Universal Basic Income: Short-Term Results from a Long-Term Experiment in Kenya*

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Abstract

What would be the consequences of a long-term commitment to provide everyone enough money to meet their basic needs? We examine this hotly debated issue in the context of a unique field experiment in rural Kenya. Communities receiving UBI experienced substantial economic expansion—more enterprises, higher revenues, costs, and net revenues—and structural shifts, with the expansion concentrated in the non-agricultural sector. Labor supply did not change overall, but shifted out of wage employment and towards self-employment. We also compare the effects to those of shorter-term transfers delivered either as a stream of small payments or a large lump sum. The lump sums had similar, if not larger, economic impacts, while the short-term transfers had noticeably smaller effects, despite having delivered the same amount of capital to date. These results are consistent with a simple model of forward-looking lumpy investment, and more generally with a role for savings constraints, credit constraints, and some degree of (locally) increasing returns, among other factors.

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1 Introduction

Any discussion of the redesign of the welfare system these days very quickly leads to questions about Universal Basic Income (UBI), the idea that every adult who wants it should be entitled to a minimum weekly or monthly income from the government without having to do anything to "earn" it. This represents both a philosophical departure from the narrative typically used to justify safety net programs, i.e. that only those unable for some reason to support themselves should receive help (Ferguson, 2015) but also a different perspective on the economic costs and benefits.

The economic case for UBI is partly based on the premise that targeting transfers is costly. This is partly because the process of being identified for the transfer often imposes both time costs (e.g. queuing to apply) and psychological costs (e.g. loss of dignity) on the recipients. These costs also discourage applicants (including some of the most needy) and, combined with errors in the targeting process, often end up depriving precisely those whom society most wants to benefit.[1]

However (obviously), the case also depends on the economic benefits from the transfers. In particular, even though economists resist the idea that there are better and worse uses of the money, policy makers are often less comfortable with encouraging beneficiaries to cut back on work or spend more on, say, alcohol or tobacco, either for paternalistic reasons or based on behavioral considerations.[2] This often leads to a focus on the increase in spending on desirable forms of consumption (food, schooling etc.) as a measure of benefits to society.[3]

One could, in principle, be even more demanding. One could ask whether the transfer raises the incomes of the recipients by at least as much as the amount of the transfer. That a transfer will lead to increased earnings is by no means obvious—indeed there is a long tradition in economics of worrying that "easy money" may make people lazy, though there is now lot of recent evidence from developing countries pushing back against this view (Banerjee et al., 2017). The more recent literature from developing countries suggests, instead, a number of different reasons why incomes might go up in response to transfers. Some of the theories are psychological—the security of an assured income might relieve mental pressures and make the recipients more able to focus on the job. Or it may make them less depressed and more able to do productive work. Other theories are based on the idea that there

[1] Hanna and Olken (2018) and Banerjee et al. (2019) discuss these costs in more detail. Hanna and Olken (2018) provide quantitative estimates of the welfare gains from targeting v.s. universal approaches in Indonesia and Peru, where they assume that the only cost come from mis-targeting and the physical machinery of targeting (the psychological costs are not counted.

[2] Leisure, for example, may be over-valued by the decisive short-run self relative to what the long-run self might want

[3] This might partly reflect the elite concern with workers being productive

are many unexploited opportunities that result from financial market imperfections. For example, if investments generate a risky but attractive income stream, many poor people will avoid making them because they are unable to take on the risk. The guarantee of an income reduces their risk aversion and therefore encourage them to take on the project. It could also be that the individual lacks the funds to make an investment that could be quite lucrative, and is unable to borrow enough.[4] Perhaps they can pay for the investment either by saving up the UBI payments or borrowing against them. If the income gains from the investment are large enough, the extra earnings could finance further investments and even make the income gain sustainable, the transfer might even help the household to escape from a poverty trap. Moreover, UBI might have positive spillovers on income generation if there is excess capacity in the economy to start with, acting as a demand shock which can lead to higher earnings for the usual Keynesian reasons. Recent evidence from more narrowly targeted transfers in Kenya (Egger et al., forthcoming) and Brazil (Gerard et al., 2021) is consistent with this idea.

The focus on whether UBI leads to an increase in earnings is especially important for studies from the developing world because these countries often face a rather cruel dilemma. On one side they lack the data and the capacity to do effective targeting, which argues for a more or less universal transfer. On the other hand, they face tight fiscal constraints which makes it very costly to transfer large amounts to a large part of the population. The promise of a durable impact on earnings from the transfer is particularly important in these settings because that might partially relieve this tension by allowing some beneficiaries to lift themselves to the point where they voluntarily give up the transfers (especially if it requires some effort to , pay taxes into the system, and/or become employers for other beneficiaries). Perhaps eventually this might even allow for the phasing out of the UBI.

Set against this backdrop, the goal of this paper is two-fold: (a) assess the impact of a *long-term* UBI intervention committed to last for 12 years, run by the NGO GiveDirectly (henceforth GD) in two counties in Kenya, on measures of well-being, labor supply, occupations as well as earnings (with the idea that at least in the first years 12 years feel like forever) and (b) compare this long-term UBI to two other related but less expensive interventions also implemented by GD on the same set of outcomes: a *short-term* version of the UBI where the payments are exactly the same as in the long-term UBI but stop after 2 years and a *lump sum* program where the household gets a single payment equal in present value to that of the short-term UBI.

The motivation for the first question is obvious. However, the second main plank of this

[4] de Mel et al. (2012) and Fafchamps et al. (2014) find evidence that a single lump sum transfer to owners of tiny businesses have very high returns, consistent with the idea that they are credit constrained.

paper is also very important, both because it helps us identify potential practical alternatives to UBI, but equally importantly, because it helps us understand the mechanisms that may be at play with UBI and makes it possible to design better alternatives to it, which do not have to be the specific ones we consider.

Specifically, the motivation for the lump-sum intervention came from the idea that if the income gains in UBI could come from financing a lumpy investment that the typical household cannot afford, then the same outcome may be achieved more efficiently and cheaply by directly giving the household a one-time lump sum amount that covers the cost of that investment, so that they don't have to go through the process of trying to save up the needed amount out of their small regular transfers.[5] Consistent with this idea, households in an earlier GD program who were given the *choice* between lump sums and streams of smaller transfers with the same present value overwhelming chose the former (Kansikas et al., 2022).

The motivation for the short-term UBI intervention is similar. It is possible that even a short-term relaxation of economic pressure has positive productivity effects through the various financial and psychological mechanisms already discussed. If earnings go up as a result, and that in turn also further relaxes economic pressures, the household may even become permanently more productive.[6] Of course, the same trade off between a more permanent intervention and more temporary one would arise even if the impact of these short term interventions on earnings and other welfare outcomes lasted for some time eventually faded away.

On the other hand, the fact that the full UBI intervention, at least in the early years, may be perceived as almost permanent, means that its economic and psychological impacts could be quite different from any of the transitory interventions—for one, it may be much easier to borrow against such transfers, or to feel less insecure about possible economic shocks. Moreover, the fact that it lasts for much longer, creates option value—beneficiaries can afford to wait till they see an opportunity that matches their skills or their preferences before acting: for some the money may finance migration, while for others it might pay for a place in a good high school for a child or a machine that they learn about.[7] Under the short-term UBI or the lump sum, it might be too hard to hold onto the funds, and therefore households may take preemptive action. For all of these reasons, it is important to know

[5] Of course, the lump sum intervention can only be cheaper by giving households less. Whether or not that is desirable depends on the fiscal constraints on the government and its social objectives.

[6] For example, since risk is often highest when the business is being set up, even a short run intervention that mitigates earnings risk can encourage starting a new business.

[7] See Adhikari and Gentilini (2018) and Bastagli et al. (2019) for reviews of the evidence on effects of other cash transfer programs on migration and education, respectively.

the time path of the impacts from all three interventions, as well as the relative sizes of the impacts.

However, setting up a proper UBI experiment to answer these questions is challenging for a number of reasons. First, the fact that it has to be universal, which we take as given, means that the program has to cover every adult in the targeted areas, which in our case are villages in Kenya with, on average, 52 households and 118 eligible adults. Covering every adult (as opposed to every household) is unusual, and potentially important since a household transfer may mean some adults need to remain in the same household in order to continue to benefit. Second, we want to capture the sense of a "permanent" entitlement which means that there needs to be a credible commitment to continue the program for long enough, which we decided would be 12 years. Third, we wanted to be powered to be able to evaluate the impact of the two other variants mentioned above, and ideally to be able to detect economically meaningful differences in their impacts.

All of this implies that the scale of the experiment in both space and time needed to be quite large. For one, any valid experiment would need to cover the cost of UBI payments for a long period of time for a large population. Moreover, the funders need to be patient enough to accept the fact that the results will take several and, in some cases, many years to arrive. In addition, the data collection challenge is non-trivial. The program potentially has impacts on a very wide range of outcomes stretching from business creation, to labor supply, to consumption patterns, to asset purchases, to mental health. To deal with the risk of picking and choosing outcomes, we pre-registered 10 sets of (mostly) aggregated outcomes, but there are obviously many more outcomes of interest. Moreover, the fact that it is a universal program and covers the entire village, means that there are likely to be large equilibrium effects from the program: for example, prices may go up and this needs to be accounted for carefully in assessing the interventions. Wages might go up as well, which helps some and hurts others. School fees may go up, new schools might open, new businesses may start, new public investments may be undertaken, there may be crowd in or crowd of NGOs other than GD. All of those need to be accounted for. Finally, to the extent that some of our beneficiaries were much more advantaged than others to start with (unlike in a more targeted intervention), we expect that the impacts to be quite heterogeneous.

These are, no doubt, some of the reasons why this is the first experiment of its kind—there are other small pilot-style experiments but they offer neither the necessary scale nor the promise of durability that are essential in answering this set of questions. Appendix A provides a brief survey of the relevant literature.

At this point we are able to report the results from a set of surveys conducted between October 2019 and January 2020. At this time, on average, the lump sum, long term and short term arms had received, respectively, 92%, 87% and 92% of the total two year amount. This coincides with the period just before the end of the 2 year intervention and almost 2 years after the lumpsum was paid out (in 2 tranches)—in other words, at a point when adults in all three arms had received, on average, approximately the same amount of money to date, but only those in the long-term arm expected to receive more in the future.

We begin with the results of the long-term UBI treatment (LT). We see that the intervention was implemented as planned and that while there were some (mostly insignificant) effects on formal and informal taxes and transfers, none of them were big enough to alter the main story, which is that the amount of total transfers received in the village was about 15% of the total spending in control villages.

Starting with the village level aggregates using data from a census of enterprises we carried out, we find that both revenues and costs go up significantly in LT, and the difference between them, which we call net revenues (i.e. profits if we do not count the cost of household labor), go up by slightly more than 50%.

We do not reject the null that consumer prices in nearby markets were unchanged, both for agricultural and non-agricultural products, though to be fair these estimates are not precise enough to rule out meaningful appreciation (or depreciation) given the design and scale of the experiment.

Almost all of the economic change we see comes from non-agricultural enterprises—net revenues from these enterprises nearly double and this effect is highly significant. This expansion is associated with the acquisition of substantially more inventories and business assets and the creation of new enterprises. The number of non-agricultural enterprises goes up by 25% and that effect is significant. Within the non-agricultural sector almost the entire effect comes from an expansion in the retail sector with positive, but far from significant, effects on transport and manufacturing. Consistent with this, there is a doubling of the number of items sold within 2 km of the village, an effect which just fails to be significant (with a p-value of 0.114). Overall the picture is of an expanding distribution system, potentially financed in part by the transfers while also meeting the increase in local demand resulting from them. (The geography of expenditure shifts in parallel, as households spend a larger share of their budget within their own village.)

There is also an increase in the number of agricultural enterprises and their net revenues—but it is much smaller in proportional terms and not significant. There is also a 35% increase in the value of agricultural assets, which is statistically significant. A part of this seems to be an increase in the price of land (without any change in the actual amount of land owned) but there are increases in the value of both big and small livestock. The fact that sales are going up and new enterprises are getting set up suggests that we should observe a corresponding shift in labor supply: there is a significant reduction in hours of wage work, all of which comes from work in agriculture, and a slightly larger increase in hours of non-agricultural self-employed work, so there is no net effect on total household labor supply. Overall there is no evidence of UBI promoting "laziness," but evidence of substantial effects on occupational choice.

We also find no effect on equilibrium wage employment, suggesting that wage workers from outside the village took up the slack. This should imply that wages went up, and indeed both reported wages in agriculture and construction (the representative sector used for non-agriculture) as well as reservation wages to work on a public works project increased.

As mentioned already, there is disagreement about the social valuation of the benefits accruing to individuals and families. We therefore opt to remain agnostic, and report on a wide range of welfare-related outcomes from a household survey. LT causes a small (5%) increase in total consumption, almost of it in the form of extra food. It also meaningfully and significantly raised both protein consumption and food variety at almost all points in the distribution. The effect on protein consumption is close to a 25% increase at the bottom of the distribution and closer to 15% near the top of the distribution.

The effect of LT on education spending is positive but not significant, while that on health spending is negative but also not significant. There is a significant improvement in measures of well-being and life satisfaction and a substantial reduction in reports of depression, these last effects significantly smaller for individuals in the LS arm.

Turning to our second set of questions, when we compare the impacts of the lump sum (LS) arm to the LT, the most striking result is that the two have very similar impacts on village aggregates. The impacts on revenues, costs and net revenues, as well as the number of enterprises is substantially higher under LS, while the impact on assets is somewhat lower, but none of these differences are significant. On labor supply, like in LT, self-employment substitutes for wage-employment, but the overall effect, while positive, is again indistinguishable from zero.

The short-term (ST) arm has smaller impacts on all of the aggregates, and in the case of the number of enterprises, total revenues and total costs the difference with LS is highly significant and the difference with LT are borderline significant (p values under 0.15). The impact on net revenue is also at the border of significance in the case of LS ($p = 0.14$) but not in the case of LT. In terms of labor supply, the effects with ST are actually very similar to that of LT, and the net effect is again zero.

When we turn to consumption, it should be noted that for credit and savings constrained households, higher consumption in the short run does not necessarily mean they are better off. We find that the effects on consumption (of non-durables) are actually larger in the LS and ST arms than in LT, but the differences though large, are not statistically significant. Food variety and protein consumption go up in all three arms, and the only significant difference is that there is more food variety in the ST than in LS. In terms of consumer durables/household assets, both ST and LS have more positive effects than LT and the difference between LT and ST is actually significant.

As for wealth overall, productive assets for both agriculture and non-agricultural uses increase substantially, though the former changes are significant in all arms while the latter are less precisely estimated. Financial assets increase modestly, with significant increases in liquid savings and no significant changes in debt. Taken together with the increases in consumer durables, these imply total accumulation of non-land assets of of between \$580 and \$1150 (depending on the arm), relative to the approximately \$3000 of UBI transferred. The value of landholdings, as assessed by respondents, increased even more—in fact the effects are larger than the amount of money households had received, even after trimming the top percentile of landholdings. This is driven entirely by appreciation of existing landholdings, as opposed to land acquisition, and as we discuss below the interpretation of that appreciation is subtle—some of it can be explained by investment in structures and other upgrades, but some of it likely reflects a belief that the same land could command a higher price, possibly because treated villages had become more desirable places to live.

Both interventions have a positive and significant effect on education spending but we cannot reject that all three interventions have similar effects. In terms of education outcomes, the fraction of children getting a score of more than 250 in the KCPE public exam improves significantly only in LS and the effect is significantly greater than that of LT. While ST has a negative but not significant effect on health expenditures, LS has a positive and significant one, and the latter is significantly larger than the effect of LT. In terms of health outcomes, a child anthropometric index that we pre-specified improves with ST which also has the biggest effect on food consumption. The other two interventions have no detectable impact on this outcome, and LT effect is clearly smaller than the ST.

Finally, all the interventions cause significant improvements in mental health, though the impacts of the LS are (significantly) smaller than the LT and smaller than the ST as well. This may be the case because the LS households have taken on the risk of their investments and know they have no further cushion should those investments not work out. It also could be because both the LT and ST arms have received payments in the last month, unlike the LS, and there are salience effects and the LT arm knows these payments will continue for an additional decade.

Taken together these results seem to be consistent with a simple model of a *saving and credit constrained* individual who is forward-looking. Some of them have access to a business opportunity which requires an initial sunk lump sum investment but then offers high marginal returns. This very simple model, developed in the framework section, has the prediction that at the same initial wealth level "high-type" LT and LS households are more likely make the lump-sum sunk investment than the ST households. This is because the LS households can make the sunk investment sooner and therefore enjoy the high returns longer than the ST households, while the LT households, have a greater incentive to invest in it even when they have exactly the same income streams as the ST (the first two years) because they have longer stream of high incomes to invest in the future. Since each LS and LT have different reasons to be more pro-investment, the comparison of the two can go either way.

