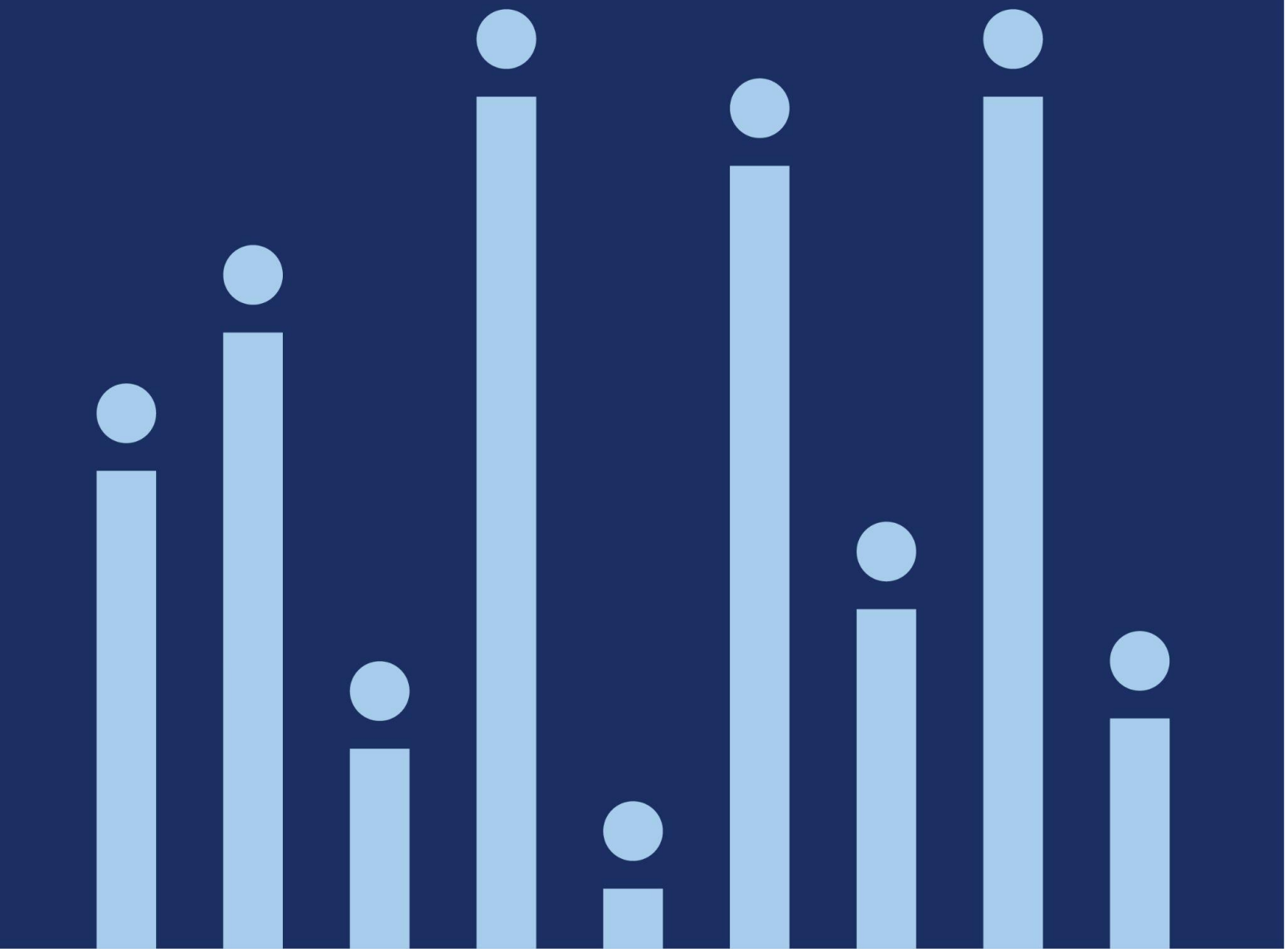

Village Enterprise Development Impact Bond Evaluation Findings

IDinsight

Global Development Incubator

USAID FROM THE AMERICAN PEOPLE

INSTIGLIO TYING FUNDING TO IMPACT

Foreign, Commonwealth & Development Office

30 November 2021

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About IDinsight

IDinsight uses data and evidence to help leaders combat poverty worldwide. Our collaborations deploy a large analytical toolkit to help clients design better policies, rigorously test what works, and use evidence to implement effectively at scale. We place special emphasis on using the right tool for the right question, and tailor our rigorous methods to the real-world constraints of decision-makers.

IDinsight works with governments, foundations, NGOs, multilaterals and businesses across Africa and Asia. We work in all major sectors including health, education, agriculture, governance, digital ID, financial access, and sanitation.

We have offices in Bengaluru, Dakar, Johannesburg, Lusaka, Manila, Nairobi, New Delhi, San Francisco, and Washington, DC. Visit **www.IDinsight.org** and follow on Twitter **@IDinsight** to learn more.

Abbreviations

BM – Business Mentor

BG - Business Group

BSG – Business Saving Group

DIB – Development Impact Bond

FCDO – the Foreign, Commonwealth & Development Office

GDI – Global Development Incubator

ITT – Intention to Treat

PPI – Poverty Probability Index

RCT – Randomized Controlled Trial

TOT – Treatment on Treated

USAID-DIV – United States Agency for International Development - Development Innovation Ventures

VE – Village Enterprise

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Finally, we thank all of the respondents who took part in the study, responding to surveys and sharing their opinions and thoughts which were valuable in generating evidence for this impact evaluation.

Executive Summary

This report describes the results of the evaluation of the Village Enterprise poverty graduation program, conducted in Kenya and Uganda between November 2017 and December 2020. The objective of this evaluation was to measure the causal impact of the program on household consumption and assets. The Village Enterprise program was randomized across 241 treatment villages and 241 control villages in western Kenya and eastern Uganda and rolled out to cohorts of treatment households from January 2018 to December 2020. We surveyed 9,888 households from all study villages from May to August 2021, which was 6 months to 2.5 years after treatment households had completed the one year program. All analyses were pre-specified and registered on the American Economic Association's registry for randomized controlled trials.[1] The findings from this evaluation determine the final payments of the Village Enterprise Development Impact Bond (DIB) from the Trustee of the Outcome Payers, Global Development Incubator, to the Service Provider, Village Enterprise, for the achieved results.

Key Findings

- **Consumption:** The Village Enterprise program had a positive and statistically significant impact on monthly consumption for households that were offered the program. On average, treatment households consumed 9.9 USD (or 6.3%) more per month than the control group. Effect sizes were larger in Kenya (15.2 USD or 7.3%) than in Uganda (3.9 USD or 3.6%).

- **Assets:** The Village Enterprise program also had a positive and statistically significant impact on household net assets after the end of the intervention. On average, households in the treatment group have USD 40.5 (or 5.8%) more in net assets than those in the control group. Effect sizes were larger in Kenya (60.9 USD or 8.5%) than in Uganda (15.6 USD or 2.3%).

- **Larger versus smaller grants:** In Kenya only, some households received larger grants (150 USD/household) while others received smaller grants (50 USD/household). Households that received larger cash grants did not have significantly different treatment effects for consumption than households that received smaller grants, though they did have significantly larger treatment effects for assets (87.7 USD for larger grants vs 5.9 USD for small grants).

- **Household wealth:** Treatment effects on consumption were generally larger for households that had more wealth prior to the introduction of the program. Treatment effects on assets are not strongly correlated with baseline wealth.

- **Other subgroups:** There were no clear patterns of differential program effects for other pre-specified subgroups (cohort of treatment household, gender of household head, household member with disability).

- **COVID-19 impacts on well-being**: 87% of respondents reported that the COVID-19 pandemic has had an effect on their economic well-being. Households that reported being negatively affected by COVID-19 achieved similar treatment effects as other households.

Based on the evaluation results, we find that the DIB Outcome Payment Type 2 (P_2) = $2,493,964.36, such that Village Enterprise maxes out the total DIB payment of $4,280,618.

[1] https://www.socialscienceregistry.org/trials/7168

1. Background

a. The Village Enterprise Development Impact Bond

Village Enterprise pursues a mission of ending extreme poverty in rural Africa through entrepreneurship and innovation. The United States Agency for International Development-Development Innovation Ventures (USAID-DIV); the Foreign, Commonwealth & Development Office (FCDO); Instiglio; Global Development Incubator (GDI); an anonymous funder; and Village Enterprise have designed a development impact bond (DIB) to enable Village Enterprise finance their graduation model intervention and focus on achieving measurable results. The DIB model harnesses private risk capital so that outcome payers, such as donor agencies or foundations, pay only for the achievement of agreed-upon outcomes.

Village Enterprise launched this first DIB in Sub-Saharan Africa in 2017, with a maximum outcome payment of $4,280,618. IDinsight was the independent, third-party evaluator of the DIB. We conducted a four-year randomized controlled trial (RCT) to measure the causal impact of the Village Enterprise program financed through the DIB on household consumption and assets. In this report, we report the results of the RCT and we use these results to determine the payments from the Trustee of the Outcome Payers to the Service Provider, Village Enterprise, for the achieved results.

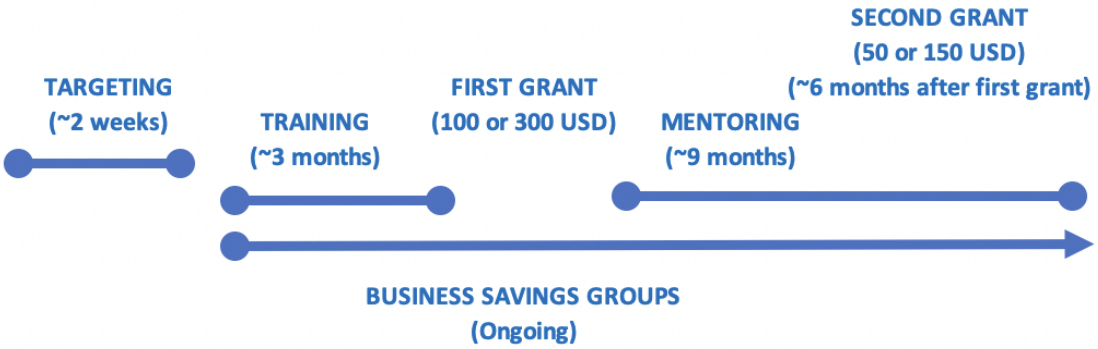
b. The Village Enterprise Program

Village Enterprise is a non-profit that administers a micro-enterprise development program in East Africa. The goal of the Village Enterprise graduation program is to lift the poorest East Africans out of extreme poverty. Village Enterprise's program runs similar to an ultra-poor graduation model, as it targets the poorest members of the communities they operate in and provides them with an integrated package of cash transfers and training. The program places a large emphasis on business skills and microenterprise development. Prior evidence suggests that this integrated package, compared to cash transfers alone, is critical to enabling ultra-poor households to generate enough income to break out of poverty (Banerjee et al., 2018).

The Village Enterprise program lasts for approximately one year, as illustrated below in *Figure 1*. Village Enterprise helps participating individuals form business groups (BGs) of two to three entrepreneurs and provides an initial cash transfer as seed capital. Program participants receive the first grant ($100 per BG, or ~$33 per household) after successfully completing the 3 months training and a second grant ($50 per BG, or ~$17 per household) is delivered 6 months after the first grant.[2] Some BGs in Kenya received a larger grant size ($300 for the first grant and $150 for the second) to assess the impact of the grant size on outcomes. These cash transfers are accompanied by training and ongoing mentoring by a local business mentor (BM). Entrepreneurs are also organized into business savings groups (BSGs) of 30 entrepreneurs to allow access to growth capital, provide a safe space for savings, and build social capital.

[2] If a BG only included two entrepreneurs, then it received a grant amount equal to ⅔ of the grant amount received by 3-member BGs in the same village, so that the amount received by each household is consistent across 2-member and 3-member BGs.

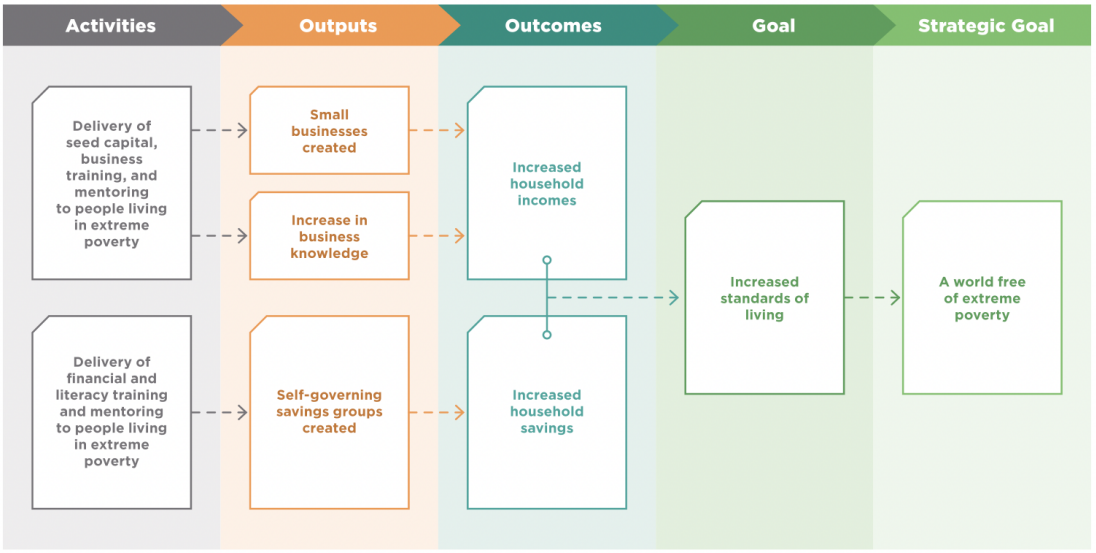
Figure 1. Village Enterprise Programme Timeline



Source: Village Enterprise

Figure 2 shows a simplified Theory of Change for how Village Enterprise envisions the program improving household well-being and standards of living.

Figure 2. Village Enterprise Theory of Change



Source: Village Enterprise

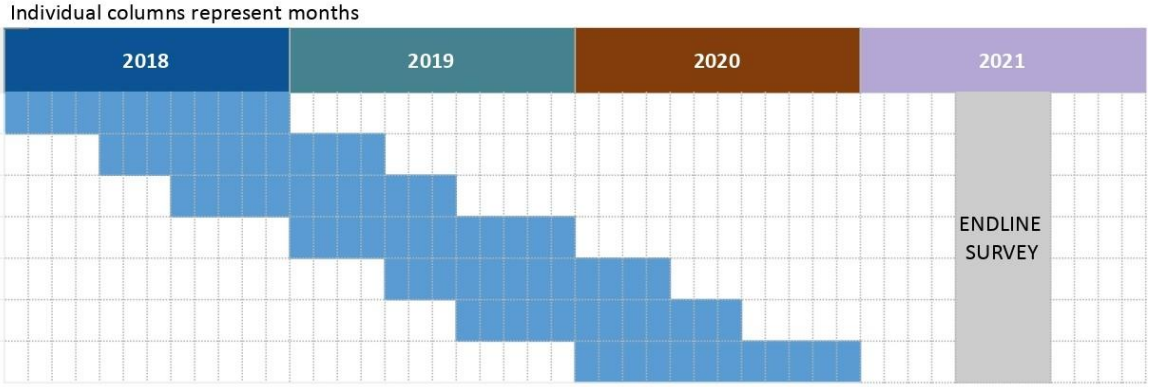
The program funded by the Village Enterprise DIB was implemented in 241 villages across western Kenya and eastern Uganda from 2017 to 2020. A total of 14,772 ultra-poor households were offered to participate in the program.[3] These households were divided into seven cohorts, which were offered the program at different times, as detailed in Figure 3. Out of those offered the program, 14,130 households, or 95.7% received at least the first cash grant, which is how we define 'participation' for the purposes of estimating the treatment-on-the-treated effect. A total of 13,839 households received both the first and second grants.

[3] This number includes the original sixty households per treatment village selected to participate in the program, as well as any alternate households who were offered the program due to initial households refusing to participate. See appendix J for the breakdown of original vs alternate households offered the program.

Figure 3: Village Enterprise Cohort Timelines[4]

Individual columns represent months

Cohort	# HH	Program Start	Program End
1	885	Jan 2018	Dec 2018
2	1,919	May 2018	Apr 2019
3	2,031	Aug 2019	Jul 2019
4	2,269	Jan 2019	Dec 2019
5	2,557	May 2019	Apr 2020
6	2,561	Aug 2019	Jul 2020
7	2,550	Jan 2020	Dec 2020



IDinsight independently verified the number and amount of grants disbursed to each cohort. Verification involved several processes, including IDinsight enumerators witnessing disbursements, financial audits of Village Enterprise's bank statements, cash retirement documents, M-Pesa[5] statements, and phone surveys with randomly sampled program participants. We successfully verified grant disbursement in all treatment villages. Some aspects of implementation of the program were slightly delayed for Cohort 6-Jul2020[6] and Cohort 7-Dec2020 due to COVID-19.[7]

The evaluation was originally intended to include two endlines: one for Cohort 1-Dec2018 through Cohort 4-Dec2019 in April/May 2020 and one for Cohort 5-Apr2020 through Cohort 7-Dec2020 in April/May 2021. This timeline would have ensured that data was collected within 1.5 years of the conclusion of the program for all cohorts. However, due to the COVID-19 pandemic, the two rounds of data collection were combined into a single endline conducted from May to August 2021. As a result, the outcomes for Cohort 1-Dec2018 through Cohort 4-Dec2019 were measured more than a year later than planned. The results for these early cohorts should therefore be interpreted more in light of the sustainability of the program's impact over time (within the context of a global health and economic shock), rather than reflecting the short-to-medium term impact of the program as initially intended.

[4] This represents households that were offered the program (ITT), which is more expansive than the population who actually received the first transfer. ITT analysis includes every household that was randomized, maintaining the balance generated from the original randomization assignment. This analysis is slightly more conservative compared to treatment-on-treated analysis (TOT), which is affected by noncompliance and withdrawal from the program after randomization. In this evaluation, noncompliance was low but non-zero (95.7% of households that were offered the program opted to participate and receive the first grant), and thus ITT impact estimates are slightly lower than TOT impact estimates.

[5] M-Pesa is a mobile phone-based money transfer service that allows users to store and transfer money through their mobile phones

[6] Cohorts in this report are referenced as [cohort number]-[MonthYear when the cohort finished the program].

