Youth Employment and Productivity: Medium-term Experimental Evidence from Rwanda

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Abstract

We present the results of a 3.5-year followup on a randomized experiment benchmarking a workforce training program against cash transfers. Examining self-employment outcomes in a sample of poor and underemployed youth, this study measures the impact of the training program relative not only to a control group, but also to the counterfactual of simply disbursing the cost of the program directly to beneficiaries in cash. We continue to find impacts of the job training program on time use, productive assets, and business knowledge, while the cash transfers have strong continued effects on productive assets, livestock values, savings, and subjective well-being. Both interventions enhance the likelihood that individuals operate businesses and the sales in those businesses, with large cash transfers sustaining strong improvements in business profits more than three years after disbursement. Nonetheless, impacts have faded by roughly one-half compared to what was seen at the 18-month midline, making most endline comparisons at cost-equivalent levels statistically insignificant. Estimated consumption effects are attenuated by approximately one third, and are statistically significant relative to control at the 10 percent level only in a combined arm that received both cash transfers and HD. Our results suggest that these interventions lead to modest medium-term improvements in the well-being of participants, but that impacts achieved at midterm were not robust to economic shocks in either the job-training or cash-transfer arms. Deeper reforms may be necessary to allow self-employment to provide a transformative pathway out of poverty.

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Executive Summary

This report gives the medium-term results of a study designed to compare a youth workforce readiness intervention against cash transfers in Rwanda. The workforce program is called Huguka Dukore Akazi Kanoze (meaning 'get trained and get to work/work well done' in Kinyarwanda); it is is a five-year project (2017-2021) aimed at providing 40,000 vulnerable youth with employability skills. The program targets youth ages 16–30 from poor households with less than secondary education, with an emphasis on women and youth with disabilities and those living in rural areas. Huguka Dukore aims improve workforce readiness through education, training, and on-the-job training or internship experiences. Each of the three components of the program lasts 10 weeks, consisting of i) workforce readiness preparation; ii) individual youth entrepreneurship and microenterprise start-up; and iii) technical training for specific trades, after which trainees may be placed in apprenticeships. The program builds on lessons learned from the precursor program, the USAID-supported Akazi Kanoze Youth Livelihoods Project, also implemented by the Education Development Center. The unconditional cash grants were provided as lump-sum transfers, in two installments, via mobile money by the U.S. non-profit GiveDirectly.

Methodology

We designed a randomized controlled trial (RCT) to measure the impact of Huguka Dukore relative to cash grants at comparable cost to the funder. The agreed-upon primary outcomes of the study are i) beneficiary employment status, ii) time use, iii) beneficiary income, iv) household consumption, and v) productive assets. Secondary outcomes include measures of psychological welfare, household wealth, and cognitive and non-cognitive skills. Impacts presented here come from an endline survey conducted three and half years after the interventions began.

The study sample is comprised of the types of poor, underemployed youth who are targeted by Huguka Dukore, and who expressed willingness to enroll in that training program at baseline, and who met the technical poverty criteria to be eligible for Give Directly funding. Public lotteries were used to randomly assign the youth into five groups, receiving either:

1. Huguka Dukore;
2. Cash transfers only (three smaller cash amounts intended to bracket the cost of Huguka Dukore);
3. Cash transfers and Huguka Dukore combined (to test if the interventions complement each other);
4. A larger cash grant (which happened to be roughly equal to the cost of the combined arm, or about $845);
5. Control group, in which no program was offered at the time of study.

The Huguka Dukore program ended up being less expensive than anticipated at the design phase, meaning that all of the cash transfer amounts were more expensive than Huguka Dukore.

The cost of Huguka Dukore was $338, the smallest of the cash arms cost $394, and the other cash arms cost $494, $590, and $847. By chance, the cost of the Combined arm was $886, very close to the cost of the largest cash arm. The study uses a pre-specified regression adjustment of costs to compare Huguka Dukore to cash at a comparable cost.

The baselines for the study were conducted during December of 2017 and January of 2018, Huguka Dukore began treatment began February 2018, and Give Directly began treatment May of 2018. This endline activity was conducted in October and November of 2021. Hence the endline is 3 years 8 months after HD started, 3 years 6 months after GD started, and 3 years 10 months after baseline.

Findings

- Both interventions saw endline benefits that are roughly half of the benefits generated at the 18-month midline.
- Given this overall fade in impacts, none of the primary or secondary outcomes are significantly different between Huguka Dukore and cash at the 95% level.
- As in the midline, we find no evidence of complementarities (better effects from providing Huguka Dukore and cash together than we would expect from providing them each separately), or of spillovers from the interventions onto others in the study.
- Both interventions had a consistent effect across richer and poorer, male and female, older and younger, and across local labor market conditions.
- 78% of control individuals in the study report that COVID led to a negative shock to their income, and there is a dramatic deterioration in productive assets and business ownership between the midline and endline (the COVID era) in the control. The results presented here therefore inherently blend the 'business as usual' impacts of the programs with their effects on providing resilience to this shock.
- The treatments caused youth to 'stick their necks out' in self-employment prior to COVID, so while the treatment groups lost more than the control in the shock, because they entered the lockdowns with more assets, they were still better off than they would have been without treatment and with the COVID shock.
- Taken across the midline and endline, cash appears to have had a larger overall effect than the training program on the final outcomes of income, consumption, and subjective well-being. The takeaway is that cash should be considered as a component of programs whose intent is to generate short- to medium-term economic welfare.

Huguka Dukore versus Control:

- Youth were working 3.3 more hours per week in productive activities, compared to 19.4 in the control, significant at the 90% level.
- Productive assets remained 92% higher than the control group average, also significant at the 90% level.

- Huguka Dukore elevates the probability that individuals are working full-time (40 hours per week) by 6 percentage points over the control group rate of 14 percent.
- Beneficiaries were 8 percentage points less likely to be doing agricultural wage labor, 11 percentage points more likely to be running businesses, and had average daily business sales that were $2.12 higher than the control group at endline.
- Knowledge of the business practices trained in Huguka Dukore remained 0.26 standard deviations higher than the control, significant at the 99% level.

Cash versus Control:

- Cash recipients continued to see elevated values for productive assets, ranging from 139% of the control group in the smallest arm to 297% in the largest arm.
- Impacts on income and consumption, very significant at midline, had largely faded to insignificance by endline after accounting for multiple-inference corrections, though point estimates for individuals receiving at least the mid-sized transfer represent an approximate 20 percent increase in consumption relative to control, which is statistically significant at the 10 percent level even after multiple-inference correction.
- The larger cash transfers amounts were still generating significant improvements in subjective well-being, livestock value, and savings at endline.
- Cash transfers continue to drive strong improvements in beneficiary-run business outcomes at endline; even in the small arm individuals own .17 more businesses, work 3 days more per month, see $6.54 more daily sales, and realize monthly profits that are $3.30 larger than the control arm.
- Large cash amounts elevate the rate at which men marry and have children, but do not have this effect for women.
- Between a half and 85% of the original amount transferred in cash remains in elevated asset wealth after almost four years.
- The 'multiplier effect' of cash transfers (the total income generated as a fraction of the amount transferred) varies from 1.3 (upper arm) to 2.4 (middle arm).

1 Introduction

Sub-Saharan Africa combines a rapidly growing population with low formal-sector employment, meaning that future economic growth will be largely dependent on enhancing productivity in the informal sector (Bandiera et al., 2022). In this context, few questions have greater long-term import than how best to help the burgeoning young population achieve a successful transition into a productive adulthood (Bongaarts, 2016; Fox et al., 2016). The best means to achieve this are anything but clear, however. While skills are almost certainly a constraint for a population with the lowest average schooling levels in the world, entrepreneurship and job training programs have an uneven record in contexts with little formal employment (Kluve et al., 2017; McKenzie, 2021). Credit constraints also certainly play a role, but while a large literature has shown that cash transfers are invested in productive assets in the short term (Blattman et al., 2013; Gertler et al., 2012; Haushofer and Shapiro, 2016; De Mel et al., 2012), the ability of transfers to affect durable improvements in productivity is more uncertain (Aizer et al., 2016; Baird et al., 2019; Balboni et al., 2019; Blattman et al., 2018; Brudevold-Newman et al., 2017; Hoynes et al., 2016). More broadly, it is possible that macro-level constraints to demand or to the scope for business expansion fundamentally limit the extent to which the informal sector can provide a pathway out of poverty (La Porta and Shleifer, 2014).

The effort to help youth make productive transitions into adulthood is inherently a long-term agenda. We cannot move the needle on long-term productivity question with interventions that have only palliative, short-term impacts (Bouguen et al., 2019). The existing empirical evidence base on labor market interventions is largely short-term, making it clear that they can drive asset ownership, entrepreneurship, and employment over a one to two year time frame, but few studies have been able to track these outcomes experimentally over a longer period of time (exceptions include Blattman et al. (2020, 2022)). Serious questions about the durability of the informal sector as a pathway to long-term security have been raised by the COVID epidemic, which has dealt a huge shock to self-employed individuals whose income streams are vulnerable to lock-downs and who work without access to employer-based safety nets (Egger et al., 2021; Mahmud and Riley, 2021). Since the ability to affect long-term impacts requires the ability to weather shocks, the durability of impacts through the COVID epidemic speak both to the dynamics of wealth accumulation and also to the resiliency of different forms of shocks to wealth.

We contribute to this conversation with a study providing a multifaceted window on how best to raise the productivity of vulnerable youth, in this case under-employed 18–25 year olds in Rwanda. Our study is a randomized controlled trial with one arm providing an intensive year-long vocational training, one arm providing unconditional cash transfers, and an arm that receives both of these interventions at the same time. Randomization of cash transfer amounts provides the ability to make cost-equivalent comparisons between cash and kind, as well as to form a rich set of counterfactuals for the complementarity arm that receives both interventions. We follow up with subjects three years after the interventions were completed, and have a permanently untreated control group so

the study faces no internal contamination. Tracking rates in the study were a remarkable 98.6%, and a relatively even split of male and female subjects allows us to speak to the differential gender dimension interventions. This environment provides an unusually rich environment in which to consider the medium-term impact of programs that support youth productivity in the African context.

We find evidence of durable impacts from both interventions. *Huguka Dukore Akazi Kanoze* (the workforce training program, henceforth HD) continues to elevate productive hours per week by 3.3, productive assets are almost twice the control group, and an index of business knowledge is higher by 0.25 standard deviations even three year later. The cash arms, implemented by US non-profit GiveDirectly (henceforth GD) led to durable increases in productive assets (between 1.4–3 times the control), subjective well-being, household livestock value, and savings, along with modest and insignificant increases in consumption per capita (10-20% above the control group). As was the case in the one-year evaluation results from this study (McIntosh and Zeitlin, 2022), we find no evidence of complementarity; the combined arm demonstrates the impacts seen in either arm with no additional benefits arising from them being implemented together. Most of these outcomes represent a 'fade' of about 50% relative to the impacts seen in the midline study, meaning that roughly half of the benefit observed after one year is still present more than three years later. Likely due to this overall diminution in the magnitude of results we find little evidence of significant difference between programs at cost-equivalent level; HD is marginally better in producing business knowledge and other than this we fail to reject differences across arms.

A careful focus in our surveys on time use and entrepreneurship allows us to provide substantial nuance to the analysis of the ways in which these interventions alter their productive activities. Both interventions decrease participation in agricultural wage labor; the workforce training program weakly pushes individuals into non-agricultural wage labor (5 pp impact), and cash transfers, particularly large ones, drive income in micro-enterprise and particularly non-agricultural self-employment. These sectoral shifts prove quite constant over time despite the income benefits of the shifts fading after three years. Both interventions lead to a burst of new business formation over the shorter term; as of the midline the control group had created an average of 0.5 new businesses per person, HD elevated this by 0.2, and the cash arms by 0.5–0.6 new businesses per beneficiary on average. The rate of new business creation between midline and endline in the control slows to 0.24, and only the GD Large treatment leads to additional new businesses during this interval. A sizeable fraction of the businesses created at midline die by endline (0.24 in the control group) but this is not more likely in any of the treatment arms. Many midline businesses are reported as extant but inoperative at endline (0.14 in the control group), and here we see elevated rates for the treatment arms (≈ 0.15 for the cash arms), suggesting that roughly one third of the businesses created with the cash transfers do not continue to operate three years later. Nonetheless, both programs have sizeable effects on entrepreneurship at endline, with working days, sales, and profits being higher than the control for both training and cash, and profits for the larger cash arms being more than double the control group on average.

The interventions also affect the transitions to adulthood in some subtle but important ways. Overall there is a remarkable similarity of the final economic impacts of the programs on male and female beneficiaries, suggesting that gender is not exerting a constraint on entrepreneurship within the group of motivated individuals who had overcome the barriers to enroll themselves as eligible for this study. Nonetheless, we uncover clear evidence of the ways in which lack of income inhibits marriage for males; while no intervention drives cohabitation rates, the larger cash arms have a strong effect on increasing marriage, and only for men. Fertility rises in line with marriage rates, again only for men and only for the larger cash arms. Desired lifetime fertility, on the other hand, shows a sharp decrease from cash transfers, and only for women, indicating that the substantial changes in female entrepreneurship and time use induced by cash transfers do alter the way that they think about family size in line with the predictions of Becker (1965).

Our results should be read in light of some important contextual factors. First, the study takes place largely in rural areas and so many micro-enterprises are typically engaged with agriculture in some way. Whether such interventions could have more transformative effects in an urban context with larger demand pools remains an open question. Second, Rwanda is a tightly governed, rapidly growing country. While in some ways that means that this study likely represents a 'best-case' scenario for such interventions, it is also the case that the three COVID lock-downs imposed in the two years prior to our endline were unusually strongly enforced, and may have hit small businesses harder than in more loosely governed countries. The impact of the COVID era on the overall business climate can be seen in our control group: while employment status, consumption, and consumption appear to have been protected over the course of the pandemic, control households have dramatically stripped productive assets, losing approximately 63 percent of the value of the assets they held at midline. Hence the exigencies of this unusual time are an inextricable part of what this study has to say about long-term impacts.

This study makes several contributions to the literature. First, we deepen the evaluation literature by expanding the set of questions that can be asked with rigorous comparative analysis. Ours is the first study to be able to conduct a rigorously cost-equivalent comparison of two programs over such a long time frame.[1] Given the variation in cash transfer amounts we can examine medium-term impacts both allowing the program to change and holding costs constant (cost-equivalence) or allowing program cost to change across modalities (cost-effectiveness). Finally, because the large cash transfer arm has a cost almost identical to the combined arm that gets both interventions, we can create multiple counterfactuals for the complementarities analysis and ask both whether the combination is differentially effective, and whether the combination is better than the cost of the combination given all in cash. This suggests several ways of using the ready scalability of cash transfers to create transparent, policy-relevant comparisons.

[1]The most common form of benchmarking in the literature is the comparison of food aid to cash aid (Ahmed et al., 2016; Cunha et al., forthcoming; Hidrobo et al., 2014; Hoddinott et al., 2014; Leroy et al., 2010; Schwab et al., 2013). Efforts to benchmark more complex, multi-dimensional programs to cash include BRAC's Targeting the Ultra-Poor program (Chowdhury et al., 2016), microfranchising (Brudevold-Newman et al., 2017), and graduation programs (Sedlmayr et al., 2020).

Second, by providing a clean and well-powered window on the impact of training and cash in a relatively long-term time frame, this study makes a critical contribution to our understanding of the durability of these interventions. Much long-term literature on the impact of cash programs looks at CCTs, which have a pathway to impact either through human capital or the transfers themselves (Araujo et al., 2017; Barham et al., 2014; Fernald et al., 2009). The long-term literature on income support programs in developed countries illustrates potentially transformative effects on schooling, health, income, and life expectancy (Aizer et al., 2016) and increases in economic self-sufficiency (Hoynes et al., 2016). Fewer studies have looked at the long-term impact of unconditional transfers in the developing context, but it is far from clear that these impacts are durable, with a number of RCTs showing dissipating long-term benefits (Araujo et al., 2017; Baird et al., 2019; Brudevold-Newman et al., 2017). A a long-term study in neighboring Uganda providing cash grants to groups to start businesses showed dissipation of impacts by 9 years from the intervention, with some lasting effects on assets and skilled work (Blattman et al., 2020). For both short- and long-term studies the training literature has returned mixed results (Heckman et al., 1999; McKenzie, 2021), with long-term studies showing some durable impact on formal employment and earnings in the Dominican Republic (Ibarrarán et al., 2019). Particularly in the presence of negative economic shocks, it is therefore an open question whether investments in human capital will prove more durable than investments in physical capital enabled by cash transfers.

Finally, the study speaks on a structural level to the constraints that exist to the creation of durable income increases in the informal sector. On the one hand, our results confirm a literature showing that skills matter (Kluve et al., 2017), and that credit constraints matter (Beaman et al., 2014). They do not suggest there is any special issue at the intersection of credit and human capital constraints that rewards a simultaneous relaxation of these two obstacles. On the other hand, neither intervention alone, nor the two together, appears capable of delivering a really meaningful escape from poverty over a 3–4 year time frame in this population. The depressing conclusion of this is that even high-cost interventions may struggle to achieve transformative impacts for vulnerable youth over the longer term. The more expensive interventions in this study cost approximately $750 per individual, surely more than most development agencies willing/able to spend, and still do not lead to meaningful decreases in consumption-based poverty after 3.5 years. A possible reading of this is that we need to think more carefully about interventions that relax constraints on the informal sector as a whole (infrastructure, titling, legal reforms, sector-wide technological investments) rather than investing in individuals while treating these broader capacity constraints as fixed.

The remainder of the paper is organized as follows: Section 2 presents the design of the experiment and the approach to comparative costing, Section 3 gives the core experimental results on the pre-registered primary and secondary outcomes, Section 4 examines the dynamics of the transition into adulthood and catalogs the nature of the covid shocks experienced since midline, Section 5 looks at spillovers and accounts for the total cash flows experienced since treatment, and Section 6 concludes.

2 Design

2.1 Interventions

Huguka Dukore: Employment and entrepreneurship readiness training

Huguka Dukore Akazi Kanoze (meaning 'get trained and get to work/work well done' in Kinyarwanda) is a five-year program that in all has provided 40,000 vulnerable youth with increased opportunities for wage and self-employment through a suite of interventions that includes work readiness training, employability skills training, work based learning, internship opportunities, links to employment, and entrepreneurship training at the youth level. The program builds on lessons learned from EDC's prior work in this area through the predecessor Akazi Kanoze Youth Livelihoods Project.

The core of the HD program consists is three sequential modules taken serially over the course of a year. The first of these is *Work Ready Now!*, which focuses on a combination of traditional business skills—such as basic accounting—and "soft" skills hypothesized to be both valuable and transferable across jobs and employment sectors (see Campos et al., 2017, for related evidence). This *Work Ready Now!* curriculum consists of eight sub-modules: Personal Development, Interpersonal Communication, Work Habits and Conduct, Leadership, Health and Safety at Work, Worker and Employer Rights and Responsibilities, Financial Fitness, and Exploring Entrepreneurship. This module consists of 10 five-day weeks of full-day training. The next module of HD relevant to our study sample encourages students to focus on self-employment. The *Be Your Own Boss* training is an entrepreneurship curriculum that is tailored to the specific interests and opportunities in each cohort of students, and lasts another 10 weeks. The curriculum for this component includes forming a business idea, identifying a practical business opportunity, outlining the details of business operations and financing, and establishing a formal business plan. Finally, HD participants can participate in a further 10-week Technical Training module that provides specific skills in an employment area (example would include tailoring, hairdressing, carpentry, or beekeeping).

80% of those assigned to HD participated in at least one of these components: 64% of the HD trainees enrolled in both the *Be Your Own Boss* training and focused technical training in a specific work area, 11% enrolled only the former, and 4% only the latter. Nearly a half of all individuals assigned to the HD arm completed the technical training workshops. After completing their classroom training, HD students are typically placed in an internship or apprenticeship position with a local entrepreneur working in the selected employment sector. 39% of those in the HD arm undertook an apprenticeship during the study period, with the large majority of these in tailoring (53%) or hairdressing (22%). This combination of several months of classroom training followed up by internships and workforce experience programs is typical of, if slightly more intensive than, comparable programs globally such as the *Jóvenes en Acción* program in Colombia (Attanasio et al., 2011).

McIntosh and Zeitlin (2022) provide more details of the program, participation rates in the

different components of HD, and the specific types of training received. For our experimental analysis we do not utilize the (endogenous) choices over the specific training received and instead focus on the Intention to Treat (ITT) effect of being offered the bundle that is Huguka Dukore.

GiveDirectly: Household grants program

The cash transfers were provided by GiveDirectly, a U.S.-based 501(c)3 nonprofit organization. GiveDirectly specializes in sending mobile money transfers directly to the mobile phones of beneficiary households to provide large-scale household grants in developing countries including Kenya, Uganda, and Rwanda. The organization supports an in-country infrastructure that enrolls participants, makes the transfers to the households, and confirms (via calls from a phone bank) that transfers have been received by the correct people and in a timely manner. Since eligibility did not condition on having a cellphone, during the enrollment process individuals who did not themselves own a cell phone provided a number belonging to a trusted family member or friend, and transfers were sent to them through this intermediary. The payments were made to beneficiaries in two installments two months apart, with the first payment comprising 40% of the total to be paid to the beneficiary, and the second payment completing the transfer.

The value of household grants was not disclosed at the time of the lottery. GD treatment (where transfer values were disclosed to recipients) did not commence anywhere until the lotteries have been conducted everywhere in the district so as to avoid emphasizing the cash treatment prior to the completion of recruitment.

2.2 Enrollment and Assignment

The study recruits youth from 13 geographic 'sectors' in the districts of Rwamagana, Muhanga and Nyamagabe.[2] Study participants had to be eligible for Huguka Dukore, to attend an informational session about Huguka Dukore, to enroll in a lottery to determine participation in that program following that informational setting, and to be traceable to a residence in a village in the sector where they were recruited. Attendance in person at the public lottery was not required for program enrollment. The study enrolled in its sample all individuals who met the criteria for treatment by Huguka Dukore in the study sectors.

