Customized cash transfers: financial lives and cash-flow preferences in rural Kenya

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Abstract

We examine the preferences of low-income households in Kenya over the structure of unconditional cash transfers. We find, first, that most preferred lumpy transfers, and some preferred deferred receipt—unlike the structures typical of safety-net programs, but consistent with evidence on the financial challenges of poverty. Second, modest deferral of transfers increased income 1.5 years later. Finally, small changes in cash flow around the time of decision-making about transfer structure affected demand for deferral, with large downstream consequences. Taken together, these results illustrate how adapting cash transfer design to the decision-making environment of those in poverty could improve financial choices and outcomes.

Keywords: cash transfers, revealed preferences, choice architecture, poverty dynamics, seasonality
JEL codes: D91, H53, I38, O2

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1 Introduction

Managing cash flow is a recurring challenge for low-income households in developing countries (Collins et al., 2009). Their low and varying income flows—arising for example from seasonality, unpredictable harvests, or unsteady employment—often do not align temporally with their expenditure needs. Unpredictable, lumpy demands on cash, such as the need to pay for medical treatment, pose a particular challenge. And the financial tools at their disposal to manage these mismatches are often costly or unreliable. For all these reasons, providing low-income households with better tools to manage cash flow is generally seen as a high priority.

One potentially under-exploited way to do so involves the design of anti-poverty programs themselves. Cash transfers, our focus here, are arguably the most widely used poverty alleviation tool in developing countries.[1] These transfers are typically structured as small, regular (e.g. monthly) payments—a format that seems intuitively to address needs for subsistence and stability, but that may not meet other financial needs of low-income households. For instance, it does not address the need to put together lump sums of cash in order to make large purchases, or elevated needs for liquidity during the "lean season" in agricultural areas (Bryan et al., 2014, Fink et al., 2020).

Motivated by these observations, we study the preferences of recipients themselves over the structure of a cash transfer they receive. We worked in rural Kenya, in a setting featuring the kinds of variable income sources, agricultural seasonality, limited formal financial sector penetration, and dependence on informal financial vehicles (e.g. ROSCAS) characteristic of many poor areas. In this setting, the NGO GiveDirectly (henceforth GD) issued cash transfers to 513 low-income households, fixing the total amount (at approximately USD 1,000) but granting recipients some degree of control over structure: specifically, tranching and timing. It then randomized the actual structure of transfers delivered to most (90% of) subjects along these dimensions. This design yields data on preferences over structure as well as identification of the causal effects of structure (across a subset of the menu of options initially offered) on household outcomes.

In contrast to the structure of a typical social protection transfer, most recipients preferred "lumpy" tranching. In fact, almost all preferred to receive funds in one (35.6%) or two (62.6%) large tranches, while almost none (0.4%) preferred to receive twelve monthly installments. These findings parallel demand for lumpiness in private-sector contracts (Brune et al., 2021, Casaburi and Macchiavello, 2019).

On transfer timing, most recipients preferred to receive transfers starting immediately. Yet intriguingly, a sizeable 27% minority preferred at least some *deferral*, i.e. to have transfers commence in a month after February 2015, the earliest option.[2] Several patterns in the data suggest that selecting deferral was not merely a response error: it is highly correlated across decisions, for example, and positively associated with measures of cognitive ability. And respondents' qualitative explanations for these choices are coherent. Some desired deferral per

[1] As of 2007, 97% of developing countries provided some type of cash transfer program and 77% provided unconditional cash transfers as part of their safety net (World Bank Group, 2017), and cash transfer programming expanded dramatically as part of policy responses to the COVID-19 pandemic (Gentilini et al., 2022).

[2] Deferral in this sense is distinct from unanticipated *delay* in receiving transfers. See, for instance, Bazzi et al. (2015), for a study of the latter phenomenon.

se, in order to better plan their spending (consistent with, for example, the predictions and evidence in Thakral and Tô (2020, 2022) that advance notice helps recipients make better use of transfers). Other recipients sought to align transfer timing to seasonal agricultural demands, to ensure (for instance) that money arrived after planting when they would have time for construction projects, or when building materials would be less expensive. These narratives call to mind evidence of the difficulties that low-income rural households with limited access to financial instruments face in managing even predictable seasonal variation in cash flows (Augenblick et al., 2022). They also echo the strong influence of seasonality on financial decision-making under poverty documented in other recent work (e.g. Burke et al. (2018), Fujii et al. (2021), Glennerster and Suri (2022)).

These preferences are not unique to our setting. Subsequent to and motivated by these findings, GD conducted its own independent survey in three countries (Kenya, Malawi & Liberia) soliciting respondents' stated preferences over transfer structure, and offering even more flexibility in it: respondents were asked how they would like to split a GD grant arbitrarily over the course of a 12 month period. Majorities (60–80%) in every country preferred at most 2 tranches, with hardly anyone requesting more than three. Even larger majorities (52–97%) preferred to defer at least some of their transfer beyond the earliest possible date. At the same time, respondents did take advantage of the added flexibility they were given, with a majority (61%) choosing a structure that would not have been feasible in our original experiment. Overall the data reinforce the narrow message that low-income households often value lumpiness and deferral in cash transfers, as well as the broader point that they value customizability per se.

Taken together, the preference data raise the question: what are the consequences of different transfer structures? To examine this we focus on the 90% of recipients in our study who were assigned a structure experimentally (as opposed to the 10% who received their revealed preference), and estimate effects as of an endline survey conducted 1.5 years after preferences were elicited. With respect to tranche count, preferences to not receive small monthly payments were so overwhelmingly strong that GD decided to limit the support of the randomization to the two options with meaningful demand, i.e. 1 or 2 tranches (as they had indicated to participants they would). We take this as evidence in favor of lumpy designs, but it also means that we cannot contrast their effects to those of monthly payments. Comparing the two tranching structures GD did implement, a single tranche had larger estimated effects across a range of outcomes, but these estimates are not precise enough to support strong conclusions.

With respect to timing, on the other hand, GD implemented the full range of possible timings and the data thus have more to say. We find that recipients whose transfers began a little later in the year experienced faster subsequent income growth. This impact was highest when transfers began after planting but prior to the harvest season. This result replicates in data from Egger et al. (2022) (henceforth EHMNW), who also randomized the timing of lump-sum transfers as part of a separate subsequent study in the same region. Respondents whose transfers began a bit later also reported deliberating more about how to use them and making more progress towards their overall goals, but these latter effects are not precise enough to support firm conclusions.

How did deferring the transfer yield higher income 1.5 years later? In our design, there are

at least two plausible mechanisms: more time for *planning* and deliberation on how the transfer should be spent, and also *seasonality* effects which could affect the return on investments made from the transfer at different times of the year—both of which participants mentioned as reasons for their choices, as noted above. In the EHMNW design, on the other hand, only seasonality varied, as there was no systematic variation in the amount of time elapsed between when recipients were registered for transfers and when they received them. The timing of transfer receipt seems to have mattered at least as much in their experiment as in ours, suggesting that the benefits of deferral we find in our setting are at least in part driven by seasonality.

Given such benefits of deferral, the salient question is perhaps not why some respondents prefer deferral, but rather why more do not. The third piece of our analysis considers this question—specifically, whether cash flow around the time of decision-making itself affected their preference for deferral. We explore this question via an experimental intervention that varied the timing of cash flows around the date on which recipients decided. We exploited a feature of GD's transfer protocol, which involved delivering a small, initial "token" transfer to test payment logistics before delivering the bulk of the transfer. We experimentally varied the timing of this token transfer: some participants received it roughly four weeks before choosing how to structure the remainder of the transfer, and others, roughly four days before. This induced modest differences in recipients financial situation at the moment of decision: roughly USD 6 more in unspent cash on hand, for example, and slightly less self-reported difficulty with bills. More interestingly, it also influenced recipient's preferences over the structure of transfers tomorrow: more recent token transfer recipients were 13.6 percentage points (35%) more likely to demand some deferral. This finding is reminiscent of recent results from "Money Earlier or Later" experiments conducted in nearby Nairobi by Alem et al. (2024); while they show that receiving money as opposed to no money induces willingness to defer subsequent payments, we find that merely varying the timing of a (much smaller) earlier transfer is sufficient to induce the same effect.[3]

Taken together, these last two results illustrate a feedback loop: small changes in cash flow today can influence forward-looking choices, which in turn can have substantial effects on tomorrow's financial situation. Combining the estimates from the two experiments, we calculate that the shift in preferred timing for the main transfer induced by receiving the token transfer more recently was enough to increase annualized earnings by an estimated USD 48, or 5% of the total transfer.[4] We are able to document and quantify this feedback loop here because GD's transfer protocol created a somewhat unique situation: all recipients faced the same potentially high-stakes choice (how to structure their transfer) *at the same time*, as opposed to the more

[3]Explaining these result clearly requires a model in which recipients face difficulties moving money across time, so that the timing of external transfers matters. Several such difficulties could be relevant, due to a combination of behavioral and savings frictions. With respect to behavioral factors, we see the modest reduction in difficulty dealing with bills noted above, and positive but imprecisely estimated effects on most measures of cognitive performance that have featured in work on the psychological effects of poverty (Mullainathan and Shafir, 2013a, Kremer et al., 2019, Kaur et al., 2022, Fehr et al., forthcoming, Duquennois, 2022). We do not see significant effects on the time horizons recipients reported considering when making their decisions. Savings frictions may also have played a role: having more liquidity on hand at the time they decided may have enabled recipients to meet an expense that they would otherwise have tried to meet by moving their main transfer earlier than their preferred time of receipt.

[4]Here and throughout we express outcomes in USD using an exchange rate of KES 87 per USD 1, the rate prevalent in mid-2014 when our study commenced.

typical situation in which important forward-looking choices arise at different times for different people. But this in turn raises the intriguing possibility—building on the classic idea of a "big push" in the process of development—that even a relatively "small push" can help people break out of poverty if it comes during a critical decision-making window. Other such windows might include the "first 1000 days" of a child's life,[5] or moments of decision about whether to get married, continue one's education, or take a job.[6]

More broadly, we see our findings as opening doors for further work in two (related) directions. First, they suggest that it may be possible to (re)design cash transfer programs for greater benefit to recipients without higher fiscal cost. This effort would parallel (and could build on) recent work examining ways to increase the flexibility of micro-credit contracts (e.g. Field et al., 2013, Battaglia et al., 2021, Morduch, 2021). It might also help bridge the gap in policy-makers minds between "social protection" programs, which are often seen as palliative, and lumpy asset transfer designs which can can play a role in accelerating investment (Banerjee et al., 2015, Haushofer and Shapiro, 2016) if not outright help escape from a poverty trap (Balboni et al., 2021). Second, they suggest that designing choice architectures sensitive to the decision-making environments of people living in extreme poverty can lead to better choices and outcomes. We discuss these possibilities further, along with open questions, in the conclusion.

2 Context and design

Our study is set in rural Kenya in Siaya County, where our implementing partner GiveDirectly (GD) has been working for several years. The economy is primarily agricultural; most households engaged in some form of crop farming or animal husbandry as well as potentially a non-agricultural enterprise. There are two main planting seasons each year (see top panel of Figure 1).

Households in this area primarily use informal financial instruments. In data provided by EHMNW, for example, only 13% of households have a bank account, but 57% participate in a ROSCA and 35% borrowed money from (29% loaned money to) another household during the past year. In our own data we see borrowing for a variety of reasons, including investment in farm (12%) or non-farm (16%) enterprise, consumption-smoothing in response to large expenses such as medical bills (23%), school fees (23%) and funerals (8%), and also for buying food (18%). Overall, households appear to face challenges managing financial needs against seasonal cash flows, as is typical for those in considerable poverty (Collins et al., 2009). Few would have previously received cash transfers to help with these challenges: According to government data received by GD, participation in the National Safety Net Programme (which provides streams of small payments) is no higher than 8%. GD had previously delivered cash transfers in other nearby regions, but no villages or individual households in our sample had previously received transfers from GD.

[5] See for example https://thousanddays.org/.

[6] In this vein, Barrera-Osorio et al. (2011) find that postponing a monthly cash transfer to align with the timing of education decisions results in a significant increase in enrolment in secondary and tertiary education in Colombia, especially among the most at-risk children. Coffman et al. (2019) find that just a few hundred dollars can influence major career choices, albeit in a much higher-income context.

GD enrolled beneficiaries by identifying all households in a program village whose homes had a grass-thatched (as opposed to a metal) roof, an indicator of relative poverty, through a village census and follow-up visits. Each eligible household was issued an unconditional transfer of USD 1000, delivered via the mobile money service M-Pesa. GD structured these transfers as follows: it first made a "token" transfer (USD 35, $< 5\%$ of the total) to ensure the process was working correctly, and then transferred the remaining balance in one or more tranches (after which transfers ceased permanently). Our experiment involves manipulating three features of this structure: the timing of the token transfer, and the timing and tranching of the remainder. GD also cross-randomized an information intervention in which recipients received information about either the investment choices that previous recipients had made or the returns they realized by doing so, which did not significantly affect spending choices (see Section 2.3 of the pre-analysis report).[7]

2.1 Project timeline

The study evolved as follows. During enrollment, GD staff conducted a baseline survey of 533 households. Of these, 20 were removed during subsequent eligibility back-checks or attrited for other reasons, leaving 513 households in our study sample. GD staff then conducted a preferences survey with these households in January 2015, eliciting preferences over the structure of transfers and also capturing psychometric and attention measures. All subjects' preferences were elicited at the same time of the year, and timing preferences may reflect both seasonal considerations and "pure" preferences for deferral, e.g. in order to plan.

We randomized the timing of the token transfers relative to this preferences survey. Half the participants were assigned to receive their token transfer in December 2014 and the other half in January 2015, resulting in gaps of roughly four weeks or four days, respectively, between the dates of token transfer receipt and preference elicitation (Figure A.1).[8] All households received the same (token) amount—and thus had comparable reason to trust GD's commitment to making the large transfer—but those who received it more recently were likely to have more cash on hand as of the preferences survey.[9]

Staff elicited recipients preferences over the transfer structure in two steps. First, they asked about participants' preferred number of tranches among four options: one, two, four or twelve. Next, they elicited their preferred month for the *first* tranche of *each* option. Any subsequent tranches were to be evenly spaced over the remainder of the next 12 months, so that the choice of first month set the month(s) of the subsequent tranches. Conditional on receiving two tranches, for example, a recipient could receive the first of these in months 1-6, and would then receive the second six months after the first. We did not elicit timing preferences for the case of 12 tranches, as there was only one way to space 12 tranches evenly over 12 months. Full instruments with question wording and visualization aids used to explain the options are available via the AEA RCT registry (AEARCTR-0000541).

[7] https://www.socialscienceregistry.org/versions/169913/docs/version/file.

[8] A handful of households were surveyed later than scheduled due to logistical issues; omitting them does not substantively change any results.

[9] Potentially reinforcing this effect, early token transfers happened to arrive on 24th December, so may have been spent disproportionately on holiday expenses.

Staff indicated that GD would implement the two most popular tranche preferences and give each recipient a 55% (45%) chance of receiving their more (less) preferred of these. We implemented this by giving 10% of participants their preferred option and the other 90% a uniform random draw. We similarly gave 10% of participants their preferred timing option, and the other 90% a uniform random draw over the feasible start dates.

In designing this elicitation, we sought to balance several considerations. Implementing preferences with positive probability gives participants meaningful incentives to report thoughtfully and truthfully, while randomizing tranching and timing for most participants allows us to estimate causal effects. Assigning random start dates uniformly ensures that for any discount rate the expected net present value of transfers was constant with respect to tranche count (since the expected amount of money arriving in any given month was constant with respect to tranche count). This feature implies tranching preferences should not be confounded with time preferences.[10] Overall, we sought to give participants a meaningfully broad range of options compared to other (typically fixed) transfer schemes. That said, there may well be other structures participants would have ranked even higher—a series of small transfers during the "lean" season to meet food needs combined with a single lump-sum transfer to finance an investment, for example, or a tranche timed to coincide with the date school fees are due. We examine the structures recipients selected when offered even more flexibility to customize cash transfers using stated preference data (from a separate GD survey across 3 countries) in Section 3.4 below.

After eliciting preferences, we assigned participants to transfer structures as originally indicated. Table A.1 summarizes the assignment to tranching and timing, distinguishing between the 10% of subjects randomly selected to receive their preferred structures from the 90% who received uniform random draws. One important feature of this assignment is that transfers were issued only in one or two tranches, not in four or twelve tranches. This was simply because the first two tranching options were (as we will see) overwhelmingly the most popular, and GD only issued these, consistent with what it had told recipients prior to preference elicitation. The design is therefore not very informative about the effects of tranche count, as everyone received very lumpy transfers. GD informed all participants about their assigned transfer structure in February 2015, immediately prior to commencing transfers. [11]

Finally, we conducted an endline survey in July-August 2016, about 1.5 (0.5) years after the first (last) scheduled transfer payment. Of the 513 households in our sample we successfully interviewed 479, or 93%, at endline. To mitigate desirability bias in responses, the survey was conducted by temporary staff hired specifically for the survey, not by the operational GD staff who conducted enrollment. This survey covered participants' deliberation over how to use transfers, actual use of funds, satisfaction with their spending decisions and outcomes, and current income and assets. We discuss relevant variables in more detail below. Figure 1

[10] Our preference elicitation exercise did not aim to capture conventional time preferences, i.e. tradeoffs between (less) resources today versus (more) at a later date.

[11] The design also cross-randomized an information treatment in which some recipients were given information about the investment decisions of previous recipients: either the popularity of various common investments (e.g. in chickens), or the returns that their peers expected from them. This intervention was exploratory, and did not significantly alter investment behavior. See Section 2.3 of the pre-analysis plan report available at https://www.socialscienceregistry.org/versions/169913/docs/version/file.

summarizes the project timeline and its overlap with the seasonal agricultural cycle. As seen in its top panel, transfers commenced in February 2015 during the planting cycle, while a larger fraction of households report (financially) lean periods during the growing season that follows soon after. This hints at the possibility that receiving transfers a bit later than the earliest possible date might be appealing, an idea we will explore below.

2.2 Experimental integrity

Randomization successfully balanced household characteristics with respect to the timing of token transfers (Table A.2), the number of tranches (Table A.3), and the timing of transfer onset (Table A.4). The p-values of corresponding F-tests for joint orthogonality across all covariates are 0.30 for token assignment, 0.19 for tranche assignment, and 0.32 and 0.35 for linear and quadratic terms in months of deferral, respectively.