[7] The second cash disbursement for Cohort 6-Jul2020 in Uganda and the first cash disbursement for Cohort 7-Dec2020 in Kenya were delayed for about a month due to the implementation of new procedures during COVID-19. Business mentoring was also delayed and had to be extended until the end of December 2020 for Cohort 5-Apr2020 through Cohort 7-Dec2020 to ensure they receive in-person mentoring after the lockdown was lifted.

2. Study Design

a. Design Overview

The main objective of this evaluation was to determine the impact of Village Enterprise's program on household consumption and assets. The results from this evaluation were used to determine the appropriate outcome payments for the DIB. A secondary goal for this study was to contribute to the body of knowledge on graduation-style interventions and to serve as an example for similar interventions in the future.

This study aimed at answering the following research questions:

- **Primary question:** What was the impact of the Village Enterprise program on household consumption and asset value, relative to control households? (Intention-to-treat effect)

- **Secondary questions:**
    - What was the impact on household consumption and assets for those households that received at least the first business grant? (Treatment-on-the-treated effect)
    - Were there differences in the impact on household consumption and asset value, based on: cash transfer size; cohort; country; gender of the household head; the presence of a household member with a disability; baseline wealth levels?
    - How does endline consumption and asset vary by: business type; business group size; business health;[8] participant gender; participant disability?

These secondary questions gave additional important insights on the impact of the Village Enterprise program along these different dimensions.

To assess the impact of the program, we designed and conducted a cluster randomized controlled trial (RCT) in Trans-Nzoia and Bungoma counties of Kenya, and Soroti, Amuria and Dokolo districts of Uganda. For all major research questions outlined, the village is the unit of treatment and the household is the unit of analysis.

Sampling and randomization proceeded as follows: first, Village Enterprise produced a list of eligible villages in study areas, which included information on the number of households, trading centers, cattle dips, and water sources used. IDinsight analyzed the village data to produce a final list of villages that met the minimum requirements for inclusion in the study: had at least 70 households, and either did not share a trading center or water source (Kenya) or did not share a water source or cattle dip (Uganda).[9] Village Enterprise then allocated study villages to cohorts and, within cohorts, to Business Mentors (BM). To identify eligible households within selected study villages, Village Enterprise conducted a targeting exercise, which consisted of a participatory wealth ranking exercise conducted with local leaders. All households ranked as 'poor' or 'very poor' in this exercise were administered a Poverty

[8] Business health variables include business value, profit, record keeping, number of active members, and the proportion of grant funds invested in the business.
[9] We ensured that study villages did not share a trading center, water source, or cattle dip to limit spillover effects. Selection of eligible study villages was conducted twice: first for Cohort 1-Dec2018 through Cohort 4-Dec2019, and later for Cohort 5-Apr2020 through Cohort 7-Dec2020.

Probability Index (PPI)[10] survey by Village Enterprise enumerators.[11] Villages that did not have 70 qualified households were determined ineligible for the study.

Next, within each pair of villages under a BM, IDinsight randomly assigned one village to the treatment group and the other village to the control group.[12] In addition to improving statistical power, stratification at the BM-level ensured that each BM had the same number of treated villages on the same programmatic timeline and that BMs did not need to be reallocated after the targeting process and randomization. Households in the control villages received no treatment.

Within treatment villages, IDinsight randomly selected 60 of the 70 poorest households to be invited to the program. The remaining ten households were designated as alternates in case any of the original 60 households declined to participate. Offers to alternates were made in the order of IDinsight's randomized ranking.[13] The original 60 households and any alternates invited to the program were included in the study sample frame. In control villages, all 70 of the poorest eligible households were included in the study sample frame. In total, our final sampling frame consisted of 31,642 households. Of these, we randomly selected 21 households per village to participate in our endline survey, for a total of 10,122 households. We successfully collected data from 4,935 households in control villages and 4,953 households in treatment villages, for a total of 9,888 households.[14] Table 1 below provides descriptive statistics for baseline variables across treatment groups for the households for which data was collected.

Table 1: Balance checks on baseline characteristics (N=9,888)

Variable[15]	Treatment	Control	Difference	P-value
PPI score	25.600	25.348	0.253	0.237
Household size	6.492	6.505	-0.013	0.718
Household head age	48.955	49.585	-0.630	0.004
Number of cows	0.828	0.882	-0.053	0.083
Number of bodas[16]	0.045	0.035	0.009	0.002
Owns a radio	0.327	0.308	0.019	0.007
All have shoes	0.096	0.085	0.011	0.033
Has iron roof	0.559	0.555	0.004	0.653
Number of beds	0.885	0.891	-0.006	0.720
Number of pigs	0.176	0.190	-0.014	0.188
Number of bikes	0.377	0.386	-0.009	0.338

[10] www.povertyindex.org
[11] VE enumerators could also administer the PPI survey to additional households, at their discretion.
[12] Village Enterprise's PWR exercise and IDinsight's subsequent randomization were conducted separately for each cohort.
[13] Alternate households were offered the program in 80 of the 241 treatment villages. In five villages all ten alternates were offered the program.
[14] During data collection, 16% of sampled households were unavailable for the endline survey despite field team efforts to track them. Out of this number, we replaced 14% with randomly-selected alternates from the list of eligible households. We describe our protocol for replacement sampling and implications for estimation in Appendix D.
[15] These variables represent household data collected during baseline
[16] This is the number of motorcycles a household reported to have during baseline

Has frying pan	0.006	0.006	0.000	0.767
Number of jerry cans	3.118	3.169	-0.050	0.160
Has phone	0.681	0.674	0.007	0.354
Number of school years of household head[17]	6.358	6.232	0.126	0.048
Missing school years of household head[18]	0.064	0.067	-0.003	0.371

Although we observe imbalance in some baseline variables, the differences are small in magnitude and not in a consistent direction.[19] Therefore, we find no material concerns with the randomization process. In keeping with our pre-analysis plan, we include these variables as controls in our analytical model.[20]

b. Main Outcomes

This evaluation estimated the effects of treatment on two primary outcomes of well-being: household consumption and assets.

Household monthly consumption, measured at endline included the sum of[21]:

1. Total food, beverage, and temptation goods expenditure over the preceding seven days including food prepared at home, purchased outside, and given in-kind.
2. Total recurrent expenditure, such as fuel and transportation expenditures, utilities, personal hygiene and health over the preceding four weeks.
3. Total infrequent expenditure on larger social and religious activities (e.g., wedding, funeral), clothing, taxes, housing maintenance, migrations, travel and educational costs and fees over the last 12 months.

Household net asset ownership, measured at endline included the sum of:

1. Total durable assets
2. Total home improvements
3. Total productive assets
4. Total household savings, including savings generated through the Village Enterprise business savings groups[22]
5. Total business assets, accounting for business ownership share by the household

[17] Number of school years of the household head is missing for 650 observations, and so the values in this row come from 9,238 non-missing observations.
[18] This variables represents missing number of years of school for household head at baseline
[19] For instance, treatment households are slightly more likely to have a boda, radio, and shoes, while control households are slightly more likely to have cows, pigs, and jerry cans. We also note that differences with similar magnitudes and statistical significance levels were found in many of the same variables among all eligible households at baseline (bodas, radios, shoes, jerry cans, and years of schooling were significant at the 5% level for eligible households), so these slight imbalances are an artifact of the randomization process rather than the household sampling process.
[20] With the exception of PPI score, since its component parts were included in the lasso covariate selection model.
[21] Consumption measured over different recall periods were converted to monthly amounts. Note, the sum of these components was approximated through a subset approach. See Appendix E for details.
[22] Survey instrument aimed to make sure that only the household's share of business assets and savings were captured rather than the full business group assets in order to provide an accurate measure of asset ownership.

6. Total household and share of business liabilities (subtracted from 1 – 5 above to arrive at total net ownership).

Owing to the ongoing COVID-19 pandemic during the data collection period, the length of the survey was reduced from 2.5 hours to 1 hour to prevent prolonged interactions and reduce exposure between enumerators and respondents. This involved eliminating items from several sections of the survey. For the consumption modules, we identified the most commonly purchased goods from recent representative consumption and expenditure surveys in each country that accounted for 85% of total consumption.[23] This process resulted in cutting 222 items down to 41 in Kenya and 144 items down to 31 in Uganda. During analysis, we multiplied the consumption value in each country by approximately (1/0.85) to estimate total consumption.[24] Prior studies have shown that shortening consumption modules in this way and rescaling aggregate values produces accurate measurements of total consumption.[25] Therefore, we believe that the estimated effect sizes are reasonable given previous work.

For the household asset module, we cross-checked our initial list of 33 assets with prior studies[26] and identified the 15 most commonly measured assets, which accounted for 95% of asset ownership in the previous RCT of Village Enterprise's program in Uganda (Sedlmayr et al. 2020).[27] For the business asset module, we identified the 10 most common categories of assets used by businesses in Village Enterprise's business asset database, accounting for 95% of business asset ownership.[28] We further shortened the survey by eliminating the full household roster and the disability module.[29]

c. Data Collection

To measure the causal impact of the Village Enterprise program on household consumption and assets, IDinsight conducted one round of data collection across all 482 study villages in Kenya and Uganda between May and August 2021.[30] Depending on each treatment village's cohort, data collection occurred 6 months to 2.5 years after the conclusion of the Village Enterprise program. Data was collected through enumerator-administered in-person surveys on tablets using SurveyCTO. Despite government restrictions and various challenges presented by COVID-19 during the course of data collection, we surveyed 98% of our target sample.[31]

[23] In Kenya we used the Kenya Integrated Household Budget 2015-16 dataset (https://sun-connect-news.org/fileadmin/DATEIEN/Dateien/New/KNBS_-_Basic_Report.pdf) and in Uganda we used the Uganda National Budget Survey 2015-16 (https://microdata.worldbank.org/index.php/catalog/3460).

[24] In Kenya we estimated that the consumption module would include 85.06% of total consumption, so we multiplied consumption values by 1/0.8506 = 1.176. In Uganda we estimated that the consumption module would include 85.18%. of total consumption, so we multiplied consumption values by 1/0.8518 = 1.174.

[25] See for instance Beegle et al (2012) and Natali & de Neubourg (2014).

[26] Including Banerjee et al (2015), Dupas et al (2018), Haushofer & Shapiro (2016), and Sedlmayr et al (2020).

[27] Since we also included an 'other' asset category to capture any large household assets not included in this list, we did not rescale asset measurements. Note that household savings and loans were captured separately, and no savings or loans survey items were cut from the final survey.

[28] ibid. Note that we administered the business asset module separately for each household business.

[29] In the subgroup analysis below, we use data collected by Village Enterprise during the household targeting exercise to estimate treatment effects by whether a household included a member with a disability at baseline.

[30] Data collection occurred in Kenya from June 2 to July 29, 2021. Data collection started in Uganda on May 6, 2021 and continued until government-imposed lockdowns went into effect on June 18. Data collection resumed in Uganda on August 9, 2021, and continued until all villages were completed on August 31. A first endline was initially planned for April and May 2020 but this was canceled due to the COVID-19 pandemic.

[31] See details on our replacement sampling protocol in Appendix D.

To ensure data quality, IDinsight conducted audio audits, back-checks, supervisor spot-checks, and daily checks for outliers on key variables, incomplete surveys, and other anomalies. Surveys that were flagged for possible anomalies were conducted again.

d. Analytical Model

The causal effect of the Village Enterprise program on assets and consumption was estimated using the following Ordinary Least Squares model:

$$Y_{ij} = \beta_0 + \beta_1 T_j + X_{ij}^{'}\gamma + \alpha_m^{'}\delta + \varepsilon_{ij}$$

Where:

- $\mathbf{Y_{ij}}$ denotes the outcome variable (either monthly consumption or net assets) for household *i* in village *j.*
- $\mathbf{T_j}$ denotes the treatment status of village *j* (1 for Treatment Group; 0 for Control Group).
- $\boldsymbol{\beta_1}$ is the estimated treatment effect of the Village Enterprise program that is used to calculate DIB payments.
- $\mathbf{X_{ij}}$ is a vector of controls for household *i* in village *j* as measured during the baseline targeting exercise.[32]
- $\boldsymbol{\alpha'_m}$ denotes a vector of dummy variables corresponding to the Business Mentors-village pairs in which we randomized, which is 1 when household *i* is allocated to Business Mentor-village pair *m* and 0 otherwise.
- $\boldsymbol{\varepsilon_{ij}}$ denotes the household error term *i*, clustered at the village-level *j* to reflect the randomization process.

We pooled data for all seven cohorts. The impact of the program was measured using an intention-to-treat (ITT) analysis in which we assess the impact of the program on all households that were *offered* the Village Enterprise program. We also estimated the treatment effect on the treated (ToT), which is the impact for households that actually took up the program, by using treatment assignment as the instrumental variable for receiving the treatment.[33]

We also conducted analyses for key subgroups including: whether households received regular grant sizes ($50/household) or increased grant sizes ($150/household); country; cohort; gender of the head of the household; household baseline wealth level (as proxied by PPI scores); and whether a household member has a disability (as measured during Village Enterprise's household targeting). These analyses sought to address the secondary questions outlined above with the aim of understanding the effects of the program in these dimensions.

[32] The control variables were pre-specified in the PAP and include: # of people living in HH, age of HH head, # of cows/cattle the HH owns, # of bodas/motorcycles the HH owns, whether the HH owns a radio, whether every HH member has a pair of shoes, whether the HH has an iron roof, # of beds the HH owns, # of pigs the HH owns, # of bicycles the HH owns, whether the HH owns a frying pan, # of jerry cans the HH owns, whether the HH owns a mobile phone, and # of years of school for the HH head. Note that the variables for radios, frying pans, and mobile phones were initially intended to capture the number of each item; however, given the data collected by Village Enterprise during baseline targeting, these were updated to whether the HH owned at least one of each item. We also pre-specified that if any households were missing covariate information, we would replace the missing observation values with zero and add a missing indicator for that covariate. In the final dataset the only covariate with missing data was the number of years of school for the HH head, which was missing for 7% of observations. Accordingly, we created a dummy variable for missing values in this variable, and replaced missing values in the original variable to zero.

[33] Treatment was defined as households that at least received the first small business grant.

During the course of data collection, Kenya was experiencing the third wave of COVID-19 while Uganda was in the second wave. To better understand the impacts of COVID in our study population, IDinsight included questions in our survey about whether and how the pandemic had an effect on households' economic well-being.[34]

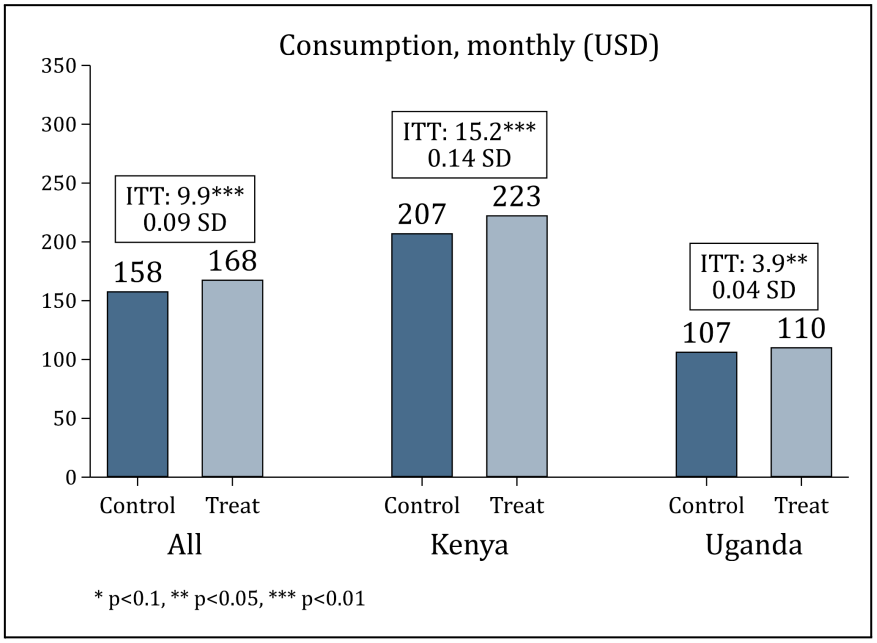
[34] We asked respondents whether their household's livelihood had been in any way impacted by the ongoing COVID-19 pandemic; and if so, if the impact was mostly positive or mostly negative; and finally requested them to briefly explain their responses (the full report with the findings from this analysis is on Appendix C).

3. Results

a. Main Results

The Village Enterprise program had a positive and statistically significant impact on household monthly consumption. Figure 4 below shows how consumption compares between treatment and control villages in both countries, as well as for each country where the program was rolled out.[35]

Figure 4: Effects of Village Enterprise program on consumption

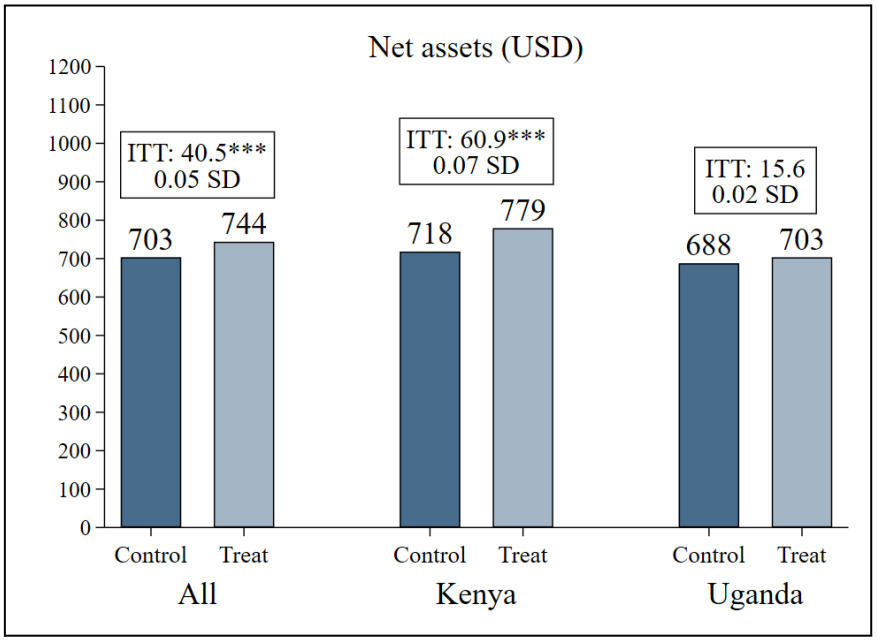


On average, those households that were offered the program consumed 9.9 USD (or 6.3%) more per month than those in the control group across both countries. The program effect in Kenya (15.2 USD or 7.3%) was larger than the effect in Uganda (3.9 USD or 3.6%).

