Structuring cash transfers: cash flow preferences, seasonality, and financial decisions 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. Most preferred lumpy transfers, and some preferred deferred receipt—in contrast to the typical structures of safety-net programs, but consistent with evidence on the financial challenges of poverty. Turning to consequences, receiving transfers later in the year raised income 1.5 years later—but willingness to defer receipt was sensitive to small changes in cash flow around the time of decision-making. 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.[1] 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.[2] 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 (see for example Abubakari et al., 2024), 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 draw for this purpose on original fieldwork we conducted 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 over the course of one year, fixing the total amount (at approximately USD 1,000) but granting recipients some degree of control over structure: specifically, tranching and timing.

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). With respect to timing, meanwhile, most recipients preferred to receive transfers starting immediately. Yet a sizeable 27% minority preferred at least some *deferral*, i.e. to have transfers commence in a month after February 2015, the earliest option.[3] Some said that they desired deferral per se, in order to better plan their spending (consistent with, for example, the predictions and evidence in Thakral and Tô (2020, 2025) that advance notice helped recipients make better use of transfers; and perhaps also to the finding in Burlando et al. (2025) that a delay in loan disbursement led to higher repayment rates down the line). Other recipients sought to align transfer timing to seasonal agricultural demands, to ensure

[1] One consequence, as Merfeld and Morduch (2024) document, is that a household's poverty status can fluctuate substantially within any given year.

[2] 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).

[3] 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.

(for instance) that money arrived after planting when they would have time for construction projects and building materials would be less expensive or at harvest which would be a favorable time to run a non-farm business. These narratives call to mind 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., 2024) and 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. Motivated by our findings, GD subsequently conducted its own independent survey in three countries (Kenya, Malawi, and 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. Many took advantage of this added flexibility, with a majority (61%) choosing a structure that would not have been feasible in our original experiment. But similar broad patterns emerge: 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. How these results may generalize intertemporally is less clear from the data, as preferences were elicited at around the same time, but we discuss several pieces of evidence that suggest they may.

From the point of view of neoclassical welfare analysis, preference data like these are sufficient to determine optimal policy. A more paternalistic perspective, however, would also consider how different transfer designs affect outcomes the paternalist wishes to influence. This is particularly true for income, as the hope—at least in lumpy cash transfer programs—is often that the transfers will be used for productive investment and thus help people "graduate" out of poverty. We therefore examine next how transfer structure affected subsequent income. We draw for this part of the paper on data from two distinct studies. First, we consider the 90% of recipients in the Kenya preferences study to whom GD assigned a dimension of transfer structure experimentally (omitting the 10% who received their revealed preference). While GD randomized both timing and tranching, it restricted the support of the tranching randomization to the two most popular options (i.e. 1 or 2 tranches); this experiment is thus informative primarily about the effects of timing. And second, we re-analyze data from Egger et al. (2022) (henceforth, EHMNW), who also randomized the timing (but not tranching) of lump-sum transfers as part of a separate experiment conducted with GD in the same region.

With respect to tranche count, results from the preferences study are (unsurprisingly) inconclusive: a single tranche had larger estimated effects than two tranches on a range of outcomes, but these estimates are not precise enough to support strong conclusions. With respect to timing, on the other hand, we find a consistent pattern in both experiments: recipients whose transfers began towards the middle of the year had higher income subsequently. This pattern is robust to variations in the timing of income measurement (which was fixed in the preferences study, but exogenously varied relative to the date of transfer receipt in EHMNW) suggesting that it does in fact reflect the impact of the timing of transfer receipt on income. Our estimates of differential impacts on cash outflows (expenditure and changes in assets) are less precise, on the other hand, and so there we do not draw strong conclusions.

How might transfer timing affect subsequent income? In the preferences study there were (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. As noted above, participants both in the preferences study and the subsequent three-country survey mentioned both of these mechanisms in the explanations they gave for their preferences. Respondents whose transfers began a bit later also subsequently reported having deliberated more about how to use them, and making more progress towards their overall goals, consistent with a planning channel, though these effects are not precise enough to support firm conclusions. 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. Overall there is thus strong evidence for seasonality, and some suggestive evidence for a planning channel.

We then revisit recipient preferences in light of these observed treatment effects. Recipients need not necessarily prefer the timing that maximizes treatment effects on future income; they may have additional goals in mind, such as minimizing food insecurity. They may also face pressures and uncertainties that make it difficult to prioritize the future as much as they themselves might wish to. We have seen that some did desire deferral, which tended to increase effects on income, but we can also readily reject the null that giving each recipient money at the time they preferred would have maximized income effects ($p < 0.001$).[4] There is thus some tension between respecting recipient preferences and maximizing their income.

The third piece of our analysis explores the roots of this tension. In particular, we examine whether stated preferences were themselves sensitive to cash flow around the time of decision-making. To do so we exploit 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 in the preferences study: 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, for those who received the token transfer later, and slightly less self-reported difficulty with bills. More interestingly, it also influenced recipients' preferences over the structure of transfers tomorrow: more recent token transfer recipients were 13.6 percentage points (35%) more likely to demand some deferral.[5][6] This suggests that financial pressures did limit to some

[4]Formally, we reject the null that this would have yielded the same average treatment effect on income growth as giving all recipients money at the time that maximized income. See Section 5 for details.

[5]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. In a broader sense it is consistent with the finding in Fehr et al. (2022) that financial circumstances can affect decision-making in ways that are not obviously mediated by choice sets themselves.

[6]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,

extent their willingness to choose timings that yielded faster subsequent income growth. And, consistent with learning about this point, recipients asked at endline what timing they would in retrospect have preferred tended to report a preference for significantly later transfer onset than what they originally chose.

Overall the picture is of recipients who had meaningful information about what would work best for them, but also scope to learn more, and who faced challenges prioritizing the future in the face of the pressures of the day. This last point implies a potential 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. In the preferences study, the shift in timing preferences induced by receiving the token transfer more recently was enough to increase annualized earnings by an estimated USD 41, or 5% of the total transfer.[7] This suggests 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.

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. Transfer structures could in principle be customized to the preferences of individual recipients, which (even among the more limited options available in the Kenya data) are quite varied: the number of distinct tranching × timing pairs that was the first choice for at least one recipient was 19, and the most popular such structure was preferred by only 39.4% of the sample. A less demanding approach would be to customize transfers at the regional level, which would allow program designers to take advantage of variations in seasonality. Giving everyone a lumpy transfer during the growing season, for example, would have both better respected their preferences and increased their average earnings in the settings we study. Reforms like these 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., 2024, Morduch, 2021). They 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 play a role in accelerating investment (Banerjee et al., 2015a, Haushofer and Shapiro, 2016) if not outright help escape from a poverty trap (Balboni et al., 2021).

Second, and continuing the "small push" line of thinking above, they suggest that timing transfers (and other help) judiciously relative to the timing of important forward-looking decisions may help people living in extreme poverty make those decisions more effectively. Here, the forward-looking decision was one that we ourselves created: how to structure the remainder of the transfer. But many others arise organically in people's lives: how much to invest in children during the "first 1000 days" of their lives;[8] whether to enroll in secondary school (which Barrera-

Kremer et al., 2019, Kaur et al., 2022, Fehr et al., 2022, 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.

[7]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.

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

Osorio et al. (2011) find is affected by the timing of a monthly cash transfer in Colombia); and whether to take a rewarding but low-paying job (which Coffman et al. (2019) find, even in a much higher-income context, is affected by receipt of just a few hundred dollars). In India even decisions to marry a daughter to a suitor, shaping the subsequent course of her entire life, have been shown to respond to short-term income shocks (Corno et al., 2020). In Brazil, the receipt of monthly social protection transfers a few days before an important high school exam had a significant positive impact on student exam performance, college enrollment, graduation rates seven years later and even employment nine years later (Eizmendi and Reyes, 2025). These examples suggest a role for social protection that goes beyond targeting households when—and because—their marginal utility from consumption is high, to also target crucial moments of decision.

2 Context and preferences study design

Both studies on which we draw are 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) of which the long rainy season is the more important; maize is the main crop cultivated by respondents, and approximately 85% of the region's total annual maize production occurs during the long rainy seasons (Kenya Food Security Steering Group, 2010).

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 villages, but no villages or individual households in our sample had previously received transfers from GD.

The remainder of this section focuses on the design of the preferences study we conducted; Section 4 describes relevant differences between this design and that in EHMNW. In the preferences study, 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. [9]

2.1 Project timeline

The preferences 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.2).[10] 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.[11]

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. Staff indicated that GD would implement two or more of these options, and give each recipient a 55% chance of receiving their preferred option from among the options implemented. We put this into effect by giving 10% of recipients their preferred option from among the (two) options implemented, and the other 90% a random draw, which was thus 50% likely to be the one they preferred: $10\% + 90\% \times 50\% = 55\%$.[12] The exact wording of this and subsequent elicitation questions is available in Appendix B.1, along with visual diagrams that staff used to explain the options.

Staff then elicited participants' 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

[9] 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.

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

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

[12] As we discuss below, GD chose two tranching options based on their overall popularity. Had subjects anticipated this, they might in principle have considered the strategic influence their choices could have on the effective choice set. In practice, however, such effects were extremely small. For example, the sampling-based probability that a subject's reported preference for the third-most-popular design (four tranches) as opposed to the second-most-popular design (one tranche) would affect their ranking was approximately 7×10^{-66}.

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. The wording of the survey questions about timing paralleled that about tranching ("We would also like to know which months you would like to receive the transfer...") but did not make explicit how reported preferences would affect the probability distribution over timing actually received, as this was more complex than in the tranching case. Specifically, we again gave 10% of subjects their preferred structure on this dimension, and gave the other 90% a uniform random draw over the various start dates possible given the tranching structure they received. We instructed staff to make clear, however, that it was in respondents interest to express their true preferences for both tranching and timing.

In designing this elicitation, we sought to balance several considerations. Implementing participants' preferences with positive probability gives them an incentive 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.[13] Finally, while we sought to give participants a meaningfully broad range of options compared to other (typically fixed) transfer schemes, they might have ranked other structures 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 preferences over even more flexible choice sets in Section 3.4 below, using stated preference data from a separate survey.

To mitigate the risk of confusion, we trained staff extensively to explain the elicitation and field questions about it. We also provided them with visual aids to show to participants, representing the amounts they would get and the times on which they would get them depending on the preferences they submitted. One way to gauge how successful these efforts were is to examine the comments we solicited from staff on the interviews. Out of 513 interviews with study households, we identified seven in which the comments arguably indicate recipient confusion. Of these, only one referred to the preference elicitation (a case in which the respondent asked if the enumerator could choose for her). We will also see below that participants gave coherent explanations for their preferences, and that reported preferences typically covary with measures of cognitive ability in the opposite of the direction one would expect if confusion were driving the results. Exactly how well respondents grasped the underlying incentivization mechanism (and thus how "revealed" as opposed to "stated" their preferences were) is difficult to assess. But we are confident that respondents understood the decisions posed to them.

Table A.1 summarizes the assignment to tranching and timing that resulted from this process, distinguishing for each dimension 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

[13] 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.

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.

Panel B of Figure 1 describes the timing of transfers and survey activity (including an endline survey, which we will discuss in more detail below). 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.

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.3 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 had given 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.4). Since the single tranche was the option listed first, and hence in some sense the "easiest" one to select, this also suggests that less cognitively capable respondents were not more likely to take the "easy way out" of the elicitation. Nor can risk aversion over uncertain transfer timing explain preferences: lumpy transfers implied *greater* uncertainty than the twelve-tranche option.

When asked why they preferred the tranching they did, subjects' responses illustrated several coherent rationales (see Table A.2 for tabulations based on human and machine coding). Around 40% mentioned the need to finance lumpy investments or economize on fixed costs:

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

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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 build a house with the first lumpsum then
      reorganize myself to start some business with the second lumpsum
      after settling in my own home.
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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:

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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.
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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).[14] 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).

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).

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

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.3 and Table A.3).

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? As noted above, 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—as, for example, if less confused respondents choose to receive transfers immediately, while more confused ones give responses that are essentially random. 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.4; 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.

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, 2025, Burlando et al., 2025). 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.4 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.4 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.4 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.4) 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.

We also separately examine how preferences correlate with cross-subject differences in seasonal financial circumstances. The endline survey asked respondents which month of the year was typically hardest for them financially. January—the month in which preferences were elicited—was named most often, but by only a minority (35%) of respondents, with the majority naming some other month. This gives us one way to explore whether preferences covary with the degree of "leanness" of the season in which they are elicited. We find no significant or economically meaningful relationships: the correlation with preferred number of months of deferral is $\rho = -.0155$ ($p = 0.88$), and that with preferring one tranche over two is $\rho = .066$ ($p = 0.16$). This suggests that the preference patterns we see in January would not be unique to January in a richer design that varied the timing of elicitation across the calendar year.

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 all three countries, they were conducted about a month later than in the Kenya Preferences survey (i.e. between the end of February and mid-March, rather than between the end of January and mid-February). 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.5). At the time this survey was conducted it was planting season in Kenya, growing season in Malawi, and either harvest or planting season in Liberia, depending on crop. 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.6). 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, at different levels of development. The fact that these locations were also at different points in their agricultural cycles when preferences were elicited further suggests that the patterns we observe may generalize intertemporally, though making sure of this would require eliciting preferences at different times.[15]

4 Effects of transfer structure

In traditional welfare analysis, recipient preferences are the input needed to determine optimal policy—here, optimal transfer design. These preference may reflect tradeoffs between a wide

[15] 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.

variety of objectives, from ensuring adequate nutrition during "lean seasons" to longer-term income growth, among many others. A paternalistic policy-maker, however, may have some more specific goal in mind. One commonly expressed desire among anti-poverty program designers is to raise future earnings—partly out of a paternalistic concern that transfer recipients may tend to inadequately prioritize the future, and partly out of a pragmatic desire to reduce the fiscal burden of safety net programs over time by encouraging "graduation" to a higher standard of living, at which current recipients would in the future no longer be eligible.[16]

Motivated by these questions, we turn next to examining the impact of different transfer structures on future earnings and related outcomes. To do so we make use of two distinct experimental datasets. The first consists of the 90% samples of participants in our preferences study, described above, who were assigned a given dimension of transfer structure experimentally, as opposed to receiving the one they preferred. The second is the publicly available data from EHMNW, which describes households who were part of a similar experiment also conducted by GD in Siaya and which also distributed unconditional transfers of USD 1,000 each, with the timing of these transfers determined randomly. Panel C of Figure 1 displays the timing of transfers and measurement in EHMNW for comparison to that in Panel B from the preferences study. For our purposes, the key differences between the two experiments are as follows:

(a) **Transfer structures**. In the preferences study GD experimentally varied both tranche count and the timing of transfers, while in EHMNW it varied only timing; all recipients were given two tranches, six months apart. Both studies are thus useful for studying the effects of timing, while only the preferences study lets us examine effects of tranching. (Moreover, because in that study the support of the tranche count randomization was limited to either one or two tranches, tranche structure will not end up being material there either).

