FINDINGS

Microenterprise Program

MISSION To increase incomes of extremely poor residents of rural sub-Saharan Africa.

PROBLEM Extremely poor residents of rural sub-Saharan Africa face multiple, reinforcing barriers to investing, saving, borrowing and acquiring education.

INTERVENTION Village Enterprise delivers a yearlong, four-part program to the extreme poor: cash to start a business, training, mentoring and a savings group.

ENGAGEMENT Entrepreneurs trained.

IMPACT Higher consumption per household; higher assets (wealth) per household.

IMPACT AND COST $1.60 in combined consumption and assets per $1 spent

IMPACT AND COST CALCULATION From the perspective of Village Enterprise — taking account of the costs it incurs to deliver the program — we estimate the benefit/cost ratio at 1.6:1. (This excludes the costs of facilitating a randomized controlled trial.) The ratio rises to 220:1 when only the costs incurred by participating households are considered: $4 per household. Taking account of all costs caused by the program, regardless of who bears them, the benefit/cost ratio is almost identical to the first: 1.6:1. Our calculations assume that consumption gains persist for seven years after the intervention begins, then gradually decline over the next three years. We also assume that any differences in assets (wealth) between participating and non-participating households as a result of the program disappears by the 10-year mark.

CONFIDENCE IN ESTIMATE The estimate of impact is based on a high quality R.C.T., which found modest, statistically significant increases in consumption, assets and income for households in the treatment group relative to the control group. We assume the impacts on consumption last for five years beyond the two directly observed in the R.C.T. Our assumption is based on two medium quality R.C.T.s of similar programs, which observed increasing impact on consumption over a span of seven years. Lacking evidence past the seven-year mark, we assume impacts gradually dwindle to zero by year 10. This audit was updated in October 2018 to reflect the new 5-star scale.
Organizational Effectiveness

GEOGRAPHY    Kenya and Uganda
STAGE        Scale
AGE          Current model in operation for seven years
SIZE         Village Enterprise trained 1,179 entrepreneurs in its 2014 R.C.T. To date, it has trained 156,200 entrepreneurs.

QUALITY OF MONITORING SYSTEMS ★★★★★

Although there is small room for improvement, Village Enterprise’s monitoring systems are, on the whole, very robust, suggesting that it delivers its program consistently and at high quality.

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LEARNING AND ITERATION ★★★★★

Village Enterprise staff, at almost all levels of the organization, proactively and continuously pilot-test new ideas for the design of the microenterprise program and generally make decisions based on the results of those tests. We also commend Village Enterprise for participating in an R.C.T. that tested its flagship microenterprise program against variants and isolated components of the program.

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</tr>
</tbody>
</table>
# Table of Contents

**Findings**  
- Executive Summary  
- Mission  
- Theory of Change  
- Problem  
- Activities  
- Measures of Impact  
- Assumptions  
- Risks  
- Program Details  
- Geography  
- Stage  
- Age and Scale  

**Impact And Cost**  
- Findings  
  - Strategy for Estimating Impact  
  - Calculations  
- Displacement and Other Effects  

**Confidence in Estimate**  

**Duration of Benefits**  
- Comparison to the Research Literature  

**Quality of Monitoring Systems**  
- Rating  
- Activities  
- Targeting  
- Engagement  
- Feedback  
- Outcomes  

**Learning And Iteration**  
- Rating  
- Iterations  
- Data Quality  
- Systematic Change  
- Periodic Change  

**Annex**  
- Nonprofit Information  
- Audit Information  
- Glossary  
- References  

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EXECUTIVE SUMMARY

This impact audit is a re-issuance of an audit of Village Enterprise completed in 2016. ImpactMatters is re-issuing the audit in light of new evidence on the Village Enterprise microenterprise program. In the present audit, we update our findings on Impact and Cost and Confidence in Estimate. Our analysis of Quality of Monitoring Systems and Learning and Iteration remains unchanged from the 2016 audit.

Program Description and Key Findings

Village Enterprise aims to increase income for extremely poor residents of rural sub-Saharan Africa (starting with Kenya and Uganda). Its yearlong microenterprise program aims to counter multiple, reinforcing barriers that they face, including barriers to investment, savings, borrowing and access to education.

The microenterprise program consists of the following components:

- **Identify participants** by consulting community members and conducting surveys.
- **Raise seed capital** of $114\(^1\) for each entrepreneur, paid in two installments: the first serves as start-up capital to launch a business; the second is conditional on a successful launch.
- **Teach entrepreneurs tools of business management and personal finance** in weekly sessions delivered over four months by a professional business mentor employed by Village Enterprise.
- **Form “business savings groups,”** whose members save together and lend to one another.

\(^1\) 120,000 Ugandan shillings converted to U.S. dollars at purchasing power parity (P.P.P.). We use the P.P.P. conversion factor for the weighted average grant transfer date (1,056 Ugandan shillings per 1 U.S. dollar), following Sedlmayr et al. The grant transfers occurred in 2014 and the average transfer date, which accounts for the amount of each installment, was August 2014.
• **Conduct six monthly visits by business mentors**, who advise entrepreneurs how to manage and grow their businesses.

This impact audit reviews the microenterprise program implemented in Uganda from 2014 to 2015 and studied in a randomized controlled trial (R.C.T.). Over that period, a total of 1,179 entrepreneurs participated.

ImpactMatters estimates the impact of the program as the combined increase in the consumption expenditures and assets of participants’ households; the increases are calculated net of counterfactual effects (any changes in consumption and assets that participants’ households would have achieved in the absence of Village Enterprise’s intervention). Consumption expenditure is defined as the value of goods and services purchased.\(^{ii}\) Assets refers to the combined value of livestock, durable assets and cash net of all debts. An increase in consumption and assets indicates achievement of Village Enterprise’s mission: poverty reduction.