The Village Enterprise program also had a positive and statistically significant impact on household net assets. Figure 5 shows the effect size on net assets value for those households that were offered the program.

[35] In all figures that show control & treatment values, the control value is the average value of that outcome in the control group, whereas the treatment value is the control average plus the treatment effect estimate from the regression model described in the Analytical Model section.

Figure 5: Effects of Village Enterprise program on net assets



On average, households in the treatment group had USD 40.5 (or 5.8%) more in net assets than those in the control group across both countries. Similar to what we observed for consumption, the program effect for households in Kenya (60.9 USD or 8.5%) was larger than the effect for households in Uganda (15.6 USD or 2.3%).

In Appendix F we report treatment effects for individual components of consumption and net assets, while correcting for multiple hypothesis tests by following the procedure described in Benjamini et al (2006) and implemented in Anderson (2008).

Since 95.7% of households that were offered to participate in the program accepted the invitation and received at least one grant, the treatment-on-the-treated (ToT) results closely mirror the ITT estimates. The ToT estimate for consumption is 10.3 USD ($p < 0.01$) and for assets is 42.2 USD ($p < 0.01$) across both countries.

b. Subgroup Results

Cash transfer size

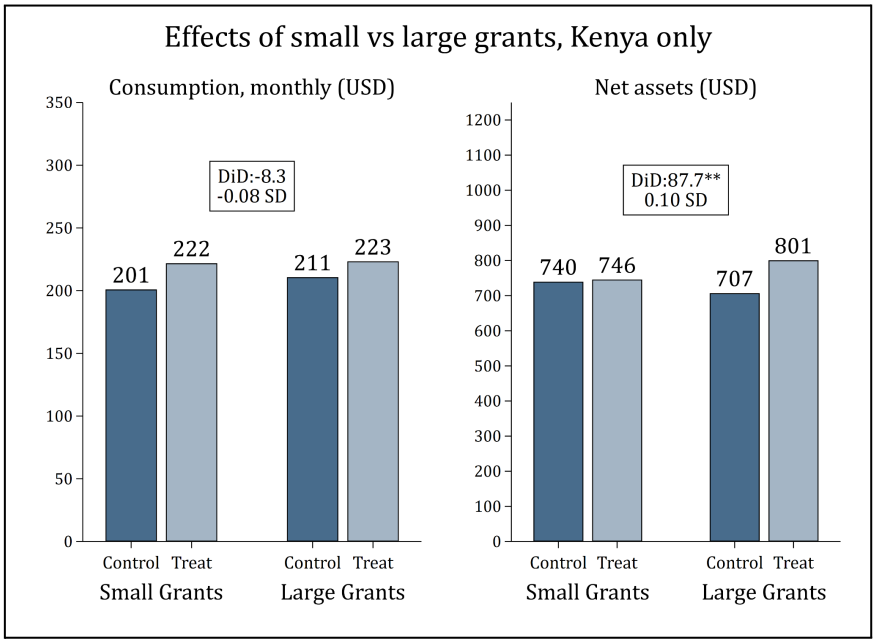
The Village Enterprise program implemented two different cash transfer sizes in Kenya.[36] Some of the BGs in Kenya received $100 during the first disbursement while others received $300.[37] For the second grant, the BGs that received a smaller grant in the first round received $50, while the BGs that received

[36] In Uganda, all BGs received the same cash transfer sizes, unless the BG had only two members instead of three, in which case it received ⅔ of the grant amount disbursed to 3-member BGs.

[37] Treatment villages were non-randomly selected by Village Enterprise to receive small or large grants. Thus any difference in outcomes or treatment effects between villages that received small or large grants is suggestive but should not be interpreted as causal evidence of the impact of larger grants. Differences between control villages in village pairs where the treatment village received large grants versus village pairs where the treatment village received small grants reflect differences in the underlying characteristics of those types of villages. All BGs within a village received the same cash transfer sizes, unless a BG had only two members instead of three, in which case it received ⅔ of the grant amount disbursed to 3-member BGs in the same village.

the larger grant in the first round received $150. Figure 6 below shows differences in the impact based on the cash transfer size.

Figure 6: Grant-size effects[38]



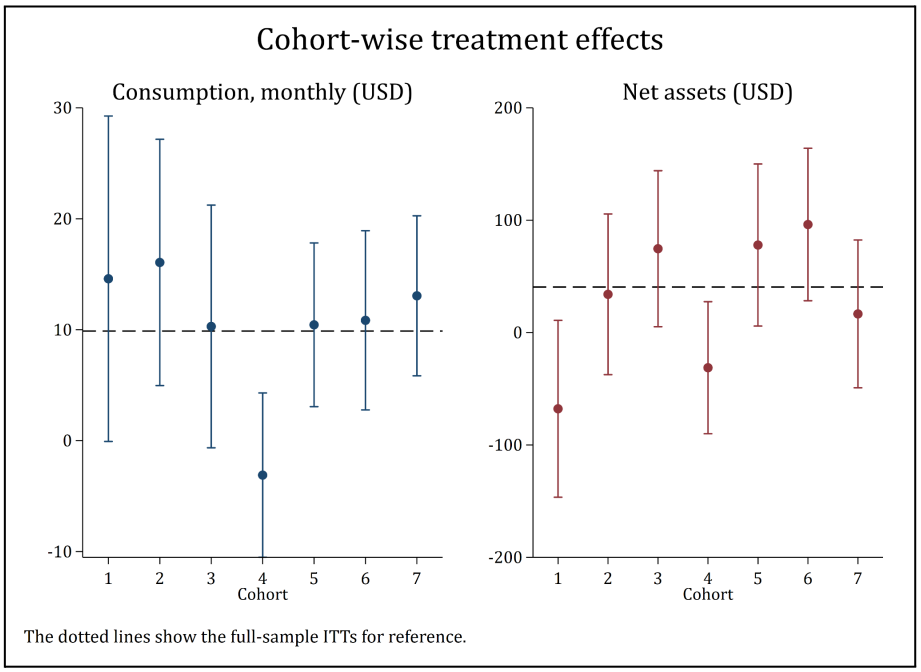
There was no statistically significant difference in treatment effects on consumption for households that received larger vs smaller grants. However, households that received larger grants had on average 87.7 USD more in net assets than households that received smaller grants (p = 0.02). Appendix G provides details on how the treatment effects for specific asset components differ for households that received large versus small grants, and which components are driving the difference in the aggregate index.

Cohort of household

As shown in Figure 3, cohorts received cash grants and participated in the Village Enterprise program at different points in time. As of endline in May-August 2021, treatment households in Cohort 1-Dec2018 had completed the Village Enterprise program 2.5 years prior, whereas treatment households in Cohort 7-Dec2020 had completed the Village Enterprise program only 6 months prior. If the impact of the program grows or diminishes over time, we may find different treatment effects for different cohorts. If the COVID-19 pandemic affected the impact of the Village Enterprise program, we may find different treatment effects for cohorts that completed the program prior to pandemic, by December 2019, (Cohort 1-Dec2018 through Cohort 4-Dec2019) compared to those that completed the program during the pandemic in 2020 (Cohort 5-Apr2020 through Cohort 7-Dec2020). Figure 7 shows treatment effects (with 95% confidence intervals) for each cohort.

[38] The difference-in-differences or "DiD" effect reported in these graphs refers to the difference in treatment effects between the group on the left and the group on the right. A significant DiD effect indicates that treatment effects are different across subgroups.

Figure 7: Cohort effects

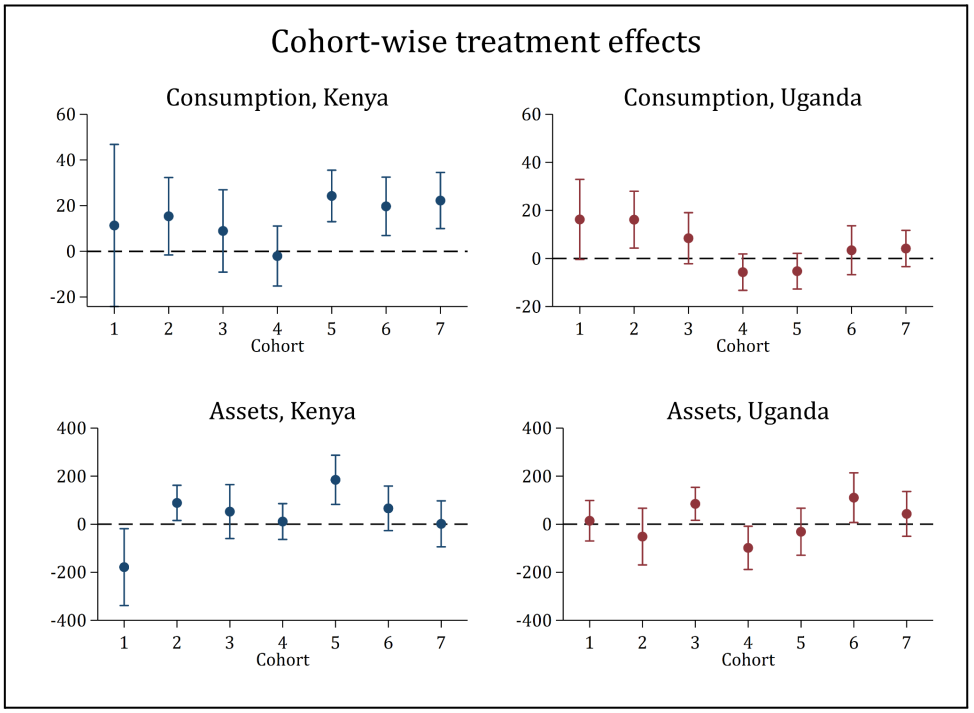


Surprisingly, given the differences in program timing across cohorts, there are no significant differences in treatment effects on household consumption across cohorts, except for Cohort 4-Dec2019. The treatment coefficient on consumption for Cohort 4-Dec2019 is significantly smaller than the coefficient for the other six cohorts ($p < 0.01$), and is statistically indistinguishable from zero. This may be driven in part by timing: Cohort 4-Dec2019 completed the program shortly before the COVID-19 pandemic and ensuing lockdowns, which may have given inadequate time for new businesses to become established.

There is slightly more variation in treatment effects on net assets across cohorts. Effects for Cohort 2-Apr2019, Cohort 3-July2019, Cohort 5-Apr2020, and Cohort 7-Dec2020 are statistically indistinguishable from each other or from the average effect across all cohorts. The treatment effect for Cohort 6-Jul2020 is slightly greater than the cross-cohort average, though the difference is marginally significant ($p = 0.07$). The treatment effect on net assets for Cohort 4-Dec2019 is significantly smaller than the coefficient for the other six cohorts ($p = 0.01$) and is statistically indistinguishable from zero. The treatment effect on net assets for Cohort 1-Dec2018 is also significantly smaller than the coefficient for the other six cohorts ($p < 0.01$). Surprisingly, the coefficient for Cohort 1-Dec2018 is negative and marginally significant ($p = 0.07$).

In Figure 8 we show cohort-wise results disaggregated by countries.

Figure 8: Cohort effects by country



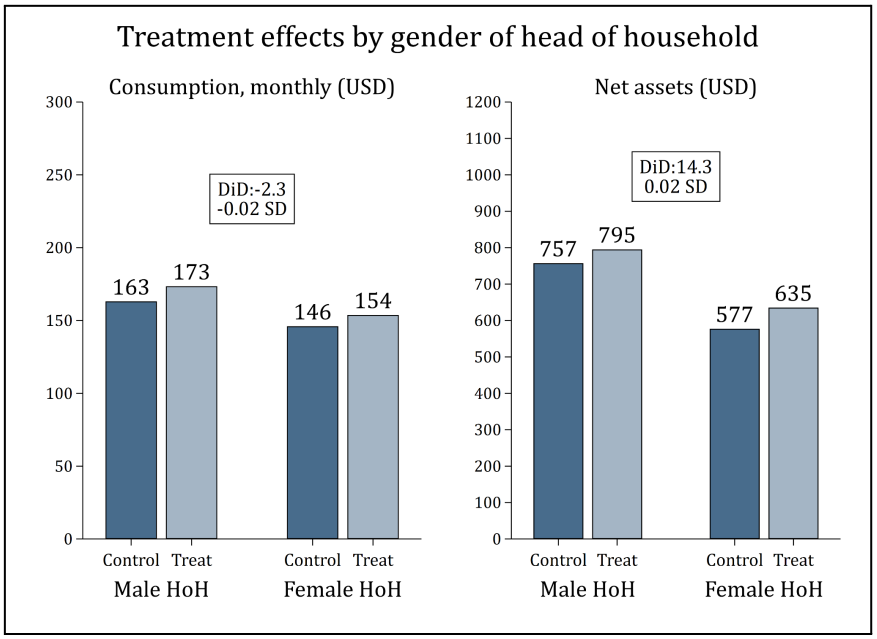
An interesting pattern emerges in consumption effects: whereas treatment effects are greater for the last three cohorts than the first three cohorts in Kenya, the reverse is true in Uganda. We posited that the pattern of consumption treatment effects in Uganda could be driven by interactions between the data collection schedule and the timing of the lockdown, where households in later cohorts tended to be surveyed more after the lockdown was lifted than before the lockdown was put into place. However, the evidence to support this hypothesis is mixed. In Appendix K we show cohort-wise effects in Uganda for households surveyed pre-lockdown vs post-lockdown. While we do see that households surveyed post-lockdown in Cohort 6-Jul2020 and Cohort 7-Dec2020 had lower effect sizes than households in the same cohorts surveyed pre-lockdown, which partially explains the lower effect sizes for those cohorts in Uganda, we do not see a consistent pattern in effect sizes across cohorts (e.g. effect sizes are actually larger for household surveyed post-lockdown in Cohort 1-Dec2018, Cohort 4-Dec2019, and Cohort 5-Apr2020).

In both countries, the consumption effect is smallest for Cohort 4-Dec2019 and statistically indistinguishable from zero. There are less clear patterns in asset effects across cohorts, though we see that the negative coefficient on assets for Cohort 1-Dec2018 is driven by Kenya.

Gender of the head of household

Figure 9 below shows differences in program impact based on the gender of the household head as reported by respondents during the survey. Within the study sample, 29.5% of households have a female household head.

Figure 9: Treatment effects by gender

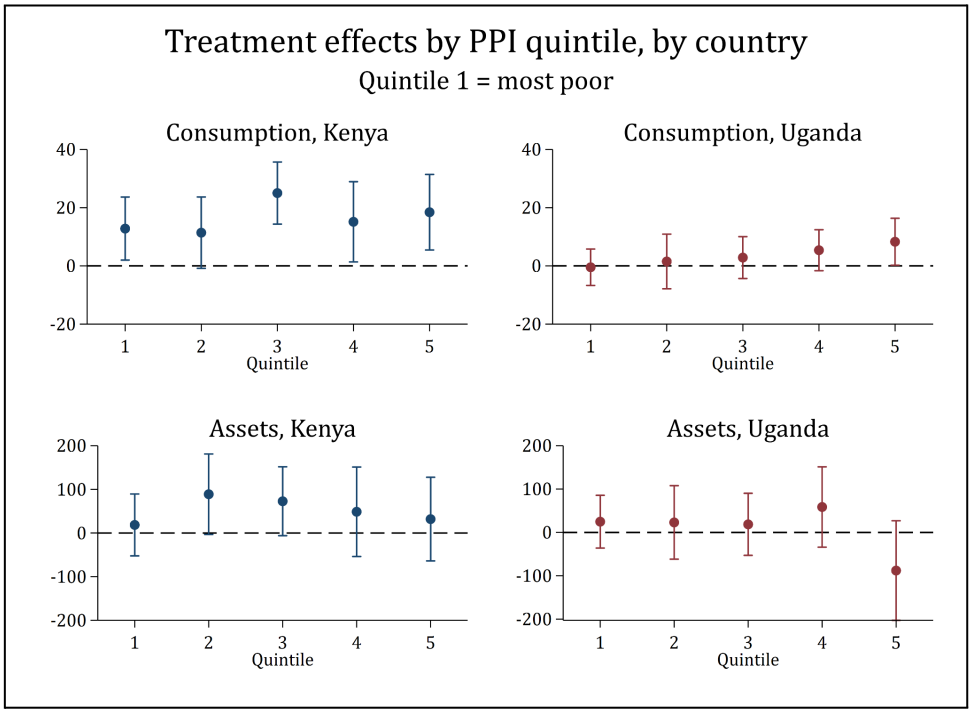


We find no statistically significant differences in treatment effects for households headed by men versus women. However, female-headed households in both treatment and control groups have lower consumption and fewer assets.

HH baseline wealth level (as proxied by PPI scores)

We analyzed the effect of the program on households for each wealth quintile, proxied by PPI scores determined by Village Enterprise during the targeting exercise. We divided households into wealth quintiles, separately for each country, and estimated treatment effects for each country-quintile. Figure 10 below illustrates the effects on consumption and net assets. We perform this analysis by country since wealth levels differ substantially between the Kenya sample (wealthier) and the Uganda sample, and since PPI scores and national poverty lines are country-specific.

Figure 10: Treatment effects by baseline wealth level, by country



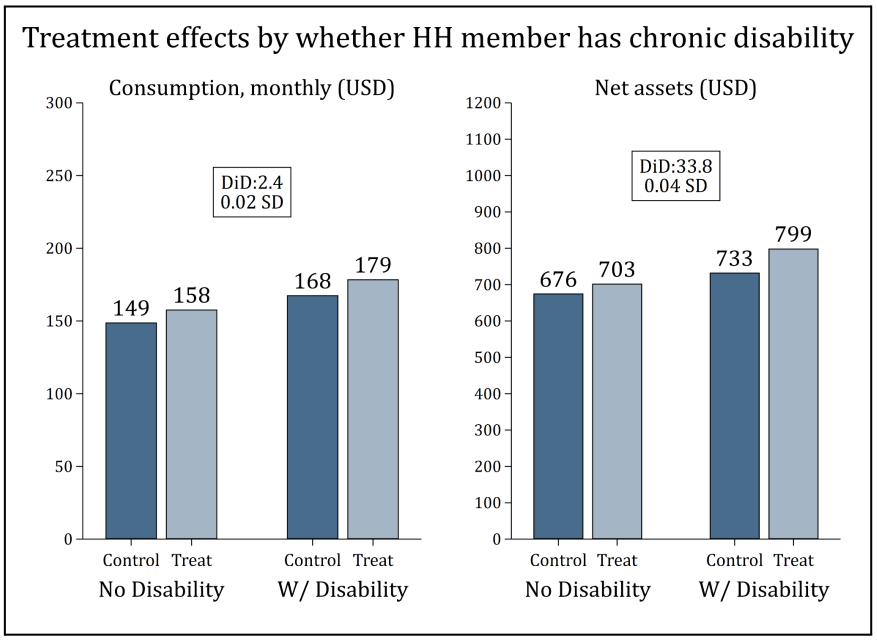
Treatment effects on consumption are generally greater for households that had more wealth at baseline. The one exception is that households in the middle quintile in Kenya had the highest treatment effect for any quintile in either country. There is less of a clear pattern for assets. We observe the largest treatment effects on assets for the 2nd and 3rd quintiles in Kenya and the 4th quintile in Uganda. Effects are smallest for the 1st quintile in Kenya and the 5th quintile in Uganda.