The flip side of this is that those who have no access to the high-return business option or know that they are unable/unwilling to make that investment, will have a problem smoothing consumption. For this reason the STs may be more inclined to invest in non-business projects such as education, agriculture, household assets and nutrition as well as consuming more than they would like to, compared to the LT and especially the LS.

While we do not claim that this is the unique way to explain our findings, we argue that in any explanation, savings and credit constraints and some degree of (local) increasing returns must play a role. And supporting the fact that in our theory that the reason why the LT behave differently from the ST is that they find it worthwhile to save up for a bigger project, we see enormously higher ROSCA participation from the LT compared to the LS (who don't need to save as much) and ST (who may not want to save).

2 Setting and Research Design

In this section we describe the context, experimental design, and data collection, and report some basic checks of experimental integrity.

2.1 Context

Our study is set in rural Kenya in Bomet and Siaya counties, two of the poorer counties in the country. In our baseline survey, for example, 85% of households reported having experienced hunger at least once in the preceding year, and maize consumption (typically approximately 40% of total consumption) was $0.60 (in PPP terms) per capita per day. Households work primarily in agriculture—73% had a farm enterprise, and 21% had a non-farm enterprise—and own 1.7 acres of land on average. 86% had a phone, but only 13% a bank account (including digital bank accounts).

While quite rural, the villages sampled for our study are also reasonably close to centers of commerce. The average village has 3 markets within 5km (a one-hour walk), and most are fairly close to the county capital. Figure 2 maps both counties, highlighting both the study villages and the surrounding markets (which were included in our data collection).

Transfers were implemented by GiveDirectly, a multinational NGO specializing in digital cash transfers, typically (in Africa) via mobile money. Kenya has an extremely well developed mobile money systems that are widely used and that therefore makes digital cash transfers relatively cheap and easy, even to very poor households. As early as 2014 an estimated 96% of Kenyan households had a mobile money account (Suri and Jack, 2016), the majority of which were M-PESA accounts on the Safaricom network. All the households in our study area have easy access to one or more mobile money agents where they can withdraw money from their accounts.[8]

2.2 Experimental Design

To sample villages for the study, we first selected two sub-counties in each of Siaya and Bomet. In Siaya, we picked two sub-counties where GD had (largely) not worked before. GD has worked in Siaya for many years with a number of studies examining their programming there, including Haushofer and Shapiro (2016), Egger et al. (forthcoming), Orkin et al. (2022), and Kansikas et al. (2022). In Bomet (where GD had not previously worked) we choose one sub-county in which GD had conducted a pilot to test operations in a new county, and a second on the opposite side of the county capital, Bomet Town (see Figure 2).

Within these four sub-counties we listed (and conducted a census in) all villages, which amounted to 390 villages. Of these, we restricted ourselves to villages containing between 30 and 100 households in Siaya and between 30 and 73 households in Bomet, for a total of 325 villages. We focused on these relatively small villages to improve power given a fixed budget for transfers: with treatment assigned at the village level, the cost per treated unit is proportional to (and hence the size of treated sample is inversely proportional to) the size of the average treated village. We conducted a census between April and June of 2017 of all 325 villages meeting this criterion, and then randomly selected 295 to be part of our study. Overall, these sampled villages contained 15,189 households and 73,511 individuals as of our census, with an average village population of 249 people, of whom 23,082 were transfer eligible adults.

We randomized these 295 study villages to the following experimental groups:

- **Control**: 100 villages (approximately 11,000 people) received no transfers.

[8]As of 2014 the median distance from a study village to the nearest M-PESA agents was 3km, and this has likely fallen since then as the overall number of mobile money agents has grown substantially.

- **Long-term** UBI: in 44 villages (approximately 5,000 people) each adult over the age of 18 receives US $0.75 nominal per day, or $1.88 PPP, delivered in monthly installments for 12 years. We calculated this amount as sufficient to cover the most basic needs, such as basic staple food consumption, and perhaps some health and education. In addition, teenagers aged 15-17 in long-term villages were told that they would begin receiving transfers upon turning 18.
- **Short-term** UBI: in 80 villages (approximately 8,800 people) each adult over the age of 18 receives transfers as in the long-term arm, but for only 2 years.
- **Lump-sum** cash transfer: in 71 villages (approximately 8800 people) each adult over the age of 18 received a one-time payment of about US $500 nominal, or $1250 PPP. (In practice GD delivered this amount in two equally sized installments two months apart, in order to fit within M-PESA transaction limits.) The lump-sum transfers was thus the equivalent in net present value terms of the short-term transfers discounted at an annual rate of 8%.

Randomization was stratified by location, a geographic unit which (in our data) contains an average of 14 villages; in this sense the design was optimized for comparisons between treated and control villages—including spillovers across individuals within those villages—as opposed to potential spillovers into other villages.[9]

To the best of our knowledge this experiment is the largest randomized control trial of UBI to date, both by the number of adults treated and (in the long-term arm) by payment duration. (Table 1 provides a systematic comparison between the major UBI or basic income pilots to date on these and other dimensions.) In terms of statistical power, ex ante calculations based on data from other nearby studies suggested it should be sufficient to detect plausible effects of each arm on outcomes without a long right tail (such as migration and time use outcomes), and potentially sufficient to detect effects on outcomes that were more skewed or exhibited a higher intra-cluster correlation, such as daily per capita consumption. Appendix C provides more detail on these calculations.

The design was *not* expected to be powered for two-sided tests of the null of equality *between* treatment arms, particularly given that, as of this round of surveys, recipients would all have received approximately the same amount of money. At the same time, however, the existing literature on cash transfers (and on the financial lives of household living in or near extreme poverty more generally) strongly suggests one-sided alternative hypotheses to these nulls. In particular, one would expect LT to have impacts at least as great as ST, due to

[9] The design of Egger et al. (forthcoming) is relatively attractive for estimating these spillovers as it includes all villages in the studied region, and varies treatment intensity at the sub-location level. It also includes more than twice as many villages (653) as in this sample.

opportunities to spend in anticipation of and potentially borrow against future transfers. Similarly one would expect LS to have impacts at least as great as ST, as it provided money up front and in a single tranche. (We formalize both of these intuitions in the theoretical framework below.) Given this, we also report below results from tests of the null of equality against these one-sided alternatives.

In addition to the main treatments we also cross-randomized (but do not examine here) two "nudges" within the treatment groups and at the household level. The first was a planning nudge, encouraging the household to plan what they were going to spend the transfers on and informing them that GD would ask what these plans were after payments had started. The second was a savings nudge, reminding them that they had the option to save some of their transfers in an interest-bearing digital bank account called M-Shwari (see Suri et al. (2021) for more on how M-Shwari works) and providing instructions on how to use M-Shwari. We stratified the nudge randomization by village.

It is important to note that all treatments are *individual*: each adult is treated individually and separately. If a woman in the long-term arm were to leave her partner, for example, she would be able to take her remaining transfers with her. In this sense the interventions (and potential effects on household dynamics) were quite different from those in most other cash transfer studies, where the unit of treatment is the household, even though many outcomes will as usual still be measured at the household level. Overall, 23,000 adults were treated across the 195 treatment villages.

We sampled 30 households in each study village to constitute our study sample, which we track to measure impacts. We conducted a baseline survey (discussed in more detail below) of this sample and, after completing each interview, gave every adult in the survey sample a mobile phone in order to facilitate tracking. Both the baseline survey and phone distribution were completed before GD enrolled any individual into the various treatments. GD then separately enrolled adults in treatment villages and implemented all payments. Payments began between January and June 2018, as different villages had been enrolled at different times. Individuals who moved into any of the treatment villages after GD's enrollment were not eligible for transfers.

2.3 Data Collection Activities

The analysis in this paper draws on extensive data on individuals, households, firms, markets, and local government, collected during baseline survey activities in Fall 2017 and follow-up survey activities in Fall 2019–Spring 2020. Figure 1 illustrates the timing of these activities, plotting the densities of the dates on which various surveys were conducted as well as the timing of transfer delivery in the different treatment arms for reference.

Baseline activities included in-person surveys of 8,753 household heads to ask about the household's activities and outcomes; phone surveys of 4,383 spouses covering sensitive topics (e.g. domestic violence); in-person surveys of 295 village elders to gather information on local public goods and labor markets; and surveys of vendors in 105 markets to gather price data.

Follow-up activities included in-person surveys of 8,292 household heads, as well as 407 heads of households that had migrated together outside of their original village and phone surveys of an additional 1,034 individuals who had migrated out of their original village; phone surveys of 6,969 spouses; surveys of 295 village elders; surveys of vendors in 104 markets (where we collected up to 3 price observations each for up to 175 commodities); a census of 9,850 (non-agricultural) enterprises (including all those located in study villages as well as in the markets and shopping centers surrounding these villages); and more detailed surveys of 3,418 enterprises, mapped back to the households that owned them.[10]

Survey response rates were high: we achieved a 97% household response rates in all arms in the main endline survey. In addition, because we separately surveyed individual adult migrants, we are able to add their outcomes back to the totals for the sending household where appropriate (e.g., when calculating total household income).[11]

In addition to these primary data-collection activities, we also conducted a short phone survey in Fall 2022 to gather further detail on a few topics that emerged as important during analysis of the primary data. Specifically, this survey gathered data on land values, investments and transactions; on alcohol use; and on participation in rotating savings and credit associations (ROSCAs).

2.4 Experimental Integrity

We next examine the integrity of the experiment in terms of balance with respect to the initial randomization, attrition from subsequent surveys, and adherence to treatment assignment.

In general, our sample is balanced. Table D.1 reports measures of balance with respect to baseline characteristics; we cannot reject the joint null of no mean difference for any outcome across all treatment arms ($F = 0.04, p = 1.00$).

[10] We also subsequently conducted two short follow-up surveys with household heads by phone during the COVID-19 pandemic, in May/June 2020 and November 2020, focused on a small subset of COVID-related outcomes. We report results from these separately in Banerjee et al. (2020).

[11] Specifically, for individual adult migrants living alone (58.2% of adult migrants) we add back the full values of their income, assets, expenditure, etc. For individual adult migrants who have joined or formed another household (41.8% of adult migrants), we directly elicited the migrants' share where possible (e.g. for assets) and otherwise assign them a proportionate share in the household's outcome. Our results are not sensitive to omitting these migrants entirely.

Attrition from the household survey was low overall, as implied by the high response rates of 97% noted above. The response rate was slightly higher in the lump-sum arm than the control arm (Table D.2), but the composition of attritors was similar across arms, and we cannot reject the joint null of no differences for all arms and all outcomes ($F = 1.18, p = 0.14$; see Table D.3). There was no attrition from the village elder survey, nor (at the market level) from the market price survey. The response rate to the enterprise surveys was, on average, 88%, with slightly higher response rates in the ST and LT arms (between 3-4% higher, see D.2). Attrition rates for the follow-up phone survey were if anything slightly lower, with an overall response rate of 98% and slightly higher response rates in the LS and ST arm relative to control.

Adherence to the experimental assignment was high. Table D.4 reports adherence metrics based on administrative data on transfers issued by GD. We calculate these as of Fall 2019, when the follow up data collection activities were being conducted, and at a time when each treatment arm had should have received approximately the same amount of money in net present values terms. We examine an indicator for whether the household was issued a transfer during the 40 days prior to the date on which we surveyed it, as well as the total quantity issued to the household (in net present value terms) as of the date it was surveyed.

For both of these outcomes the control group mean is exactly zero, implying that no households in control villages were issued any transfers at any point in time. As of the date households were surveyed in Fall 2019, most of those in the short-term and long-term arms had received a transfer in the prior 40 days, and (as expected) none of those in the lump-sum arm had received a transfer in the prior 40 days. The total amounts issued were almost identical across arms, with the differences (attributable to differences across arms in the timing of enrollment) very small.

One other aspect of compliance deserves mention. As mentioned above, GD ran a pilot of the long-term UBI in one Bomet village to test implementation logistics. We unknowingly included this village in our experimental sample, and (randomly) assigned it to the short-term arm. We drop this pilot village in the results reported below, but did collect follow-up data from it, and the results do not change meaningfully if we include it in the short term treatment arm (yielding intent-to-treat estimates).

3 Framework

This section develops and simulates a model to help us interpret patterns of effects on investment and consumption that we will see in our data. We are particularly interested in identifying conditions under which long-term, short-term, and lump-sum transfers lead to different outcomes at the time of our endline surveys, i.e. when the total amounts transferred are similar in each case. The model we examine here is of course not the only one that could generate such differences, but it is illustrative of the kinds of constraints needed, and it makes the point that even a fairly simple model is capable of generating rich patterns akin to those we will see.

Households maximize their lifetime discounted utility:

$$\sum_{t=0}^{T} \delta^t u(c_t) \tag{1}$$

where

$$u = \frac{1 - c^{1-\sigma}}{1 - \sigma} \tag{2}$$

Each household begins with an initial wealth of w_0. Households receive exogenous income as follow: those in the LT condition receive income $\overline{y}$ till period T; those in the ST condition receive income $\overline{y}$ for S periods and then $\underline{y} < \overline{y}$ thereafter; and those in the LS condition receive a lump sum transfer at time 0 that is equivalent to the net present value of the ST transfer, and then $\underline{y}$ in each period thereafter.

In addition to this exogenous income, households can also earn endogenous income from investments they make. All households have access to the same constant-returns savings technology which yields a real rate of return $r < 1$. This captures the idea that saving is difficult in our setting because households do not have access to safe, inflation-protected saving vehicles. In addition, some households (type H) have access to a opportunity which yields a higher return of $R >> r$ on incremental capital if they have previously made a one-time sunk investment of $I > w_0 + \overline{y}$. This captures opportunities such as starting a small business that require a lumpy up-front investment to begin, but subsequently yield a higher return on capital than alternatives. The other type L households do not have access to this opportunity.

Together these assumptions imply the dynamic budget constraint

$$w_{t+1} = R_t(w_t - c_t + y_t) \tag{3}$$

where $R_t = R$ if they have made the sunk investment prior to t and $R_t = r$ otherwise. We assume that there is no credit, so that $w_t \geq 0$.

3.1 Analysis and Results

This very simple model has the following comparative statics implications that we first list and then verbally explain, with a more formal argument in Appendix B.

1. **Endowments & preferences:** Regardless of treatment condition, type H households are more likely to invest at some point if they start with a higher w_0 and if they have a higher δ.
2. **LT vs ST:** Type H households are more likely to invest under LT than under ST, given the same δ and w_0, and might save more before period S. Type L households, on the other hand, should save less before period S under LT than under ST.
3. **LT vs LS:** Type H households may save more or less in early periods under LT than under LS, given the same δ and w_0. Type L households, on the other hand, save less before period S under LT than under ST.

The intuition for these results come mostly from thinking about the incentive to save. There are two reasons to save. One, shared by both types, is to smooth consumption. This is strongest for the LS and then the ST who have temporarily elevated income; the LT have constant flows for the foreseeable future and can therefore consume relatively smoothly by consuming their current income (unless they start with a lot of wealth). Consumption smoothing is why L type STs and LSs have a much stronger incentive to save than L type LTs.

The second reason to save applies only to the H types, who have the option of earning a much higher return on their savings if they make the sunk investment of I. This is relatively easy for the LS, at least if the lump sum they get is large enough, but for LT and ST it is not so easy as they need to save up to get to I, and until they have done so the rate of return on their savings is negative. Because of this they need to save fast. But saving fast requires a steep sacrifice in terms of consumption smoothing—it involves consuming little now to get to high levels of consumption in the future. This is why the discount factor matters, and why it is easier for richer households (who need to save less) to invest.

The value of access to the high returns investment technology is more valuable if the household plans to have large amounts of savings. The problem for the H type ST households is that by the time they have saved up and invested in the sunk technology, their extra income may be about to run out and therefore their ability to save is also about to go down. Therefore they are less likely to want to invest than the corresponding LT households who will have larger income flows into the future and therefore savings to invest in the high return technology. On the other hand, for the same reason, the ST households that do invest might save more than the LT households that do invest because they need to get to make the sunk investment faster to benefit from it. Finally the LS households that do invest have both a good savings technology and a strong reason to smooth consumption, which is a reason for them to save more than the LT. On the other hand they have a higher income in the early period so the savings rate could be higher or lower.

Figures 3–5 illustrate the model dynamics for H types via simulation. We set the parameters that we observe in the design of the experiment or in our data to their actual values: initial wealth at $w_0 = \$1432$ (mean wealth in the control group), non-transfer income at $\underline{y} = \$210$ (mean monthly income in the control group), monthly income including transfer income at $\overline{y} = \$341$ ($\underline{y}$ plus the mean monthly transfer), and the value of the lump sum equal to the net present value (discounted at an annualized rate of 0.08) of all transfers in the short-term arm, i.e. \$3000 per household. We then set the remaining unobserved preference and production parameters as follows: $\delta = 0.999$ (corresponding to an annual discount factor of 0.988), $\sigma = 0.6$; time horizons at $T = 72$ months, the duration of the short-term transfers at $S = 12$, and rates of return at $r = 0.9$ and $R = 1.2$. The figures show the time paths of consumption and wealth for all three treatment groups and the time at which an investment is made (if at all). Note that the wealth variable measures liquid wealth and does not include the sunk investment (I), so that wealth goes down when the household sinks the investment cost.