(b) **Outcomes**. In the preferences study we measured outcomes in addition to income (and expenditure) to capture features of the *process* by which recipients made decisions about how to use funds. These outcomes—measures of deliberation, social input, progress towards goals, and retrospective valuation of purchases—were included in the preferences study to shed light on behavioral mechanisms; the EHMNW study did not measure them.

(c) **Unit of randomization**. In the preferences study transfer timing varied at the household level, while in EHMNW it varied at the village level. Any direct effects of transfer timing on the recipient themselves would thus have been similar, but in the EHMNW timing effects may also reflect differences over time in the indirect effects of transfers to neighbors. This is potentially useful in the sense that the preferences study is arguably most informative about what might happen if recipients could customize transfers at the individual level, while the EHMNW study is more informative about what might happen if timing was set at the community or regional level (to match the median recipient's preference, say).

(d) **Advance notice**. In the preferences study, both the date on which recipients were informed that they would receive transfers and the date of the endline survey were fixed and identical across recipients. Those assigned to receive transfers later in the year thus had

[16] That said, graduation out of ineligibility was not an issue in our context, as GD based transfers on eligibility at a single point in time with no expectation that there would be additional, subsequent transfer rounds.

more time between being informed and receiving funds, and less time between receiving funds and being surveyed at endline. In the EHMNW study, on the other hand, all recipients were informed that they would receive funds roughly 2 months before they were scheduled to, regardless of the time of year. The time between receipt of funds and the endline survey was also independent of the date of transfer receipt, and experimentally varied. These differences will be particularly important to the interpretation of transfer timing effects, as we discuss below.

In light of this last distinction, we will continue to use the term "deferral" to refer to time elapsed between preference elicitation and transfer receipt in the preferences study, but use the more generic term "timing" when referring to time of transfer receipt in both studies.

4.1 Endline measurement & experimental integrity

In the preferences study we conducted an endline survey in July-August 2016, about 1.5 years after the first scheduled transfer payment and 0.5 years after the last. 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.

Randomization successfully balanced household characteristics with respect to the timing of token transfers (Table A.5), the number of tranches (Table A.6), and the timing of transfer onset (Table A.7). 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.8). 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.9), 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.10). In particular, attrition is unrelated to assigned number of tranches ($p = 0.94$) or to assigned timing of transfer onset ($p = 0.94$).

For the EHMNW experiment, Table A.11 shows that the timing of transfer onset was balanced with respect to household characteristics analogous to those in Table A.6 for the preferences study. Endline surveys covered 8,242 households and was conducted between May 2016 and June 2017. Endline data was collected between 9 and 31 months after each household was assigned to start receiving transfers in treated villages (or would have been so assigned had it been treated), in a random order and with a mean (median) gap of 18 (19) months between the first transfer and endline survey. EHMNW report that attrition was low and not differential with respect to treatment; we further confirm that it was not differential (among treated subjects) with respect to the specific month in which transfers were scheduled to commence (Table A.12; $p = 0.376$).

4.2 Effects of tranching

We first examine the impacts of tranche count, using data from the preferences study. Following the pre-analysis plan for the preferences study, we estimate

$$y_h = \alpha + \beta_1 L_h(\tilde{L}_h) + \beta_2 L_h(1 - \tilde{L}_h) + \gamma \tilde{L}_h + \epsilon_h \tag{1}$$

where y_h is an outcome for recipient household h, L_h ($\tilde{L}_h$) indicates whether 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. This is the approach recommended by Athey and Imbens (2017) for regression-based estimation of average treatment effects in the presence of covariates.

Table 1 reports the results. Income growth was meaningfully higher for subjects who received their transfer in one, as opposed to two, tranches, but this difference is not statistically significant. There was little difference in cash outlays (the sum of the treatment effects on assets and expenditures), with a wide confidence interval.[17] We also pre-specified two subjective measures: "goal progress," which measures recipients' holistic self-assessment of the extent to which they have made progress against their own goals, and "retrospective valuation," which measures the amount they were willing to accept at endline for items they previously purchased with the transfer.[18] Again the point estimates are positive, but not significantly different from zero. Overall the results are all directionally consistent with the idea that lumpier transfers have longer-term effects, but not precisely estimated enough to support strong conclusions. This is—if somewhat frustrating—not particularly surprising, given that GD chose to implement two designs which are both much lumpier than the status quo of monthly transfers. That said, the estimates line up broadly with the findings in Haushofer and Shapiro (2016) who study a design that contrasts lump sum with monthly transfers, and find that the latter were more likely to improve food security while the former were more likely to be spent on durables.

4.3 Effects of timing

With respect to timing, on the other hand, both experiments provide meaningful experimental variation. To study timing effects in the preferences study we estimate

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

[17] We chose (and pre-specified) to measure impacts on the stock of assets, rather than the flow of investment, as the former can be elicited without recall issues.

[18] This measure aims to capture how well recipients' decisions reflected their longer-term preferences. A recipient who purchased something for price p thinking that they would value it at $\overline{v}$ who later realized they valued it at $\underline{v} < \overline{v}$ should report their valuation as $\underline{v}$.

where $q_{h,t}$ is the share of the transfer issued to h in month t, where time is measured relative to February 2015 (i.e., that $t = 0$ in February 2015).[19] 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.13). We instrument for $q_{h,t}$ to account for (slight) non-compliance, but ITT estimates (which we provide for completeness in Table A.14) are generally similar both qualitatively and quantitatively. First-stage F-statistics are (unsurprisingly) all large enough to address concerns about instrument strength (Table A.15). As pre-specified we estimated both the full non-linear specification and also a linear one that imposes the restriction $\beta_2 = 0$; because we find evidence of some significant non-linearities, our discussion will focus on the more flexible specification. Finally, we conduct inference using heteroskedasticity-robust standard errors.[20]

This approach modifies the one we proposed in the pre-analysis plan in two 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 (2). That said, results from the pre-analysis plan specification are similar in all respects (Table A.16).

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 for uneven cluster 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 focus the discussion on the heteroskedasticity-robust standard errors, while reporting clustered standard errors for the sake of transparency.

To study timing effects in the EHMNW data, we estimate

$$y_h = \alpha + \beta_1 \sum_{t=0}^{11} q_{h,t}\, t + \beta_2 \sum_{t=0}^{11} q_{h,t}\, t^2 + \delta T_h + \rho E_h + \epsilon_h \tag{3}$$

which mirrors Equation (2) but with a few differences which reflect differences in the designs of the experiments. Specifically, T_h is an indicator for receiving *any* transfer, since the EHMNW design included a pure control group; this ensures that estimates of the βs reflect purely variation in the timing of transfer receipt among treated households, analogous to the preferences study, and not the main effect of receiving a transfer per se.[21] E_h is the date (measured in months from the start of the experiment) on which household h's endline survey was conducted. This was randomly varied conditional on the timing of actual transfer receipt, and so reflects calendar effects on outcomes (as opposed to duration effects of time since treatment). Note that we

[19] For instance, if household h received half of its transfer in month 2 and the other half in month 8, then the values of its regressors would be $\frac{1}{2}2 + \frac{1}{2}8 = 5$ and $\frac{1}{2}2^2 + \frac{1}{2}8^2 = 34$.

[20] In results not reported here, we also examined interactions between tranching and timing, estimating Equation 2 fully interacted with an indicator for being assigned to receive one as opposed to two tranches. The estimates are quite similar for both groups, and statistically indistinguishable.

[21] Including the pure control group provides some additional power for estimating common terms, but omitting it (and the indicator) yields qualitatively similar results (not reported).

normalize the time t at which a dollar was received so that $t = 0$ corresponds to February 2015, hence $q_{h,t}$ has the same meaning in both datasets.

Table 2 reports results from the preferences study (Columns 1–3) and EHMNW (Columns 4–6). The former show that *some* deferral to times later in the year led to significantly higher income at endline (Column 1; $p = 0.09$). This conclusion is somewhat stronger if we condition on baseline income (Column 2; $p = 0.06$).[22] Eventually, however, receiving the transfer later in the year yields smaller income effects; the quadratic terms are negative and significant both when estimating the regression in levels ($p = 0.07$) and controlling for baseline income ($p = 0.03$). The corresponding results in the EHMNW study are similar: the effect of receiving money later than February is initially positive (Column 4; $p < 0.001$), but the negative quadratic term (Column 4; $p = 0.001$) implies that it eventually decreases. In short, we observe the same time-varying pattern of income effects in two independent studies, conducted by different research teams, in the same setting. Panels (a) and (b) of Figure A.1 display scatter plots of the data from the preferences study and EHMNW, respectively, overlaid with the regression lines from equations (2) and (3).

One potential caveat to this comparison concerns the timing of measurement. As noted above, endline surveys in the preferences study were conducted at roughly the same time for all participants, while those in the EHMNW study were rolled out gradually over time, with the duration of the interval between treatment onset and endline measurement experimentally varied. Columns 4 & 5 of Table 2 show that endline survey date positively predicts income in the EHMNW data, raising the question how this measurement timing difference between the studies might affect their comparability. To examine this we estimated a specification based on Equation 3, but replacing the survey date regressor E_h with the duration D_h of the period between treatment onset and measurement, and interacting this measure with the two timing terms. Full details and results of this estimation are provided in Table A.17 and its notes. We then calculated the implied effects of timing when substituting in $\overline{D} = 12.77$, the average number of months elapsed between treatment onset and measurement in the preferences study. This estimates the timing effects we *would* have seen in the EHMNW data had they been collected with the same lag as that in the preferences study.

We draw two conclusions from the exercise. First, the timing of measurement clearly does matter; the estimated main effect of the duration D_h in Table A.17 is large, positive, and significant. This implies that studies of the impact of timing should ideally measure outcomes on a rolling basis over time to capture a full impulse-response relationship, as EHMNW did, rather than a point-in-time snapshot. But second, this factor does not meaningfully or significantly affect estimates of the role of transfer timing itself. The implied timing coefficients at $\overline{D}$ are similar to and slightly larger than the baseline EHMNW estimates, which reflect the actual EHMNW measurement timing.

Returning to Table 2, Columns 3 & 6 report effects on cash outlays (the sum of effects on assets and expenditures). The patterns here are consistent with those for income, but less precise, and, in the EHMNW case, meaningfully smaller. This could reflect some degree of

[22] We did not (in an oversight) pre-specify that we would use baseline values of the outcomes as controls when available; the estimates in Column 1 are thus the pre-specified ones, while those in Column 2 yield the smallest standard errors.

crowd-in of investment at the expense of consumption (as in some microfinance studies, e.g. Banerjee et al. (2015b)), or could indicate that the key difference was not *whether* recipients made investments with their transfers, but rather *when* these investments were made. That said, the estimates are noisy—we cannot reject the null that the coefficients for outlays are the same as those for income—so we cannot draw definitive conclusions.[23]

Table 3 then examines effects on the remaining pre-specified outcomes from the preferences study (more detailed descriptions of which are provided in Appendix B.2). These include additional measures of satisfaction both with the *process* recipients used to decide how they spent their transfers—an index capturing the extent to which they deliberated over their spending choices—and also their satisfaction with the outcomes realized, captured in their subjective assessment of progress towards their personal goals (Column 2) and retrospective valuation (Column 3) of the items they purchased. Across all of these outcomes the consistent pattern is that modest amounts of deferral had a positive impact (i.e., the linear term is positive) while further prolonging it eventually reduces the impact (i.e., the quadratic term is negative). None of the estimates are significant, however, and so they should be taken as only suggestive.

Results from a linear specification (Table A.18) are also insignificant, including for income. This is not surprising, given that the significant effects on income in Table 2 are non-linear. That said, it is naturally important to consider carefully how much weight to place on results from one specification when another yields insignificant ones. To that end, we emphasize a few points. First, we pre-specified a non-linear form, and did so because it is not easy to rationalize monotonic effects. If temporal effects are seasonal, for example, then by definition they are cyclical, not monotonic. If due to planning effects, then it is hard to believe that the benefits of more time to plan *never* diminish. Second, the usual concern that a non-linear parametric specification may give undue influence to small amounts of data in the tails of the distribution is less relevant here since, by design, our data are distributed relatively uniformly throughout the one-year interval we study. Third, we see the same non-linear pattern across results from two independent experiments. Fourth, tests based on a fully non-parametric alternative to the quadratic model—the seasonal effects depicted in Figure 3—reject the null that timing does not matter ($p = 0.057$ for the preferences study coefficients, and $p = 0.001$ for EHMNW). In addition, a Lind and Mehlum (2010) test rejects the null of an *arbitrary* monotone or convex effect of timing on income ($p = 0.047$). Overall, we feel confident taking seriously the results from the non-linear specification.

4.4 Why wait?

Together these results, derived from two independent studies, provide reasonably strong evidence that the time at which recipients received funds had a meaningful effect on the subsequent growth of their income. Why might this be?

One potential mechanism, applicable in both studies, is that the *time of year*, or season, in which they received money modulated its impact. Figure 3 makes this interpretation explicit, associating different times of year with the corresponding seasons in the agricultural calendar.

[23] Note that the *overall* effect of treatment on both assets and expenditure in EHMNW were strongly positive; see Panels A & B in Table 1 of their paper.

Specifically, it plots the coefficients obtained from regressions analogous to Equations (2) and (3) but with $\sum_s \beta_s q_{h,s}$ as the key timing terms, where $q_{h,s}$ is the share of overall transfer that household h received in a given agricultural season s. Seasons are defined as in Ndungu et al. (2019), spanning two planting / growing / harvest cycles per year, of which the first—from February through September—is the more important one.[24] There are some differences between the two income series, to which we will turn shortly, but a consistent pattern in both is that receiving funds during the main growing or harvest season led to stronger subsequent income growth than receiving them during the first planting season.