Presence of a household member with a disability[39]

Figure 11 below shows the differences in impact between households that reported having a member with a disability compared to those that did not have any reported cases of disability. Within the study sample, 47.4% of households report having at least one member with a disability.

[39] Village Enterprise administered the Washington Group Short Set of Questions to households at baseline, which defines a disability as having a lot of difficulty with the following abilities: eyesight, hearing, language, memory, mobility, and self-care.

Figure 11: Treatment effects by disability



We find no statistically significant differences in treatment effects for households that had a member with a disability versus those that did not.

c. Descriptive Analysis by Business Type

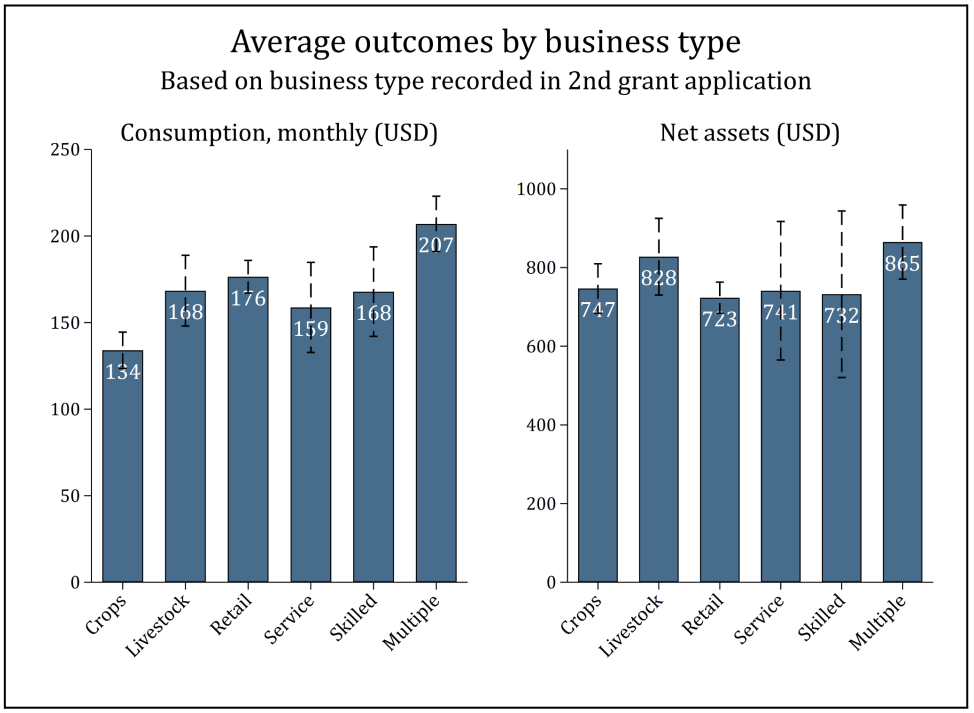
In this section we report descriptive statistics for treatment households belonging to different types of businesses (control households and villages are excluded from the analysis in this section). Range bars in all graphs in this section denote 95% confidence intervals.

Outcomes by business type

In the first and second grant application, households reported the type of business in which they were involved. Figure 12 shows average outcomes for treatment households according to the business type that they reported in their *second* grant application.[40] In the second grant application, 60% of households reported running a retail business, 21% crops, 8% livestock, 3% service, 2% skilled labor, and 5% reported running a business that fell into multiple categories.[41]

[40] We report business type according to the second grant application since we believe that this would correspond more closely to the business type at endline than what BGs reported in the first grant application.
[41] 2% of BGs did not have a business type listed in the second grant application. The percentages above correspond to the 98% of BGs who reported a business type.

Figure 12: Outcome means by business type



Average consumption is similar across business types, though households with a crop business consumed less, and households with a business that falls into multiple categories consumed more. Households in the livestock business and with a business across multiple categories reported higher average net assets.

In Appendix I we show average outcomes by business type disaggregated by country and, within Kenya, by cash transfer size.

Outcomes by business group size

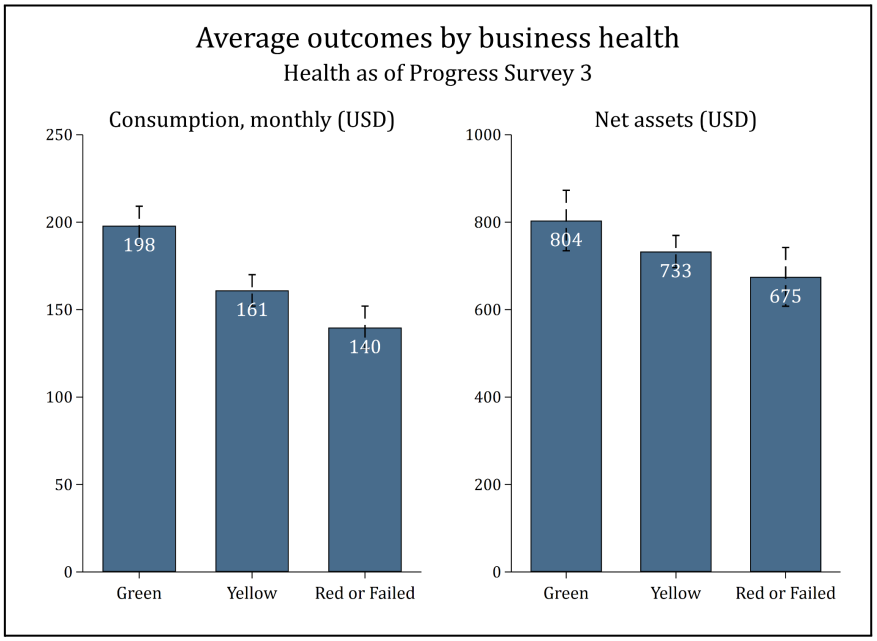
Since only 2% of BGs in treatment villages had 2 members, the confidence intervals on outcomes for two-member BGs are very large and not informative, and so we omit this analysis from the report.

Outcomes based on business health

During progress visits, Village Enterprise tracked key performance indicators of business health, including the proportion of grant funds invested by the business, the total business value, the total business profit, the number of members dropped from the BG, and whether the business had up-to-date records. Each indicator was rated on a "Red-Yellow-Green" scale, and then the overall business was rated as the worst of its indicators.[42] Figure 13 shows average outcomes for treatment households by the business's color rating in the most recent progress survey. In this survey, 23% of businesses were rated as Green, 65% Yellow, 10% Red, and 2% of businesses had failed and were not given a color rating.

[42] For instance, if a business had a Red rating in one indicator and a Green rating in the other four indicators, the business would have had an overall rating of Red.

Figure 13: Outcome means by business health



Households with more successful businesses had higher consumption levels and higher net asset levels. To explore which business health indicators are driving this pattern, we estimate correlation coefficients between outcomes and business indicators (the same five indicators that are used by Village Enterprise to assign the business health color) and report the results in Table 2.

Table 2: Correlation coefficients between business health indicators and outcomes

Indicator	Consumption	Net Assets
Proportion of PR grant funds invested in business	0.05***	0.01
Total business value	0.35***	0.08***
Total business profit	0.28***	0.03**
Any members dropped from group	-0.01	-0.02
Up do date business records	0.03**	0.01

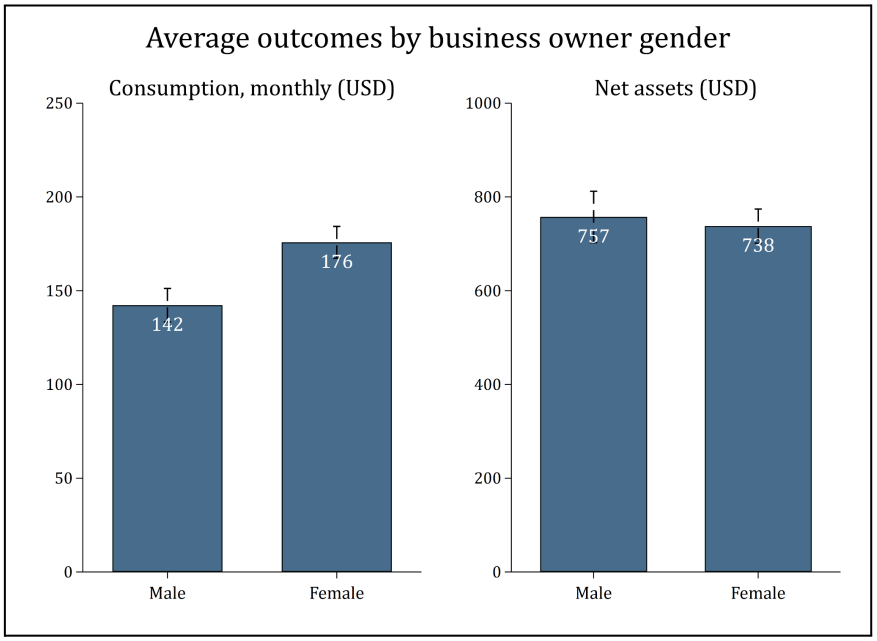
* p < 0.10, ** p < 0.05, *** p < 0.01

Business value is most strongly correlated with outcomes, followed by business profits.

Business owner gender

Three out of four business owners were women. Figure 14 shows average outcomes by whether the business owner in the household was a man or a woman.

Figure 14: Outcome means by business owner gender

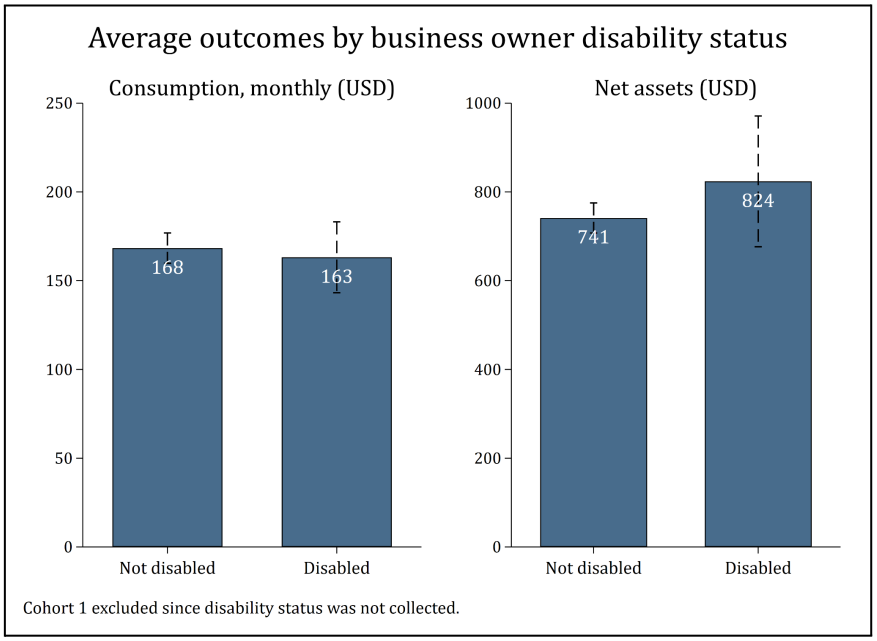


Households with female business owners had higher average consumption, though net assets were similar for households with male versus female business owners.

Participant disability

Starting with Cohort 2-Apr2019, Village Enterprise administered the Washington Group Short Set of Questions on Disability to all business owners. This survey included six questions about whether the respondent had difficulty with various activities, including seeing, hearing, mobility, concentration, self-care, and communication, and if so, how much difficulty the respondent faced in attempting each activity. A respondent was considered as having a disability if they responded that they had 'a lot of difficulty' with an activity or 'cannot do it at all'. Figure 15 shows average outcomes for business owners by disability status. Four percent of business owners reported a disability.

Figure 15: Outcome means by business owner disability status



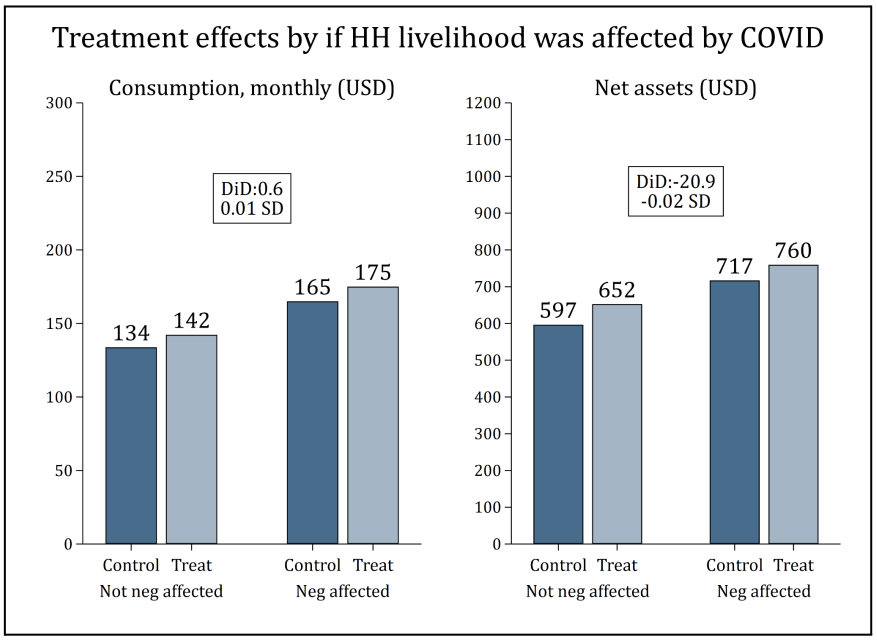
Consumption levels were similar for businesses with an owner who reported a disability versus others. Average net asset levels were higher for businesses with an owner who reported a disability, however, due to the relatively small number of business owners with a disability in our sample, estimates are imprecise and the difference in net assets between business owners with and without a disability is not statistically significant.

d. COVID and Livelihoods

Endline data was collected during the period when Kenya was experiencing the third wave of COVID-19 (July-September 2021) while Uganda was in the second wave (May-July 2021). We included questions at the end of the survey asking respondents how the ongoing COVID-19 pandemic affected their economic well-being. The aim was to find out how the pandemic had impacted the livelihoods of households in the study sample. Out of the 9,888 households that we surveyed, 87% of respondents reported that their household's livelihood had been in one way or other impacted by the ongoing COVID-19 pandemic. The vast majority of these households (96.4%) reported that the impact had been mostly negative, while 3.6% expressed that the impact was mostly positive for them.

Figure 16 shows program impact by whether a household reported being negatively affected by the COVID-19 pandemic.

Figure 16: Reported impact of COVID-19 on HH livelihoods



First, it's notable that households that were more likely to report being negatively affected by COVID have *better* outcomes than households that reported not being negatively affected. This likely reflects geographic differences (87% of households in Kenya reported being negatively affected versus 81% of households in Uganda) as well as differences in households' sources of livelihood (85% of treatment households engaged in retail, which tends to be a higher-earning business type, reported being negatively affected versus 76% of laborers, who tend to be lower-earning).

Second, there is no statistically significant difference in treatment effects between households that reported being negatively affected by COVID-19 compared to those that reported a positive effect or no effect.[43] This may indicate that Village Enterprise was successful in helping treatment households to realize the potential of the program even when those households were navigating the economic shocks of the pandemic. At the same time, we advise against interpreting this result to mean that the pandemic had no effect on the program's impact. Our metric for COVID-19's impact on a household is self-reported, and it may be that a household was financially impacted by the pandemic even if they did not realize it (and similarly some households may not have been as impacted as they perceived). Instead, we regard this metric as distinguishing between households that were more versus less-obviously impacted, and providing suggestive evidence that even the most impacted households still benefited from the program.

Qualitative responses shared by respondents show that the pandemic affected the economic well-being of both treatment and control groups. Respondents mentioned that the restrictions put in place due to the COVID-19 pandemic - which included curfews; market, road, and border closures; as well as restriction on social gatherings - affected their economic activities and increased the cost of living. Sellers mentioned that market closures interrupted their operations since they were not able to meet their customers, which reduced their income significantly. Some respondents also mentioned limited ability to meet their basic needs due to a decrease in income caused by loss of formal and casual labor,

[43] Additionally, the fraction of households that report being negatively affected by COVID are similar in treatment (84.4%) and control (83.5%) villages.

as well as a decrease in remittances for those who were dependent on family or organizational support. The pandemic also affected the price levels of different items, with some basic food items, as well as medicine, becoming more expensive, while demand for other non-food items like apparel decreased, forcing sellers to reduce prices.

We provide more details about the self-reported effects of the COVID-19 pandemic on respondents' livelihoods in Appendix C.

6. Discussion

Six months to 2.5 years after the conclusion of the program, the Village Enterprise program continued to have a positive, statistically significant, and meaningful impact on the livelihoods of its ultra-poor recipients. Households in the treatment group reported an increase in consumption and assets, relative to the control group, despite disruptions caused by the COVID-19 pandemic. The program had a positive and statistically significant impact on both countries, though the impact estimates show a larger effect in Kenya.

We hypothesize that treatment effects were larger in Kenya for several reasons. First, the study sample in Kenya was wealthier at baseline than the study sample in Uganda, and consumption effects were positively correlated with baseline wealth levels. Second, consumption effects were lower on average for households surveyed after the lockdown vs before the lockdown in Uganda (-0.1 USD vs 5.9 USD per month). However, the consumption effect for the poorest quintile in Kenya (12.8 USD) was still larger than the consumption effect for the richest quintile in Uganda after removing households surveyed after the lockdown (7.0 USD per month), suggesting that there may be other reasons for larger treatment effects in Kenya. For instance, it may be that BGs in Kenya had better access to markets than their counterparts in Uganda. We do not think that the difference is explained by the larger transfer sizes to certain households in Kenya, since consumption effects are actually larger for households that received smaller transfers (though the difference is not statistically significant).