The sample is 54% female with an average age of 23.5 (among the random sample assigned to control). They have an average of 7.6 years of education and typically live in households of approximately five individuals. 33 percent of (control-group) respondents reported being employed at baseline, using a definition that *excludes* agricultural work on a farm belonging to their own household. Nonetheless, individuals in the study population are quite poor. 32% reside in households that the Government of Rwanda categorizes as Ubudehe I—its lowest socio-economic category, denoting a condition of 'extreme poverty'. Median consumption per adult equivalent is 5,879 RWF per month, which in 2018 PPP terms translates to a consumption level of USD 0.66 per day.

[2]In Rwanda, the *sector* is the geo-political unit below the district. There are 30 districts in Rwanda, and 416 sectors in total across those 30 districts.

A public lottery was used as the assignment mechanism for the study given the large sums of money being transferred and the desire by all parties to ensure that the assignment was considered fair and impartial by the research subjects. Participants drew their own treatment status as tokens of different colors from a sack, where each token corresponded to a given treatment arm and the number of tokens in the hat was determined by IPA according to the number of participants with fixed proportions assigned to each treatment. The proportion of individuals assigned to each treatment was fixed within each sector-level lottery, resulting in a standard block-randomized structure across the 13 blocks in the study.[3]

As illustrated in Table A.1, this mechanism was used to assign individuals to one of four broad categories: a control arm, the HD program, the GD-administered cash transfers, or a *Combined* arm. In total, 485 individuals were assigned to HD, 672 to GD, 203 to the Combined arm, and 488 to control. Within the cash transfer arm, individuals were randomly assigned to the three bracketing transfer amounts (*GD-Lower*, receiving $317.34; *GD-Middle*, receiving $410.19; and *GD-Upper*, receiving $503.04), or the (*GD-Large*) arm, receiving $750.

The Combined arm received the HD treatment as well as the GD-Middle transfer. Both interventions were received at the same time as others in their same sector, meaning that they typically started the HD treatment several months before they would receive the household grant from GD. Receipt of cash transfers in the Combined arm was not made conditional on participation in HD training, a point which was emphasized at the lotteries.

Compliance with GiveDirectly treatment was nearly perfect. For GD the Intention to Treat (ITT) is therefore effectively the average treatment effect. For HD, 86% of the full HD treatment group (both HD-only and Combined arms) were counted as enrolled according to the contractual definition (attending the end of the first week of WRN training). This is the rate that the costing exercise uses since it alone determines the amount paid from USAID to the local implementing partner. Retention during the course of WRN is high; 79% of the overall sample completes this 10-week training program, which focuses on general workforce readiness. 69% of the of the HD sample complete the Be Your Own Boss class (which is focused on entrepreneurship and self-employment); 13 individuals who did not take WRN did then go on to enroll in Be Your Own Boss (BYOB). Finally, the Technical Training component of the HD intervention provides focused vocational instruction in a specific job sector, and was offered as a complement to BYOB. 48% completed the Technical Training component of the program. In the combined arm, participation with each of these components is about 5 pp higher than in the HD-only arm.

[3]In the first phase of lotteries, comprising 792 study participants—we randomized purely at the individual level, as the study design did not anticipate multiple enrollees from the same household. In fact, the 792 participants in the first tranche of lotteries comprised 732 unique households. This resulted 34 households in which individuals in the same household were assigned to different treatments (at the level of the major arms of the study). Having recognized this issue, we altered the protocol in the second phase of lotteries and assigned treatment at the household level, where the 1,056 study members comprise 952 unique households. To reflect this issue we cluster standard errors at the household level.

2.3 Cost Measurement

The costing exercise in the study utilized the 'ingredients method' (for more discussion, see Dhaliwal and Tulloch, 2012; Levin and McEwan, 2001; Levin et al., 2017; Walls et al., 2019). An ex-ante exercise, which was based on projected budgets and staffing costs, was used to predict the cost at the time of the study design and to choose the ranges over which GiveDirectly transfer amounts would be randomized. Then, a rigorous ex-post costing exercise was conducted for both programs once study implementation was complete, using actual budgets and expenditures.

We attempted to cost the full national-scale HD program (not just the study sample), inclusive of all direct costs, all indirect in-country management costs including transport, real estate, utilities, and the staffing required to manage the program, and all international operating costs entailed in managing the HD program. In order to avoid having scale effects make the costing asymmetric across implementers, we asked GiveDirectly to artificially scale up their operations and provide us with numbers reflecting the costs per beneficiary if they were running a national-scale program across eight districts, including 40,000 beneficiary households like HD. We costed each GD arm separately, asking what the operating costs would have been if GD had run a national program at the scale of HD, giving only transfers of that amount. The ex-ante projected total cost of providing HD was $452.47. The bracketing amounts were derived by supposing that the number of beneficiaries for the year two tranche of HD funding nationwide might vary between 8,000 and 12,000 beneficiaries, meaning that the per-capita cost would vary between $377.05 and $565.58.

Table 1 shows the evolution of the costing analysis. Following the study intervention period, we undertook an ex-post costing exercise to determine actual expenditures on costed ingredients and the consequent at-scale costs to USAID of each study arm. These figures show that HD was less expensive than anticipated, and GD operating costs were slightly higher than anticipated. This means that the amount USAID spent per *beneficiary* was only $388.32, while the spending for the GD middle arm was $493.96. The inclusion of non-compliance further widens this gap, meaning that USAID cost per *study* household in the HD arm was $332.27, while in the GD arms it was $394.93, $490.99, $590.41, and $846.71, respectively. The combined arm, incorporating compliance with both components of the combined treatment, ended up costing USAID $840.20 per study individual, an amount similar to the GD Large arm. These are the numbers used in the cost equivalence analysis. Because there was no additional implementation in the study sample between midline and endline, for the endline analysis we use the costing numbers from the midline exercise. For further details on the costing exercise, please refer to McIntosh and Zeitlin (2022).

2.4 Surveys and Outcome Measurement

The baselines for the study were conducted during December of 2017 and January of 2018, HD treatment began February 2018, and GD treatment May of 2018. Study midlines were conducted during July and August of 2019, and this endline activity was conducted in October and November of 2021. Hence the endline is 3 years 8 months after HD started, 3 years 6 months after GD started,

and 3 years 10 months after baseline.

A household survey was administered to the household head and a beneficiary survey was administered to the beneficiary; by endline in many cases these were the same individual (as beneficiaries formed independent households). We have five primary outcomes for the study. Employment is a binary measure indicating that the individual spent more than 10 hours in the prior week in paid work or as the primary operator of a micro-enterprise. Productive Hours is the number of hours in the prior week spent in off-farm paid work or in micro-entrepreneurship. Both measures exclude own-farm agricultural work, namely labor put into the farm owned by the household. Monthly income is the total amount earned over the prior month, including enterprise revenue. Productive asset stocks and household consumption per adult equivalent round out the primary outcomes.

Our secondary outcomes are divided into three groups. Additional measures of beneficiary welfare are subjective well-being and mental health, as well as the personal consumption of the beneficiary. Household wealth is measured using net non-land wealth, livestock wealth, and the stocks of savings and debt. Cognitive and skills dimensions are measured using Locus of Control, the Big Five index, as well as measures of the aspirations, business knowledge, and business attitudes of the beneficiary. All monetary outcomes, both primary and secondary, are winsorized at 1% and 99% and measured in inverse hyperbolic sine (so that marginal effects can be interpreted as percent changes).

2.5 Enterprise Data

Given the strong focus of both interventions on self-employment and the lack of formal employment opportunities in surrounding job markets, the most likely medium for longer-term impacts on income and welfare is enterprises run by the beneficiaries. To explore this, we examine the results of the two survey modules that were used to measure enterprises. One of these was based in the household module, and was built to track enterprises primarily run by individuals other than the beneficiary him or herself. There were relatively few existing household enterprises at baseline, and as we will show the intervention had quite limited effects on these. The second module was located in the beneficiary survey, and was built to track businesses either run directly by the beneficiary or to which that individual devoted substantial time or resources. Because these two instruments were not necessarily administered to the same people or at the same time, it was impossible to design this to completely preclude the possibility of double-counting businesses across these two instruments. For that reason, we never add together outcomes from these two instruments, instead counting the 'household' and 'beneficiary' businesses simply as two different types of entities that are examined separately.

For both household and beneficiary businesses, we collected a number of core outcomes. For all extant enterprises we asked whether the business was currently in operation, and if so the number of household and non-household members employed regularly in the business, as well as the number of days that business was operative in a typical month, and the number of customers in a typical month. We then asked for the typical daily sales on a day when the business is operating, and the

total profit the business had earned over the month prior to the survey. We transform all monetary amounts into US dollars, and then we sum all of these outcomes across all businesses reported at the household level and at the beneficiary level for each beneficiary.[4] These totals are then merged back into the experimental dataset (of all beneficiaries, whether they run a businesses or not), missings are replaced with zeros, and we run standard analysis of the effects of the interventions as elsewhere in the study. These impacts can then be interpreted as average effects of the interventions on the total of each variable in a manner that combines extensive margin impacts on the existence of a business with intensive margin impacts on the size of existing businesses.

We also attempted to panel track these entities, thereby generating the ability to answer questions about the birth and death of specific enterprises over time. Because the household enterprises are less dynamic the interest in this exercise applies mostly to the beneficiary businesses. Using panel tracking, we can define a number of different variables at the enterprise level that pertain to the extensive margin. For both midline and endline we can define 'new firms' that had not existed in previous waves and are born in that round. Then, for the midline, we classify the existing firms that we observe in that round into three categories; 'will survive', 'will be inoperative' (in endline the respondent says the firm still exists but is not currently operative), and 'will die' (firm no longer exists at endline). As above, because the sample of individuals with firms is strongly endogenous to the treatments, it is unattractive to analyze these outcomes in rates at the firm level; instead we total them at the beneficiary level, replacing missings with zeros for the new businesses, and so create outcomes that can be analyzed in a standard experimental context that are 'number of new firms'. Among the endogenous sample of firms at midline, we can then examine what happens to those firms by endline as a function of treatment status.

2.6 Attrition and Balance

We attempted to follow up with all study beneficiaries at endline, 46 months after baseline, regardless of whether they had been successfully tracked at midline or not. We followed the beneficiary youth as an individual, and considered the 'household' to be the place in which that individual was resident at the time of endline even when that differed from the baseline household. The survey teams initiated a first phase of tracking where they attempted to find all individuals who had moved within their home districts or had gone to Kigali, the capital. We had originally intended to randomly sample from the remaining un-found individuals to conduct an 'intensive tracking' exercise, as we did in the midline, but the original tracking was so successful and the remaining sample sufficiently small that in the end we simply intensively tracked everyone in the study. This intensive tracking phase involved sending an enumerator to speak with them in person if they were located anywhere in Rwanda or Uganda (where IPA has a sister office and therefore could easily mount in-person surveys), and then conducting a phone survey with anyone who could not be located through the above means or who had migrated to a different country.

[4] As in the rest of the analysis, non-binary outcomes are Winsorized at 99% and monetary outcomes are inflation adjusted to make them real midline US dollars.

For a potentially highly mobile sample of youth, our tracking was remarkably successful; in the end we managed to survey 98.6% of all baseline youth. This is higher than we had anticipated and may be due to the advent of the COVID shock in the interim that made migration and work away from home more difficult, thereby keeping the study sample less mobile than they might have been in business-as-usual circumstances. Of the 1,848 baseline individuals, at endline we found 8 in jail, 8 passed away, 3 mentally ill, 1 in military training, 4 refused the endline survey, and 2 individuals that we failed to find, for a total of 26 baseline individuals who were not included in the endline survey.

Table A.2 analyzes differential attrition by arm. Overall, as is unsurprising with such a high tracking rate, we do not find differential attrition by arm. The one exception is in the GD Large arm, where we succesfully tracked all 178 individuals assigned to this arm, and the resulting tracking rate of 100% is significantly different from the control rate of 98.6%, although clearly the absolute difference is very small in magnitude. Table A.3 looks for signs of differential determinants of attrition by regressing baseline covariates on a dummy for whether the individual was successfully tracked at endline. Only for one covariate do we see any signs of differences, namely that we were least successful in tracking those individuals who were wealthiest at baseline in terms of consumption. Nonetheless, once adjusted for multiple inference across outcomes we find no evidence of overall tracking differentials, meaning that the endline sample is representative of the baseline universe.

We can then examine the balance of the experiment using the attrited endline sample that will be used for analysis. Table A.4 shows an exceedingly well-balanced sample, with not a single covariate significant for any arm once adjusted for multiple inference. The endline sample therefore appears to provide clean internal validity and a remarkably well-tracked sample given the duration of the effects we estimate here.

3 Core Results

3.1 ITT

In tables 2 and 3, we estimate impacts of assignment to HD or cash transfers on primary and secondary outcomes. These are generally attenuated since those observed at midline, when compared with a control group who have maintained their employment rate and even slightly increased both income and consumption by increasing productive hours (or sacrificing labor) and, most strikingly, stripping productive assets.

Individuals in our control group have an endline employment rate of 50 percent, statistically indistinguishable from the 48 percent employment rate observed at midline. Control group incomes are actually 12 percent higher in real terms than they were at midline, and real consumption in the control group is up approximately 47 percent at endline relative to midline. These successes in sustaining income and consumption among control-group members have been accompanied by rising work hours and reductions in productive asset stocks. Work hours have increased by nearly

one hour per week, or approximately 4 percent, implying that earnings per productive hour have risen slightly in the control group between the midline and endline. On the other hand, control-group participants have seen marked drops in the value of productive assets they hold: a loss of approximately 63 percent of the value of productive assets from midline to endline. Falls in livestock wealth appear to be a primary contributor to these declines in productive asset values; we also see rising debt among the control group (and, perhaps surprisingly, some increases in savings stocks).

Against this context, ITT results for primary outcomes in table 2 show proportionally attenuated estimates on income, assets, and consumption, with a striking change in the ordering of treatment effects on productive hours. Aggregate employment effects, combining all types of work, remain essentially unaffected. Monthly incomes, which had risen by between 70 and 114 percent among cash and combined cash and training arms at midline, are substantially reduced, with only the Large transfer sustaining a statistically significant, 72 percent increase in monthly income (very similar to that arm's effects at midline). Similarly, impacts on household consumption per capita are no longer statistically significant, with point estimates falling to as little as half of their previous value. Consumption impacts relative to control of 21 percent in GD Middle and of 17 percent in the Combined arm, each of which is about two thirds of its midline impact, have multiple-inference adjusted ("sharpened") q-values just above 10 percent.[5] To the extent that consumption is the typical omnibus measure of economic welfare, it may be more appropriate to consider the unadjusted p-values for this outcome, in which case most arms have impacts that are just significant at the 95% level. Program impacts on productive assets stocks had at midline increased by 154 percent in the HD arm and approximately 400 percent across cash transfer and combined arms. These differences are now smaller relative to control—with impacts ranging from 93 percent in the HD arm to as much as 315 percent in the Combined arm—and given the reduced level of productive assets in the control arm, these reflect even smaller absolute differences than they did at midline.[6]

In Table 4, we see that transitions in sectors of occupation induced by the interventions have remained remarkably stable, in spite of the lack of movement in the overall employment rate.[7] Cash transfers in the Upper, Large, and Combined arms have induced statistically significant levels of entry into non-agricultural microenterprises among between 11 and 17 percent of the population assigned to those arms, with a further 5-6 percent induced to enter into non-agricultural microenterprises in the Large and Combined arm. As in the midline, this appears to be mostly associated with a commensurate movement out of agricultural wage labor, the prevalence of which is also reduced by 8 percentage points by the HD intervention. Although no longer statistically significant, estimated HD-induced movements into non-agricultural wage labor of 5 percent of

[5]The stars in our tables use the multiple inference-corrected Q-values from Anderson (2008) to account for the multiple outcomes and treatments being tested in each table.

[6]Because the cash arms have virtually 100% compliance, the ITT estimated here is also the Treatment on the Treated (ToT). For the HD arm where the core measure of compliance is 85.6%, if we are willing to assume that those not participating received no indirect effect of being included in the treatment, then we can back out the ToT by dividing by the compliance rate. The resulting ToT estimate would 17% larger than the ITT for each variable, with the same significance level.

[7]Note that because this whole table effectively studies a single outcome: 'how are beneficiaries using their time' we base the stars on the unadjusted p-values, although the sharpened Q-values are also provided in hard brackets.

the population remain consistent with those found at midline. Taken together, these findings suggest that cash transfers—particularly those above the Middle value—induced movements into self-employment that persisted in spite of the pandemic. On the other hand, while HD's impacts on movements into wage labor did persist, it seems plausible that the more limited productive-asset buffer in those sectors explains the lesser persistence of HD-induced microenterprises over the course of the pandemic.

Similar attenuation of midline impacts is observed for measures of beneficiary welfare. HD impacts on subjective well being fall from 0.19 standard deviations at midline to (statistically insignificant) 0.12 standard deviations at endline, while cash transfer impacts are approximately half of those previously observed—though still significant at 0.29 and 0.39 standard deviations, respectively, for the Middle and Upper transfer values. We see no impacts on our survey measures of mental health, and we see modest impacts on beneficiary-specific consumption in the Upper and Combined transfer arms.

We continue to see signs of persistent wealth effects from cash transfers, though these are generally smaller than at midline and somewhat imprecisely estimated. Point estimates for net non-land wealth suggest gains in the Upper and Large transfer arms of 70-80 percent relative to control—though statistically insignificant—down from impacts in excess of 110 percent at midline. There remain positive impacts on household livestock wealth from the Middle, Large, and Combined arms, with the latter delivering the smallest of these impacts at approximately 126 percent over control. And savings impacts of cash transfers largely persist from midline, e.g., at a 99 percent impact over control in the Middle arm. The prior estimates of large savings impacts of HD have largely evaporated.

Finally, we see little evidence of sustained impacts on beneficiaries' cognitive and non-cognitive skills. We see no impacts on Aspirations (and survey data do not provide the Locus of Control measure from midline). Measures of business knowledge remain statistically significant in the HD and Combined arms, suggesting some persistent human capital effects, but these are approximately half of their prior magnitudes.

3.2 Cost-equivalent benchmark

As discussed in McIntosh and Zeitlin (2022), we estimate a regression-adjusted, cost-equivalent comparison between HD and cash transfers by estimating a model of the form

$$Y_{ihb2} = \delta^{T} T_{ihb} + \delta^{HD} T_{ibh}^{HD} + \beta X_{ihb0} + \rho Y_{ihb0} + \gamma_1 \tau_c + \mu_b + + \epsilon_{ihb2} \tag{1}$$

for outcome Y of individual i in household h, randomization block b, and round 2. Here, T_{ihb} is an indicator for whether this household was assigned to *any* treatment, and T_{ibh}^{HD} an indicator for assignment to the HD arm in particular, such that the coefficient δ^{HD} estimates the differential effect of assignment to HD, relative to a cash grant. We include a measure, τ_c, defined as the difference between a given arm's expenditure per beneficiary and that of the HD arm (set equal to

zero in both HD and Control); doing so ensures that the coefficient δ^{HD} estimates HD's differential impacts relative to cash transfers at costs equivalent to HD costs to donors per beneficiary.

Results presented in tables 5 and 6 show that at these cost-equivalent levels, we find no statistically significant differences on primary outcomes at endline between HD and cash transfers. Among secondary outcomes, these differences between arms are different only for our measure of business knowledge. Proportional attenuation of program impacts across a wide range of outcomes appears to mean not only that it is harder to detect the impacts of individual programs at these more modest impact levels, but, moreover, the resulting attenuation of the differences between arms makes it harder to find statistically significant contrasts between them. If we willing to interpret differences that appear quantitatively meaningful even if statistically insignificant, HD edges cash in terms of productive hours and debt reduction, while cash has the advantage for outcomes that pertain to asset ownership and consumption.

We can probe the robustness of the linearity assumption used to cost-adjust the cash arms. To interrogate this, tables A.5 and A.6 present the estimated cost-equivalence comparison using a variety of different functional forms to control for cost, for primary and secondary outcomes respectively. In each table, Column 1 present the base linear case from the prior tables. Column 2 uses a quadratic, and column 3 a third-order polynomial, functional form to control for cost. Columns 4–7 then serially drop one of the GD transfer amount arms and present the cost equivalence comparison if that arm had not been in the study. In general the results are quite robust; the significant benefit of HD at building business knowledge is always positive and is significant in 5 of the 7 specifications.

3.3 Cost-equivalence versus Cost effectiveness

This study provides the capacity to make comparisons both across two interventions implemented at (nearly) the same cost, and also to compare across different costs to evaluate differential cost effectiveness. Tables 7 and A.7 divide the arm-specific benefits measured in ITT regressions by the cost of each arm in hundreds of dollars, and so give the benefit per amount spent. The columns to the left of this table then provide p-values on F-tests of the differential cost effectiveness across arms. As was the case with the cost-effectiveness comparisons, the overall impacts are now sufficiently attenuated that none of the benefit/cost ratios are different across arms, with the exception of the business knowledge question. Figure 1 presents a graphical contrast of the cost equivalence and cost effectiveness approaches to our study results. Cost equivalence is visualized in the left panels by the vertical difference between the black diamond (HD) and the hollow circle (predicted cash impact at HD cost). Cost effectiveness is visualized in the right panels by the slope of the line connecting zero with the arm-specific outcome represented in benefit/cost space. While we have already shown that these differences are not statistically significant, the takeaway from these different approaches emphasizes the superiority of HD at driving productive hours (both in terms of cost equivalence and cost effectiveness), and the cost effectiveness superiority of the middle cash transfers in producing consumption and productive assets.

3.4 Complementarities

We test for complementarities by comparing impacts of the Combined arm with the sum of impacts in the arms comprising its constituent parts—HD and the GD Middle transfer arm. As in McIntosh and Zeitlin (2022), we do so by dropping active-treatment arms not involved in this comparison. We then create indicators for whether the individual received HD or a cash transfer, defining these to take a value of one in the combined arm as well. In Table A.8 and A.9, we estimate a model that includes these alongside an indicator taking a value of one for individuals assigned to the Combined arm: the coefficient on the Combined arm indicator therefore directly estimates the extent of complementarities.

Whereas at midline we had found some evidence of negative complementarities—in particular, on productive hours and subjective well being—we find no such evidence here. The Combined arm's impacts are statistically indistinguishable from the sum of its HD and GD components.