GD complied exactly with the experimentally assigned tranching (Table A.5). Five households assigned to receive two tranches had received only the first of these by the time of the endline survey due to issues with mobile money accounts (3 cases), an intra-household dispute (1 case), and a death (1 case). Results are robust to omitting these observations. With respect to timing (Table A.6), ten subjects (2%) received transfers 1-2 months later than assigned due to registration delays, and 3 subjects received transfers earlier than assigned. Given these (slight) deviations, we use assigned structure as an instrument for implemented structure in our data analysis.

Finally, attrition from the endline survey was modest for this context, at 7%, and balanced across treatment arms (Table A.7). In particular, attrition is unrelated to assigned number of tranches ($p = 0.94$) or to assigned timing of transfer onset ($p = 0.94$).

3 Recipient preferences over transfer structures

3.1 Tranching preferences

Our first main finding is that households reported a strong preference for "lumpy" transfers (Figure 2). Overall, just 0.4% of households preferred twelve monthly payments—the structure most similar to a typical social protection program—as their first choice. The most popular first-choice structure was two tranches (62.6%), followed by one tranche (35.6%), with four tranches a distant third (1.4%). Even as a second or third choice the stream of smaller payments was not a popular option; the great majority of participants (86.4%) said that twelve monthly payments was their least-preferred option (Figure A.2 reports the full preference ranking distribution).

There are several reasons, both internal and external to our study, to think that these preferences reflect a genuine demand for lumpiness. It is possible that some participants thought GD *wanted* to give them two tranches (earlier programs in nearby regions gave out transfers in two tranches, typically spaced around 6 months apart), but this does not explain why they overwhelmingly preferred *one* tranche to twelve tranches. Some may have found the elicitation questions confusing, but the cognitive measures we collected in the preferences survey are weakly *positively* correlated with choosing a single tranche, conditional on other characteristics (Figure

A.3). Nor can risk aversion over uncertain transfer timing explain preferences: lumpy transfers implied *greater* uncertainty than the twelve-tranche option.

Participants' remarks when asked about these preferences illustrate several coherent rationales for lumpy transfers. Many mentioned the need to finance lumpy investments or economize on fixed costs:

> R17: He prefer[s] to build a house with the money hence needs a lot of money at once.

> R18: She can do all her plans once hence it is cheap in terms of transport.

Some articulated benefits of splitting the money into two tranches rather than one:

> R28: Gives time to evaluate profit from first venture and advise on ne[x]t action steps with the next transfers.

> R39: This will enable me to built a house with the first lumpsum then reorganize myself to start some business with the second lumpsum after settling in my own home.

And when asked why they did *not* prefer their fourth-choice structure, respondents described a number of challenges—both financial and behavioral—that a stream of small payments would create for them:

> R24: It will be hard to save to do the project, the money might be squandered.

> R40: Will bring the hard task of banking to accumulate to reasonable capital.

> R127: Many small transfer may be wasted on daily demands and you may not do any tangible project.

> R132: Too little to solve a big case and keeping money is tricky and dangerous.

A priori we anticipated that the structure of mobile money fees might also play a role in tranching preferences, as M-PESA's withdrawal fees are non-linear. Quantitatively, however, it would have cost only USD 5 more in total to cash out USD 965 in 12 as opposed to 2 tranches, even under the extreme assumption that all transfers were cashed out immediately. Reflecting this, only two respondents (0.4%) mentioned withdrawal fees as a consideration.

The responses above align with evidence that savings constraints often bind in rural Kenya (Dupas and Robinson, 2013) and with the idea that periodic spikes in spending—for instance, to build a house or buy a large sack of grain—are sometimes needed to smooth subsequent consumption flows (Morduch, 2021).[12] These preferences are also consistent with the "lumpy" liquidity needs documented by Herskowitz (2021), who find that households may be willing to gamble for expected negative returns in order to accumulate lump sums for investment or financing of large, anticipated expenses. Some also indicate a notable degree of self-awareness—the respondent who worried that they might "squander" a stream of small payments, for example—reminiscent of the emphasis in the behavioral literature on the importance of psychological sophistication as opposed to naivete (O'Donoghue and Rabin, 1999).

[12] "Kin taxes" may also contribute to saving difficulties, though interestingly EHMNW do not find evidence of such taxes on GD transfers.

The preferences over philanthropic transfers we see here also align with recent evidence on private-sector contracting in similar settings. In Casaburi and Macchiavello (2019), Kenyan dairy farmers incur sizable costs to receive lumpier payments from buyers in order to solve self-commitment problems. In Brune et al. (2021), Malawian employees opt to partially defer wage payments at 0% interest in order to receive larger tranches and make big-ticket purchases. These examples underscore the point that "building lump sums" is a core financial challenge facing low-income households (Collins et al., 2009).

3.2 Timing preferences

Our second main finding is that a sizeable minority of participants demanded a small but positive amount of deferral before receiving transfers, preferring in January to have transfers begin *after* February (Figure 2). Conditional on receiving their first-choice tranching structure, 27% of participants preferred deferral of at least one month. Demand for deferral was meaningful for one, two and four tranches, but greatest (at 38%) when receiving one tranche, perhaps because under two- and four-tranche structures at least one installment is "deferred" automatically. The total amount of deferral demanded was almost always modest, however; conditional on demanding some deferral, 83% of respondents preferred two months or less, and only 2% preferred six months or more (Figure A.2 and Table A.8).

Of course, demand for *any* deferral is intriguing. Why would people who discount the future—and typically face very high interest rates—prefer to wait? Enumerators were trained to explain the decision to participants carefully using visual aids designed specifically for this purpose, but one might still worry about errors. The data themselves do not suggest this, however. Choosing deferral is positively associated with cognitive ability as measured by the Raven's test (see Figure A.3; the p-value from the corresponding test of the null of no relationship is 0.025). Demand for deferral is highly correlated across the one-tranche and two-tranche elicitations ($p < 0.001$ from a Fisher exact test of independence across the two choices).

An alternative point of view is of course that this population finds it difficult to save—both because the available vehicles are imperfect (Dupas and Robinson, 2013) and because it is difficult to stick to a saving plan without some form of commitment Ashraf et al. (2006). From this point of view a desire to postpone the receipt of a transfer makes good sense: the option to defer effectively embeds a basic, low-risk commitment savings device into the transfer scheme. We will see below that holding a preference for deferral is a positive predictor of subsequent income growth, which is consistent with the idea that understanding the need for commitment and taking advantage of opportunities to do so is a valuable trait.

As for reasons to *use* money after some deferral, the literature suggests two broad considerations. One is that having some time to plan before acting is valuable (e.g. Thakral and Tô, 2022). The other is that seasonality matters in rural, predominantly agricultural areas of Kenya (e.g. Burke et al., 2018) as well as elsewhere in sub-Saharan Africa (e.g. Glennerster and Suri, 2022) and further afield (e.g. Fujii et al., 2021). Informal debriefings with our survey enumerators yielded examples of each of these. Some recipients said that they wanted deferral in order to have time to plan, or to consult with family members. Others wanted to receive money after they had finished planting crops for the main growing season. In some cases this

was simply because they would then have more time to work on another project, while in others it was because they expected economic conditions to be more favorable (e.g., building materials would be cheaper) or because of cultural considerations, viewing this as the appropriate time to build a home in keeping with Luo tradition (97% of the sample are Luo).

3.3 Who wants what?

Panel A of Table A.9 reports associations between preferences and households characteristics, including core demographic characteristics as well as some other economic indicators akin to those often used in low- and middle-income countries to target programs via proxy means tests.

Our interest in these is partly conceptual, as different areas of existing research suggest different factors may matter for preferences. A life-cycle consumption perspective, for example, might emphasize the role of age, with younger household heads more likely to make major investments and perhaps preferring larger tranches to enable this. A household economics perspective might emphasize the idea that (given labor market frictions) a household with more working-age adults can take on more ambitious projects. In practice, however, household characteristics are not at all predictive of recipient preferences in our sample. For deferral, none of the 20 predictors we consider is individually significant at the 5% level. For one tranche (as opposed to two), only one of 20 predictors is significant at the 5% level, just the rejection rate one would expect by chance.

For policy-making, a more immediate question is not which predictors (if any) are individually significant, but how much explanatory power these covariates have overall. If substantial, then policy-makers could customize cash transfers to the predicted preferences of recipients using the same information they already typically collect to determine who is eligible for a program in the first place. If, on the other hand, the explanatory power is slight, then customization would require deeper changes to the program rollout process, such as directly eliciting preferences, as we do here.

The regression results in Panel A of Table A.9 do not offer much hope. For both deferral and tranching preferences an F-test of the joint null that all relationships are zero is not rejected ($p = 0.54$ and $p = 0.42$, respectively), and the overall fit of the models (as measured by their Adjusted R^2) is 0.00. But it is possible that these linear OLS estimates miss some more nuanced pattern in the data. To examine this we also train predictive models using the Generalized Random Forest method of Athey et al. (2019), which—as with tree-based models generally—is well-suited to detect non-linearities and interactions between variables in an "out-of-the-box" fashion. We train a separate model for each subject using data on all other subjects to obtain out-of-sample predictions, using either the limited set of baseline covariates from Table A.9 or using all available baseline covariates.

This approach yields no more predictive power than the linear models. The error rates (reported in Panel B of Table A.9) for the GRF predictions (Rows 2 & 3) are essentially identical to the "naive" benchmark rates we obtain by simply assigning all households the modal preference (Row 1). Customization of cash transfer designs in this setting, we conclude, would require direct elicitation of preferences household-by-household.

3.4 Variation across contexts

Subsequent to the experiment above (and motivated by its results), GD also conducted surveys of past and future recipients in three countries—Kenya, Liberia, and Malawi—asking them their stated preferences over transfer structure. These surveys gave recipients substantially more flexibility than our original design: they were asked how many tranches they would want to receive, when they would want to receive each tranche, and how much they would want to receive in each tranche. In the case of future recipients these questions were framed as what they would want if they could choose, while in the case of past recipients as what they would have wanted if they could have chosen.

These data are useful in several ways. First, they let us examine how much preferred structures vary across low-income populations in very different contexts. In terms of seasonality, for example, which appears to be one important consideration for recipients when customizing their transfers, these three countries are on quite different cycles (Figure A.4). Second, they can give us some idea how much recipients would make use of additional degrees of freedom to customize the structure of their transfers if given the opportunity. And while these are stated as opposed to revealed preference data, the fact that we observe them from a sample in Kenya similar to those from whom we elicited revealed preferences lets us gauge whether the difference in stakes leads us to reach meaningfully different conclusions.

Qualitatively speaking, stated preferences in all three countries were consistent with revealed preferences in the original study. Recipients demanded lumpy transfers, with the majority (80% / 60% / 69% in Kenya / Liberia / Malawi) preferring at most 2 tranches and almost all (95% / 94% / 94%) preferring at most 3, while no recipient desired monthly or bi-monthly transfers (Figure A.5). Note that if anything the tranche count metric somewhat under-states the demand for lumpiness since (unlike in the original study) tranches could vary in size. Some recipients who preferred three tranches, for example, preferred two large tranches and one small one; one can think of this structure as having a degree of lumpiness somewhere between that of three equally sized transfers and two equally sized transfers. Finally, many recipients demanded some deferral: 52% / 97% / 92% preferred to receive at least some share of their transfer after the earliest possible date.

At the same time, recipients did take advantage of the additional flexibility given them. For example, one respondent (LR004) said they would prefer to receive 70% of their transfer in January during the dry season to purchase farming materials including corn seeds, tools and chemicals, and the remaining 30% in February to use for paying school fees. This structure would not have been achievable with the more limited scope for customization offered in the original experiment. Overall, only 54% chose a tranching structure that would have been feasible in our earlier experiment, and taking timing into account only 39% chose an overall transfer structure that would have been feasible. This gives some sense of the potential value of added flexibility.

As in the original experiment, seasonality loomed large in respondents' thinking. What is interesting is the range of ways in which it factored in. For example, one respondent mentioned seasonal variation in prices and the direct effects of rainfall on the feasibility of construction:

> MN003: Food is cheap [at] this time, lumpsum will help purchase more, and

> it[']s ideal time for building a house with no rains.

Others mentioned seasonality in the returns to (non-agricultural) enterprises, due both to variation in how much purchasing power their neighbors have and how functional supply chains into their communities are:

> MN0032: Business is ideal when people have harvested their crops.
> LN016: Feeder roads are often deplorable during the rainy season, as such there are often limited supply of essential commodities to rural communities in rainy season. The respondent wants to invest in provision business to substain [sic] regular supply of goods to his communit[y].

And some stated timing preferences without reference to seasons, emphasizing the value of time to plan per se:

> MN0032: ..So that he can plan well and have time to think to avoid confusion with a lot of money

The overall picture that emerges is of a fairly nuanced set of timing considerations that are likely quite specific to the person and to the nature of the project they plan to undertake. This underscores the point that a single, common disbursement schedule is unlikely to be optimal.

Taken together, the stated preference data show both that specific preferences do vary across settings in ways that reflect important differences (such as seasonality and a desire for planning) between them, but also that the broad patterns we observed in the revealed preference data—in particular, demand for lumpiness and for deferral—are not isolated or unusual phenomena. Rather, they appear quite consistently across countries in different regions of sub-Saharan Africa, and at different levels of development.[13]

4 Effects of transfer structure

Having documented the transfer structures that recipients preferred, we next examine the impacts of receiving different structures. We do so taking advantage of the fact that while 10% of recipients received what they wanted on any given dimension of structure (tranching or timing) in order to make preference elicitation incentive-compatible, the other 90% of recipients were assigned an option experimentally. The estimates in this section use only the latter group. The methods follow a pre-analysis plan available at the AEA RCT Registry (AEARCTR-0000541), with any deviations noted.[14]

[13] Incidentally, we do not observe significant differences in stated preferences between past and future recipients. p-values from Kolmogorov-Smirnov tests of the null of no difference in the distribution of preferred tranche counts ($p = 0.71$) and of preferred deferral ($p = 0.98$) are well above conventional significance thresholds.

[14] In the pre-analysis plan report, available at https://www.socialscienceregistry.org/versions/169913/docs/version/file, we also examine the effects of getting the transfer structure one preferred. For tranching we see few meaningful or significant differences. For timing we see mixed results, with some positive and some negative effects, which we take to reflect the fact that while most recipients did not want any deferral, some deferral turned out to be beneficial (as we will see below).

4.1 Tranching

We begin with the impacts of tranche count. A priori one would expect to see the greatest differences between the lumpiest structures (1 or 2 tranches) and smoothest one (12 tranches), and this prior is reinforced by the fact that recipient preferences were so stark, with essentially all respondents preferring a lumpy structure. But recall that for tranching (as opposed to timing) GD limited itself to implementing only the most popular options. As a result, everyone in the experiment received money in either 1 or 2 tranches, so that we can examine the effects of variation only within that narrow range of structures—both of which are quite lumpy relative to the status quo.

With that caveat in mind, Table 1 reports estimates of the effects of receiving one as opposed two tranches.[15] We consider four pre-specified outcomes: a subjective measure of overall progress against goals, the change in total household income between baseline and endline, total spending (measured as the sum of effects on asset value and flow expenditures), and respondents' stated willingness to accept for the things they purchased using the transfer. None of the estimates are precise enough to support strong conclusions, which is perhaps not surprising given the narrow range of experimental variation. That said, the estimates are all positive, which is consistent with the idea that lumpiness is valuable. The estimates also line up with the findings in Haushofer and Shapiro (2016), who study a design with greater tranche count variation: in their experiment some recipients received monthly transfers while others received lump sums. They find that the former were more likely to improve food security, while the latter were more likely to be spent on durables.

4.2 Timing

We turn next to the effects of transfer timing. Here GD implemented the full range of possible options, without limiting to those that were most popular. We therefore expect to have better power to detect effects. We use the following specification to estimate the relationship between outcomes y_h for household h and transfer timing:

$$y_h = \alpha + \sum_{t=0}^{11} q_{h,t} \left[\beta_1 t + \beta_2 t^2\right] + X_h\gamma + \epsilon_h \tag{1}$$

where $q_{h,t}$ is the share of the transfer issued to h in month t (centered such that $t = 0$ in February 2015). X_h are controls for preferences over structure; as one would expect, given random assignment, results are essentially identical if we omit these (Table A.10). As pre-specified we estimated both a non-linear specification and a linear one that imposes the restriction $\beta_2 = 0$; because we find evidence of some significant non-linearities we focus our discussion on the more flexible specification, while reporting the linear estimates in the appendix (Table A.11). We instrument for $q_{h,t}$ to account for (slight) non-compliance, but ITT estimates are generally similar both qualitatively and quantitatively. First-stage F-statistics are (unsurprisingly) all

[15] Following the pre-analysis plan, we estimate these from a regression that interacts an indicator for assigned tranche size with indicators for preferred tranche count, and then aggregate these back up to calculate the estimate average treatment effect. See table notes for details.

large enough to address concerns about instrument strength (Table A.12). We conduct inference using heteroskedasticity-robust standard errors.

This approach modifies the one we proposed in the pre-analysis plan in the following ways. First, we define the second regressor as $\sum_{t=0}^{11} q_{h,t}t^2$ as opposed to $\left(\sum_{t=0}^{11} q_{h,t}t\right)^2$. Simply put, the latter definition was an oversight. It implies (for example) that receiving two tranches in months 3 and 9 is equivalent to receiving the same total amount in month $(3+9)/2 = 6$, which need not hold if the true relationship is indeed non-linear (and the data will reject it in some cases). We therefore focus our discussion on estimates of (1). That said, results from the pre-analysis plan specification are similar in all respects (Table A.13).

Second, we emphasize robust standard errors as opposed to standard errors clustered by recipient timing preference. This is conservative: we reject more null hypotheses with the latter, but believe those tests may not adequately control size, as ex post we have only 14 clusters of which 2 account for $\sim$ 80% of the data. Clustered standard errors may perform poorly in such settings (Donald and Lang, 2007, Chiang et al., 2024), and more generally recent guidance (e.g., Abadie et al., 2023) is to not cluster when the design itself was not clustered, as was the case here. We therefore report clustered standard errors for the sake of transparency but focus the discussion on the heteroskedasticity-robust ones.

Finally, we modified the dependent variable in two cases: income and the deliberation index. For income we report effects on the *change* in income between baseline and endline (rather than endline income level). The results below remain significant (at the 10% level) if we estimate the model in levels, but conditioning on baseline income increase precision, as does conditioning on other baseline measures of living standards such as assets or expenditure; see Table A.14 for a comparison of these specifications. For the deliberation index we use a version normalized to have mean 0 and standard deviation 1 so that treatment effects can be interpreted in standard deviation units.