Although effect sizes were smaller in Uganda, in nominal terms, the effect on consumption was nearly identical to the previous impact estimate of the Village Enterprise program in Uganda. Sedlmayr et al (2020) reported that the Village Enterprise program increased per capita annual consumption by UGX 26,061. Using the 2017 exchange rate of 1 USD to 3,580 UGX and average household size of 6.5 members (as observed in our Uganda sample), Sedlmayr et al's estimate in terms of household-level monthly consumption converts to 3.94 USD, compared to our impact estimate of 3.87 USD. On the other hand, Sedlmayr et al (2020) found larger effects on net assets in Uganda: 29.67 USD per household, or roughly twice the effect that we observed among Ugandan households (and half the size of the effect among Kenyan households). The replication of impact on consumption is particularly impressive given the effects of the COVID-19 pandemic on livelihoods, though the reduction in asset effects may reflect households coping with the effects of the pandemic. Appendix L provides more details on the comparison of effect sizes between our study and Sedlmayr et al (2020).

While treatment effects on average were positive and significant, magnitudes varied by program cohort. In particular, we observe smaller effect sizes on consumption and assets in both countries for Cohort 4-Dec2019 that are statistically indistinguishable from zero. This may be driven in part by timing: Cohort 4-Dec2019 completed the program shortly before the COVID-19 pandemic and ensuing lockdowns, which may have given inadequate time for new businesses to become established. Moreover, Village Enterprise provided extended mentoring to Cohort 5-Apr2020 through Cohort 7-Dec2020 to help offset COVID disruptions; this mentoring may have been a critical input to enable these cohorts to achieve sustained consumption effects (especially in Kenya) that was not available to Cohort 4-Dec2019.

In Kenya, where some BGs received larger grants ($150 per household) than others ($50 per household), the size of the grant appears to have had little influence on consumption effects. This is consistent with recent evidence from cash transfer programs, where larger cash transfers do not translate into

meaningfully larger or longer-term program impact (Kondylis & Loeser 2021).

At the same time, households that received larger grants had significantly larger treatment effects on net assets than households that received smaller grants. Treatment effects for households that received larger grants are driven primarily by new home purchases, household assets (especially large electronics, furniture, chickens, and goats), savings with community savings groups, and business stock. These results mirror the results for the full Kenya sample, suggesting that households that received larger grants invested in 'more of the same' as households that received smaller grants. It is therefore somewhat surprising that we do not observe a concomitant increase in consumption effects for households that received larger grants. Perhaps such consumption differences would emerge over a longer time horizon as households that received smaller grants spend down their asset values more rapidly than households that received larger grants.

Consistent with previous poverty graduation programs (e.g. those studied in Banerjee et al 2015), treatment effects on consumption are generally larger for households with more baseline wealth. Of course, all households in the study were categorized as ultra-poor at baseline, so differences in wealth levels are relative to an already low distribution in each country. Nevertheless, our results are consistent with previous evaluations, such as Banerjee et al 2015, suggesting that poverty graduation programs may be most effective for households that are not at the very bottom of the wealth distribution.

The results from this study show that the Village Enterprise program had a statistically significant positive impact on the livelihoods of its beneficiaries and provides valuable insights for future similar interventions. First, the use of an RCT for the evaluation of the DIB provides an unbiased estimate of the impact of the program at the time of data collection to determine the outcome payments to be made to Village Enterprise. These results may have been different if data collection had not been delayed or the lockdown in Uganda had not occurred. Second, to our knowledge this is the first evaluation that looks at the impact of graduation style interventions in household consumption and assets during the COVID-19 pandemic. Our findings show that, in spite of the pandemic, beneficiaries of the Village Enterprise program were still able to increase their consumption and assets, which points to the success of the Village Enterprise model in promoting self-reliance amongst its beneficiaries. Lastly, the use of administrative data sources containing information on business groups and business owners, shared by Village Enterprise to complement the data that was collected during field work, allowed the team to look at the different dimensions of analysis that provide a richer and better understanding on the impact of the program at different levels.

Limitations of the study

Nonresponse and replacement sampling

During data collection, some sampled households were unavailable for the endline survey and had to be replaced with randomly-selected alternates from the list of eligible households. These replacements occurred for various reasons, though they were exacerbated by the COVID-19 pandemic. The overall replacement rate was 14.0% (13.0% in treatment, 14.7% in control).

Although nonresponse and replacement rates were similar across treatment arms, nonresponse could introduce bias if nonrespondents differ between treatment arms, or could reduce the external validity of impact estimates if nonrespondents differ from replaced households. In Appendix D we show that nonrespondents were similar across treatment arms and the final sample with replacements was

statistically representative of the population of eligible households.

Missing values

Missing values in the endline dataset occurred when the respondent said that their household or business consumed or owned an item but did not know the cost, quantity, or total value of that item. Although enumerators encouraged respondents to give their best estimate when the respondent was not certain of a value, some respondents could not provide a reasonable guess for some survey items and the value was coded as missing. The average survey item is missing 0.3% of responses or 2.0% of all non-zero responses.

As specified in our PAP, for missing costs or prices, we used the median of reported sub-county cost or price for the same item. For missing covariate information, we replaced the missing observation values with zero and added a missing indicator for that covariate.[44] However, for missing quantities or total values, corresponding to 0.3% of all values from these variables, we deviate from our pre-specified approach of replacing the missing value with zero. Instead, we replaced missing quantities or total values with the median reported by other households in the same village. We do this because households with missing values reported consuming the good in question, but they were unsure of the amount. If a different percentage of households in treatment and control reported consuming a good but are unsure of the amount, then replacing missing values with zero would bias estimates of differences in consumption between treatment and control groups.

We describe the prevalence of missing data and our approach to imputation in detail in Appendix E. We note that since missingness is relatively rare, imputation has a small effect on outcome estimates: for the average household, imputed values comprise 1.0% of total consumption and 2.0% of net assets.

Outliers

As pre-specified in our PAP, to limit the influence of outliers on variance estimators, we winsorized consumption and net assets variables at the 97.5th and 2.5th percentiles.[45]

COVID-19 impact

Conducting fieldwork during the COVID-19 pandemic exacerbated some technical risks. In particular, it may have affected the generalizability of the findings to a non-pandemic context. From the analysis of the self-reported effects of COVID-19, we noted that the COVID-19 crises reportedly affected livelihoods for 87% of our respondents. As such, while the evaluation results may still be internally valid for this context, there is a risk that impact estimates may not generalize to the non-COVID-19 context.

[44] In practice this only occurred for one covariate - number of school years of household head - which was missing data for 650 or 6.6% of observations.

[45] Winsorization is a technique used to limit the influence of outliers on variance estimators. It replaces extreme values above a predefined percentile with the values at that percentile. In this case, all values above the 97.5th percentile were replaced with the 97.5th percentile of the estimate in question for our primary outcomes, and all values below the 2.5th percentile were replaced with the 2.5th percentile of the estimate in question for our primary outcomes.

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8. Appendices

Appendix A: Outcome payment and impact projection

Outcome payment under OPA-A2

In this appendix we calculate the DIB Outcome Payment (payment type 2, or P_2) to be made to Village Enterprise. Based on the evaluation results, we find that P_2 = \$2,493,964.36, such that Village Enterprise maxes out the total DIB outcomes payment of \$4,280,618. Village Enterprise will use these funds to repay investors. All payment formulas are copied from the *Outcomes Payment Agreement--2nd Amendment (OPA-A2)*, effective as of January 1, 2021.

Previously, two payments had been made to Village Enterprise: reimbursement of seed capital to business groups for all seven cohorts (payment type 1, or P_1) = \$1,138,715.64 and reimbursement for intervention costs for Cohort 1-Dec2018 through Cohort 4-Dec2019 (payment type 1.5, or $P_{1.5}$) = \$647,938. $P_{1.5}$ was equivalent to 60% of implementation costs that were incurred due to the COVID-19 pandemic.

Since the average increase in assets as of the evaluation a_m = \$40.49 ≥ 0, we use the payment formula corresponding to the "Optimistic scenario" in the *OPA-A2*:

$P_{2B} = P_{2A} + c_m \frac{1}{s}$, where $P_{2A} = c_m m - \overline{a_0}$, and

- c_m -> ITT estimate of consumption = \$9.88
- m -> weighted average of number of months from the seed transfers to data collection, accounting for the size of transfer and timeline of transfers made = 24.3
- s -> annual discount rate of 10%, as defined in *OPA-A2*, corresponding to a monthly discount rate 10%/12 = 0.83%
- $\overline{a_0}$ -> weighted average seed capital size made to each household for all cohorts = \$77.14

Thus,

$P_{2A} = c_m m - \overline{a_0}$ = 9.88*24.3 - 77.14 = \$162.77,[46] and

$P_{2B} = P_{2A} + c_m \frac{1}{s_m}$ = \$162.77 + 9.88*(1/0.0083) = \$1,347.82.

According to *OPA-A2*, P_{2B} is subject to a payment cap of \$265 per household, thus we have P_{2B} = \$265, and the sum of all three payments is:

$P_{1total} + P_{1.5total} + P_2 * ITT\ population$ = \$1,138,715.64 + \$647,938 + \$265*14,772 = \$5,701,233.64

Since the sum of all three payments is subject to a payment cap of \$4,280,618, we have:

P_{2total} = \$4,280,618 - P_{1total} - $P_{1.5total}$ = \$2,493,964.36.

[46] Note that for all equations, we list rounded values in the text of this appendix, but we do not round in any of the calculations. Thus the values in the equations as written in this appendix do not necessarily sum to the exact values listed after each equality.

Outcome payment under OPA-1

In this section we estimate what the outcome payment would have been under the original Outcomes Payment Agreement (OPA-1). This calculation is purely for illustration; it has no bearing on the DIB payments. Note that OPA-1 intended for there to be two outcome payments: $P_{2(1)total}$, corresponding to the outcome payment for Cohorts 1, 2, 3, and 4 based on data collected during the first endline in April/May 2020, and $P_{2(2)total}$, corresponding to the outcome payment for all cohorts based on data collected during the second endline in April/May 2021 minus $P_{2(1)total}$. Since IDinsight conducted a single endline in May-August 2021, we set $P_{2(1)total}$ = 0 in the calculations below. This calculation differs from what $P_{2(1)total}$+$P_{2(2)total}$ would have been under OPA-1 to the extent that treatment effects vary over time.

Since the average increase in assets as of the endline is a_m = \$40.49, and the weighted average seed capital size made to each household for all cohorts $\overline{a_0}$ = \$77.14, we have $\overline{a_0} > a_m > 0$, corresponding to the "Medium Scenario" in OPA-1. To calculate the appropriate payment in this scenario, we first estimate the depreciation rate:

$d = 1-(a_m / \overline{a_0})^{(1/m)} = 1-(40.49/77.14)^{(1/24.3)} = 2.6\%$

Thus,

$P_{2A} = c_m m - \overline{a_0}$ = \$162.77 (as before under OPA-A2), and

$P_{2B} = P_{2A} + c_m \frac{1-d}{s+d} = \$162.77 + 9.88 * (\frac{1-0.026}{0.0083+0.026}) = \$441.38$[47].

Since P_{2B} was still subject to a payment cap of \$265 per household under OPA-1, we have P_{2B} = \$265, so P_{2total} = \$265*14,772 = \$3,914,580. However, since P_{1total} = \$1,138,715.64, then P_{1total}+P_{2total} > \$4,280,618, which is the maximum DIB payment, and so we have P_{2total} = \$4,280,618 - \$1,138,715.64 = \$3,141,902. Thus, even without $P_{1.5}$ (the reimbursement of 60% of programming costs due to COVID-19), VE would still have maxed out the total DIB payment of \$4,280,618 under OPA-1.

Projection of total lifetime impact

We estimate total lifetime impact of the program as the sum of consumption effects at the time of endline data collection, plus a projection of future consumption effects:

- *Impact from program start to endline data collection*: At the time of endline data collection in mid-2021, the average household had started the program 24.3 months ago, and so program impact to that point is estimated as (\$9.88 treatment effect on monthly consumption)*(14,772 treated households)*(24.3 months) = \$3.55M.
- *Projected future impact*: Future impact is estimated as (\$9.88)*(14,772 treated households)*(12 months per year)/(0.1 annual discount rate) = \$17.51M.

Thus we estimate total lifetime impact of the Village Enterprise program is \$3.55M + \$17.51M = \$21.06M.

[47] In this calculation, we use unrounded figures. If rounded figures are plugged into the formula, the result would be \$443.33

Appendix B: Regression results for main outcomes

Table B1: Treatment effects of main outcomes

	(1) Control Mean	(2) Treatment Effect	(3) p-value	(4) Std. Error	(5) N	(6) R2
ITT Consumption	158.007	9.875***	0.000	1.700	9888	0.3297
ITT Assets	703.296	40.490***	0.003	13.638	9846	0.1606
TOT Consumption	158.007	10.290***	0.000	1.752	9888	0.3297
TOT Assets	703.296	42.185***	0.003	14.017	9846	0.1606

* p < 0.10, **p < 0.05, *** p< 0.01

Appendix C: Self-reported effects of COVID on livelihoods

During endline data collection, Kenya was experiencing the third wave of COVID-19 while Uganda was in the second wave. For this reason, we added a short section on the effect of COVID-19 on the economic well-being of the households we were surveying, consisting of the following questions:

- Has your household's livelihood been in any way impacted by the ongoing COVID-19 pandemic?
- Would you say that the impact has been mostly positive or mostly negative?
- Please briefly explain your answer in a few sentences

87% of our respondents reported that their household's livelihood had been in one way or another impacted by the ongoing COVID-19 crisis. Out of this number, 96.4% reported that the impact had been mostly negative, while 3.6% expressed that the impact was mostly positive for them. We then requested the respondents to share more information to support their responses and below is a summary of the key themes that came out of all the responses.

1. **Access to markets**

Just over a third (38%) of the respondents who reported that the pandemic affected their livelihoods negatively mentioned accessibility to markets. Market closures were one of the directives by the Governments of Kenya and Uganda as part of efforts to contain the spread of the virus. This affected both buyers and sellers in different ways. Sellers were not able to reach their usual customers as before, and some small-scale sellers who would typically get their stock from the open-air markets were left without a source for their stock. Also, owing to the pandemic, the number and the purchasing power of customers went down. People were no longer able and willing to pay as before because of the uncertainty that came with the different COVID waves; demand for some items decreased. Others expressed that there was the loss of casual labor occurring in markets, like for those who make their daily living from transporting items from the market at a fee or are employed as extra hands or to attract customers by those who own businesses during the market day. Additionally, there was a 7pm curfew in both countries that reduced the “selling time”. Some businesses would sell more in the evenings but this was restricted since they had to close early. Others, like the alcohol business, were directed to completely close which left the owners without a source of income.

2. **Change in the price level**

Respondents mentioned that the cost of living increased due to the pandemic. Basic food items, transportation, and medicine became more expensive due to disruptions in the supply chain caused by market and border closures, as well as the increased transport costs incurred by sellers transporting the commodities. Public transport was required to only carry half as many passengers to ensure social distancing. As a result,fare prices increased to cover the cost of fuel. On the other hand, some respondents noted a decrease in the prices of some commodities, such as apparel, due to a decrease in demand.

3. **Business activity**

Many business owners reported that their businesses shrunk or closed as a result of COVID-19. Roads, border closures, and lockdowns in both countries affected business activities. In Uganda, bodaboda riders faced restrictions to only carry one passenger which reduced their income. Some respondents expressed that they needed to hire a middleman to transport their goods which reduced their profit. In

some cases respondents no longer had access to a middleman and thus could not sell their goods in the market. There was also an increased number of customers taking products on credit and failing to pay in good time which affected restocking. This was the case for some casual workers who reported to work but were not paid or their pay was delayed. Some respondents stated that they were forced to use business capital to pay for household expenses which led to business failure. Others who had plans to expand their businesses or start a business canceled their plans due to COVID. For farmers, a number reported that they had to end group farming as they would not be able to pay casual workers, and some reverted to subsistence farming. Many respondents also mentioned that they could no longer meet in their local savings group since people didn't have much to save, and hence they could not borrow from the groups to boost their businesses as before.

4. Impact on livelihoods

We had several self-reported cases of increased poverty/hunger attributed to the effects of COVID-19. One of the reasons given for this was the increased cost of food. Household income also decreased as business profit was reduced due to COVID or the breadwinner of the family lost their job. Many survey respondents depended on casual work and, due to COVID, the demand for casual labor declined while the supply increased since those who were formally employed and lost their jobs joined this pool. Therefore, even if one was lucky enough to get casual work, the salary was much lower compared to pre-pandemic rates. Others, especially the elderly who depended on support from family, friends, and the church reported that there was a decrease in financial assistance since the onset of COVID-19. There were households that were receiving support from the government and NGOs as a form of social protection prior to COVID, and they also reported that this was no longer happening or its frequency decreased.

Schools were closed to prevent the spread of the virus. This meant that those households with children in boarding schools as well as those who ate some meals from school now had to cover their children's food expenses at home. This significantly increased the household's expenditure on food items and some struggled to provide for their families. Some parents also expressed frustration that the school fees they had already paid as well as the other school-related expenses they had incurred before their children were sent home due to COVID would go to waste. Unfortunately, there were also a few cases where it was reported that the teenage pregnancy rate increased due to COVID. Further, respondents expressed that they were burdened by the need to spend additional money to purchase masks, sanitizers, or pay police fines during the lockdown.

5. Positive impact

COVID-19 did not entirely have negative effects; 3.6% of our respondents reported that it mostly had positive effects on their livelihoods. Some sellers who were able to navigate the challenges presented by COVID ended up benefiting more due to lack of or reduced competition when their counterparts' businesses failed. There are others who had commodities that became more expensive due to COVID and as a result, they were able to make more profit. Additionally, demand increased for some respondents' products, like firewood to accommodate an increase in cooking since children were out of school and needed to take their meals at home. Some respondents started businesses in response to COVID, like selling masks. Some household heads reported having an additional set of hands to help with farming and house chores as a result of the closure of schools. For those respondents who were farming in addition to other business activities, since they could not go out as much due to COVID, they reported

that they put more time and focused on farming which increased their productivity. Also, the closures of businesses and decreased activity forced some respondents to learn to limit or reduce their discretionary spending. Furthermore, some respondents reported having been beneficiaries of COVID-related initiatives by NGOs and humanitarian organizations.

6. No COVID impact

13% of respondents reported that their livelihoods were not impacted by the ongoing pandemic. Respondents who sold produce from their homes and subsistence farmers were not affected by the closure of markets. The elderly and other dependents who mostly stayed at home pre-pandemic were less affected by the lockdown.. Respondents living in extreme poverty without a business, a job, or many assets were less affected by the pandemic. Further, some respondents reported that their sectors of work were not affected, like those offering essential services.