We vary the remaining parameter, the lumpy investment cost I, to illustrate the consequences this has. In Figure 3 we set $I = \$2900$, at which point the investment is expensive but still feasible for LS recipients to make immediately upon receiving their transfers. We see that in this scenario the H-type LS recipients do indeed invest, and that LT recipients do so as well, though it takes the latter group longer to accumulate enough liquid wealth do so. H-type ST recipients, on the other hand, do not invest. Because of this their consumption is higher than that of the LT's during the early periods of the model—matching what we observe in the data—but lower in the later in the periods. Meanwhile the fact that the LS can save at a high return from the beginning gives them a marked advantage in terms of long-term consumption.

In Figure 4 we lower the cost of the investment to $I = \$1450$, and here in contrast all groups invest (and do so immediately, in period 1). This illustrates the role of lumpiness in the model: the minimum scale of the capital investment required must be large enough relative to the size of the transfers to make saving up to cover it attractive for the LT group but not for the ST group. The corollary to this point is that sufficiently large transfer streams would also enable all groups to invest; we illustrate this in Figure 5, where we revert to the larger value $I = \$2900$ but counterfactually set the transfers to be double their actual size. Here once again the H-types in all the groups invest, though the ST and LT groups take a bit longer to accumulate the capital required to do so.

Comparing these scenarios, we see that H types may choose to invest under all three treatments, but only if resources are plentiful enough. If not, those in ST will choose not to invest for the reasons discussed above. These reasons are of course not the only ones that could lead to greater investment and earning in the LT than the ST arm. To the extent that households in the LT arm can borrow against their future transfers (something we will explore in the results below), it will be easier for them to invest in the early periods. LT households might be more willing to take on risk, knowing that they are assured of a bare minimum income in the future. And LT households should anticipate that their neighbors will have more to spend in the future, which might make sinking an investing today—in a small retail business, for example—more attractive. The point here is that substantial differences can emerge even absent these forces.

4 Empirical Methods and Results

4.1 Empirical Specification

In this section we report on the main results from our empirical analysis of the program impacts. We focus on the pre-registered outcomes and specifications, but supplement it as needed to build up our overall narrative. Our basic specification is:

$$Y_i = \alpha + \beta_{LT} LT_{v(i)} + \beta_{ST} ST_{v(i)} + \beta_{LS} LS_{v(i)} + \gamma X_i + \psi_{s(i)} + \epsilon_i$$

where i indexes households, firms, elders; $v(i)$ indexes villages; $s(i)$ indexes strata. The controls, X_i, include the baseline value, if available, of the outcome or a close analogue if the exact outcome was not collected in the baseline. The controls also include an indicator for any part of the data collected by phone.

For household outcomes, we interact the three treatment indicators with the number of baseline adults and report the derived average treatment effect (Athey and Imbens, 2017). We expect that the treatment effects may be differential by the number of treatment-eligible adults in a household, so model this explicitly. In practice, this has little effect on the results. Where we have the data and where relevant, we add adult migrant outcomes back in to the household totals. We report all monetary outcomes in \$ PPP, and often annualize the flow values, unless otherwise noted.

We adjust the standard errors for spatial auto-correlation (Conley, 1999, 2010). We also report tests of each cross-arm equality restriction with the power caveats from earlier. We also test the null of no cross-village spillovers and, for most outcomes, we do not reject this null.

This is not surprising from a power perspective. Compared to Egger et al. (forthcoming), we are half as dense, with about half as many villages and no super-village intensity variation.

For outcomes with long right tails, we show some additional specifications that account for these, including (conditional) quantile regressions. We report all monetary outcomes in $ PPP, the flow values are annualized unless otherwise noted. We pre-specified some outcomes (and how they are calculated) in a pre-analysis plan (`https://www.socialscienceregistry.org/trials/1952`). Those pre-specified outcomes are mostly aggregates to represent a family of outcomes or an aggregate outcome (like income or consumption).

4.2 Results

Table 1 describes the implementation of the interventions, drawing on administrative payment records which we match back to our sample. The total amounts issued in the last two years (Column 2) are very similar for the three interventions, as they should be, since the expected value of disbursements over the first two years was meant to be the same. No households in LS had been issued a payment within the last 40 days as of either Fall 2019 or Summer 2020, while 90% of LT and ST households had.[12] The total amounts of money issued to an average treatment village are stated in Column 3; in the LT and ST arms these amount to 11% of total household expenditure in the average control village in the corresponding period, and in the LS arm to 12%.

Of course these are the gross transfers. We need to check whether they are partly crowding out other transfers or whether either the family or the local political system imposes additional taxes on them ((Jakiela and Ozier, 2015), (Squires, n.d.)). Table E.3 in the appendix shows that there is no significant effect on taxes paid to or transfers received from government, and the confidence intervals let us rule out effects that are substantial relevant to the size of the transfers. Remittance sent go up significantly in LS, and though the impacts in LT and ST are smaller and not significant, they are not too dissimilar in magnitude.[13] Again however the amounts are not large–of the order of 1-2% of the amount received from GD

[12]There are three reasons this is not 100%. The first is that not everyone we identified as living in a treatment villages was subsequently enrolled by GD, primarily because GD sometimes adopted a different definition of village boundaries than that used by our survey team. The second is that the match rate between our data and GD's data need not be 100% even among people that GD did, in fact, pay. The consequence of these two factors is that we can identify 92% of our sample in GD's data. The third is that not everyone GD identified had been paid due to deaths, administrative delays, etc., but this is a very small group: conditional on a match to GD's data, 99%, 99%, and 98% of the households in the LS, ST, and LT arms, respectively had received some payment. The bottom line is simply that the ITT estimates we report are likely to be slightly smaller than the full treatment-on-treated effects.

[13]However the fact that the effect is largest in LS may not be an accident. As we suggest, some people in LS have the biggest need for consumption smoothing, and a gift to a relative or friend now may be seen as a way to acquire a future claim.

over the last year. We also see that there is a negative effect on the presence of any NGO other than GD, significant in both ST and LS, though these NGOs do not seem have been major source of transfers.

Table 2 reports on the village level aggregates. We see that the number of enterprises, their costs, revenues and net revenues (i.e. profits without deducting the price of household labor) go up with LT and even more with LS (the difference though quite large is never significant). ST has smaller but still positive, effects on each of these outcomes which are only significant for net revenue, and the difference with LS is significant in most cases. Table 3 tells us that much of the overall effect comes from from enterprises in the non-agricultural sector despite the fact that both LS and ST have positive and significant effects on agricultural costs and revenues (but not on net revenue). The effect of LT on agricultural outcomes is smaller and not significant. Consistent with this, there is some evidence of modest increases in agricultural input usage (e.g. 5-7 percentage point increases in the extensive margin of fertilizer use in all arms, and a $\sim 10\%$ increase in labor input to livestock and farms in the ST arm — see Table E.7). Within the non-agricultural sector, the effect on retail is both the largest and the only one that is systematically significant (Table E.9).

Regarding consumer prices the data have less to say. We conducted one round of detailed price measurement in all relevant markets, but the variation induced by our design in the share (or count) of villages near these markets that were treated is necessary much less than the variation in village treatment status, which we can use to estimate effects on outcomes measured in villages. Overall we do not reject the null that consumer prices were unaffected, in which case the revenue increases discussed above come entirely from increased sales, albeit with fairly wide confidence intervals (Table E.1). One should also keep in mind that Egger et al. (forthcoming) studying lump-sum transfers in the same region (and with more spatial variation) precisely estimate consumer goods inflation at 0.1% in response to transfers amounting to 15% of GDP.

Turning to household level outcomes, Table 4 shows that the treated households are not working less. The point estimates are positive, but not significantly so. The corresponding impacts on total household income are also positive and significant for LS and ST. The effect of LS is large–50% of the control group income–and significantly greater that of LT. When we split this up by wage employment and self-employment, in LT we see the expected shift towards self-employment, and we see the same in ST and LS, though the reduction in wage employment is not significant.

The reduction in labor supply, combined with a growth in the number of enterprises, means that we would expect wages to go up, and generally speaking this is indeed what we see (Table E.4). We report two measures of wages: typical hourly wages paid to labor hired from outside study villages for agricultural or construction work, as reported by village elders, and the reservation wage the respondent reported they would require to accept work on a public works project during the main or off-season. We observe the former measures only in villages where the elder reports that hiring-in is common enough for there to be a prevailing wage, around 60% of villages, but this rate is not significantly different across arms (Columns 3 and 4). These measures goes up in all arms and significantly so in most cases (the exception being the agricultural wage in the LS arm). We observe the latter measures, on the other hand, for all households. Again we see positive estimated effects, significant in some cases, but the point estimates are much smaller (Columns 5 & 6). This suggests that the increase in realized wages in Columns 1 & 2 may reflect in part a shift in the supply of local labor, but is predominantly due to higher labor demand.

One factor that may have moderated wage effects is an in-migration response, shown in Appendix Table E.6. There is a small but significant increase in the number of new arrivals in existing households of about the same size in LT, ST and LS (Column 2), and a much larger increase in the number of households in both LT and LS (significant in LS) but no corresponding effect in ST. This in-migration likely also contributed to the increases in overall levels of economic activity documented above.[14]

Table 5 reports on consumption outcomes. All the interventions have positive effects on total consumption, though the LT effect is smaller than the other two and the only that is not significant. The effect is moderate sized, between 6-11% of the control median and 19%-33% of the average amount transferred by GD. The effect on food consumption is positive, similar-sized and significant in all three cases, and in all cases there is an similar significant improvement in protein consumption and in food variety as measured by the number of goods consumed, though the effect of LS on food variety is a bit smaller. Education spending goes up in all 3 arms, though only the somewhat larger ST and LS effects are actually significant. Health spending actually goes down, except in LS, but none of the effects are significant.

Spending on "temptation goods" in particular is a sensitive issue in this context, where alcoholism in particular is a well-documented problem (see for example Lo et al., 2013). Survey reports of alcohol consumption may understate the truth if the respondent under-reports their own consumption or is not fully aware of other household members' consumption.[15]

[14]On thie note, it is striking that the in-migration responses are very different between ST on the one hand and LS and LT on the other. The increase in new households is significantly larger in LS than in ST, and LT-ST difference though not significant (p=0.19) is large. One might speculate that these reflect the expectations of the future of these villages. The LS villages saw the biggest growth in enterprises and perhaps the expectation was that there will be a durable structural transformation in these. The LT villages do not yet see as large a change, but the intervention and hence the increased level of demand is expected to last for many years. In ST the current impact is smaller and its expected to end relatively soon.

[15]Though Schilbach (2019) obtains similar results when using either self-reported drinking or breathalyzer

We asked respondents in our Fall 2022 phone survey both about their own consumption and also about drinking they observed in their village. Table E.10 reports the results. While estimated effects on self-reported measures are approximately zero (and rule out large changes), effects on measures of drinking by others are significantly *negative*: respondents reported seeing fewer of their neighbors drinking daily, and were less likely to perceive drinking as a problem.

Table 6 reports on physical asset ownership. The first salient pattern is that self-reported value of agricultural real estate increases substantially and significantly in all three intervention groups (Column 1), but this does not appear to be primarily because they are acquiring more land from others (Column 6). Instead they ascribe greater marketable value to the same land. In part this likely reflects investments that they have made in structures that do enhance the value of the land—the value of existing structures goes up, significantly so in LS (Column 8), as does the value of recent investments in these structures, significantly so in LT and ST (Column 9). But these investments cannot come close to accounting for the very large appreciation in overall land value. As market transactions are rare in this setting we cannot compare these self-assessed values to market prices, and they may of course be mis-assessed to some extent. But the in-migration we response we noted earlier also suggests that these communities may indeed have become more desirable places to live.

Turning to non-real-estate assets, agricultural assets go up significantly in all three interventions (Column 3). While the impacts on non-agricultural business assets are similarly large, they are too noisy to be significant (Column 4). Non-real estate household assets (gadgets etc.) go up significantly with ST and LS but not with LT and the difference between ST and LT is significant (Column 5). In terms of financial assets (Table 7), the effect on both household and enterprise savings are positive and mostly significant and of similar magnitudes. The amount the households have in ROSCAs also goes up significantly with all three, but the effect of LT is significantly larger than that of the other two. LT has a negative effect on net household borrowing and essentially no effect on enterprise borrowing. ST has small positive effects on both types of borrowing but nothing is significant, and the same is of LS, except that it is effect on enterprise borrowing is actually large but very noisy. The difference in household borrowing between LT and LS households is significant–there is more borrowing in LS.

Table 8 reports on what happens to inequality within the village, which is both interesting in itself and as an input into people's well-being. There are no significant changes in either the 75-25 ratio or the Gini coefficient for either wealth or income from any of the interventions. On the other hand, the 75-25 ratio for non-land wealth goes down substantially and

test results.

significantly in all three, and LS effect is significantly bigger than the LT effect. For consumption the 75-25 ratio goes down significantly in ST, while the other interventions have no significant effects on either measure of inequality.

A distinct but related question is whether the interventions affected economic mobility, construed here to mean lack of dependence of eventual standards of living on initial ones. It is of course relatively early in the course of a 12-year experiment to examine this question, but still of interest given the large economic changes that had already taken place. We can reject large effects, however, on the *correlation* between baseline and endline assets or consumption per capita (Table 8, Columns 9 & 10). Mobility in this sense had not changed substantially.

Table 9 reports on a range of household welfare outcomes that are not directly measured by purchases or spending. In Table 5 we saw that education spending goes up in LS and ST. We now ask if this shows up in educational outcomes. We do not see any effect on years of schooling, but the likelihood of getting more than 250 in the KCPE exam goes up significantly substantially (14%) in LS, which is also the one with the biggest point effect on educational spending. Likewise the biggest effect in food expenditures is in ST and it is also the one with the biggest and the only significant positive effect on our anthropometric index. The index is defined and its individual components are described in Table E.2. The rest of the columns report on mental health outcomes. We see that all three treatments significantly reduced standard measures of depression though the LS effect is about half the size of the LT effect, and the difference is significant. The results on locus of control are more puzzling. The LT treatment has the most negative effect–it seems to disempower the households even as it makes them happier. The other two treatments also have negative effects but they are smaller and not significant. In terms of satisfaction with life outcomes, somewhat surprisingly ST is the one with the clearest positive effects, with LS always the least positive (and none of its effects are significant). Domestic violence seems to be most clearly reduced in LS, in the sense that the effect is the largest and the only one to be significant, but the point estimates are similar-sized and negative in all three. Finally, all three treatments have almost identical (positive, significant) effects on self-reported happiness as measured using Cantril's ladder, indicating that they were currently roughly $\frac{1}{5}$ of one step closer to their best possible life on a 0-10 step scale (Cantril, 1965).

4.3 Discussion of Results

The results mostly tell a story consistent with households that are both savings and credit constrained. The fact that the investment patterns are significantly different between ST and

LS despite the wealth transfer being the same in present value–which rules out a pure wealth effect on investment choices, acting say through a change in demand or in risk preferences–is evidence of a credit constraint. This is reinforced by the fact that the LT households, despite getting a much bigger wealth transfer in present value terms, choose to invest less than the ST households on agriculture and the LS households on both agriculture and non-agricultural businesses, though the differences are never quite significant.

While we have no direct evidence for credit constraints (or for that matter for savings constraints), it is striking that in Table 6, we see that total borrowing of a household in the control group is less than a month's consumption in the control group, as shown in Table 5. The amount borrowed for example is very low compared to say the subjects in Banerjee et al. (2015b) for whom loans outstanding is about ten times average monthly household consumption. We cannot say whether this is supply or demand, but credit seems not to be a major part of how households optimize over time in this setting.

Of course for the credit constraint to matter at the extensive margin (we see many fewer enterprises being created in ST compared to LS), there must be some non-convexity. Otherwise, there would be the same number of enterprises created in ST (and LT), just smaller. When we look at the data on household purchases of "lumpy" assets (defined as an asset worth KShs 5000 or more), we see larger effects on the total value purchased of these lumpy assets in the ST and LS arms, and reject the joint null that the effect was the same in all arms, even though effects on the frequency of large purchases was similar across arms (Table E.5).

Some of the strongest evidence for credit frictions comes from participation in rotating savings and credit associations (ROSCAs). People often mention these to us during our field visits after transfer had begun, especially as a means of turning streams of smaller payments into larger lumps of capital. Consistent with this, the follow-up phone survey indicates substantial increases in ROSCA participation in the LT arm, with annual payments higher by 19.8% of the average annual transfer value and by 300% of the control median (unconditional on making any payment). Note that the reference period for this outcome was the trailing three years, and thus includes the period after transfer began but before our first endline survey. This means that the (differential) effects we see here are consistent with the possibility that ROSCA participation increased differentially in the LT during that period, which would help explain the larger effects of LT on other outcomes—though we cannot be certain of this.