It is important to note that the microenterprise program raises consumption and assets for the household representative who participates in the program (the “participant” or “entrepreneur”). Since the participant is a member of her household and contributes to the household’s finances, any benefits accruing to the participant are equivalent to benefits accruing to the household. In the same vein, costs to the participant from engaging in the program are also the household’s costs.

By our calculations, households increased their consumption and assets by a modest $900 over 10 years, net of counterfactual effects.\(^{iii}\) From the perspective of Village Enterprise, therefore taking account of only its costs to deliver the program (and therefore omitting costs to facilitate a randomized controlled trial), we estimate that the program raised the total of consumption and assets by $1.60 per household for every $1 that Village Enterprise spent per household (a benefit/cost ratio of 1.6:1). The benefit/cost ratio remains 1.6:1 if we count both Village Enterprise’s costs and households’ costs incurred from participating in the program. Finally, from the perspective of the households (counting only the costs they incur to participate in the program, but not counting costs incurred by Village Enterprise), the ratio rises to 220:1.

We deem the quality of the evidence upon which we base our estimates of impact to be high. The evidence comes from: (1) an R.C.T. of the Village Enterprise program conducted

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\(^{ii}\) Specifically, Sedlmayr et al. define consumption to include food and beverage consumption, recurring consumption and infrequent consumption.

\(^{iii}\) All impacts and costs are presented in 2016 U.S. dollars, converted from Ugandan shillings at purchasing power parity (P.P.P.) and adjusted for inflation.
by Sedlmayr et al.; and (2) two R.C.T.s of similar “graduation” programs conducted by Banerjee et al. and Bandiera et al., on which we base our assumption about future impacts. (Graduation programs, like the Village Enterprise microenterprise program, aim to “graduate” participants out of extreme poverty by providing an in-kind or cash grant (an “asset transfer”), training and mentorship.)

Impact and Cost

BENEFIT/COST RATIOS

We calculate three benefit/cost ratios: one from the perspective of Village Enterprise, one from the perspective of participating households and a third from a societal perspective (counting all costs caused by the program, regardless of who bears them). The three ratios share the same numerator: a $900 increase in consumption expenditure and assets per household over 10 years.

From the perspective of Village Enterprise, the denominator of the benefit/cost ratio is Village Enterprise’s cost of delivering the program, $570 per household. The resulting benefit/cost ratio is 1.6:1, meaning that every $1 that Village Enterprise spent per participating household yielded a $1.60 increase in consumption and assets for the average household, net of counterfactual effects. Below, we discuss why the 1.6:1 ratio might overestimate the true impact of Village Enterprise.

From the perspective of participating households, the denominator of the benefit/cost ratio is the average household’s cost of participating in the program: just $4, by our estimates. We assume that their only major cost is the time they give up to participate. The resulting benefit/cost ratio is 220:1. The total of lifelong consumption and assets increases by $220 for every $1 a household spends to participate.

From the perspective of society as a whole, the benefit/cost ratio takes account of all costs caused by the program, no matter who incurs them. Societal costs include those borne by Village Enterprise, households and — though not applicable here — government. Because households incur very little cost to participate, the third benefit/cost ratio is nearly identical to the first: 1.6:1.

We calculate total impact based on Sedlmayr et al.’s recent R.C.T. of the Village Enterprise program, which estimated a small, statistically significant increase in the consumption expenditure of each household over the last year: about 155,000 Ugandan shillings ($140)
above counterfactual values.\textsuperscript{iv} That figure is equivalent to about 4 percent of the average household’s consumption over the year prior to the intervention ("baseline consumption"). Meanwhile, by the end of the intervention, the value of each household’s assets was 97,000 Ugandan shillings ($90) higher than counterfactual values.\textsuperscript{v} That figure is equivalent to about 17 percent of the value of the average household’s assets before the intervention ("baseline assets").

However, the R.C.T. ceased to collect data on impacts after two years. We therefore consult Banerjee et al.’s and Bandiera et al.’s R.C.T.s of similar graduation programs in India and Bangladesh, respectively, both of which tracked participants and non-participants for seven years. In both studies, there was no evidence of the impact fading out even at the end of the seventh year: the difference in the consumption of participants and non-participants was sustained or widened over time; the difference in asset stock was sustained. Based on these studies, we assume that the impact of Village Enterprise on consumption is sustained over seven years. We then assume that impact on consumption fades out in equal amounts each year between year seven and year 10. By year 10, we assume impact falls to zero.

In addition, we make the working assumption that any initial boost in assets will disappear by year 10. The interventions studied by Banerjee et al. and Bandiera et al. provided participants with physical assets (mostly livestock) rather than cash. We cannot be certain that Village Enterprise participants did not spend down their cash grants over the course of 10 years. Nor can we be sure that whatever assets they bought with the cash will have any residual value by year 10. Therefore, in our calculation, total impact simply equals the difference in consumption expenditure between participating households and non-participating households, estimated each year and summed up over 10 years. In other words, from increases in consumption over a 10-year period, we subtract the increase in consumption that we estimate households would have achieved even had the Village Enterprise intervention not existed.

We estimate that the total impact of Village Enterprise is $900 per household, accrued over the 10-year period. This might be an overestimate if its program underperforms compared to the Banerjee et al. and Bandiera et al. results. Indeed, the short-term gains in consumption and assets measured by Sedlmayr et al. are smaller than those found by

\textsuperscript{iv} We calculate this figure by multiplying the “per capita” gains in consumption presented in Sedlmayr et al. (26,061 Ugandan shillings) by the average household size (5.96). This is the pooled (average) result of two surveys: one at the end of the yearlong program and the second about a year after the first.