Given the coincidental fact that the GD Large arm has almost the same cost as the Combined arm, our study provides an alternate ability to think about complementarities. This is to ask: given that a youth has already received a cash transfer of approximately the GD Middle amount, is it better then to spend additional resources on that individual by giving her more cash, or by giving the HD training? This comparison is given in the p-values of the F-tests in column (c) of Tables 2 and 3. Again, we find no significant differences between these interventions with the exception of business knowledge, showing again that HD is uniquely productive of that outcome.

3.5 Heterogeneity of Primary Impacts

As was the case during the midline, we uncover surprisingly little evidence of heterogeneity within the study sample. A set of four appendix tables use interaction analysis to examine the pre-specified dimensions over which we anticipated the study might have differential effects. These are gender (Table A.10), age (Table A.11), baseline consumption (Table A.12), and baseline local employment rates (Table A.13). None of these tables surfaces any meaningful evidence of heterogeneity. One possible explanation of this result is that our study ended up with a relatively narrow set of targeting criteria (youth who were qualified for and interested in Huguka Dukore, while being poor enough to qualify for the use of Give Directly transfers), thereby limiting the overall diversity within our sample. The conclusion is that both of the interventions studied are having consistent effects and retargeting within this group would not substantially improve overall program effectiveness on primary outcomes.

3.6 Impacts on Household-Reported Businesses

For the purpose of showing the evolution of outcomes over time, we present results for both midline and endline. We begin with the household-reported enterprises in Tables A.14 and A.15. In both rounds, these businesses seem to have been largely untouched by the substantial interventions being directed at youth in these households. As a starting point we see that control households

report operating only .06 businesses on average in midline and endline (there is only one control household operating more than one business, so in effect this means that only 6 percent have any business at all). Treatment of beneficiaries does not lead to any elevation of the probability that there is a household-reported business, and if anything seems to lead to a light *decrease* in the devotion of household labor to the household enterprise. Similarly, core businesses outcomes such as the number of customers, daily sales, and monthly profits typically show weak negative effects, stronger in the endline than midline. So the main picture is that there are very few businesses in these households, they are generally unaffected by the presence of the treatments, and to the extent that they are impacted they appear to be suffering from a drawing away of labor.

3.7 Impacts on Beneficiary-Reported Businesses

We begin our analysis of beneficiary businesses on the extensive margin, examining firm birth and death. In Table 8 we consider the number of newly born firms in midline and in endline in the first two rows. The average control individual created .5 new businesses in midline, HD elevates this by .22, and all of the cash arms lead this to more than double. Control individuals created .24 new firms on average between midline and endline, and only the GD Large and Combined arms continuing to elevate business creation by midline. In the remaining rows we then look at the (endogenous) sample of firms that exist at midline, and ask what happens to them at endline as a function of treatment status. In the third row we see that in the control group .24 of the firms have died, this rate is not significantly different for any treatment arm. In row four we look at the rate of 'inoperative' businesses, however, and see that while this is relatively rare in the control group (.14) the rate of midline businesses becoming inoperative more than doubles in most of the cash arms. However, because the overall number of businesses was so much larger in the cash arms, they are have more businesses created in midline that remain operative in endline as well. So, the takeaway from this table is that all of the interventions led to a short-term burst of business creation; while this effect was smaller in the HD arm those businesses proved more durable. The cash arms created more businesses that survived to endline but also more businesses that become inoperative by endline as well. So the interventions have powerful effects on the extensive margin.

We turn to the impacts on midline beneficiary businesses in Tables 9. Here we see transformative effects of all the interventions. Beginning again with business ownership in the control group, we see that by midline the average control individual reports operating .79 and owning .71 businesses (the maximum number reported by control individuals is 5 different enterprises, with 52% reporting owning any business). The interventions all drive this number up, with HD increasing owned businesses by .14 or 18% of the baseline mean, and the cash arms having at least three times this effect, with the largest transfers and the Combined arm almost doubling the number of businesses owned. All interventions increase hired labor but particularly draw heavily on the use of household workers, explaining the weak negative effects seen on household-reported enterprises. HD drives up the number of days worked per month by 2.7 over a base of 9.1, and leads to a large increase monthly profits (treatment effect of $4.36 per month), but does not change either customers or

sales significantly. The cash arms lead to a doubling of days worked, number of customers per month and sales are more than doubled in the smallest arms and tripled in the Combined arm, and profits are more than doubled everywhere. The implication of this latter result is that the receipt of $503 was generating an enterprise profit increase of $11.32 per month 14 months after receipt of the cash grant.

The endline beneficiary-reported business impacts are presented in Table 10. Perhaps the most important thing to point out here is the sharp contraction in the overall rate of business ownership in the control group, which falls from .79 at midline to .4 at endline. Given that we would expect the control group to be becoming monotonically more economically active over time as they age, this is strong evidence of the fact that COVID has driven a substantial number of the self-employed out of business. Similarly, control group endline days worked in self-employment fall by 38%, sales by 53%, and profits by 24% relative to the midline. So there seems to be no doubt that business conditions have worsened overall and the endline impacts need to be interpreted in light of their ability to insulate beneficiaries against this shock.

As is the case with many of the results presented in this study, the core enterprise treatment effects in the endline represent a fade of about 50% relative to what we saw at midline. HD continues to elevate the number of businesses owned by about .1, and both days worked and sales are significantly elevated relative to the control. Unfortunately the HD effects on profits have fallen to about a third of what they were at midline (now $1.64 per month), and are significant only at the 10% level. The cash arms elevate the number of endline businesses by .2-.4, retain substantial impacts on days worked and sales, and continue to significantly elevate profits by amounts ranging from $3.08 (GD Middle) to $7.11 per month (GD Large). While it is impressive to see significant impacts on business profits across the board even 42 months after the GD intervention, these endline impacts represent between 30%-90% of the profit impacts at midline, suggesting that all the cash arms are seeing a fade in business profits over time.[8] Given that the treatment effects on profits and hours worked contract by similar amounts, impact on the effective wage rate appears similar at midline and endline.

Table A.16 shows the gender interactions with treatment on endline beneficiary business outcomes. While the table contains very few significant gender interactions, the signs and the magnitudes of the differential female effects are troubling. For the key outcomes of sales and profits the female interactions are negative for all treatments, implying that women are benefiting less than men. While for the cash arms these interaction effects are smaller than the male treatment effect, meaning that women still benefit overall, for HD this is not the case. Adding together the uninter-

[8]One admittedly heroic way of contextualizing these cash effects is as follows: take the business profit impact of GD Upper 14 months and 42 months after treatment and linearize it from month 1 until it becomes negative (which occurs in month 65) then the sum of the resulting profits is $463 for an arm that cost $572 and delivered $503. The implication is that the average total improvement in business revenues is not larger than the original transfer, despite the very substantial increase in days worked over the course of this time interval (450 total additional days, using the same linear extrapolation method). Even taking the total profit effect as return (ignoring the initial cash received) this method suggests a wage rate of around a dollar per day the business is open, similar to the rate observed in the control group.

acted male effect with the female coefficient to get the total effect on women implies that females are getting little endline benefit from HD on customers, sales, or profits compared to women in the control group. So the modest long-run effects of HD on business outcomes appear to be confined to males (although the difference between men and women is not significant).

In summary, then, even the GD Lower arm drives larger enterprise impacts than HD across most outcomes at both midline and endline. Business conditions have worsened substantially overall between midline and endline, and the impact of the interventions has faded by about half across most outcomes in the 28 months between these two surveys. These results indicate that enterprise activity is a key conduit for the overall income and consumption impacts seen elsewhere in the study, that both human and physical capital can deliver better livelihoods through self-employment, but neither of these appears to generate a dynamic shift in business outcomes that represents a real pathway out of poverty.

4 Dynamics and Shocks

4.1 Household Transitions

We capture in our long panel data the period of time where the study subjects are forming their own households, getting married, and having children; understanding how these interventions may advance or retard that process is key to interpreting the total effects of the interventions. Given the pivotal role that gender plays in mediating the opportunities that young people have (both because of cultural expectations and also because of the relationship between childbearing and labor supply during this time of life), we are particularly interested in examining male/female differentials for this set of outcomes. To explore these questions, we dig deeper into a number of outcomes not included in the Pre-Analysis Plan for the study, but widely used in the literature that examines similar interventions.

A starting point is understanding differences in how individuals form new households as they age out of adolescence and start financially independent adult lives. Do these individuals, who were typically dependents in their parents' households at baseline, become household heads (or spouses of household heads) themselves? A less stringent measure is to examining the share of beneficiaries who have moved away from their baseline households by the time of the midline and the endline. Table A.17 examines these outcomes. HD has no significant effect on any measure of new household formation. The cash arms paint a more complicated picture; overall there is an elevation of about 10 percentage points in the likelihood that the beneficiary lives in a new household at endline, but there is no change in the probability of being the head of that household (these effects are significant before controlling for multiple inference).[9]

To understand whether the cash-induced movements are sending youth from that arm into more

[9] Additional analysis (not reported) shows that these new households are no smaller on average (4.7 members in either case), and so it looks more like cash is causing beneficiaries to move to different locations at which opportunities to run a business are improved, rather than actually establishing independent households.

urban settings, we examine movements from one village to another across rounds. First we can create a dummy in the midline and endline for being in a different location altogether than the baseline, Then, we match the district, sector, cell, and village of residence in each survey to an official Rwandan government classification of these locations as urban, peri-urban, semi-rural, or rural, and examine treatment effects on the classification of their locations. Table A.18 shows only a weakly elevated probability from the cash arms of moving across villages, suggesting that about half of the household switching shown previously is within-village. When we examine how urban the locations are, we find that against an overall upward trend in the control (at baseline only 12% of control individuals live outside of rural villages, a rate which rises to 18% in the midline and 22% in the endline)the cash arms actually hold people in rural areas, retarding the move to town. In sum, then, HD plays no strong role in this type of mobility, and while cash encourages people to change households this is often within-village and rarely involves the beneficiary establishing an independent household.

4.2 Marriage and Fertility

Next we consider a set of variables related to marriage transitions and fertility. To study marriage, we examine whether an individual is married or cohabiting, as well as separate indicators for each of these two statuses. For fertility, we examine whether the individual has ever had any children, and we examine their survey-reported desired fertility (total number of children they hope to have including those they already have). Table 11 shows the treatment effects and Table 12 the gender interactions for this analysis. Here we see muted and somewhat confusing results when we pool men and women, but then quite a clear and a clearly differential picture in the interaction analysis. Money has a significant effect on allowing men to marry, not on whether they cohabit, and this effect appears quite monotonic in the amount transferred. For women this effect is weaker and in some cases significantly so. In general the sum of the male effect and the female differential is approximately zero, suggesting that money overall has no effect on women's proclivity to marry (and explaining why the pooled results are insignificant). Perhaps unsurprisingly, the story for whether individuals have any children is very similar; large amounts of money amplify male's probability of having kids, are significantly less likely to do so for women, resulting in a net effect of zero for women (this is despite the fact that the women in our sample overall are 38% more likely to have a child than the men). The cash arms weakly amplify the desired fertility of males, and here the significant negative effects are strong enough to mean that on net there is a mild overall depressive effect of cash on female desired fertility.

In order to investigate possible pathways for this latter set of effects, we examine outcomes around education choices and valuation of time, both of which would be key inputs to a Beckerian consideration of fertility choices (Becker, 1965). First, in Table A.19 we examine completed schooling. The intent of the program was that it was enrolling individuals who are old enough to have completed their schooling and so neither the cash arm or the HD arm were intended to generate education other than through HD itself. We confirm that this is true, showing that none of the

programs have an effect either on the years of completed schooling at endline nor on the time put in to schooling. A different use of time is that spent doing household chores, an activity that we confirm women devote hugely more time to on average than men (31 hours per week for women, 13.4 for men). We have already seen that the interventions move productive time use from on-farm agriculture to self-employment (cash arms) and to a lesser extent paid work (HD). Here we see that the interventions have no effect on decreasing overall time in domestic chores.[10] Finally, we use survey questions that asked the beneficiary how much money (as a daily wage) they would need to be paid to accept a job in their village, and in the nearest town.[11] These provide a survey measure, albeit unincentivized, of the opportunity cost of time. If the pathway to the impacts on female desired fertility operated through the way she perceives her time on the margin, we would expect to see it here. The results are quite clear that this is not the case; we see no pattern of the treatments increasing the opportunity cost of time overall, or for women specifically. This again is consistent with the idea that the effective wage rate being achieved in project-created businesses is not out of line with the counterfactual returns they would have achieved in the absence of the program.

Taken together, these results are interesting in a number of dimensions. First, Huguka Dukore, despite content focusing on family planning and HIV, did not move marriage or observed fertility, although it did have a weak depressive effect on desired fertility, particularly among women. Second, we see evidence that men in Rwanda are income-constrained in marrying, and when this constraint is relaxed they move more quickly to formally marry and to have children. These constraints do not appear to bind in the same way for women (as would be consistent with brideprices from grooms to brides being culturally typical). Finally, while we have not found impacts of these interventions on the economic aspirations of youth or the opportunity costs of time, the considerable effects of cash transfers both on entrepreneurship and on desired fertility for women do suggest a pathway whereby relaxing credit constraints increases young women's economic prospects and thereby alters the way they think about childbearing in a dynamic way.

4.3 The incidence of Covid-related shocks

The study takes place in the context of a large-scale health and economic shock. Restrictions in movements and economic activity were potential sources of distress for households, even as the actual prevalence of Covid-19 infection in our study population remained relatively rare.

To understand the incidence of Covid-related shocks, we asked study participants at endline about their experiences during three key and salient periods: first from the beginning of the nation-wide lockdown of March 2020 until the beginning of the genocide memorial period in May, 2020; second, in a period of relative normalcy, during which children were allowed to return to school,

[10] In unreported interaction results we show that HD differentially decreases time in chores for women relative to men by about 4.5 hours per week, suggesting that this program has a labor empowerment effect that closes about a quarter of the gap between male and female time in domestic work.

[11] The questions were asked in the form 'would you be willing to accept 1000 RwF', 'if not would you accept 2000 RwF', 'if not 4000 RwF', and if still no then 'how much would you have to be paid to accept this job'.

running from November 2020 through January 2021; and third, during a later lockdown in July of 2021. In Figure 3, we document the incidence of four measures of shocks during this period. We focus on those that are plausibly exogenous to assigned treatments, excluding those (e.g., asset stripping) that are more likely to be driven by treatment-induced accumulation of wealth or economic opportunity. We report the incidence of actual disease incidence, self-reported income loss, and experiences of food-market closures and food product shortages.

Figure 3 shows the burden of these shocks by treatment arm. Variation in these outcomes across arms is of modest magnitude and statistically imprecise. Reports of direct experience of Covid illness are relatively low in most periods, though increasing over time. We see markedly greater measures of induced economic hardship in the first of the reference periods, with some resurgence of these shocks in the later lockdown period. In particular, a substantial portion of the sample reports income losses, particularly in the first lockdown period. And the consequences of this lockdown are also visible in reported experiences of access to food: both reductions in market access and shortages of specific products are widespread, especially in the initial lockdown period.

These shocks are borne out in stalled improvements in income-generating activities in the control group. This is documented in Figure 4. Because study participants were selected on the basis of conditions of need—which may have been transitory—it is not entirely surprising to see growth across a range of outcomes between the baseline and midline survey rounds. This growth trajectory in employment status, productive hours, and monthly income comes to a near-complete stop between the midline and endline, however, as control members experience the economic consequences of the Covid-19 pandemic.

Strikingly, control-group members are able to protect and even increase measured consumption at endline.[12] This may in part reflect the delayed effect of earlier accrued income gains. Data on productive assets point to another, and more concerning, mechanism by which households sustained consumption: through the stripping of assets. Here is the place where the pandemic shock is most visible in primary outcomes, as control-group households lose more than half of the value of their assets at midline, substantially eroding the gains they had made since baseline. And examination of the rate of business operation by control-group members highlights that this asset stripping is associated with a marked decline in the operation of businesses by individuals in the control group (results are similar for measures of business ownership). Taken together, these findings suggest that individuals in the control arm reduced or sold off the businesses that they had launched by midline; in doing so, they did not become unemployed, but rather switched back to a focus on agriculture, with some surplus generated by asset sales that drove a rise in consumption.

4.4 Association between Covid-related shocks and endline economic outcomes

Our ability to distinguish the consequences of covid shocks—and their differential effects across treatment arms—from trends that would have occurred in the absence of this pandemic hinges on the existence of measurable, cross-sectional variation in exposure to these shocks. Unfortunately,

[12] Consumption figures are deflated to midline prices, so this does not reflect an inflationary effect.

identifying plausibly exogenous sources of cross-sectional measures of shocks is difficult, not least because these must seemingly be individual-level attributes: the geographic sector intra-class correlation is less than 0.01 across all four of the shock measures highlighted above. We therefore retain our focus on the set of plausibly exogenous survey measures of shocks previously introduced.

Appendix table A.20 plots the association between survey-reported shocks and endline outcomes. Results are mixed. Food market closures, which are presumably reported by anyone who experienced them (because everyone buys food) generally have the expected sign, with shocks correlating to negative effects on productive hours and income. Reporting on shocks to income, however, consistently has the reverse sign, with those reporting income shocks being the people who had *higher* employment, income, and assets. This clearly suggests the endogenous nature of exposure to covid-driven income shocks: individuals engaged in own-farm agriculture continued relatively uninterrupted while the economic incidence of the lockdowns fell heavily on those engaged in off-farm enterprises. More generally, the raw levels of these measures do not predict productive assets or consumption well. The measurable and plausibly exogenous variation in shock exposure across the cross section seems not to be the mechanism driving variation in productive asset declines within the sample, but it is not possible to distinguish the absence of an impact from limited cross-sectional heterogeneity in this shock exposure.

4.5 Do the treatments have protective effects?

To what extent is the attenuated impact of the treatments at endline attributable to their inability to protect against shocks? Given the cross-sectional variation in plausibly exogenous shock exposure measures, we are inherently limited in our ability to conclusively answer this question. Nonetheless, we can gain some traction by considering heterogeneity in treatment effects along the dimension of predicted endline outcomes, using our shock measures as predictors.

Following the approach to such "endogenous stratification" problems posed by Abadie et al. (2018), we proceed in the following steps. First, we use second-order polynomials in the cumulative shock indices to predict endline outcomes in the control group; following Abadie et al. (2018), we use a leave-one-out approach to omit each control-group observation from the regression on which its prediction is based. We then use predicted endline outcomes in the control group to predict counterfactual endline outcomes in other treatment arms, and interact the centered predictions with treatment indicators. If attenuation in treatment effects since midline were attributable to the observed shock measures, we would expect coefficients on the interaction between predicted endline outcomes and treatments to be positive: this would indicate that income losses are more severe among the treated.

Results are shown in Table 13, where the 'index' variable refers to the predicted value of that outcome in the control group. Overall, we see limited evidence of a concentration of treatment effects across levels of the predictive index, with the exception of the GD Large treatment arm's impacts on employment and consumption. For those outcomes, results are suggestive of particularly strong deterioration in outcomes among those GD-Large recipients who experienced negative shocks.

In sum, then three pieces of evidence suggest that the covid shock hurt those who had 'stuck their neck out' by engaging in entrepreneurship, which is of course precisely what these interventions intended to do. First, the absolute fall in measures of entrepreneurship during the covid era are larger for the treatment groups than the control.[13] Second, table A.20 illustrates that within the control group reported incidence of shocks to income were actually *higher* among those with higher employment rates, income, and productive assets. Finally, table 13 shows that the kinds of individuals who were most likely to be hit by covid shocks in many cases have somewhat larger income fluctuations if treated than if in the control. So the overall takeway is that this period was particularly difficult for people who were doing the things that the treatments intended to induce (running their own businesses rather than working on the farm), and hence the presence of the covid shock is likely to have attenuated the enterprise-driven income effects observed in this study (although the covid lockdowns likely hit the self-employed sector harder than agriculture in a way that would not be seen from other shocks, such as drought or food price changes). This illustrates the role that shocks can play in eroding the gains that transfers can enable. While it might appear that human capital interventions would prove robust to such shocks, in this context we find the proportional fade to be almost perfectly symmetric.

5 Study Extensions

5.1 Spillovers

We follow McIntosh and Zeitlin (2022) to exploit experimental variation in the shares of HD, GD Main, and GD Huge recipients in a given village in order to test for the presence of local spillovers. We estimate two types of models of spillovers: a *levels spillovers* specification that allows for neighbors' treatments to affect one's own outcomes in levels, and a *general interference* model that allows not only for these levels effects, but also for the share of individuals in each arm to modify the impacts of each treatment.

Across these models, we find only very limited evidence of program spillovers at follow-up. As Table A.21 shows, in the levels spillover model we see some evidence of a negative spillover from HD saturation levels onto peer consumption. However, there is limited additional support for this estimate in the richer interference model for consumption outcomes presented in Table A.22, where point estimates for spillovers from HD treatment onto control and cash-transfer arms are negative but insignificant. If anything, the full saturation model suggests a statistically significant, negative spillover effect of the GD Main treatments onto consumption levels in Control. We see no further evidence of interference on other outcomes in the analysis of spillovers to employment (A.23), income (A.24), productive hours (A.25), or asset values (A.26). Hence the within-village variation in treatment intensity reveals very little evidence of contamination in the study.

[13] Midline to endline changes in business ownership rates are -.39 in the control, -.43 for HD, and -.63 for GD large. IHS productive asset changes are -1.6, -2.3, and -2.6 respectively.

5.2 Aggregating financial impacts

We conclude our empirical analysis with an exercise intended to sum up all of the financial flows observed through both follow-up rounds of the survey to estimate the total effect of the cash intervention on household financial flows. Aggregating impacts across financial outcomes and over time provides one approach to summarizing the relative impact of each intervention studied.

This exercise us requires us to values of financial resources between study measurement points. Given that our study asks beneficiaries about wealth, transfers, income, and consumption with different recall periods as recommended by the survey literature, and given that we do not have survey data explicitly asking about these flows in all months from baseline to endline, aggregating the total flow of resources during the course of the study necessarily requires making some strong assumptions about how these flows change over time. The extent to which our measures of inflows, expenditures, and asset value changes balance—that is, the extent to which we can account for all impacts on beneficiary income in either expenditure or asset accumulation—will provide one indication of the accuracy of these assumptions.