We also do not observe all the households in LS villages starting an enterprise and some households in ST do start one, which probably reflects some natural heterogeneity either in productivity or in access to capital. On the other hand, it is possible that some households would like to start a business enterprise, but only when they are able to invest at a certain scale, and therefore would like to save up. This may be particularly the case in LT households, who can foresee a long time horizon over which they will have resources that they can save.

Since many of the households are unable or unwilling to invest in a business enterprise, but a significant fraction would like to save, either for consumption smoothing or for eventually starting their enterprise, this where the savings constraint comes in to play. However, financial savings are not a big part of the household portfolios in the control group–the total mean household and enterprise savings is just over half of one month's consumption (Table 7, Columns 3 & 4). Most forms of savings go up with the interventions, but after two years the total increment is only about another 50% of the control group savings. We also do not see significant changes in outstanding debt (Columns 1 & 2). Given that the ST households are about to face the end of the GD inflows and for the LS households this has already happened this is strikingly little, and suggests that saving is more likely taking place in the form of physical assets.

One asset, in particular, that the households could potentially use for savings is land. The average household in control owns land valued at about twice the mean of household annual consumption spending. The value of land owned goes up by about 15-20% as a result of the intervention. This seems to be mostly the effect of revaluation without much improvement in its quality but with some improvement in amenities (refer to Appendix Table E.8). This is consistent with an increased demand for a store of value, which ends up raising the price, since the supply is inelastic. Given that there were was little reallocation of land (in our phone survey, in the year prior only 1.7% and 2% of households respectively sold and bought land), this means that most households have not found a location for their extra savings (though it is true that they have more wealth in terms of land, which means they need savings less).

Consistent with that, we see that the households are also acquiring non-enterprise related household assets. They are also repairing their homes. However we do not see big differences across the three treatments. In particular we might have expected the ST households to be under more pressure to acquire assets than the LT households, since their money runs out much sooner and most of them are unwilling or unable to make large business investments.[16] Where we do see those expected difference is in nutrition and education and in protein consumption, where the ST households spend more than the LT, though the difference is never significant. We do, however, find significant differences in the outcome of nutrition

[16] In the case of the LS households, there are two countervailing forces. On one side they have more money at once and therefore a greater compulsion to smooth consumption, but on the other side more of them start business enterprises and/or invest in exiting business enterprises.

- the ST households do significantly and substantially better in terms of anthropometric outcomes for children.

Finally turning to the mental health results, the fact that LS has the least positive effects on depression despite being the group that has the largest increment in earnings, may be consistent with the fact that getting a large one-time payment is stressful ("what happens if we waste the money"). Though the ST-LT difference on depression is not significant, it also goes in the right direction, in the sense that LT households have the least reason to be stressed. On the other hand, the fact that the impact on happiness is similar across the treatments, and the impact on satisfaction with living standards is higher in ST than in LT (though the difference is not significant) is a bit puzzling. Finally the fact that domestic violence actually goes down more in LS than in the other two is striking. Perhaps the fact that these households need to combine the assets of domestic partners in deciding on the right investments plays a role here?

5 Conclusion

This paper has described what we have learned two years in to a unique experiment designed to examine the effects of Universal Basic Incomes—here, a commitment to giving everyone in rural communities in Kenya income transfers sufficient to meet basic needs for over a decade. The design also lets us compare these effects to those of two shorter-term transfer programs which delivered similar amounts of money during the period we studied—as small monthly payments or in a single lump sum—but without the promise of ongoing funding.

Communities that received long-term transfers saw substantial economic expansions. Overall enterprise counts, revenues, costs, and net revenues increased by 14%, 41%, 35%, and 52%, respectively—substantial changes relative to the quantity of money transferred in, which amounted to 11% of control mean expenditure. This reflected structural shifts, with the expansion disproportionately concentrated in the non-agricultural sector, particularly in retail—indeed much of the economic story appears to have been the expansion of supply chains to meet increased local demand for goods manufactured elsewhere.

Labor supply did not fall—we estimate a small, insignificant increase, and can reject large decreases—but also did not rise by enough to account for much of this expansion. Instead the main pattern was one of changing occupational choices, with workers switching out of wage employment and into self-employment (and wages rises in response). Household well-being improved on some common measures (e.g. food consumption, depression) but not others (e.g. children's anthropometrics and schooling).

Relative to these effects, those of the shorter-term transfers were striking. Communities that received lump sum transfers experienced economic expansions similar to and on some measures even larger than those that received long-term transfers, as well as significant population growth (13% more households). Yet communities that received the same amount of money structured as short-term monthly payments saw very different effects: aggregate output grew significantly less, while by some measures (e.g. consumption) short-run household well-being increased more. Collectively this pattern of effects implies that both the way in which a given quantum of transfers is structured and the promised future duration of transfers can have large consequences for behavior and outcomes today. It is consistent, for example, with a model in which borrowing and saving are difficult and investment projects require both a large up-front capital outlay and ongoing flow investment to turn a profit—though this is hardly an exclusive explanation.

The pattern of effects on mental health—specifically, depression—are also noteworthy. Depression scores fell in all arms, but more so in short-term and especially the long-term arm than in the lump sum arm, even though the lump-sum arm saw the largest economic response. Perhaps the lump sum recipients feel the weight of the future more heavily, knowing that they have made their bets and have no further cushion to anticipate if those do not pan out.

Time (and subsequent round of data collection) will shed some light on this point, letting us examine how these interventions are affecting the volatility as well as the average levels of living standards. It will also reveal whether more generally the trajectories of households in the long-term communities diverge from those in the others as they continue to receive transfers. The long horizon of their transfers may allow them to bide their time, waiting for the right opportunities to arrive. Or it may prove that the initial wealth transfer in the lump sum arm was sufficient to kickstart those communities onto permanently better trajectories at much lower cost than the long-term approach.

In the meantime we conclude with three remarks relevant to the broader dialogue about UBI. First, UBI here did not lead to the adverse effects that some of its critics have argued it would. Most notably, people did not work or earn less on their own. This is in line with and extends the partial-equilibrium evidence from other shorter-term and more finely-targeted transfer programs in low- and middle-income countries (Banerjee et al., 2017). Second, while there is much to learn from the many short-term pilots now underway around the world, one should be cautious extrapolating from these to forecast the consequences of longer-term policies. Households here made their plans with the future in mind, and those in the long-term arm experienced very different outcomes—whether because they could borrow against or plan to spend their own future transfers, or plan to win future business from their neighbors. And third, tranching matters. Discussions about UBI usually begin from a narrative of meeting basic needs. But even the most destitute households often look for ways to accumulate sums of money large enough to make larger, lumpier purchases (Collins et al., 2009). Designing UBI schemes in ways that respond to this need could make them a more compelling strategy for addressing extreme poverty over time.

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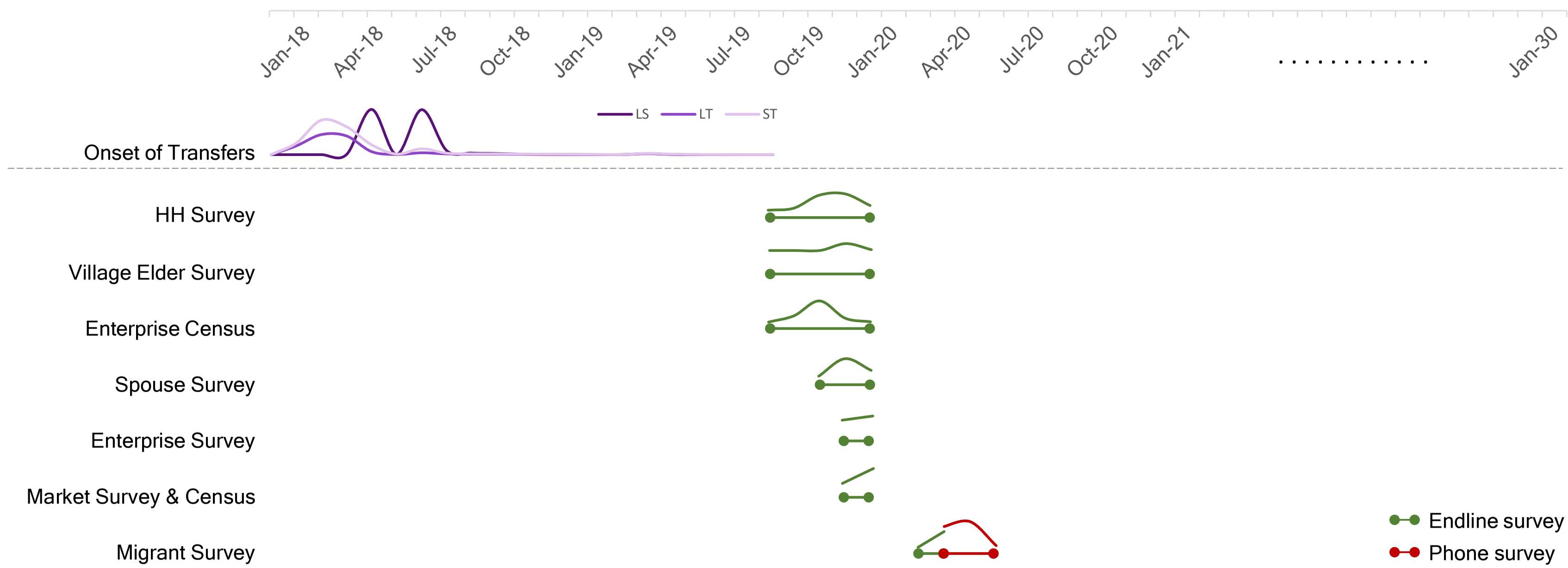
Figure 1: Project Timeline

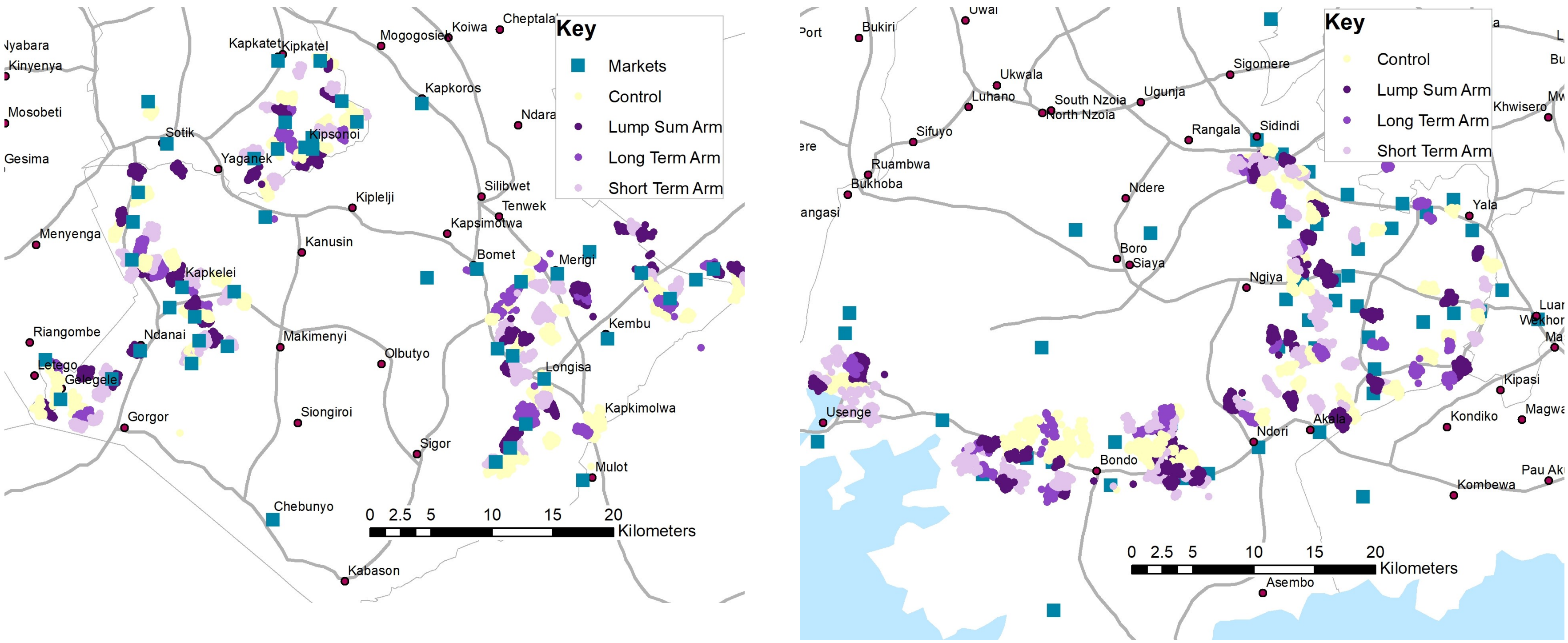


Notes: This figure plots the distribution of actual transfer onset date (top panel), and the temporal distribution of data gathering activities (bottom panel). In addition to the surveys depicted in the picture, we conducted a household survey over the phone between March and June of 2020. Analysis from this set of data is not covered in this paper. We also conducted phone surveys with migrant households between March and May of 2020, and analysis from this data is included in this paper.

Figure 2: Spatial Configuration of Households and Markets

(a) Bomet

(b) Siaya

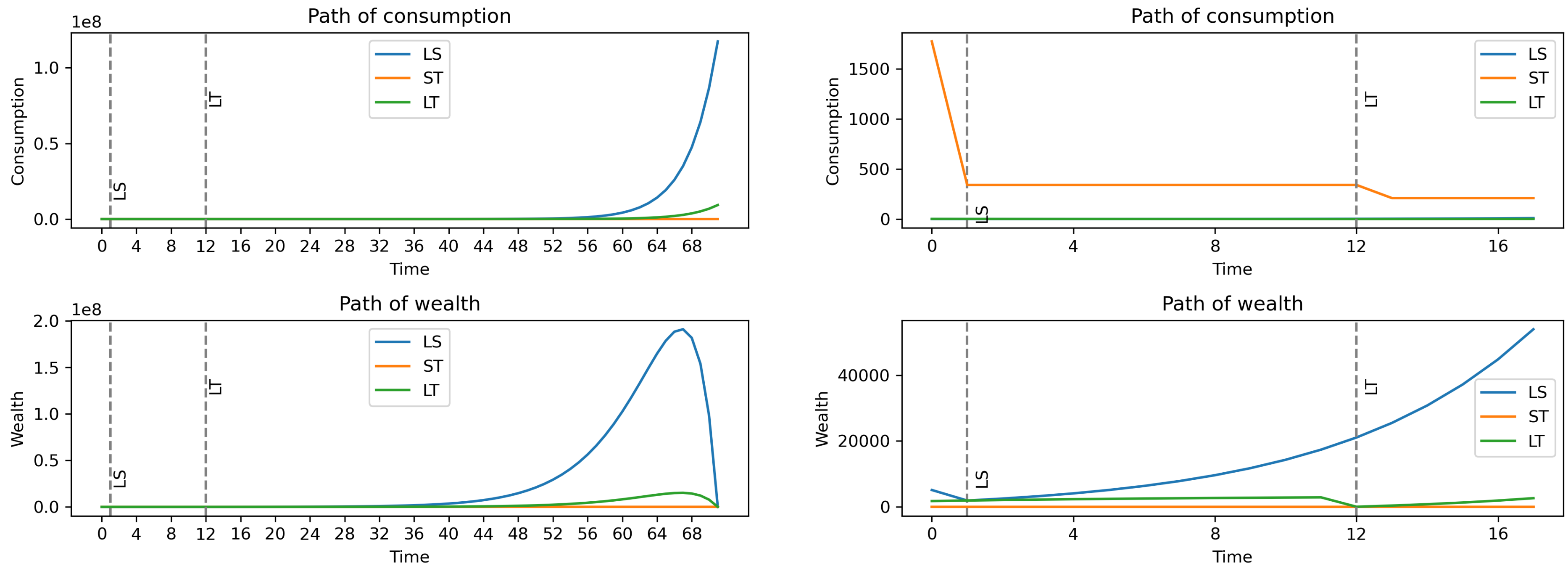


Notes: This figure maps surveyed households (circles) and all markets that these households reported frequenting (squares) in Bomet and Siaya counties. The unstudied region in the center of Siaya encompasses areas which GD had previously delivered transfers.