Table 2 reports the results. The overall pattern is that *some* deferral led to directionally more positive outcomes across the board, and significantly faster income growth. Starting from no deferral (i.e. transfer onset in February, the modal preference), deferring an additional month led to weakly more deliberation about how to use funds (Column 1), more social input into this process (Column 2), and more progress against overall goals (Column 3). It significantly increased income growth (Column 4, $p = 0.036$), in line with (insignificant) increases in cash outlays, measured as the sum of impacts on assets and annualized non-durables expenditure (Column 5), and in the valuation respondents assigned to the items they purchased ("Valuation", Column 6). Note that if we use clustered standard errors the positive effects on deliberation and overall goal progress are also significantly different from zero, but as noted above these may be too liberal.

Prolonged deferral, on the other hand, worsens endline outcomes. The quadratic terms are negative, significantly so in the case of income. This is logical, in that *indefinite* deferral cannot be beneficial. What is more interesting is how the resulting pattern of returns aligns with the local agricultural cycle. If we estimate a specification analogous to (1) but with a separate coefficient for each season, as defined for Western Kenya by (Ndungu et al., 2019), we see that effects are concentrated in the growing season (Figure 3, Panel B). The month-by-month

preferences for deferral in transfer timing (Table A.8) are consistent with the fact that some deferral seems beneficial in terms of impact on several core measures of well-being.[16]

One nuance in interpreting these results is that more deferral *before* receipt of transfer also mechanically implied less time elapsed *after* receipt and before endline, since the endline was conducted at the same time for everyone. The patterns in Figure 3 may thus reflect differences due to time elapsed since treatment as well differences due to time elapsed before treatment. For our purposes, the issue that is particularly important is the extent to which the upward-sloping portion of the impact curve captures benefits of deferral before receiving transfers, as opposed to fade-out in effects after receiving them.[17] The results in EHMNW provide a useful point of triangulation in this regard, as they studied the effects of similar cash transfers also delivered by GD in the same region of Kenya (Siaya), and their design varied the length of time elapsed between transfer receipt and outcome measurement. The impulse-response function they estimate for income (Figure 1, Panel B of their paper) is in fact modestly increasing over the 15-18 month range, the range that corresponds to the 0-3 months of deferral in our own Figure 3 (since endline was 18 months after first transfers). While this comparison should be made with due caution, it suggests that the upward slope we observe may if anything understate the benefits of deferral.

One should not read too far into this result. Income is not welfare. Nutritional intake during the "lean season," for example, may also have been affected by the timing of transfers, and there may be tradeoffs between gains on that margin and on longer-run earnings. One would need richer, high-frequency panel data to explore these tradeoffs.

4.3 Why wait?

Why might deferral help recipients achieve faster subsequent income growth? There are at least two plausible mechanisms. First, deferral affected the amount of time that elapsed between when recipients learned they would be receiving money and when they actually received it. Deferral could thus afford more time to form a plan, which could in turn be helpful for resisting temptations and social pressures to use money in myopic ways. And second, deferral affected the time of year at which money arrived, which could matter independent of planning effects. Recall that recipients mentioned both of these channels in their comments to survey enumerators (Section 3.2). Both channels reflect constraints that a policy-maker would presumably wish to relax, but they motivate different responses: planning would imply that there should be a suitable (but fixed) time lag between the announcement and disbursement of transfers at all times of the year, while seasonality would imply that disbursement should be concentrated at

[16] It is also interesting to note that preference for deferral itself predicts several of the same outcomes (Row 3 of Table 2). Preferring some deferral is associated with significantly more progress against goals, as well as (insignificantly) more subsequent income growth. These are of course purely correlational—because the sample here is limited to the 90% of respondents who did *not* receive their preferred structure, preferring deferral had no influence on the transfer structure actually received, and so the estimates isolate the cross-sectional relationship between preferences and outcomes. But they are consistent with the idea that the households willing or able to postpone the receipt of cash tend to do better over the longer run.

[17] The downward-sloping portion of the impact curve could similarly reflect treatment effect dynamics, e.g. the time required for investments to bear fruit. This strikes us as unlikely, since even the latest transfers were a full 7 months before endline, but would be consistent with and even strengthen our interpretation of the upward-sloping portion.

certain times of the year.

The fact that treatment effects on our deliberation index mirror those for income is consistent with the planning channel playing some role, but these are not significantly different from zero. Estimated effects on the underlying components of this index mostly follow the same pattern but are also not significant; see Table A.15 for results, and Appendix B for more detailed variable descriptions. This evidence is therefore suggestive at best.

For the seasonality channel, on the other hand, we can do more by juxtaposing the time pattern of effects in our data with that in the data from EHMNW. Their design similarly involved transfers delivered at different, experimentally-assigned times of the year. But, unlike in our design, there was no correlation between the month (or season) of transfer and the amount of time recipients had to plan how to use their transfers. This is because recipients were enrolled on a rolling basis throughout the course of the study, and regardless of when they were enrolled, the timing of subsequent events was the same: token transfers were sent once a majority of the eligible households in each village had completed the enrollment process, and the rest of each transfer was sent in two tranches at two and eight months, respectively, after the initial token. The absence of systematic variation in time-to-plan in this design makes it useful for isolating seasonality effects.

To operationalize this idea, we estimate regression models analogous to Equation 1 but using endline income data from EHMNW. We normalize time such that $t = 0$ corresponds to February, so that $q_{h,t}$ has the same meaning in both datasets. In the EHMNW data we have a pure control group that did not receive any transfers; we therefore set $q_{h,t} = 0 \; \forall \; t$ for that group and include an indicator for treatment group status so that, as in estimates from our own data, the βs are estimated solely from variation in transfer timing within the treated group.

With respect to income, the results mirror those we find in our own data: the effect of receiving money later than February is initially positive, but then eventually decreases (Table A.16, Column 1). Figure A.6 visualizes this pattern, grouping months as before into seasons as defined in Ndungu et al. (2019). (Note that since the estimated coefficients here identify the relative effect of the seasons we normalize the coefficient for the "planting" season to match that in our data, so that the remaining inter-seasonal changes are easier to compare visually.) As in our data, the estimated effects first increase and then decrease as we move across seasons. There are also some differences—the increase is larger and peaks during the harvest season, for example, and estimates are somewhat less precise (reflecting the clustered design)—but overall the timing of transfer receipt seems to have mattered at least as much in the EHMNW experiment, in which planning horizons did not vary, as it did in ours. This suggests that seasonality plays the larger role in the effects of deferral we observe, though of course it does do not rule out a contribution from the planning channel.

There is also a purely descriptive question about what recipients did differently as a function of the timing of their transfers which led to differences in longer-term outcomes. This is a subtle issue and it is not clear that we should expect to find answers in typical survey data. Suppose, for example, that more time to plan helps recipients select projects that are a better match for their abilities. Then deferral might lead one recipient to switch from investing in a retail business to a transportation business, while another might make exactly the opposite switch,

so that the net average treatment effect is zero.

That said, the data do suggest some broad patterns, particularly around structural change in self-employment. If we split the sample into those who report that their primary occupation is self-employment (70%) and wage employment (27%), we find that the seasonal pattern in Figure 3 is driven by the former group (Table A.17). This suggests that investment in one's own enterprise played some role. And this investment was likely concentrated in the non-agricultural sector: we see substantial movement between baseline and endline surveys out of farming, fishing and animal husbandry (from 65% to 44%) and into non-farm enterprise (from 10% to 27%) as a primary source of income. Effects on various secondary outcomes are broadly consistent with this: for example, the seasonal pattern we see for overall income is not mirrored in measures of agricultural earnings or investment, but does appear in the likelihood that a household reported using its transfer to start or invest in a non-agricultural enterprise, as well as the share of spending on durables overall, though the seasonal variation in these effects is not statistically significant (Figure A.7).

While only suggestive, these patterns do line up with the fact that deferral until *after* the main agricultural investment period (i.e. planting) is associated with higher income. They are also consistent with prior evidence that lumpy transfers have driven diversification from farming into non-agricultural enterprise in this area (Egger et al., 2022, Orkin et al., 2022).

5 Financial feedback loops: cash flow & decision-making

Given the results so far, the salient question is perhaps not why some recipients prefer deferral, but why more do not.

One obvious possibility is that choosing to wait a bit longer for a very substantial transfer is not easy given the financial pressures these households face. We asked respondents some questions about these pressures at the same time that we elicited their preferences; 56% reported that they had experienced worries about money in the last 7 days, and 88% reported that they were having difficulty coping with bills or expenses in the last 7 days. While partial and imperfect, these measures perhaps give us some indication of respondents' likely state of mind at the time they made decisions about transfer structure.

Our design gives us one way to examine whether financial circumstances at that moment affected decision-making. Specifically, we examine the consequences of the experimental variation in the timing of the small token transfers that GD delivered before it elicited preferences. Figure 4 illustrates the effects of receiving this token transfer more recently on outcomes measured in the preference elicitation survey. Recency affected cash on hand: recent token transfer recipients reported having around $6 more unspent out of that transfer, or 13% of the total, at the time preferences were elicited. They also reported less difficulty dealing with bills, though not lower worries about money overall. Overall it seems fair to say that the timing of the token transfer induced some modest but meaningful differences in recipients' financial circumstances at the moment of decision.

Interestingly, token timing also significantly altered preferences for deferring the main transfer. Thirty percent of recent token recipients preferred some deferral, compared to 17% of less

recent token recipients ($p = 0.009$). A non-parametric Fisher's exact test (reported in Table A.18) also rejects the null of identical distributions of deferral preference, both conditional on receiving one tranche ($p = 0.007$) and conditional on receiving two ($p = 0.043$). Mean effects are noticeably and significantly larger in the former case, however (Table A.19). This seems intuitive given that anyone receiving two tranches was already guaranteed to receive half of their money at least six months after the date of preference elicitation; they effectively had a fair bit of deferral already built in to their transfer schedule. Overall, across all tranching structures, more recent token transfers increased the preference for deferral by an average of 0.37 months. In contrast, token timing did not significantly alter tranching preferences; we can reject changes as small as 30% (10.8pp) at the 95% level.

Why did the timing of past cash flows affect preferences over the timing of future cash flows? The results suggest that we should consider models in which timing matters, i.e. in which recipients face some difficulty optimizing the movement of money across time due to some mix of external constraints (e.g. faulty savings vehicles) and behavioral factors (e.g. present bias). Starting with behavioral issues, one possibility is that respondents who had received their token more recently were able to think differently about their future plans. Effects on measures of cognitive performance are generally positive, reminiscent of other results on the effects of scarcity (Mullainathan and Shafir, 2013b), though not precisely estimated enough to support strong conclusions (Table A.20). We also asked respondents about the time horizon they considered when making their plans, and do not see significant differences between the two groups (Figure A.8). As for savings frictions, saving was indeed likely difficult in this setting for a variety of reasons—negative real interest rates, risks of theft or of pressure from others to share, and so on. Deferring an electronic transfer would then be a relatively attractive way to save, and being in better financial shape at the moment of decision (as recent token recipients seem to have been) would of course increase the appeal of saving. In this interpretation, having \$6 (or about two day's wages) more on hand at the time they made their decision enabled some recipients to meet a cash flow need that they would otherwise have met by moving forward a much larger transfer (of \$483 or \$965) by several months from the time that would otherwise have been optimal.[18] [19]

Overall, our read is there are several factors that may have contributed to the dynamic of interest here—that a high-stakes decision over *future* cash flows can be quite sensitive to small changes in *current* cash flows. This is the critical fact for understanding poverty dynamics. Viewed in a negative light, it implies that people's circumstances may be quite volatile in response even to small financial perturbations. Viewed in a positive light, it suggests that cash

[18] One might also imagine that some respondents who preferred the money earlier did so simply because they did not entirely trust GD's commitment to make transfers in the future of course. However, all timing options involved some amount of waiting (i.e. there was no option to receive cash immediately), and at the time they stated their preferences all recipients had received an identical amount from GD as token transfer. Hence, there is no obvious reason to think that trust in GD would be *differentially* lower among less recent token recipients.

[19] Note that either of these factors—behavioral or technological constraints on saving—could also contribute to demand for lumpy transfers. After all, if saving were straightforward it would be easy to convert a stream of small payments into a larger lump sum. But it seems unlikely that a small change in recipients' cash flow in January would meaningfully alter how easy it would to be for them to to save out of a transfer in June (say), and thus more generally how easy it would be to convert smaller future tranches into larger lump sums. Given this, it is not surprising to us that tranching preferences did not change significantly due to the timing of the token transfer.

injections that are well-timed to arrive during critical windows for decision making can be very influential—a point to which we will return in the conclusion.

To illustrate this sensitivity point quantitatively, our final exercise combines the estimated effects of deferral on subsequent income growth (from Section 4.2) and the estimated effects of cash flow timing on willingness to defer (above), to calculate how sensitive future earnings are to (the timing of) current cash flows. We use the coefficients from Table 2 to calculate the difference in mean earnings growth under two distributions of transfer onset timings: the empirical distribution $\{T_h\}$ actually observed in the early-token transfer group, where T_h is the onset date preferred by household h, and the same distribution right-shifted by the average treatment effect on deferral of 0.37 months. We thus calculate

$$\frac{1}{|\mathcal{H}^e|}\sum_{h\in\mathcal{H}^e}\left(142.2\times(T_h+0.37-T_h)-13.5\times\left[(T_h+0.37)^2-(T_h)^2\right]\right) \tag{2}$$

where $\mathcal{H}^e$ is the set of households assigned to the early token group. Calculating (2) yields an estimated annual income gain of USD 48, or 5% of the transfer amount. In other words, simply perturbing the *timing* of the initial USD 35 token transfer induced a USD 48 better forward-looking decision.[20]

One should not necessarily expect to see similar absolute effects at times when households do not have such a consequential financial decision to make. Brune et al. (2017) do not detect significant effects of a smaller amount of deferral (1-8 days, as opposed to one or more months) in the receipt of a much smaller transfer ($60, as opposed to $965) to households in Malawi, for example. That said, the calculation here indicates how impactful it can be to relieve financial pressures at critical moments when major financial decisions are being made. Because these moments do not always come at the same time for everyone, average treatment effects (in, say, a cash drop experiment) may not capture the importance of cash flow around them. But the design here engineers a situation in which many people faced the same high-stakes decision at the same time. This is artificial in a sense, but useful in that it allows us to see in the average treatment effects phenomena that might otherwise be hard to see given the logical impossibility of observing individual treatment effects.

6 Conclusion

Our exploration of "customized" cash transfers has found that most recipients preferred structures different from those typical of social safety net programs—including larger tranches and (for around a quarter of recipients) some deferral. These preferences are coherent with what we know about the financial lives of households living in extreme poverty, and with subjects' stated reasoning about the structures that work best for them. That said, preferences need not be the last word in transfer design: we also see that they are malleable, influenced by small

[20] One assumption implicit in this calculation is that the treatment effects of deferral on income are the same on average as they are for those recipients who are "compliers" with the recent token transfer intervention, i.e. those who choose more deferral when given a recent token transfer. To help assess the plausibility of this assumption we can test for heterogeneity along observable dimensions in the effects of the recent token transfer. We do not see evidence of any significant heterogeneity, as an F-test of the null that all the interaction terms (between recent token receipt and baseline covariates) are zero fails to reject ($p = 0.17$; see Table A.21).

changes in cash flow around the time they were elicited, and that this can lead to meaningful effects on subsequent income growth.

The central policy implication is that there may be scope for inexpensive reforms that increase the value of existing cash transfer programs. Most programs currently provide small, regular payments. Some have considered how to make them more "graduative," in the sense that participation makes households less likely to need them in future. We find here that recipients themselves demand transfer structures better-suited to financing graduative investments. There may thus be scope to meet this demand while also furthering policy objectives. One such approach would be to allow recipients to simply defer one or more tranches so that they arrive bunched together. This would accommodate demand both for lumpiness and for deferral, including deferrals that help to manage the challenges of seasonal cash flow and risky or imperfect savings devices, while at the same time relaxing government budget constraints by deferring an expense.

One might worry that customized transfers are beyond the operational capacity of some states, at least at present. We suspect that such pessimism is undue. A deferral scheme like that above, for example, would require new administrative processes, but could also in some ways make program administration simpler, as it would likely lead to fewer total payments to be issued and spread those payments out over time. Even India, known for its struggles executing central government schemes on the ground, has been able to provide a "deferral" mechanism that is functionally equivalent to the one above: some states allow recipients of subsidized grain from the Public Distribution System to collect grain up to one month after it was initially allotted to them (see for example Muralidharan et al., 2023). That said, the preferences we observe for something other than the status quo are so widespread that even giving all recipients the most popular structure or accounting for seasonal variations in income (say), without any customization, would likely be a meaningful improvement.

For future research, the results—especially those from GD's followup surveys (Section 3.4)—suggest a number of opportunities to learn through experimentation with richer menus of transfer designs. These could be used to price out recipients' valuations of different design features, for example, quantifying *how much* they value these. Menus could include contingent structures, such as payouts conditional on weather indices. This might help address barriers such as distribution costs and liquidity constraints (Casaburi and Willis, 2018) which have limited the distribution of market-rate insurance in rural, low-income areas. In an early pilot along these lines, for example, GD gave recipients a choice how much of their transfer to receive in the normal, non-contingent manner and how much to receive in the form of a state-contingent weather index insurance product offered by a commercial insurer. Take up of the insurance was high (62%) and the implied customer acquisition cost was 75% lower than that normally incurred by the insurer. Menus could also include longer-term payment streams that come closer to "basic income" (Banerjee et al., 2020). It could be that the streams we offered here were too short, but that longer-term streams that provide a degree of protection against risk would be more compelling than cost-equivalent lump sums.

Four additional directions strike us as promising. First, future work could examine preferences in other settings, especially urban ones in which seasonality may loom less large. Second,

it could examine how preferences respond to planning aids such as those in (Augenblick et al., 2022), or to better availability of financial products (whose absence may explain the preferences we observe). Third, it could intersect these questions with issues of intra-household decision-making, yielding policy design that is more equitable within as well as across households. Finally, it could further explore the impact of cash transfers that coincide with critical life transitions or decision points. A few examples come immediately to mind: whether to continue with a high school education (or quit school), which career path to choose during high school or college, choosing a first job or when to marry or start a family after marriage. A well-timed financial intervention that provides some slack when faced with such key decisions could significantly influence individual choices and hence life trajectories.