\textsuperscript{v} Again, we calculate this figure by multiplying the “per capita” gains in assets (16,343 Ugandan shillings) by the average household size (5.96). This is also the pooled result of the two surveys.
Banerjee et al. and Bandiera et al. — perhaps an indication that the impact of Village Enterprise might not last as long as that of the two other programs. But, as we explain above, to lower the risk of exaggerating Village Enterprise’s impact, we assume consumption gains are constant (rather than increasing) over the seven-year period and that both consumption and asset gains disappear by year 10.

DISPLACEMENT AND OTHER EFFECTS

Any program that helps participants start new businesses runs the risk of displacing other, existing businesses. This might be of concern to Village Enterprise and its funders, focused as they are on reducing poverty, if the displaced are just as poor or poorer than participants. However, we think this is unlikely because Village Enterprise has a meticulous process for ensuring that only the poorest households are offered the program. Their competitors, we expect, are generally richer. Moreover, the R.C.T. by Bandiera et al. referenced above concluded that richer women are not displaced by the ultra-poor women who newly enter the livestock-rearing business as a result of their participation in the Graduation program.

Confidence in Estimate ★★★★★

The impact of Village Enterprise has been validated by an R.C.T., the highest quality evidence in the social sciences. The R.C.T. by Sedlmayr et al. indicates that the Village Enterprise program increased consumption, assets and incomevi among participating households up to two years after the start of the program. The study was carefully designed and executed: It had high survey response rates and an adequate sample of participants and non-participants. Most important, it successfully assigned at random virtually identical households to either receive the program or not, making possible cause-and-effect conclusions about the impact of the program.

The biggest shortcoming of Sedlmayr et al.’s R.C.T. is its short follow-up period. We therefore turn to Banerjee et al.’s and Bandiera et al.’s seven-year R.C.T.s of similar graduation programs in India and Bangladesh, respectively, to inform our assumption about the longevity of impacts. Though Banerjee et al.’s and Bandiera et al.’s studies were of high quality in and of themselves, the interventions they studied differed from Village Enterprise in important ways: the type of asset transfer, population targeted and duration

vi Income is described by Sedlmayr et al. as “total productive cash inflows,” which is the sum of net cash inflows from farming, income from other self-employment and income from paid employment.
of the program. The results of Banerjee et al.’s and Bandiera et al.’s trials are therefore an imperfect guide to the longevity of Village Enterprise’s impact. Nevertheless, they are the best available evidence on the long-term impacts of graduation programs.

Quality of Monitoring Systems ★★★★★★★

Village Enterprise has high quality systems for monitoring five aspects of its program: (1) intervention activities; (2) targeting of the ultra-poor; (3) engagement of participants; (4) feedback from participants, staff and partners; (5) and the outcomes of participants. Although there is small room for improvement, Village Enterprise’s monitoring systems are, on the whole, very robust, suggesting that it delivers its program consistently and at high quality.

Most notably, Village Enterprise excels at gathering data on participants’ engagement, surveying participants both regularly and in random spot-checks. The findings of these surveys help Village Enterprise monitor the conditions necessary for the program to have its intended impact, such as whether participants are able to spend their cash grants on their businesses and run their businesses without interference from unsupportive family members.

Learning and Iteration ★★★★★★★

Village Enterprise staff, at almost all levels of the organization, proactively and continuously pilot-test new ideas for the design of the microenterprise program. It convenes an annual “innovation summit,” bringing together staff to present the results of their pilot tests. At the end of the summit, all attendees vote on which pilot tests to advance. Village Enterprise has thus developed a system for routinely sourcing ideas for changes to its program from those who know the program and its participants most intimately.

Finally, Sedlmayr et al. and Village Enterprise’s R.C.T. tested not only the flagship microenterprise program, but also several other variants of the program, such as one that included just a cash grant and another that incorporated principles of behavioral psychology. We commend Village Enterprise for experimenting outside its modus operandi and contributing to the evidence base for poverty alleviation programs.
PROGRAM DESCRIPTION

This section summarizes the program’s mission and constructs a theory of change that describes the problem, Village Enterprise’s intervention and ImpactMatters’ chosen measure of impact.

Mission

To increase income for the extreme poor in rural sub-Saharan Africa.

Village Enterprise’s stated mission is to “end extreme poverty in rural Africa through entrepreneurship and innovation.” ImpactMatters measures achievement of this mission as the increase in consumption and assets caused by the program among participants and their family members.

Theory of Change

PROBLEM

Village Enterprise addresses a problem known in economic theory as a poverty trap. Participants face mutually reinforcing barriers to investment, asset accumulation, education and credit. Village Enterprise’s microenterprise program is designed to address specific facets of the poverty trap, providing participants with a cash grant, training, mentorship and mechanisms for saving money.

A poverty trap refers to a situation where poverty is self-reinforcing. Outside of the poverty trap, households can productively invest and raise productivity. But for those in a poverty trap, long-term investments are not viable, and all funds go to subsistence needs. Poverty traps can persist for a myriad of reasons, including missing credit and savings markets, poor information, behavioral constraints that hinder investment and missing insurance markets. We discuss these reasons below.
MISSING CREDIT AND SAVINGS MARKETS

The rural poor in sub-Saharan Africa have limited access to financial services. Bank lending is scarce and expensive. High borrowing costs can result from poor information about creditworthiness, poor access to collateral, and high transaction costs to the lender. Savings are also scarce and expensive. Nonfinancial assets, when they do exist, may be tied up in illiquid forms such as livestock, and poorly suited to use as collateral. The absence of insurance in agriculture across low-income countries is a drag on investment.