Figure 3: Model simulation results (high I, actual transfers): only LS and LT groups invest

(a) All periods

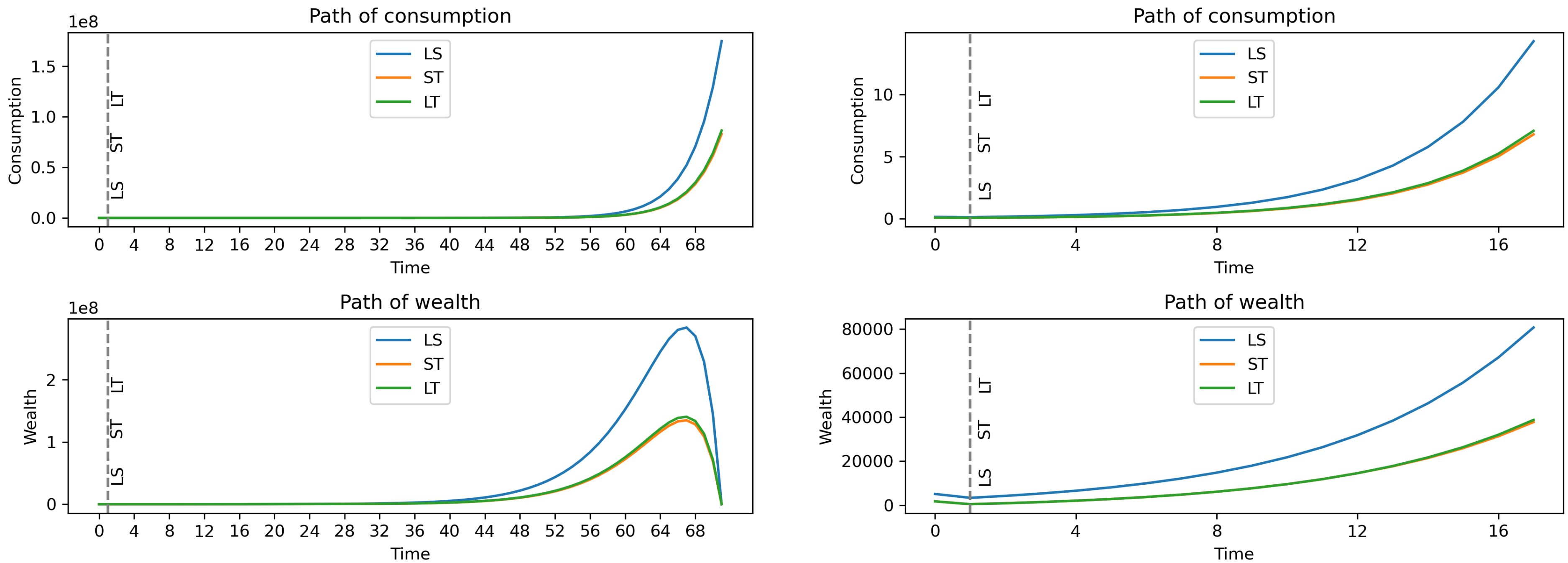
(b) Early periods



Notes: This figure shows the time paths of wealth and consumption under different treatments. The vertical dashed line shows the period of investment. Parameters were set as follows: $T = 72, S = 12, \delta = 0.999, r = 0.95, R = 1.5, \sigma = 0.6, w_0 = 1432, \overline{y} = 341, \underline{y} = 210, I = 2900$.

Figure 4: Model simulation results (low I, actual transfers): all groups invest

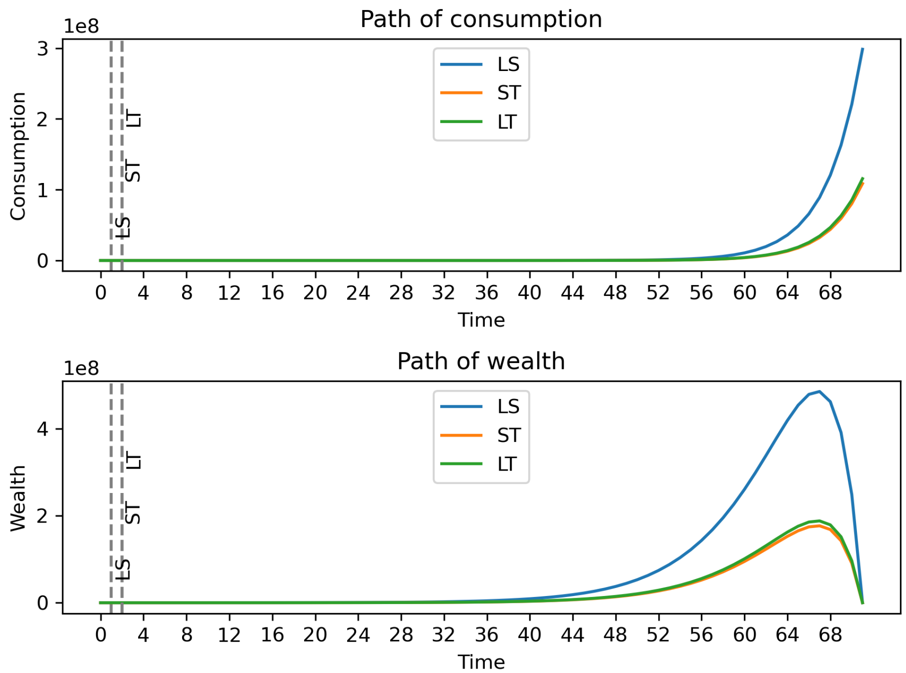
(a) All periods (b) Early periods



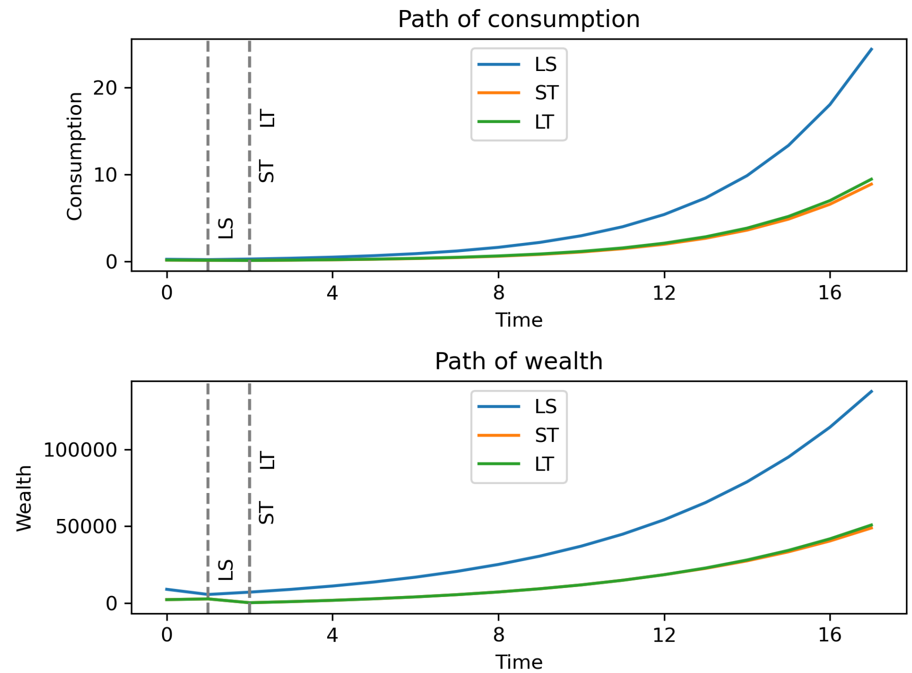
Notes: This figure shows the time paths of wealth and consumption under different treatments. The vertical dashed line shows the period of investment. Parameters were set as follows: $T = 72, S = 12, \delta = 0.999, r = 0.95, R = 1.5, \sigma = 0.6, w_0 = 1432, \overline{y} = 341, \underline{y} = 210, I = 1450$.

Figure 5: Model simulation results (high I, larger transfers): all groups invest

(a) All periods



(b) Early periods



Notes: This figure shows the time paths of wealth and consumption under different treatments. The vertical dashed line shows the period of investment. Parameters were set as follows: $T = 72, S = 12, \delta = 0.999, r = 0.95, R = 1.5, \sigma = 0.6, w_0 = 1432, \overline{y} = 472, \underline{y} = 210, I = 2900$.

Table 1: First-Stage and Compliance

	Fall 2019		
	Last 40 Days	Amount Recd.	Total GD Transfers
	(1)	(2)	(3)
Long Term Arm	0.91***	1937.04***	56156.89***
	[.01]	[55.6]	[1506.37]
Short Term Arm	0.92***	1934.23***	55809.05***
	[.01]	[31.5]	[920.63]
Lumpsum Arm	0.00	2133.92***	62435.67***
	[0]	[40.81]	[1086.47]
R-squared	0.86	0.54	.95
Control Mean	0.00	0.00	0.00
Control Median	0.00	0.00	0.00
p-value: ST = LT	0.64	0.96	0.84
p-value: LT > ST	0.68	0.48	0.42
p-value: ST = LS	0.00***	0.00***	0.00***
p-value: LS > ST	1.00	0.00***	0.00***
p-value: LS = LT	0.00***	0.00***	0.00***
p-value: LT > LS	0.00***	1.00	1.00
p-value: LT = ST = LS = 0	0.00***	0.00***	0.00***
Observations	8426	8480	294

Col (1) is an indicator variable for whether or not a household received any payment in the last 40 days before the endline survey. Col (2) is the total USD amount received by each household. Col (3) shows total GD transfers USD received in each village. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table 2: Overall Village Aggregates: Summed

	# Enterprises (1)	Revenues (2)	Costs (3)	Net Revenues (4)	Assets (5)
Long Term Arm	9.93**	61379.40**	32055.29*	28226.05**	36050.66***
	[3.96]	[24346.05]	[16478.13]	[12334.27]	[12589.11]
Short Term Arm	3.39	23177.47	8497.42	14824.71*	16441.81
	[3.57]	[16080.92]	[10462.44]	[8143.69]	[10029.27]
Lumpsum Arm	14.67***	107746.75***	71903.23***	35576.39***	29404.54***
	[3.92]	[34895.03]	[24360.84]	[13382.81]	[10977.68]
R-squared	.3	.24	.19	.27	.4
Control Mean	73.23	150207.24	92636.84	54533.59	100036.59
Control Median	70.07	126344.96	71651.40	45200.76	83927.59
p-value: ST = LT	0.12	0.13	0.15	0.33	0.11
p-value: LT > ST	0.06*	0.07*	0.08*	0.16	0.06*
p-value: ST = LS	0.01***	0.02**	0.01***	0.14	0.23
p-value: LS > ST	0.00***	0.01***	0.00***	0.07*	0.11
p-value: LS = LT	0.27	0.24	0.15	0.65	0.59
p-value: LT > LS	0.87	0.88	0.92	0.68	0.30
p-value: LT = ST = LS = 0	0.00***	0.00***	0.01**	0.01**	0.02**
Observations	294	294	294	294	294

The non-agricultural enterprise data comes from the enterprise census while the agricultural enterprise data comes from the household survey and has been scaled up to the village level using the sampling ratio. Columns (2)-(4) are for the last 30 days (for non-agricultural enterprises) and converted from annual to monthly level (for agricultural enterprises). Enterprises with missing data assumed to be zero for the purpose of this analysis. The aggregates have been calculated by summing up respective agricultural and non-agricultural sector aggregates at the village level. Total income data comes from the household survey and has been scaled up to the village level using the sampling ratio. Total income includes wage earnings, agricultural net revenues, non-agricultural net revenues, and capital income from interest on savings and net rents received. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table 3: Agricultural and Non-Agricultural Village Aggregates

	Ag					Non-Ag					
	# Ent. (1)	Rev. (2)	Costs (3)	Net Rev. (4)	Assets (5)	# Ent. (6)	Rev. (7)	Costs (8)	Net Rev. (9)	Inv. (10)	Assets (11)
Long Term Arm	4.36	8521.72	5277.01	2137.42	29779.09***	5.57***	52857.68**	26778.28*	26088.63**	1766.12	6271.58
	[2.93]	[5523.36]	[3785.31]	[4615.71]	[10833.07]	[1.81]	[22776.31]	[15747.31]	[11245.63]	[1483.32]	[6082.45]
Short Term Arm	0.49	10174.38**	5966.48*	4368.46	15558.74*	2.90**	13003.09	2530.93	10456.25	1411.88	883.07
	[2.6]	[5136.73]	[3464.9]	[4033.6]	[8977.02]	[1.35]	[14778.92]	[9819.96]	[6713.02]	[1222.48]	[4328.38]
Lumpsum Arm	8.10***	13306.62***	8449.31***	4543.20	20301.19**	6.56***	94440.13***	63453.92***	31033.19**	3331.04*	9103.35
	[2.68]	[4576.77]	[3111.32]	[3880.54]	[8654.36]	[1.63]	[33864.96]	[24255.76]	[12384.62]	[1801.44]	[6457.17]
R-squared	.2	.51	.28	.46	.45	.45	.19	.17	.2	.29	.17
Control Mean	57.10	61554.32	30432.46	28089.51	86786.77	16.13	88652.93	62204.39	26444.08	5028.02	13249.82
Control Median	53.83	50356.59	23380.90	20260.07	70248.29	15.00	63386.46	49227.87	17928.76	2288.50	5076.47
p-value: ST = LT	0.21	0.79	0.86	0.66	0.18	0.13	0.09*	0.13	0.20	0.80	0.39
p-value: LT > ST	0.11	0.61	0.57	0.67	0.09*	0.07*	0.05**	0.06*	0.10	0.40	0.20
p-value: ST = LS	0.01***	0.56	0.47	0.97	0.58	0.02**	0.02**	0.01**	0.11	0.29	0.21
p-value: LS > ST	0.00***	0.28	0.24	0.48	0.29	0.01***	0.01***	0.01***	0.05*	0.14	0.10
p-value: LS = LT	0.23	0.40	0.40	0.63	0.34	0.60	0.27	0.19	0.74	0.38	0.70
p-value: LT > LS	0.88	0.80	0.80	0.68	0.17	0.70	0.86	0.91	0.63	0.81	0.65
p-value: LT = ST = LS = 0	0.01**	0.03**	0.05*	0.59	0.03**	0.00***	0.01**	0.02**	0.02**	0.28	0.47
Observations	294	294	294	294	294	294	294	294	294	294	294

Data for agricultural enterprises in this table comes from the household survey and has been scaled up to the village level. Columns (2)-(5) have been converted to a monthly level. Enterprises with missing data assumed to be zero for the purpose of this analysis. Agricultural revenues include the value of crop output produced in the main and short seasons, the value of livestock products produced, and the value of livestock sold. Agricultural costs include the land preparation costs, seed costs, fertilizer costs, fertilizer transportation costs, hired labor costs in the main and short seasons, as well as the rent paid out on land rented in. Agricultural net revenues is calculated as the difference between agricultural revenues and agricultural costs. Agricultural assets includes assets like tractors, grain stores, farm implements, machinery, etc. Data for non-agricultural enterprises comes from the enterprise census. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table 4: Occupational Choice, Hours, Earnings, and Wage Rates

	Overall		Wage Employment		Self Employment (Non-Ag)		Self Employment (Ag)	
	Hours	Income	Hours	Income	Hours	Income	Hours	Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Long Term Arm	61.92	503.40	-99.85**	-275.53***	95.70*	692.69**	66.07	4.99
	[67.8]	[314.8]	[46.45]	[105.3]	[54.74]	[284.59]	[53.18]	[59.81]
Short Term Arm	110.23	671.60*	-68.73	323.03	90.72**	215.67	88.24**	94.10*
	[77.59]	[365.13]	[47.18]	[217.4]	[45.69]	[285.76]	[40.92]	[52.68]
Lumpsum Arm	79.42	1274.80**	-27.30	272.42*	81.79	875.18*	24.94	41.59
	[78.02]	[571.56]	[40.01]	[149.54]	[51.89]	[520.79]	[37.81]	[50.11]
R-squared	0.13	0.03	0.11	0.03	0.04	0.01	0.12	0.05
Control Mean	2752.04	2517.63	1031.87	1478.71	522.78	589.43	1197.39	491.62
Control Median	2208.00	1097.09	240.00	211.93	0.00	0.00	848.00	201.52
p-value: ST = LT	0.57	0.65	0.56	0.00***	0.92	0.10	0.71	0.17
p-value: LT > ST	0.71	0.67	0.72	1.00	0.46	0.05*	0.65	0.91
p-value: ST = LS	0.73	0.31	0.36	0.80	0.88	0.20	0.15	0.41
p-value: LS > ST	0.63	0.15	0.18	0.60	0.56	0.10	0.93	0.80
p-value: LS = LT	0.83	0.10	0.13	0.00***	0.83	0.66	0.39	0.54
p-value: LT > LS	0.58	0.95	0.93	1.00	0.42	0.67	0.20	0.73
p-value: LT = ST = LS = 0	0.48	0.09*	0.15	0.00***	0.14	0.10	0.14	0.27
Observations	8300	8211	8300	8291	8300	8480	8300	8396