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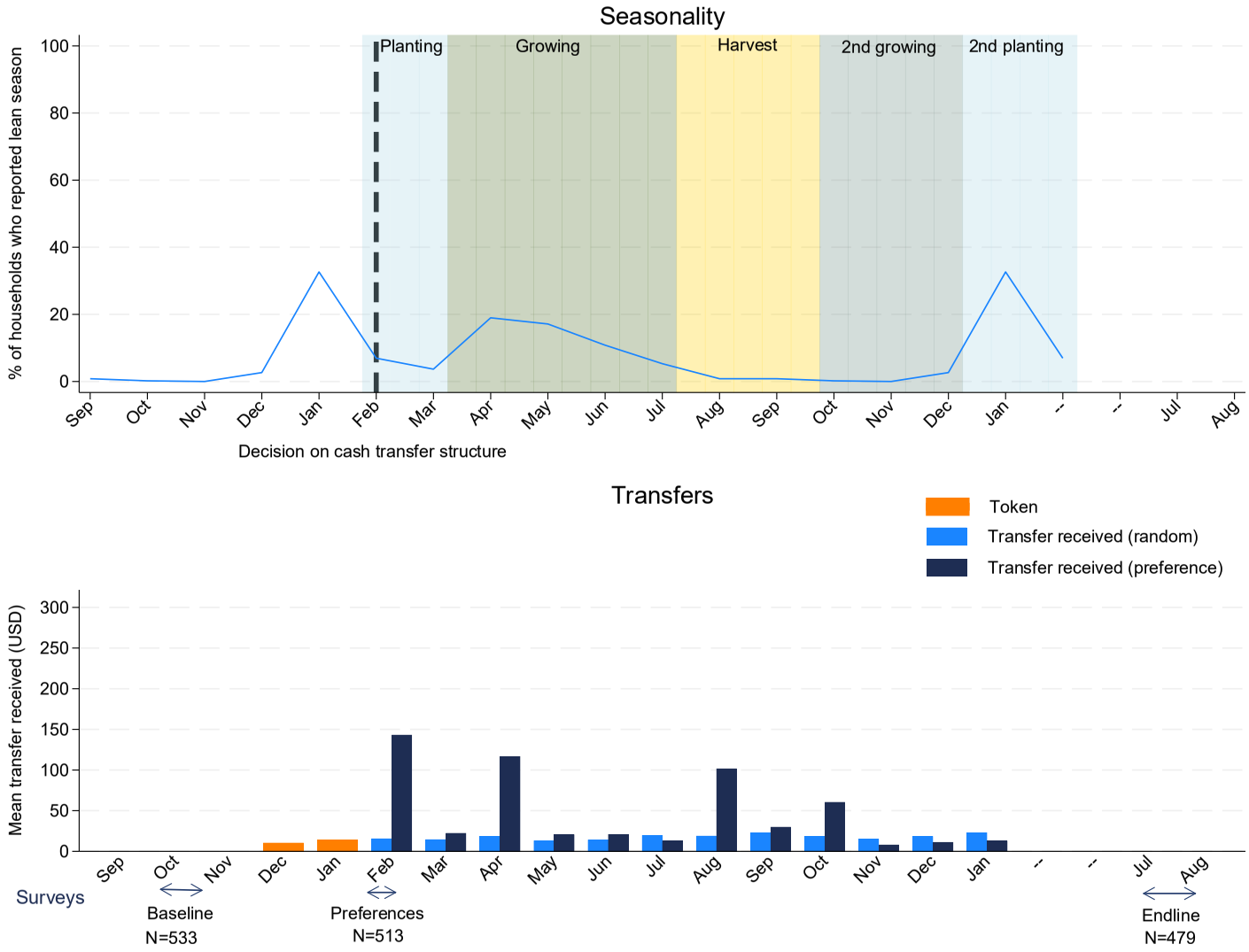
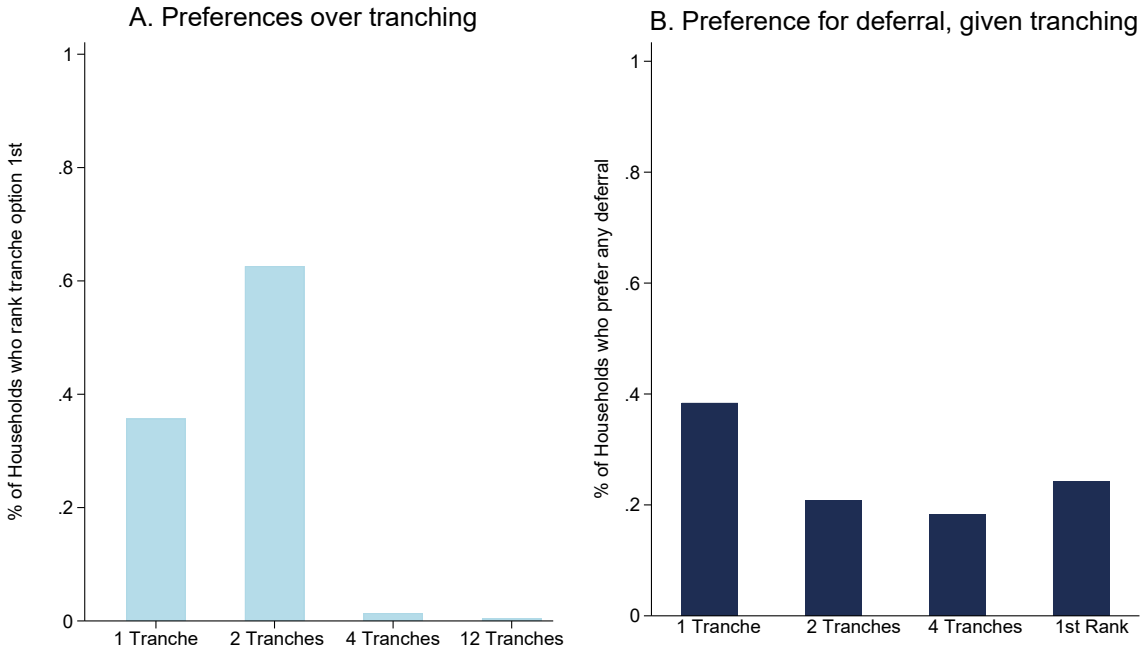
Figure 1: Study design and timeline



Notes: This figure displays the timeline of the study from September 2014 to August 2016, including the experimental design and the way it overlaps with the seasonal agricultural cycle for maize, the main local crop. The top panel, titled "Seasonality," displays phases of the annual agricultural cycle as defined by Ndungu et al. (2019) along with the fraction of respondents who self-reported each month as one that is typically financially "lean" for their household. The middle panel, titled "Transfers," indicates the mean monthly amount (in USD) transferred to participants, colored to differentiate between initial token transfers; main transfers to households that received either their preferred tranching, their preferred timing, or both; and main transfers to households that received random tranching and timing. The bottom panel, titled "Surveys," indicates the timing of the three surveys conducted over the study period—the baseline survey, preferences survey, and endline survey.

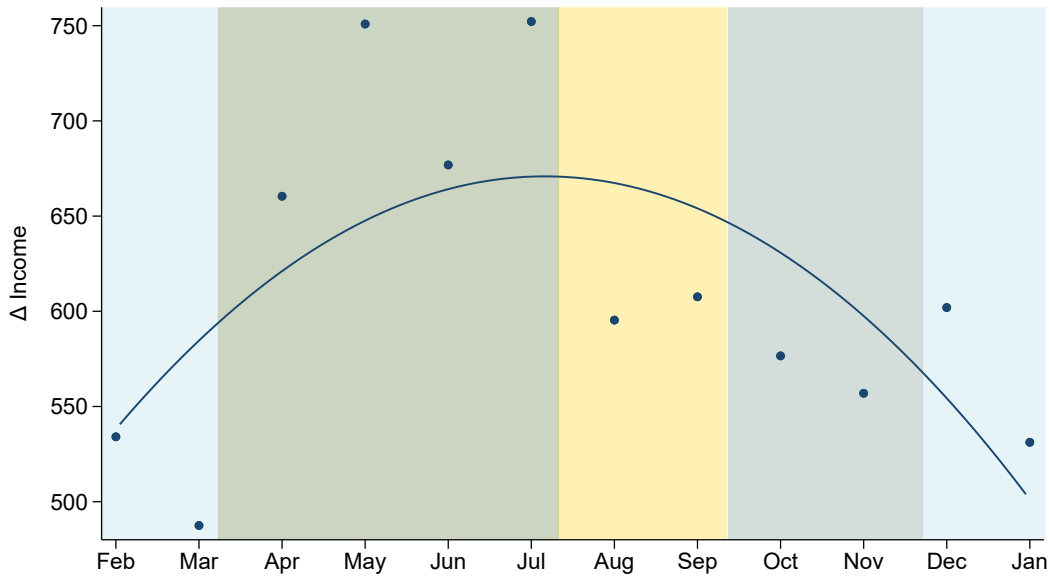
Figure 2: Preferences over cash transfer structures



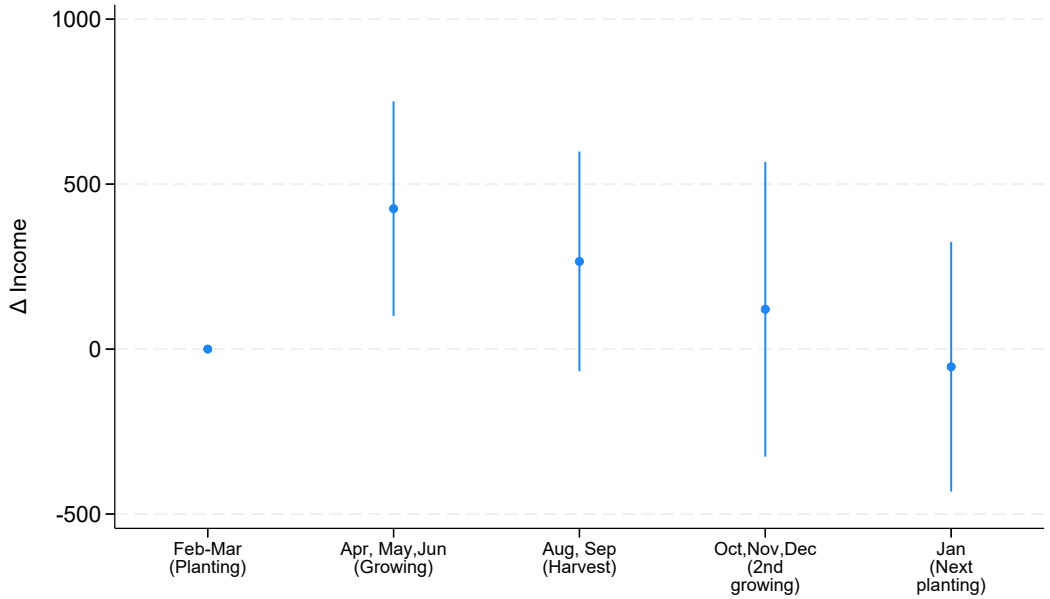
Notes: This figure summarizes participants' preferences over the number of tranches and timing of their cash transfer. In the left panel, each bar displays the fraction of study participants who rank the number of tranches given on the x-axis label as their first choice. In the right panel, each bar displays the fraction of households that indicated a preference for any deferral beyond the first possible month (February 2015), conditional on receiving the number of tranches indicated by the x-axis label (where '1st rank' indicates their most preferred tranche structure, and thus represents a popularity-weighted average of the other three items). The full marginal distributions of tranching and timing preferences are in Figure A.2, and the full joint distribution is in Table A.8.

Figure 3: Deferral and income growth

(a) Income growth by month of receipt

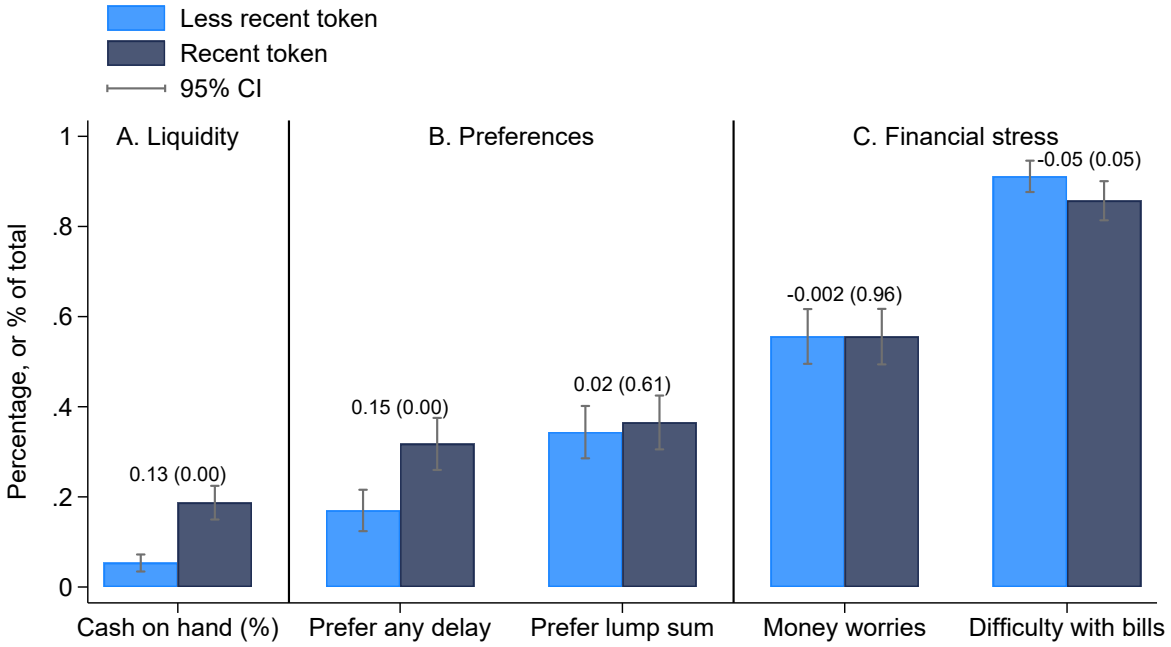


(b) Income growth by season of receipt



Notes: This figure displays the relationship between the time at which recipients received cash transfers (x axis) and the change in income between baseline and endline surveys (y axis). In Panel (a), time is denoted in months and data points represent the mean income change for households weighted by the share of funds $q_{h,t} = \{0, 0.5, 1\}$ received in that month. The overlaid curve is the quadratic fit obtained by estimating Equation 1. In Panel (b), time is denoted in seasons as defined in Ndungu et al. (2019) by grouping corresponding calendar months (also indicated by the background colors in panel A); point estimates and 95% confidence intervals are obtained using the following regression specification: $y_h = \alpha + \sum_s \beta_s q_{h,s} + X_h\gamma + \epsilon_h$ where y_h is the outcome of household h, α is a constant, $q_{h,s} = \{0, 0.5, 1\}$ is the share of overall transfer that household h received in a given season s = {growing, harvest, 2^{nd}growing, next planting} where the first season, planting, is the omitted category, X_h denotes controls (as in Equation 1) and ϵ_h is the error term. The estimation sample is the 90% participants who were randomly assigned a transfer timing. Error bars indicate 95% confidence intervals based on heteroskedasticity-robust standard errors. p-values for tests of the nulls of equality between the estimated coefficients for the planting season and those for the growing, harvest, 2^{nd} growing, and 2^{nd} planting season are 0.01, 0.12, 0.59, and 0.78, respectively.

Figure 4: Effects of cash flow prior to preference elicitation



Notes: This figure displays differences between recipients who received their token transfer less vs. more recently with respect to three sets of outcomes—liquidity, preferences over transfer structure, and measures of financial stress—all measured as part of the preferences survey, i.e. at the time preferences were elicited. Lighter (darker) bars shows the outcome for the less (more) recent token transfer group. Panel A shows the fraction of cash from the token transfer remaining on hand (% of total). Panel B shows the percentage of households with a preference for any deferral in receipt of the first main transfer and for one tranche (as opposed to two tranches). Panel C shows the percentage of households reporting money worries and difficulty coping with bills. The numbers above each pair of bars are the regression-estimated differences between the two groups in that outcome, and the numbers in parentheses are heteroskedasticity-robust standard errors. Analogous regression results are reported in Table A.22. p-values of a test of the null of no difference are reported in parentheses. Whiskers indicate 95% confidence intervals for group means.

Table 1: Impacts of tranche structure

	Goal progress	Δ Income	Assets + Expenditures	Retrospective valuation
	(1)	(2)	(3)	(4)
Single tranche	0.04	161.4	6.3	1338.4
	(0.03)	(178.8)	(497.6)	(1151.8)
N	434	400	434	427
Dependent variable mean	0.8	896	4485	2390

Notes: This table presents estimates of the effect of receiving money in one tranche (as opposed to two tranches) on a set of pre-registered outcomes, defined as follows: "Goal Progress" is an index aggregating measures of participants' self-reported progress on goals with respect to income, assets, and social status. "Δ Income" is the change in participants' total annual income from baseline to endline. "Assets + Expenditures" is the sum of assets owned at endline by the household, and the annualized value of household expenditures at endline. "Retrospective valuation" is the retrospective valuation (in USD) respondents assigned to the things they purchased using the transfer. The sample in each column is all recipients for whom tranche structure was randomly assigned and for whom the given outcome is observed. Following the pre-analysis plan, the underlying specification is the regression $y_h = \alpha + \beta_1 L_h(\tilde{L}_h) + \beta_2 L_h(1 - \tilde{L}_h) + \gamma \tilde{L}_h + \epsilon_h$, where L_h ($\tilde{L}_h$) indicates whether recipient h was assigned to (preferred to) receive payment in a single lump sum, as opposed to two installments. This yields group-specific estimated effects, which we then aggregate back up to an overall average treatment effect by calculating the weighted sum $\rho^1\beta_1 + \rho^2\beta_2$ where ρ^k is the fraction of subjects preferring to receive k tranches. Heteroskedasticity-robust standard errors are reported in parentheses.