In rural Uganda, 16 percent of the population borrowed from a bank in 2014, but 81 percent borrowed money from any source. The most common reasons to borrow money are for school and health care, and 22 percent borrowed money to start, operate or expand a business (including farms). Similarly, in rural Kenya, 16 percent of the population borrowed from a bank in the past year, but 79 percent borrowed money from any source. 24 percent borrowed to start, operate or expand a business.

POOR INFORMATION

A so-called information failure occurs when a market actor has imperfect knowledge. The rural poor systematically lack information about how to productively and efficiently manage their assets. Village Enterprise participants lack information in several domains: business management, financial literacy and knowledge of new productivity-enhancing technologies that would, for instance, enhance crop yields and thereby lead to increased incomes.

BEHAVIORAL BIASES AND MISSING INSURANCE MARKETS

The extreme poor may suffer from low productivity because of lowered expectations for the future and low levels of hope and aspiration, leading to lower investment and income. Some economic research suggests that “temptations” and social obligations cause the poor to fritter away savings or borrow and thus incur interest costs, which makes poverty self-reinforcing. Savings accounts with reminders and commitment devices, at even very low balances, can reverse this. Even the normal distractions of life can cause disproportionate interference with the work of the poor, although there remains little empirical work on inattention and poverty traps.

Furthermore, the extreme poor may be highly risk averse. If they are unable to purchase insurance, they may make cautious investment decisions that minimize the risk of losses rather than maximize productivity. Insurance would encourage higher investment in improved farm inputs, such as seed, fertilizer, feed and veterinary care, by protecting against catastrophic losses.
OTHER FACTORS

There are a number of other factors that may play a role in sustaining the poverty trap faced by Village Enterprise’s clients. These include:

- **Land policy:** Because of land subdivision policies, many ultra-poor individuals in Kenya have very little or no land and cannot start an agricultural business.22
- **Conflict:** Village Enterprises implements in Northern Uganda. The legacy of civil war may have an ongoing impact on clients due to ongoing displacement or instability or low levels of investment in human capital and productive assets.23
- **Poor nutrition:** The extreme poor may not consume enough food to make them productive.24 However, recent research suggests that malnutrition is seldom the primary cause of low productivity in a poverty trap.25
- **Tragedy of the Commons:** Village Enterprise clients that live near nature preserves (the Kisere Forest in Kenya and the Budongo Forest in Uganda) may use limited natural resources in an unsustainable way, leading to a negative impact on livelihoods and the environment.26

ACTIVITIES

Village Enterprise helps the rural poor start microenterprises. The Village Enterprise program is delivered over one year. First, Village Enterprise uses a multi-step process to identify people living in extreme poverty to participate in the program. Participants then begin a four-month sequence of trainings led by business mentors. At the same time, participants form groups of three to start a business together. They also form business savings groups (B.S.G.s) in groups of 30. B.S.G.s allow participants to save together and lend to one another. Village Enterprise then transfers an initial grant of about $230vii in seed capital to each business group. About seven months later, if business groups satisfactorily meet business goals, they receive a second grant of about $120.

TARGETING

Village Enterprise conducts a multi-step process to identify participants who are living in extreme poverty. Village Enterprise starts by selecting poor districts and then working with district officials to identify poor villages within those districts. At the village level, Village Enterprise conducts a participatory wealth-ranking exercise (P.W.R.), during which local opinion leaders define poverty in the local context and rank households from “very

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vii 240,000 Ugandan shillings converted to U.S. dollars at purchasing power parity. We use the conversion factor for 2014, the year when the cash grants were disbursed.
poor” to “rich.” Finally, Village Enterprise uses a standard checklist, the Poverty Probability Index (P.P.I.), to verify that participants are poor. Participants who are identified through the P.W.R. and P.P.I. are then invited to join the program.

SEED CAPITAL

Village Enterprise provides a $340 grant viii to business groups (about $114 per entrepreneur) in two installments: $230 after several training sessions and, contingent on satisfactory achievement of business goals, the remaining $110 after another seven months. Village Enterprise does not provide loans, with the twin rationales that entrepreneurs (1) need to be protected from the risk of default and (2) need time to launch their businesses before cash is available for paying interest.27

TRAINING

With the assistance of business mentors,27 individual entrepreneurs form groups of three, called business groups, to co-own and operate a business. Business groups receive 16 training sessions over a span of four months. The curriculum includes business planning, profit analysis, marketing, savings and bookkeeping.28 Each group must create and submit a business plan to Village Enterprise.

MENTORSHIP

After the initial cash grant is disbursed, entrepreneurs receive monthly visits from business mentors to help them manage and grow their microenterprises.

BUSINESS SAVINGS GROUPS

Entrepreneurs form 30-member B.S.G.s, pooling their savings and making loans to one another, thereby easing the credit constraints on their businesses. Participants are trained in bookkeeping and savings and loans strategies. Each B.S.G. must create a constitution and elect its own executive committee.29 B.S.G.s are the “exit strategy” for Village Enterprise — the means by which the entrepreneurs it trains can continue to access support and capital from fellow entrepreneurs, beyond the duration of Village Enterprise’s intervention.

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viii 120,000 Ugandan shillings converted to U.S. dollars at purchasing power parity (P.P.P.). We use the P.P.P. conversion factor for the weighted average grant transfer date (1,056 Ugandan shillings per 1 U.S. dollar), following Sedlmayr et al. The grant transfers occurred in 2014 and the average transfer date, which accounts for the amount of each installment, was August 2014.
MEASURES OF IMPACT

ImpactMatters estimates the impact of the program as the combined increase in the consumption expenditures and assets of participating entrepreneurs; the increases are calculated net of counterfactual effects (any changes in consumption and assets that the entrepreneurs would have achieved in the absence of Village Enterprise’s intervention). Consumption expenditure is defined as the value of goods and services purchased. Assets refers to the combined value of livestock, durable assets and cash net of all debts. Increases in consumption and assets (above counterfactual changes) reflect achievement of Village Enterprise’s mission: reducing poverty.