Columns (1), (3), (5), and (7) show total hours worked overall in the last 12 months on self-employed and wage work, wage work and, self-employed agricultural and non-agricultural work respectively. Col (2) shows annualized non-transfer income in USD. Col (4) shows earnings from wage work over 12 months. Col (6) and Col (8) show annual non-agricultural and agricultural enterprise profits in USD. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 5: Consumption

	Total (1)	Non-Food (2)	Food (3)	# Food Items (4)	Protein (5)	Education (6)	Health (7)
Long Term Arm	368.12	-15.47	387.69***	0.78**	52.53*	45.07	-115.57
	[244.92]	[187]	[98.37]	[.31]	[28.44]	[39.09]	[83.5]
Short Term Arm	640.78**	261.28	365.79***	0.96***	67.24**	77.87**	-75.35
	[288.79]	[201.61]	[133.51]	[.22]	[26.2]	[39.67]	[93.46]
Lumpsum Arm	514.51*	281.82	229.53**	0.42*	61.17*	103.15**	70.88
	[308.68]	[244.85]	[112.85]	[.22]	[32.61]	[40.74]	[118.37]
R-squared	0.13	0.12	0.09	0.04	0.05	0.15	0.01
Control Mean	7866.62	4043.84	3822.78	18.99	714.10	566.43	906.57
Control Median	5839.43	2117.24	3260.05	18.00	523.51	111.54	35.69
p-value: ST = LT	0.32	0.15	0.85	0.51	0.52	0.45	0.60
p-value: LT > ST	0.84	0.93	0.42	0.74	0.74	0.77	0.70
p-value: ST = LS	0.73	0.94	0.24	0.01**	0.83	0.63	0.14
p-value: LS > ST	0.63	0.47	0.88	0.99	0.58	0.32	0.07*
p-value: LS = LT	0.61	0.18	0.13	0.21	0.78	0.22	0.08*
p-value: LT > LS	0.69	0.91	0.06*	0.10	0.61	0.89	0.96
p-value: LT = ST = LS = 0	0.10*	0.17	0.00***	0.00***	0.08*	0.03**	0.23
Observations	8459	8459	8459	8365	8365	8480	8480

Col (1) shows total annual consumption expenditure on food, non-food items, education, and appliances. Columns (2), (3), (5), (6), and (7) look at annual consumption expenditure on non-food items, food, protein, education, and health. Col (4) refers to number of food items consumed every week. Columns (4)-(5) exclude individual migrants. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 6: Physical Assets

	Real Estate (Value)		Non-Real Estate (Value)			Ag	Ag Plots (Value)		
	Ag	Res.	Ag	Non-Ag	Res.	Acreage	Plot	Structures	Upgrades
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Long Term Arm	3100.09***	-216.05	348.91***	138.33	187.50	0.13	5622.17***	346.61	404.68**
	[1060.22]	[151.1]	[113.93]	[199.41]	[115.57]	[.1]	[2020]	[495.43]	[187.11]
Short Term Arm	2752.12***	81.36	293.64***	629.06	462.33***	0.18**	5059.81***	654.79	414.23**
	[756.99]	[151.88]	[96.40]	[565.5]	[144.11]	[.09]	[1363.45]	[459.66]	[161.97]
Lumpsum Arm	2291.15***	-62.27	201.57**	550.03	399.14***	-0.02	5109.34***	1932.53***	100.86
	[779]	[165.21]	[90.84]	[460.13]	[151.03]	[.09]	[1587.11]	[488.63]	[144.94]
R-squared	0.15	0.03	0.09	0.01	0.07	0.05	0.12	0.05	0.02
Control Mean	14152.59	1145.79	1459.63	667.29	1432.06	1.61	25107.71	6628.15	946.68
Control Median	7436.23	0.00	838.44	0.00	645.65	1.00	12393.72	2478.74	0.00
p-value: ST = LT	0.67	0.11	0.65	0.40	0.05**	0.61	0.70	0.49	0.96
p-value: LT > ST	0.34	0.95	0.32	0.80	0.98	0.69	0.35	0.75	0.52
p-value: ST = LS	0.40	0.41	0.33	0.92	0.76	0.02**	0.97	0.01***	0.03**
p-value: LS > ST	0.80	0.79	0.84	0.54	0.62	0.99	0.49	0.00***	0.98
p-value: LS = LT	0.29	0.41	0.10	0.32	0.17	0.07*	0.76	0.00***	0.07*
p-value: LT > LS	0.15	0.80	0.05*	0.84	0.91	0.03**	0.38	1.00	0.03**
p-value: LT = ST = LS = 0	0.00***	0.39	0.00***	0.41	0.00***	0.04**	0.00***	0.00***	0.02**
Observations	8161	8262	8480	8347	8480	7476	6943	7062	7483

Data for columns (1-5) comes from the endline survey. Data for columns (6-9) comes from the follow-up phone survey. For columns (1-2) and (7-8), we exclude values in the top 1 percentile. Columns (1-2) show values of agricultural and residential real estate assets held by households. Columns (3-5) refer to value of non-land agricultural assets (this includes farm and livestock assets), non-agricultural enterprise assets, and household assets (value of savings, physical household assets, and net outstanding loans) held by each household. Col (7) refers to value of all the plots of land owned, excluding value of any structures on the plot. Col (8) refers to the value of all built structures on the plot. Col (9) refers to any money spent in the last 5 years upgrading the structures on the plot. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table 7: Financial Assets

	Borrowing		Savings		ROSCAs	
	HH	Ent.	HH	Ent.	No. of ROSCAs	Annual Payment into ROSCAs
	(1)	(2)	(3)	(4)	(5)	(6)
Long Term Arm	-89.85	2.19	69.14***	85.75*	0.09***	192.35***
	[68.53]	[51.32]	[22.99]	[46.74]	[.02]	[32.53]
Short Term Arm	5.00	23.83	82.26***	45.68	0.02	22.36
	[60.95]	[36.12]	[27.68]	[40.61]	[.01]	[20.32]
Lumpsum Arm	26.94	221.26	76.77**	97.00*	0.03**	44.00**
	[61.43]	[233.23]	[33.25]	[56.84]	[.02]	[21.95]
R-squared	0.02	00	0.03	0.01	0.11	0.09
Control Mean	425.84	56.54	241.46	149.90	0.32	274.45
Control Median	29.74	0.00	40.16	0.00	0.33	64.45
p-value: ST = LT	0.11	0.63	0.65	0.37	0.00***	0.00***
p-value: LT > ST	0.95	0.69	0.67	0.19	0.00***	0.00***
p-value: ST = LS	0.73	0.39	0.88	0.33	0.28	0.26
p-value: LS > ST	0.37	0.20	0.56	0.16	0.14	0.13
p-value: LS = LT	0.05*	0.26	0.80	0.83	0.00***	0.00***
p-value: LT > LS	0.97	0.87	0.60	0.59	0.00***	0.00***
p-value: LT = ST = LS = 0	0.22	0.15	0.00***	0.24	0.00***	0.00***
Observations	8356	8358	8363	8365	7519	7519

Col (1) refers to the net amount borrowed by the household while Col (2) refers to the total amount borrowed by non-agricultural enterprises. Col (3) refers to household savings while Col (4) refers to the value of working capital held by a non-agricultural enterprise. Data for Col (5) comes from the survey question - "How many ROSCAS have you and members of your household participated in the last 3 years?", and Col (6) refers to annual contributions to ROSCAs by households. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table 8: Inequality

	Wealth		Non-Land Wealth		Income		Consumption		Correlation b/w	Correlation b/w
	Ratio (75/25) (1)	Gini (2)	Ratio (75/25) (3)	Gini (4)	Ratio (75/25) (5)	Gini (6)	Ratio (75/25) (7)	Gini (8)	Bline & Eline Assets (9)	Bline & Eline Cons. (10)
Long Term Arm	-0.26	-0.01	-1.09***	-0.03	-88.33	-0.15	-0.06	-0.01	0.00	0.01
	[.65]	[.01]	[.36]	[.02]	[54.08]	[.64]	[.13]	[.01]	[.04]	[.04]
Short Term Arm	-0.32	0.00	-1.48***	-0.01	-26.28	-0.50	-0.16*	-0.02	0.04	0.02
	[.34]	[.01]	[.32]	[.02]	[26.73]	[.54]	[.09]	[.01]	[.03]	[.03]
Lumpsum Arm	-0.04	0.01	-1.63***	0.01	61.31	-1.14	0.05	0.00	0.04	0.05
	[.32]	[.01]	[.36]	[.03]	[43.32]	[.8]	[.1]	[.01]	[.03]	[.03]
R-squared	.23	.27	.26	.13	.19	.18	.27	.17	.2	.11
Control Mean	4.58	0.50	5.55	0.59	13.73	1.18	2.62	0.38	0.40	0.38
Control Median	3.93	0.49	4.94	0.57	8.62	0.68	2.48	0.36	0.43	0.39
p-value: ST = LT	0.92	0.28	0.18	0.39	0.25	0.39	0.47	0.56	0.39	0.73
p-value: LT > ST	0.46	0.86	0.09*	0.81	0.87	0.19	0.24	0.28	0.80	0.63
p-value: ST = LS	0.38	0.85	0.57	0.63	0.17	0.36	0.04**	0.17	0.98	0.45
p-value: LS > ST	0.19	0.43	0.72	0.31	0.08*	0.82	0.02**	0.09*	0.49	0.22
p-value: LS = LT	0.70	0.23	0.08*	0.29	0.07*	0.20	0.45	0.54	0.41	0.35
p-value: LT > LS	0.65	0.89	0.04**	0.86	0.96	0.10	0.77	0.73	0.80	0.82
p-value: LT = ST = LS = 0	0.78	0.62	0.00***	0.55	0.35	0.42	0.15	0.41	0.49	0.50
Observations	294	294	294	294	286	294	294	294	294	294

Col (1) shows the ratio of wealth (measured as value of all assets, the top 1 percentile trimmed) at the 75th and 25th percentiles, and Col (2) shows the Gini index for wealth. Col (3) shows the ratio of non-real estate wealth (measured as the value of all farm assets, livestock assets, enterprise assets, household assets, savings, and net outstanding loans, the top 1 percentile trimmed) at the 75th and 25th percentiles, and Col (4) shows the Gini index for non-land wealth. Col (5) shows the ratio of annualised non-transfer income (the top 1 percentile trimmed) at the 75th and 25th percentiles, and Col (6) shows the Gini index for annualised non-transfer income. Col (7) refers to the ratio of total annual consumption at the 75th and 25th percentiles, and Col (8) shows the Gini index for total annual consumption. Col (9) shows correlation between baseline and endline assets which include household assets, land assets, financial assets, enterprise assets, livestock assets, farm assets, and net value of loans. Col (10) shows correlation between baseline and endline expenditure on food, cellular airtime, clothing, transport, education, and health. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 9: Softer Measures

	Anthro. Index	Years of Schooling	KCPE Score >= 250	Mental Health			Domestic Violence	Ladder: Current
				CES-D	Depressed (Y/N)	Locus of Cont.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Long Term Arm	0.00	-0.09	0.04	-1.97***	-0.08***	-0.20*	-0.05	0.20***
	[.04]	[.08]	[.04]	[.49]	[.02]	[.1]	[.04]	[.07]
Short Term Arm	0.07*	-0.01	-0.00	-1.73***	-0.07***	-0.07	-0.06	0.22***
	[.04]	[.07]	[.03]	[.44]	[.02]	[.09]	[.04]	[.06]
Lumpsum Arm	0.01	0.01	0.08***	-1.03***	-0.04**	-0.09	-0.08**	0.21***
	[.04]	[.06]	[.03]	[.38]	[.02]	[.08]	[.04]	[.06]
R-squared	0.14	0.44	0.05	0.23	0.17	0.06	0.02	0.09
Control Mean	-0.54	7.82	0.57	16.43	0.47	16.75	0.00	4.39
Control Median	-0.54	8.00	1.00	15.00	0.00	17.00	-0.27	5.00
p-value: ST = LT	0.15	0.30	0.26	0.61	0.59	0.21	0.84	0.81
p-value: LT > ST	0.93	0.85	0.13	0.69	0.71	0.90	0.42	0.59
p-value: ST = LS	0.13	0.79	0.01***	0.10	0.13	0.79	0.71	0.75
p-value: LS > ST	0.94	0.40	0.00***	0.05*	0.06*	0.61	0.64	0.62
p-value: LS = LT	0.94	0.20	0.20	0.04**	0.04**	0.36	0.54	0.98
p-value: LT > LS	0.53	0.90	0.90	0.98	0.98	0.82	0.27	0.51
p-value: LT = ST = LS = 0	0.29	0.59	0.02**	0.00***	0.00***	0.22	0.16	0.00***
Observations	10267	4398	2118	8219	8219	8341	4107	8323

Anthropometric index is the average of the weight-for-age and height-for-age z-scores of children aged 2-12 years at baseline. Years of schooling completed is only for children present at baseline who would have made a primary to secondary transition. KCPE stands for Kenya Certificate of Primary Education. Domestic violence index includes whether women reported having been threatened, hit or had something thrown at them during the past 30 days. Depression Index is calculated as a mean of responses to questions from the Center for Epidemiological Studies Depression questionnaire (scores range from 0-60). Depressed (Y/N) uses a cut-off score of 16 or greater on the CES-D scale that aids in identifying individuals at risk for clinical depression. Locus of control is the degree to which people believe that they, as opposed to external forces, have control over the outcome of events in their lives, calculated here as a mean of normalized responses to questions assessing the extent to which respondents felt in control of their lives. Quality of life ladder scale runs from 0-10 where 0 is the worst possible life and 10 is the best possible life. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

A Related Studies

There is a large growing literature on interventions that are more or less closely related to UBI. The closest are the regular unconditional cash transfers, either UBI or targeted Basic Income programs. These are summarized below in Table A.1. To our knowledge there are two evaluations of a proper UBI, one based on the Alaska Permanent Fund (Jones and Marinescu, 2018) and the other Iran's nation-wide UBI (Salehi-Isfahani and Mostafavi-Dehzooei, 2017). The former uses a synthetic control method, the latter uses a difference-in-differences. In both cases the focus is on the labor supply effect, rather than on changes in the overall well-being of the household.

In terms of an experimental evaluation, there are a number of evaluations of small *targeted* basic income interventions, where the number of targeted individuals is 2000 or less. As a result they typically cover a small fraction of the population of the area where they are being implemented. This means that the studies typically do not capture the spillovers, both positive (say through demand and risk-sharing) and negative (say through business-stealing) that would be expected when the program is more or less universal. A previous evaluation of a Give Directly program in Kenya (Haushofer and Shapiro, 2016) finds that household monthly consumption went up from \$158 PPP to \$193 PPP nine months after the transfer, and the transfers had a sizable effect on self-reported measures of well-being (happiness, life satisfaction, stress, and depression). Moreover the targeting has the potential to alter the impact; for example, a program that targets the neediest may have much larger effect on nutrition, but for the same reason may lead to fewer new businesses.

Results suggest that these interventions led to improvements in consumption/reductions in poverty and self-reported well-being went up. Strikingly there is also no evidence of a significant reduction in adult labor supply. For example, in the most recent GD study for parts of Siaya County (close to one half of our UBI study sample), Egger et. al 2022 find that recipient households did not work less. In fact, total hours worked by recipient households in agriculture, self employment and employment increased slightly but not significantly. [17]

There is also a substantial body of evidence from one time unconditional transfers similar to our LS intervention. Banerjee et al. (2015a) reports on evaluations from six countries of the so-called Graduation program, a multi-faceted intervention targeted towards the poorest of the poor which combines an asset transfer with some short-term training/hand-holding and small amounts of temporary cash support. The evidence suggests that beneficiary households work longer hours, and have durably higher consumption levels. Balboni et al. (2021) reach a similar conclusion based on evidence from the same program implemented in Bangladesh and argue that the intervention may have unlocked a poverty trap. Blattman et al. (2013) also finds evidence of large gains in earnings several years after an intervention which offered young men \$400 in cash to start a livelihood.

The many experimental and quasi-experimental evaluations of Conditional Cash Transfer Programs

[17] The labor supply of children is potentially a very different outcome compared to adult labor supply, since there is a strong case to be made that children should be in school rather than working.

constitute another closely related literature. These interventions have been shown to increase years of schooling, improve health practices, nutrition and increase business creation, though the effects on education and health practices are at least partly due to the conditions imposed on receiving the transfers.[18] These programs tend to target low income households with young children. In principle this could means a relatively large fraction of the population in many developing countries receive it - Progresa/Opportunidades reached a quarter of Mexican households–but the definition of low-income and therefore eligibility varies a lot across countries. There is also variation how long a household stays eligible–in Mexico, households typically become eligible to participate in this program at or just before the birth a child and remain eligible until the child gets to high school as long as the child does not drop out, so in principle the transfers can be very durable, but in Nicaragua's RPS, for example, the eligibility only lasted two years. In this sense these programs may be seen as a close parallel to our ST and LT interventions.