The simplest values to account for are stocks; the survey directly asks for the current value of things such as productive assets, livestock, savings, and debt, and so the total effect of the treatment on these values at a moment in time is fully reflected by differences in stock value. An intermediate case are irregular flows that asked about in the survey 'over the past year', which include transfers made to and received from other households. It is standard to ask these questions at a longer time frequency since these flows tend to be large and irregular, meaning that short recall windows become very noisy. Then we have variables that are measured as short-term flows, of which the most important are consumption and income. Here we ask questions either over a month recall window (durables) or a week (non-durables) and so can aggregate consumption and income to monthly levels over the month prior to the survey. For the annual and monthly flow variables, to calculate total impacts over any period of time, an assumption must be made about how these flows change during the course of the study.

At midline we conducted a cash accounting exercise covering the 12 months since treatment by summing the stock values, the annual flows, and then multiplying the monthly flows times 12 which implicitly assumes that the impacts seen a year out had been exactly sustained during the course of that year. We now attempt to repeat this exercise for a survey conducted 40 months after baseline and 28 months after midline. To aggregate values at this point we assume the flows followed a step function, taking their midline values up through the midline and their endline values between the midline and the endline. Total stock values are simply the endline treatment effects on stocks. Annual flow values are then the midline treatment effect plus 2.33 times the endline annual treatment effects (reflecting the ratio of the period between midline and endline to the duration of the one-year recall period), and the monthly flow values are 12 times the midline treatment effect plus 28 times the endline treatment effect. The estimates arrived at through this step-function assumption are conservative relative to the other obvious assumption which would involve a linear interpolation of the outcomes through the midline and endline outcomes for the

duration of treatment. The ingredients for this exercise, then, can be seen in the midline monthly flow income and consumption ITTs (replicated for clarity in Appendix Table A.27), the midline and endline annual flow impacts on intra-household transfers shown in Table A.28, the endline treatment effect on stock variables and consumption flows in Tables 2 and 3.

The results of this exercise are presented in Table 14. The top row of the table provides the cash amount received in each arm. A starting point is to observe that, relative to impacts observed at midline, 50–85% of the original transfer is still present in the total stock value of assets, depending on arm. So the incremental wealth value of the transfer has not been spent down, even in a period in which the control group was spending down assets on the whole. If the remainder had been simply spent without ever producing any income, we would then expect to see the total inflow equal the transfer amount, and the total consumption equal the difference between the original transfer and the current stock impact. Instead, we see that the outflows exceed the difference between transfers and stock values, but an amount ranging from about \$140 in the lower and large arms to \$430 in the middle arm. So what then is the source of this extra money? The answer is clear in the total inflows; in every case the total inflows greatly exceed the value of the transfer. The ratio of the total inflows to the transfer is a simple measure of the 'multiplier' effect of the cash; this ratio varies from 1.3 in the upper arm to 2.5 in the middle arm, showing that in all arms the transfers were put to work to create additional income. In all arms the additional income generated (over and above the transfer) is similar to or larger than the spending not accounted for by the draw-down in asset value (original transfer minus current stock impacts).[14]

This approach to aggregating financial impacts across the full duration of the study provides an omnibus test of the statistical significance of cumulative impacts relative to control. To undertake such a test, we undertake a randomization inference exercise: we permute treatments to provide alternative randomizations consistent with our block-randomized allocation. To test the significance of individual arms, we permute (within blocks) the assignment between those arms and control; to test the significance of comparisons between arms, we permute assignment of the relevant pairwise combinations of arms, again within blocks. Randomization inference provides a distribution of the total income, total expenditure, and total final stock values. We compare the realized differences between arms to these permutation distributions to obtain a randomization inference p-value.

Consistent with ITT results, this exercise in Table 14 confirms the statistical significance of cumulative expenditure and stock-value effects of all cash-transfer arms, relative to control. Cash-transfer effects on cumulative income are significant for the Middle and Large transfer values. The Combined arm has significant effects on cumulative values of each of income, expenditure, and asset values. HD alone has statistically insignificant cumulative effects on income and expenditure, and just misses significance at the ten percent level for its impacts on final stock values ($p = 0.11$).

Clearly, on average, the cash transfers have been put to work to drive substantial additional income, enabling outflows to increase by a total of 65 to 120% of the transfer amount while leaving

[14]The last row 'Survey share accounted' gives the fraction of outflows expected as a function of changes in net inflows that we are able to capture. If all measures are complete, this is an accounting identity, so the ratio of outflows to inflows provides a measure of survey quality.

the majority of the transfer intact after almost four years in asset values. At the same time, the fact that arms that more than double the value of productive assets lead only to a 20% increase in consumption at endline suggests that the return on these assets is low, and the labor devoted to operating them is receiving an effective wage rate similar to counterfactual uses of time. So these transfers are used by beneficiaries in a careful, productive way with an eye to the long term, but the opportunities for transformative enterprise-driven growth appear limited.

6 Conclusions

This study provides a unique window on the comparative effects of efforts to help disadvantaged youth climb the economic ladder. Given clean experimental variation, high treatment compliance rates, excellent survey tracking for more than three years, and little apparent contamination from spillovers, it is a very straightforward environment in which to view the relative benefits of cash- versus kind-based programming in the medium term. Divergent trajectories are apparent, with workforce training weakly increasing paid work and strongly elevating the chance of full-time employment, and cash transfers enabling self-employment and engendering the creation of profitable businesses that survive for years. Both interventions induce quantitatively large improvements in productive assets. The effect of HD on productive hours, and the effect of cash on productive assets, remains relatively constant across midline and endline, suggesting that these impacts may prove durable in the long term. So meaningful changes in the lives of beneficiaries are visible years after the interventions.

On the other hand, the general pattern is that the benefits that are seen in this 3.5-year endline are about a half of what was observed in the midline one year after treatment. This suggests that—while there were significant effects on economic well-being during the study duration—over the longer term, beneficiaries are on a slow slide back towards the outcomes that they would have achieved in the absence of the programs. Roughly half of the new enterprises started at midline were no longer operative at endline, and the critical final outcomes of income and consumption are no longer improved relative to the control. So the takeaway is that while HD beneficiaries are indeed working more, and cash beneficiaries are operating more businesses with larger productive assets, ultimately the economic returns of these activities to the youth may not be higher than what they would have been doing otherwise (typically own-farm agriculture or agricultural wage labor). This suggests more systemic problems with the nature of the markets in which these youth work. Low returns to skill in local labor markets will limit what can be gained through training programs, and weak demand will constrain the potential of a self-employment led exit from poverty. So the fact that such substantial and expensive interventions do not transform the lives of disadvantaged youth refocuses attention on the macro constraints to growth that limit the ability of individuals to climb out of poverty.

A critical contextual factor for this study is that the period between the midline and endline includes the COVID era. While many programs that find diminishing long-term impacts use the

language of 'catch-up by the control' (Blattman et al., 2018), in this case the control group has seen a 30% drop in productive asset value between midline and endline, and the share of controls operating businesses dropped from 79% to 40%. Since both interventions induced beneficiaries to go into business and put assets at risk, the treatment groups were more exposed to the COVID shock at the same time as they had more wealth to protect themselves against it. While the treatment groups lost more productive assets between midline and endline than the control during COVID, they nonetheless retained more by endline as well. While more treatment-induced businesses became inoperative during COVID, because so many had been created initially, still more survived at the end than in the control. Evidence in this study suggests that it may be the limited ability of interventions to shield beneficiaries from the consequences of such shocks—as much as the rising tide of economic outcomes among those not receiving such benefits—that drives the attenuation of program impacts in the long term. While the specifics of the COVID shock were certainly unique, uncertainty and disruption are an unfortunate fact of life for entrepreneurs in developing economies. Hence, this mixture of treatment and resiliency impacts may provide a realistic picture of the extent to which these interventions are able to deliver longer-term benefits that persist through good times and bad.

Over the longer term we can focus our emphasis on the core outcomes of income, consumption, and subjective well-being that exemplify the ultimate impacts on economic welfare, rather than things like business assets or business knowledge that are merely instrumental to long-term welfare. In the endline none of these outcomes have significant cost-equivalent differences, but for household and individual consumption, subjective well-being, and business income the point estimates all suggest an edge for cash. Only overall income is somewhat higher at endline for HD, which hints at the possibility of more persistent income effects from the human-capital investment of HD. In the midline every one of these outcomes is significantly better for cash at cost-equivalent levels. Integrating these two snapshots in time over the entire duration of the study, then, it seems relatively clear that cash has done a better job of moving ultimate welfare outcomes at cost-equivalent levels than HD. The policy takeaway from this is that investments seeking create economic well-being over the short- to medium-term will do well to incorporate cash as at least a part of their programming.

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Tables and Figures

Table 1: Results of Costing Exercise

Treatment Arm:	Ex Ante Cost	Value received	Ex Post Cost	Fraction operating cost	Compliance Rate	Cost per study household
Huguka Dukore	$464.25	$153.47	$388.32	60.5%	85.6%	$332.27
GD lower	$377.03	$317.16	$394.39	19.6%	100%	$394.39
GD mid	$464.25	$410.65	$493.96	16.9%	99.4%	$490.99
GD upper	$571.74	$502.96	$590.41	14.8%	100%	$590.41
GD large	$828.47	$750.3	$846.71	11.3%	100%	$846.71
Combined	$928.5	$561.11	$885.64	36.3%	89.6%(HD), 100%(GD)	$840.20

Note: The first column shows the ex-ante costing data on which study was designed; the core number is the HD cost around which the GD actual transfer amounts in column 2 were designed. Column 3 shows the results of the ex post costing exercise. Column 4 provides the share of spending that did not reach the beneficiaries either in cash or in direct training and materials costs. Column 5 shows the compliance rates, and since all costs are averted for non-compliers then the final column shows the final cost per study subject for each arm that are the basis of the cost-equivalent comparisons.

Table 2: **Simple ITT, Primary Outcomes**.

		GiveDirectly					Control			*p*-values		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2	(a)	(b)	(c)
Employed	0.03	0.03	−0.02	−0.03	0.02	0.01	0.50	1822	0.15	0.91	0.71	0.79
	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)						
	[0.65]	[0.73]	[0.73]	[0.73]	[0.73]	[0.98]						
Productive hours	3.32*	0.35	0.63	0.56	2.32	2.09	19.43	1822	0.18	0.13	0.74	0.93
	(1.53)	(2.17)	(2.21)	(2.23)	(2.13)	(2.04)						
	[0.09]	[0.98]	[0.95]	[0.95]	[0.46]	[0.48]						
Monthly income	0.20	0.17	0.58	−0.23	0.71*	0.21	8.11	1822	0.16	0.86	0.65	0.21
	(0.26)	(0.37)	(0.35)	(0.38)	(0.33)	(0.35)						
	[0.73]	[0.73]	[0.16]	[0.73]	[0.09]	[0.73]						
Productive assets	0.92*	1.39*	2.43***	2.63***	2.97***	3.13***	3.90	1822	0.14	0.61	0.99	0.77
	(0.37)	(0.54)	(0.55)	(0.54)	(0.51)	(0.51)						
	[0.06]	[0.06]	[0.00]	[0.00]	[0.00]	[0.00]						
HH consumption per capita	0.03	0.11	0.20	0.17	0.18	0.18*	9.84	1810	0.19	0.48	0.76	0.94
	(0.06)	(0.10)	(0.10)	(0.09)	(0.09)	(0.08)						
	[0.73]	[0.43]	[0.11]	[0.11]	[0.11]	[0.07]						

Note: The six columns of the table provide the estimate on dummy variables for each of the treatment arms, compared to the control group. The five primary outcomes are in rows. Regressions include but do not report the lagged dependent variable, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates. Standard errors (in parentheses) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in brackets. Stars on coefficient estimates are derived from the FDR-corrected *p*-values, *=10%, **=5%, and ***=1% significance. Reported *p*-values in final three columns derived from *F*-tests of hypotheses that cost-benefit ratios are equal between: (a) GD Lower and HD; (b) GD Lower and GD Large; and (c) GD Large and Combined treatments. Employed is a dummy variable for spending more than 10 hours per week working for a wage or as primary operator of a microenterprise. Productive hours are measured over prior 7 days in all activities other than own-farm agriculture. Monthly income, productive assets, and household consumption are winsorized at 1% and 99% and analyzed in Inverse Hyperbolic Sine, meaning that treatment effects can be interpreted as percent changes.

Table 3: **Simple ITT, Secondary Outcomes**.

		GiveDirectly					Control			*p*-values		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2	(a)	(b)	(c)
Panel A. Beneficiary welfare												
Subjective well-being	0.12	0.17	0.28**	0.39***	0.16	0.28***	0.00	1822	0.10	0.80	0.29	0.21
	(0.07)	(0.09)	(0.10)	(0.08)	(0.08)	(0.08)						
	[0.13]	[0.12]	[0.04]	[0.00]	[0.12]	[0.01]						
Mental health	0.06	0.10	0.11	0.03	0.14	−0.01	−0.00	1822	0.06	0.78	0.67	0.14
	(0.07)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)						
	[0.23]	[0.20]	[0.19]	[0.41]	[0.16]	[0.48]						
Beneficiary-specific consumption	0.11	0.23	0.20	0.33*	0.11	0.28*	8.31	1822	0.23	0.46	0.14	0.25
	(0.10)	(0.13)	(0.14)	(0.15)	(0.14)	(0.11)						
	[0.20]	[0.12]	[0.17]	[0.08]	[0.28]	[0.06]						
Panel B. Household wealth												
HH net non-land wealth	0.24	0.66	0.02	0.70	0.85	0.50	11.30	1818	0.11	0.51	0.60	0.58
	(0.42)	(0.52)	(0.60)	(0.52)	(0.52)	(0.54)						
	[0.57]	[0.37]	[0.82]	[0.33]	[0.26]	[0.47]						
HH livestock wealth	−0.00	0.61	1.53**	0.84	1.65***	1.26**	7.65	1818	0.17	0.29	0.77	0.52
	(0.39)	(0.55)	(0.51)	(0.54)	(0.50)	(0.50)						
	[0.82]	[0.42]	[0.01]	[0.26]	[0.01]	[0.04]						
Savings	0.33	0.26	0.98***	1.17***	0.65*	0.80**	10.10	1822	0.14	0.69	0.89	0.61
	(0.23)	(0.33)	(0.26)	(0.25)	(0.29)	(0.26)						
	[0.29]	[0.53]	[0.00]	[0.00]	[0.07]	[0.01]						
Debt	−0.44	−0.02	−0.03	−0.23	−0.24	0.00	9.75	1822	0.14	0.24	0.83	0.61
	(0.28)	(0.41)	(0.42)	(0.40)	(0.40)	(0.37)						
	[0.26]	[0.82]	[0.82]	[0.57]	[0.57]	[0.82]						
Panel C. Beneficiary cognitive and non-cognitive skills												
Aspirations	0.07	0.15	0.08	−0.02	0.08	0.01	0.00	1822	0.06	0.43	0.14	0.50
	(0.07)	(0.08)	(0.08)	(0.09)	(0.09)	(0.08)						
	[1.00]	[0.47]	[1.00]	[1.00]	[1.00]	[1.00]						
Business knowledge	0.26***	−0.03	0.03	0.04	−0.03	0.37***	−0.00	1822	0.11	0.00	0.85	0.00
	(0.07)	(0.09)	(0.10)	(0.10)	(0.09)	(0.09)						
	[0.00]	[1.00]	[1.00]	[1.00]	[1.00]	[0.00]						
Business attitudes	−0.01	0.14	0.05	0.10	0.00	0.13	−0.00	1822	0.08	0.12	0.12	0.22
	(0.06)	(0.09)	(0.09)	(0.08)	(0.09)	(0.08)						
	[1.00]	[0.56]	[1.00]	[0.91]	[1.00]	[0.56]						

Notes: Regressions include but do not report the lagged dependent variable, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and *p*-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected *p*-values, *=10%, **=5%, and ***=1% significance. Reported *p*-values in final three columns derived from *F*-tests of hypotheses that cost-benefit ratios are equal between: (a) GD Lower and HD; (b) GD Lower and GD Large; and (c) GD Large and Combined treatments.

Table 4: **ITT Employment Breakdown**.

		GiveDirectly					Control			
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2	p-value
Panel A. Employment composition										
Non-agricultural microenterprise	0.05*	0.07*	0.07*	0.11***	0.17***	0.14***	0.16	1822	0.09	0.00
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)				
	[0.16]	[0.16]	[0.17]	[0.03]	[0.00]	[0.00]				
Other microenterprise or self-employment	0.02	0.04*	0.04	0.01	0.05**	0.06**	0.04	1822	0.06	0.08
	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)				
	[0.20]	[0.18]	[0.20]	[0.34]	[0.08]	[0.07]				
Agricultural processing or trading	0.04*	0.01	0.02	0.03	0.05	0.02	0.12	1822	0.06	0.61
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)				
	[0.18]	[0.35]	[0.33]	[0.30]	[0.18]	[0.33]				
Agricultural wage labor	−0.08***	−0.05	−0.05	−0.10***	−0.05	−0.08**	0.26	1822	0.15	0.04
	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)				
	[0.02]	[0.22]	[0.22]	[0.03]	[0.22]	[0.05]				
Non-agricultural wage labor	0.05*	−0.01	−0.03	−0.05	−0.03	0.02	0.30	1822	0.20	0.09
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)				
	[0.16]	[0.36]	[0.32]	[0.22]	[0.30]	[0.33]				
Panel B. Alternative hours thresholds										
Employed (0 hr)	0.02	0.01	0.01	−0.01	0.06	0.04	0.68	1822	0.13	0.73
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Employed (10 hr)	0.03	0.03	−0.02	−0.03	0.02	0.01	0.50	1822	0.15	0.85
	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Employed (20 hr)	0.07**	0.03	0.01	−0.01	0.01	0.03	0.29	1822	0.15	0.27
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)				
	[0.27]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Employed (30 hr)	0.08***	0.04	0.01	0.03	0.02	0.06	0.20	1822	0.15	0.14
	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)				
	[0.13]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Employed (40 hr)	0.06**	0.03	0.02	0.01	0.01	0.05*	0.14	1822	0.14	0.33
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)				
	[0.28]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				

Notes: Panel A presents impacts on indicators for employment of any hours in the corresponding activity type in the preceding week. Panel B presents impacts on an indicator for overall employment, using the reported threshold for minimum hours. Regressions include but do not report an indicator for lagged employment status, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across outcomes in each panel are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance.

Table 5: **Cost Equivalent Analysis, Primary Outcomes**.

	Differential impact of HD	Cost-equivalent GD impact	Transfer Value	Control Mean	Obs.	R^2
Employed	0.03	−0.01	0.00	0.50	1622	0.15
	(0.04)	(0.04)	(0.01)			
	[0.88]	[1.00]	[1.00]			
Productive hours	3.41	−0.07	0.44	19.43	1622	0.18
	(2.07)	(1.99)	(0.56)			
	[0.48]	[1.00]	[0.88]			
Monthly income	0.11	0.06	0.11	8.11	1622	0.16
	(0.33)	(0.33)	(0.09)			
	[1.00]	[1.00]	[0.59]			
Productive assets	−0.72	1.62**	0.30	3.90	1622	0.15
	(0.50)	(0.49)	(0.14)			
	[0.59]	[0.02]	[0.22]			
HH consumption per capita	−0.11	0.15	0.01	9.84	1612	0.20
	(0.09)	(0.09)	(0.02)			
	[0.59]	[0.48]	[1.00]			

Note: This table uses a linear adjustment of primary outcomes for program cost to compare HD and GD at exactly equivalent costs. The *Transfer value* column estimates the marginal effect of spending an extra $100 through cash transfers. The *Cost-equivalent GD impact* column is estimated as a dummy for either HD or GD treatment, and estimates the impact of cash at the exact cost of HD. The *Differential impact of HD* column then estimates the differential effect of HD above cash at this benchmarked cost. Regressions include but do not report the lagged dependent variable, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. Employed is a dummy variable for spending more than 10 hours per week working for a wage or as primary operator of a microenterprise. Productive hours are measured over prior 7 days in all activities other than own-farm agriculture. Monthly income, productive assets, and household consumption are winsorized at 1% and 99% and analyzed in Inverse Hyperbolic Sine, meaning that treatment effects can be interpreted as percent changes.

Table 6: **Cost Equivalent Analysis, Secondary Outcomes**.

	Differential impact of HD	Cost-equivalent GD impact	Transfer Value	Control Mean	Obs.	R^2
Panel A. Beneficiary welfare						
Subjective well-being	−0.15	0.29***	−0.01	0.00	1622	0.11
	(0.09)	(0.08)	(0.02)			
	[0.22]	[0.01]	[0.76]			
Mental health	−0.02	0.08	0.01	−0.00	1622	0.07
	(0.08)	(0.08)	(0.02)			
	[0.82]	[0.52]	[0.82]			
Beneficiary-specific consumption	−0.18	0.29	−0.03	8.31	1622	0.23
	(0.13)	(0.12)	(0.03)			
	[0.30]	[0.10]	[0.58]			
Panel B. Household wealth						
HH net non-land wealth	−0.10	0.33	0.10	11.30	1618	0.11
	(0.51)	(0.49)	(0.13)			
	[0.85]	[0.85]	[0.85]			
HH livestock wealth	−0.75	0.77	0.16	7.65	1618	0.18
	(0.49)	(0.49)	(0.13)			
	[0.73]	[0.73]	[0.73]			
Savings	−0.32	0.66	0.04	10.10	1622	0.15
	(0.27)	(0.28)	(0.08)			
	[0.73]	[0.29]	[0.85]			
Debt	−0.50	−0.01	−0.06	9.75	1622	0.14
	(0.37)	(0.37)	(0.11)			
	[0.73]	[0.96]	[0.85]			
Panel C. Beneficiary cognitive and non-cognitive skills						
Aspirations	−0.04	0.12	−0.01	0.00	1622	0.07
	(0.08)	(0.08)	(0.02)			
	[0.80]	[0.32]	[0.75]			
Business knowledge	0.24*	0.01	−0.01	−0.00	1622	0.11
	(0.09)	(0.09)	(0.02)			
	[0.05]	[0.84]	[0.84]			
Business attitudes	−0.15	0.14	−0.03	−0.00	1622	0.09
	(0.08)	(0.08)	(0.02)			
	[0.31]	[0.31]	[0.37]			

Note: This table uses a linear adjustment of secondary outcomes for program cost to compare HD and GD at exactly equivalent costs. The *Transfer value* column estimates the marginal effect of spending an extra $100 through cash transfers. The *Cost-equivalent GD impact* column is estimated as a dummy for either HD or GD treatment, and estimates the impact of cash at the exact cost of HD. The *Differential impact of HD* column then estimates the differential effect of HD above cash at this benchmarked cost. Regressions include but do not report the lagged dependent variable, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all outcomes within each family are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance.