Because the preferences study delivered transfers over the course of a single year (from February 2015), it does not address whether the temporal heterogeneity we observe in it would recur in other years. The EHMNW data do give us some insight into this, however: while most transfers were delivered during the same year from February 2015, the full delivery window was somewhat wider, starting in November 2014 and (largely) concluding by May 2016 (see Panel C of Figure 1). In Table A.20 we exploit this fact, re-estimating Equation 3 but (in Columns 2 onward) defining the timing variable t as the number of months since the *most recent* February, rather than since February 2015.[25] Results using both transfer tranches (Column 2) are qualitatively similar to those using the original timing definition, but less precisely estimated. Results using only the first transfer tranche (Column 3), on the other hand, are highly significant. We conjecture that this may reflect the large general equilibrium effects that EHMNW document: by the time second tranches arrived, treated villages would have received a large positive demand shock, on the order of 10% of GDP with an estimated transfer multiplier of 2.5x, which would tend to mute seasonal patterns in the local economy. Consistent with this interpretation, Columns 5–7 show that the seasonality we observe in first-tranche effects is driven by wage earnings, which require neighbors with money to spend.

A second potential mechanism involves time to plan. In the preferences study, receiving a transfer later in the year also meant more time elapsed between when a recipient learned that they would be receiving money, and when they actually received it. This might afford them more time to form a plan, which could in turn be helpful for resisting temptations and social pressures to use money in myopic ways. The fact that treatment effects on the deliberation index and social input measures (Table 3, Columns 1 and 2) mirror those on income is consistent with this interpretation, though these effects are not significantly different from zero.[26] And planning was the reason most often mentioned by respondents in their qualitative explanations for their preferences, even over tranching (see Table A.2). This mechanism cannot explain the time patterns we see in the EHMNW data, on the other hand, since in that study enrollment and transfers were conducted on a rolling basis so that the amount of time between learning one would receive a transfer and actually receiving it was unrelated to the calendar date of receipt.

[24]The primary planting season occurs in February and March, followed by the growing season from April to June and the harvest period in August and September. The second planting cycle begins in August and September, with the subsequent growing season spanning October to December, and harvesting taking place in January. In the regression underlying the figure the omitted category is the first planting season, so that the other seasonal coefficients are estimated relative to it.

[25]For instance, t would equal 9 months for a transfer occurring in November 2014 and 6 months for a transfer occurring in May 2016.

[26]Estimated effects on the underlying components of the deliberation index mostly follow the same pattern but are also not significant; see Table A.19.

The effects we see there must therefore reflect temporal heterogeneity.

Significant *differences* between the time pattern of effects in the studies, on the other hand, would provide strong evidence for non-seasonal mechanisms such as planning. Here the evidence is mixed: we do not reject the joint nulls that the timing regressors in Table 2 are the same across studies ($p = 0.33$ for the coefficients β_1 on timing, $p = 0.16$ for the coefficients β_2 on timing squared), but do reject the null that the seasonal indicators in Figure 3 are the same across studies ($p < 0.001$). Interestingly, the main difference in the latter case is that estimated effects of receiving transfers during harvest seasons, relative to planting season, are *larger* in EHMNW than in the preferences data. To the extent the differences between the studies reflect the effects of planning, therefore, they suggest that the planning channel offsets rather than amplifies the purely temporal effects.

Overall, our interpretation is that there is good evidence for seasonality per se in the treatment effects on income (since this is the only explanation applicable to the EHMNW results), and some suggestive evidence that the planning channel may also have played a role. It is noteworthy in this regard that the explanations that participants in the preferences study gave to survey enumerators include remarks that evoke both channels (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 certain times of the year.

There is also a more mechanical question about what recipients did differently with their transfers as a function of timing which led to differences in income. This is a subtle issue. Having more time to plan might lead one recipient to decide to start a business while leading another to decide *not* to, for example, and so have no average effect. That said, the data do suggest some broad patterns. In data from both studies, the seasonal pattern we see for overall income is mirrored in the share of spending on durables overall and in the likelihood that a household reported using its transfer to start or invest in a non-agricultural enterprise (Figure A.7), though the seasonal variation in these effects (all estimated relative to the effect of transfer receipt in the planting season) is not statistically significant. In the EHMNW data income gains are driven by wage earnings, with a smaller contribution from other sources (Table A.21), which is consistent with employment generation (keeping in mind that transfer timing was assigned at the village level in their study). In the preferences study we did not collect income by source, so can at best split the sample based on primary occupation; doing so shows that gains were largest for the non-farm self-employed, with little effect on wage laborers, presumably reflecting the individual-level randomization in that design (Table A.22).[27] This is consistent with qualitative responses cited earlier, in which subjects emphasized their perceived benefits of setting up non-farm businesses in particular seasons of the year. Overall these patterns are broadly consistent with prior evidence that lumpy transfers have driven diversification from farming into non-agricultural enterprise and employment generation in this area (Egger et al.,

[27]Primary occupation is potentially endogenous, but in practice we see no significant treatment effects on it (Table A.23). Overall there was 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.

2022, Orkin et al., forthcoming).

5 Financial feedback loops: cash flow & decision-making

We next reconsider recipients' preferences over transfer timing in light of the fact that (as we have seen) it affected subsequent income. While it was natural a priori to wonder why recipients in the preferences study wanted any deferral, one might argue having seen its effects on income that the salient question is why they did not want more. To sharpen this point, and for the sake of argument, suppose a common relationship between transfer timing and treatment effects on (expected) income. Then we can use the estimates in Table 2 to calculate the timing that maximizes the effect (5.3 months, at which point the estimated effect is $1,368) as well as the average effect of giving each recipient money at the time they preferred (yielding an average effect of $1,173). We readily reject that these are the same ($p < 0.001$).[28]

One reason, as discussed above, may simply have been that subjects were not optimizing for income alone, but had a broader set of goals in mind.[29] Another is that they may not have fully anticipated the income effects that transfer timing would have. In the endline survey we asked respondents whether they would have preferred to receive the transfer starting at a time other than the one they were randomly assigned, and, if so, when. 77% said that they would *not* have wanted timing different from what they were assigned. Since randomly assigned timings were distributed uniformly, this implies that they were at least content with timings much later than those they initially preferred. Of course, this may simply indicate that they did not want to look a gift horse in the mouth. But even among the 23% who did report a preference for a transfer start date different from the one they were assigned, we see a strong preference for dates later than those they initially preferred (Figure A.8) and readily reject the null that the two distributions had the same mean ($p < 0.001$). These subjects may have learned, from their own experiences or those of neighboring recipients, about the merits of receiving money later in the year.[30]

A third contributing factor may have been that choosing to wait a bit longer for a very substantial transfer was not easy, even for households that did want to increase their income and did believe that waiting would help them do so, given the financial pressures they faced. We asked respondents some questions about these pressures at the same time that we elicited

[28]The maximum-impact calculation is appropriate if the true relationship is not merely concave, but inverse U-shaped. Table A.24 reports results from the test proposed by Lind and Mehlum (2010), which reject the null of a non-inverse-U-shaped relationship both with ($p = 0.028$) and without ($p = 0.047$) controls for baseline income.

[29]We looked specifically, for example, at whether income effects move in the opposite direction as measures of food consumption, suggesting a tradeoff between income generation and immediate needs. They do not, but the results (available on request) are not precise, and the consumption measures refer to a reference period well after transfers were first received, so this is not dispositive.

[30]One logical extension of this line of inquiry is to examine whether treatment effects were heterogenous in such a way that getting the timing (or tranching) a subject preferred led to different effects. One can in principle examine this question in two ways. One is to look within the 90% of subjects who were assigned a given dimension of structure at random, and compare those who happened to get the structure they wanted to those who did not. This makes use of most of the sample, but is not necessarily clearly interpretable, since heterogeneity in preferences could covary with other factors which also predict treatment effects. The other approach is to simply compare the 90% of subjects assigned a structure at random to the 10% who all received their preferred structure; this is cleanly interpretable, but likely underpowered.

The pre-analysis plan report presents reports from the former approach, and Table A.25 presents results from the latter. Unfortunately neither yields estimates precise enough to support meaningful conclusions.

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.26) 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.27). 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.28). 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.9). 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.[31,32]

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 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 4.3) 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, Column (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

$$\frac{1}{|\mathcal{H}^e|}\sum_{h\in\mathcal{H}^e}\left(121.5\times(T_h+0.37-T_h)-11.9\times\left[(T_h+0.37)^2-(T_h)^2\right)\right] \tag{4}$$

where $\mathcal{H}^e$ is the set of households assigned to the early token group. Calculating (4) yields an estimated annual income gain of USD 41, or 5% of the transfer amount. In other words, simply perturbing the *timing* of the initial USD 35 token transfer induced a USD 41 better forward-looking decision.[33]

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)

[31]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.

[32]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 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.

[33]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.29). Another assumption is that the relationship between main transfer timing and outcomes is stable with respect to token transfer timing; consistent with this assumption, we tested for and did not find any significant interactions between the effects of the two on downstream income growth (not reported).

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 typical 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.

6 Conclusion

Our examination of the structure of 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 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 in their region, or accounting for seasonal variations in income (say), 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 over 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., 2023). It could be that since all the options (including streams) offered here were relatively short, lasting at most one year, recipients prioritized investment and therefore preferred lumpy structures. Were longer-term streams—providing a secure source of income over an extended period—an option, we might see more demand for them. That said, Banerjee et al. (2023) study very long-term transfer streams and find that these generate a sharp uptick in ROSCA activity, as recipients reverse-engineer those streams into lumpier structures.

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. Hensel et al. (2025), for instance, find that around half of female factory workers in Ethiopia preferred to receive job displacement insurance in monthly payments instead of the most common design featuring a single, lump-sum payment, and that the monthly stream of payments yielded larger long-term consumption and employment effects than the lump-sum. Second, 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. Third, it could examine how preferences respond to planning aids such as those in (Augenblick et al., 2024), or to better availability of financial products (whose absence may explain the preferences we observe). Finally, 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.

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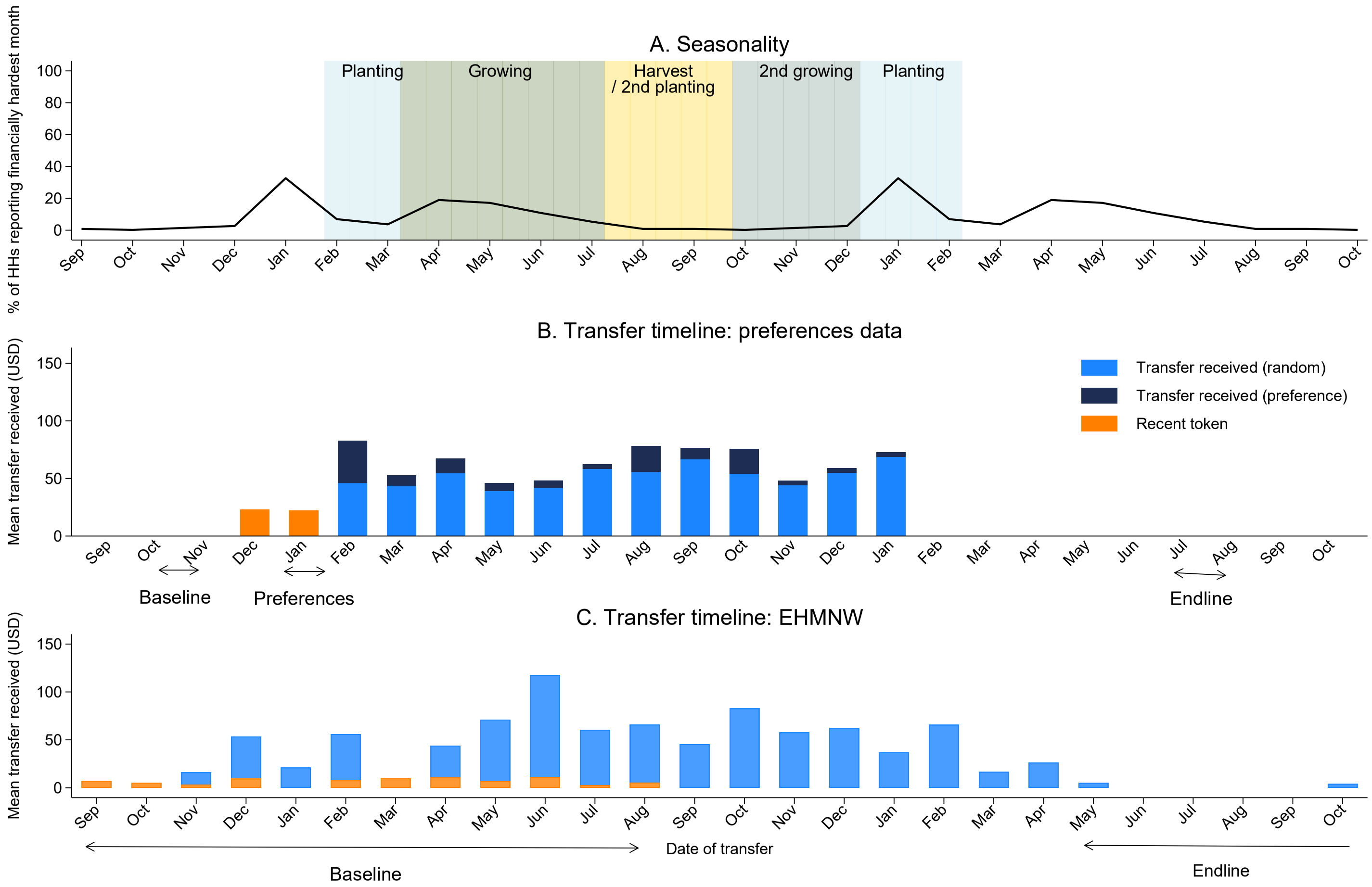
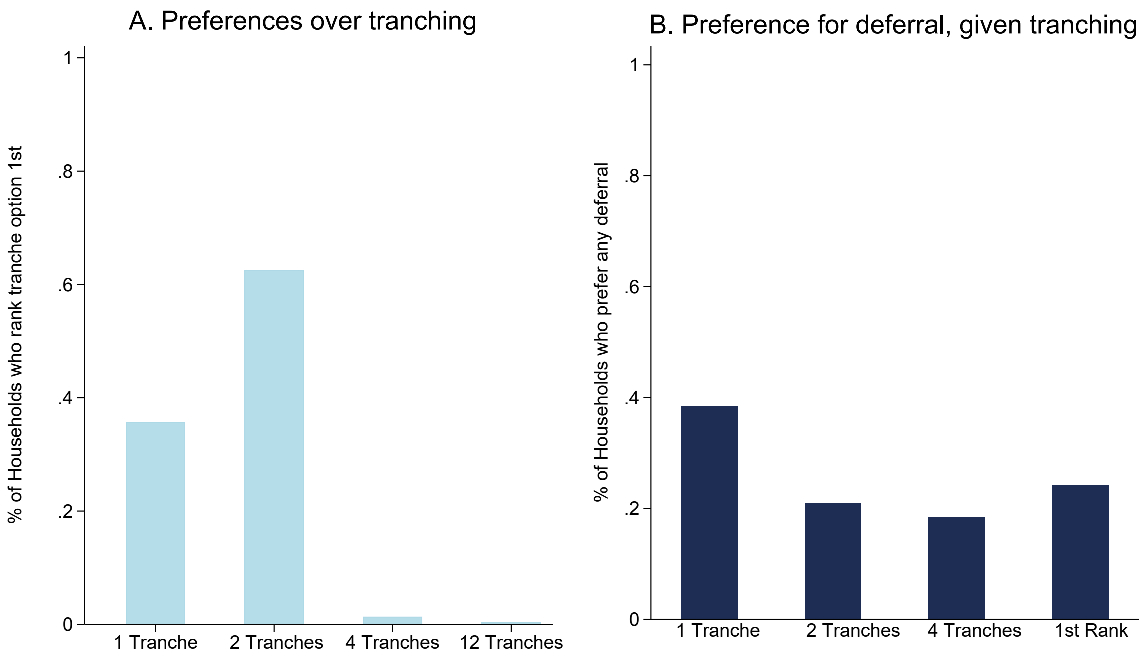
Figure 1: Study designs and transfer timelines