ASSUMPTIONS

Theoretically, in order for Village Enterprise’s intervention to maximize effectiveness, a number of assumptions must be met.

1. **Profitable business opportunities**: Village Enterprise participants have profitable business opportunities that can be accessed with a small grant of working capital.

2. **Access to inputs**: Lack of money and business skill are the only barriers to accessing business inputs (such as livestock feed). Entrepreneurs do not, for instance, face discrimination when trying to purchase inputs.

3. **Access to markets and market information**: Markets for selling goods from the small business (typically in agriculture and livestock) are accessible and predictable. Business owners know how and when to sell their goods.

RISKS

A number of risks could potentially undermine the impact of Village Enterprise’s program.

1. **Social pressure**: Female business owners may face social pressure to share ownership of the business and profits with male members of the household or family. The business owners may run the risk of losing control of their assets, whether productive assets (such as livestock and agriculture inputs) or liquid assets (cash). Household members could also make additional demands on the business owners’ time outside of work, which may interfere with their productivity.

2. **Climactic and health factors**: Business owners primarily purchase livestock or agricultural inputs with seed capital provided by Village Enterprise. Climactic
factors, such as drought, or health factors, such as outbreaks of livestock diseases, may reduce or destroy their assets.

3. **Conflict:** Interpersonal conflicts could threaten the survival of business groups and B.S.G.s. In addition, Village Enterprise operates in areas of Uganda that have experienced recent violence that heavily disrupted local economies. There is a risk that violence could resume, jeopardizing the entrepreneurs’ businesses.

4. **Appropriation:** Entrepreneurs are endowed with new wealth through the program in such a way that it is widely known throughout the community. Village Enterprise operates in areas with underdeveloped local legal systems, increasing the risk that new assets could be appropriated. Similarly, local cultural and political factors could prevent the efficient operation of participants’ businesses, even if no direct appropriation occurs.

5. **Local price effects:** Entrepreneurs run the risk of crowding into the same markets. If entrepreneurs produce too many similar goods and services, prices and profits could fall. They are exposed to scarcities of inputs and gluts in output.

### Program Details

**GEOGRAPHY**

Village Enterprise works in Kenya and Uganda, specifically targeting villages with the highest rates of poverty in the following sub-counties or districts:

- **Kenya:** Cherangany; Kiminini; Kwanza; Saboti; and Soy
- **Uganda:** Amuria; Dokolo; Hoima; Katakwi; Kiryandongo; Kitgum; Masindi; Nwoya; and Soroti.

**STAGE**

**Village Enterprise is at the “scale” stage.** Village Enterprise was founded in 1987 and has had a professional C.E.O. since 2010. It has been implementing its flagship microenterprise program and expanding its reach since 2011.

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IX ImpactMatters classifies programs on a continuum from “design” stage to “validation” and “scale.” At the design stage, the program is focused on discovering the right way to implement intervention. Programs at the “validation” stage are focused on testing that the intervention is cost-effective, before expanding access...
AGE AND SCALE

In 2017, Village Enterprise trained 10,600 business owners who opened 3,500 businesses. Since its founding in 1987, Village Enterprise has trained 156,200 owners who opened 39,200 businesses.
IMPACT AND COST

WHY WE ESTIMATE

Impact audits estimate the philanthropic impact and cost of a nonprofit’s programmatic interventions. We base those estimates on best available evidence, however imperfect, drawn from the auditee (internal evidence) and research literature (external evidence). As such, our estimates are the best possible evidence-based gauge of philanthropic success.

HOW WE ESTIMATE

First, we identify outcomes that best capture the auditee’s mission. We then settle upon ways to measure progress against those outcomes, relying on the tools of modern social science.

Second, we report our estimate of “impact,” the change in outcomes that can be attributed to the auditee’s intervention over a designated period of time. We take explicit account of counterfactual success — the change in outcomes that would have occurred without the program. And whenever possible, we take explicit account of third-party effects, especially unintended harm to vulnerable individuals because of the auditee’s intervention. For benefits that accrue over time — for example, the increased earnings from high school graduation — we discount these future benefits (at a 5 percent discount rate). The length of time over which benefits are assumed to accrue is based on the specifics of the intervention under review and available internal and external data.

Third, we report total costs. Total costs include marginal costs (direct costs of delivering the intervention) and fixed costs (for example, administrative overhead) regardless of who bears those costs (nonprofit, public agencies, private funders or participants). For programs that generate commercial revenue, the revenue is treated as a subtraction of costs. For costs that kick in over time, we discount (as we do benefits). The length of time over which costs accrue depends on the specifics of the intervention under review and available internal and external data.
Fourth, we report the ratio of impact to cost (a benefit/cost ratio).

Finally, we analyze key factors — for example, stage of development, whether the nonprofit be in pilot phase or expansion phase — relevant for understanding the audit findings.

Typically, impact is estimated on a single outcome. However, if an auditee’s intervention affects several outcomes, we report impacts on distinct outcomes separately. Concretely, suppose that a program seeks to raise incomes and improve health status. We do not, as yet, attempt to combine the impact on multiple outcomes into a single aggregate outcome — concretely, by combining the value of the income effects and health-status effects. To aggregate, we would need weights — the relative value of outcomes — that would reflect the nonprofit’s or funder’s values (not those of ImpactMatters as auditor).