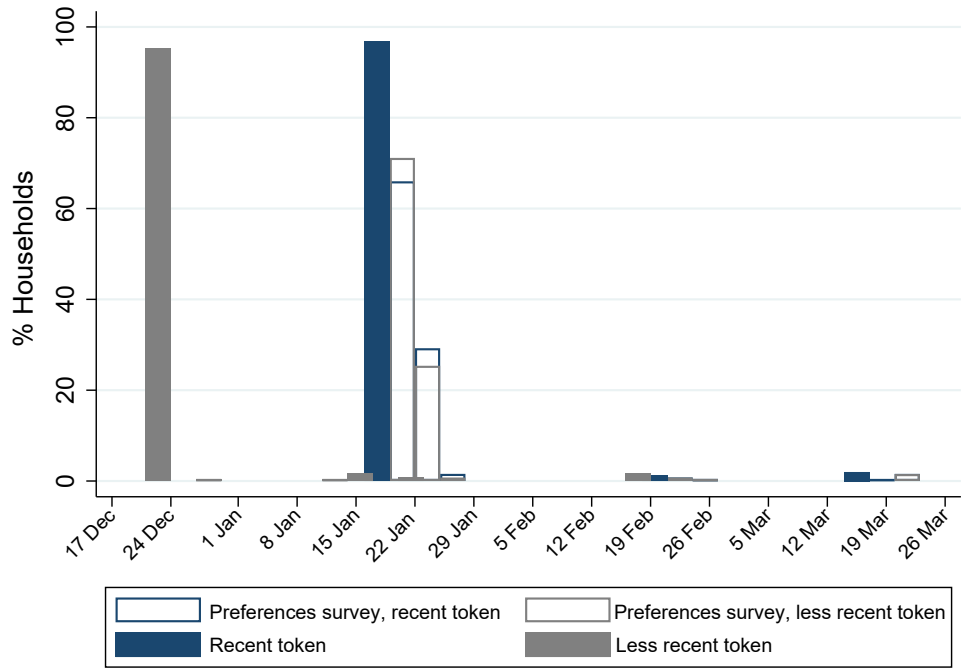
Table 2: Impacts of deferral

	Deliberation	Social input	Goal progress	Δ Income	Assets + expenditures	Valuation
	(1)	(2)	(3)	(4)	(5)	(6)
Deferral ($\sum$ _tq_h, tt)	0.042	0.013	0.011	142.2	96.4	655.6
	(0.034)	(0.014)	(0.013)	(65.1)	(222.9)	(686.8)
	[0.014]	[0.0081]	[0.0039]	[26.5]	[181.8]	[524.4]
Deferral squared ($\sum$ _tq_h, tt^2)	-0.0027	-0.0013	-0.0014	-13.5	-5.59	-58.8
	(0.0029)	(0.0012)	(0.0012)	(5.35)	(20.7)	(60.1)
	[0.0011]	[0.00071]	[0.00045]	[2.47]	[16.3]	[42.3]
Prefer deferral	-0.026	-0.016	0.050	114.8	169.7	-516.2
	(0.054)	(0.020)	(0.017)	(129.8)	(468.4)	(530.5)
	[0.044]	[0.013]	[0.0096]	[64.8]	[168.2]	[565.3]
Prefer 1 tranche	0.028	-0.0041	0.0064	-1.99	217.5	-495.0
	(0.044)	(0.017)	(0.018)	(119.0)	(336.8)	(575.7)
	[0.044]	[0.010]	[0.022]	[43.9]	[296.2]	[883.7]
N	424	424	424	393	424	417
Dependent variable mean	-0.00	0.60	0.81	893	4576	2480

Notes: This table reports estimated effects of the timing of the main transfer. The estimation sample is the 90% of participants for whom timing was assigned randomly, with variation across columns reflecting variation in the availability of the outcome variables. Estimates are from an instrumental variable analogue of Equation 1 which accounts for minor non-compliance with assigned deferral. The first stage regression is $q_{h,t} = \rho + \delta q_{h,t}^{\text{assigned}} + \mu_{h,t}$, where $q_{h,t}^{\text{assigned}}$ is the share of household h's transfer that it was assigned to receive in month t and $q_{h,t}$ the share it actually received. First stage coefficients and F-tests for instrument relevance are reported in Table A.12. The second-stage regression is then as defined by Equation 1 in the text, with additional controls that are indicators for preferring any deferral, and preferring one tranche more than two tranches. Outcomes are defined as follows: "Deliberation" is a standardized Anderson (2008) index aggregating measures of the extent to which recipients reported planning how to use their transfer; "Goal progress" is an index aggregating measures of participants' self-reported progress on goals with respect to income, assets, and social status; "Δ Income" is the change in participants' total annual income from baseline to endline; "Assets + expenditures" is the sum of assets owned at endline by the household, and the annualized value of household expenditures at endline; 'Social input" is an index indicating the extent to which a participant consulted their social network when deciding on the use of the grant transfer; and "Valuation" is the retrospective valuation (in USD) respondents assigned to the things they purchased using the transfer. Further details on the construction of some of these variables is in Appendix B. Heteroskedasticity-robust standard errors are in parentheses; standard errors clustered by timing preference (conditional on tranche count) are in square brackets.

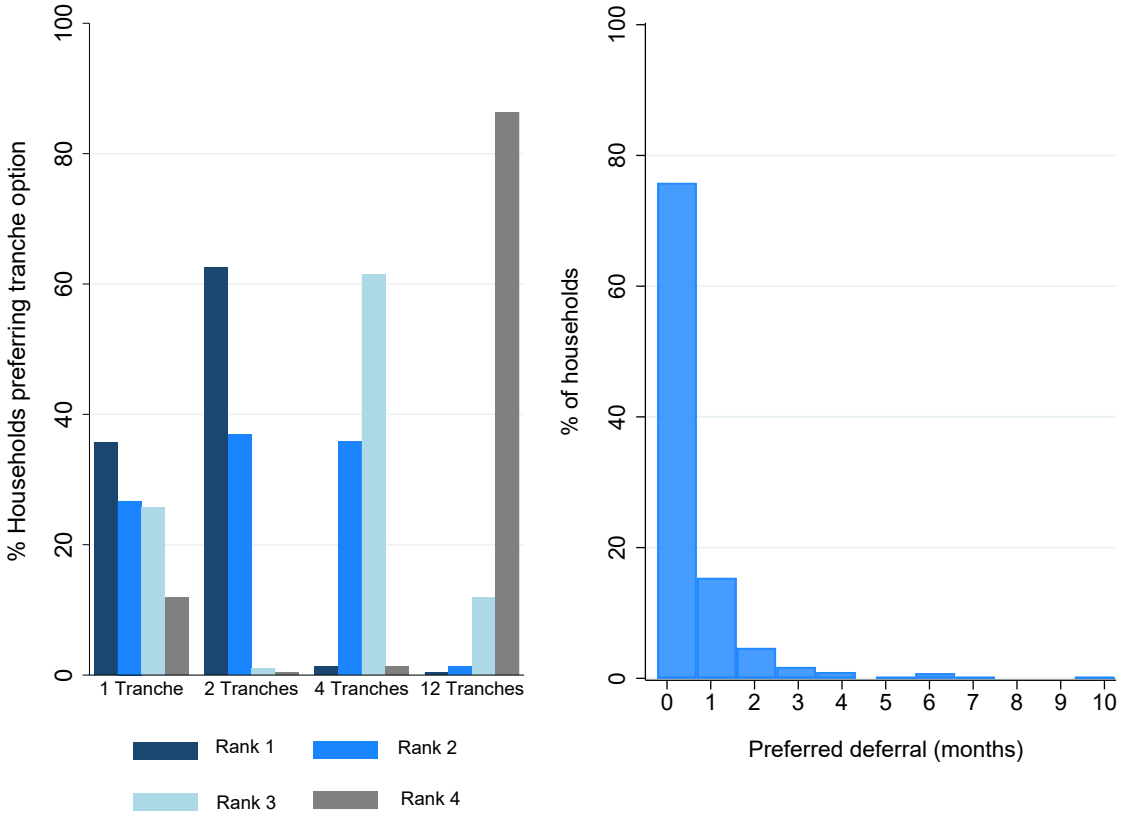
A Additional exhibits

Figure A.1: Preference survey dates and compliance with token transfer treatment



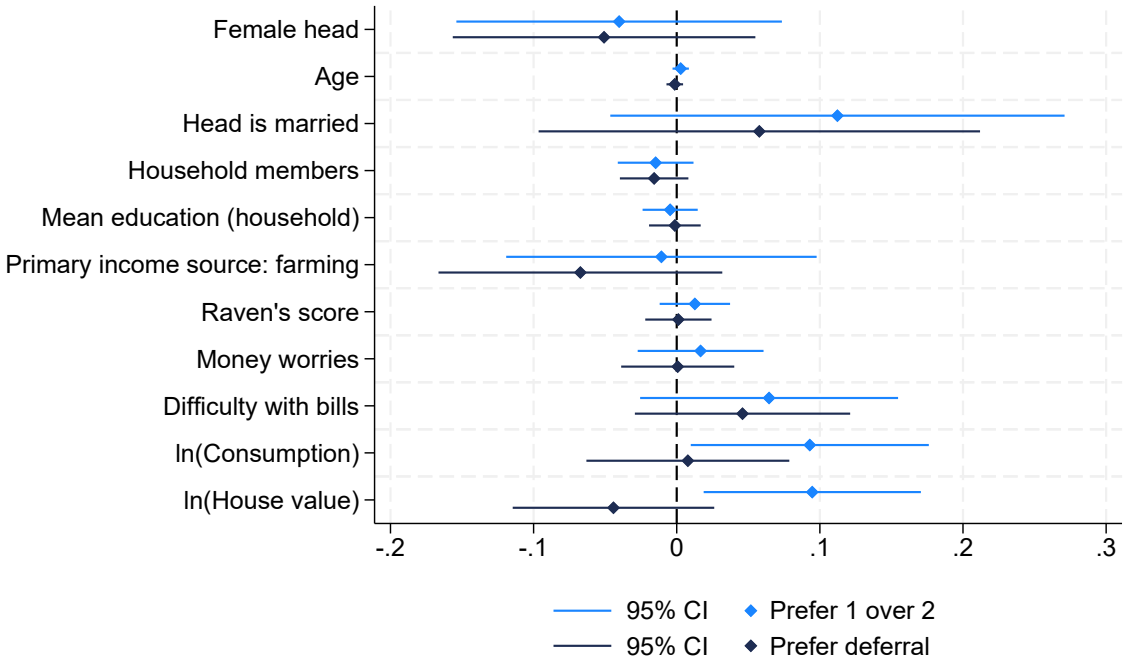
Notes: This figure presents distributions of the dates on which token transfers were issued (solid bars) and preference surveys were conducted (hollow bars) for the recent token group (in blue) and the less recent token group (in grey). Note that the great majority (94%) of preference surveys were conducted between 19–23 January 2015.

Figure A.2: Distributions of preferences over tranching and deferral



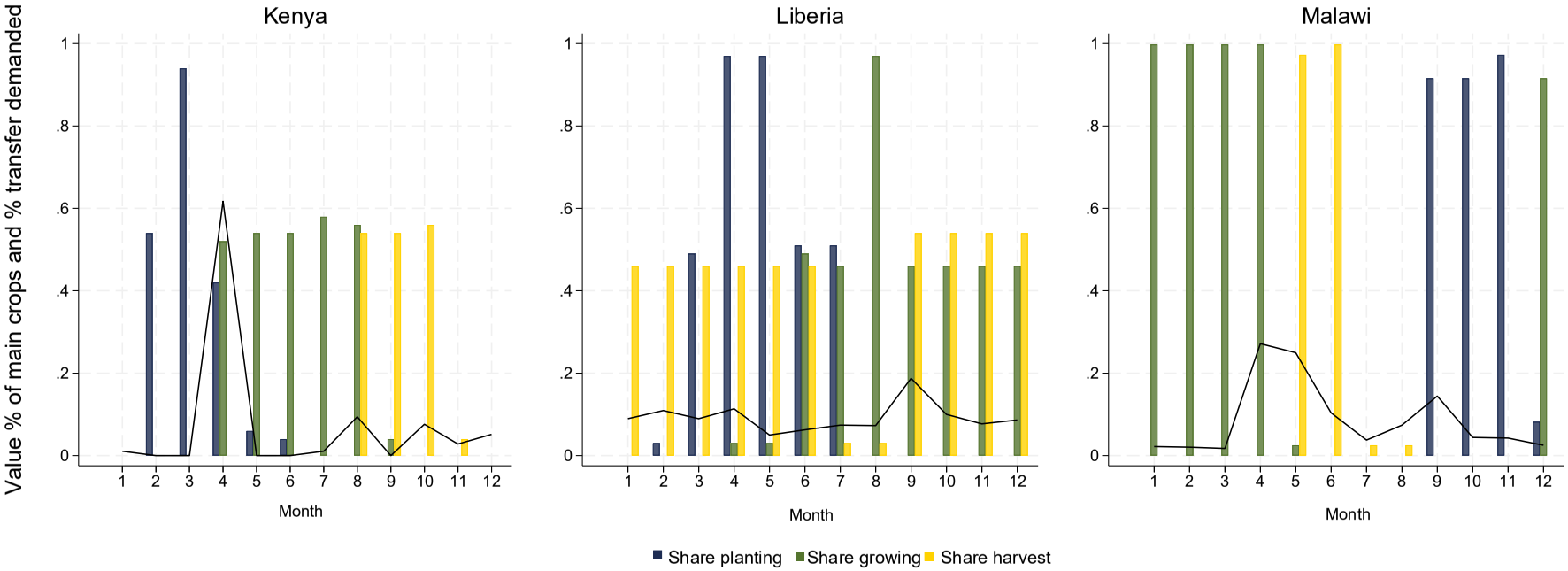
Notes: This figure presents distributions of participants' preference over transfer structures. The left panel illustrates their preference rankings of the available tranche structures (1, 2, 4, and 12 tranches). x-axis groupings indicate tranche counts, and the height of each bar within a grouping indicates the share of participants who ranked that tranche count first, second, third or fourth in their preference ordering. The right panel displays a histogram of the full distribution of deferral preferences among participants. Here, the x-axis denotes the number of months of deferral, with February 2015 represented as 0, and the height of each bar indicates the share of participants who preferred that many months of deferral.

Figure A.3: Predictors of preferences for tranching and deferral



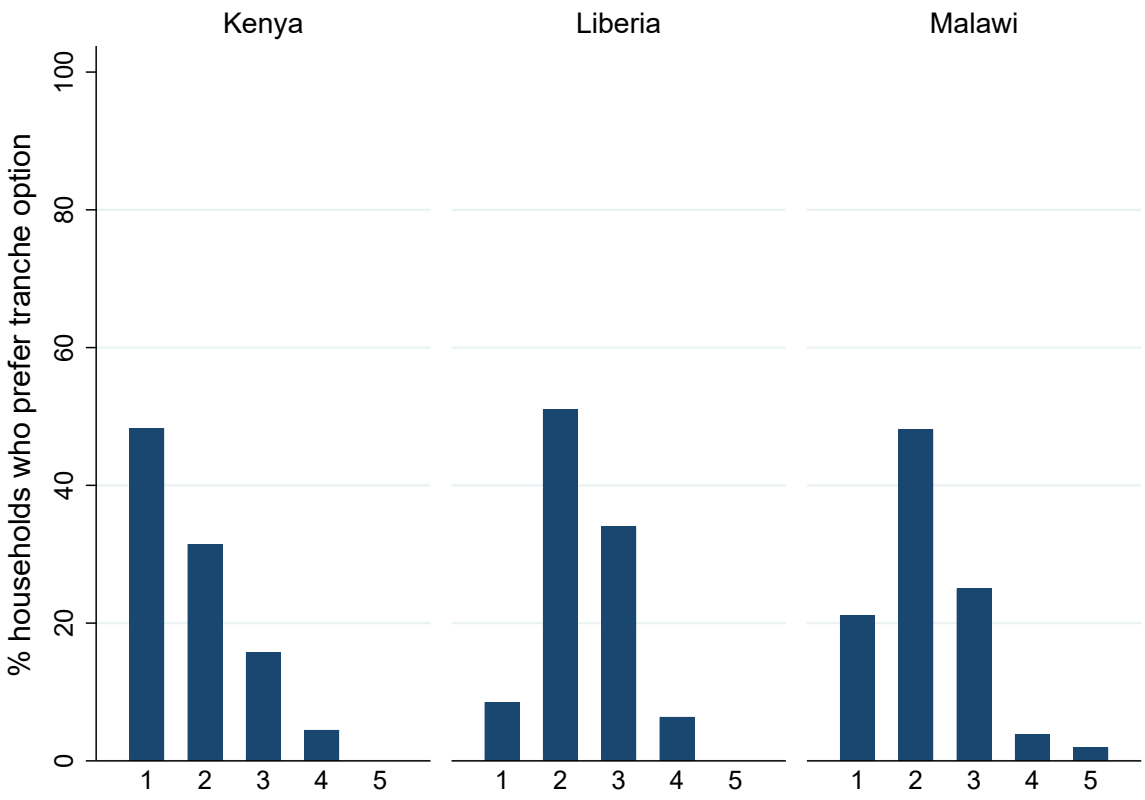
Notes: This figure plots estimated coefficients and confidence intervals from a probit model where the dependent variable is an indicator for preferring one tranche as opposed to two tranches (in grey) or an indicator for preferring any deferral (in blue), where any deferral means receiving the first tranche in at least a month after February 2015. Regressors are those indicated on the y-axis; see Appendix B for more detailed definitions of some of these variables. Diamonds represent estimated coefficients, and whiskers represent 95% confidence intervals based on heteroskedasticity-robust standard errors.

Figure A.4: Seasonal patterns in Kenya, Liberia, and Malawi



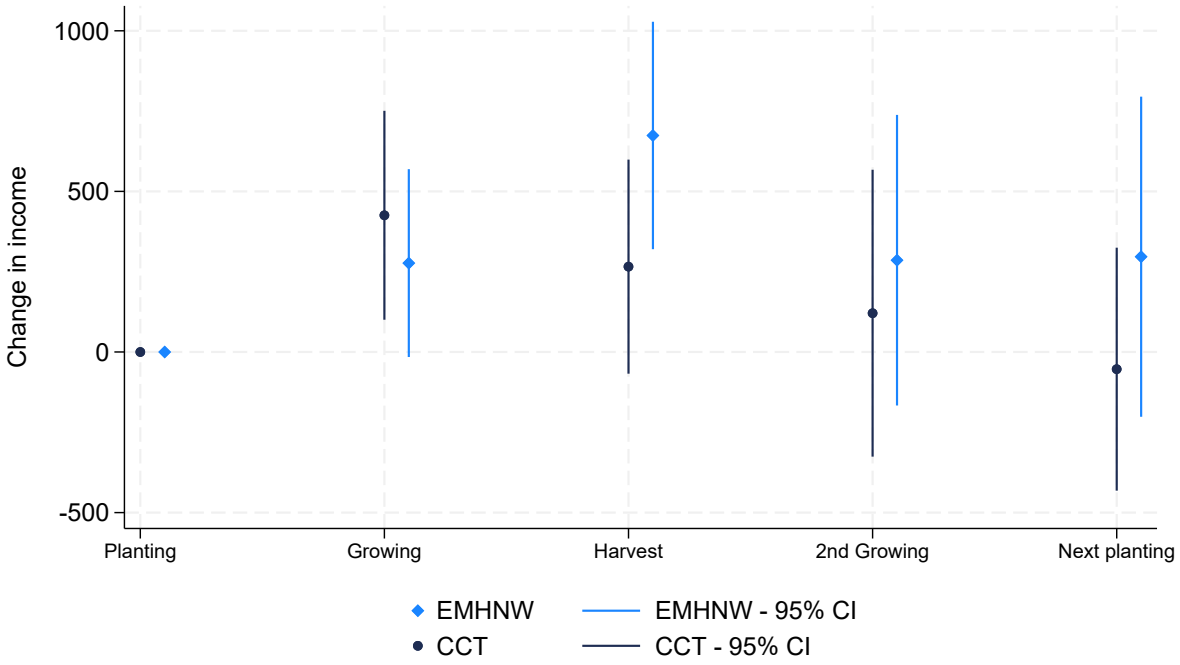
Notes: This figure displays measures of agricultural seasonality for the the three countries covered in the GD preference surveys discussed in Section 3.4. The x-axis in each panel indicates the calendar month (where 1 is January, 2 is February, and so on). For each month, bars indicate the value-weighted share of crops for which that month is typically a planting month (blue series), growing month (green series), or harvest month (yellow series). Value shares were calculated using production values sourced from FAOSTAT (FAO, Date of Access: 16.7.2023b) for 2021, the most recent available year. Crop calendars were sourced from FAO. (2023a) for Liberia and Malawi, and from Ndungu et al. (2019) for Kenya. The main crops for Kenya, used for the calculation of value shares in each season, are beans, barley, maize, millet, sorghum, and wheat; for Liberia, they are cassava, rice, and yam; and for Malawi, maize, rice, sorghum, and wheat. The overlaid black line depicts the share of the total cash transfer that the average respondent wished to receive in each month.