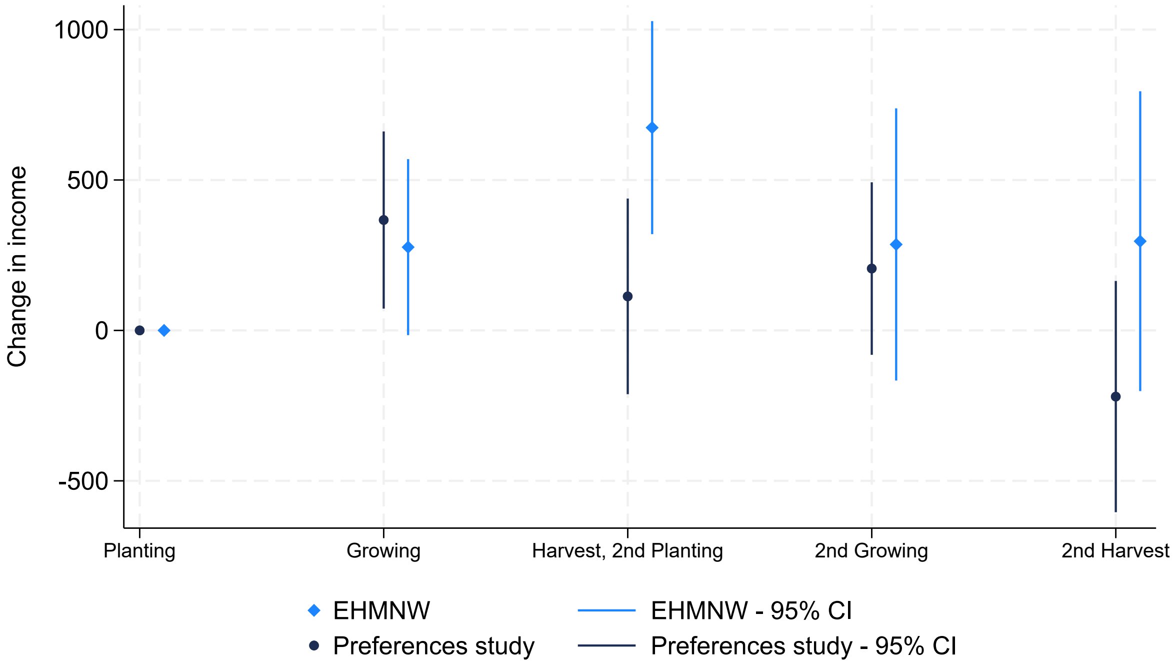
Notes: This figure displays the timing of transfers and measurements, with information about local agricultural seasonality for context. Specifically, Panel A displays phases of the agricultural cycle as defined by Ndungu et al. (2019) along with the fraction of respondents from the preferences study who self-reported each month as one that is typically financially difficult for their household. Panel B reports 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. Annotations on the x-axis indicate the timing of the surveys. Panel C then reports analogous information for the EHMNW study (note that in that design preferences were not elicited and no subjects were given transfers at times they had indicated they preferred).

Figure 2: Preferences over cash transfer structures



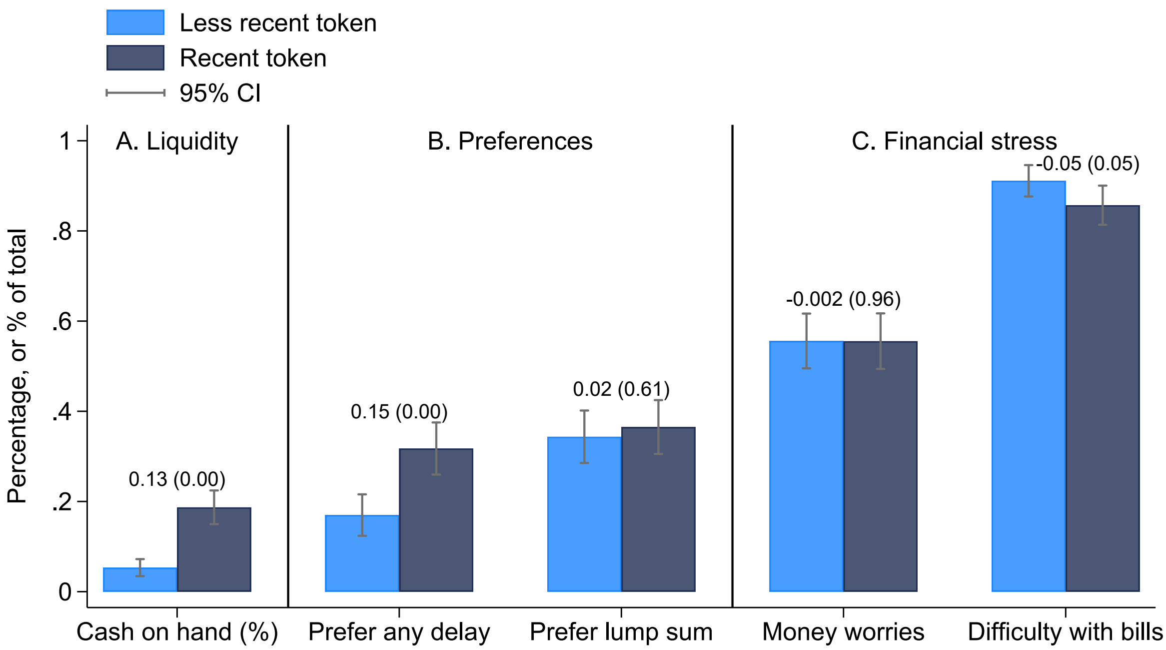
Notes: This figure summarizes participants' preferences over the number of tranches and timing of their cash transfer. In the left panel, each bar displays the fraction of study participants who rank the number of tranches given on the x-axis label as their first choice. In the right panel, each bar 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.3, and the full joint distribution is in Table A.3.

Figure 3: Effects of transfers on income, by season



Notes: This figure plots estimated effects of cash transfers on the change in participants' income (controlling for baseline income) by the season in which the money was received, using data both from our own study (Preferences study, 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 preferences study data. To further facilitate comparison, both planting season coefficients are normalized to zero. Point estimates and 95 percent 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 2) 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 a control for endline survey date (as their endline survey took place at varying dates). The regression for the preferences study 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). The seasons labeled as '2nd' refer to the crop cycle associated with the short rainy season of October and November. The estimation samples are the 90% of the preferences study sample that was randomly assigned to deferral, and the full EHMNW sample. Standard errors are heteroskedasticity-robust for the preferences study 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 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. 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

	(1) Goal progress	(2) Income	(3) Assets + Expenditures	(4) Retrospective valuation
$\rho^1\beta_1 + \rho^2\beta_2$	0.04	58.3	6.3	1,338.4
	(0.03)	(137.80)	(497.63)	(1,151.83)
N	434	430	434	427
Dependent variable mean	0.8	1,234	4,485	2,390

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 in the preferences study, 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. "Income" is participants' total annual income at 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 valuation (in USD) respondents assigned at the time of the endline survey to the things they purchased earlier 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. The underlying specification is as described in Equation (1). Heteroskedasticity-robust standard errors are reported in parentheses.

Table 2: Impacts of transfer timing

	Preferences study			Egger et al. (2021) data		
	(1) Income	(2) Income	(3) Assets + Expenditures	(4) Income	(5) Income	(6) Assets + Expenditures
Timing ($\sum_t q_{h,t}t$)	110.25	121.50	96.42	57.19	58.20	11.80
	(65.53)	(63.47)	(222.87)	(15.45)	(15.15)	(29.09)
	[72.86]	[43.60]	[181.84]			
Timing squared ($\sum_t q_{h,t}t^2$)	-10.39	-11.94	-5.59	-3.75	-4.16	-1.99
	(5.72)	(5.36)	(20.71)	(1.12)	(1.11)	(2.22)
	[6.95]	[3.91]	[16.34]			
Prefer deferral	86.74	67.74	169.72			
	(123.62)	(125.33)	(468.38)			
	[62.27]	[58.49]	[168.18]			
Prefer 1 tranche	63.38	67.73	217.46			
	(104.92)	(107.67)	(336.83)			
	[63.75]	[63.75]	[296.15]			
Baseline income		0.11			0.41	
		(0.13)			(0.14)	
		[0.09]				
Treated				-65.59	-50.43	231.22
				(53.51)	(52.14)	(97.24)
Endline survey date				22.74	23.87	6.18
				(3.42)	(3.51)	(8.43)
N	420	393	424	8,156	8,156	8,143
Dependent variable mean	1,242	1,257	4,576	617	617	2,089

Notes: This table reports estimated effects of the timing of the main transfer in the preferences study (Columns 1–3) and EHMNW study (Columns 4–6). For the preferences study, the estimation sample is the 90% of participants for whom timing was assigned randomly, and estimates are from an instrumental variable analogue of Equation 2 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 in Table A.15. The second-stage regression is then as defined by Equation 2 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: "Income" is participants' recorded income at endline, in USD; "Baseline income" is the participants' total annual income at baseline; "Assets + expenditures" is the sum of assets owned at endline by the household, and the annualized value of household expenditures at endline. Further details on the construction of some of these variables is in Appendix B.2. Heteroskedasticity-robust standard errors are in parentheses; standard errors clustered by timing preference (conditional on tranche count) are in square brackets. For EHMNW, estimates are from Equation (3) which includes controls for assignment to any treatment ("Treated") and the date of the endline survey. Standard errors are clustered at the village level (the level at which treatment (and timing) was assigned).

Table 3: Impact of deferral on planning and subjective measures of progress

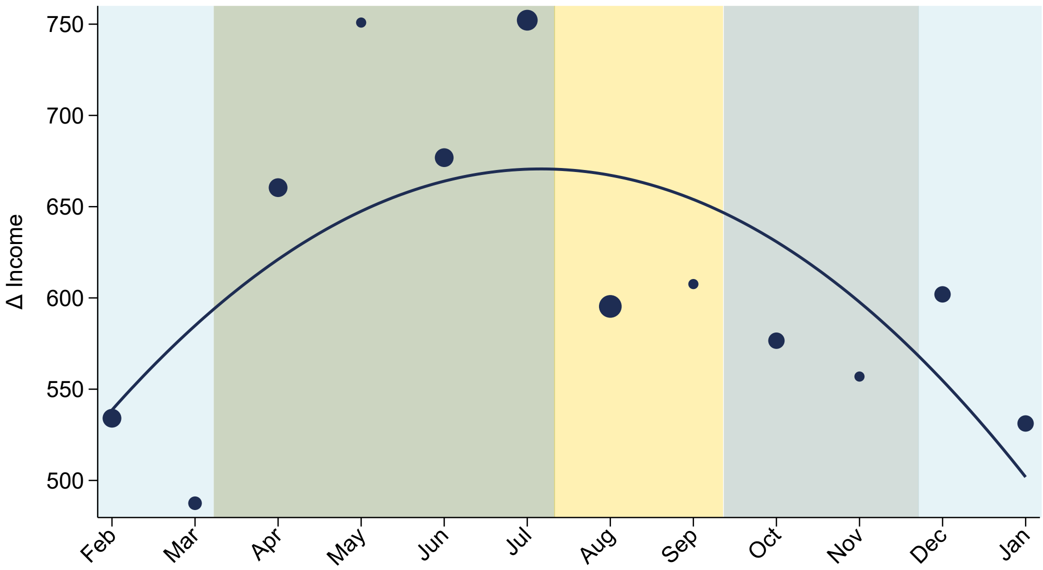
	Deliberation	Social Input	Goal Progress	Retrospective Valuation
	(1)	(2)	(3)	(4)
Timing ($\sum_t q_{h,t}t$)	0.042	0.013	0.011	655.6
	(0.034)	(0.014)	(0.013)	(686.8)
	[0.014]	[0.0081]	[0.0039]	[524.4]
Timing squared ($\sum_t q_{h,t}t^2$)	-0.0027	-0.0013	-0.0014	-58.8
	(0.0029)	(0.0012)	(0.0012)	(60.1)
	[0.0011]	[0.00071]	[0.00045]	[42.3]
Prefer deferral	-0.026	-0.016	0.050	-516.2
	(0.054)	(0.020)	(0.017)	(530.5)
	[0.044]	[0.013]	[0.0096]	[565.3]
Prefer 1 tranche	0.028	-0.0041	0.0064	-495.0
	(0.044)	(0.017)	(0.018)	(575.7)
	[0.044]	[0.010]	[0.022]	[883.7]
N	424	424	424	417
Dependent variable mean	0.00	0.60	0.81	2,480

Notes: This table reports estimated effects of the timing of the main transfer in the preferences study. 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 2 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.15. The second-stage regression is then as defined by Equation 2 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; '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 "Retrospective Valuation" is the valuation (in USD) respondents assigned at the time of the endline survey to the things they purchased earlier, using the transfer. Further details on the construction of some of these variables is in Appendix B.2. Heteroskedasticity-robust standard errors are in parentheses; standard errors clustered by timing preference (conditional on tranche count) are in square brackets.