Findings

ImpactMatters measures the impact of Village Enterprise’s microenterprise program by estimating the increase in consumption expenditures and assets among participating households above any changes in consumption and assets that they would have achieved in the absence of Village Enterprise’s intervention. (In other words, impact is measured by actual increases in consumption and wealth of households above counterfactual increases in consumption and wealth.) Consumption expenditure is defined as the value of goods and services consumed.$^x$ Assets refers to the combined value of livestock, durable assets (such as farm equipment) and “net financial position” (cash net of all debts). Increases in consumption and assets (above counterfactual changes) reflect achievement of Village Enterprise’s mission: reducing poverty.

Note that the microenterprise program raises consumption and assets for the household representative who participates in the program (the “participant” or “entrepreneur”). Since the participant is a member of her household and contributes to the household’s finances, any increases accruing to the participant are equivalent to increases accruing to

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$x$ We follow Sedlmayr et al.’s definition of consumption: the sum of food and beverage consumption, recurring consumption and infrequent consumption.
the household. In the same vein, costs to the participant from engaging in the program are also the household's costs. Village Enterprise prefers reporting at the household level since its intervention aims to improve the well-being of the household as a whole.

ImpactMatters estimates benefit/cost ratios from three different points of view. From the perspective of Village Enterprise, therefore taking account of only those costs borne by Village Enterprise in delivering the program, the benefit/cost ratio is 1.6:1. For every $1 Village Enterprise spent on the program, each household raised their consumption and assets by $1.60.\textsuperscript{xii} Said differently, for each $1 spent on the Village Enterprise program, participating households derived $1.60 in benefits.

By our estimates, each participating household experienced a $900 increase in consumption and assets over a projected 10 years.

From the point of view of households — therefore factoring their costs of participating but not those of Village Enterprise or any other source — consumption and assets rose by an impressive $220 for every $1 they spent on the program. Finally, we estimate a benefit/cost ratio from a societal perspective, counting all costs caused by the program, regardless of who bore them. We find that $1 of total cost caused by the program yields a $1.60 increase in consumption and assets.\textsuperscript{xii}

\textbf{Table 1. Impact and Cost Findings}

<table>
<thead>
<tr>
<th>Specification</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>TOTAL ENTREPRENEURS TRAINED (TOTAL PARTICIPATING HOUSEHOLDS)</td>
<td>1,179</td>
</tr>
<tr>
<td>INCREASE IN CONSUMPTION AND ASSETS PER HOUSEHOLD (OVER 10 YEARS)</td>
<td>$900</td>
</tr>
<tr>
<td>BENEFIT/COST RATIO, COUNTING ONLY COSTS COVERED BY VILLAGE ENTERPRISE</td>
<td>1.6:1</td>
</tr>
<tr>
<td>BENEFIT/COST RATIO, COUNTING ONLY COSTS COVERED BY PARTICIPATING HOUSEHOLDS</td>
<td>220:1</td>
</tr>
<tr>
<td>BENEFIT/COST RATIO, COUNTING ALL COSTS, REGARDLESS OF PAYER</td>
<td>1.6:1</td>
</tr>
</tbody>
</table>

\textsuperscript{xii} All impacts and costs are presented in 2016 U.S. dollars, converted from Ugandan shillings at purchasing power parity (P.P.P.) and adjusted for inflation.

\textsuperscript{xii} In fact, the benefit/cost ratio from the perspective of Village Enterprise alone is 1.57:1 versus a benefit/cost ratio of 1.56:1 from a societal perspective. The difference disappears with rounding.
We base our estimate of impact on a randomized controlled trial (R.C.T.) conducted by Sedlmayr et al. on the Village Enterprise microenterprise program in Eastern and Western Uganda. The researchers estimated impacts on three indicators of poverty: consumption, assets and income.

In their benefit/cost analysis, Sedlmayr et al. used estimates of changes in household consumption and in the value of household asset stock, but not changes in income. We adopt this strategy for measuring the benefits of the Village Enterprise program. Several other R.C.T.s of similar “graduation” programs have also opted to focus on consumption and assets instead of income. The rationale is that, in the settings where Village Enterprise works, self-reported estimates of income are notoriously inaccurate. Consumption and assets, which are two primary uses of income, tend to be more reliably measured. Also, excluding changes in income avoids double counting — changes in income provide for changes in consumption and assets.

The Sedlmayr et al. study of Village Enterprise’s microenterprise program surveyed households at the end of year one (“midline”) and after the second year (“endline”). In theory, we could infer a trend in impact over the two-year period based on the midline and endline results. However, the authors cautioned that their estimates at midline and endline likely contain considerable measurement errors. A more reliable estimate of impact would be the average between the two, called a “pooled estimate,” which is what we use in our model. The pooled estimated impacts of the program on consumption and assets are about 155,000 Ugandan shillings ($140) and 97,000 Ugandan shillings ($90) per household, respectively. These estimates are based on “intent to treat,” which means they reflect the impact of the program for everyone initially selected to participate (even if they chose not to participate or complete the program).

These estimates apply to only the first two years after the start of the program, which were directly observed in the R.C.T. To determine how long we can expect consumption

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xiii Sedlmayr et al. describe income as “total productive cash inflows,” defined as cash earned from farming, income from other self-employment and income from paid employment.

xiv These programs “graduate” participants out of poverty, typically by offering some combination of an in-kind or cash grant, training and mentorship. Village Enterprise’s microenterprise program is considered a graduation program.

xv We calculate these figures by multiplying the “per capita” impacts for consumption and assets presented in Sedlmayr et al. (26,061 Ugandan shillings and 16,343 Ugandan Shillings) by the average household size (5.96).
and assets gains to last thereafter, we turn to recent evaluations of the long-term impact of two different graduation programs.

Banerjee et al. conducted an R.C.T. of a graduation program in rural West Bengal, India, called Targeting the Hard-core Poor (T.H.P.). They found that the difference in consumption expenditure between participants and non-participants generally widened over the seven-year period under study, even as consumption among non-participants also grew over time. The gap in the value of assets of participants and non-participants was sustained over time.