On the other hand the fact that these program excludes many households both because of targeting and the conditionalities, and the fact that the average amounts are relatively small means that the overall additional inflow into the local economy from these programs is small compared to the 11-12% we estimate for our interventions. For example, in 2000, Progressa covered about 2.6 million families in 72,345 localities. The total amount of transfers delivered in 2000 was $934 million, equivalent to about $1.4 billion in 2018 dollars (to allow a comparison to our transfers). This implies a transfer of $19,000 per locality and $533 per family (Coady, 2000). This is about half of the size of our transfers per recipient family and somewhere between 30% and 40% of households in the most marginalized communities were covered by Progressa (see Figure 2 in (Parker and Vogl, 2021))

However the most important difference between these experiments and ours is however the fact that we are set up to compare the three interventions, ST which is like some of the more short-term CCTs, LT which is closer to some of the more durable CCTs, both without the conditionalities of course, and LS which is related to the Graduation programs and other one-time transfer. From the point of view of better policy design, these comparisons are obviously key.

[18] The evidence from the Malawi experiment which allows us to compare the education impacts of a Conditional Cash Transfer to that of an Unconditional Cash Transfer suggests that a majority of the impact comes from the conditionality.

Table A.1: Evidence from Other Studies

	Finland	Stockton	Spain	Chelsea	Compton	Y-Combinator	Iran	Alaska
Annual Amount	€6,720	$6,000	€3,000 - €7,800	$1,400-$2,800	$3,600-$7,200	$12,000	28% med income	$2000
Type	BI	BI	BI	BI	BI	BI	UBI	UBI
Sample Size	2,000	125	1,000	2,074	700	1,000	Adults	Adults
Duration (Years)	2	2	2	0.58	2	3	5-10	39
Labor Supply	↑	↑*	→		-	-	↑	→
Income	NA	NA	NA	-	-	-	NA	NA
Wealth	NA	NA	NA	-	-	-	NA	NA
Mental Health	↑↑	↑↑	↑↑	-	-	-	NA	NA
GDP	NA	NA	NA	-	-	-	NA	NA

The number of arrows gives an indication of the effect size. For effects that are not statistically different from zero, we use a horizontal arrow, indicating no evidence of an increase or decrease. NA stands for not applicable, i,e. the study does not have data on these outcomes. The Compton and Y-Combinator studies are in progress and do not have results yet. For more on these studies, see:

- Finland: (Hämäläinen et al., 2019)
- Stockton: `https://bit.ly/45KLqll`
- Spain: (Kirchner et al., 2019)
- Chelsea: (Liebman et al., 2022)
- Compton: `https://bit.ly/3KZnGBU`
- Y Combinator: `https://www.openresearchlab.org/basic-income`
- Iran: (Salehi-Isfahani and Mostafavi-Dehzooei, 2017)
- : Alaska: (Jones and Marinescu, 2018)

B Theory Appendix: Analysis

The model in Section 3 above can be analyzed as follows. Here, we use a verbal sketch of the analysis of the model because we ultimately simulate out the model to show the key comparative statics.

L type households will consume their income in the LT arm because $r < 1$. However, in the ST arm, L type households may consume less than their incomes order to smooth consumption. To see this, assume that these households always consume their incomes. Then consumption optimality requires that in period $S - 1$

$$u'(y_1) \geq \delta r u'(y_2)$$

which may not hold if $y_2 \ll y_1$.

Now suppose $u'(y_1) < \delta r u'(y_2)$. Then $c_{S-1} < y_1$. In this case, it is possible that it is also true that

$$u'(y_1) < \delta r u'(c_{S-1})$$

which would imply that $c_{S-2} < y_1$. This saving on the part of L type households in the ST arm may therefore start a few periods before the money runs out.

Moreover, their consumption is always non-increasing. This follows from the fact that either

$$u'(c_t) = \delta r u'(c_{t+1})$$

in which case it is automatic given $\delta r < 1$ or

$$u'(c_t) > \delta r u'(c_{t+1})$$

in which case $c_t = w_t + y_t > c_{t+1} \leq y_{t+1} \leq y_t$.

If we then define the realized discounted utilities to be $U(ST)$ and $U(LT)$, the most interesting case is the H type households in both the ST and LT arms.

For these households there are two choices: either behave like the L type households in each case, or else save up to hit I.

In the latter case, suppose these households invest I in period T^*. Then, the following Euler equation needs to hold as an equality in every period after $T^* : u'(c_t) = \delta R u(c_{t+1})$.

Before T^*, these households will typically accumulate wealth (unless the household hits I in the first period).

In any period after T^*, since people can always save at the rate R, the only possibility we need to take into account is where $u'(w_t + y_t) > \delta R u'(y_{t+1})$. However, since $\delta R > 1$ and $y_{t+1} \leq y_t$, this inequality cannot hold.

In period T^* and before, it is possible that the credit constraint binds and that $u'(c_t) > \delta r u(c_{t+1})$. However, just like for L types before period T^* consumption must be non-increasing.

To see this, consider a period $T_1 \leq T^*$ such that in period $T_1 - 1$, the household does not save. Then

$w_{T_1} = 0$. Therefore $c_{T_1} \leq y_{T_1}$. Moreover if $c_{T_1} = y_{T_1}$, then $w_{T_1+1} = 0$. Hence, as long as $y_t = y_{T_1}$ in any period, then $t > T_1, c_t \leq y_{T_1}$. Moreover if $c_{T_1} < y_{T_1}$, since consumption is non-increasing, as long as $y_t = y_{T_1}$ in any period, then $t > T_1$, $c_t < y_{T_1}$.

In other words at any given income level the household might not save for some periods, but once the household starts saving, it continues to accumulate wealth until either it's income drops or it invests in the sunk cost.

To summarize this discussion, we have the following useful set of results. For any fixed T^*:

1. A H type LT household before T^* can be characterized by at most three phases: a phase $[0, T_1 - 1]$ where $c_t < w_t + y_1$; a phase $[T_1, T_2 - 1]$ where $c_t = w_t + y_1$; and a phase $[T_2, T^* - 1]$ where $c_t < w_t + y_1$. None of these phases necessarily must exist, since it could be that $T^* = 0$.
2. A H type LT household starting at T^* has $c_t < w_t + y_1$ except in period T when it has $c_t = w_t + y_1$.
3. For a H type ST household there are two cases:
   (a) With $S > T^*$, their behavior before T^* is similar to the LT case. Their behavior after T^* is also similar, since the household has an even stronger need to save.
   (b) With $S \leq T^*$, there are potentially six phases: a phase $[0, T_1 - 1]$ where $c_t < w_t + y_1$; a phase $[T_1, T_2 - 1]$ where $c_t = w_t + y_1$; a phase $[T_2, S - 1]$ where $c_t < w_t + y_1$; a phase $[S, T_3 - 1]$ where $c_t < w_t + y_1$; a phase $[T_3, T_4 - 1]$ where $c_t = w_t + y_1$; and a phase $[T_4, T^* - 1]$ where $c_t < w_t + y_1$.

C Sample Sizes and Power

As a reminder, the main goal of this experiment is to understand the impacts of a long term UBI on communities in rural Kenya. At this stage, that is at the time of the first follow up survey, we are powered to see impacts of the UBI on outcomes and impacts of the various treatment arms on outcomes relative to the control. However, we are not powered to see differences between the treatment arms, especially more so given that the three arms have received the same amount of money at the time of the first follow up, just in different tranches (lump sum vs monthly) and in terms of the expectations of the future (short term vs long term).

For transparency, we briefly describe the power calculations we conducted prior to the study. Using data collected by one of the authors on this paper in Kenya for two of her previous studies, we looked at the following variables: daily per capita consumption, total assets, the incidence of extreme poverty, occupation of the household head (farming and business), where anyone from the household had migrated the time spent on chores, the time spent on all leisure, and the time spent working.

Appendix Table C summarizes the power calculations. We looked at three different sample sizes of the number of households we would survey within village, 20, 30 or 40. In addition, we looked at 3 different effect sizes, 20%, 25% and 30% (for most of the variables 0.2SD was between the 20% and 25% effect sizes). Appendix Table C shows the range of the lowest number of villages needed in each arm for a given combination of village sample sizes and outcome effect sizes. There is a range as the necessary sample size is different for different outcome variables.

The lowest village sample sizes in the range come from the following variables: whether anyone from the household migrated, the time spent on chores, the time spent on all leisure, and the time spent working. The number of villages needed for all time use variables was very low as the ICC for these variables is very low. The highest village sample sizes in the range come from daily per capita consumption. For two of the variables we looked at, much larger sample sizes were needed for all village sample sizes and effect sizes: total assets and whether the head has a business.

Given these power calculations, and the amount of money GD had raised at the time (a total of $20,527,121), we decided that we were powered for 70-80 short term and lump sum arms. We also expected that we would want to see much larger effect sizes for the long term UBI arm, given its cost, we would need 41 villages to pick up a 30% effect size. This ultimately led to our final village sample size decisions: 44 villages in the long term UBI arm, 80 villages in the short term arm, 71 villages in the lump sum arm and 100 control villages. We maximized the size of the control villages subject to the funding we had raised for data collection.

To give these effect size magnitudes some relevance, here are some relevant benchmarks from the Kenyan context. Between 2008 to 2014, real per capita consumption for non-Nairobi Kenya increased by just over 15%. M-PESA reduced extremely poverty in Kenya over eight years by about 2% (Suri and Jack, 2016). Deworming in Western Kenya raised work hours ten years later by 17% for boys (Baird et al., 2016). Savings account access in Western Kenya raised daily food expenditures for women by

10-20% (Schaner, 2017).

Finally, it is important to emphasize again that we are not powered to detect small cross group comparisons. And we should expect that the differences across groups are not large for most outcomes, given all the arms had received exactly the same amount of money in net present value at the time of our data collection. In our results, any cross group comparisons should be interpreted as just suggestive.

Table C.1: Power Calculations

	Effect Size 20%	Effect Size 25%	Effect Size 30%
Village Size 20	139-36	89-23	62-16
Village Size 30	123-27	79-18	55-13
Village Size 40	115-23	74-15	51-11

D Experimental Integrity

Table D.1: Balance

	Mean	F-statistic	p-value
Household Demographics			
Number of household members	4.92	2.27	0.1
Fraction of males	0.48	0.39	0.67
Household head age	49.14	0.92	0.4
Household experienced hunger	0.85	0.99	0.37
Height for age zscore	-.66	1.3	0.27
Weight for age zscore	-1	2.65	0.07
Social Integration Index	0	0.4	0.67
CES-Depression Scale	19.82	1.52	0.22
Domestic Violence Index	0.01	0.7	0.5
Remittance sent in last 2 months	16.8	1	0.37
Consumption			
Maize in last 7 days (USD PPP)	16.8	1	0.37
Meat in last 7 days (USD PPP)	1.61	3.04	0.05
Outside food in last 7 days (USD PPP)	7.82	0.92	0.4
Non-food consumption in last 30 days (USD PPP)	76.84	0.34	0.71
Education in the last 12 months (USD PPP)	543.31	0.36	0.7
Assets			
Value of assets	19234.53	1.4	0.25
Employment			
Household member employed	0.65	0.95	0.39
Monthly paid employment wages (USD PPP)	167.37	1.02	0.36
Owns a non-ag enterprise	.21	.5	.61
Monthly non-agricultural enterprise sales (USD PPP)	81.95	1.41	0.25
Owns agricultural enterprise	0.73	1.55	0.21
Sold agricultural output	0.48	2.25	0.11
Annual agricultural enterprise sales (USD PPP)	151.74	1.64	0.19
F-test of Joint Significance		0.04	1.00

Table D.2: Survey Completion Rates

	Fall 2019 (1)	Enterprise Survey (2)	Enterprise Census (3)	Follow-up Survey (4)
Long Term Arm	0.007	0.043**	0.001	0.092***
	[0.006]	[0.021]	[0.001]	[0.014]
Short Term Arm	0.010	0.041**	0.002**	0.033**
	[0.007]	[0.018]	[0.001]	[0.013]
Lumpsum Arm	0.018***	0.017	0.001	0.053***
	[0.006]	[0.017]	[0.001]	[0.014]
R-squared	.01	.02	0	.02
Control Mean	0.97	0.86	1.00	0.83
p-value: ST = LT	0.57	0.91	0.28	0.00***
p-value: ST = LS	0.11	0.21	0.68	0.11
p-value: LS = LT	0.05*	0.26	0.53	0.01**
p-value: LT = ST = LS = 0	0.03**	0.10*	0.06*	0.00***
Observations	8723	3181	17162	8723

Table D.3: Composition of Attritors

	F-Stat	p-Value
Household Demographics		
Number of household members	0.64	0.53
Fraction of males	0.84	0.43
Household head age	0.79	0.46
Household experienced hunger	0.61	0.55
Height for age zscore	4.42	0.01
Weight for age zscore	1.15	0.32
Social Integration Index	0.65	0.52
CES-Depression Scale	3.73	0.03
Domestic Violence Index	0.93	0.39
Remittance sent in last 2 months (USD PPP)	2.07	0.13
Consumption		
Maize in last 7 days (USD PPP)	1.48	0.23
Meat in last 7 days (USD PPP)	2.27	0.1
Outside food in last 7 days (USD PPP)	2.31	0.1
Non-food consumption in last 30 days (USD PPP)	1.06	0.35
Education in the last 12 months (USD PPP)	0.35	0.71
Assets		
Value of assets (USD PPP)	1.64	0.2
Employment		
Household member employed	0.99	0.37
Monthly paid employment wages (USD PPP)	0.02	0.98
Owns a non-ag enterprise	0.19	0.83
Monthly non-agricultural enterprise sales (USD PPP)	2.55	0.08
Owns agricultural enterprise	1.42	0.24
Sold agricultural output	0.15	0.86
Annual agricultural enterprise sales (USD PPP)	1.81	0.17
F-test of Joint Significance	1.18	0.14

Table D.4: Adherence

	Fall 2019		Summer 2020	
	Last 40 days	Amount recd	Last 40 days	Amount recd
	(1)	(2)	(3)	(4)
Long Term Arm	0.91***	1945***	0.83***	2595***
	[0.01]	[52.8]	[0.03]	[70.5]
Short Term Arm	0.91***	1923***	0.01*	2222***
	[0.01]	[31.6]	[0.01]	[34.4]
Lumpsum Arm	0.01	2140***	0.00	2150***
	[0.01]	[41.9]	[0.00]	[43.7]
R-squared	.86	.54	.79	.54
Control Mean	0.00	0.00	0.00	0.00
p-value: ST = LT	0.90	0.63	0.00***	0.00***
p-value: ST = LS	0.00***	0.00***	0.04**	0.13
p-value: LS = LT	0.00***	0.00***	0.00***	0.00***
Observations	8456	8510	8456	8510

E Supplemental Exhibits

Table E.1: Consumer Price Index (CPI) - 2 km Radius

	Log(CPI) - Study Shares (Overall) (1)	Log(CPI) - Numbers (Overall) (2)	Log(CPI) - Study Shares (Ag) (3)	Log(CPI) - Numbers (Ag) (4)	Log(CPI) - Study Shares (Non-Ag) (5)	Log(CPI) - Numbers (Non-Ag) (6)
Overall Price Effects	0.094	0.105	0.129	0.167	0.177	0.296*
	[.104]	[.151]	[.092]	[.131]	[.161]	[.165]
Long Term Arm	0.212	0.001	0.336**	0.002	0.243	0.000
	[0.181]	[0.002]	[0.158]	[0.001]	[0.274]	[0.002]
Short Term Arm	-0.103	-0.001	-0.012	-0.000	0.347	0.003**
	[0.201]	[0.002]	[0.184]	[0.001]	[0.252]	[0.002]
Lumpsum Arm	0.331**	0.002**	0.299**	0.003**	0.170	0.002
	[0.163]	[0.001]	[0.138]	[0.001]	[0.257]	[0.001]
# Non-Study Households	0.001*	0.001*	0.001	0.001	-0.001	-0.001
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
# Study Households	-0.000	-0.001	-0.000	-0.001	0.000	-0.001
	[0.000]	[0.001]	[0.000]	[0.001]	[0.000]	[0.001]
R-squared	0.1	0.089	0.093	0.106	0.051	0.077
Observations	93	93	93	93	93	93

Some markets did not have any households within a 2 km radius. Regressors in column (1) refer to the share of respective treatment households in the study sample in the 2 km radius. Regressors in column (2) refer to the number of treatment households in the study sample in the 2 km radius. Items which were sold in less than half the markets have been dropped. Items in the consumer price index include: fertilizer (DAP), soap, washing powder, toothpaste, vaseline, batteries, maize flour, wheat flour, eggs, cooking fat, sugar, bread, biscuits, soda, oranges, mangoes, kerosene, white grain, rice, beans, beef, slippers, paracetamol, kale, onions, tomatoes, potatoes, and cabbage. Items in the agricultural price index include: fertiliser (DAP), maize flour, wheat flour, eggs, milk, cooking fat, sugar, tea leaves, bread, oranges, mangoes, white grain, rice, beans, beef, kale, onions, tomatoes, potatoes, and cabbage. Items in the non-agricultural price index include: soap, washing powder, slippers, toothpaste, vaseline, batteries, bread, cakes, soda, biscuits, kerosene, and paracetamol. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table E.2: Anthropometrics: Drill-Down