Appendix D: Nonresponse and replacement sampling

This appendix clarifies issues over nonresponse, attrition and replacement sampling in the Village Enterprise DIB evaluation. Since the evaluation did not include a baseline, there is conceptual ambiguity over the meaning of attrition in the pre-analysis plan. In this appendix we introduce the related but distinct concept of replacement sampling, or substituting initially sampled households with randomly selected replacements. The final replacement rate of 14.0% (13.0% in treatment, 14.7% in control) was largely driven by COVID restrictions on data collection, especially curfews enforced in response to surges in COVID cases, as we predicted may happen in our pre-analysis plan. We show that nonrespondents are similar across treatment and control groups and the final sample with replacements is statistically representative of the population of eligible households. As a result, the initially specified analytical model with our final sample, including replacements, continues to offer the most robust estimate of the impact of the Village Enterprise program.

1. Attrition protocols in the pre-analysis plan

In our pre-analysis plan,[48] we defined attrition as being unable to collect data from participants who dropped out of the study between baseline and endline (p. 15). We specified possible actions in response to various levels of attrition, citing 10% as our target threshold (p.16). We also added a section in the latest version of the PAP related to technical risks due to collecting data during the COVID-19 pandemic (p. 17), in which we noted that attrition may be higher than projected (>10%) due to respondent reluctance to speak with enumerators or transportation and mobility challenges during the pandemic.

While these protocols provided important guidance for our data collection approach, we realized after data collection that there was conceptual confusion over the meaning of 'attrition' in the PAP. Attrition refers to the inability to collect data from individuals who were in the original study sample.[49] In this evaluation, although Village Enterprise conducted a census of eligible households, there was no sample taken at baseline, and instead the study sample was taken at endline. As a result, 'attrition' is not a meaningful concept for this evaluation. Instead, the PAP should have described the related but distinct concept of nonresponse at endline.

The distinction between nonresponse and attrition is a common feature of RCTs. For instance, the World Bank's Impact Evaluation in Practice describes 'unit nonresponse' as "aris[ing] when it becomes impossible to collect complete data for some sampled units" and 'attrition' as "a common form of nonresponse that occurs when some units drop from the sample between data collection rounds". While researchers often do not report nonresponse rates in their RCTs, some do and, if applicable, will differentiate the nonresponse rate from the attrition rate. For instance, in the poverty graduation RCT literature, Sedlmayr et al (2018) distinguish between sampled respondents who "opted to participate in the study" (p. 9) (pertaining to 6.4% of the sample) from those for whom "follow-ups were successful" (p. 20). Blattman et al 2016 distinguish between "nonresponse" (p. 42) (pertaining to 5.5% of the sample) and "attrition" (p. 44). Fafchamps et al 2014 distinguish between sampled units that could be found and willing to participate in the study (p. 215) and attrition (p. 216).

Nonresponse at endline should be tracked and investigated since missingness resulting from

[48] https://www.socialscienceregistry.org/trials/7168
[49] This definition is given on p.58 in Duflo, Glennerster, and Kremer (2006) "Using Randomization in Development Economics Research: A Toolkit" CEPR No. 6059, and elsewhere.

nonresponse at endline can reduce study power, different nonresponse rates at endline in the treatment and control groups can bias impact estimates, and different characteristics among those who did and did not respond can reduce external validity of the study. In the following section we describe how replacement sampling was successfully used to ensure that non-response at endline did not harmfully reduce power, that missingness is independent of potential outcomes (MIPO), and that the final sample with replacements likely remains representative of the population of target households.

2. Replacement sampling in the Village Enterprise DIB evaluation

Replacement sampling refers to substituting households that were in the initial sample with randomly selected replacements. Had a baseline survey been conducted, it is likely that some of the households that were replaced at endline would have been replaced at baseline as well for similar reasons (e.g. if a household refused consent at endline, it is likely that they would have refused consent at baseline as well). Of those in the initial sample, the nonresponse rate was 16%. We executed a random replacement sampling approach that led to an endline replacement rate of 14.0% (13.0% in T, 14.7% in C), and collection of data from 98% of the target number of households. Using random sampling ensured that nonresponse at endline would not harmfully reduce study power.

These replacements occurred for a variety of reasons, but were exacerbated by the COVID pandemic. In particular, the most common reason for replacement, accounting for ~50% of replacements, was that no one was available to be surveyed at the household until after COVID-induced curfews, which were enforced at 7pm in both countries. In such cases our enumerators attempted to track someone from the household to another location in the village and complete the survey at that location. Enumerators also returned to villages to reach households that were unavailable on the first visit. Households were only replaced if no one was available within a reasonable distance, or couldn't be reached during up to 3 revisits, until after field teams had to return to their hotels due to curfews. Our protocols were based on standards that are commonly used in deciding whether to replace households in evaluations, such as those described in the section on "Conducting Fieldwork and Undertaking Quality Control" in the World Bank's Impact Evaluation in Practice: Second Edition (p. 308-311).

3. Missingness, Internal Validity, and External Validity

Village Enterprise collected targeting and covariate data at baseline that we use to explore the independence of missingness coming from nonrespondents with potential outcomes. The nonresponse rates were similar across treatment (14.8%) and control (16.8%) groups.[50] We explore MIPO conditioned on different indicators that are likely predictive of household consumption and assets, such as PPI scores and household characteristics, making comparisons between treatment and control groups as well as between nonresponse and replacement households.

[50] One concern raised was that households may have been unavailable because they were engaged in economic activities (such as buying/selling at the market) resulting from participation in the Village Enterprise program, and thus we would be missing out on the households most impacted by the program. The fact that replacement was slightly higher in the control group suggests that this was not occurring.

Table D1. Balance across treatment and control groups for nonrespondents

Variable	Treatment	Control	Diff	p-value
PPI score	27.24	27.44	-0.19	0.88
Number of people living in the HH	5.97	6.08	-0.11	0.48
Number of cows the HH owns	0.55	0.57	-0.02	0.59
Number of bodas the HH owns	0.04	0.04	-0.01	0.64
Whether HH owns a radio	0.29	0.27	0.02	0.71
Whether every HH member has a pair of shoes	0.11	0.10	0.01	0.53
Whether HH has an iron roof	0.63	0.64	-0.02	0.87
Number of beds the HH owns	0.76	0.82	-0.05	0.14
Number of pigs the HH owns	0.15	0.10	0.04	0.60
Number of bicycles the HH owns	0.29	0.29	0.00	0.83
Whether HH owns a frying pan	0.01	0.02	-0.01	0.23
Number of jerry cans the HH owns	2.98	3.20	-0.22	0.05
Whether the HH owns a mobile phone	0.66	0.68	-0.02	0.78
Number of years of school for the HH head	6.08	6.21	-0.12	0.94
N	**750**	**849**		

Table D1 shows that the characteristics across treatment and control groups for nonrespondents are very similar. None of the differences are significant at the 5% level. This, along with the similar replacement rates across treatment and control groups, indicates that the replacement strategy employed did not jeopardize internal validity.

Table D2. Balance across treatment and control groups for nonrespondents (Kenya)

Variable	Treatment	Control	Diff	p-value
PPI score	31.08	31.75	-0.68	0.12
N	**453**	**533**		

Table D3. Balance across treatment and control groups for nonrespondents (Uganda)

Variable	Treatment	Control	Diff	p-value
PPI score	21.40	20.16	1.25	0.06
N	**297**	**316**		

Table D4 below presents the findings from the comparison between nonrespondents and replacement households.

Table D4. Balance across treatment and control groups for nonrespondents and replacements

Variable	Nonrespondents	Replacements	Diff	p-value
PPI score	27.35	26.05	1.29	0.00
Number of people living in the HH	6.03	6.60	-0.58	0.00
Number of cows the HH owns	0.56	0.81	-0.25	0.00
Number of bodas the HH owns	0.04	0.04	0.01	0.46
Whether HH owns a radio	0.28	0.32	-0.04	0.02
Whether every HH member has a pair of shoes	0.11	0.10	0.01	0.82
Whether HH has an iron roof	0.63	0.64	0.00	0.22
Number of beds the HH owns	0.79	0.90	-0.11	0.00
Number of pigs the HH owns	0.12	0.14	-0.02	0.48
Number of bicycles the HH owns	0.29	0.36	-0.08	0.00
Whether HH owns a frying pan	0.01	0.01	0.00	0.39
Number of jerry cans the HH owns	3.09	3.28	-0.19	0.01
Whether the HH owns a mobile phone	0.67	0.71	-0.04	0.01

Number of years of school for the HH head	6.15	6.02	0.13	0.45
N	**1599**	**1365**		

Table D4 shows that there are some differences between nonrespondents and replacement households. PPI scores, for instance, are slightly lower for replacement households (27.3 vs 26.1).[51] Replacement households have more household members, cows, radios, beds, bikes, jerry cans, and phones. However, the differences are generally small in magnitude, and given that the replacement rate for the study is low overall (14%), these differences are likely to have a limited effect on external validity. The final sample with replacements has nearly the same PPI average score as the list of all eligible households (25.5 and 25.7, respectively).

Similarity across these indicators for both sets of comparisons provides suggestive evidence that differential nonresponse across treatment and control groups is not introducing bias to the study, and that the final sample with replacements likely remains representative of the population of target households. For these reasons, we are confident that the original model with the final sample (including replacements) continues to offer the most robust estimate of the impact of the Village Enterprise program.

Table D5. Balance across treatment and control groups for nonrespondents and replacements (Kenya)

Variable	Nonrespondents	Replacements	Diff	p-value
PPI score	31.44	29.79	1.65	0.00
N	**986**	**831**		

Table D6. Balance across treatment and control groups for nonrespondents and replacements (Uganda)

Variable	Nonrespondents	Replacements	Diff	p-value
PPI score	20.76	20.24	1.25	0.06
N	**613**	**534**		

[51] This magnitude is small relative to the range of PPI scores among eligible households at baseline, which vary from 0 to 75 with a standard deviation of 10.3.

Appendix E: Missing values and imputation approach

This appendix describes our process for imputing missing values in the endline dataset. While missingness occurs in our dataset, it is not common: the average survey item is missing 0.3% of all responses or 2.0% of all non-zero responses. As a result, imputed values comprise a relatively small fraction of primary outcome values: for the average household, 1.0% of total consumption and 2.0% of net assets rely on imputed quantities or values within component items. However, as described below, we deviate slightly from our pre-specified approach in how we impute missing values.

1. Prevalence of missing values in the Village Enterprise DIB evaluation dataset

Missing values in the endline dataset occurred when the respondent said that their household or business consumed or owned an item but did not know the cost, quantity, or total value of that item. Although enumerators encouraged respondents to give their best guess when the respondent was not certain of a value, some respondents could not provide a reasonable guess for some survey items and the value was coded as missing.

The average survey item relating to quantity or total value is missing 0.3% of all responses or 2.4% of all non-zero responses.[52] 23% of survey items are not missing any responses, 90% of survey items are missing fewer than 1% of all responses, and 59% of survey items are missing fewer than 1% of non-zero responses. The survey item with the highest prevalence of missingness is the quantity of alcohol consumed by households in Kenya, which is missing 4% of all responses or 22% of responses where the household reported consuming at least some alcohol.

The average survey item relating to cost is missing 0.4% of all responses or 1.2% of responses from individuals who consumed or owned those items.[53] 91% of survey items relating to cost have at least some missing values, though only 11% of survey items are missing more than 1% of all response values. As with survey items relating to quantity or total value, the survey item with the highest prevalence of missingness is the cost of alcohol consumed by households in Kenya, with 21% of respondents who reported consuming some alcohol failing to provide a cost for that consumption.

2. Deviation from pre-specified approach to dealing with missing data

In our PAP we specified three methods for addressing missing values:

- “the median of reported sub-county prices will be used when a price is unknown” (p. 12)
- “If any households have missing covariate information, we will replace the missing observation values with zero and add a missing indicator for that covariate” (p. 13)
- “If a participant does not know the answer to a response or refuses to give an answer, we will treat that category as a zero value for the household consumption and asset value.” (p. 17)

We document one update to the pre-specified approach of dealing with missing quantities/total values, relating to #3 above.

[52] A table listing all 172 consumption and asset survey items relating to quantities or total values, along with the number of responses, the number of non-zero responses, the number of missing values, and the frequency of missing values as a percent of all responses and all non-zero responses, is available upon request. We omit it from this report due to space constraints.
[53] A table listing all 75 consumption and asset survey items relating to costs, along with the same statistics listed in the previous footnote, is available upon request. We omit it from this report due to space constraints.

For quantities and total values, we were concerned that the pre-specified approach of replacing missing values with zero could bias treatment effect estimates toward zero. Households with missing values reported consuming the good in question, but they were unsure of the amount. If a different percentage of households in treatment and control reported consuming a good but are unsure of the amount, then replacing missing values with zero would mis-estimate true differences in consumption between treatment and control groups.

Instead, our preferred and updated approach is to replace missing values with the median reported by other households in the same village.[54] Where there are no non-missing observations for a particular item from the same village, we replace missing values with the median reported by other households in the same treatment group and subcounty. We believe that this updated approach results in more accurate estimates of the amount of each good consumed or owned than replacing missing values with zero, and less biased treatment effect estimates.

3. Missing data imputation process

In this section we describe the process followed for imputing missing values for each category of survey items.

Consumption (one-week recall items)

For one-week recall items, respondents reported the amount consumed, the units of consumption, and the price per unit. For most items, respondents in our study reported different units of consumption. For instance, 77% of households in Kenya reported milk consumption in cups, while 9% reported milk consumption in liters, 9% reported milk consumption in bottles, and the remainder reported milk consumption in other units.

For missing cost values for these items, we calculated the median cost per unit in each subcounty, and replaced missing cost values for respondents that reported consumption in the same units.

For missing quantities, we first calculated the total value of consumption (quantity*price per unit) for each item among households that have non-missing quantity and price values. We then imputed missing total values for households with missing quantities, rather than imputing quantities directly, since households reported consumption in different units. We calculated the median total value spent on each item within each village, and used these values to impute missing total values for households in the same village.

Consumption (one-month and one-year recall items)

For one-month and one-year recall items, respondents only reported the total amount spent on each item, and not the amount consumed or the price per unit. For this reason, we only impute total values, using the median total value for each item reported by households within the same village.

Construction services

Since construction services (such as new homes, additions to homes, plumbing additions and repairs, etc.) are relatively rare in our sample, we do not have sufficient data to impute median costs of different

[54] Since all households in a village have the same treatment status, this implies that quantities and total values are imputed within the treatment group.

types of construction projects in each village. Instead, we calculate the median cost per construction type within each subcounty X treatment group, and apply those values to missing values for the same types of construction projects in the same subcounty X treatment group.

Household assets

Respondents reported the number of household assets owned and the total amount that they would expect to earn if they sold all of those assets in their current condition. For households that reported the number but not the total value of an asset type, we calculated the median value-per-unit of that asset type within the same sub-county, and multiplied the result with the number of assets reported by the household. More rarely, a household did not know the number of assets of a particular type.[55] In these instances, we calculated the median number of that type of asset owned by other households in the same village, and replaced the missing value with the result.

Household savings

For household savings, respondents reported all of the places where they saved money (at home, mobile banking, in a savings group, etc.) and the amount saved in each location. For most savings types, there are insufficient observations to impute within the village, and so we calculate the median amount saved within each subcounty X treatment group and apply those results to missing values for the same savings type. We make an exception to this approach for savings with savings groups, since nearly half of households held this type of savings. For the households that reported savings with a savings group but did not know how much was saved, we calculated the median amount saved with savings groups by other households in the same village, and applied the result to the missing savings amount.

Household loans

For household loans, respondents reported the source of the loan (e.g. from a private bank, mobile banking, etc.), the original loan amount, and the amount still owed. For respondents that knew the original loan amount but not the amount still owed, we calculated the median fraction still owed on the same type of loans within the same subcounty X treatment group, and multiplied the result with the respondent's original loan amount to estimate the amount still owed. For respondents who did not know the original loan amount or the amount still owed, we calculated the median amount still owed on loans of the same type within the same subcounty X treatment group, and applied the result to the missing value.

Business assets

Respondents reported the total value of each type of asset owned by each household business, along with the number of business partners outside of their household for each business, and whether each asset was owned solely or with other business partners.[56] We do not know the cost-per-unit or the number of units of each asset type, and so we imputed the total value for missing asset types. Since there are relatively few business assets reported in our data, we do not have sufficient data to calculate total values for each asset type within each village. Instead, we calculated the median total value of each business asset type within each subcounty X treatment group, and applied those values to missing

[55] The asset type with the most missing values is chickens; 7 respondents or 0.09% of all 7,599 respondents with chickens did not know how many chickens they owned.

[56] 29% of respondents reported no household business, 48% reported 1 business, 18% reported 2 businesses, and the remaining 5% reported more than 2 businesses. The most businesses, reported by 2 respondents, was 7.

values for the same asset types within the same subcounty X treatment group.

Business savings

For business savings, respondents reported all of the places where they saved money for each business, the total amount saved, and the portion saved belonging to the household. Similar to household loans, we first imputed missing values for respondents who knew the total amount saved but not the portion belonging to the household. For these respondents we calculated the median fraction of each business savings type belonging to the household in each subcounty X treatment group and applied the result to missing values. For respondents who did not know the total amount saved or the portion belonging to the household, we calculated the median amount of each type of business savings belonging to the household in each sub-county X treatment group and applied the result to missing values.

Business loans

For business loans, respondents reported the source of each loan, the total loan amount, and the amount that the household owes. Similar to business savings, we first imputed missing values for respondents who knew the total loan amount but not the amount owed by the household. For these respondents we calculated the median fraction of each business loan type owed by the household in each subcounty X treatment group and applied the result to missing values. For respondents who did not know the total amount owed or the amount owed by the household, we calculated the median amount of each type of business loan owed by the household in each sub-county X treatment group and applied the result to missing values.

4. Imputed values in consumption and net assets variables

In this section we report the fraction of each outcome (monthly consumption, net assets) that relies on imputed data.

For the average household, 1.0% of total consumption relies on imputed quantities or values in its component items. 1.6% of total consumption relies on imputed quantities, total values, or costs. The median household has no imputed consumption data. For 95% of households, less than 5.4% of total consumption relies on imputed quantities or values in its component items, and less than 9.7% relies on imputed quantities, values, or costs.

For the average household, 2.3% of net assets relies on imputed quantities or values in its component items. Similarly, 2.3% of net assets relies on imputed quantities, total values, or costs.[57] The median household has no imputed net assets. For 95% of households, less than 6.3% of net assets relies on imputed quantities, values, or costs.