Bandiera et al. also observed positive long-term impacts in their R.C.T. of a graduation program in rural Bangladesh called Building Resources Across Communities (BRAC). Impacts on consumption and assets rose from year two to year four after the start of the intervention, then were sustained at the year-four level in year seven.

Based on Banerjee et al.’s and Bandiera et al.’s findings, we assume that Village Enterprise’s impact on consumption persists for seven years. That is to say, we assume that each participant consumes $140 more each year for seven years (discounted to present value). Whereas Banerjee et al. observed an increasing trend in consumption gains over seven years, we assume consumption gains are constant over that period, which is more consistent with Bandiera et al.’s findings — a more cautious assumption with lower risk of overstating Village Enterprise’s impact.

Lacking evidence that impacts persist beyond year seven, we assume impact on consumption declines thereafter, falling in equal amounts each year until it reaches zero in year 10.

There are two major concerns in adopting the duration of consumption gains from Banerjee et al. and Bandiera et al. First, Village Enterprise, T.H.P. and BRAC intervene in different ways, for different periods of time. Most important, T.H.P. and BRAC give participants “productive” (income-generating) assets, while Village Enterprise gives participants cash. If spent immediately on consumption goods, the cash transfer would boost consumption in the short term, but a productive asset is perhaps more likely to continue to increase consumption in the long term. (That said, Village Enterprise disburses the cash in two rounds and incentivizes participants to invest in productive assets by making the second disbursal conditional on having invested the first in their microenterprise.) Moreover, the value of the assets transferred by T.H.P. and BRAC is

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xvi 15 percent at the 1.5-year mark, 12 percent at the 2.5-year mark and 25 percent at the seven-year mark. All are presented as percentages of average consumption expenditure among non-participants (members of the control group).
about four times as large as the Village Enterprise cash grant. The Village Enterprise program is also shorter: While Village Enterprise’s program lasts one year, T.H.P. and BRAC last 1.5 years and two years, respectively. Second, the operating contexts differ. While all interventions target poor individuals in rural areas, T.H.P. and BRAC operate in India and Bangladesh, respectively — both higher-income countries than Uganda. We discuss these concerns in depth in the Confidence in Estimate section below.

Recall that we define the total impact of the microenterprise program as the sum of consumption and asset gains. But whereas consumption is a “flow variable” (measured over the 10-year period), the value of assets is a “stock variable” (measured at a single point in time). To combine the two, we therefore take the sum of consumption gains over 10 years and the increased value of assets measured at the end of year 10. The increased value of assets at the end of year 10 is equal to any unspent portion of the cash grants Village Enterprise provided to participants plus the residual value of any assets participants may have bought (whether using the cash transfers or additional money gained from participating in the program). Our approach follows Banerjee et al.’s six-country study of graduation programs.32

However, because we do not know how Village Enterprise participants have spent their grants, and by extension, whether the goods they spent their grants on have any value after 10 years, it is difficult to predict the impact of the program on assets at the 10-year mark. In the best-case scenario, participants invested in assets that continue to generate revenue. But it is also possible that, after 10 years, the asset will have depreciated or become worthless (e.g., a sewing machine no longer functions or livestock has been stolen). Alternatively, the participant may have purchased consumption goods, rather than assets, with her cash grant. Because we have no data on how the grants were spent, we make the working assumption that gains in assets disappear by year 10. The total impact of the program simply equals the sum of the increases in consumption each year.

If, contrary to our assumption, part of the initial money transfer is unspent by the end of the program period, then households will be able to convert the saved money into consumption after the end of the program period. Similarly, the residual value of assets bought with the money transfers can be salvaged by consuming the asset (e.g., consuming one’s livestock directly or selling one’s sewing machine on the second-hand market and using the cash for further consumption). Any such after-program increases in consumption will not be captured in our estimate of program benefits.
CALCULATIONS

ImpactMatters measures the impact of Village Enterprise’s microenterprise program as the increase in consumption expenditure and assets among entrepreneurs, above any changes in consumption and assets that would have occurred regardless of Village Enterprise’s intervention.

At the one-year mark, we estimate that each household's consumption expenditure increases by $140; that is, they spend $140 more on goods and services than they otherwise would have spent in the absence of the Village Enterprise program. At the two-year mark, each household's consumption increases by $129 above what they would have spent if they had not in fact participated in the program. (The difference between the two figures arises because the second-year impacts occur further in the future. We discount all impacts that occur in the future by a social discount rate of 5 percent.)

Using the results from Banerjee et al. and Bandiera et al., we assume that the effect of the intervention on consumption expenditure remains constant for up to seven years. We also assume impacts decline thereafter, falling in equal amounts each year until they reach zero in year 10. Further, we assume the initial cash transfer is completely spent, leaving no residual impact on the value of household assets. Summing up impacts each year over the 10-year period yields a total impact on consumption per household of $900. The area under the graph below represents total impact.

Figure 1. Impact of Village Enterprise's Microenterprise Program Over Time

![Graph showing the increase in consumption expenditure over time.](image-url)
Village Enterprise's cost to deliver the program is $570 per household. This figure includes all program delivery, managerial and administrative costs associated specifically with the microenterprise program, as reported by Sedlmayr et al. based on Village Enterprise’s financial reports.\textsuperscript{xvii} Using the cost per household as the denominator ($570) and the impact per household as the numerator ($900), the benefit/cost ratio is 1.6:1, estimated from Village Enterprise's point of view.