	Anthro. Index (1)	Ht. for Age (2)	Wt. for Age (3)	BMI for Age (4)	BMI (5)	Underwt. (6)	Stunted (7)
Long Term Arm	0.00	-0.04	0.04	0.08	0.18**	0.00	0.00
	[.04]	[.04]	[.04]	[.06]	[.09]	[.01]	[.01]
Short Term Arm	0.07*	0.06	0.08*	0.06	0.09	-0.02	-0.01
	[.04]	[.05]	[.04]	[.05]	[.09]	[.01]	[.01]
Lumpsum Arm	0.01	0.05	-0.02	-0.08	-0.10	0.00	-0.01
	[.04]	[.04]	[.05]	[.05]	[.09]	[.01]	[.01]
R-squared	0.14	0.09	0.11	0.06	0.17	0.05	0.03
Control Mean	-0.54	-0.36	-0.72	-0.73	15.54	0.15	0.12
p-value: ST = LT	0.15	0.05**	0.47	0.75	0.29	0.17	0.12
p-value: ST = LS	0.13	0.88	0.02**	0.00***	0.01**	0.05*	0.92
p-value: LS = LT	0.94	0.07*	0.23	0.00***	0.00***	0.97	0.10
p-value: LT = ST = LS = 0	0.29	0.17	0.09*	0.00***	0.01**	0.21	0.19
Observations	10267	10267	10267	10174	10267	10267	10267

Anthropometric index is the average of the weight-for-age and height-for-age z-scores of children aged 2-12 years at baseline. Height-for-age, weight-for-age, and BMI-for-age are all z-scores from standardized nutition indicator charts. In column (5), the regression for BMI (which is calculated as the weight in kg over the square of height in meters) includes controls for age and age squared. Underweight is a dummy for weight-for-age z-score less than -2 s.d. Stunted is a dummy for height-for-age z-score less than -2 s.d. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Table E.3: Transfers, Remittances and NGO Presence

	Transfers		Remittances		Total NGOs	GD	IPA	Other
	Informal & Formal Taxes Paid	Received	Sent	Received				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Long Term Arm	-22.2	7.82	9.75	-2.75	0.74***	0.72***	0.04	-0.03
	[13.7]	[11.5]	[5.99]	[8.39]	[.19]	[.07]	[.08]	[.14]
Short Term Arm	13.6	14.5	8.60	11.8	0.30*	0.62***	-0.07	-0.25**
	[18.1]	[10.5]	[5.48]	[10.3]	[.17]	[.06]	[.07]	[.12]
Lumpsum Arm	26.1	-4.58	17.0**	6.90	0.17	0.47***	-0.10	-0.19*
	[23.1]	[6.58]	[6.6]	[9.15]	[.16]	[.07]	[.07]	[.11]
R-squared	0.02	0.01	0.06	0.03	.27	.45	.35	.34
Control Mean	70.1	37.6	67.6	76.6	1.06	0.09	0.41	0.56
p-value: ST = LT	0.02**	0.65	0.84	0.13	0.03**	0.21	0.19	0.09*
p-value: ST = LS	0.64	0.06*	0.21	0.65	0.45	0.05*	0.69	0.54
p-value: LS = LT	0.03**	0.26	0.28	0.22	0.00***	0.00***	0.10*	0.19
p-value: LT = ST = LS = 0	0.02**	0.22	0.07*	0.33	0.00***	0.00***	0.26	0.10
Observations	8473	8479	8509	8506	294	294	294	294

Number of NGOs refers to NGOs running programs in the village in the last 12 months. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table E.4: Wage Rates - Agricultural and Non-Agricultural

	Wages (VE)		Missing Wages (VE)		Accepted Wage	
	Ag	Constr.	Ag	Constr.	Off Season	Main Season
	(1)	(2)	(3)	(4)	(5)	(6)
Long Term Arm	1.10***	3.99**	0.08	-0.06	0.14*	0.18**
	[.41]	[1.88]	[.09]	[.09]	[.07]	[.08]
Short Term Arm	1.00**	2.23**	-0.01	-0.08	0.04	0.02
	[.43]	[1.1]	[.07]	[.08]	[.06]	[.07]
Lumpsum Arm	0.50	3.11**	0.11	-0.09	0.04	0.14**
	[.36]	[1.33]	[.08]	[.07]	[.05]	[.06]
R-squared	.32	.26	.28	.21	0.02	0.02
Control Mean	5.72	9.92	0.39	0.41	4.83	6.97
Control Median	4.96	9.91	0.00	0.00		
p-value: ST = LT	0.84	0.40	0.30	0.87	0.18	0.07*
p-value: ST = LS	0.34	0.53	0.10	0.95	0.98	0.04**
p-value: LS = LT	0.17	0.68	0.77	0.82	0.18	0.67
p-value: LT = ST = LS = 0	0.02**	0.02**	0.33	0.63	0.28	0.02**
Observations	172	187	294	294	8480	8480

Data for columns (1), (2), (3), and (4) comes from the village elder survey. Ag wage refers to the average wages paid for land preparation, planting, and weeding respectively. Not included wages for harvesting and post-harvest activities since many villages did not hire labor for these activities. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table E.5: Lumpy Investments (>5,000 KSh)

	Any (1)	Household (2)	Ag Enterprise (3)	Non-Ag Enterprise (4)	Any Home Improvement (5)	Value (6)
Long Term Arm	0.09***	0.08***	0.01**	0.01***	0.05***	25.94
	[.02]	[.02]	[.01]	[.01]	[.02]	[37.38]
Short Term Arm	0.11***	0.10***	0.01**	0.01	0.06***	67.03**
	[.02]	[.02]	[.01]	[0]	[.02]	[31.99]
Lumpsum Arm	0.09***	0.07***	0.01**	0.01*	0.04**	87.24**
	[.02]	[.02]	[.01]	[0]	[.02]	[39.35]
R-squared	0.03	0.03	0.02	0.01	0.02	0.01
Control Mean	0.25	0.22	0.03	0.02	0.20	183.92
p-value: ST = LT	0.33	0.30	0.61	0.10*	0.60	0.41
p-value: ST = LS	0.25	0.13	0.57	0.69	0.45	0.64
p-value: LS = LT	0.84	0.61	0.98	0.35	0.79	0.23
p-value: LT = ST = LS = 0	0.00***	0.00***	0.02**	0.04**	0.00***	0.06*
Observations	8365	8365	8365	8365	8365	8365

Outcome variables indicate whether the household undertook a lumpy investment (>5,000 KSh) in the last 12 months. Home improvement refers to any expenditure or investment in home renovation, construction, and repair. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table E.6: Village and Household Size

	Household Size						Migration		Village Size
	Members	Arrivals	Deaths	Adults	Children	Births	Households	Adults	Households
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Long Term Arm	0.07	0.07*	-0.01	0.02	0.05	0.03**	0.00	-0.00	4.30
	[.05]	[.04]	[.01]	[.03]	[.04]	[.02]	[.01]	[.01]	[3.04]
Short Term Arm	0.05	0.08**	0.01*	0.03	0.03	0.01	-0.00	-0.02	0.09
	[.05]	[.04]	[.01]	[.03]	[.03]	[.01]	[0]	[.01]	[2.68]
Lumpsum Arm	0.12**	0.10**	-0.01	0.05*	0.07**	0.02*	-0.01	-0.01	7.94***
	[.05]	[.04]	[.01]	[.03]	[.03]	[.01]	[.01]	[.01]	[2.81]
R-squared	0.65	0.01	0.01	0.54	0.7	0.03	0.02	0.18	.21
Control Mean	5.18	0.80	0.06	2.66	2.52	0.21	0.03	0.20	59.25
p-value: ST = LT	0.80	0.92	0.00***	0.88	0.66	0.33	0.69	0.23	0.19
p-value: ST = LS	0.19	0.66	0.00***	0.47	0.19	0.46	0.39	0.44	0.01***
p-value: LS = LT	0.35	0.57	0.78	0.34	0.60	0.59	0.21	0.78	0.27
p-value: LT = ST = LS = 0	0.08*	0.02**	0.00***	0.31	0.10	0.07*	0.56	0.50	0.02**
Observations	8480	8480	8547	8480	8480	8480	8723	8480	294

All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table E.7: Fertiliser and Livestock: Costs

	Livestock		Farm		Fertiliser Use		
	Input Costs (1)	Labor (Days) (2)	Input Costs (3)	Labor (Days) (4)	Y/N (5)	Cost (6)	Quantity (7)
Long Term Arm	28.87	-4.03	20.22	2.56	0.06**	8.15	-3.32
	[31.22]	[13.5]	[35.75]	[4.88]	[.03]	[5.48]	[3.64]
Short Term Arm	16.58	30.09*	67.48*	11.25**	0.05**	9.59*	10.45
	[18.43]	[16.14]	[35.47]	[4.73]	[.02]	[5.54]	[7.24]
Lumpsum Arm	39.79	11.36	46.89	6.83	0.07***	4.55	-2.58
	[24.99]	[11.97]	[37.41]	[4.34]	[.02]	[3.58]	[5.31]
R-squared	0.03	0.06	0.02	0.05	0.22	0.03	0.01
Control Mean	191.19	328.25	325.72	83.05	0.53	40.62	25.21
p-value: ST = LT	0.65	0.05**	0.23	0.17	0.63	0.82	0.10*
p-value: ST = LS	0.23	0.19	0.57	0.41	0.25	0.30	0.20
p-value: LS = LT	0.69	0.26	0.45	0.46	0.68	0.43	0.87
p-value: LT = ST = LS = 0	0.46	0.21	0.28	0.09*	0.00***	0.28	0.40
Observations	8396	8480	8396	8480	8480	8480	8068

Col (1) refers to total input costs for livestock which includes green fodder feed, crop residue, hay, vaccinations, medical care, salt, and wages paid out to hired labor. Col (2) refers to total labor days (both hired and family labor) in livestock. Col (3) refers to total input costs for farming which includes the cost of land preparation, seed, fertiliser, transport, hired labor, and rent on land. Col (4) refers to total labor days (both hired and family labor) in farming. Col (5) refers to whether or not a household used fertilizers, while Col (6) and Col (7) refer to the cost and the quantity of fertilizer used. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table E.8: Amenities Index

	Amenities Index		
	Overall (1)	Community Services (2)	Local Economic Activity (3)
Long Term Arm	0.07	0.06	0.08
	[.07]	[.07]	[.1]
Short Term Arm	0.09*	0.13**	0.05
	[.06]	[.06]	[.08]
Lumpsum Arm	0.16**	0.12*	0.21*
	[.07]	[.06]	[.11]
R-squared	.2	.18	.24
Control Mean	-0.00	-0.00	-0.00
p-value: ST = LT	0.75	0.38	0.73
p-value: ST = LS	0.29	0.88	0.12
p-value: LS = LT	0.24	0.46	0.28
p-value: LT = ST = LS = 0	0.16	0.12	0.30
Observations	294	294	294

Amenities index is calculated as a normalized mean of the count of both unpriced and priced amenities (community services and local economic activity) in the village. Community services include: water points, feeder roads, bridges, clinics, dispensaries, other health centres, schools, other education centres, and other public facilities (cattle dips, tea buying centres, etc.). Local economic activity measures include: kiosks, permanent markets, periodic markets, M-PESA agents, tailors, pharmacists, hotels, and transport enterprises. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table E.9: Non-Agricultural Enterprise Aggregates

	Retail Trade		Manufacturing		Transportation		Services	
	# Enterprises (1)	Net Revenues (2)	# Enterprises (3)	Net Revenues (4)	# Enterprises (5)	Net Revenues (6)	# Enterprises (7)	Net Revenues (8)
Long Term Arm	3.89***	1601.42*	0.02	51.90	0.53*	100.76	0.23	198.64
	[1.28]	[824.74]	[.27]	[120.79]	[.29]	[85.37]	[.33]	[205.6]
Short Term Arm	2.34**	464.60*	0.03	17.82	-0.12	-3.85	-0.04	70.95
	[.95]	[273.59]	[.21]	[133.58]	[.18]	[48.33]	[.29]	[218.2]
Lumpsum Arm	4.24***	770.01**	0.28	254.11	0.38	101.00	0.45	718.68
	[1.03]	[338.12]	[.23]	[221.06]	[.29]	[61.26]	[.38]	[440.08]
R-squared	.48	.17	.16	.23	.23	.22	.26	.29
Control Mean	11.05	1356.55	0.89	185.99	0.94	136.04	1.56	233.14
p-value: ST = LT	0.23	0.21	0.95	0.78	0.03**	0.15	0.38	0.54
p-value: ST = LS	0.07*	0.41	0.28	0.25	0.08*	0.09*	0.14	0.13
p-value: LS = LT	0.78	0.36	0.33	0.22	0.68	1.00	0.53	0.12
p-value: LT = ST = LS = 0	0.00***	0.01**	0.61	0.67	0.08*	0.20	0.49	0.44
Observations	294	294	294	294	294	294	294	294

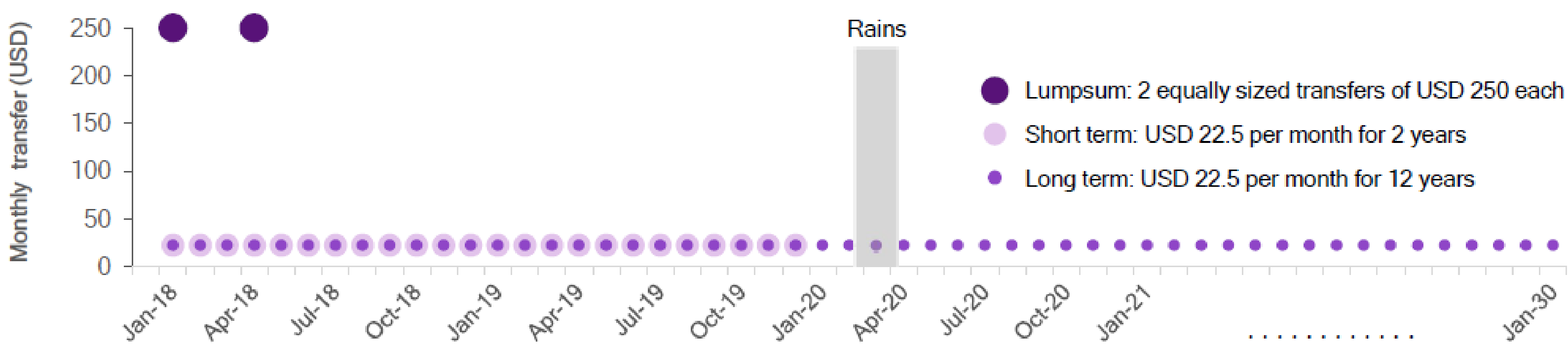
Enterprises with missing data assumed to be zero for the purpose of this analysis. Data in this table comes from the enterprise census. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10$,$^{**}p < .05$,$^{***}p < .01$.

Table E.10: Alcohol

	Any Drink Last Night (1)	No Drinks Last Week (2)	Villagers who Drink Daily (3)	Problem, This Community (4)	Net Revenues (5)
Long Term Arm	-0.00	-0.02	-2.30**	-0.24***	53.46*
	[.01]	[.07]	[.97]	[.08]	[29.78]
Short Term Arm	-0.01	-0.04	-1.21	-0.16**	8.70
	[.01]	[.04]	[.79]	[.08]	[13.75]
Lumpsum Arm	0.00	0.01	-1.94**	-0.15*	46.45*
	[.01]	[.05]	[.81]	[.08]	[27.88]
R-squared	0.01	0.01	0.04	0.15	0.01
Control Mean	0.06	0.28	9.34	3.37	23.46
Control Median	0.00	0.00	5.00	4.00	0.00
p-value: ST = LT	0.59	0.81	0.16	0.34	0.14
p-value: ST = LS	0.39	0.25	0.27	0.87	0.20
p-value: LS = LT	0.88	0.65	0.62	0.32	0.84
p-value: LT = ST = LS = 0	0.82	0.68	0.07*	0.03**	0.20
Observations	7519	7511	7096	7420	8195

Column (1) refers to whether the respondent had a drink the previous night. Column (2) refers to the number of drinks consumed in the last week. Column (3) refers to the number of individuals in the village the respondent thinks drink daily. Column (4) refers to the respondent's perception of alcohol as a problem within the community on a scale of 1-5. Column (5) shows net revenues of enterprises brewing or selling alcohol, and data for this column comes from the enterprise survey.

Figure E.1: Timeline



Notes: This figure plots the evolution of transfer payments for a prototypical household whose transfers began at the very start of the study.