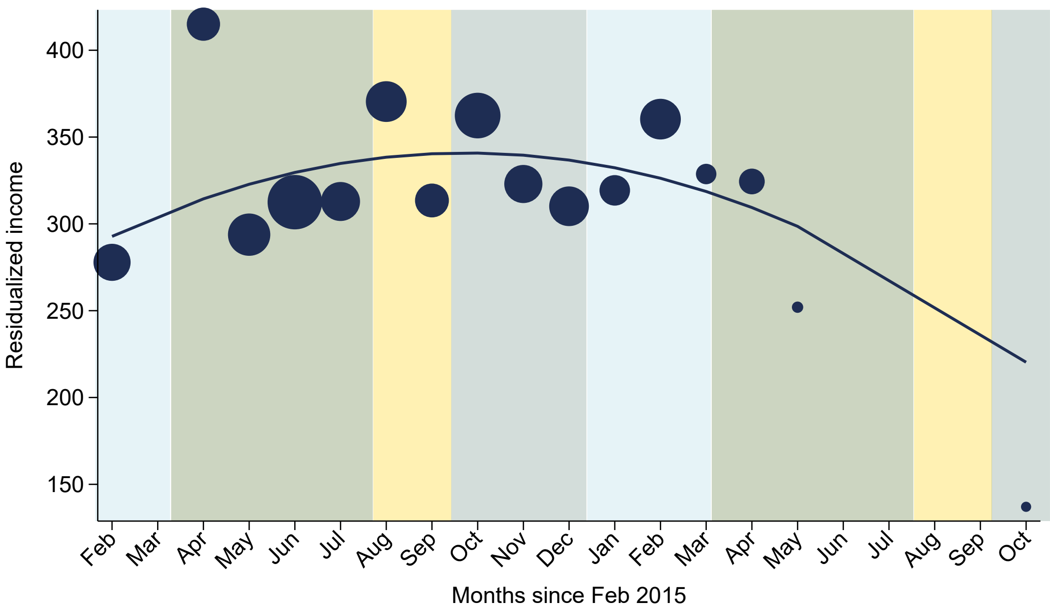
A Additional exhibits

Figure A.1: Transfer timing and income growth

(a) Preferences study

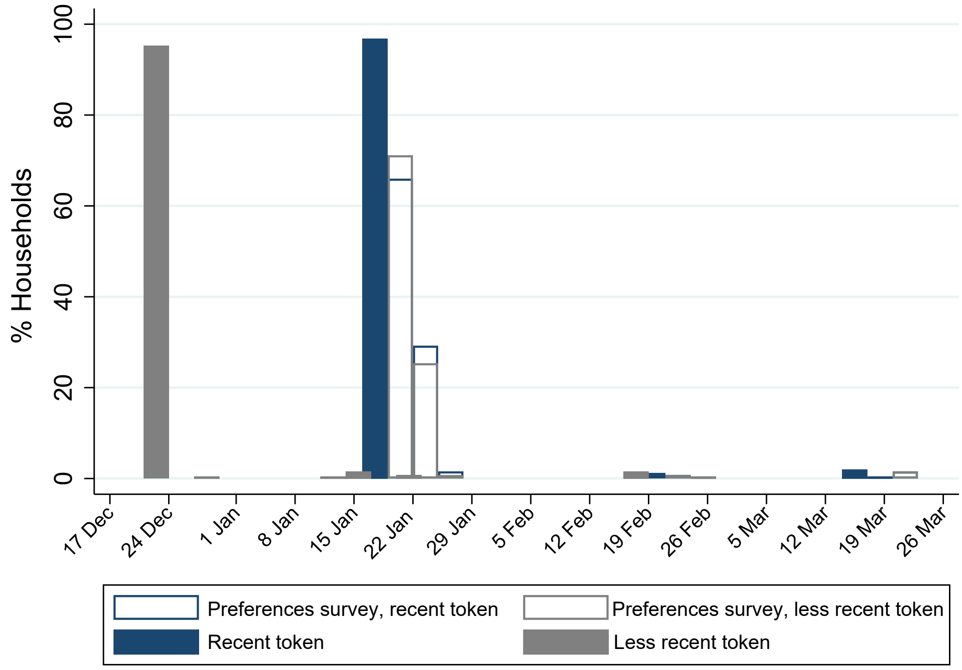


(b) Egger et al. (2022) study



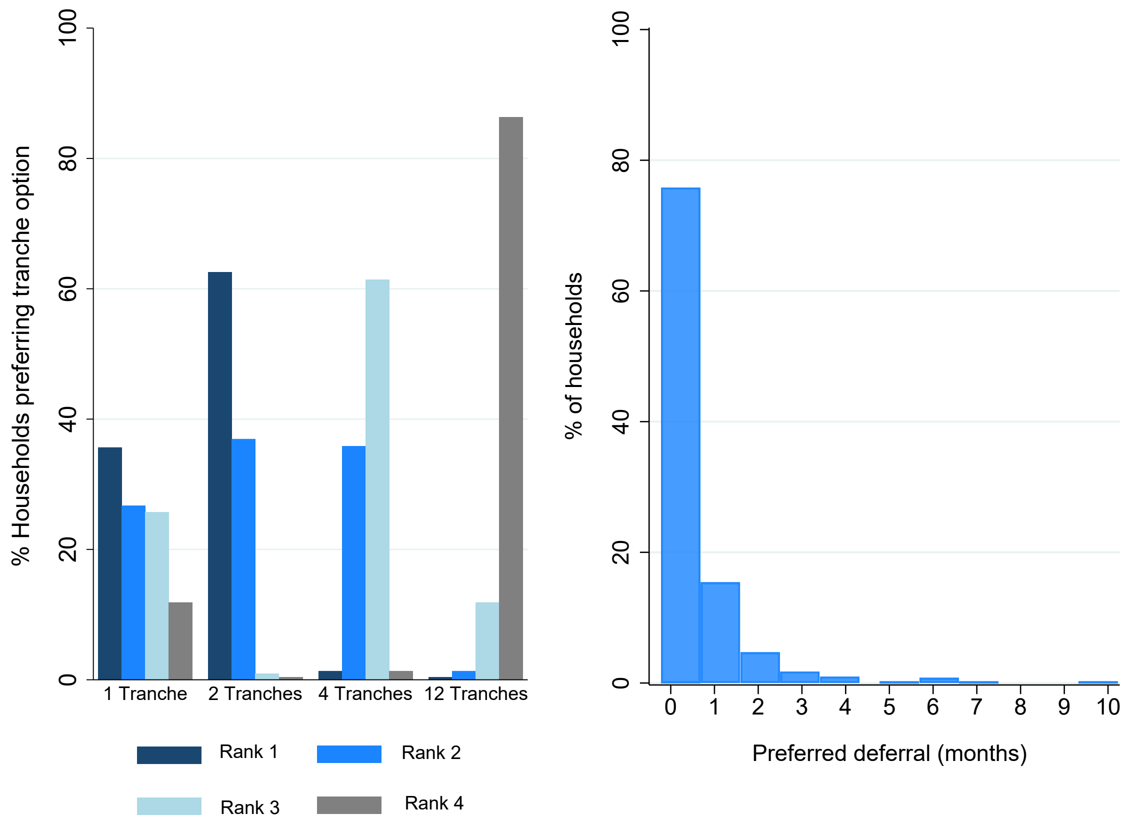
Notes: This figure displays the relationship between the time at which recipients received cash transfers (x axis) and the change in income between baseline and endline surveys (y axis). Time is denoted in months. Dots represent the mean income change for households, weighted by the share of funds $q_{h,t} = \{0, 0.5, 1\}$ received in that month, with dot size representing the total amount received in that month. In Panel (b) these dots were constructed using an income measure that has been residualized by projecting on the date of the endline survey (E_h), as this independently predicts measured income. The overlaid curves are the quadratic fits obtained by estimating Equation 2 in the preferences study panel, and Equation 3 in the EHMNW panel. Seasons are defined as in Ndungu et al. (2019) by grouping corresponding calendar months. In both panels, the size of bubbles is proportional to the number of observations in each bin.

Figure A.2: Preference survey dates and compliance with token transfer treatment



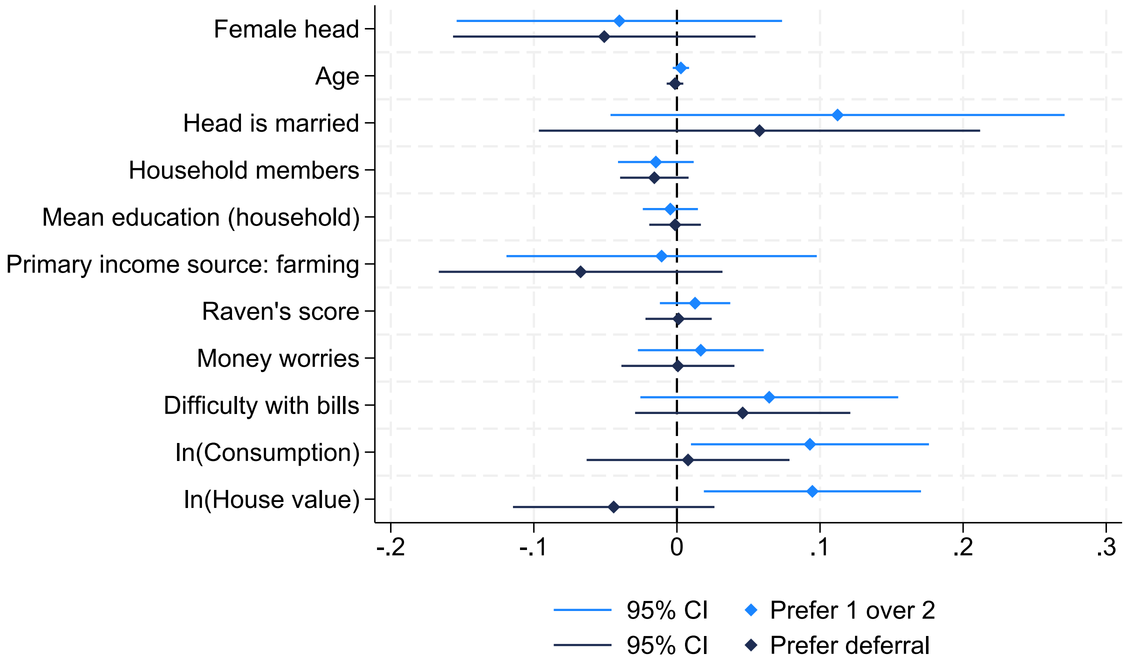
Notes: This figure presents distributions of the dates on which token transfers were issued (solid bars) and preference surveys were conducted (hollow bars) for the recent token group (in blue) and the less recent token group (in grey). Note that the great majority (94%) of preference surveys were conducted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

Figure A.3: Distributions of preferences over tranching and deferral



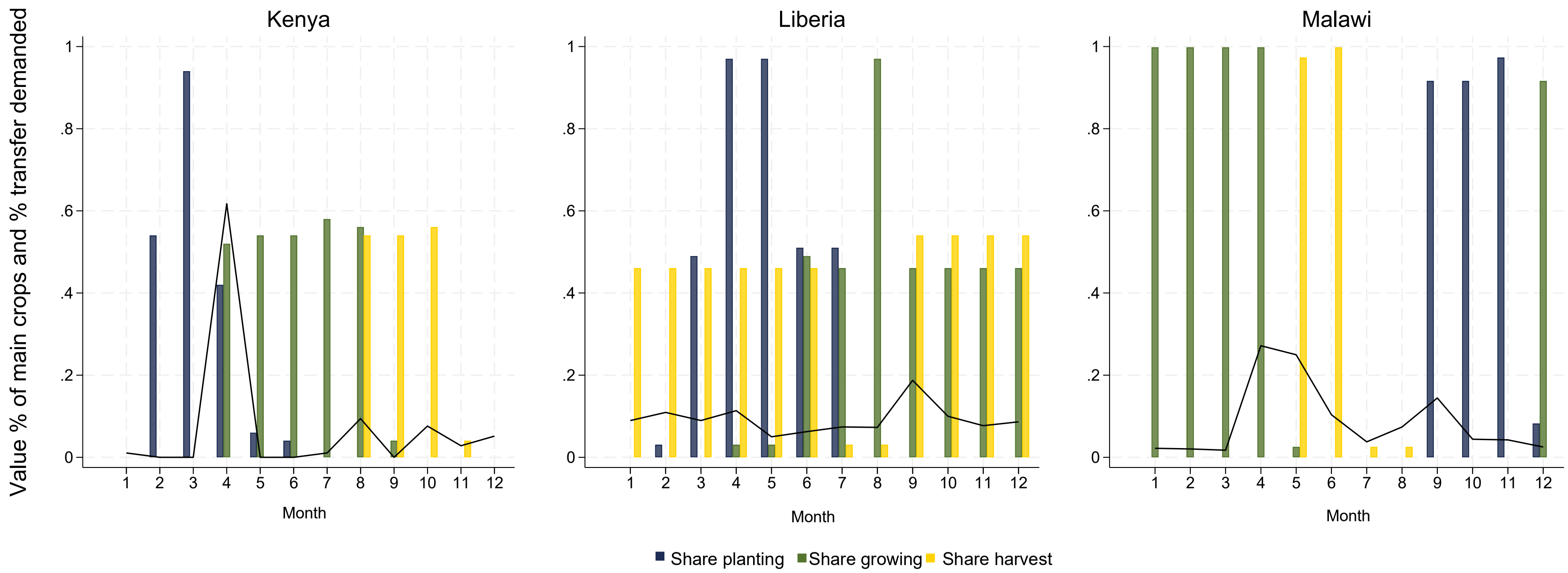
Notes: This figure presents distributions of participants' preference over transfer structures. The left panel illustrates their preference rankings of the available tranche structures (1, 2, 4, and 12 tranches). x-axis groupings indicate tranche counts, and the height of each bar within a grouping indicates 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.4: Predictors of preferences for tranching and deferral