Figure A.5: Tranching preferences in Kenya, Liberia, and Malawi



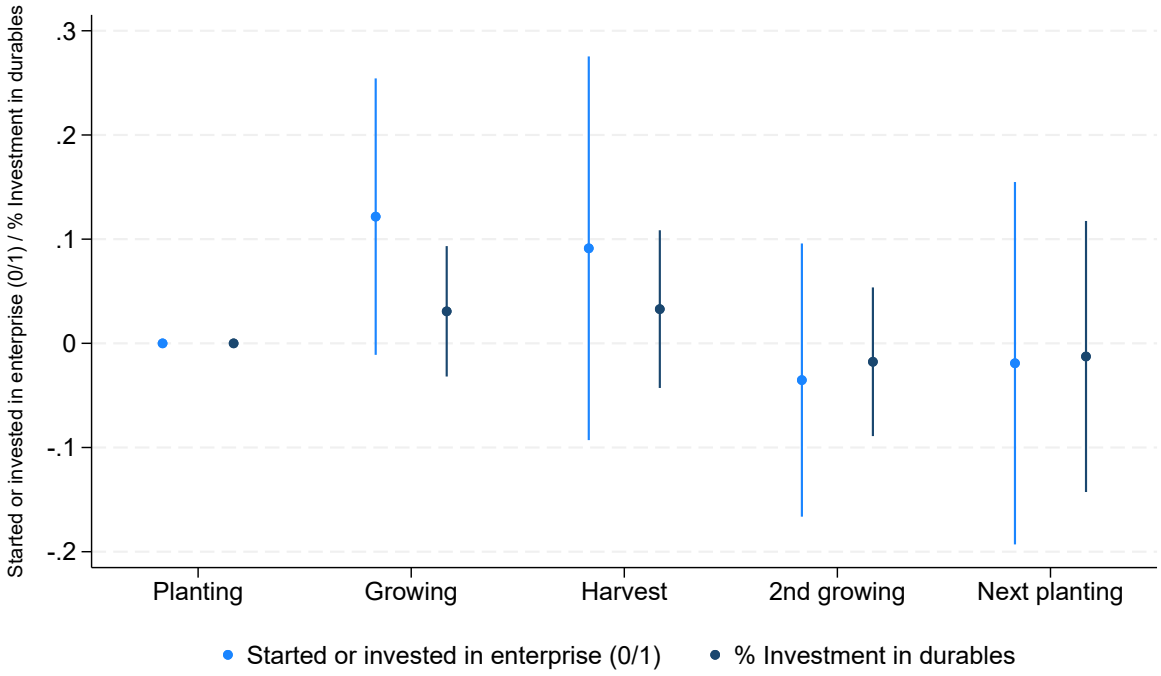
Notes: This figure displays stated preferences over number of tranches from the GD preference surveys discussed in Section 3.4. The x-axis indicates numbers of tranches, and bars represent the share of participants who preferred to receive their transfer in that many tranches. Note that while in practice no participant expressed a preference for more than 5 tranches, in principle up to 12 tranches were allowed.

Figure A.6: Comparison with the impact of transfer timing in Egger et al. (2022)



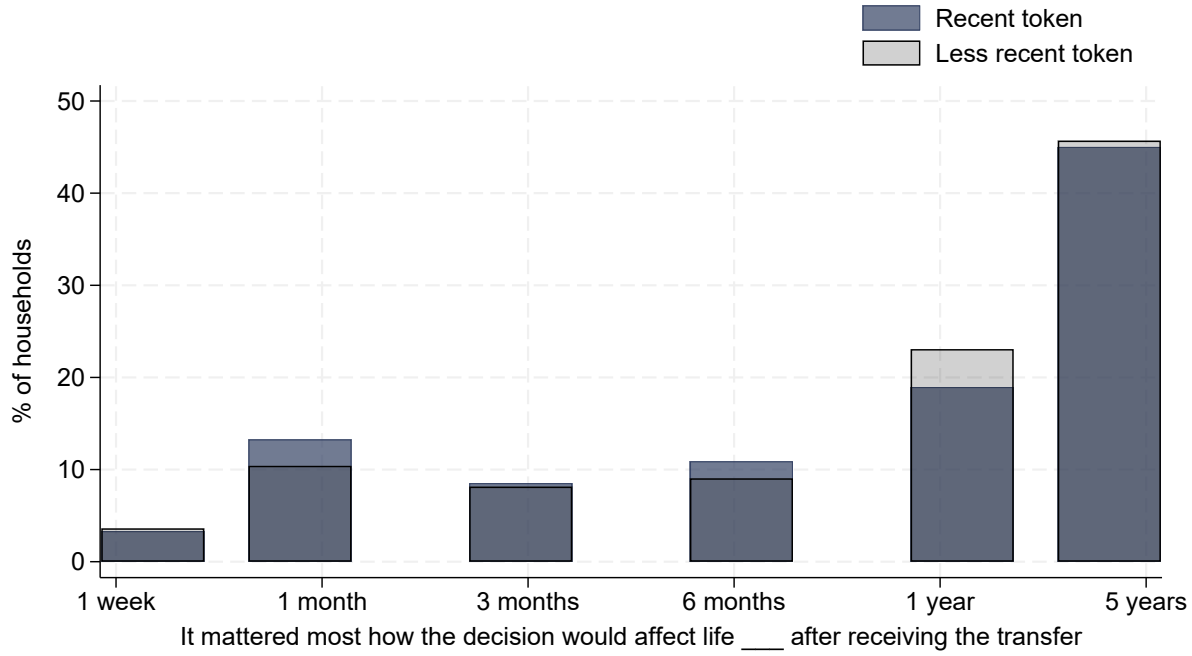
Notes: This figure plots estimated effects of cash transfers on the change in participants' income from baseline to endline by the season in which the money was received, using data both from our own study (CCT, dark blue series) and from Egger et al. (2022) (EHMNW, bright blue series). For comparability, income data from EHMNW has been converted to USD at the same rate of 1 USD per 87 KES applied to the CCT data. To further facilitate comparison, both planting coefficients are normalized to zero. As in Panel B of Figure 3, points and confidence intervals are derived from an underlying regression of the form $y_h = \alpha + \sum_s \beta_s q_{h,s} + X_h\gamma + \epsilon_h$ where y_h is the outcome of household h, α is a constant, $q_{h,s} = \{0, 0.5, 1\}$ is the share of overall transfer that household h received in a given season $s = \{$growing, harvest, 2^{nd}growing, next planting$\}$ where the first season, planting, is the omitted category, X_h denotes controls (as in Equation 1) and ϵ_h is the error term. The regression using the EHMNW data includes a control for treatment assignment per se (as their design included a pure control group that received no transfers) and indicators for the month of the endline survey (as unlike our endline survey theirs took place at varying dates). The regression for the CCT data include controls for preference for deferral and preference for one as opposed to two tranches. Seasons are defined as in Ndungu et al. (2019), with the refinement that we assign January 2016 to the "next planting" season to distinguish it from the part of the planting season (February–March 2015) immediately after preferences were elicited. The estimation samples are the 90% of the CCT sample that was randomly assigned to deferral, and the full EHMNW sample. Standard errors are heteroskedasticity-robust for the CCT results, and clustered at the village level for the EHMNW results (since treatment assignment in their design was clustered at the village level). Whiskers represent 95% confidence intervals.

Figure A.7: Impact of deferral on measures of investment



Notes: This figure plots estimated effects of transfer timing on the share of expenditure on durables (dark blue markers) and on an indicator for using the transfer to start or invest in a non-agricultural enterprise (bright-blue markers), with the data broken out by season as defined in Ndungu et al. (2019). The share of durables is calculated as the proportion of total expenditures allocated to construction material, furniture, transportation vehicles, and other durable assets. (For reference, non-durables include food, clothing, household items, airtime, as well as ceremony and funeral expenses). The regression coefficients plotted (point estimates plotted as circles or diamonds and confidence intervals as whiskers) come from estimating the following specification: $y_h = \alpha + \sum_s \beta_s q_{h,s} + X_h\gamma + \epsilon_h$ where y_h is the outcome of household h, α is a constant, $q_{h,s} = \{0, 0.5, 1\}$ is the share of overall transfer that household h received in a given season $s = \{$growing, harvest, 2^{nd}growing, next planting$\}$ where the first season, planting, is the omitted category, X_h denotes controls (as in Equation 1) and ϵ_h is the error term. Months belonging to each season are defined as in Ndungu et al. (2019), with the refinement that we assign January 2016 to the "next planting" season to distinguish it from the part of the planting season (February–March 2015) immediately after preferences were elicited. In the case of the enterprise investment, there is an additional control for the number of enterprises owned by each recipient before receiving the transfer, to enhance precision in the estimates obtained. Standard errors are heteroskedasticity-robust. Whiskers on the bars denote 95% confidence intervals. p-values for tests of equality across seasons are 0.63 for the durables expenditure share and 0.26 for the enterprise investment indicator.

Figure A.8: Time horizons when planning spending, by token transfer timing



Notes: This figure plots the distribution of participants' responses to the question: "When deciding on how to use the main grant transfer, it mattered most how the decision would affect life .. (i) One week after receiving the transfer; (ii) One month after receiving the transfer; (iii) Three months after receiving the transfer; (iv) One year after receiving the transfer; (v) Five years after receiving the transfer." Distributions are plotted separately for recipients experimentally assigned to receive a recent v.s. less recent token transfers. The p-value for a Kolmogorov-Smirnov test of the null that the distributions are the same is 0.99.

Table A.1: Transfer tranche & timing assignment conditional on preferences

	Assigned preferred		Assigned random		Totals
A. Tranches	1 Tranche	2 Tranches	1 Tranche	2 Tranches	
Preferred 1 tranche	18	0	84	81	183
Preferred 2 tranches	0	33	147	150	330
Total	18	33	231	231	513
B. Deferral	Immediate	Deferral	Immediate	Deferral	
Preferred immediate	41	0	41	307	389
Preferred deferral	0	14	14	96	124
Total	41	14	55	403	513

Notes: This table presents information on the joint distribution of preferences and treatment assignment with respect to the structure of the main transfer on two dimensions, tranching (Panel A) and timing (Panel B), focusing in later case on whether transfers can with any deferral. The column group "Assigned preference" describes the 10% of subjects who were randomly assigned to receive their preferred transfer structure (so that preferences and assignments are identical). The column group "Assigned random" describes the 90% of subjects assigned to receive transfer structures drawn uniformly at random from the set of possible structures (so that preferences and assignments are independent).

Table A.2: Balance with respect to token transfer timing

	Less recent token	More recent token	p-value
	(1)	(2)	(3)
Income	350.92	351.03	1.00
	(31.28)	(51.61)	
Total assets, including land	1569.97	1854.69	0.54
	(175.00)	(435.32)	
Total monthly consumption	132.57	132.22	0.97
	(7.39)	(5.36)	
Age	34.24	34.47	0.85
	(0.80)	(0.88)	
Education	8.32	7.95	0.23
	(0.22)	(0.21)	
Female head	0.68	0.65	0.49
	(0.03)	(0.03)	
Mean education (household)	8.48	8.08	0.22
	(0.25)	(0.21)	
Head is married	0.77	0.80	0.42
	(0.03)	(0.03)	
Head is a widow/widower	0.15	0.12	0.24
	(0.02)	(0.02)	
Total adults above age 70	2.10	1.95	0.08
	(0.07)	(0.05)	
Total adults below age 70	0.06	0.06	0.94
	(0.02)	(0.02)	
Total children ages 0-3	0.57	0.62	0.53
	(0.05)	(0.05)	
Total school-age children (ages 3–18)	2.72	2.56	0.31
	(0.11)	(0.11)	
% Household members sick	0.46	0.49	0.32
	(0.02)	(0.02)	
Number of stoves owned	0.21	0.19	0.64
	(0.03)	(0.03)	
Number of waterdrums owned	0.55	0.56	0.93
	(0.06)	(0.06)	
Number of radios owned	0.62	0.62	0.96
	(0.04)	(0.04)	
Number of TVs or computers owned	0.05	0.04	0.72
	(0.01)	(0.01)	
Assets: electronic appliances	42.90	44.56	0.73
	(3.09)	(3.73)	
Assets: transport	29.92	36.97	0.49
	(6.25)	(8.01)	
Land size	0.74	0.72	0.90
	(0.08)	(0.10)	
Remittances	−1.53	−0.57	0.74
	(2.68)	(0.99)	
Household skipped meals	0.59	0.59	0.99
	(0.03)	(0.03)	
N	260	253	
H_0: all means are equal			0.30

Notes: This table presents balance on baseline characteristics between the groups randomly assigned to less versus more recent token transfer receipt. The sample includes all households observed at endline. Columns 1 and 2 present the mean and heteroskedasticity-robust standard errors (in parenthesis) of the baseline variables listed for each of those groups. Appendix B provides further details on the construction of some of these variables. Column 3 reports the p-value from an F-test of the null of orthogonality between treatment assignment and each covariate individually, and then at bottom the p-value from an F-test of the joint null of orthogonality with respect to all covariates.

Table A.3: Balance with respect to main transfer tranching

	1 Tranche	2 Tranches	p-value
Income	371.34	314.25	0.40
	(61.41)	(28.93)	
Total assets, including land	1675.32	1880.36	0.71
	(215.49)	(516.00)	
Total monthly consumption	135.85	124.15	0.21
	(7.47)	(5.69)	
Age	34.67	34.86	0.88
	(0.89)	(0.94)	
Education	7.93	8.25	0.35
	(0.24)	(0.24)	
Female head	0.68	0.67	0.82
	(0.03)	(0.03)	
Mean education (household)	8.09	8.40	0.41
	(0.26)	(0.26)	
Head is married	0.77	0.80	0.52
	(0.03)	(0.03)	
Head is a widow/widower	0.16	0.12	0.22
	(0.03)	(0.02)	
Total adults above age 70	2.02	2.06	0.73
	(0.06)	(0.07)	
Total adults below age 70	0.06	0.07	0.79
	(0.02)	(0.02)	
Total children ages 0-3	0.69	0.55	0.09
	(0.06)	(0.05)	
Total school-age children (ages 3–18)	2.58	2.78	0.26
	(0.12)	(0.13)	
% Household members sick	0.48	0.48	0.90
	(0.02)	(0.02)	
Number of stoves owned	0.21	0.20	0.78
	(0.03)	(0.03)	
Number of waterdrums owned	0.63	0.52	0.26
	(0.07)	(0.07)	
Number of radios owned	0.67	0.58	0.13
	(0.04)	(0.04)	
Number of TVs or computers owned	0.04	0.05	0.60
	(0.01)	(0.02)	
Assets: electronic appliances	43.42	42.80	0.91
	(3.72)	(3.88)	
Assets: transport	34.31	37.09	0.81
	(7.48)	(9.06)	
Land size	0.76	0.75	0.91
	(0.09)	(0.12)	
Remittances	−0.12	−1.17	0.76
	(0.93)	(3.27)	
Household skipped meals	0.57	0.64	0.14
	(0.03)	(0.03)	
Recent token	0.53	0.49	0.36
	(0.03)	(0.03)	
N	219	212	
H_0: all means are equal:			0.19

Notes: This table presents balance on baseline characteristics between the groups randomly assigned to receive one versus two tranches. The sample includes all households observed at endline and thus assigned; it excludes households that were assigned to receive the number of tranches they preferred. Columns 1 and 2 present the mean and heteroskedasticity-robust standard errors (in parenthesis) of the baseline variables listed for each of those groups. Appendix B provides further details on the construction of some of these variables. Column 3 reports the p-value from an F-test of the null of orthogonality between treatment assignment and each covariate individually, and then at bottom the p-value from an F-test of the joint null of orthogonality with respect to all covariates.