Table 7: **Benefit-Cost Ratios, Primary Outcomes**.

		GiveDirectly					p-values			
	HD	Lower	Middle	Upper	Large	Combined	(a)	(b)	(c)	(d)
Employed	0.008	0.007	−0.004	−0.005	0.002	0.001	0.61	0.91	0.71	0.79
	(0.010)	(0.012)	(0.010)	(0.007)	(0.005)	(0.005)				
Productive hours	0.999	0.088	0.129	0.095	0.274	0.249	0.36	0.13	0.74	0.93
	(0.460)	(0.550)	(0.450)	(0.378)	(0.251)	(0.243)				
Monthly income	0.060	0.043	0.118	−0.039	0.084	0.025	0.35	0.86	0.65	0.21
	(0.080)	(0.094)	(0.072)	(0.065)	(0.039)	(0.041)				
Productive assets	0.278	0.352	0.494	0.445	0.351	0.373	0.43	0.61	0.99	0.77
	(0.112)	(0.137)	(0.112)	(0.091)	(0.060)	(0.061)				
HH consumption	0.010	0.028	0.041	0.028	0.021	0.022	0.70	0.48	0.76	0.94
per capita	(0.019)	(0.024)	(0.021)	(0.015)	(0.010)	(0.009)				

Note: Table gives the impact per $100 spent, which is calculated by dividing the estimated ITT impacts by the cost per arm in hundreds of dollars. The standard errors in the table are similarly the ITT SEs divided by costs. Reported p-values in final three columns derived from F-tests of hypotheses that cost-benefit ratios are equal between: (a) joint test across all arms, (b) GD Lower and HD; (c) GD Lower and GD Large; and (d) GD Large and Combined arms.

Table 8: **Beneficiary Business Birth and Death.**

	GiveDirectly						Control		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2
New business at midline	0.22***	0.55***	0.55***	0.61***	0.64***	0.65***	0.50	1770	0.12
	(0.05)	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)			
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]			
New business at endline	0.05	0.01	0.05	0.05	0.16***	0.10**	0.24	1822	0.02
	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)			
	[0.11]	[0.53]	[0.22]	[0.19]	[0.00]	[0.05]			
Dead at endline	-0.00	0.00	0.02	-0.01	-0.03	0.03	0.24	1240	0.02
	(0.04)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)			
	[0.54]	[0.54]	[0.50]	[0.50]	[0.40]	[0.36]			
Inoperative at endline	-0.00	0.07	0.14*	0.16**	0.20***	0.17***	0.14	1240	0.04
	(0.04)	(0.06)	(0.07)	(0.06)	(0.06)	(0.05)			
	[0.54]	[0.19]	[0.05]	[0.02]	[0.00]	[0.00]			
Operative at endline	0.04	0.12*	0.16**	0.16**	0.26***	0.16**	0.36	1240	0.03
	(0.05)	(0.07)	(0.07)	(0.07)	(0.08)	(0.06)			
	[0.28]	[0.09]	[0.03]	[0.03]	[0.00]	[0.02]			

Notes: Table uses the panel of firms reported on in the beneficiary survey, totalling variables within each round. The first two rows count the number of new firms in each round that had not existed in the prior round, by beneficiary, and then impute zeros for individuals who reported no firms in the survey. Rows 3-5 then take the universe of individuals who reported on any firm in the midline, and count the outcomes for those firms at endline; whether they were no longer operational (died), existed but were inoperative, or were operative. Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table 9: **Midline Beneficiary enterprise analysis**.

		GiveDirectly					Control		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2
Operating Businesses	0.14***	0.44***	0.55***	0.62***	0.65***	0.63***	0.79	1770	0.10
	(0.06)	(0.09)	(0.10)	(0.09)	(0.10)	(0.08)			
	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]			
Owned Businesses	0.14***	0.48***	0.57***	0.60***	0.66***	0.66***	0.71	1770	0.11
	(0.06)	(0.08)	(0.09)	(0.09)	(0.09)	(0.07)			
	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]			
Household Employees	0.17***	0.22***	0.22***	0.42***	0.43***	0.24***	0.26	1770	0.03
	(0.06)	(0.09)	(0.08)	(0.11)	(0.10)	(0.09)			
	[0.00]	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]			
Non Household Employees	0.09**	0.14**	0.17***	0.32***	0.29***	0.24***	0.14	1770	0.03
	(0.05)	(0.07)	(0.08)	(0.09)	(0.09)	(0.08)			
	[0.01]	[0.01]	[0.01]	[0.00]	[0.00]	[0.00]			
Days Worked per month	2.71***	8.01***	6.65***	11.59***	11.37***	8.63***	9.11	1770	0.07
	(1.07)	(1.51)	(1.77)	(1.70)	(1.76)	(1.50)			
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]			
Customers per month	9.95**	42.22***	46.65***	38.47***	42.46***	38.26***	26.28	1770	0.03
	(6.69)	(14.72)	(18.21)	(12.72)	(11.93)	(11.53)			
	[0.02]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]			
Daily Sales	1.67**	7.76***	13.48***	12.25***	10.23***	13.73***	6.64	1770	0.05
	(1.42)	(2.36)	(3.11)	(3.13)	(2.85)	(3.44)			
	[0.03]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]			
Monthly Profits	4.36***	9.00***	8.63***	11.34***	10.85***	8.88***	6.04	1770	0.05
	(1.38)	(2.25)	(2.02)	(2.42)	(2.35)	(1.70)			
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]			

Notes: Table analyzes the results of the midline beneficiary enterprise survey, totalling variables across all businesses operated by a given beneficiary and then imputing zeros to survey respondents with no businesses. Analysis is weighted using attrition weights. Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table 10: **Endline Beneficiary enterprise analysis**.

	GiveDirectly						Control		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2
Operating Businesses	0.10**	0.18**	0.27***	0.29***	0.42***	0.33***	0.40	1822	0.05
	(0.04)	(0.07)	(0.07)	(0.07)	(0.08)	(0.06)			
	[0.03]	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]			
Owned Businesses	0.11**	0.17**	0.27***	0.30***	0.44***	0.34***	0.43	1822	0.05
	(0.05)	(0.07)	(0.07)	(0.07)	(0.08)	(0.07)			
	[0.02]	[0.02]	[0.00]	[0.00]	[0.00]	[0.00]			
Household Employees	0.06*	0.10*	0.10*	0.20***	0.20***	0.13**	0.17	1822	0.02
	(0.04)	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)			
	[0.07]	[0.05]	[0.05]	[0.00]	[0.00]	[0.03]			
Non Household Employees	0.03	0.04	0.06*	0.03	0.23***	0.13**	0.09	1822	0.03
	(0.04)	(0.05)	(0.05)	(0.05)	(0.08)	(0.06)			
	[0.14]	[0.14]	[0.10]	[0.16]	[0.01]	[0.03]			
Days Worked per month	1.34*	3.03**	3.33***	4.69***	5.59***	6.27***	5.66	1822	0.03
	(0.80)	(1.27)	(1.18)	(1.28)	(1.31)	(1.29)			
	[0.05]	[0.02]	[0.01]	[0.00]	[0.00]	[0.00]			
Customers per month	-0.55	24.18*	8.90	13.50	24.78**	11.40	33.29	1822	0.01
	(8.10)	(16.35)	(13.73)	(15.25)	(13.41)	(11.87)			
	[0.22]	[0.06]	[0.14]	[0.14]	[0.05]	[0.14]			
Daily Sales	2.12**	6.54***	1.93*	5.12**	5.50***	3.25**	3.11	1822	0.02
	(0.99)	(2.18)	(1.24)	(1.95)	(1.72)	(1.47)			
	[0.03]	[0.01]	[0.06]	[0.01]	[0.00]	[0.03]			
Monthly Profits	1.64*	3.30**	3.08**	5.13***	7.11***	6.46***	4.57	1822	0.04
	(0.96)	(1.61)	(1.46)	(1.69)	(1.97)	(1.57)			
	[0.05]	[0.03]	[0.03]	[0.00]	[0.00]	[0.00]			

Notes: Table analyzes the results of the endline beneficiary enterprise survey, totalling variables across all businesses operated by a given beneficiary and then imputing zeros to survey respondents with no businesses. Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table 11: **Marriage and Fertility**.

		GiveDirectly					Control		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2
Married or Cohabiting	-0.00	0.10	0.08	0.06	0.09	0.06	0.33	1822	0.04
	(0.03)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)			
	[0.79]	[0.33]	[0.33]	[0.40]	[0.33]	[0.40]			
Married	-0.01	0.02	0.05	0.07	0.06	0.06	0.16	1822	0.06
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)			
	[0.67]	[0.67]	[0.36]	[0.33]	[0.36]	[0.36]			
Cohabiting	0.01	0.08	0.03	-0.01	0.03	-0.01	0.18	1822	0.04
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)			
	[0.67]	[0.33]	[0.59]	[0.67]	[0.59]	[0.74]			
Any Children	0.01	0.06	0.01	-0.03	0.07	0.09	0.54	1822	0.02
	(0.03)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)			
	[0.67]	[0.40]	[0.67]	[0.59]	[0.36]	[0.33]			
Desired Fertility	-0.11	-0.09	-0.01	-0.02	0.03	-0.10	2.85	1822	0.01
	(0.05)	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)			
	[0.33]	[0.40]	[0.74]	[0.67]	[0.67]	[0.36]			

Notes: Table analyzes endline marriage and fertility outcomes at the beneficiary level. The first row is a dummy for whether the individual was either married or cohabiting at endline, and then rows 2 and 3 break these outcomes apart and analyze them separately. Row 4 analyzes a dummy for whether the beneficiary has any children as of the time of the endline, and Row 5 uses the response to the question "what is the total number of children you would like to have in your lifetime, including those that you have already". Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table 12: **Marriage and Fertility with Gender Interactions**.

	Married Cohabiting	Married	Cohabiting	Any Children	Desired Fertility
HD	−0.01	0.02	−0.03	0.02	−0.00
	(0.05)	(0.04)	(0.04)	(0.05)	(0.07)
	[0.88]	[0.64]	[0.55]	[0.81]	[0.95]
GD Lower	0.12	0.09	0.04	0.13	0.11
	(0.07)	(0.06)	(0.06)	(0.07)	(0.10)
	[0.17]	[0.22]	[0.61]	[0.17]	[0.38]
GD Mid	0.12	0.10	0.01	−0.01	0.24*
	(0.07)	(0.06)	(0.06)	(0.07)	(0.11)
	[0.22]	[0.16]	[0.88]	[0.88]	[0.09]
GD Upper	0.05	0.14*	−0.09	0.02	0.13
	(0.07)	(0.06)	(0.05)	(0.07)	(0.09)
	[0.55]	[0.07]	[0.16]	[0.88]	[0.26]
GD Large	0.16*	0.11	0.05	0.18*	0.09
	(0.07)	(0.05)	(0.06)	(0.07)	(0.09)
	[0.08]	[0.13]	[0.47]	[0.06]	[0.38]
Combined	0.18*	0.17**	0.01	0.18*	0.08
	(0.06)	(0.06)	(0.05)	(0.06)	(0.09)
	[0.05]	[0.04]	[0.88]	[0.05]	[0.47]
HD × Female	0.01	−0.05	0.06	−0.03	−0.18
	(0.06)	(0.05)	(0.05)	(0.06)	(0.09)
	[0.90]	[0.38]	[0.38]	[0.70]	[0.14]
GD Lower × Female	−0.03	−0.11	0.08	−0.10	−0.33*
	(0.09)	(0.07)	(0.08)	(0.09)	(0.14)
	[0.81]	[0.22]	[0.40]	[0.38]	[0.08]
GD Mid × Female	−0.06	−0.09	0.03	0.01	−0.41**
	(0.09)	(0.07)	(0.08)	(0.09)	(0.14)
	[0.55]	[0.38]	[0.82]	[0.90]	[0.04]
GD Upper × Female	0.02	−0.12	0.14	−0.08	−0.25
	(0.09)	(0.08)	(0.07)	(0.09)	(0.13)
	[0.88]	[0.22]	[0.14]	[0.47]	[0.13]
GD Large × Female	−0.11	−0.08	−0.03	−0.19*	−0.10
	(0.09)	(0.07)	(0.07)	(0.08)	(0.12)
	[0.30]	[0.38]	[0.74]	[0.08]	[0.47]
Combined × Female	−0.20*	−0.18*	−0.02	−0.14	−0.30*
	(0.08)	(0.07)	(0.07)	(0.08)	(0.12)
	[0.07]	[0.07]	[0.81]	[0.17]	[0.07]
Female	0.13**	0.12**	0.01	0.38***	0.32***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.07)
	[0.04]	[0.01]	[0.86]	[0.00]	[0.00]
Control mean	0.42	0.20	0.22	0.58	2.87
Observations	1822	1822	1822	1822	1822
R^2	0.05	0.06	0.04	0.13	0.03

Notes: Table presents tests for heterogeneity of marriage and fertility effects by Gender. Interacted coefficients in the first six rows give the differential effect of each arm for women, 'Female' gives the difference between women and men in the control group, and the uninteracted treatment terms give the impact of each arm for men. Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table 13: **Are treatments protective against measured Covid shocks?**

	Employed	Productive hours	Income	Productive assets	Consumption
HD	0.02	3.52**	0.16	0.82**	0.05
	(0.03)	(1.63)	(0.26)	(0.37)	(0.06)
× index	0.15	-0.17	0.29	0.12	0.46
	(0.32)	(0.31)	(0.24)	(0.26)	(0.28)
GD main	0.00	1.48	0.10	2.14***	0.17***
	(0.03)	(1.64)	(0.26)	(0.37)	(0.06)
× index	0.17	0.38	-0.12	0.33	0.19
	(0.33)	(0.32)	(0.24)	(0.27)	(0.28)
GD large	0.02	2.17	0.64*	2.80***	0.16*
	(0.04)	(2.22)	(0.36)	(0.50)	(0.09)
× index	0.75*	0.58	0.27	0.00	0.77**
	(0.43)	(0.43)	(0.31)	(0.35)	(0.38)
Combined	0.02	3.36	0.28	3.09***	0.15*
	(0.04)	(2.13)	(0.34)	(0.48)	(0.08)
× index	0.09	-0.03	-0.24	0.30	0.18
	(0.41)	(0.41)	(0.30)	(0.35)	(0.37)
Predictive index	0.09	0.06	0.59***	0.51**	-2.73***
	(0.24)	(0.24)	(0.18)	(0.20)	(0.41)
	1822	1822	1822	1822	1810

Notes: All specifications include controls for the baseline value of the outcome, as well as lasso-selected controls and block fixed effects, as in the ITT specification. *Predictive index* is the predicted value of the endline outcome, based on plausibly exogenous covid shock measures, with model estimated by cross-validated lasso in the control group only.

Table 14: **Aggregating cash flows over the study period**.

			Cash transfers				
	Control mean	HD	Lower	Middle	Upper	Large	Combined
Panel A. Flow impacts through midline							
Cash received	0.00	0.00	317.16	410.65	502.96	750.30	410.65
Beneficiary income	209.36	65.73	158.39	226.36	239.48	152.48	218.09
Transfers received	23.38	−7.78	40.91	62.42	61.20	72.71	57.55
Household consumption	625.85	34.19	124.50	166.52	146.09	223.07	169.13
Loans made	3.83	0.43	3.46	1.74	5.41	2.61	4.74
Transfers made	4.53	1.78	3.08	8.48	2.56	1.47	3.66
Panel B. Flow impacts between midline and endline							
Beneficiary income	612.46	121.86	103.35	354.79	−140.25	437.05	127.87
Transfers received	27.83	−8.00	−25.64	−17.79	1.36	−37.89	−6.88
Household consumption	1606.45	54.99	179.17	325.16	269.98	282.53	292.69
Loans made	20.44	−2.32	−11.62	−16.18	−10.76	−20.47	−7.43
Transfers made	7.54	1.26	1.15	0.36	1.99	−1.41	1.41
Panel C. Final stock values							
Livestock	118.64	−0.35	72.79	181.61	99.88	196.00	149.56
Productive assets	49.89	46.14	69.29	120.98	131.07	148.07	156.20
Savings	51.99	17.22	13.53	50.98	60.93	33.96	42.42
Debt	61.93	−27.46	−1.41	−1.77	−14.01	−14.95	0.31
Panel D. Totals							
Total income	.	171.81	594.16	1036.44	664.75	1374.65	807.29
		[0.29]	[0.24]	[0.01]	[0.51]	[0.01]	[0.06]
Total expenditure	.	90.33	299.74	486.08	415.26	487.81	464.20
		[0.47]	[0.09]	[0.01]	[0.01]	[0.01]	[0.00]
Total stock values	.	90.48	157.02	355.34	305.88	392.98	347.87
		[0.11]	[0.09]	[0.00]	[0.00]	[0.00]	[0.00]
Share accounted	.	1.05	0.77	0.81	1.08	0.64	1.01

Notes. Table presents control means and estimated impacts on financial values, in dollars. Flow consumption is measured in the survey monthly, and here we use the midline treatment effect for the first 12 months and the endline effect for the subsequent 28 months. Inter-household flows are measured with an annual recall, and we take a similar approach, using midline estimates for the midline period and endline estimates for the period between midline and endline. All other variables are stocks measured at follow-up. *Total income* is the sum of cash received, beneficiary income, and transfers received. *Total expenditure* is the sum of household consumption, loans made, and transfers made. *Total stock values* are the sum of livestock values, other productive asset values, savings values, and the negative of debt values. Randomization inference p-values, in brackets, from test of null of no cumulative effect of each arm on income. *Share accounted* is the ratio of the sum of total outflows plus stock values to total inflows.

Figure 1: Cost Equivalence versus Cost Effectiveness, Endline

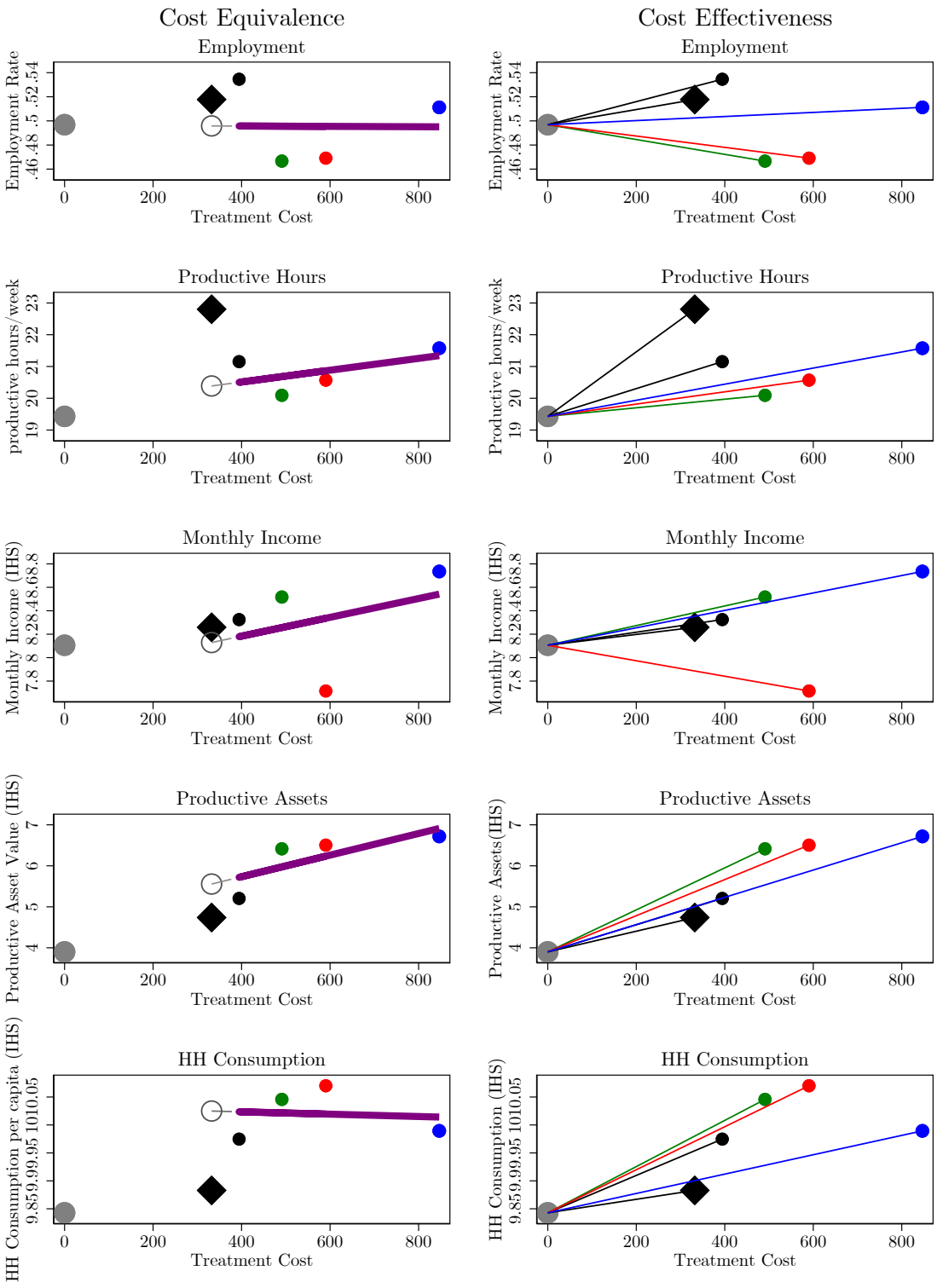
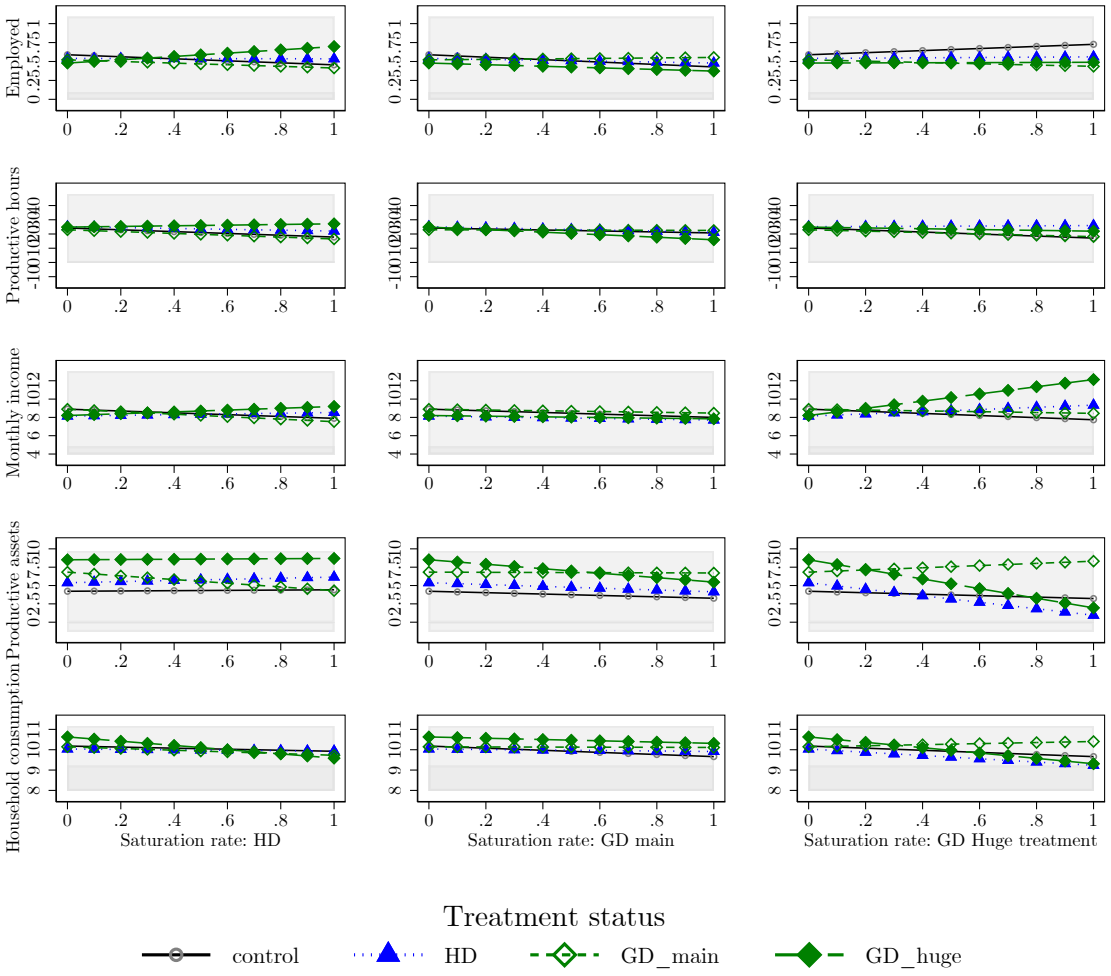