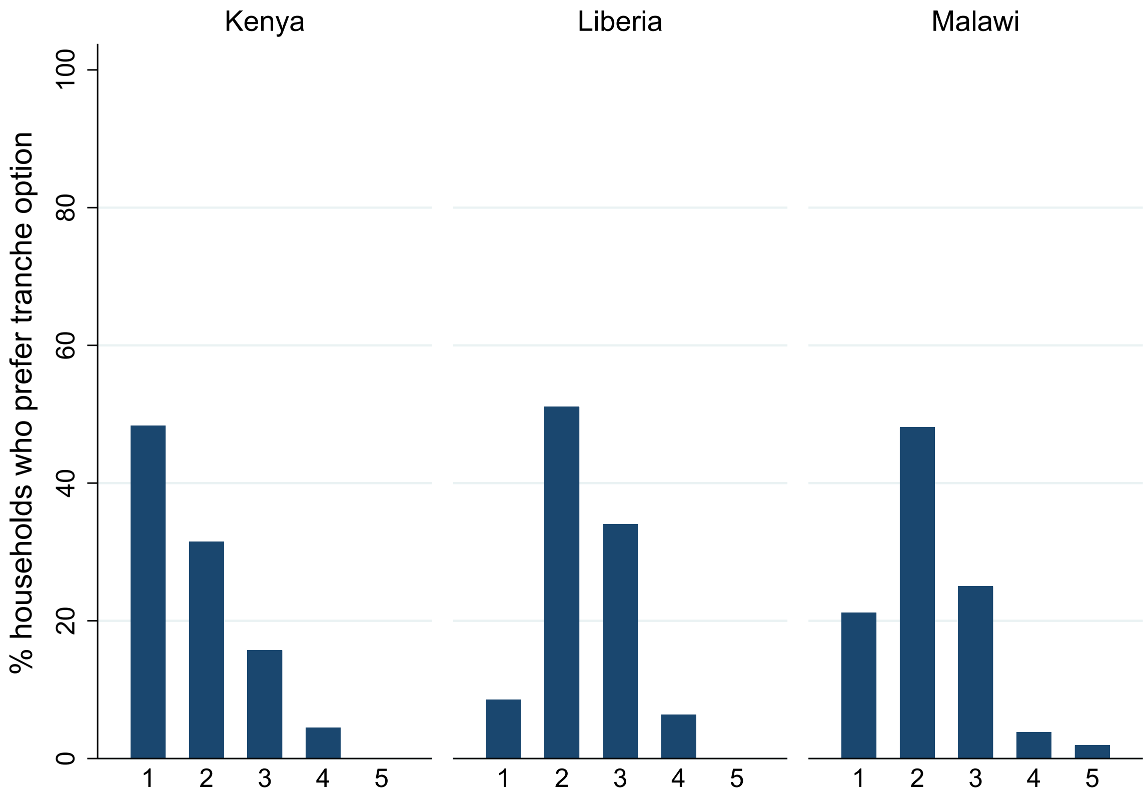
Notes: This figure plots estimated coefficients and confidence intervals from a probit model where the dependent variable is an indicator for preferring one tranche as opposed to two tranches (in grey) or an indicator for preferring any deferral (in blue), where any deferral means receiving the first tranche in at least a month after February 2015. Regressors are those indicated on the y-axis; see Appendix B.2 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.5: Seasonal patterns in Kenya, Liberia, and Malawi



Notes: This figure displays measures of agricultural seasonality for 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.

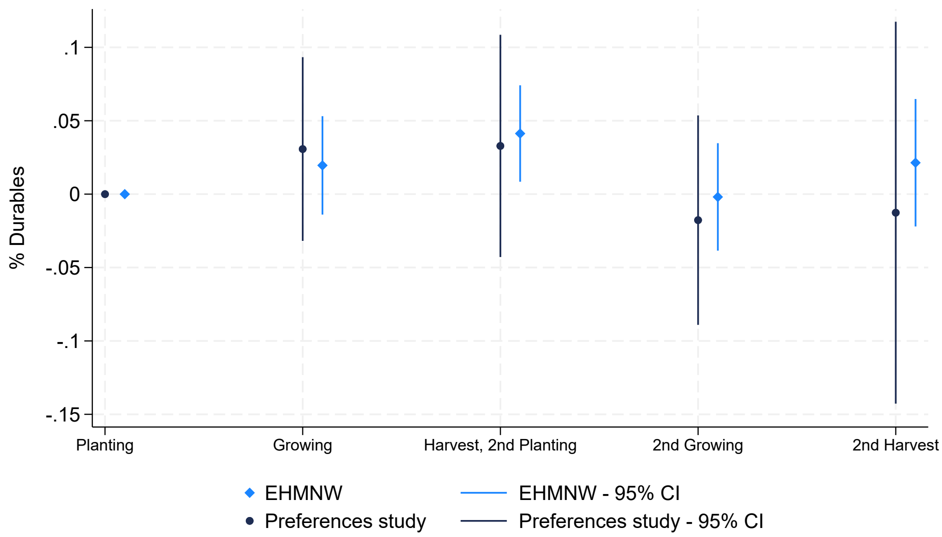
Figure A.6: Tranching preferences in Kenya, Liberia, and Malawi



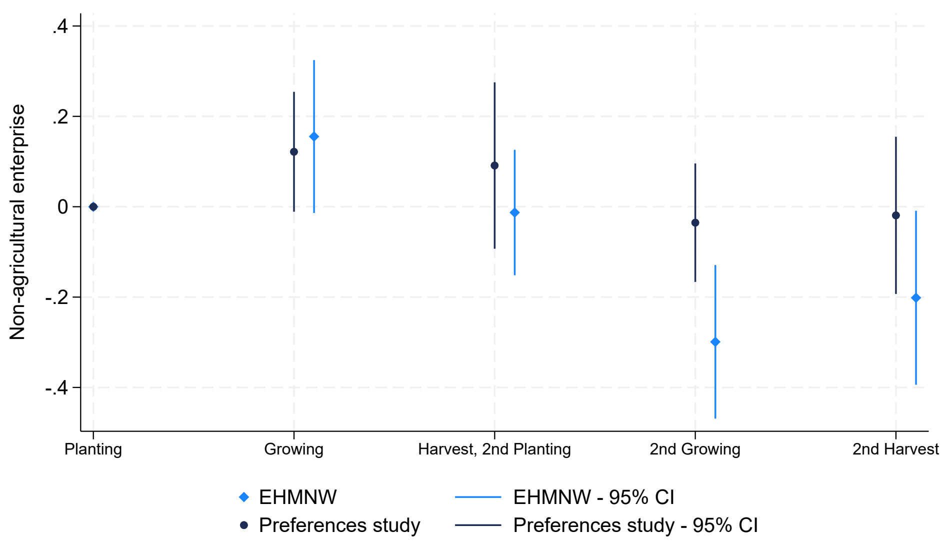
Notes: This figure displays stated preferences over number of tranches from the GD preference surveys discussed in Section 3.4. The x-axis indicates numbers of tranches, and bars represent the share of participants who preferred to receive their transfer in that many tranches. Note that while in practice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

Figure A.7: Impact of transfer timing on measures of investment

(a) Durables expenditures

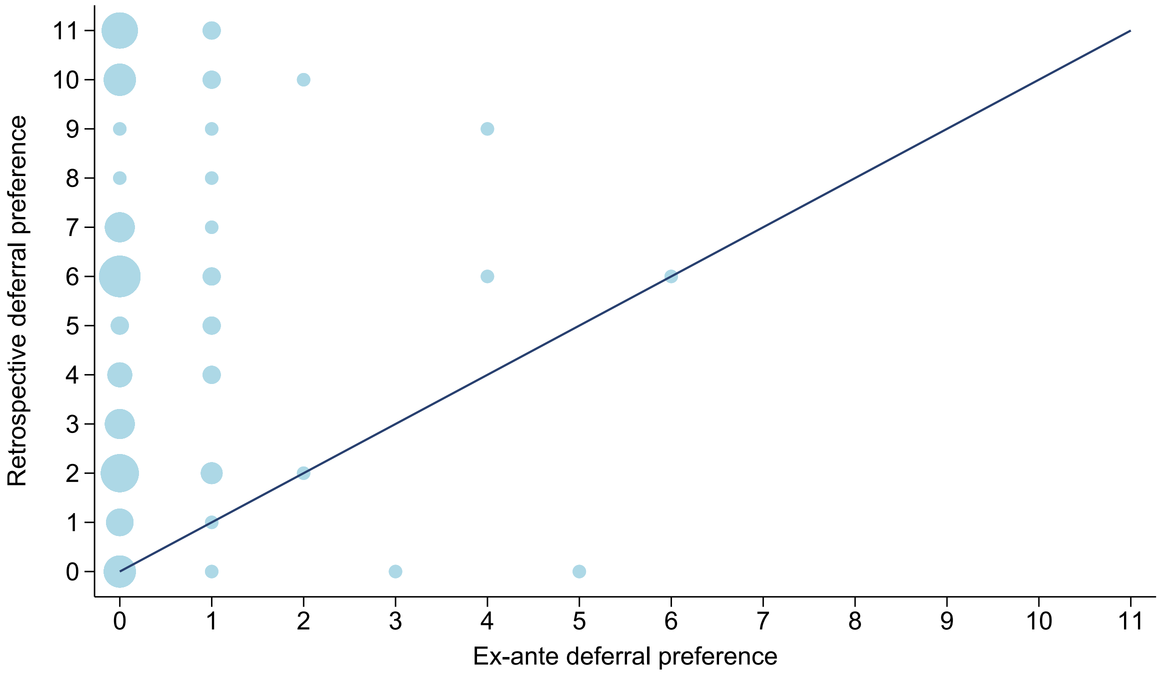


(b) Non-agricultural self-employment



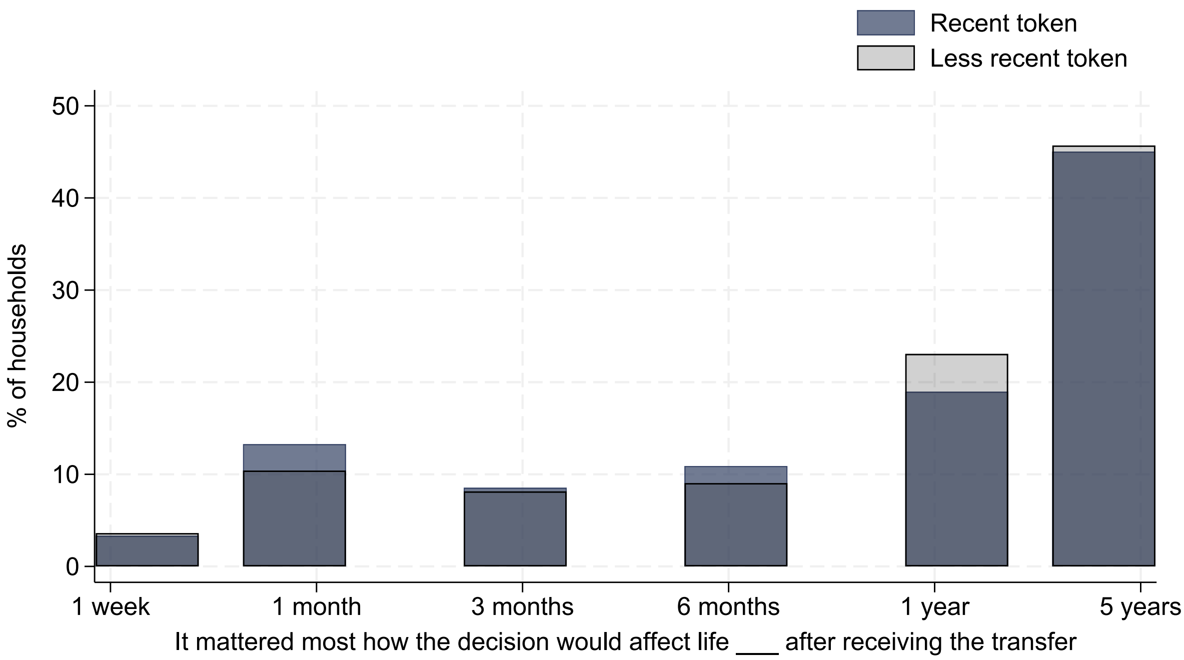
Notes: This figure plots estimated effects of transfer timing on the share of expenditure on durables (Panel A) and on an indicator for using the transfer to start or invest in a non-agricultural enterprise (Panel B). Results are broken out by season as defined in Ndungu et al. (2019), where the seasons labeled with "2nd" are those associated with the short rains of October and November. The bright blue markers show coefficients estimated on EHMNW data and the dark blue markers show coefficients estimated on the preferences study data. These coefficients are estimates from 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 and ϵ_h is the error term. Controls included for the preference study are indicators for preferring one tranche to two and preferring any deferral, and the number of enterprises owned by the recipient before receiving the transfer. Controls included for the EHMNW study are a treatment indicator, the date of the endline survey, and an indicator for being self-employed in a non-agricultural enterprise at baseline. The durables share is 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 meaning of the non-agricultural self-employment indicator varies slightly by study, reflecting data availability: in the preferences study it is an indicator for whether any of the grant was used to invest in an existing business, while in the EHMNW study it is an indicator for whether the respondent is self-employed in a non-agricultural enterprise at endline. Standard errors are heteroskedasticity-robust in the preferences study specification and clustered at the village level in the EHMNW specification. Whiskers on the bars denote 95% confidence intervals. p-values for tests of equality across seasons are 0.63 (0.01) for the durables expenditure share and 0.26 (0.00) for the enterprise investment indicator in the preferences study and the EHMNW study respectively.

Figure A.8: Retrospective preference for deferral



Notes: The figure shows a scatterplot of the distribution of retrospective preference for deferral (on the y-axis) and ex-ante preference for deferral (on the x-axis) in the preferences study. Deferral is defined as the start month of the first lump-sum transfer relative to February. The circle sizes in the scatterplot represent the frequency of each retrospective and ex-ante preference pair in the data. The figure only includes the 95 respondents, from among the 90% sample randomly assigned to timing, who retrospectively expressed a preference to have received the transfer in a different month than when they actually received it.

Figure A.9: 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 in the preferences study. 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 on the given dimension (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 on that dimension (so that preferences and assignments are independent).