Table A.4: Balance with respect to main transfer timing

	0	1	2	3	4	5	6	7	8	9	10	11	p−*value*
Income	411.88	366.01	289.47	299.86	477.03	306.74	269.78	223.12	380.08	953.35	180.75	222.99	0.80
	(63.69)	(77.85)	(52.30)	(57.95)	(82.68)	(40.28)	(86.74)	(45.06)	(159.27)	(663.67)	(28.16)	(55.01)	
Total assets, including land	1293.01	3209.95	1545.63	1239.35	1328.73	1412.11	1330.93	2388.68	1481.49	3851.48	1378.58	1670.89	0.87
	(194.35)	(1955.73)	(289.30)	(178.94)	(260.92)	(273.90)	(405.18)	(877.72)	(698.68)	(1947.90)	(478.86)	(313.63)	
Total monthly consumption	130.17	134.96	135.54	129.60	129.81	121.37	132.86	118.57	117.87	141.25	153.83	154.18	0.34
	(10.59)	(15.97)	(13.74)	(20.23)	(11.67)	(11.88)	(16.26)	(19.78)	(12.94)	(26.25)	(29.13)	(43.23)	
Age	34.24	35.00	34.70	34.38	33.80	33.48	31.33	35.67	36.76	33.33	38.22	41.47	0.05
	(1.63)	(1.95)	(1.97)	(2.18)	(1.60)	(1.59)	(1.92)	(3.83)	(3.77)	(3.64)	(2.60)	(4.25)	
Education	8.91	7.30	8.37	7.96	8.57	7.93	8.33	7.61	7.71	7.22	8.39	7.41	0.76
	(0.53)	(0.44)	(0.41)	(0.58)	(0.43)	(0.52)	(1.06)	(0.71)	(0.81)	(0.76)	(1.05)	(0.90)	
Female head	0.70	0.81	0.65	0.62	0.67	0.64	0.61	0.78	0.71	0.56	0.50	0.76	0.18
	(0.06)	(0.05)	(0.07)	(0.07)	(0.06)	(0.06)	(0.12)	(0.10)	(0.11)	(0.12)	(0.12)	(0.11)	
Mean education (household)	9.05	7.64	8.48	7.70	8.45	8.28	8.71	8.00	7.90	6.58	8.58	7.70	0.75
	(0.55)	(0.42)	(0.54)	(0.66)	(0.32)	(0.57)	(1.03)	(0.70)	(0.89)	(1.08)	(1.01)	(0.88)	
Head is married	0.81	0.83	0.81	0.77	0.83	0.77	0.83	0.67	0.88	0.78	0.72	0.71	0.55
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)	(0.09)	(0.11)	(0.08)	(0.10)	(0.11)	(0.11)	
Head is a widow/widower	0.11	0.11	0.09	0.13	0.11	0.16	0.06	0.33	0.12	0.17	0.17	0.24	0.44
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.06)	(0.11)	(0.08)	(0.09)	(0.09)	(0.11)	
Total adults above age 70	1.98	1.85	2.37	1.90	2.13	2.12	2.06	2.00	1.82	1.89	1.89	2.12	0.93
	(0.12)	(0.09)	(0.19)	(0.08)	(0.13)	(0.17)	(0.26)	(0.31)	(0.21)	(0.29)	(0.16)	(0.21)	
Total adults below age 70	0.04	0.08	0.04	0.08	0.06	0.07	0.00	0.06	0.18	0.06	0.00	0.18	0.43
	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.00)	(0.06)	(0.13)	(0.06)	(0.00)	(0.13)	
Total children ages 0-3	0.70	0.65	0.76	0.35	0.65	0.47	0.85	0.45	0.58	0.45	1.00	0.71	0.35
	(0.13)	(0.09)	(0.12)	(0.10)	(0.13)	(0.11)	(0.25)	(0.16)	(0.15)	(0.21)	(0.15)	(0.29)	
Total school-age children (ages 3–18)	3.11	2.55	2.71	2.60	2.60	2.74	2.31	2.31	2.54	2.71	2.93	3.18	0.52
	(0.24)	(0.20)	(0.26)	(0.27)	(0.25)	(0.23)	(0.44)	(0.40)	(0.46)	(0.40)	(0.52)	(0.55)	
% Household members sick	0.52	0.50	0.47	0.47	0.48	0.40	0.52	0.60	0.34	0.41	0.52	0.61	0.71
	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.09)	(0.09)	(0.07)	(0.08)	(0.08)	(0.10)	
Number of stoves owned	0.24	0.17	0.13	0.13	0.22	0.25	0.17	0.17	0.12	0.33	0.39	0.24	0.45
	(0.06)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.12)	(0.09)	(0.08)	(0.18)	(0.12)	(0.14)	
Number of waterdrums owned	0.65	0.62	0.52	0.52	0.57	0.48	0.61	0.61	0.41	0.22	0.83	0.76	0.73
	(0.11)	(0.16)	(0.10)	(0.16)	(0.16)	(0.12)	(0.24)	(0.20)	(0.15)	(0.10)	(0.29)	(0.25)	
Number of radios owned	0.78	0.55	0.67	0.52	0.57	0.54	0.61	0.67	0.59	0.94	0.83	0.65	0.31
	(0.10)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.14)	(0.18)	(0.15)	(0.24)	(0.15)	(0.15)	
Number of TVs or computers owned	0.06	0.00	0.04	0.00	0.02	0.09	0.17	0.00	0.06	0.00	0.06	0.00	0.78
	(0.03)	(0.00)	(0.03)	(0.00)	(0.02)	(0.05)	(0.09)	(0.00)	(0.06)	(0.00)	(0.06)	(0.00)	
Assets: electronic appliances	57.37	31.46	34.88	36.68	40.45	50.36	73.63	39.71	38.18	63.48	50.51	29.50	0.88
	(11.16)	(2.49)	(2.32)	(5.36)	(3.66)	(10.21)	(28.72)	(8.69)	(5.31)	(23.57)	(10.50)	(4.05)	
Assets: transport	50.68	54.04	12.11	26.77	15.26	34.42	43.36	102.30	9.13	21.58	48.28	10.89	0.75
	(20.40)	(24.96)	(2.93)	(13.40)	(3.49)	(15.50)	(31.54)	(62.88)	(4.33)	(8.38)	(19.80)	(4.59)	
Land size	0.61	0.94	0.79	0.67	0.49	0.65	0.57	1.07	0.57	1.26	0.59	0.75	0.96
	(0.15)	(0.38)	(0.17)	(0.15)	(0.09)	(0.11)	(0.18)	(0.24)	(0.36)	(0.69)	(0.20)	(0.13)	
Remittances	−3.38	−7.78	−0.55	3.30	6.15	−5.33	−2.81	−0.50	1.35	−0.38	1.61	0.24	0.16
	(2.40)	(6.59)	(1.79)	(5.37)	(7.16)	(6.47)	(2.60)	(2.64)	(0.70)	(1.92)	(2.64)	(4.01)	
Household skipped meals	0.54	0.66	0.67	0.60	0.57	0.52	0.50	0.44	0.71	0.61	0.89	0.47	0.54
	(0.07)	(0.07)	(0.06)	(0.07)	(0.07)	(0.07)	(0.12)	(0.12)	(0.11)	(0.12)	(0.08)	(0.12)	
Recent token	0.50	0.51	0.56	0.35	0.50	0.48	0.61	0.44	0.59	0.50	0.33	0.47	0.39
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)	(0.12)	
$H_0 : \beta_1 = 0$													0.32
$H_0 : \beta_2 = 0$													0.35
N	54	53	54	52	54	56	18	18	17	18	18	17	

Notes: This table presents balance on baseline characteristics between the groups randomly assigned to receive transfers beginning in the months indicated in the columns, with 0 corresponding to February 2015. The sample includes all households observed at endline and thus assigned; it excludes households that were assigned to receive the timing they preferred. Columns 1–12 present the mean and heteroskedasticity-robust standard errors (in parenthesis) of the baseline variables listed for each of those groups. Appendix B provides further details on the construction of some of these variables. Column 13 reports the p-value from an F-test of the null of orthogonality between timing assignment and each covariate individually (obtained by estimating Equation 1), and then at bottom the p-value from an F-test of the joint null of orthogonality with respect to all covariates.

Table A.5: Compliance with tranche assignment

	Received		
Assigned	1 tranche	2 tranches	Total
1 tranche	234	0	234
2 tranches	0	223	223
Total	234	223	457

Notes: This table illustrates compliance with experimentally assigned transfer tranching (one or two tranches) within the 90% of the sample for which tranching was randomly assigned. The rows represent the number of tranches each household was assigned to receive, and the columns represent the number they actually received. Entries in the diagonal cells are thus compliers, and those in the off-diagonal cells are non-compliers.

Table A.6: Compliance with timing assignment

	Received												
Assigned	0	1	2	3	4	5	6	7	8	9	10	11	Total
0	56	0	0	0	0	0	0	0	0	0	0	0	56
1	0	55	1	0	0	0	0	0	0	0	0	0	56
2	0	0	56	0	1	0	0	0	0	0	0	0	57
3	0	0	0	54	1	1	0	0	0	0	0	0	56
4	0	0	0	0	56	1	1	0	0	0	0	0	58
5	0	1	0	0	0	54	0	2	0	0	0	0	57
6	0	0	0	0	0	0	18	0	1	0	0	0	19
7	0	1	0	0	0	0	0	18	0	0	0	0	19
8	0	1	0	0	0	0	0	0	18	0	0	0	19
9	0	0	0	0	0	0	0	0	0	18	0	0	18
10	0	1	0	0	0	0	0	0	0	0	18	0	19
11	0	0	0	0	0	0	0	0	0	0	0	18	18
Total	56	59	57	54	58	56	19	20	19	18	18	18	452

Notes: This table illustrates compliance with experimentally assigned transfer timing within the 90% of the sample for which timing was randomly assigned. The rows represent the month in which each household was assigned to begin receiving transfers, and the columns represent the months in which they actually began receiving transfers. Entries in the diagonal cells are thus compliers, and those in the off-diagonal cells are non-compliers. Section 2.2 describes the circumstances which gave rise to the handful of cases of non-compliance.

Table A.7: Attrition

	(1)	(2)
Assigned 1 tranche	-0.01	
	(0.02)	
Assigned preferred # of tranches	-0.01	
	(0.04)	
Assigned to preferred months of deferral		0.06
		(0.05)
Assigned 1 month of deferral		0.05
		(0.05)
Assigned 2 months of deferral		0.02
		(0.05)
Assigned 3 months of deferral		0.07
		(0.05)
Assigned 4 months of deferral		0.03
		(0.05)
Assigned 5 months of deferral		0.02
		(0.05)
Assigned 6 months of deferral		0.02
		(0.07)
Assigned 7 months of deferral		0.02
		(0.07)
Assigned 8 months of deferral		0.07
		(0.07)
Assigned 9 months of deferral		-0.04
		(0.07)
Assigned 10 months of deferral		0.02
		(0.07)
Assigned 11 months of deferral		0.02
		(0.07)
N	513	513
p-value	0.942	0.945

Notes: This table describes relationships between attrition and treatment assignment. Each column reports the results of a separate regression, and the outcome in each regression is an indicator equal to one if the household was not surveyed at endline. The regressors are indicators for the various possible treatment arms; each regression specification includes a constant term, so that effects are measured relative to an omitted category which is "Assigned 2 tranches" in Column 1 and "Assigned 0 months of deferral" in Column 2. Note that "Assigned preferred # of tranches" and "Assigned preferred months of deferral" indicate whether the household was assigned to receive its preferred tranching or timing, respectively. Standard errors are in parentheses; note that we are unable to report heteroskedasticity-robust standard errors in this case, as the resulting variance-covariance matrix is not full rank and hence the F-test of interest cannot be computed. The p-values reported at the bottom of the table are from tests of the joint null of orthogonality between the outcome and all regressors.

Table A.8: Joint distribution of preferences over timing and tranching

	Preferred tranche count				
Preferred months of deferral	1	2	4	12	Total
0	131	253	3	2	389
1	29	47	3	0	79
2	11	12	1	0	24
3	3	6	0	0	9
4	2	3	0	0	5
5	1	0	0	0	1
6	4	0	0	0	4
7	1	0	0	0	1
8	0	0	0	0	0
9	0	0	0	0	0
10	1	0	0	0	1
11	0	0	0	0	0
Total	183	321	7	2	513

Notes: This table presents the joint distribution of recipients first-choice preferences over transfer tranches (columns) and timing (rows). A deferral preference of 0 corresponds to transfer onset in February 2015.

Table A.9: Predicting preferences using baseline covariates

(A) Regression coefficients for PMT covariates	Tranching	Deferral
	(1)	(2)
Age	0.00	-0.01
	(0.00)	(0.00)
Education	0.01	0.00
	(0.02)	(0.02)
Female head	0.09	-0.02
	(0.08)	(0.07)
Mean education (household)	-0.01	-0.01
	(0.02)	(0.02)
Head is married	0.26	0.08
	(0.19)	(0.21)
Head is a widow/widower	0.05	0.10
	(0.20)	(0.23)
Total adults above age 70	-0.04	0.06
	(0.05)	(0.05)
Total adults below age 70	-0.31	0.14
	(0.19)	(0.21)
Total children ages 0-3	-0.08	0.01
	(0.04)	(0.04)
Total school-age children (ages 3–18)	-0.02	-0.03
	(0.02)	(0.02)
% Household members sick	-0.17	0.10
	(0.10)	(0.10)
Number of stoves owned	-0.07	-0.04
	(0.07)	(0.07)
Number of waterdrums owned	0.03	0.01
	(0.03)	(0.03)
Number of radios owned	0.03	0.06
	(0.06)	(0.06)
Number of TVs or computers owned	-0.09	-0.10
	(0.20)	(0.18)
Assets: electronic appliances	0.00	-0.00
	(0.00)	(0.00)
Assets: transport	-0.00	-0.00
	(0.00)	(0.00)
Land size	-0.02	-0.02
	(0.01)	(0.01)
Remittances	0.00	-0.00
	(0.00)	(0.00)
Household skipped meals	0.03	-0.11
	(0.07)	(0.06)
N	269	247
Adjusted R^2	-0.00	0.00
(B) Error rates from prediction models		
Modal preference	0.36	0.24
Generalized random forest, PMT covariates	0.36	0.24
Generalized random forest, all covariates	0.35	0.24

Notes: This table describes the predictability of respondents' preferences using baseline observables. Panel A presents results from regressions of indicators for preferring one tranche to two tranches (Column 1) and for preferring any deferral (Column 2) on the variables indicated in the rows. Appendix B provides further details on the construction of some of these variables. Panel B presents results from models trained to predict recipient preference for one as opposed to two tranches (Column 1) and for some rather than no deferral (Column 2). The first row ("Modal preference") reports the error rate obtained if we simply predict that each household prefers the modal choice (which is two transfers with no deferral). The second and third rows report the error rate obtained using a Generalized Random Forest model (Athey et al., 2019) trained on a limited set of baseline covariates akin to those commonly use in proxy means tests, and on all available baseline covariates, respectively.

Table A.10: Impact of deferral: robustness to omitting controls

	Deliberation		Social input		Goal Progress		Δ Income		Assets + Expenditures		Valuation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Deferral ($\sum_t q_{h,t}t$)	0.01	0.04	-0.0008	0.01	-0.005	0.01	-7.1	137.1	32.7	76.6	9.3	642.3
	(0.008)	(0.03)	(0.003)	(0.01)	(0.003)	(0.01)	(23.0)	(64.5)	(60.5)	(221.5)	(59.7)	(676.8)
Deferral squared ($\sum_t q_{h,t}t^2$)		-0.003		-0.001		-0.001		-13.1		-4		-57.2
		(0.003)		(0.001)		(0.001)		(5.3)		(20.6)		(58.8)
N	424	424	424	424	424	424	395	395	426	426	417	417
Dependent variable mean	-0.00	-0.00	0.6	0.6	0.8	0.8	891	891	4565	4565	2480	2480

Notes: This table presents results analogous to those in Table A.11 (odd-numbered columns) and Table 2 (even-numbered columns), but omitting the control variables included there. The omitted controls are indicators for: preferring one tranche to two and preferring any delay. Results are estimated for the 90% sub-sample for which timing was assigned randomly. Heteroskedasticity-robust standard errors are in parentheses.

Table A.11: Impact of deferral: linear specification

	Deliberation	Social input	Goal progress	Δ Income	Assets + expenditures	Valuation
	(1)	(2)	(3)	(4)	(5)	(6)
Deferral ($\sum_t q_{h,t}t$)	0.012	-0.00085	-0.0043	-6.55	34.5	4.93
	(0.0084)	(0.0033)	(0.0034)	(22.5)	(60.3)	(58.4)
N	424	424	424	393	424	417
Dependent variable mean	-0.00	0.60	0.81	893	4576	2480

Notes: This table presents estimated effects of deferral using a linear specification of deferral on the outcomes presented in Table 2. Results are estimated for the 90% sub-sample for which timing was assigned randomly, with differences in observation counts across columns reflecting differences in availability of the outcomes. The specification also includes controls for preferences (preference for any deferral, and preference for one as opposed to two tranches) results for which are not reported. Heteroskedasticity-robust standard errors are in parentheses.

Table A.12: First-stage results

	Deliberation			Δ Income		
	(1)	(2)	(3)	(4)	(5)	(6)
	Deferral ($\sum_t q_{h,t}t$)		Deferral squared ($\sum_t q_{h,t}t^2$)	Deferral ($\sum_t q_{h,t}t$)		Deferral squared ($\sum_t q_{h,t}t^2$)
Assigned deferral ($\sum_t q_{h,t}t$)	0.99	1.00	0.12	1.00	1.00	0.16
	(0.0044)	(0.035)	(0.32)	(0.0042)	(0.037)	(0.35)
Assigned deferral squared ($\sum_t q_{h,t}t^2$)		-0.00063	0.99		-0.00077	0.99
		(0.0030)	(0.028)		(0.0032)	(0.031)
N	424	424	424	393	393	393
F-statistic	50081		4102	56225		3876

Notes: This table presents results from the first-stage regressions underlying the estimated effects of deferral in Table 2. Each column group reports results for the sample for which the stated outcome is observed; in the interests of space we report results for the extreme cases in which the outcome is observed for the most respondents (i.e., Deliberation) and for the least respondents (i.e., Δ Income). Each column reports the results of a separate regression. The dependent variable is mean deferral ($\sum_t q_{h,t}t$) in Columns 1, 2, 4 and 5, and mean actual deferral squared ($\sum_t q_{h,t}t^2$) in Columns 3 and 6. The independent variables are the corresponding values of deferral ($\sum_t \tilde{q}_{h,t}t$) and deferral squared ($\sum_t \tilde{q}_{h,t}t^2$) that were experimentally assigned. Heteroskedasticity-robust standard errors are in parentheses. F-statistics reported in the final row are calculated using the method of Olea and Pflueger (2013) in cases with a single instrument and endogenous variable (e.g. Column 1) and using the method of Kleibergen and Paap (2006) in cases with multiple instruments and endogenous variables (e.g. Columns 2 & 3). The estimated relationships are strong because non-compliance with assigned transfer timing was slight, with only 13 cases in total (see Table A.6 for details).

Table A.13: Impact of average deferral: nonlinear specification

	Deliberation	Social input	Goal Progress	Δ Income	Assets + Expenditures	Valuation
	(1)	(2)	(3)	(4)	(5)	(6)
Mean deferral ($\sum_t q_{h,t}t$)	0.052	0.015	0.015	146.9	117.9	211.2
	(0.031)	(0.012)	(0.012)	(63.2)	(193.9)	(345.7)
Mean deferral squared ($[\sum_t q_{h,t}t]^2$)	-0.0036	-0.0014	-0.0017	-13.9	-7.53	-18.6
	(0.0025)	(0.0011)	(0.0011)	(5.47)	(19.1)	(29.1)
N	424	424	424	393	424	417
Dependent variable mean	-0.00	0.60	0.81	893	4576	2480

Notes: This table presents estimated effects of average deferral on the outcomes presented in Table 2 using a non-linear specification. Results are estimated for the 90% subsample for which timing was assigned randomly, with differences in observation counts across columns reflecting differences in availability of the outcomes. The specification also includes controls for preferences (preference for any deferral, and preference for one as opposed to two tranches) results for which are not reported. Heteroskedasticity-robust standard errors are in parentheses.