Figure 2: Graphical Representation of Spillovers



Notes: Each panel presents predicted outcomes under each of the four main treatment arms (Control, HD, GD Main, and GD Large), as the saturation level of a specific active treatment arm changes. Rows correspond to the outcomes of employment, productive hours, and the inverse hyperbolic sine of monthly incomes, productive assets, and household consumption per adult-equivalent, respectively. Horizontal shaded bands highlight one standard deviation above and below the control mean. Columns illustrate effects of variation in saturation rates in HD, GD-main, and GD-large, respectively. All predicted outcomes evaluated at means of covariates used in the estimating equation.

Figure 3: Incidence of Covid-19 shocks, by treatment arm

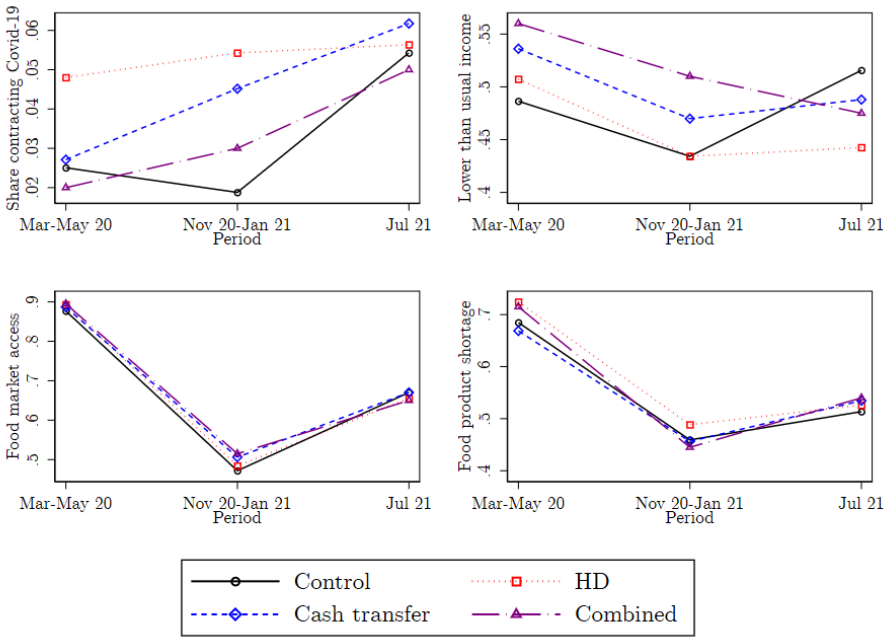
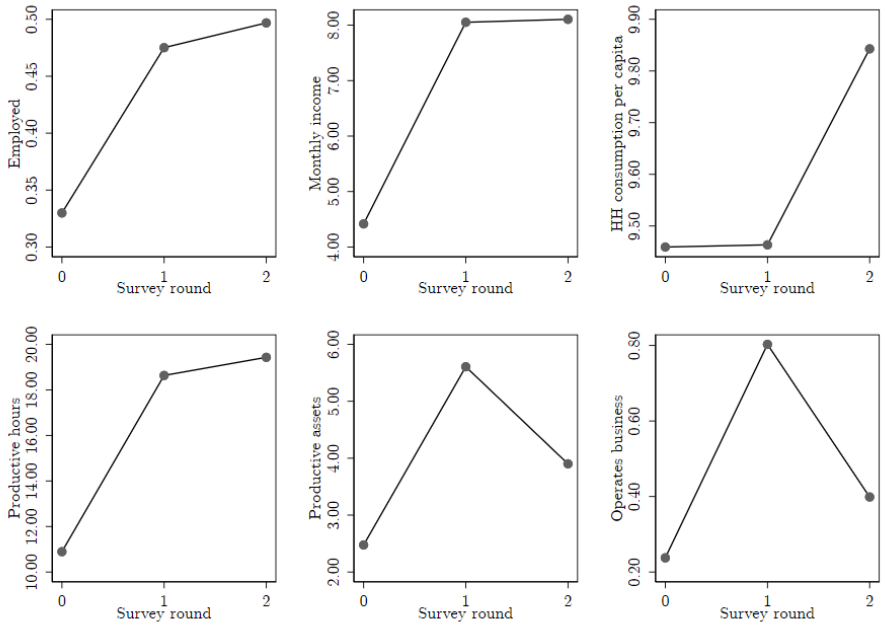


Figure 4: Evolution of control-group primary outcomes across survey rounds



Notes: Endline outcomes deflated to nominal midline Rwanda francs.

Appendix A Appendix Tables

Table A.1: Study Design

			GiveDirectly				Combined
Sector	Control	Huguka Dukore	317.16	410.65	502.96	750.30	HD + 410.65
Kaduha	63	60	21	21	22	22	26
Kibumbwe	32	37	10	10	12	13	13
Kigabiro	14	12	4	5	4	5	5
Kiyumba	17	17	6	6	6	6	8
Mugano	51	51	18	18	18	18	22
Muhazi	39	40	13	19	13	18	17
Munyaga	34	34	10	10	10	12	14
Munyiginya	25	25	8	8	8	10	10
Musange	30	29	10	10	10	9	12
Mushishiro	24	23	6	6	6	9	8
Nyakariro	49	50	16	17	19	17	22
Nyarusange	57	54	21	20	19	19	24
Shyogwe	53	53	18	18	18	20	22
Total	488	485	161	168	165	178	203

Note : This table gives the number of study individuals assigned to each treatment arm in each of the 13 sectors within which lotteries were conducted. The lotteries were blocked so that fixed fractions of individuals are assigned to each arm.

Table A.2: **Attrition: Tracking at Endline by Treatment**.

	Any Treatment	Basic Treatment	Granular Treatment
Any Treatment	0.00594		
	(0.00678)		
HD		0.00607	0.00607
		(0.00790)	(0.00790)
Treatment arm: Gd		0.00654	
		(0.00738)	
Combined		0.00366	0.00366
		(0.0104)	(0.0104)
GD Small			0.00602
			(0.0107)
GD Middle			0.000585
			(0.0119)
GD Upper			0.000261
			(0.0120)
GD Huge			0.0184***
			(0.00608)
Observations	1848	1848	1848
Mean DV	0.986	0.986	0.986
R2	0.000495	0.000545	0.00202

Notes: Table examines overall attrition rates from the endline survey by treatment arm. Sample is the entire baseline survey, outcome variable is a dummy for being successfully tracked at endline. Covariates are the treatment arm for each individual. Standard errors clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table A.3: **Attrition: Tracking at Endline on Covariates**.

	R3 tracked	Control mean	Observations	R^2
Ubudehe category I	−0.10 (0.10) [1.00]	0.33	1797	0.00
Beneficiary female	0.18 (0.10) [0.89]	0.60	1848	0.00
Beneficiary age	0.17 (0.66) [1.00]	23.36	1848	0.00
Beneficiary years of education	0.04 (0.43) [1.00]	7.58	1848	0.00
Household members	−0.32 (0.49) [1.00]	4.84	1844	0.00
Employed	−0.08 (0.10) [1.00]	0.34	1848	0.00
Productive hours	−5.86 (4.47) [1.00]	11.19	1848	0.00
Monthly income	−1.05 (1.01) [1.00]	4.45	1848	0.00
Productive assets	−0.30 (0.91) [1.00]	2.20	1848	0.00
HH consumption per capita	−0.17 (0.19) [1.00]	9.39	1844	0.00
Beneficiary-specific consumption	−0.67 (0.24) [0.10]	7.51	1848	0.00
HH net non-land wealth	−0.22 (1.17) [1.00]	10.59	1844	0.00
Savings	0.41 (0.94) [1.00]	7.87	1848	0.00
Debt	−1.01 (0.83) [1.00]	7.97	1848	0.00
HH livestock wealth	−0.01 (1.17) [1.00]	7.32	1844	0.00
Business Knowledge	0.15 (0.18) [1.00]	−0.00	1848	0.00

Notes: Table correlates attrition from the endline survey with baseline covariates. Sample is the entire baseline survey, outcome variable is a dummy for being successfully tracked at endline. Standard errors clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table A.4: **Balance using Endline Sample**.

		GiveDirectly					Control			
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2	p-value
Ubudehe category I	0.01	−0.00	0.04	0.00	0.00	−0.03	0.32	1771	0.07	0.90
	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Beneficiary female	0.02	−0.02	0.06	−0.01	0.01	−0.03	0.60	1822	0.04	0.66
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Beneficiary age	−0.12	−0.48	−0.20	−0.72	0.28	−0.38	23.53	1822	0.04	0.11
	(0.22)	(0.30)	(0.34)	(0.31)	(0.31)	(0.30)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Beneficiary years of education	0.10	0.00	−0.15	0.10	0.15	−0.25	7.55	1822	0.07	0.47
	(0.14)	(0.20)	(0.19)	(0.19)	(0.20)	(0.17)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Household members	−0.29	−0.34	0.02	0.06	−0.04	−0.26	4.99	1818	0.03	0.31
	(0.15)	(0.23)	(0.33)	(0.20)	(0.21)	(0.18)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Employed	0.04	0.02	−0.03	0.00	0.01	0.02	0.33	1822	0.02	0.74
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Productive hours	0.62	0.24	0.32	1.76	−0.24	−0.59	10.89	1822	0.02	0.94
	(1.20)	(1.78)	(1.79)	(1.93)	(1.52)	(1.37)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Monthly income	0.20	0.09	−0.18	−0.04	−0.11	0.09	4.42	1822	0.01	0.98
	(0.32)	(0.46)	(0.45)	(0.46)	(0.44)	(0.41)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Productive assets	−0.54	−0.36	−0.09	−0.27	−0.44	−0.26	2.48	1822	0.03	0.57
	(0.27)	(0.38)	(0.38)	(0.39)	(0.37)	(0.36)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
HH consumption per capita	−0.12	−0.09	−0.12	−0.10	−0.16	−0.03	9.46	1818	0.05	0.49
	(0.07)	(0.09)	(0.09)	(0.10)	(0.09)	(0.09)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Beneficiary-specific consumption	−0.09	0.05	−0.11	−0.24	0.15	−0.00	7.55	1822	0.03	0.74
	(0.14)	(0.19)	(0.21)	(0.22)	(0.17)	(0.19)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
HH net non-land wealth	−0.09	0.06	−0.02	−0.03	1.11	−0.17	10.57	1818	0.03	0.17
	(0.45)	(0.56)	(0.64)	(0.68)	(0.45)	(0.57)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Savings	−0.27	−0.47	−0.46	0.05	0.10	0.21	8.00	1822	0.04	0.61
	(0.29)	(0.40)	(0.43)	(0.39)	(0.37)	(0.36)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Debt	0.04	−0.24	−0.51	−0.17	0.04	0.57	7.97	1822	0.02	0.48
	(0.31)	(0.44)	(0.46)	(0.44)	(0.43)	(0.38)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
HH livestock wealth	0.18	−0.14	0.28	0.26	−0.16	−0.25	7.31	1818	0.02	0.96
	(0.40)	(0.56)	(0.57)	(0.54)	(0.53)	(0.51)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				
Business Knowledge	−0.03	0.06	−0.03	−0.03	−0.02	0.06	0.01	1822	0.02	0.91
	(0.07)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)				
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]				

Notes: Table examines balance of the experiment across treatment arms using baseline covariates and the attrited sample that is used for endline analysis. Standard errors clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table A.5: **Robustness of Cost Equivalence Adjustment, Primary Outcomes**.

	Base Linear	Quad-ratic	Cubic	Drop lower	Drop mid	Drop upper	Drop huge
Employed	0.030	−0.039	−0.063	0.076	0.020	0.018	−0.012
	(0.042)	(0.068)	(0.146)	(0.057)	(0.047)	(0.044)	(0.056)
Productive hours	3.412*	2.756	3.661	3.889	3.466	3.248	2.982
	(2.069)	(3.401)	(7.041)	(2.867)	(2.293)	(2.160)	(2.764)
Monthly income	0.111	−0.382	1.397	0.230	0.357	−0.089	−0.328
	(0.332)	(0.553)	(1.146)	(0.447)	(0.384)	(0.343)	(0.452)
Productive assets	−0.721	0.048	0.857	−1.287*	−0.511	−0.644	−0.277
	(0.498)	(0.810)	(1.742)	(0.686)	(0.562)	(0.519)	(0.666)
HH consumption per capita	−0.110	−0.058	0.077	−0.171	−0.081	−0.117	−0.070
	(0.086)	(0.144)	(0.315)	(0.120)	(0.096)	(0.091)	(0.118)

Notes: Table reports the coefficient on the differential effect of HD over cost-equivalent cash using seven different specifications. Column 1 is the linear adjustment reported elsewhere. Column 2 includes a quadratic, and column 3 a quadratic and cubic term in the cost deviations from Gikuriro. Columns 4-7 leave out one of the cash treatment arms and repeat the linear cost adjustment. Asterices denote significance at the 10, 5, and 1 percent levels, and are based on household-clustered standard errors, in parentheses.

Table A.6: **Robustness of Cost Equivalence Adjustment, Secondary Outcomes**.

	Base Linear	Quad-ratic	Cubic	Drop lower	Drop mid	Drop upper	Drop huge
Panel A. Beneficiary welfare							
Subjective well-being	−0.155*	0.098	−0.016	−0.293**	−0.160*	−0.105	0.013
	(0.087)	(0.136)	(0.310)	(0.123)	(0.096)	(0.092)	(0.114)
Mental health	−0.019	−0.085	0.027	0.021	−0.023	−0.039	−0.059
	(0.080)	(0.132)	(0.269)	(0.106)	(0.091)	(0.084)	(0.109)
Beneficiary-specific consumption	−0.179	−0.055	−0.305	−0.229	−0.219	−0.152	−0.099
	(0.125)	(0.192)	(0.417)	(0.181)	(0.138)	(0.128)	(0.160)
Panel B. Household wealth							
HH net non-land wealth	−0.053	−0.416	−2.292	0.510	−0.435	0.002	−0.265
	(0.509)	(0.774)	(1.764)	(0.730)	(0.549)	(0.530)	(0.645)
HH livestock wealth	−0.747	−0.773	1.351	−0.928	−0.426	−0.844*	−0.903
	(0.488)	(0.818)	(1.671)	(0.644)	(0.569)	(0.507)	(0.667)
Savings	−0.325	0.539	0.811	−0.895***	−0.211	−0.183	0.231
	(0.271)	(0.461)	(0.898)	(0.319)	(0.321)	(0.283)	(0.376)
Debt	−0.500	−0.565	−0.036	−0.542	−0.301	−0.579	−0.561
	(0.374)	(0.614)	(1.373)	(0.516)	(0.429)	(0.387)	(0.505)
Panel C. Beneficiary cognitive and non-cognitive skills							
Aspirations	−0.037	−0.194	−0.069	0.047	−0.036	−0.070	−0.160
	(0.075)	(0.120)	(0.248)	(0.105)	(0.084)	(0.077)	(0.099)
Business knowledge	0.243***	0.330**	0.367	0.185	0.253***	0.245***	0.299***
	(0.087)	(0.141)	(0.305)	(0.122)	(0.098)	(0.091)	(0.116)
Business attitudes	−0.151*	−0.150	−0.365	−0.103	−0.190**	−0.130	−0.147
	(0.081)	(0.128)	(0.277)	(0.111)	(0.088)	(0.084)	(0.106)

Notes: Table reports the coefficient on the differential effect of HD over cost-equivalent cash using seven different specifications. Column 1 is the linear adjustment reported elsewhere. Column 2 includes a quadratic, and column 3 a quadratic and cubic term in the cost deviations from Gikuriro. Columns 4-7 leave out one of the cash treatment arms and repeat the linear cost adjustment. Asterices denote significance at the 10, 5, and 1 percent levels, and are based on household-clustered standard errors, in parentheses.

Table A.7: **Benefit-Cost Ratios, Secondary Outcomes**.

		GiveDirectly					*p*-values			
	HD	Lower	Middle	Upper	Large	Combined	(a)	(b)	(c)	(d)
Panel A. Beneficiary welfare										
Subjective well-being	0.036 (0.021)	0.043 (0.023)	0.058 (0.021)	0.067 (0.014)	0.019 (0.010)	0.033 (0.010)	0.01	0.80	0.29	0.21
Mental health	0.019 (0.020)	0.026 (0.022)	0.022 (0.018)	0.006 (0.015)	0.017 (0.010)	−0.001 (0.010)	0.89	0.78	0.67	0.14
Beneficiary-specific consumption	0.034 (0.030)	0.059 (0.032)	0.042 (0.029)	0.055 (0.025)	0.013 (0.016)	0.033 (0.013)	0.43	0.46	0.14	0.25
Panel B. Household wealth										
HH net non-land wealth	0.071 (0.126)	0.167 (0.132)	0.004 (0.123)	0.119 (0.087)	0.100 (0.061)	0.060 (0.064)	0.86	0.51	0.60	0.58
HH livestock wealth	−0.001 (0.117)	0.156 (0.139)	0.312 (0.104)	0.143 (0.092)	0.195 (0.059)	0.150 (0.059)	0.15	0.29	0.77	0.52
Savings	0.100 (0.068)	0.066 (0.083)	0.199 (0.053)	0.198 (0.042)	0.077 (0.035)	0.096 (0.031)	0.02	0.69	0.89	0.61
Debt	−0.133 (0.084)	−0.006 (0.103)	−0.006 (0.086)	−0.038 (0.068)	−0.029 (0.048)	0.001 (0.044)	0.64	0.24	0.83	0.61
Panel C. Beneficiary cognitive and non-cognitive skills										
Aspirations	0.022 (0.020)	0.038 (0.020)	0.017 (0.017)	−0.003 (0.016)	0.009 (0.011)	0.001 (0.010)	0.40	0.43	0.14	0.50
Business knowledge	0.079 (0.021)	−0.008 (0.024)	0.006 (0.020)	0.007 (0.016)	−0.004 (0.011)	0.044 (0.011)	0.00	0.00	0.85	0.00
Business attitudes	−0.002 (0.019)	0.035 (0.022)	0.010 (0.019)	0.018 (0.014)	0.001 (0.010)	0.016 (0.010)	0.42	0.12	0.12	0.22

Note: Table gives the impact per $100 spent, which is calculated by dividing the estimated ITT impacts by the cost per arm in hundreds of dollars. The standard errors in the table are similarly the ITT SEs divided by costs. Reported p-values in final three columns derived from F-tests of hypotheses that cost-benefit ratios are equal between: (a) joint test across all arms, (b) GD Lower and HD; (c) GD Lower and GD Large; and (d) GD Large and Combined arms.

Table A.8: **Standard Complementarities test, Primary Outcomes**.