Table A.2: Classification of reasons for tranche preferences, by evaluator

	Human		Perplexity		ChatGPT		Gemini Pro		Average	
	N	%	N	%	N	%	N	%	N	%
Financing lumpy investments	264	44	191	33	228	38	253	40	234	39
Saving on fixed costs	1	0	0	0	2	0	0	0	1	0
Behavioral constraints	104	18	108	19	86	14	148	23	112	18
Security concerns	2	0	0	0	2	0	2	0	2	0
Transaction costs	0	0	0	0	0	0	0	0	0	0
Consistency with GD practice	0	0	0	0	0	0	2	0	0	0
Planning	219	37	280	48	229	38	231	36	240	40
Kin tax	0	0	0	0	0	0	0	0	0	0
Other: vague	4	1	2	0	57	9	1	0	16	3
Total	594		581		604		637		604	

Notes: this table reports the number of times that each of four evaluators assigned one of the classifications noted in the rows to a subject's explanation for their preferred tranche structure. Evaluators were allowed to assign more than one classification, and did so with differing frequency, so that the total number of classifications varies slightly across evaluators. Categories are as follows: financing lumpy investments (respondent mentions the need to finance large purchases), saving on fixed costs (respondent mentions saving on transportation costs or the cost of materials through the transfer structure), behavioral constraints (respondent mentions management of self-control issues, such as avoiding temptation spending), security concerns (respondent mentions risk of theft or other security concerns), transaction costs (respondent mentions M-Pesa withdrawal fees or distance to the location of cash withdrawal), consistency with GD practice (respondent mentions familiarity with the GiveDirectly transfer structure or that recipients have previously received this structure), planning (respondent mentions that the preferred structure enables better deliberation or planning of activities, without reference to self-control), kin tax (respondent mentions that other household or social network members may request the money if received in an alternative structure), and other: vague (answers such as "I like to receive it this way" with no specific explanation).

Table A.3: 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) in the preferences study. A deferral preference of 0 corresponds to transfer onset in February 2015.

Table A.4: Predicting preferences using baseline covariates

	Tranching (1)	Deferral (2)
(A) Regression coefficients for Proxy Means Test covariates		
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
GRF, PMT covariates	0.36	0.24
GRF, all covariates	0.35	0.24

Notes: This table describes the predictability of respondents' preferences using baseline observables in the preferences study. 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.2 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.5: 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 in the preferences study. 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.6: 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 in the preferences study. 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.2 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.7: 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) in the preferences study. 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.2 provides further details on the construction of some of these variables. Column 13 reports p-values from F-tests obtained by estimating Equation (2) with each row variable as a dependent variable, and then testing the null that the coefficients on both timing regressors are zero (i.e., $\beta_1 = \beta_2 = 0$). The final p-values at the bottom of Column 13 correspond to a joint test that all these coefficients are zero for all row variables.

Table A.8: 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 in the preferences study. 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.9: 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 in the preferences study. 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 4.1 describes the circumstances which gave rise to the handful of cases of non-compliance.

Table A.10: 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 the relationship between attrition and treatment assignment in the preferences study. 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.11: Balance in timing assignment in EHMNW

	1	2	4	5	6	7	8	9	10	11	12	p-value
Total assets, including land	479.82	1030.75	453.90	476.74	379.16	411.90	435.38	471.88	433.49	491.21	432.27	
	(41.59)	(589.42)	(28.87)	(38.09)	(24.44)	(23.91)	(26.96)	(105.11)	(29.12)	(60.52)	(28.54)	0.57
Age	42.07	43.39	43.09	43.94	44.25	44.74	42.80	43.66	43.35	44.15	43.15	
	(1.40)	(0.58)	(0.62)	(0.69)	(0.71)	(0.80)	(0.56)	(1.46)	(1.06)	(1.58)	(0.65)	0.02
Education	6.56	5.88	6.36	6.00	5.91	5.89	6.26	5.59	6.47	5.83	6.04	
	(0.29)	(0.18)	(0.19)	(0.19)	(0.15)	(0.25)	(0.15)	(0.35)	(0.23)	(0.25)	(0.18)	0.21
Head is female	0.72	0.71	0.67	0.71	0.71	0.72	0.74	0.76	0.74	0.73	0.80	
	(0.04)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)	(0.02)	0.34
Head is married	0.62	0.59	0.64	0.59	0.56	0.57	0.58	0.61	0.59	0.66	0.59	
	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)	0.57
Head is widow / widower	0.22	0.28	0.24	0.27	0.29	0.27	0.26	0.27	0.29	0.25	0.29	
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.04)	(0.02)	(0.04)	(0.02)	0.15
Children age 0-3	0.59	0.59	0.59	0.57	0.55	0.51	0.55	0.51	0.56	0.57	0.61	
	(0.06)	(0.03)	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.05)	(0.04)	(0.07)	(0.03)	0.55
School-aged children (3-18)	1.79	1.78	2.00	1.87	1.90	2.08	1.89	1.73	1.77	1.82	1.74	
	(0.12)	(0.07)	(0.10)	(0.07)	(0.07)	(0.09)	(0.07)	(0.13)	(0.08)	(0.11)	(0.07)	0.21
Furniture value	149.67	150.06	166.33	148.61	142.90	137.17	158.24	143.74	143.70	146.02	140.50	
	(15.65)	(9.99)	(12.87)	(7.57)	(9.34)	(6.85)	(7.56)	(19.34)	(7.16)	(12.39)	(8.72)	0.35
Radio and TV value	15.17	13.58	14.23	14.42	13.52	13.04	14.79	16.68	12.88	13.27	12.12	
	(2.95)	(1.92)	(2.12)	(1.55)	(1.34)	(1.46)	(1.78)	(5.03)	(1.63)	(3.62)	(1.32)	0.89
Assets: transport	176.33	113.77	74.70	51.63	47.67	38.24	100.57	21.60	46.22	96.36	51.87	
	(125.56)	(71.86)	(25.53)	(16.08)	(12.94)	(8.13)	(43.26)	(10.45)	(11.27)	(62.02)	(11.15)	0.38
Land size	2.30	2.83	2.21	2.63	144.15	2.58	2.76	2.67	2.84	4.26	3.05	
	(0.38)	(0.32)	(0.20)	(0.17)	(141.76)	(0.25)	(0.16)	(0.29)	(0.24)	(0.79)	(0.28)	0.61
Household skipped meals (days)	1.28	1.24	1.17	1.37	1.48	1.46	1.28	1.04	0.92	1.13	1.23	
	(0.18)	(0.08)	(0.09)	(0.09)	(0.09)	(0.11)	(0.07)	(0.09)	(0.09)	(0.13)	(0.08)	0.07
N	191	493	397	512	564	355	574	150	283	146	483	

Notes: The table presents balance with respect to transfer timing in the EHMNW data. Some covariates used in Table A.7 were not available in this dataset, and were omitted or replaced with a proxy. Consumption data was not collected at baseline in Egger et al. (2022). Each column represents a transfer start month, with standard errors in parentheses. The table includes all observations observed at endline. Column 13 reports p-values from an F-test of the null of orthogonality between timing assignment and each covariate individually (obtained by estimating Equation 3).

Table A.12: Attrition in EHMNW

	(1) Attrition
Assigned to April	-0.02
	(0.02)
Assigned to May	0.02
	(0.02)
Assigned to June	-0.01
	(0.02)
Assigned to July	-0.01
	(0.02)
Assigned to August	0.01
	(0.02)
Assigned to September	0.02
	(0.03)
Assigned to October	0.02
	(0.02)
Assigned to November	0.11
	(0.07)
Assigned to December	0.00
	(0.02)
Assigned to January	0.02
	(0.03)
Constant	0.07
	(0.01)
N	3,932
R^2	0.01
p-value	0.376

Notes: This table describes relationships between attrition and timing assignment among the treated individuals in the EHMNW data. The outcome variable is an indicator equal to one if the household was not surveyed at endline. The regressors are indicators for the various possible timing treatment arms, where "assigned to MONTH" indicates that the recipient was assigned to receive the first of two tranches in MONTH. The regression specification includes a constant term and the omitted indicator is "Assigned to February." Standard errors clustered at the village level are in parentheses. The p-value reported at the bottom of the table is from a test of the joint null of orthogonality between the outcome and all regressors.

Table A.13: Impact of deferral: robustness to omitting controls

	Deliberation		Social input		Goal Progress		Income		Assets + Expenditures		Retrospective valuation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Timing ($\sum_t q_{h,t}t$)	0.012	0.04	-0.0008	0.01	-0.005	0.01	-5.5	108.1	32.7	76.6	9.3	642.3
	(0.008)	(0.03)	(0.003)	(0.01)	(0.003)	(0.01)	(17.4)	(65.3)	(60.5)	(221.5)	(59.7)	(676.8)
Timing squared ($\sum_t q_{h,t}t^2$)		-0.003		-0.001		-0.001		-10.3		-4.0		-57.2
		(0.003)		(0.001)		(0.001)		(5.7)		(20.6)		(58.8)
N	424	424	424	424	424	424	422	422	426	426	417	417
Dependent variable mean	-0.00	-0.00	0.6	0.6	0.8	0.8	1,241	1,241	4,565	4,565	2,480	2,480

Notes: This table presents results for the preferences study equivalent to those in Table A.18 (linear specification) and Tables 3 and 2 (non-linear specification), but omitting the control variables included in those tables—specifically, an indicator for preferring one tranche to two, an indicator for preferring any delay, and baseline income. Results are estimated for the 90% sub-sample for which timing was assigned randomly. Heteroskedasticity-robust standard errors are in parentheses.

Table A.14: Impact of deferral: intention-to-treat estimates

	Deliberation	Social input	Goal progress	Income	Assets + expenditures	Retrospective valuation
	(1)	(2)	(3)	(4)	(5)	(6)
Timing ($\sum_t q_{h,t}t$) - Assigned	0.042	0.013	0.011	120.2	95.8	648.8
	(0.034)	(0.014)	(0.013)	(63.3)	(221.8)	(684.1)
Timing squared ($\sum_t q_{h,t}t^2$) - Assigned	-0.0027	-0.0013	-0.0014	-11.87	-5.58	-58.5
	(0.0029)	(0.0012)	(0.0012)	(5.36)	(20.71)	(60.1)
Prefer deferral	-0.027	-0.016	0.050	63.7	169.1	-533.9
	(0.054)	(0.020)	(0.017)	(126.2)	(471.3)	(548.1)
Prefer 1 tranche	0.028	-0.0041	0.0063	67.1	219.0	-491.9
	(0.044)	(0.018)	(0.018)	(108.5)	(338.7)	(576.1)
Baseline income				0.11		
				(0.14)		
N	424	424	424	393	424	417
Dependent variable mean	-0.00	0.60	0.81	1,257	4,576	2,480

Notes: This table presents intention-to-treat estimates for the effects of deferral on endline outcomes in the preferences study. The regressors are constructed as in Columns 1–3 of Table 2 but using assigned deferral. See table A.9 for details on compliance in timing assignment. Results are estimated for the 90% sub-sample for which timing was assigned randomly. The controls include indicators for preferring one tranche to two and preferring any delay. Heteroskedasticity-robust standard errors are in parentheses.

Table A.15: First-stage results

	Income		
	Timing ($\sum_t q_{h,t}t$)		Timing squared ($\sum_t q_{h,t}t^2$)
	(1)	(2)	(3)
Timing ($\sum_t q_{h,t}t$) - Assigned	0.99	1.00	0.13
	(0.0044)	(0.036)	(0.33)
Timing squared ($\sum_t q_{h,t}t^2$) - Assigned		-0.00067	0.99
		(0.0031)	(0.029)
N	420	420	420
F-statistic	49,982	4,014	

Notes: This table presents results from the first-stage regressions underlying the estimated effects of deferral on income in the preferences study—specifically, Columns 1 and 2 of Table 2. Each column reports the results of a separate regression. The dependent variable is mean deferral ($\sum_t q_{h,t}t$) in Columns 1 and 2 and mean actual deferral squared ($\sum_t q_{h,t}t^2$) in Columns 3. 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.9 for details).

Table A.16: Impact of average deferral: nonlinear specification

	Deliberation	Social input	Goal Progress	Income	Assets + Expenditures	Retrospective valuation
	(1)	(2)	(3)	(4)	(5)	(6)
Mean timing ($\sum_t q_{h,t}t$)	0.052	0.015	0.015	114.3	117.9	211.2
	(0.031)	(0.012)	(0.012)	(61.9)	(193.9)	(345.7)
Mean timing squared ($[\sum_t q_{h,t}t]^2$)	-0.0036	-0.0014	-0.0017	-11.31	-7.53	-18.6
	(0.0025)	(0.0011)	(0.0011)	(4.99)	(19.15)	(29.1)
Prefer deferral	-0.025	-0.015	0.051	73.8	173.1	-497.5
	(0.054)	(0.020)	(0.017)	(125.4)	(469.1)	(512.1)
Prefer 1 tranche	0.030	-0.0029	0.0078	77.5	223.1	-461.8
	(0.044)	(0.017)	(0.017)	(106.7)	(335.6)	(545.9)
Baseline income				0.11		
				(0.13)		
N	424	424	424	393	424	417
Dependent variable mean	-0.00	0.60	0.81	1,257	4,576	2,480

Notes: This table presents estimated effects of average deferral on the outcomes presented in Tables 2 and 3 using data from the preferences study and a non-linear specification. Results are estimated for the 90% sub-sample for which timing was assigned randomly. The specification also includes controls for preferences (preference for any deferral, and preference for one as opposed to two tranches) and, in Column 4, for the baseline value of income. Heteroskedasticity-robust standard errors are in parentheses.

Table A.17: Differential effects of transfer timing with respect to transfer→measurement duration

	Income
Timing ($\sum_t q_{h,t}t$)	60.40
	(101.64)
Timing ($\sum_t q_{h,t}t$)# D_h	0.82
	(4.92)
Timing squared ($\sum_t q_{h,t}t^2$)	-1.35
	(6.23)
Timing squared ($\sum_t q_{h,t}t^2$) # D_h	-0.21
	(0.30)
D_h	22.36
	(3.92)
Treated	-362.57
	(374.22)
Treated # D_h	17.55
	(17.47)
Baseline income	0.40
	(0.14)
N	8,156

Notes: This table reports estimates from the EHMNW study of the specification

$$y_h = \alpha + (\beta_1 + \gamma_1 D_h) \sum_{t=0}^{11} q_{h,t}\, t + (\beta_2 + \gamma_2 D_h) \sum_{t=0}^{11} q_{h,t}\, t^2 + \delta T_h + \rho D_h + \lambda D_h T_h + \epsilon_h$$

where y_h is income and D_h is the duration of the period between treatment onset and measurement, and other variables are as in Equation 3. Standard errors are clustered at the village level (the level at which treatment (and timing) was assigned). Evaluated at $\overline{D} = 12.77$, the mean duration between treatment onset and measurement from the preferences study, the estimates imply total effects of transfer timing as follows:

$$\hat{\beta}_1 + \hat{\gamma}_1 \overline{D} = 70.859(\text{se} = 41.499)$$
$$\hat{\beta}_2 + \hat{\gamma}_2 \overline{D} = -4.056(\text{se} = 2.566)$$

which can be compared with those from the preferences study itself in Table 2.

