
	(0.00992)	(0.155)	(0.0421)	(0.00984)	(0.123)	(0.285)	(0.180)
N	137	35	18	173	37	8	11
Regret							
$\kappa = 1$	-0.166	0.687*	0.464	0.0114	0.0430	-0.667*	0.208
	(0.0652)	(0.0991)	(0.168)	(0.121)	(0.239)	(0.285)	(0.341)
Constant	1.277**	1.313**	1.286*	1.515***	1.290***	1.667***	1.125***
	(0.0652)	(0.0991)	(0.168)	(0.0179)	(0.0826)	(0.285)	(0.134)
N	137	35	18	172	37	8	11
Social input							
$\kappa = 1$	-0.0464	-0.245**	-0.217*	-0.0905**	-0.073	-0.0528	-0.00417
	-0.0376	-0.0134	-0.0323	-0.028	-0.124	-0.0595	-0.139
Constant	0.582**	0.617**	0.634**	0.633***	0.598***	0.686***	0.621***
	-0.0376	-0.0134	-0.0323	-0.0144	-0.00881	-0.0534	-0.0544
N	137	35	18	173	37	8	11
WTP							
$\kappa = 1$	-55783.5**	-171365	24571.4	-76620.8	-74414	39333.3	118916.7
	-1109.4	-48109.3	-8351.4	-72054.2	-58457.2	-38806.9	-125374
Constant	231172.4***	248031.3	122928.6**	257383.9**	187580.6**	120666.7***	113750***
	-1109.4	-48109.3	-8351.4	-58224.2	-65446.8	-11763.5	-16177.7
N	134	35	18	171	37	8	11

Note: The table presents results from the effects of timing match (equation 5) disaggregated by average delay preference elicited at follow-up. A delay preference of 0 corresponds to receiving the transfer in February, or immediately. Standard errors, clustered at the delay preference level, are reported in parentheses. Statistical significance is denoted: $***p < 0.01, **p < 0.05, *p < 0.1$.