[57] The amount of net assets that involves imputed costs is very small (< 0.01%) but non-zero since costs could be calculated for a small number of asset items.

Appendix F: Treatment effects on individual components of consumption and assets

In this appendix we report treatment effects on individual components in the consumption and asset indices. As a reminder, to put the following effect sizes into context, the ITT effect on monthly consumption was USD 9.9 across both countries (USD 15.2 in Kenya, USD 3.9 in Uganda), and the ITT effect on net assets was USD 40.5 across both countries (USD 60.9 in Kenya, USD 15.6 in Uganda).

Treatment effects on consumption are driven mostly by food expenditures. Table F1 reports treatment effects on consumption items in Kenya. Treatment effects in Kenya are observed across many food items, and are largest (in absolute values) for green maize, beef, dried and smoked fish, maize grain or flour, and chicken. Treatment households in Kenya consumed significantly less alcohol. Positive treatment effects are also observed on some non-food items, including vehicle fuels, domestic services, and health expenses, and mobile airtime.

Table F2 reports treatment effects on consumption items in Uganda.[58] Treatment effects in Uganda are relatively smaller but are similarly concentrated in food items. Treatment households in Uganda consumed significantly more chicken, dried and smoked fish, and tomatoes. Treatment effects on non-food expenditures are positive on average but not significant for any particular items.

Treatment effects on assets are driven more by increases in household assets than in other types of assets or savings, though effects are observed in other categories as well. Table F3 reports treatment effects on asset items in both countries. In Kenya, treatment households spent more on new roof construction, new homes, and durable assets. Treatment households were also more likely to save with savings groups, and have larger mobile money and household savings.

In Uganda, treatment households spent more on livestock assets. In contrast to Kenya, treatment effects on cattle and sheep in Uganda are large and significant. Interestingly, treatment effects are negative for the asset value of bodas/motorcycles. We also observe positive and significant effects on savings with savings groups in Uganda.

In terms of business assets, positive and significant effects are only observed in Kenya, where treatment households had significantly more business stock, as well as more machines and tools. We do not observe significant effects on business assets in Uganda, or on business savings in either country. Treatment effects on household and business loans are mostly small and non-significant.

[58] Treatment effects on consumption items are reported in separate tables for Kenya and Uganda since the list of items in the consumption modules differed across the two countries. However, the list of items in the asset modules were the same in the two countries, and so we print a single table for treatment effects on asset items (though we still report the country-wise treatment effects in separate columns).

Table F1: Treatment effects on consumption items in Kenya

Item	Avg in Control	ITT	% Increase	q-val[59]
TOTAL CONSUMPTION[60]	207.41	15.21	7.3%	0.00
Food & beverage consumption	111.72	9.19	8.2%	0.00
Rice	2.77	0.58	20.8%	0.00
Maize grain or flour	20.43	0.64	3.1%	0.05
Green maize	7.79	1.22	15.6%	0.00
Bread	1.70	0.47	27.7%	0.00
Wheat grain or flour	2.13	0.55	25.9%	0.00
Sweet potatoes	1.19	0.28	23.5%	0.00
Matooke/cooking bananas	4.01	0.54	13.6%	0.00
Beans	7.95	0.43	5.4%	0.05
Onions/leeks	1.49	0.07	4.8%	0.04
Tomatoes	1.94	0.16	8.2%	0.00
Sukuma/kale	3.55	-0.01	-0.2%	0.34
Traditional vegetables	4.21	0.27	6.4%	0.01
Ripe bananas	2.10	0.37	17.8%	0.00
Avocado	5.26	0.56	10.6%	0.01
Fresh milk	10.10	0.31	3.1%	0.06
Beef	3.70	1.17	31.5%	0.00
Chicken	2.35	0.62	26.4%	0.00
Dried/smoked fish	2.74	0.74	27.1%	0.00
Cooking oil/fat	4.53	0.22	4.9%	0.02
Sugar	7.18	0.02	0.3%	0.31
Salt	0.74	0.02	2.1%	0.09
Tea	1.71	-0.01	-0.6%	0.28
Soda/soft drink	0.67	0.16	23.4%	0.00
Alcohol	2.56	-0.42	-16.3%	0.01
Food prepared outside of home	1.36	0.07	5.4%	0.15
Recurring consumption	37.41	2.94	7.9%	0.00
Fuel/kerosene/paraffin	0.59	0.06	9.9%	0.03
Soap/laundry bar/detergents	3.49	0.06	1.7%	0.14
Match boxes	0.34	-0.01	-2.1%	0.09
Barber/beauty shops	2.10	0.07	3.4%	0.14
Personal care/cosmetics	1.41	-0.02	-1.1%	0.28
Vehicle fuels (petrol, diesel, engine oil)	2.81	0.59	21.1%	0.03

[59] q-values can be interpreted like p-values but with an adjustment for multiple hypothesis testing. Because of the number of statistical tests performed in this item-wise analysis, the likelihood of incorrectly rejecting the null for any individual item is high. To account for the multiplicity of hypotheses being tested and to reduce the likelihood of incorrectly rejecting null hypotheses, we control for the false discovery rate (FDR) and report 'sharpened q values', as defined in Anderson (2008). These q-values can be interpreted as the expected FDR in the family of outcomes if we reject the null at that level. We define four families for these corrections: consumption in Kenya and Uganda, and assets in Kenya and Uganda.

[60] The ITTs of the component items do not sum to the ITT of total consumption for two reasons. First, total consumption is scaled by 1.176 in Kenya and 1.174 in Uganda to account for missing items in the shortened consumption module. Second, for the analysis in this appendix we winsorized individual components of consumption to limit the influence of outliers on ITTs and more clearly identify patterns. For total consumption we winsorized the aggregate index, but not the individual components, to limit the number of post-hoc adjustments to the values reported by respondents.

Matatus/buses	3.14	0.20	6.4%	0.11
Bodaboda/tuk-tuk	4.16	0.08	1.9%	0.24
Mobile phone airtime	4.73	0.31	6.6%	0.05
Firewood	9.52	0.16	1.6%	0.23
Domestic services (gardener, cook, guard)	2.65	0.75	28.2%	0.00
Infrequent consumption	27.50	0.66	2.4%	0.15
Men's clothing/footwear	1.22	0.08	6.9%	0.04
Women's clothing/footwear	1.50	0.09	5.8%	0.04
Children's clothing/footwear	1.69	0.06	3.6%	0.10
Educational expenses (school fees, uniforms, books, supplies)	16.88	0.00	0.0%	0.35
Health expenses	4.90	0.42	8.6%	0.03

Table F2: Treatment effects on consumption items in Uganda

Item	Avg in Control	ITT	% Increase	q-val
TOTAL CONSUMPTION	106.63	3.87	3.6%	0.04
Food & beverage consumption	65.12	2.55	3.9%	0.14
Rice	0.74	0.11	14.8%	0.21
Maize grain or flour	3.65	-0.05	-1.5%	1.00
Sweet potatoes	1.26	0.05	3.6%	0.92
Matooke/cooking bananas	0.82	0.05	5.9%	0.75
Chapati	0.37	0.06	16.3%	0.07
Beans	8.37	0.24	2.8%	0.47
Cassava/cassava flour	11.64	0.39	3.4%	0.39
Onions/leeks	0.88	0.04	4.7%	0.35
Tomatoes	1.61	0.19	12.0%	0.01
Greens (dodo, nakati, gyobyo, malakwang, other)	4.31	-0.19	-4.4%	0.35
Groundnuts	2.62	0.13	4.9%	0.67
Fresh milk	0.89	0.00	-0.2%	1.00
Beef	2.90	-0.19	-6.6%	0.67
Chicken	5.57	0.92	16.5%	0.00
Fresh fish	2.43	-0.04	-1.7%	1.00
Dried/smoked fish	4.31	0.43	9.9%	0.03
Cooking oil/fat	2.29	0.03	1.3%	0.92
Sugar	1.85	0.09	4.6%	0.39
Salt	1.05	0.01	1.2%	0.67
Tea	0.13	0.01	4.4%	0.75
Alcohol	1.85	-0.10	-5.4%	0.67
Food prepared outside of home	0.56	-0.02	-3.3%	0.92
Recurring consumption	3.98	0.04	1.0%	0.95
Washing soap	1.98	0.03	1.3%	0.75
Match boxes	0.14	0.00	-0.6%	1.00
Personal care/cosmetics	1.77	0.02	0.9%	1.00

Infrequent consumption	18.91	0.17	0.9%	1.00
Men's clothing/footwear	0.91	-0.01	-1.0%	1.00
Women's clothing/footwear	1.00	0.03	2.7%	0.69
Children's clothing/footwear	1.10	0.03	2.4%	0.75
Educational expenses (school fees, uniforms, books, supplies)	5.58	0.41	7.3%	0.35
Funerals/other social functions	2.47	0.05	1.9%	0.99
Health expenses	6.17	-0.08	-1.3%	0.92

Table F3: Treatment effects on asset items

Item	Kenya				Uganda			
	Avg in Control	ITT	% Incr	q-val	Avg in Control	ITT	% Incr	q-val
TOTAL ASSETS[61]	718.25	60.94	8.5%	0.00	687.68	15.60	2.3%	0.43
Livestock assets	288.34	-0.03	-0.0%	0.48	338.02	29.34	8.7%	0.02
Durable assets	290.15	21.48	7.4%	0.01	161.31	-6.62	-4.1%	0.24
Home improvements								
Construction of new home	N/A[62]				N/A			
New room	5.32	0.47	8.8%	0.25	1.01	0.23	23.0%	0.07
New roof	1.70	0.49	28.5%	0.02	5.47	0.96	17.6%	0.00
Wall	N/A				0.30	-0.03	-11.0%	0.43
Latrine	N/A				0.20	0.01	7.1%	0.73
Plumbing	N/A				N/A			
Wiring	N/A				N/A			
Repair	N/A				0.63	-0.03	-4.3%	0.73
Floor	N/A				0.03	0.01	27.7%	0.33
Plaster	N/A				N/A			
Other	N/A				N/A			
New home purchase								
New home purchase	42.15	6.23	14.8%	0.03	38.53	-3.48	-9.0%	0.33
Household assets								
Mattresses/beds	57.52	3.18	5.5%	0.05	29.35	2.26	7.7%	0.04
Large electronics (TVs, radios)	22.58	2.03	9.0%	0.09	2.27	0.03	1.2%	0.73
Tables	17.69	0.28	1.6%	0.31	5.10	0.12	2.4%	0.58
Chairs	51.16	2.93	5.7%	0.05	6.76	0.25	3.6%	0.28
Cabinets (cupboards)	17.19	2.06	12.0%	0.01	N/A			
Mobile phones	22.15	0.66	3.0%	0.26	9.28	0.12	1.3%	0.73
Solar panels	15.73	-0.23	-1.5%	0.35	5.37	0.14	2.7%	0.67
Bicycles	5.71	0.34	5.9%	0.14	19.10	-0.07	-0.4%	0.77
Bodas/Motorcycles	29.06	1.20	4.1%	0.31	29.54	-4.94	-16.7%	0.04

[61] The ITTs of the asset items do not sum to the ITT of total assets since, as with consumption, we winsorized the individual components of assets for this appendix but winsorized the aggregate asset index for the top-level estimates.
[62] Some components have insufficient data for estimation of treatment effects. This occurs when fewer than 2.5% of households own that item, and after winsorization there are no positive values remaining for that item.

Pigs	1.04	0.16	14.9%	0.15	15.46	0.74	4.8%	0.42
Chickens	18.21	1.83	10.0%	0.00	16.16	1.67	10.4%	0.00
Goats	8.58	1.52	17.7%	0.02	29.20	3.51	12.0%	0.00
Sheep	8.95	-0.48	-5.4%	0.27	10.70	1.52	14.2%	0.03
Cattle	238.55	-3.36	-1.4%	0.31	254.97	22.48	8.8%	0.04
Agricultural inputs	1.02	0.33	32.7%	0.00	6.97	0.86	12.3%	0.18
Other	19.35	0.91	4.7%	0.14	25.38	1.98	7.8%	0.00
Business assets (share owned by HH)								
Buildings	1.11	0.13	12.0%	0.25	0.00	0.00		
Vehicles	4.73	1.08	22.8%	0.12	1.31	-0.14	-10.7%	0.43
Machines	1.77	0.57	32.4%	0.01	0.47	0.05	9.6%	0.43
Farming tools	0.24	0.12	51.4%	0.00	2.46	-0.35	-14.4%	0.04
Tools for carrying materials	1.47	0.59	40.2%	0.00	2.69	0.02	0.8%	0.77
Furniture	0.43	0.28	65.1%	0.00	0.84	-0.05	-5.7%	0.67
Chickens	N/A				0.17	0.00	-0.3%	0.83
Sheep & goats	N/A				N/A			
Pigs	N/A				N/A			
Stock	19.54	9.41	48.2%	0.00	23.93	0.69	2.9%	0.73
Other	0.12	0.12	93.7%	0.00	1.91	0.65	34.3%	0.03
Household savings								
At home	0.91	0.50	54.2%	0.00	2.06	-0.58	-28.1%	0.01
With relative	N/A				N/A			
With neighbor/friend	0.05	0.00	2.1%	0.40	N/A			
With shopkeeper	N/A				N/A			
With SACOs, ROSCAs, other savings groups	16.94	4.53	26.7%	0.00	15.67	4.91	31.3%	0.00
With NGO	N/A				0.02	0.19	826.3%	0.00
In bank account	1.41	0.06	4.2%	0.35	N/A			
With M-PESA/mobile money	3.95	1.24	31.4%	0.00	N/A			
With MFIs	N/A				N/A			
Other source	N/A				N/A			
Cash	1.56	0.80	51.3%	0.00	2.38	0.42	17.6%	0.01
Business savings (share owned by HH)								
At home	N/A				N/A			
At the business	N/A				N/A			
With relative	N/A				N/A			
With neighbor/friend	N/A				N/A			
With shopkeeper	N/A				N/A			
With SACOs, ROSCAs, other savings groups	N/A				N/A			
With NGO	N/A				N/A			
In bank account	N/A				N/A			
With M-PESA/mobile money	N/A				N/A			

With MFIs	N/A				N/A			
Other source	N/A				N/A			
Household loans (amount remaining)								
From friend/neighbor/relative	3.10	-0.34	-11.0%	0.08	2.99	-0.09	-3.0%	0.73
From trader/shopkeeper	7.96	1.39	17.4%	0.14	0.01	0.00	20.3%	0.21
From work place	N/A				N/A			
From savings group	5.30	0.92	17.4%	0.07	14.10	-0.18	-1.3%	0.73
From moneylender	N/A				N/A			
From mobile banking	1.45	0.20	13.6%	0.09	N/A			
From NGO	16.93	0.79	4.7%	0.32	N/A			
From MFI	N/A				N/A			
From private bank	N/A				N/A			
From government bank	N/A				N/A			
From other source	8.36	0.71	8.5%	0.26	N/A			
Business loans (amount remaining owed by HH)								
From friend/neighbor/relative	N/A				0.19	-0.04	-23.3%	0.06
From trader/shopkeeper	0.13	0.00	2.1%	0.39	N/A			
From work place	N/A				N/A			
From savings group	0.09	0.07	73.7%	0.00	2.24	-0.01	-0.4%	0.83
From moneylender	N/A				N/A			
From mobile banking	N/A				N/A			
From NGO	N/A				N/A			
From MFI	N/A				N/A			
From private bank	N/A				N/A			
From government bank	N/A				N/A			
From other source	N/A				N/A			

Appendix G: Differences in treatment effects on types of assets for households that received larger versus smaller grants

Households in Kenya that received large grants ($150/household) had significantly larger treatment effects on net asset value than households that received smaller grants ($50/household). In Table G1 we show how treatment effects on the individual components of net assets differ across households that received small versus large grants.

Larger treatment effects for households that received large grants are driven primarily by new home purchases, household assets (especially large electronics, furniture, chickens, and goats), savings with community savings groups, and business stock. Households that received large grants also had significantly larger treatment effects for business assets and savings, though the magnitudes are smaller. The only asset type where small-grant households had significantly larger treatment effects than large-grant households was construction of new roofs.

Table G1: Treatment effects on asset items, small-grant versus large-grant households, Kenya only[63]

Item	Small grants				Large grants			
	Avg in Control	ITT	% Increase	q-val	Avg in Control	ITT	% Increase	q-val
Home improvements								
Construction of new home	N/A				N/A			
New room	4.61	1.38	29.9%	0.26	5.68	-0.04	-0.7%	0.59
New roof	1.45	1.11	76.3%	0.01	1.83	0.18	9.8%	0.37
Wall	N/A				N/A			
Latrine	N/A				N/A			
Plumbing	N/A				N/A			
Wiring	N/A				N/A			
Repair	N/A				N/A			
Floor	N/A				N/A			
Plaster	N/A				N/A			
Other	N/A				N/A			
New home purchase								
New home purchase	43.27	1.43	3.3%	0.61	41.58	9.38	22.6%	0.01
Household assets								
Mattresses/beds	56.45	3.66	6.5%	0.38	58.06	3.26	5.6%	0.11
Large electronics (TVs, radios)	22.70	-3.15	-13.9%	0.35	22.53	4.63	20.5%	0.00
Tables	17.97	0.33	1.8%	0.60	17.54	0.32	1.8%	0.44
Chairs	50.30	1.73	3.4%	0.59	51.60	3.79	7.4%	0.05
Cabinets (cupboards)	16.95	1.87	11.0%	0.35	17.32	2.47	14.2%	0.02
Mobile phones	21.68	-0.05	-0.2%	0.77	22.39	1.13	5.0%	0.23
Solar panels	14.16	-0.89	-6.3%	0.60	16.54	0.19	1.2%	0.57

[63] See Appendix F for why the ITTs of individual components do not sum to the ITTs of net assets, and also for how to interpret q-values.