	HD	GD	Complementarity	Control Mean	Obs.	R^2
Employed	0.04	−0.01	−0.02	0.50	1323	0.17
	(0.03)	(0.05)	(0.06)			
	[0.67]	[1.00]	[1.00]			
Productive hours	3.78*	0.22	−1.80	19.43	1323	0.20
	(1.51)	(2.21)	(3.05)			
	[0.06]	[1.00]	[1.00]			
Monthly income	0.27	0.54	−0.58	8.11	1323	0.16
	(0.27)	(0.36)	(0.52)			
	[0.67]	[0.42]	[0.67]			
Productive assets	0.95*	2.35***	−0.26	3.90	1323	0.16
	(0.38)	(0.56)	(0.77)			
	[0.06]	[0.00]	[1.00]			
HH consumption per capita	0.04	0.21	−0.08	9.84	1313	0.24
	(0.06)	(0.10)	(0.13)			
	[1.00]	[0.14]	[1.00]			

Notes: Table replicates specification of ITT analysis of primary outcomes, with each GD transfer value treated as a separate arm. The table derives two measures of complementarities and the associated p-values for the corresponding null of no complementary in brackets. Complementarity test (a) is the canonical 2×2 test: we estimate $\delta^{Combined} - \left(\delta^{HD} + \delta^{GD\,Middle}\right)$ and test the null that this difference is equal to zero. Complementarity test (b) reports the estimated difference between the Combined and GD Large arms, and a p-value associated with the null of equality. Complementarity test (c) tests the null that the difference between the coefficients on the Combined and GD-Large treatments is equal to the difference between the coefficients on HD and that on GD-Lower. We report the point estimate for this difference-in-differences, $\left(\delta^{Combined} - \delta^{GD\,Large}\right) - \left(\delta^{HD} - \delta^{GD\,Lower}\right)$ in the notation of the primary estimating equation, and the p-value from the corresponding test of this null below it in brackets.

Table A.9: **Standard Complementarities test, Secondary Outcomes**.

	HD	GD	Complementarity	Control Mean	Obs.	R^2
Panel A. Beneficiary welfare						
Subjective well-being	0.13	0.29*	−0.12	0.00	1323	0.11
	(0.07)	(0.11)	(0.14)			
	[0.38]	[0.06]	[0.48]			
Mental health	0.06	0.11	−0.20	−0.00	1323	0.08
	(0.07)	(0.09)	(0.13)			
	[0.48]	[0.46]	[0.45]			
Beneficiary-specific consumption	0.11	0.19	−0.03	8.31	1323	0.24
	(0.10)	(0.14)	(0.19)			
	[0.47]	[0.46]	[0.61]			
Panel B. Household wealth						
HH net non-land wealth	0.24	0.04	0.25	11.30	1321	0.13
	(0.43)	(0.62)	(0.82)			
	[1.00]	[1.00]	[1.00]			
HH livestock wealth	0.03	1.60**	−0.24	7.65	1321	0.19
	(0.39)	(0.52)	(0.74)			
	[1.00]	[0.01]	[1.00]			
Savings	0.38	0.94***	−0.45	10.10	1323	0.16
	(0.23)	(0.27)	(0.38)			
	[0.51]	[0.01]	[0.76]			
Debt	−0.36	−0.03	0.54	9.75	1323	0.17
	(0.28)	(0.43)	(0.59)			
	[0.76]	[1.00]	[1.00]			
Panel C. Beneficiary cognitive and non-cognitive skills						
Aspirations	0.07	0.06	−0.11	0.00	1323	0.08
	(0.07)	(0.08)	(0.12)			
	[1.00]	[1.00]	[1.00]			
Business knowledge	0.26***	0.03	0.09	−0.00	1323	0.14
	(0.07)	(0.10)	(0.14)			
	[0.00]	[1.00]	[1.00]			
Business attitudes	0.01	0.06	0.04	−0.00	1323	0.13
	(0.06)	(0.09)	(0.13)			
	[1.00]	[1.00]	[1.00]			

Notes: Table replicates specification of ITT analysis of secondary outcomes, with each GD transfer value treated as a separate arm. The table derives two measures of complementarities and the associated p-values for the corresponding null of no complementary in brackets. Complementarity test (a) is the canonical 2×2 test: we estimate $\delta^{Combined} - \left(\delta^{HD} + \delta^{GD\ Middle}\right)$ and test the null that this difference is equal to zero. Complementarity test (b) reports the estimated difference between the Combined and GD Large arms, and a p-value associated with the null of equality. Complementarity test (c) tests the null that the difference between the coefficients on the Combined and GD-Large treatments is equal to the difference between the coefficients on HD and that on GD-Lower. We report the point estimate for this difference-in-differences, $\left(\delta^{Combined} - \delta^{GD\ Large}\right) - \left(\delta^{HD} - \delta^{GD\ Lower}\right)$ in the notation of the primary estimating equation, and the p-value from the corresponding test of this null below it in brackets.

Table A.10: **Heterogeneity analysis by Gender**.

	Employed	Productive Hours	Monthly Income	Productive Assets	Consumption
HD	0.03	4.78	0.37	1.47*	0.07
	(0.05)	(2.73)	(0.33)	(0.61)	(0.10)
	[1.00]	[0.36]	[1.00]	[0.07]	[1.00]
GD main	−0.03	2.19	0.10	2.76***	0.33**
	(0.05)	(2.78)	(0.35)	(0.61)	(0.11)
	[1.00]	[1.00]	[1.00]	[0.00]	[0.01]
GD large	−0.02	3.25	0.37	2.86***	0.14
	(0.07)	(4.02)	(0.45)	(0.83)	(0.13)
	[1.00]	[1.00]	[1.00]	[0.00]	[1.00]
Combined	0.01	3.83	0.20	3.58***	0.30*
	(0.06)	(3.55)	(0.47)	(0.80)	(0.12)
	[1.00]	[1.00]	[1.00]	[0.00]	[0.05]
HD × Female	−0.00	−1.51	−0.25	−1.06	−0.04
	(0.06)	(3.28)	(0.49)	(0.76)	(0.13)
	[1.00]	[1.00]	[1.00]	[0.66]	[1.00]
GD main × Female	0.05	−0.95	0.02	−1.07	−0.25
	(0.06)	(3.26)	(0.51)	(0.77)	(0.14)
	[1.00]	[1.00]	[1.00]	[0.66]	[0.34]
GD large × Female	0.06	−1.62	0.48	−0.14	0.01
	(0.09)	(4.63)	(0.65)	(1.06)	(0.17)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Combined × Female	0.01	−1.52	0.03	−0.92	−0.23
	(0.08)	(4.38)	(0.68)	(1.02)	(0.16)
	[1.00]	[1.00]	[1.00]	[1.00]	[0.66]
Female	−0.29***	−13.80***	−2.15***	0.03	0.00
	(0.04)	(2.20)	(0.35)	(0.51)	(0.09)
	[0.00]	[0.00]	[0.00]	[1.00]	[1.00]
Control mean	0.50	19.43	8.11	3.90	9.84
Observations	1822	1822	1822	1822	1810
R^2	0.08	0.10	0.07	0.06	0.04
p-value	0.86	0.99	0.86	0.54	0.25

Notes: Table presents tests for heterogeneity of treatment effects by Gender. Uninteracted coefficients in the first four rows give the treatment effect of the program on men, and the next four rows test for the differential effect between women and men. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. p-value in the last row from an F-test on whether treatments have a jointly differential effect by gender.

Table A.11: **Heterogeneity analysis by Age**.

	Employed	Productive Hours	Monthly Income	Productive Assets	Consumption
HD	0.04	4.42	0.65	0.68	−0.02
	(0.05)	(2.48)	(0.42)	(0.55)	(0.10)
	[1.00]	[0.71]	[0.75]	[1.00]	[1.00]
GD main	0.00	1.63	0.02	2.77***	0.24
	(0.05)	(2.40)	(0.43)	(0.55)	(0.10)
	[1.00]	[1.00]	[1.00]	[0.00]	[0.16]
GD large	0.05	1.70	1.29	2.56**	0.04
	(0.07)	(3.55)	(0.54)	(0.78)	(0.13)
	[1.00]	[1.00]	[0.16]	[0.02]	[1.00]
Combined	−0.02	2.82	0.54	2.36**	0.11
	(0.06)	(3.14)	(0.56)	(0.76)	(0.12)
	[1.00]	[1.00]	[1.00]	[0.03]	[1.00]
HD × Older than 22	−0.03	−1.60	−0.86	0.22	0.10
	(0.06)	(3.26)	(0.54)	(0.73)	(0.13)
	[1.00]	[1.00]	[0.75]	[1.00]	[1.00]
GD main × Older than 22	0.00	−0.16	0.24	−1.23	−0.10
	(0.06)	(3.18)	(0.54)	(0.74)	(0.13)
	[1.00]	[1.00]	[1.00]	[0.75]	[1.00]
GD large × Older than 22	−0.06	0.65	−1.12	0.34	0.18
	(0.09)	(4.56)	(0.69)	(1.02)	(0.17)
	[1.00]	[1.00]	[0.75]	[1.00]	[1.00]
Combined × Older than 22	0.07	1.14	−0.41	1.34	0.11
	(0.08)	(4.25)	(0.72)	(1.00)	(0.16)
	[1.00]	[1.00]	[1.00]	[0.98]	[1.00]
Older than 22	0.06	1.33	1.09*	0.25	0.04
	(0.05)	(2.19)	(0.38)	(0.50)	(0.09)
	[0.97]	[1.00]	[0.05]	[1.00]	[1.00]
Control mean	0.50	19.43	8.11	3.90	9.84
Observations	1822	1822	1822	1822	1810
R^2	0.02	0.02	0.02	0.06	0.04
p-value	0.76	0.97	0.16	0.10	0.41

Notes: Table presents tests for heterogeneity of treatment effects by age. First four rows give effect of treatment among young, and next four rows test for differential treatment effect for those 23 and over. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. p-value in the last row from an F-test on whether treatments have a jointly differential effect by gender.

Table A.12: **Heterogeneity analysis by Baseline Consumption**.

	Employed	Productive Hours	Monthly Income	Productive Assets	Consumption
HD	0.03	3.66	0.16	0.84	0.07
	(0.03)	(1.60)	(0.27)	(0.37)	(0.06)
	[0.90]	[0.13]	[1.00]	[0.13]	[0.87]
GD main	0.00	1.62	0.09	2.15***	0.20**
	(0.03)	(1.59)	(0.27)	(0.37)	(0.07)
	[1.00]	[0.87]	[1.00]	[0.00]	[0.03]
GD large	0.02	2.28	0.65	2.83***	0.17
	(0.04)	(2.23)	(0.34)	(0.51)	(0.08)
	[1.00]	[0.87]	[0.22]	[0.00]	[0.20]
Combined	0.02	3.54	0.28	3.06***	0.18
	(0.04)	(2.12)	(0.35)	(0.50)	(0.08)
	[1.00]	[0.33]	[1.00]	[0.00]	[0.13]
HD × Baseline HH consumption per AE	−0.00	−1.46	−0.01	−0.48	0.04
	(0.03)	(1.60)	(0.28)	(0.37)	(0.07)
	[1.00]	[0.90]	[1.00]	[0.57]	[1.00]
GD main × Baseline HH consumption per AE	−0.03	−1.55	0.02	0.08	0.04
	(0.03)	(1.63)	(0.27)	(0.36)	(0.07)
	[0.87]	[0.90]	[1.00]	[1.00]	[1.00]
GD large × Baseline HH consumption per AE	−0.03	−2.08	−0.06	−0.24	−0.11
	(0.05)	(2.32)	(0.37)	(0.54)	(0.08)
	[1.00]	[0.90]	[1.00]	[1.00]	[0.57]
Combined × Baseline HH consumption per AE	−0.03	−2.94	−0.03	0.08	−0.08
	(0.04)	(2.02)	(0.34)	(0.47)	(0.09)
	[1.00]	[0.50]	[1.00]	[1.00]	[0.90]
Baseline HH consumption per AE	0.03	2.07	0.06	0.49	0.18***
	(0.02)	(1.13)	(0.20)	(0.25)	(0.05)
	[0.57]	[0.25]	[1.00]	[0.20]	[0.00]
Control mean	0.50	19.43	8.11	3.90	9.84
Observations	1822	1822	1822	1822	1810
R^2	0.01	0.02	0.01	0.06	0.07
p-value	0.80	0.64	1.00	0.58	0.30

Notes: Table presents tests for heterogeneity of treatment effects by baseline Household Consumption. Consumption demeaned before interaction so first four rows give effect of treatment at average value, and next four rows test for differential treatment effect by consumption. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. p-value in the last row from an F-test on whether treatments have a jointly differential effect by gender.

Table A.13: **Heterogeneity analysis by Sector-level Employment**.

	Employed	Productive Hours	Monthly Income	Productive Assets	Consumption
HD	0.02	3.41	0.14	0.79	0.04
	(0.03)	(1.61)	(0.27)	(0.37)	(0.06)
	[1.00]	[0.25]	[1.00]	[0.25]	[1.00]
GD main	0.00	1.54	0.08	2.10***	0.18
	(0.03)	(1.60)	(0.27)	(0.38)	(0.07)
	[1.00]	[1.00]	[1.00]	[0.00]	[0.10]
GD large	0.02	2.15	0.63	2.78***	0.14
	(0.04)	(2.25)	(0.33)	(0.52)	(0.08)
	[1.00]	[1.00]	[0.40]	[0.00]	[0.53]
Combined	0.02	3.47	0.28	3.06***	0.17
	(0.04)	(2.11)	(0.35)	(0.50)	(0.08)
	[1.00]	[0.53]	[1.00]	[0.00]	[0.25]
HD × Baseline cell share employed	0.03	19.50	−0.61	0.62	−0.30
	(0.31)	(15.88)	(2.53)	(3.51)	(0.62)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
GD main × Baseline cell share employed	−0.01	−7.40	−1.53	0.00	0.01
	(0.32)	(16.04)	(2.66)	(3.64)	(0.75)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
GD large × Baseline cell share employed	0.35	4.67	−1.56	−0.29	−1.06
	(0.41)	(21.44)	(3.24)	(5.10)	(0.77)
	[1.00]	[1.00]	[1.00]	[1.00]	[0.77]
Combined × Baseline cell share employed	0.56	32.59	1.79	1.14	−0.57
	(0.39)	(20.00)	(3.41)	(4.74)	(0.74)
	[0.75]	[0.53]	[1.00]	[1.00]	[1.00]
Baseline cell share employed	0.17	0.26	1.68	1.16	0.16
	(0.24)	(11.06)	(1.88)	(2.61)	(0.49)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Control mean	0.50	19.43	8.11	3.90	9.84
Observations	1822	1822	1822	1822	1810
R^2	0.01	0.02	0.01	0.06	0.04
p-value	0.52	0.27	0.89	1.00	0.65

Notes: Table presents tests for heterogeneity of treatment effects by baseline Employment Rates. Employment demeaned before interaction so first four rows give effect of treatment at average value, and next four rows test for differential treatment effect by employment rates. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. p-value in the last row from an F-test on whether treatments have a jointly differential effect by gender.

Table A.14: **Midline Household enterprise analysis**.

	GiveDirectly						Control		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2
Owned	-0.02	-0.01	0.04	0.00	-0.02	-0.01	0.06	1770	0.02
Businesses	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.02)			
	[0.92]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]			
Household	-0.05	-0.08	-0.03	-0.03	-0.08	-0.08	0.12	1770	0.02
Employees	(0.05)	(0.04)	(0.06)	(0.05)	(0.04)	(0.04)			
	[1.00]	[0.92]	[1.00]	[1.00]	[0.92]	[0.92]			
Non Household	-0.02	-0.05	-0.04	-0.03	0.04	0.06	0.10	1770	0.01
Employees	(0.05)	(0.04)	(0.05)	(0.05)	(0.07)	(0.07)			
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]			
Days Worked per	-1.04	0.13	0.31	-1.51	1.46	0.31	4.57	1770	0.02
month	(0.74)	(1.00)	(1.23)	(0.86)	(1.11)	(1.01)			
	[0.92]	[1.00]	[1.00]	[0.92]	[0.92]	[1.00]			
Customers per	-2.47	-2.97	-0.98	-3.29	-1.90	-2.45	3.92	1770	0.01
month	(1.61)	(1.54)	(1.93)	(1.55)	(1.84)	(1.65)			
	[0.92]	[0.92]	[1.00]	[0.92]	[1.00]	[0.92]			
Daily Sales	-0.15	-1.36	0.54	-0.88	2.65	0.33	3.42	1770	0.01
	(0.86)	(0.78)	(1.30)	(0.97)	(1.71)	(1.05)			
	[1.00]	[0.92]	[1.00]	[1.00]	[0.92]	[1.00]			
Monthly Profits	-1.53	-1.54	-0.78	-1.93	0.58	0.21	5.22	1770	0.02
	(1.26)	(1.33)	(1.55)	(1.41)	(1.74)	(1.77)			
	[1.00]	[1.00]	[1.00]	[0.92]	[1.00]	[1.00]			

Notes: Table analyzes the results of the midline household enterprise survey, totalling variables across all businesses operated within the household (other than by the beneficiary) and then imputing zeros to households with no businesses. Analysis is weighted using attrition weights. Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table A.15: **Endline Household enterprise analysis**.

	GiveDirectly						Control		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2
Owned	0.01	0.01	0.02	-0.01	0.03	-0.00	0.06	1822	0.01
Businesses	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)			
	[1.00]	[1.00]	[1.00]	[1.00]	[0.90]	[1.00]			
Household	-0.02	-0.08	-0.02	-0.05	-0.02	-0.07	0.10	1822	0.02
Employees	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.03)			
	[1.00]	[0.26]	[1.00]	[0.56]	[1.00]	[0.36]			
Non Household	0.00	-0.04	-0.05	-0.05	-0.01	-0.05	0.06	1822	0.01
Employees	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)			
	[1.00]	[0.62]	[0.36]	[0.38]	[1.00]	[0.26]			
Days Worked per	-0.23	1.40	0.06	-1.17	0.20	0.14	3.59	1822	0.03
month	(0.59)	(0.97)	(0.84)	(0.76)	(0.86)	(0.90)			
	[1.00]	[0.62]	[1.00]	[0.62]	[1.00]	[1.00]			
Customers per	5.70	-1.00	-3.18	-2.24	1.00	0.17	5.19	1822	0.02
month	(4.57)	(3.04)	(2.28)	(3.46)	(4.08)	(3.53)			
	[0.88]	[1.00]	[0.65]	[1.00]	[1.00]	[1.00]			
Daily Sales	0.74	4.12	-1.02	-2.53	0.18	-2.72	3.66	1822	0.01
	(1.80)	(4.95)	(1.36)	(1.31)	(1.83)	(1.24)			
	[1.00]	[1.00]	[1.00]	[0.40]	[1.00]	[0.36]			
Monthly Profits	-0.25	0.49	-0.61	-2.14	-0.71	-2.27	5.24	1822	0.02
	(1.37)	(1.72)	(1.93)	(1.40)	(1.48)	(1.25)			
	[1.00]	[1.00]	[1.00]	[0.62]	[1.00]	[0.47]			

Notes: Table analyzes the results of the endline household enterprise survey, totalling variables across all businesses operated within the household (other than by the beneficiary) and then imputing zeros to households with no businesses. Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table A.16: **Endline Beneficiary Enterprise with Gender Interactions**.

	Operating Business	Own Business	Household Employees	NonHH Employees	Days Worked	Monthly Customers	Daily Sales	Monthly Profits
HD	0.16	0.18	0.08	0.05	2.40	0.59	5.26	3.54
	(0.08)	(0.08)	(0.07)	(0.08)	(1.45)	(15.51)	(2.15)	(1.94)
	[0.18]	[0.14]	[0.58]	[0.84]	[0.36]	[1.00]	[0.10]	[0.29]
GD Lower	0.21	0.18	0.09	0.02	3.17	19.50	6.82	3.39
	(0.11)	(0.12)	(0.10)	(0.09)	(2.14)	(25.50)	(3.65)	(2.66)
	[0.29]	[0.39]	[0.63]	[1.00]	[0.40]	[0.74]	[0.29]	[0.48]
GD Mid	0.28*	0.27	0.02	0.17	3.35	−2.00	3.17	4.71
	(0.11)	(0.11)	(0.08)	(0.11)	(1.99)	(20.97)	(1.86)	(2.77)
	[0.10]	[0.10]	[1.00]	[0.39]	[0.35]	[1.00]	[0.35]	[0.35]
GD Upper	0.34**	0.36**	0.13	0.00	4.27	−1.07	7.39	7.60*
	(0.11)	(0.12)	(0.09)	(0.10)	(2.02)	(18.48)	(3.96)	(2.93)
	[0.03]	[0.03]	[0.46]	[1.00]	[0.18]	[1.00]	[0.29]	[0.09]
GD Large	0.45**	0.46**	0.19	0.44	7.48**	51.11	8.52	11.11**
	(0.14)	(0.14)	(0.10)	(0.18)	(2.33)	(29.15)	(3.58)	(3.83)
	[0.02]	[0.02]	[0.29]	[0.10]	[0.02]	[0.33]	[0.11]	[0.04]
Combined	0.39**	0.36**	0.04	0.17	8.46**	17.76	4.61	7.48*
	(0.11)	(0.11)	(0.08)	(0.11)	(2.37)	(22.08)	(2.92)	(2.71)
	[0.02]	[0.02]	[0.84]	[0.40]	[0.02]	[0.74]	[0.39]	[0.06]
HD × Female	−0.11	−0.11	−0.04	−0.03	−1.66	−1.41	−5.03	−3.01
	(0.09)	(0.10)	(0.09)	(0.09)	(1.73)	(17.76)	(2.34)	(2.17)
	[0.58]	[0.58]	[0.94]	[1.00]	[0.63]	[1.00]	[0.18]	[0.46]
GD Lower × Female	−0.05	−0.02	0.02	0.03	−0.28	7.78	−0.52	−0.20
	(0.14)	(0.15)	(0.12)	(0.10)	(2.65)	(33.26)	(4.46)	(3.32)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
GD Mid × Female	−0.02	−0.00	0.13	−0.16	0.06	17.60	−1.82	−2.36
	(0.14)	(0.14)	(0.12)	(0.12)	(2.50)	(28.31)	(2.43)	(3.24)
	[1.00]	[1.00]	[0.58]	[0.48]	[1.00]	[0.84]	[0.74]	[0.74]
GD Upper × Female	−0.10	−0.09	0.13	0.04	0.70	24.49	−3.84	−4.19
	(0.14)	(0.15)	(0.13)	(0.11)	(2.70)	(29.22)	(4.27)	(3.55)
	[0.82]	[0.84]	[0.63]	[1.00]	[1.00]	[0.74]	[0.69]	[0.56]
GD Large × Female	−0.06	−0.04	0.03	−0.34	−3.10	−43.23	−4.97	−6.57
	(0.17)	(0.17)	(0.13)	(0.19)	(2.82)	(31.35)	(3.95)	(4.35)
	[1.00]	[1.00]	[1.00]	[0.29]	[0.59]	[0.46]	[0.49]	[0.40]
Combined × Female	−0.10	−0.02	0.15	−0.08	−3.90	−11.67	−2.45	−1.87
	(0.14)	(0.14)	(0.12)	(0.13)	(2.70)	(25.15)	(3.27)	(3.33)
	[0.74]	[1.00]	[0.48]	[0.84]	[0.43]	[0.89]	[0.74]	[0.84]
Female	−0.04	−0.03	−0.08	−0.08	−1.07	−10.44	−0.96	−1.36
	(0.06)	(0.07)	(0.05)	(0.06)	(1.11)	(14.23)	(1.14)	(1.43)
	[0.84]	[0.84]	[0.39]	[0.48]	[0.63]	[0.74]	[0.74]	[0.63]
Control mean	0.40	0.43	0.17	0.09	5.66	33.29	3.11	4.57
Observations	1822	1822	1822	1822	1822	1822	1822	1822
R^2	0.05	0.05	0.02	0.04	0.04	0.01	0.03	0.05

Notes: Table presents tests for heterogeneity of beneficiary enterprise effects by Gender. Interacted coefficients give the differential effect of each arm for women, 'Female' gives the difference between women and men in the control group, and the uninteracted treatment terms give the impact of each arm for men. Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table A.17: **Creation of New Households**.