Table A.18: Impact of deferral: linear specification

	Deliberation	Social input	Goal progress	Income	Assets + expenditures	Retrospective valuation
	(1)	(2)	(3)	(4)	(5)	(6)
Timing ($\sum_t q_{h,t} t$)	0.012	-0.00085	-0.0043	-10.3	34.5	4.93
	(0.0084)	(0.0033)	(0.0034)	(17.2)	(60.3)	(58.4)
N	424	424	424	393	424	417
Dependent variable mean	-0.00	0.60	0.81	1,257	4,576	2,480

Notes: This table presents estimated effects of deferral on the outcomes of the preferences study presented in Table 2 and Table 3 using a linear specification of deferral. The specification used to estimate effects on income includes a control for baseline income as in Table 2 (not reported). Results are estimated for the 90% sub-sample for which timing was assigned randomly. 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.19: 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)
Timing ($\sum_t q_{h,t}t$)	0.18	0.13	0.14	0.12	-0.086	-0.12
	(0.087)	(0.081)	(0.09)	(0.07)	(0.067)	(0.078)
Timing 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 estimated effects of deferral on standardized components of the Anderson deliberation index in the preferences study. Results are estimated for the 90% sub-sample for which timing was assigned randomly. The specification are based on Equation 2 with controls included for preferences (preference for any deferral, and preference for one as opposed to two tranches). Each outcome is a participant response to a particular statement on a scale from 1 = strongly disagree to 5 = strongly agree. In Column 1 the statement is that the participant "looked for information on how to best use the money". In Column 2 it is that the participant "asked other people for advice on how to use the money". In Column 3 it is, "When deciding how to use this money, thought very carefully about it." In Column 4 it is, "When deciding how to use this money, thought about a specific goal.' In Column 5 it is, "Thought a lot about how to use the money, even before receiving the first transfer." In Column 6 it is, "Made a quick decision on how to spend the money". Heteroskedasticity-robust standard errors are in parentheses.

Table A.20: Effects by definition of timing in EHMNW

	Time since Feb 2015	Time since most recent February			Time since most recent February First tranche effects by income source		
	(1) All tranches	(2) All tranches	(3) First tranche	(4) Second tranche	(5) Non-farm self-emp.	(6) Farm self-emp.	(7) Wage labor
Timing ($\sum_t q_{h,t}t$)	58.20	47.02	33.47	-22.87	2.86	-3.19	39.73
	(15.15)	(74.12)	(18.95)	(24.01)	(12.89)	(5.99)	(13.94)
Timing squared ($\sum_t q_{h,t}t^2$)	-4.16	-5.62	-3.28	1.94	0.10	-0.12	-3.73
	(1.11)	(7.19)	(1.78)	(2.03)	(1.27)	(0.51)	(1.28)
Treatment	-50.43	46.16	9.53	104.30	-6.19	31.85	-27.86
	(52.14)	(112.16)	(52.45)	(50.34)	(31.66)	(15.38)	(42.46)
Endline date	23.87	20.86	22.32	21.83	5.07	-2.08	17.69
	(3.51)	(3.07)	(3.32)	(3.34)	(1.57)	(0.79)	(2.66)
Baseline income	0.41	0.41	0.40	0.41			
	(0.14)	(0.14)	(0.14)	(0.14)			
N	8,156	8,156	8,239	8,156	8,151	8,231	8,239
Dependent variable mean	617.15	617.15	616.61	617.15	182.01	118.44	318.22

Notes: This table estimates Equation 3 using alternative definitions of transfer timing. Columns 1–4 use total income at endline as the outcome. Column 1 measures time since February 2015 (our baseline specification); columns 2–4 measure time since the most recent February, using all tranches (column 2), the first tranche (column 3), and the second tranche (column 4). Columns 5–7 use first tranche timing since the most recent February and disaggregate income by source: profits from non-agricultural enterprises (column 5), profits from agricultural enterprises (column 6), and wage earnings (column 7), all measured over the last 12 months. Standard errors clustered at the village level are in parentheses.

Table A.21: Effects by income source in EHMNW

	Non-farm self-employment	Farm self-employment	Wage labor
	(1)	(2)	(3)
Timing ($\sum_t q_{h,t}t$)	6.97	8.22	42.03
	(7.73)	(4.01)	(12.43)
Timing squared ($\sum_t q_{h,t}t^2$)	-0.13	-0.71	-2.91
	(0.58)	(0.28)	(0.96)
Endline survey date	6.89	-1.98	17.85
	(1.71)	(0.84)	(2.85)
Treated	-20.32	1.75	-46.85
	(29.34)	(13.85)	(39.49)
N	8,069	8,148	8,156
Dependent variable mean	182.5	118.6	318.2

Notes: This table presents estimated effects of cash transfer timing on endline income by source in the EHMNW study, estimated using Equation 3 with controls for the date of the endline survey and assignment to treatment. The outcome variables are profits from non-agricultural enterprises (Column 1), profits from agricultural enterprise (Column 2), and wage earnings (Column 3), all for the last 12 months. Standard errors clustered at the village level are in parentheses.

Table A.22: Income effects by sector of primary occupation

	Non-farm self-employment	Farm self-employment	Wage labor
	(1)	(2)	(3)
Timing ($\sum_t q_{h,t}t$)	322.9	145.3	-24.3
	(121.9)	(97.1)	(121.3)
Timing squared ($\sum_t q_{h,t}t^2$)	-30.6	-12.4	1.5
	(11.4)	(7.7)	(10.4)
N	105	175	108
Dependent variable mean	1,622	1,113	1,162

Notes: This table presents estimated effects of deferral on income in the preferences study, splitting the sample based on respondents' reported primary occupation as of the endline survey (see Table A.23 for estimated effects on primary occupation itself). Effects were estimated using Equation 2 including controls for preferences (preference for any deferral, and preference for one as opposed to two tranches) and for baseline income. The sample includes the 90% sub-sample for which timing was assigned randomly, but excluding 5 respondents whose primary income at endline was transfers from other, migrant household members. 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.23: Impact of deferral on sector of endline occupation

	Non-farm self-employment	Farm self-employment	Wage labor
	(1)	(2)	(3)
Timing ($\sum_t q_{h,t} t$)	-0.013	-0.015	0.020
	(0.032)	(0.035)	(0.033)
Timing squared ($\sum_t q_{h,t} t^2$)	0.000	0.002	-0.002
	(0.003)	(0.003)	(0.003)
N	423	423	423
Dependent variable mean	0.272	0.440	0.267

Notes: This table presents estimated effects of deferral on source of primary income at endline in the preferences study. Effects were estimated using Equation 2 including controls for preferences (preference for any deferral, and preference for one as opposed to two tranches) and fixed effects for the respondent's sector of primary occupation at baseline. The sample includes the 90% sub-sample for which timing was assigned randomly. In column (1), the outcome is an indicator equal to 1 if the respondent's primary endline occupation was a non-agricultural enterprise; in column (2), if it was farming or fishing; and in column (3), if it was casual or salaried labor. We do not report results on whether the primary source of income was transfers from other migrating household members as this was the primary source of income for only 5 respondents. The p-values for an F-test of joint significance of the deferral test are 0.59 for non-farm self-employment, 0.44 for farming and 0.54 for wage labor. Heteroskedasticity-robust standard errors are in parentheses.

Table A.24: Lind and Mehlum (2010) test results

	(1)	(2)
Timing maximizing income	5.305 [3.688, 6.922]	5.087 [3.606, 6.568]
Maximized income	1367.940 [1194.081, 1541.798]	1402.170 [1231.321, 1573.019]
S_L (slope at lower bound)	110.246	121.498
S_H (slope at upper bound)	-118.346	-141.224
p-value (Lind–Mehlum test)	0.047	0.028

Notes: The table reports results from tests for a non-inverse-U-shaped relationship between treatment effects on income, and transfer timing. The tests are those proposed by Lind and Mehlum (2010), based on the reduced form of Equation 2. Column (1) shows the results without controlling for baseline income, while Column (2) shows results controlling for it. The null hypothesis is that the relationship is not inverse U-shaped over the range of the data (i.e., it is monotone or convex). The alternative hypothesis is that the function is inverse U-shaped (concave with an interior maximum). The p-values reported in the last row of the table report the results of the test. S_L refers to the slope estimated at the lower bound of timing values, and S_H refers to the slope evaluated at the upper bound of timing values in the data. Timing maximizing income refers to the timing maximizing income gains, and maximized income is income evaluated at this timing, at average values of covariates. Confidence intervals for the income estimate are in square brackets.

Table A.25: Impact of assignment to receive preferred transfer structure

	Tranching		Timing	
	Income	Assets + Expenditures	Income	Assets + Expenditures
Assigned to receive preferred structure	-83.38	-428.8	37.05	-252.8
	(161.0)	(401.9)	(148.6)	(374.8)
Prefer deferral	50.78	89.68	48.51	77.21
	(117.3)	(427.2)	(116.7)	(425.5)
Prefer 1 tranche	115.8	229.5	117.0	228.7
	(103.4)	(311.2)	(104.2)	(312.2)
Baseline income	0.100		0.0997	
	(0.130)		(0.129)	
N	437	474	437	474

Notes: The table reports the estimated effects of being assigned to receive the transfer in the structure preferred by the respondent on the tranching dimension (Columns 1,2) and the timing dimension (Columns 3,4). In the Tranching panel, "Assigned to preferred structure" is an indicator equal to one if the respondent was assigned their preferred structure among one or two tranches). In the Timing panel, the regressor takes value one if the respondent was assigned a transfer start date in their preferred month. The regressions includes controls for preferences (preference for any deferral and preference for one rather than two tranches). Columns 1 and 3 additionally controls for baseline income. Heteroskedasticity-robust standard errors are reported in parentheses.

Table A.26: 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.27: Effect of token transfer timing on deferral preferences, by tranching

	Preferred 1 tranche		Preferred 2 tranches	
	If getting 1 (1)	If getting 2 (2)	If getting 1 (3)	If getting 2 (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.28: 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 in 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.2 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.29: 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.

B Variable definitions

B.1 Preference elicitation

Both tranching and timing preference elicitations were incentivized, in the sense that both impacted the (probability distribution over) transfer structures that participants received, and that enumerators were instructed to explain this. The wording of the prompts made this connection explicit for the first choice (tranching), however, and left it implicit for the second. Specifically, the prompt wording for tranching preferences was as follows:

> You'll be getting 84,000 KSH over the next 12 months. GD will implement 2 or more of the following 4 options for participants of this study.
>
> - 1 transfer of 84,000
> - 2 transfers of 42,000 each
> - 4 transfers of 21,000 each
> - 12 transfers of 7,000 each
>
> You have a 55% chance of getting your most preferred choice from the final list of options. It's in your interest to think carefully and rank the 4 options in order from the one that would work best to the one that would work worst. What would be the best way to receive the 84,000 KSH? If you cannot receive it as [X], what's the next best way?

Enumerators provided the diagram reproduced below as Figure B.1 to illustrate these options.

The question wording for tranching preferences was

> We would also like to know which months you would like to receive the transfer. We imagine that there are some times it would be more important for you to have this money than others.
>
> - Suppose you were going to receive the 84,000 KSH as 1 transfer all together. Which month between February 2015 and January 2016 would be the best to receive the money?
> - Now suppose you were going to receive the 84,000 KSH as 2 transfers of 42,000 KSH. The second transfer would come 6 months after the first transfer. [Show respondent transfer diagram 3]. Which option would be the best way to receive the transfers?
> - Now suppose you were going to receive the 84,000 KSH as 4 transfers of 21,000 KSH. Each transfer would come 3 months after the previous transfer. [Show respondent transfer diagram 4]. Which option would be the best way to receive the 4 transfers?
>
> Enumerators provided the diagrams reproduced below as Figure B.2 to illustrate these options.

Figure B.1: Visual tool for tranching preference elicitation

TRANSFER DIAGRAM 1

1 transfer	2 transfers	4 transfers	12 transfers
84,000/=	42,000/= 42,000/=	21,000/= 21,000/= 21,000/= 21,000/=	7,000/= 7,000/= 7,000/= 7,000/= 7,000/= 7,000/= 7,000/= 7,000/= 7,000/= 7,000/= 7,000/= 7,000/=
84,000/=	**84,000/=**	**84,000/=**	**84,000/=**

Figure B.2: Visual tools for timing preference elicitation

TRANSFER DIAGRAM 2

(1 Transfer of 84,000/=)

2015											2016
Dwe mar 2	Dwe mar 3	Dwe mar 4	Dwe mar 5	Dwe mar 6	Dwe mar 7	Dwe mar 8	Dwe mar 9	Dwe mar 10	Dwe mar 11	Dwe mar 12	Dwe mar 1
Feb	**Mar**	**Apr**	**May**	**Jun**	**Jul**	**Aug**	**Sep**	**Oct**	**Nov**	**Dec**	**Jan**
Mwezi wa 2	*Mwezi wa 3*	*Mwezi wa 4*	*Mwezi wa 5*	*Mwezi wa 6*	*Mwezi wa 7*	*Mwezi wa 8*	*Mwezi wa 9*	*Mwezi wa 10*	*Mwezi wa 11*	*Mwezi wa 12*	*Mwezi wa 1*

TRANSFER DIAGRAM 3

(2 Transfers of 42,000/=)

	2015											**2016**
	Dwe mar 2	**Dwe mar 3**	**Dwe mar 4**	**Dwe mar 5**	**Dwe mar 6**	**Dwe mar 7**	**Dwe mar 8**	**Dwe mar 9**	**Dwe mar 10**	**Dwe mar 11**	**Dwe mar 12**	**Dwe mar 1**
A	X						X					
B		X						X				
C			X						X			
D				X						X		
E					X						X	
F						X						X

TRANSFER DIAGRAM 4

(4 Transfers of 21,000/=)

	2015											**2016**
	Dwe mar 2	**Dwe mar 3**	**Dwe mar 4**	**Dwe mar 5**	**Dwe mar 6**	**Dwe mar 7**	**Dwe mar 8**	**Dwe mar 9**	**Dwe mar 10**	**Dwe mar 11**	**Dwe mar 12**	**Dwe mar 1**
A	X			X			X			X		
B		X			X			X			X	
C			X			X			X			X

B.2 Variable construction and 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.