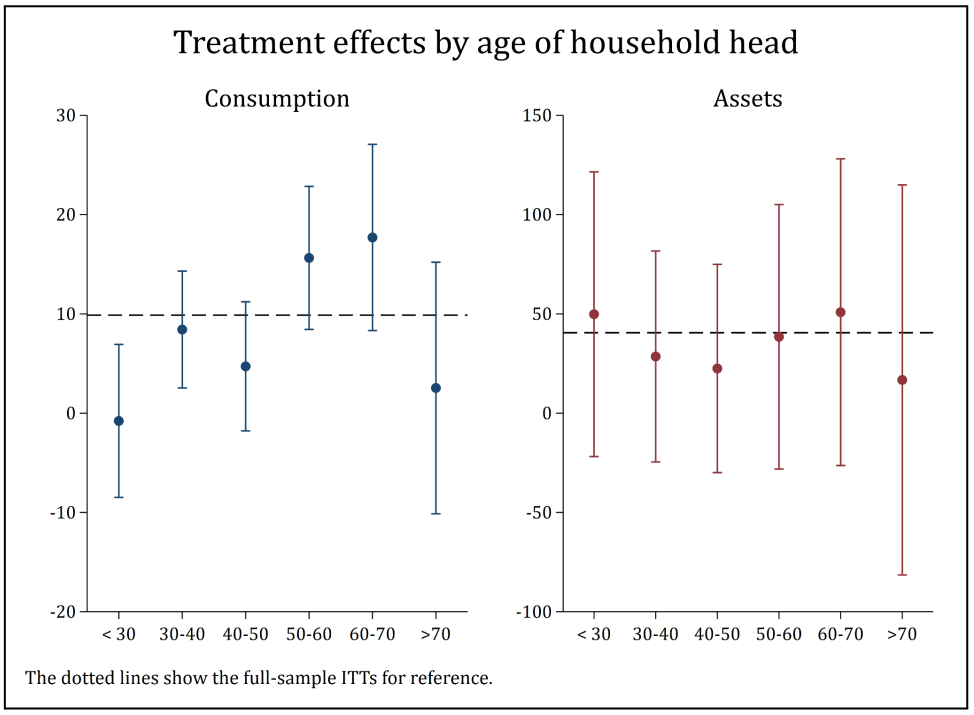
Bicycles	5.13	0.58	11.3%	0.35	6.00	0.22	3.6%	0.36
Bodas/Motorcycles	27.72	4.53	16.3%	0.40	29.75	-0.41	-1.4%	0.57
Pigs	1.72	-0.02	-0.9%	0.77	0.69	0.24	34.5%	0.09
Chickens	17.58	1.62	9.2%	0.21	18.54	1.93	10.4%	0.01
Goats	9.20	-0.07	-0.7%	0.77	8.25	2.46	29.8%	0.00
Sheep	9.71	-1.95	-20.1%	0.35	8.56	0.30	3.5%	0.47
Cattle	263.34	-15.09	-5.7%	0.40	225.84	3.42	1.5%	0.46
Agricultural inputs	0.83	0.48	57.3%	0.04	1.11	0.30	26.7%	0.01
Other	20.24	0.71	3.5%	0.60	18.90	1.10	5.8%	0.12
Business assets (share owned by HH)								
Buildings	0.98	0.08	7.7%	0.60	1.18	0.18	15.6%	0.25
Vehicles	4.37	0.75	17.1%	0.60	4.92	1.36	27.8%	0.12
Machines	1.62	0.20	12.6%	0.60	1.84	0.80	43.6%	0.00
Farming tools	0.26	0.06	22.7%	0.47	0.22	0.16	69.8%	0.00
Tools for carrying materials	1.45	0.39	27.1%	0.09	1.48	0.71	47.9%	0.00
Furniture	0.42	0.14	33.7%	0.33	0.44	0.36	81.9%	0.00
Chickens	N/A				N/A			
Sheep & goats	N/A				N/A			
Pigs	N/A				N/A			
Stock	16.16	8.39	51.9%	0.02	21.27	10.23	48.1%	0.00
Other	0.13	0.08	57.3%	0.26	0.12	0.13	113.6%	0.00
Household savings								
At home	1.00	0.31	30.8%	0.38	0.87	0.59	68.3%	0.00
With relative+A6A61:A68	N/A				N/A			
With neighbor/friend	0.06	-0.02	-28.7%	0.52	0.04	0.01	21.4%	0.24
With shopkeeper	N/A				N/A			
With SACOs, ROSCAs, other savings groups	17.58	2.04	11.6%	0.49	16.62	5.79	34.8%	0.00
With NGO	N/A				N/A			
In bank account	1.41	-0.38	-27.1%	0.40	1.40	0.29	20.4%	0.23
With M-PESA/mobile money	3.79	-0.07	-1.8%	0.77	4.04	1.93	47.7%	0.00
With MFIs	N/A				N/A			
Other source	N/A				N/A			
Cash	1.61	0.27	16.6%	0.20	1.54	1.09	71.0%	0.00
Business savings (share owned by HH)								
At home	N/A				N/A			
At the business	N/A				N/A			
With relative+A6A61:A68	N/A				N/A			
With neighbor/friend	N/A				N/A			
With shopkeeper	N/A				N/A			
With SACOs, ROSCAs, other savings groups	N/A				N/A			
With NGO	N/A				N/A			
In bank account	N/A				N/A			
With M-PESA/mobile money	N/A				N/A			

With MFIs	N/A				N/A			
Other source	N/A				N/A			
Household loans (amount remaining)								
From friend/neighbor/relative	2.98	-0.38	-12.8%	0.40	3.16	-0.31	-9.9%	0.20
From trader/shopkeeper	7.84	4.24	54.0%	0.25	8.02	-0.14	-1.7%	0.57
From work place	N/A				N/A			
From savings group	4.64	0.74	15.9%	0.55	5.65	0.94	16.6%	0.13
From moneylender	N/A				N/A			
From mobile banking	1.15	0.55	48.1%	0.09	1.60	0.02	1.4%	0.57
From NGO	18.61	-1.87	-10.1%	0.60	16.07	2.28	14.2%	0.23
From MFI	N/A				N/A			
From private bank	N/A				N/A			
From government bank	N/A				N/A			
From other source	7.47	1.13	15.1%	0.59	8.82	0.57	6.4%	0.44
Business loans (amount remaining owed by HH)								
From friend/neighbor/relative	N/A				N/A			
From trader/shopkeeper	0.14	-0.02	-11.1%	0.60	0.12	0.01	8.1%	0.44
From work place	N/A				N/A			
From savings group	0.08	0.03	38.2%	0.40	0.09	0.08	90.4%	0.00
From moneylender	N/A				N/A			
From mobile banking	N/A				N/A			
From NGO	N/A				N/A			
From MFI	N/A				N/A			
From private bank	N/A				N/A			
From government bank	N/A				N/A			
From other source	N/A				N/A			

Appendix H: Treatment effects by age of household head

In Figure H1 we show treatment effects disaggregated by the age of the head of household.

Figure H1: Treatment effects by age of household head



Appendix I: Average outcomes by business type, country, and cash transfer size

Figure I1: Average outcomes by business type & country

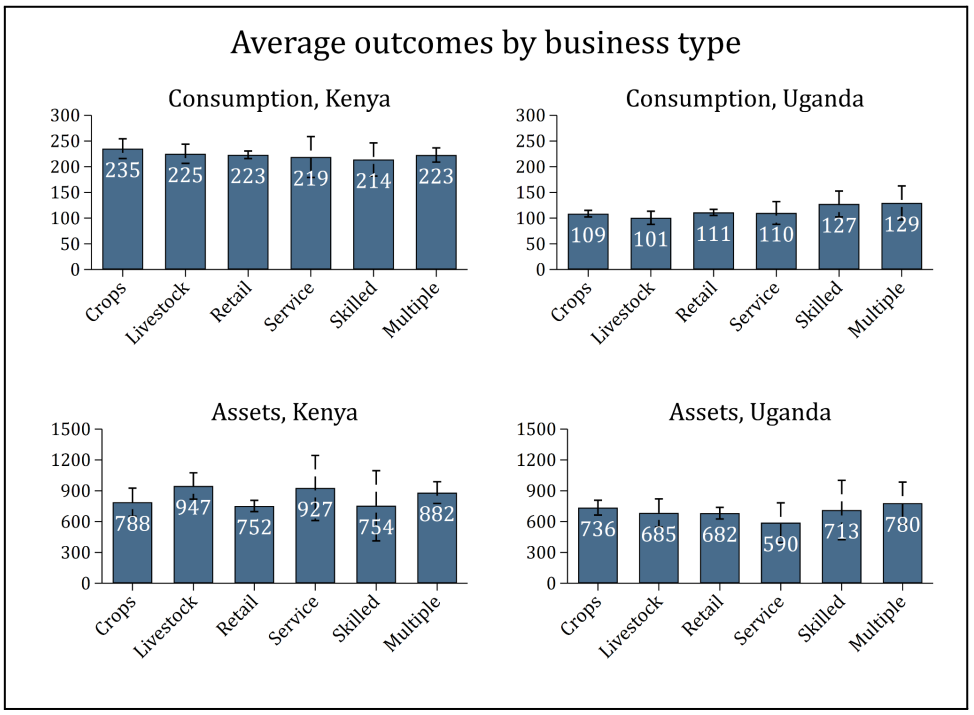
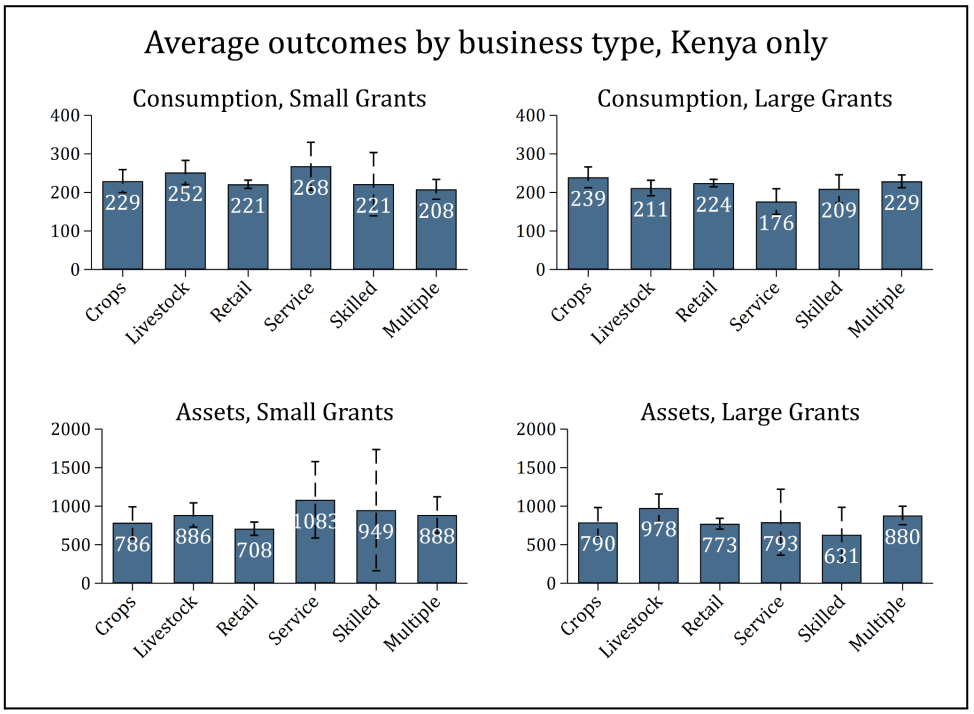


Figure I2: Average outcomes by business type & cash transfer size (Kenya only)



Appendix J: Household groupings

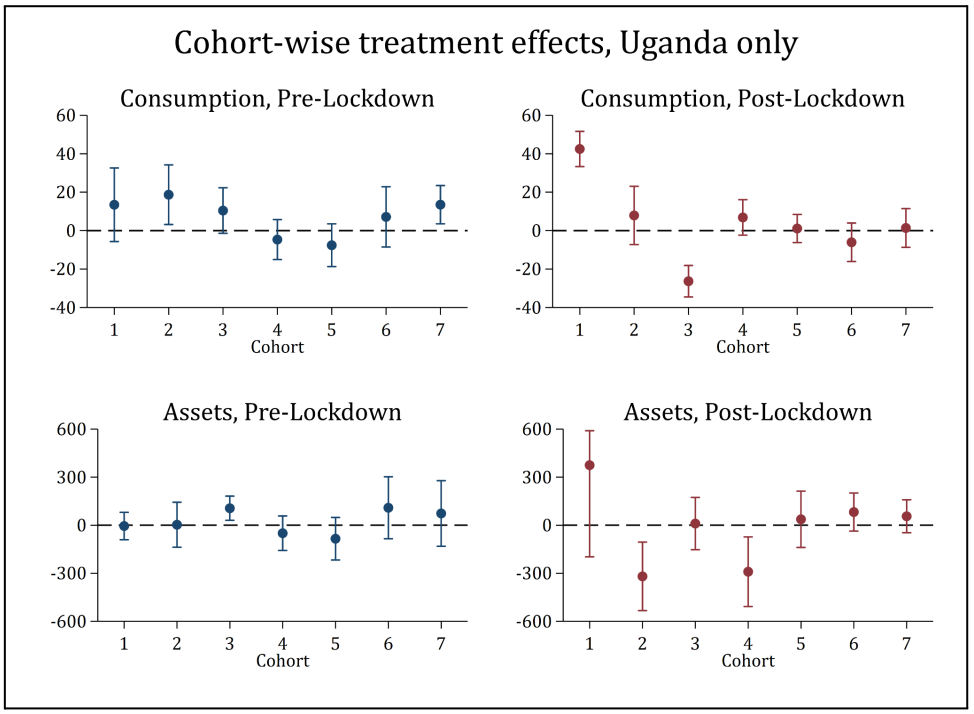
Table J1: Household groupings

# HHs offered the program (ITT population)	14,772
# HHs from original 60	14,311
# HHs from alternates	461
# HHs received grant 1 (TOT population)	14,130[64]
# HHs from original 60	13,710
# HHs from alternates	420
# HHs received grant 2	13,839
# HHs from original 60	13,438
# HHs from alternates	401

[64] 16 households in the grant transfer dataset do not have a HH rank in the baseline data, so we include them in the count of the original 60.

Appendix K: Cohort-wise effect sizes in Uganda for households surveyed pre-lockdown vs post-lockdown

Table K1: Cohort-wise effect sizes, Uganda only



Appendix L: Treatment effects in this study versus Sedlmayr et al (2020)

Table L1: Comparison of this evaluation's vs Sedlmayr et al study outcomes

	This study, full sample		This study, Uganda only		Sedlmayr et al (2020)		
	USD per HH[65]	SD	USD per HH	SD	UGX per capita	USD per HH[66]	SD
Consumption (per month)	9.88 (1.70)	0.09 (0.02)	3.87 (1.84)	0.04 (0.02)	26,061 (11,248)	3.94 (1.70)	0.07
Net assets	40.49 (13.64)	0.05 (0.02)	15.60 (19.50)	0.02 (0.02)	16,343 (5,449)	29.67 (9.89)	0.12

Standard errors in parentheses

[65] All USD values are nominal

[66] We convert the per capita values reported in Sedlmayr et al (2020) to per household values. In Sedlmayr et al (2020), the preferred treatment effect estimates for the Village Enterprise microenterprise program are 26,061 UGX per capita per year for consumption and 16,343 UGX per capita for assets. We use the 2017 nominal exchange rate of 1 USD to 3,580 UGX and average household size of 6.5 members (as observed in our Uganda sample) to make this conversion.

Appendix M: Evaluation team

Frida Njogu-Ndongwe - East & Southern Africa Regional Director

Frida is the East & Southern Africa Regional Director, based in Nairobi. Frida is a medical doctor, public health specialist, entrepreneur, and development and management consultant. Prior to IDinsight, Frida was the Director of Programs at the Center for Health Solutions – Kenya, a consultant at McKinsey & Company's Africa Delivery Hub, and CEO and co-founder of Afyakit Technologies, a Kenya-based start-up that provides data and evidence in health. She has also served as a consultant for governments, NGOs, civil society and multilateral agencies across a multitude of sectors including health, human rights, gender, housing, youth employment, city design, and other areas.

Frida holds a Master of Public Health degree from the University of Auckland and a Bachelor of Medicine and Bachelor of Surgery (MBChB) degree from the University of Nairobi.

Jeff McManus - Senior Economist

Jeff McManus is a Senior Economist on the Technical Team at IDinsight, based in Lusaka, Zambia. Jeff oversees the technical design and analysis of impact evaluations, process evaluations, and machine learning applications at IDinsight. Jeff co-founded the Technical Team at IDinsight and served as its Director for 3 years, during which he set the organizational strategy for the technical rigor of the company's portfolio. He continues to mentor and manage evaluation specialists on the Technical Team. Jeff holds a bachelor's degree in Economics from Swarthmore College and an MPA in International Development (MPA/ID) from Harvard Kennedy School.

Christy Lazicky - Consultant Economist

Christy Lazicky is a former Manager at IDinsight. Christy brings more than five years experience in the international development impact evaluation field. Prior to joining IDinsight, she worked as a Research Manager at London Business School leading the operationalization and implementation strategy of three impact evaluations in Uganda, Rwanda, and Ghana. She has also worked with MIT's Abdul Latif Jameel Poverty Action Lab (J-PAL) in South Africa and Mathematica Policy Research in Washington, DC. Christy holds a MPA in International Development (MPA/ID) from Harvard Kennedy School of Government and a bachelor' s degree in Mathematics from Dartmouth College.

Miguel Angel Jimenez Gallardo - Manager

Miguel is a manager at IDinsight. Before joining IDinsight, Miguel was Country Economist at the International Growth Centre in Mozambique. Before that, he worked in the Mexican Government and for various international organisations and NGOs, including the World Bank, and Innovations for Poverty Action.

Miguel Angel holds a Master in International Economics and a Master in Development Studies from the Graduate Institute of International and Development Studies (IHEID).

Winfred Kananu - Associate

Winfred is an Associate at IDinsight, based in Nairobi, Kenya. She supports projects through research, field team management, and statistical analysis.

Prior to IDinsight, Winfred worked as a Junior Strategy Consultant where she helped clients improve their operational efficiency and impact.

Winfred is an Economist and a Statistician by training.

Appendix N: Evaluation Discussion

This evaluation has been conducted with the support of the following stakeholders:

1) Village Enterprise
2) Wellspring Philanthropic Fund
3) The Foreign, Commonwealth and Development Office (FCDO)
4) United States Agency for International Development - Development Innovation Ventures (USAID-DIV)
5) Global Development Incubator (GDI)
6) Instiglio

This publication has been approved by all the different stakeholders. An initial draft of this publication was subjected to two different review rounds during which the different stakeholders had the opportunity to comment on the findings, recommendations, and lessons learned during the evaluation. This final document incorporates all the feedback that was received from the various stakeholders.

The evaluation design was discussed and agreed upon with all the relevant stakeholders before conducting data collection. Various rounds of discussions were conducted between all stakeholders to agree on the evaluation methodology (Section 2). All differences of opinion were fully acknowledged and the final evaluation design was discussed and approved by all stakeholders.

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