		GiveDirectly							
	HD	Lower	Middle	Upper	Large	Combined	Control Mean	Obs.	R^2
New HH in Midline	0.03	0.06	0.08	0.01	0.10	0.05	0.19	1770	0.02
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)			
	[0.55]	[0.50]	[0.28]	[0.92]	[0.23]	[0.55]			
HH Head R2	0.00	0.06	0.02	-0.01	-0.00	0.08	0.28	1770	0.02
	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)			
	[0.93]	[0.50]	[0.92]	[0.92]	[1.00]	[0.30]			
Spouse HH Head R2	-0.00	0.07	-0.01	-0.01	0.02	-0.02	0.16	1770	0.02
	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)			
	[1.00]	[0.36]	[0.93]	[0.92]	[0.92]	[0.92]			
New HH in Endline	0.02	0.06	0.10	0.06	0.04	0.01	0.31	1822	0.01
	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)			
	[0.92]	[0.55]	[0.23]	[0.50]	[0.82]	[0.93]			
HH Head R3	0.01	0.07	-0.05	0.02	0.06	0.10	0.51	1822	0.02
	(0.03)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)			
	[0.93]	[0.49]	[0.64]	[0.92]	[0.55]	[0.23]			
Spouse HH Head R3	0.01	0.09	0.04	0.04	0.02	0.01	0.22	1822	0.02
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)			
	[0.92]	[0.23]	[0.64]	[0.64]	[0.92]	[0.93]			
New HH Ever	0.04	0.09	0.13	0.11	0.12	0.03	0.40	1822	0.02
	(0.03)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)			
	[0.55]	[0.28]	[0.18]	[0.23]	[0.18]	[0.92]			

Notes: Table analyzes the movement and creation of new households by beneficiaries across survey waves. Rows 1 and 4 examine a dummy variable for whether the beneficiary was living in a different household than the baseline household at midline and endline, respectively. Rows 2 and 3 examine a dummy for whether the beneficiary is the household head or the spouse of the household head at midline, and Rows 5 and 6 for endline. The final row examines whether the beneficiary ever moved to a different household during the course of the study. Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table A.18: **Moving across villages**.

	p-values	GiveDirectly						Control		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2
Urban R2	-0.00	0.04	-0.03	-0.04	-0.04	-0.03	0.18	1770	0.24
	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)			
	[1.00]	[0.85]	[0.88]	[0.80]	[0.80]	[0.88]			
New Village R2	0.05	0.06	0.07	0.02	0.07	0.05	0.16	1770	0.02
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)			
	[0.74]	[0.80]	[0.80]	[0.88]	[0.74]	[0.80]			
Urban R3	-0.01	0.03	-0.02	-0.06	-0.07	-0.05	0.22	1822	0.16
	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)			
	[0.98]	[0.88]	[0.88]	[0.74]	[0.62]	[0.80]			
New Village R3	0.01	0.04	0.06	0.04	0.02	-0.01	0.37	1822	0.02
	(0.03)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)			
	[0.88]	[0.88]	[0.80]	[0.88]	[0.88]	[1.00]			

Notes: Table analyzes the extent to which the beneficiary had moved across villages at midline (R2) or endline (R3). 'Urban' is a dummy variable indicating that the village in which the beneficiary resides in that round is classified as semi-urban, peri-urban, or urban (rather than rural). 'New Village' is a dummy for the village being a different one than the baseline village. Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table A.19: **Education and Time Use**.

	GiveDirectly						Control		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2
Highest Grade	0.28	-0.14	-0.14	0.51	0.36	0.00	12.02	1822	0.05
	(0.25)	(0.35)	(0.33)	(0.33)	(0.34)	(0.33)			
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]			
Hours in School	-0.68	0.25	-0.88	-0.42	-0.52	-0.47	3.05	1822	0.01
	(0.45)	(0.66)	(0.51)	(0.65)	(0.58)	(0.60)			
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]			
Hours Domestic Work	-0.23	-3.16	-0.40	0.18	0.95	0.09	24.49	1822	0.02
	(1.18)	(1.51)	(1.55)	(1.53)	(1.76)	(1.59)			
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]			
Res wage in Village	-0.09	-0.00	1.43	0.05	0.03	0.16	1.68	1810	0.01
	(0.09)	(0.13)	(1.33)	(0.13)	(0.13)	(0.12)			
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]			
Res wage in Town	-0.25	0.07	1.80	0.02	-0.20	0.28	2.85	1804	0.01
	(0.17)	(0.29)	(1.68)	(0.23)	(0.20)	(0.23)			
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]			

Notes: Table analyzes endline education and time use variables. Highest grade is an ordinal variable measuring completed schooling with the control mean representing one year of post-primary education. 'Hours in School' and 'Hours Domestic Work' give the number of hours over the seven days prior to the endline that the respondent reports spending in each activity. 'Reservation wages' give the survey response to the daily wage the respendent said they would need to be paid to take a job in their village and in the nearest town, respectively (USD). Standard errors are clustered at the household level, *=10%, **=5%, and ***=1% significance.

Table A.20: Association between Covid-19 shock measures and endline outcomes in control group

	Employed	Productive hours	Income	Productive assets	Consumption
Model A. Conditional on baseline value of outcome					
Cumulative incidence of Covid 19 in household	0.01	4.71	0.22	0.35	0.05
	(0.07)	(3.21)	(0.55)	(0.73)	(0.13)
Cumulative experience of lower income	0.06***	2.01**	0.69***	0.71***	-0.00
	(0.02)	(0.91)	(0.16)	(0.21)	(0.04)
Cumulative experience of food market closures	-0.01	-3.01**	-0.40*	-0.01	-0.01
	(0.03)	(1.38)	(0.24)	(0.32)	(0.06)
Cumulative experience of food shortage	0.00	1.32	0.16	-0.33	-0.04
	(0.02)	(1.11)	(0.19)	(0.25)	(0.05)
Model B. Conditional on baseline and midline value of outcome					
Cumulative incidence of Covid 19 in household	0.01	4.28	0.17	0.41	0.05
	(0.07)	(3.17)	(0.54)	(0.68)	(0.13)
Cumulative experience of lower income	0.06***	2.13**	0.65***	0.69***	-0.01
	(0.02)	(0.90)	(0.15)	(0.19)	(0.04)
Cumulative experience of food market closures	-0.01	-2.87**	-0.45*	-0.13	-0.02
	(0.03)	(1.37)	(0.24)	(0.29)	(0.06)
Cumulative experience of food shortage	0.00	1.21	0.20	-0.35	-0.03
	(0.02)	(1.09)	(0.19)	(0.24)	(0.04)
Midline outcome	0.01	0.18***	0.18***	0.37***	0.28***
	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
Observations	479	479	479	479	473

Note: Each column within each panel represents a separate regression. All regressions control for baseline values of the corresponding outcome and for block fixed effects.

Table A.21: **Simple Spillover Analysis**.

	Employed	Productive Hours	Monthly Income	Productive Assets	Consumption
HD	0.02	2.70	0.14	0.93**	0.03
	(0.03)	(1.24)	(0.20)	(0.31)	(0.05)
	[0.69]	[0.12]	[0.69]	[0.02]	[0.69]
GD main	−0.01	0.07	0.11	2.18***	0.16**
	(0.03)	(1.27)	(0.20)	(0.33)	(0.05)
	[0.69]	[0.91]	[0.69]	[0.00]	[0.01]
GD Huge treatment	0.02	2.08	0.69	2.97***	0.17
	(0.04)	(2.22)	(0.31)	(0.51)	(0.09)
	[0.69]	[0.69]	[0.12]	[0.00]	[0.17]
HD Saturation	−0.03	−4.10	−0.37	−0.56	−0.33**
	(0.05)	(2.88)	(0.45)	(0.63)	(0.12)
	[0.69]	[0.40]	[0.69]	[0.69]	[0.03]
GD Main Saturation	−0.05	−3.41	−0.56	−0.96	−0.13
	(0.05)	(2.57)	(0.43)	(0.63)	(0.11)
	[0.69]	[0.40]	[0.40]	[0.37]	[0.40]
GD Large Saturation	0.01	−2.78	0.37	−1.46	−0.36
	(0.09)	(4.35)	(0.74)	(1.08)	(0.19)
	[0.85]	[0.69]	[0.69]	[0.40]	[0.18]
Control mean	0.50	19.43	8.11	3.90	9.84
Observations	1822	1822	1822	1822	1810
R^2	0.15	0.18	0.15	0.14	0.20
p-value	0.75	0.34	0.34	0.34	0.02

Notes: Table analyzes spillover effects of the three main treatments (HD, GD Main, and GD Large) on the five primary outcomes. The first three rows are dummy variables for own treatment status, and the next three are the saturation rates for the three treatments among others in the village, so measure the marginal effect of going from no one else treated to everyone else treated. Regressions include but do not report the lagged dependent variable, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates, and are weighted to reflect intensive tracking. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. Bottom row is the p-value on an F-test of the joint significance of the three saturation terms.

Table A.22: **Spillovers on Household Consumption**.

	Treatment		
	HD	GD Main	GD Huge
Direct effects of treatment at saturation level of zero			
Direct effect	−0.14	−0.04	0.45
	(0.13)	(0.14)	(0.26)
	[0.30]	[0.60]	[0.16]
Spillover effects of treatment onto control individuals			
Spillover to control	−0.26	−0.52*	−0.52
	(0.19)	(0.18)	(0.28)
	[0.23]	[0.05]	[0.15]
Additional effect of treatment onto individuals assigned to...			
HD	0.15	0.40	−0.28
	(0.21)	(0.21)	(0.37)
	[0.42]	[0.15]	[0.42]
GD main	−0.16	0.49	0.78
	(0.23)	(0.22)	(0.34)
	[0.42]	[0.14]	[0.14]
GD large	−0.79	0.20	−0.80
	(0.41)	(0.36)	(0.75)
	[0.15]	[0.49]	[0.30]
Saturation mean	0.36	0.36	0.09
Saturation SD	0.23	0.23	0.13
p-value	0.01	0.03	0.02

Notes: Each column describes the direct and spillover effects of a specific treatment on Household Consumption (IHS); all results in the table are from a single estimation. Saturation mean and standard deviation correspond to the distribution of saturation rates for the treatment in question. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. p-value in the last row corresponds to a test for whether the treatment in question has interference effects on any arm, including control.

Table A.23: **Spillovers on Employment**.

	Treatment		
	HD	GD Main	GD Huge
Direct effects of treatment at saturation level of zero			
Direct effect	−0.05	−0.07	−0.11
	(0.08)	(0.07)	(0.12)
	[1.00]	[0.98]	[0.98]
Spillover effects of treatment onto control individuals			
Spillover to control	−0.13	−0.16	0.14
	(0.09)	(0.09)	(0.15)
	[0.98]	[0.98]	[0.98]
Additional effect of treatment onto individuals assigned to...			
HD	0.13	0.10	−0.12
	(0.12)	(0.11)	(0.19)
	[0.98]	[0.98]	[1.00]
GD main	0.03	0.19	−0.22
	(0.12)	(0.12)	(0.19)
	[1.00]	[0.98]	[0.98]
GD large	0.35	0.06	−0.13
	(0.18)	(0.18)	(0.39)
	[0.98]	[1.00]	[1.00]
Saturation mean	0.36	0.36	0.09
Saturation SD	0.23	0.23	0.13
p-value	0.33	0.40	0.84

Notes: Each column describes the direct and spillover effects of a specific treatment on Employment; all results in the table are from a single estimation. Saturation mean and standard deviation correspond to the distribution of saturation rates for the treatment in question. Regressions include but do not report the lagged dependent variable, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates, and are weighted to reflect intensive tracking. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. p-value in the last row corresponds to a test for whether the treatment in question has interference effects on any arm, including control.

Table A.24: **Spillovers on Monthly Income**.

	Treatment		
	HD	GD Main	GD Huge
Direct effects of treatment at saturation level of zero			
Direct effect	−0.76	−0.00	−0.69
	(0.57)	(0.57)	(0.96)
	[0.74]	[1.00]	[0.74]
Spillover effects of treatment onto control individuals			
Spillover to control	−1.00	−0.90	−1.16
	(0.72)	(0.70)	(1.21)
	[0.74]	[0.74]	[0.74]
Additional effect of treatment onto individuals assigned to...			
HD	1.39	0.47	2.36
	(0.91)	(0.85)	(1.48)
	[0.74]	[0.74]	[0.74]
GD main	−0.36	0.48	0.70
	(0.90)	(0.90)	(1.54)
	[0.74]	[0.74]	[0.74]
GD large	1.99	0.54	5.08
	(1.54)	(1.24)	(2.74)
	[0.74]	[0.74]	[0.74]
Saturation mean	0.36	0.36	0.09
Saturation SD	0.23	0.23	0.13
p-value	0.35	0.74	0.24

Notes: Each column describes the direct and spillover effects of a specific treatment on Monthly Income (IHS); all results in the table are from a single estimation. Saturation mean and standard deviation correspond to the distribution of saturation rates for the treatment in question. Regressions include but do not report the lagged dependent variable, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates, and are weighted to reflect intensive tracking. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. p-value in the last row corresponds to a test for whether the treatment in question has interference effects on any arm, including control.

Table A.25: **Spillovers on Productive Hours**.

	Treatment		
	HD	GD Main	GD Huge
Direct effects of treatment at saturation level of zero			
Direct effect	0.92	−1.25	0.64
	(3.71)	(3.75)	(6.44)
	[1.00]	[1.00]	[1.00]
Spillover effects of treatment onto control individuals			
Spillover to control	−6.37	−3.49	−7.10
	(4.49)	(4.15)	(6.69)
	[1.00]	[1.00]	[1.00]
Additional effect of treatment onto individuals assigned to...			
HD	3.28	−0.50	7.88
	(5.84)	(5.35)	(9.50)
	[1.00]	[1.00]	[1.00]
GD main	−0.08	3.01	2.24
	(6.08)	(5.91)	(9.20)
	[1.00]	[1.00]	[1.00]
GD large	8.65	−5.42	4.22
	(9.63)	(7.97)	(17.10)
	[1.00]	[1.00]	[1.00]
Saturation mean	0.36	0.36	0.09
Saturation SD	0.23	0.23	0.13
p-value	0.47	0.60	0.85

Notes: Each column describes the direct and spillover effects of a specific treatment on Productive Hours; all results in the table are from a single estimation. Saturation mean and standard deviation correspond to the distribution of saturation rates for the treatment in question. Regressions include but do not report the lagged dependent variable, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates, and are weighted to reflect intensive tracking. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. p-value in the last corresponds to a test for whether the treatment in question has interference effects on any arm, including control.

Table A.26: **Spillovers on Productive Asset Values**.

	Treatment		
	HD	GD Main	GD Huge
Direct effects of treatment at saturation level of zero			
Direct effect	1.17	2.61**	4.30**
	(0.87)	(0.85)	(1.47)
	[0.58]	[0.03]	[0.03]
Spillover effects of treatment onto control individuals			
Spillover to control	0.21	−0.95	−0.99
	(1.04)	(0.97)	(1.72)
	[1.00]	[0.80]	[0.86]
Additional effect of treatment onto individuals assigned to...			
HD	0.57	−0.29	−3.45
	(1.47)	(1.35)	(2.22)
	[1.00]	[1.00]	[0.58]
GD main	−2.73	0.83	2.49
	(1.44)	(1.40)	(2.46)
	[0.35]	[0.86]	[0.80]
GD large	−0.02	−2.10	−5.57
	(2.27)	(2.16)	(4.17)
	[1.00]	[0.80]	[0.58]
Saturation mean	0.36	0.36	0.09
Saturation SD	0.23	0.23	0.13
p-value	0.30	0.34	0.04

Notes: Each column describes the direct and spillover effects of a specific treatment on Productive Assets (IHS); all results in the table are from a single estimation. Saturation mean and standard deviation correspond to the distribution of saturation rates for the treatment in question. Regressions include but do not report the lagged dependent variable, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates, and are weighted to reflect intensive tracking. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected p-values, *=10%, **=5%, and ***=1% significance. p-value in the last row corresponds to a test for whether the treatment in question has interference effects on any arm, including control.

Table A.27: **Midline ITT on Primary Outcomes**.

		GiveDirectly					Control			*p*-values		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2	(a)	(b)	(c)
Employed	0.02	0.03	0.05	0.00	0.01	0.01	0.48	1770	0.16	0.95	0.57	0.94
	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)						
	[0.30]	[0.30]	[0.16]	[0.50]	[0.46]	[0.49]						
Productive hours	2.79*	2.76	6.54***	3.56	1.12	2.31	18.64	1770	0.19	0.82	0.33	0.63
	(1.57)	(2.34)	(2.40)	(2.52)	(2.06)	(2.03)						
	[0.07]	[0.16]	[0.01]	[0.12]	[0.33]	[0.16]						
Monthly income	0.31	0.76**	1.08***	1.14***	0.73**	1.04***	8.05	1770	0.21	0.31	0.24	0.41
	(0.26)	(0.36)	(0.34)	(0.35)	(0.35)	(0.32)						
	[0.16]	[0.04]	[0.00]	[0.00]	[0.04]	[0.00]						
Productive assets	1.54***	3.94***	3.80***	3.84***	4.02***	4.42***	5.61	1770	0.26	0.00	0.00	0.42
	(0.35)	(0.46)	(0.50)	(0.46)	(0.47)	(0.44)						
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]						
HH consumption per capita	0.05	0.20**	0.27***	0.23***	0.36***	0.27***	9.46	1737	0.33	0.12	0.67	0.31
	(0.06)	(0.08)	(0.09)	(0.07)	(0.07)	(0.07)						
	[0.21]	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]						

Note: This table reproduces the midline Intention to Treat Effects of the study from **?**. Consumption impacts from this table are used in the Accounting for Cash exercise. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected *p*-values, *=10%, **=5%, and ***=1% significance. Reported *p*-values in final three columns derived from *F*-tests of hypotheses that cost-benefit ratios are equal between: (a) GD Lower and HD; (b) GD Lower and GD Large; and (c) GD Large and Combined treatments.

Table A.28: **ITT Effects on Transfers**.

		GiveDirectly					Control			*p*-values		
	HD	Lower	Middle	Upper	Large	Combined	Mean	Obs.	R^2	(a)	(b)	(c)
Panel A. Midline												
HH loans made	0.11	0.90**	0.45	1.41***	0.68*	1.24***	2.24	1705	0.15	0.10	0.18	0.27
	(0.30)	(0.44)	(0.44)	(0.45)	(0.42)	(0.43)						
	[0.31]	[0.05]	[0.19]	[0.00]	[0.09]	[0.01]						
HH gifts received	−0.33	1.75***	2.67***	2.62***	3.11***	2.46***	4.90	1704	0.17	0.00	0.60	0.33
	(0.36)	(0.57)	(0.55)	(0.59)	(0.53)	(0.50)						
	[0.19]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]						
HH gifts made	0.39	0.68*	1.87***	0.56	0.32	0.81*	3.41	1675	0.15	0.63	0.20	0.36
	(0.32)	(0.42)	(0.48)	(0.47)	(0.44)	(0.45)						
	[0.15]	[0.09]	[0.00]	[0.15]	[0.20]	[0.08]						
Panel B. Endline												
HH loans made	−0.11	−0.57	−0.79	−0.53	−1.00*	−0.36	3.08	1794	0.13	0.31	0.79	0.16
	(0.30)	(0.41)	(0.44)	(0.43)	(0.37)	(0.40)						
	[1.00]	[0.67]	[0.36]	[0.82]	[0.07]	[1.00]						
HH gifts received	−0.29	−0.92	−0.64	0.05	−1.36**	−0.25	3.34	1784	0.10	0.20	0.48	0.02
	(0.33)	(0.43)	(0.47)	(0.48)	(0.40)	(0.42)						
	[1.00]	[0.20]	[0.67]	[1.00]	[0.01]	[1.00]						
HH gifts made	0.17	0.15	0.05	0.26	−0.19	0.19	2.42	1770	0.11	0.91	0.54	0.40
	(0.28)	(0.40)	(0.41)	(0.41)	(0.38)	(0.37)						
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]						

Note: Regressions include but do not report the lagged dependent variable, fixed effects for randomization blocks, and a set of LASSO-selected baseline covariates, and are weighted to reflect intensive tracking. Standard errors are (in soft brackets) are clustered at the household level to reflect the design effect, and p-values corrected for False Discovery Rates across all the outcomes in the table are presented in hard brackets. Stars on coefficient estimates are derived from the FDR-corrected *p*-values, *=10%, **=5%, and ***=1% significance. Reported *p*-values in final three columns derived from *F*-tests of hypotheses that cost-benefit ratios are equal between: (a) GD Lower and HD; (b) GD Lower and GD Large; and (c) GD Large and Combined treatments.