Table A.14: Impact of deferral: alternative specifications for income

	Income	Δ Income		Income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Deferral ($\sum_t q_{h,t}t$)	110	130	142	119	116	152	116
	(65.5)	(66.0)	(65.1)	(64.3)	(62.9)	(67.8)	(67.5)
Deferral squared ($\sum_t q_{h,t}t^2$)	-10.4	-11.7	-13.5	-11.2	-10.9	-13.9	-10.9
	(5.72)	(5.61)	(5.35)	(5.56)	(5.43)	(6.01)	(5.86)
Baseline consumption				0.60	0.60		
				(0.54)	(0.54)		
Baseline assets, with land						0.015	0.015
						(0.012)	(0.012)
1(Missing values imputed)		-205			-298		-160
		(198)			(184)		(134)
N	420	420	393	401	420	362	420
Dependent variable mean	1242	880	893	1255	1242	1261	1242

Notes: This table presents results from estimating variants of Equation 1 with alternative definitions of the dependent variable. Each column reports the results of a separate regression. The dependent variable is the endline income level in Columns 1 and 4–7, and the change in income between baseline and endline in Columns 2–3. Observations for which the baseline value of income is missing are dropped in Column 3, and included with the mean baseline value imputed and an indicator for imputation included as a control in Column 2. Column 1 thus replicates the results shown in the pre-analysis plan report, and Column 3 replicates the results shown in Table 2. The remaining columns introduce additional controls for baseline consumption (Columns 4–5) or assets (Columns 6–7). Missingness in these baseline variables is handled as follows: in Columns 4 & 6 the corresponding observations are dropped, while in Columns 5 & 7 we impute the mean (and include in the regression an indicator for this imputation). Heteroskedasticity-robust standard errors are in parentheses.

Table A.15: Impact of deferral on aspects of deliberation

	Looked for information	Asked for advice	Thought carefully	Specific goal	Thought before receiving	Quick decision
	(1)	(2)	(3)	(4)	(5)	(6)
Deferral ($\sum_t q_{h,t}t$)	0.18	0.13	0.14	0.12	-0.086	-0.12
	(0.087)	(0.081)	(0.09)	(0.07)	(0.067)	(0.078)
Deferral squared ($\sum_t q_{h,t}t^2$)	-0.01	-0.011	-0.01	-0.0093	0.007	0.0094
	(0.0068)	(0.007)	(0.0076)	(0.0062)	(0.0057)	(0.0067)
N	424	424	424	424	424	422
Dependent variable mean	-0.008	-0.007	0.009	-0.018	0.003	0.014

Notes: This table presents effects of deferral on standardized components of the Anderson deliberation index, estimated using equation 1. Results are estimated for the 90% sub-sample for which timing was assigned randomly, with differences in observation counts across columns reflecting differences in availability of the outcomes. The specification also includes controls for preferences (preference for any deferral, and preference for one as opposed to two tranches) results for which are not reported. All recorded statements were rated by participants on a scale from 1 = strongly disagree to 5 = strongly agree. Column 1 reports the effect on agreement with the statement that the participant "looked for information on how to best use the money". Column 2 reports the effect on agreement with the statement that the participant "asked other people for advice on how to use the money". Column 3 reports agreement with the statement: "When deciding how to use this money, thought very carefully about it", column 4 reports effects on agreement with the statement: "When deciding how to use this money, thought about a specific goal', column 5 reports effects on agreement with the statement: "Thought a lot about how to use the money, even before receiving the first transfer", column 6 reports effects on agreement with the statement: "Made a quick decision on how to spend the money". Heteroskedasticity-robust standard errors are in parentheses.

Table A.16: Impact of transfer timing, holding time to plan fixed

	Income	Assets + expenditures
	(1)	(2)
Deferral ($\sum_t q_{h,t}t$)	59.73	-132.97
	(32.75)	(87.43)
Deferral squared ($\sum_t q_{h,t}t^2$)	-3.28	6.78
	(1.78)	(4.52)
Treated	-147.33	702.06
	(130.54)	(354.58)
Endline survey date	22.32	2.61
	(3.32)	(7.73)
Baseline income	0.40	
	(0.14)	
N	8,239	8,226
Dependent variable mean	616.6	2087

Notes: This table presents estimated effects of cash transfer timing estimated using Equation 1 but using data from Egger et al. (2022). "Treated" is a binary variable that takes value one when the household received a cash transfer, as the EHMNW experiment had a pure control group that did not receive the cash transfer, unlike our experimental design."Endline survey date" denotes the time of completion of the endline survey, which took place at varying time intervals from cash disbursement. "Baseline income" refers to the baseline income of the household. The "Income", "Baseline income" and "Assets + expenditures" variables were originally recorded in KES; we converted them to USD at a rate of 87 KES per 1 USD (the same conversion rate we applied to our own data). Standard errors are clustered at the village level (653 clusters).

Table A.17: Income effects by sector of primary occupation

	Non-farm self-employment	Farm self-employment	Wage labor
	(1)	(2)	(3)
Deferral ($\sum_t q_{h,t}t$)	310	139.7	44
	(126.3)	(106.5)	(127.3)
Deferral squared ($\sum_t q_{h,t}t^2$)	-29.3	-12.7	-3.12
	(11.7)	(7.71)	(10.7)
N	105	175	108
Dependent variable mean	1204	750	847

Notes: This table presents estimated effects of deferral on income growth, estimated using Equation 1 separately based on respondents' reported primary occupation as of the endline survey. The specification also includes controls for preferences (preference for any deferral, and preference for one as opposed to two tranches) results for which are not reported. The sample includes the 90% sub-sample for which timing was assigned randomly (excluding 5 respondents whose primary income at endline was reported as transfers from household members who had migrated). Column (1) presents results for the sub-sample of participants whose primary occupation at endline was non-agricultural enterprise. Column (2) presents results for those whose primary occupation at endline was farming or fishing. Column (3) presents results for those whose primary occupation at endline was casual wage labor or salaried labor. Heteroskedasticity-robust standard errors are in parentheses.

Table A.18: Token transfer timing and deferral preferences: a non-parametric test

(a) If receiving one tranche

	Recent token	Less recent token
February	135	181
March	55	37
April	31	22
May	7	8
June	10	5
July	2	2
August	8	3
September	0	1
October	3	0
November	0	1
December	1	0
January	1	0
Total	253	260
p-value, Fisher's exact test, all months		0.007
p-value, Fisher's exact test, April–January		0.016

(b) If receiving two tranches

	Recent token	Less recent token
February	192	214
March	34	27
April	14	6
May	7	2
June	5	1
July	1	0
Total	253	260
p-value, Fisher's exact test, all months		0.043
p-value, Fisher's exact test, April–July		0.002

Notes: This table presents the full distributions of timing preferences for both the recent and less recent token transfer groups, along with results from a Fisher's exact test for differences in these distributions. The first panel reports preferences conditional on receiving one tranche, and the second reports preferences conditional on receiving two tranches. Each row shows the number of participants preferring to receive the transfer starting in the indicated month. Columns divide participants into those assigned to receive a recent token transfer and a less recent token transfer. The p-values at the bottom of each panel are from Fisher's exact tests of the null of no difference between the distributions in the second and third columns, including all months and then limiting to April onwards.

Table A.19: Effect of token transfer timing on deferral preferences, by tranching

	Preferred 1 tranche		Preferred 2 tranches	
	If getting 1	If getting 2	If getting 1	If getting 2
	(1)	(2)	(3)	(4)
Recent token	0.50	0.056	0.28	0.12
	(0.21)	(0.12)	(0.24)	(0.095)
N	183	183	321	321
Dependent variable mean	0.62	0.31	2.31	1.42

Notes: This table presents estimated effects of assignment to more recent token transfer receipt on participants' preferences for deferral, measured in months relative to February 2015. Results are estimated for the 90% sub-sample for which timing was assigned randomly. Estimates are presented separately for households that preferred to receive one tranche (Columns 1 & 2) versus two tranches (Columns 3 & 4). Regardless of their tranching preference, households were asked their preference for deferal both conditional on receiving one tranche (results in Columns 1 & 3) and conditional on receiving two tranches (Columns 2 & 4). Heteroskedasticity-robust standard errors are reported in parentheses. The p-value from a joint test of the null hypotheses that both (a) the coefficients in Columns 1 & 2 are equal, and (b) the coefficients in Columns 3 & 4 are equal, is 0.052.

Table A.20: Impact of recent token transfer on measures of cognition

	Raven's score	Cognitive failures	Working memory	Stroop time	Stroop errors
	(1)	(2)	(3)	(4)	(5)
Recent token	0.24	0.55	0.11	0.40	0.024
	(0.17)	(0.63)	(0.085)	(2.70)	(0.33)
N	455	507	476	458	458
Dependent variable mean	5.41	19.5	5.02	116	4.21

Notes: This table presents estimated effects of a more recent token transfer receipt on measures of cognitive performance captured during the preferences survey. Differences in observation counts across columns reflect differences in availability of the outcomes. "Raven's score" refers to participants' score on Raven's progressive matrix test. "Cognitive failures" refers to the Cognitive Failures score, a measure of self-reported cognitive failures experienced by participants in their daily life. "Working memory" refers to participants' working memory score, "Stroop time" and "Stroop errors" record the time to complete a Stroop test and the number of errors, respectively. Appendix B provides detailed variable definitions. "Recent Token" is equal to 1 for recipients assigned to receive the cash transfer 4 days (as opposed to 4 weeks) before the preference survey, and equal to 0 otherwise. Heteroskedasticity-robust standard errors are reported in parentheses. The specification includes controls for baseline values of each variable.

Table A.21: Recent token transfer: heterogeneous effects

	Any deferral
Female head	-0.02
	(0.10)
Age	0.00
	(0.00)
Head is married	-0.03
	(0.14)
Household members	0.01
	(0.06)
Children	0.05
	(0.06)
Mean education	-0.01
	(0.02)
Owns an enterprise	-0.02
	(0.10)
Assets	-0.00
	(0.00)
Income	-0.00
	(0.00)
ln(House value)	0.07
	(0.07)
ln(Consumption)	0.12
	(0.07)
N	360
F-statistic, all interaction terms = 0	1.41
p-value, all interaction terms = 0	0.17

Notes: This table presents estimates of differential effects of recent token receipt on preference for any deferral with respect to the baseline covariates indicated in the row labels. The underlying regression includes each of these covariates individually as well as the main effect of recent token receipt, with only the coefficients on the interaction returns reported here. "Mean education" is the average years of schooling completed by household members above age 25. Appendix B provides more detailed definitions of some variables. Heteroskedasticity-robust standard errors are reported in parentheses. The F-statistic and p-value reported at the bottom of the table refer to a joint test of the null hypothesis that all of the coefficients on the interaction terms reported in the rows above are zero.

Table A.22: Impact of recent token transfer on liquidity, preferences, and financial stress

	Cash on hand (%)	Prefers any deferral	Prefers 1 tranche	Money worries (0/1)	Difficulty with bills (0/1)
	(1)	(2)	(3)	(4)	(5)
Recent token	0.133	0.15	0.02	-0.002	-0.054
	(0.021)	(0.037)	(0.042)	(0.044)	(0.028)
N	512	513	513	512	512
Dependent variable mean	0.119	0.242	0.357	0.557	0.885

Notes: This table presents the estimated effects of a more recent token transfer receipt on measures of liquidity (cash on hand), preferences over cash transfer structures (deferral and tranching), and measures of financial stress. Outcomes are defined as follows: "Cash on hand (%)" denotes the share of unspent token transfer that the participant has at survey time as a percentage of the token transfer; "Any deferral" is an indicator variable that takes value one when the participant prefers any deferral in receipt of the main transfer; "Prefers 1 tranche" denotes preference for receiving the transfer in one as opposed to two tranches; "Money worries" is an indicator variable that takes value one if the participant has experienced any worries about money in the last 7 days; and "Difficulty with bills (0/1)" is an indicator variable that takes value one if the participant has experienced any difficulty in coping with bills and expenses in the last 7 days. Appendix B provides more detailed definitions of some variables. Heteroskedasticity-robust standard errors are reported in parentheses.

B Variable definitions

- **Cash on hand (%)** equals one minus the ratio of the amount of the token transfer the household reported having spent to the amount of the token transfer it was issued.
- **Income** corresponds to the answer to the endline survey question, "What is your current level of annual income (in the last 12 months)?" **Δ income** is equal to current income minus the analogous value from the baseline survey. Answers were elicited in KES and subsequently converted to USD.
- The **deliberation index** is constructed from questions asking about agreement with the following statements, all recorded on a scale from 1 = strongly disagree to 5 = strongly agree:
  - I looked for information on how to best use this money
  - I asked other people (other than myself) for advice on how to use this money
  - When deciding how to use this money, I thought very carefully about it
  - When deciding how to use this money, I thought about a specific goal
  - I thought a lot about how to use the money even before I received the first transfer (after token)
  - I made a quick decision on how to spend the money

  Responses to the last question were inverted so that 1 (5) implied the least (most) deliberation. All responses were standardized by creating a z-score for each variable, and aggregated using inverse covariance weights as in Anderson 2008.
- The **goal progress** index is constructed from answers to the following questions, all recorded on a scale from 1 = no progress to 5 = a lot of progress:
  - Think about your goal for how much annual income you would like to achieve in your life. Since receiving the transfers, how much progress do you feel like you have made towards that goal?
  - Think about your goal for the assets you would like to achieve in your life. Since receiving the transfers, how much progress do you feel like you have made towards that goal?
  - Think about your goal for the social status you would like to achieve in your life. Since receiving the transfers, how much progress do you feel like you have made towards that goal?

  Responses to these questions were re-scaled linearly to the unit interval $[0, 1]$ and then averaged.
- The **cognitive failures score** is constructed from answers to questions about the frequency of the following events, all recorded on a scale from 0 = never to 4 = very often:
  - Forget whether did something simple in the last 7 days
  - Say something unintentionally insulting in the last 7 days
  - Fail to hear someone speaking while distracted in the last 7 days
  - Lose temper and regret later in the last 7 days
  - Forget which way to turn on road in the last 7 days
  - Cannot find something in the house in the last 7 days
  - Have trouble making decision in the last 7 days
  - Forget where put something in the last 7 days
  - Daydream in the last 7 days
  - Forget people's names in the last 7 days
  - Get distracted into doing something else in the last 7 days

- Can't remember something on the tip of tongue in the last 7 days

Responses to these questions were summed to arrive at a total score, where a lower score corresponds to fewer failures experienced and a higher score to more failures experienced. The minimum score is 0, corresponding to no failures (answering "never" to each of the 12 questions), and the maximum score is 48, corresponding to many failures (answering "very often" to each of the 12 questions).

- The **Raven's score** corresponds to the sum of correct answers to ten Raven's progressive matrix puzzles. These puzzles were preceded by the following explanation, which enumerators read aloud:

  > In each puzzle the objective is to decipher the pattern in the upper box and complete the puzzle by choosing the correct box among the choices below. By looking at the way the pieces change from left-to-right and up-to-down, you can understand the pattern and find the symbol that completes the rightmost column and bottom row. We will now work through the first five puzzles together. Please ask any questions during the examples; once you begin the final 10 puzzles, I will no longer be able to answer your questions.

  The enumerator showed 5 examples of solved puzzles, and asked for questions. After this, the participant provided answers to 10 new puzzles sequentially, and the answers were recorded for each puzzle. Each correct answer is assigned a score of 1 and each incorrect answer is assigned a score of 0. The minimum total score is thus 0 and the maximum is 10.

- The **Stroop time** and **Stroop errors** outcomes are obtained from a game in which the respondent was presented with a series of rows of numbers and asked to identify the number of digits in each row. The enumerator first worked through a few examples with the respondent to ensure they understood the task, and then conducted a series of three Stroop tasks each consisting of 25 rows to be counted. During each task the enumerator used a stopwatch to measure the total amount of time, in seconds, taken to complete it, and also recorded the number of errors made. Our measure of time spent is the sum of the recorded times spent on these three Stroop tasks, and our measure of errors is the sum of the number of errors made. The minimum possible number of errors is 0, and the maximum possible is $3 \times 25 = 75$.

- The **Social input** index is constructed from answers to two groups of questions. The first were question about agreement with the following statements, all recorded on a scale from 1 = strongly disagree to 5 = strongly agree.
  - I asked other people (other than myself) for advice on how to use this money
  - The final decision on how to spend the money was one I made alone
  - When deciding how to spend the money, I thought a lot about whether other people would agree with the decision that I made

  Responses to the second question were then reversed so that higher values indicated more input from other people. Responses were then re-scaled linearly to the unit interval $[0, 1]$. The second group of questions were about counts of categories of other people, as follows:
  - When deciding how to spend the money, were you thinking about anyone in particular (including yourself)? Answer options included "myself," "my spouse," "my children," "my parents," "my other relatives," "my neighbors," "my friends," and "other, specify," with multiple responses allowed.
  - Who do you think will benefit the most from how you decided to spend the money, in the long run? Answer options were as above.

  We create a variable from these responses equal to the share of categories (other than "myself") mentioned, counting "other" as a single category. We then average all five variables to obtain our overall index.

- The retrospective **valuation** of things purchased is the respondent's stated willingness to accept for the things they had purchased with their transfer. The specific language used to elicit this was as follows:

  > Think about everything that you spent the money on. Imagine that all of those things were in front of you right now (even the things that you might have consumed). Looking back from what you now know, how much would they have to pay you for you to be willing to give those things to them?"

  Answers were elicited in KES and subsequently converted to USD.
- **Mean education** is the average of the highest education levels (in years of schooling) attained by members of the household who are above age 25. Respondents could report information for up to 13 household members. The mapping from highest reported educational attainment to years of schooling completed is as follows:
  - None: 0 years;
  - Pre-school: 1 year;
  - Standards 1–8: 2–9 years;
  - Forms 1–6: 10–15 years;
  - College years 1–4: 16–19 years;
  - University years 1–5+: 20–24 years
- **Married** is a binary variable that takes value one if the household head is married and zero otherwise.
- **Widow/widower** is a binary variable that takes value one if the household head is a widow or a widower.
- **% Household members sick** is the percentage of household members who were sick in the last 4 weeks at baseline.
- **Assets: electronic appliances** is the value of the electronic appliances owned by the household. Values were elicited in KES and subsequently converted to USD.
- **Assets: transport** is the value of the transportation vehicles owned by the household. Values were elicited in KES and subsequently converted to USD.
- **Land size** is the number of acres of land owned by the household.
- **Remittances** is the amount of remittances received by the household in the last one month. Values were elicited in KES and subsequently converted to USD.
- **Household skipped meals** is a categorical variable recording how frequently members of the household skipped meals or cut portion sizes in the last month, with responses coded as follows:
  - "0" coded as 0.
  - "Once" coded as 1.
  - "Less than 5 times" coded as 2.
  - "Between 5 and 10 times" coded as 3
  - "More than 10 times" coded as 4.