We also calculate the benefit/cost ratio from the point of view of the participating households. Although entrepreneurs did not incur any direct costs (they did not pay for any trainings nor make major out-of-pocket investments in their businesses on the advice of the program), they likely incur an opportunity cost of time spent participating in training, mentorship and surveys. For instance, 81 percent of Village Enterprise participants are women. According to interviews with program staff, if these women had not spent their time participating in the program, they would have been doing household work such as cooking, fetching water and caring for children. We therefore assume the Village Enterprise program caused some shift in household work from women to other members of the family, who, in turn, may have had to spend less time on income-generating activities.

We estimate that participants spent around 55 hours in total on program activities. The value of their time is based on the average annual cash earnings of participants as measured by Sedlmayr et al. before the intervention.\textsuperscript{xviii} The total cost of time spent in the program is about $4 per participant, which is synonymous with the cost to the household of having one member participate in the program. With $4 in the denominator and $900 of total impact per household in the numerator, the benefit/cost ratio is very high, at 220:1. Under our assumptions, it is unmistakable that benefits from the program more than offset any costs incurred by participating households.

\textsuperscript{xvii} Village Enterprise delivered several different interventions during the 2014-15 period under analysis, one of which was the microenterprise program. Sedlmayr et al. allocated Village Enterprise's total managerial and administrative costs to the microenterprise program by using the proportion of Village Enterprise's total “field hours” that were spent on delivering the microenterprise program specifically. “Field hours” include any time spent selecting communities for intervention, conducting targeting surveys, delivering cash grants, holding trainings and providing mentorship.

\textsuperscript{xviii} Sedlmayr et al. present this figure as “total productive cash inflows,” defined as cash earned from farming, income from other self-employment and income from paid employment.
The societal benefit/cost ratio, which includes costs to Village Enterprise and to households, is 1.6:1. Said differently, for every $1 spent by any payer on the Village Enterprise program, participating households reap $1.60 in increased consumption.

Displacement and Other Effects

DISPLACEMENT

EFFECT: NONE

Village Enterprise participants — at an advantage because of their free capital, training and mentorship — are poised to launch new businesses. This may drive other businesses out of the market and reduce the incomes of other businesspeople. However, we think it is not likely that other businesses are driven to exit the market completely, as illustrated in the example from Bandiera et al. below. More likely, some sellers operating in the market prior to Village Enterprise’s intervention might have enjoyed a pseudo-monopolistic position, meaning they dominated the market and were able to sell their goods at excessively high prices. Those sellers may lose some customers to the new Village Enterprise entrepreneurs. To the extent that those sellers are relatively wealthier than Village Enterprise entrepreneurs, this reallocation of income from richer to the poorer entrepreneurs should not undermine Village Enterprise’s focus on alleviating poverty.

Bandiera et al. (2016) found that greater numbers of poor women taking up livestock rearing as a result of the BRAC program did not crowd out (displace) richer women who were already in this sector. Their entrance into the sector also did not significantly reduce the price of milk or value of cows. Relatedly, the reallocation out of casual labor actually benefited ineligible households (i.e., those that did not qualify to participate in the program because they were wealthier) who continued to work in casual labor because wages for maids and agricultural laborers increased. Overall, Bandiera et al. find that the benefits to the ultra-poor do not negatively affect other households in the village.

PRICE EFFECTS

EFFECT: AMBIGUOUS

By supporting the creation of new businesses, Village Enterprise may increase market competition, potentially leading to a drop in price (and increase in choice) for the mostly poor consumers in the area.
However, as Village Enterprise participants tend to select income-generating activities that produce goods (as opposed to selling goods), participants may not have a significant economic effect on the price of general consumer goods. In fact, by introducing money into the community, Village Enterprise may actually contribute to temporary, localized inflation. Village Enterprise has anecdotally observed such inflation.

ECONOMIC GROWTH

EFFECT: POSITIVE

Participants' businesses likely increase local economic growth, benefiting the community. Bandiera et al. find evidence to support this.\(^{37}\) In addition, participants form business savings groups (B.S.G.s), in which multiple business groups pool their savings and make loans to each other. Village Enterprise has anecdotally observed that some B.S.G.s eventually open themselves up to include members from the broader community, potentially spurring local economic growth.

DISTRIBUTIONAL EFFECT

EFFECT: POSITIVE

Bandiera et al. also find promising “distributional effects” of the intervention.\(^{32}\) Two years after the completion of the program, the consumption expenditure and the household assets gap between the ultra-poor and the near poor disappeared. Financial savings and productive assets of the ultra-poor actually surpass that of the near-poor by a factor of four and two, respectively. This implies that graduation programs have the potential to greatly improve the relative economic status of the ultra-poor.

COMMUNITY ILL WILL

EFFECT: NEGATIVE

Community members who do not receive the program may feel ill will toward those that do. One technique that may reduce ill will is “participatory wealth ranking”: by creating a transparent selection process for participants and involving community leaders, Village Enterprise may increase acceptance of the program. The program still likely generates some amount of community ill will, but we think the improved living standards for the extreme poor outweighs this negative effect.
CONFIDENCE IN ESTIMATE

Rating

ImpactMatters estimates that, from the perspective of Village Enterprise, its microenterprise program increases the consumption expenditure and assets of participants’ households (i.e. those that have a member who participates in the program) by $1.60 for every $1 that Village Enterprise spent on the program. From the perspective of households, their consumption and assets increase by $220 for every $1 they alone spent because of the program. From the perspective of the society at large, therefore taking all costs into account no matter who bears them (Village Enterprise, households, others), the benefit/cost ratio is again 1.6:1. Below, we conclude that these estimates are supported by high quality evidence.

Our three-star rating of quality takes account two main sources of evidence: (1) the randomized controlled trial (R.C.T.) of the Village Enterprise program conducted by Sedlmayr et al.\textsuperscript{30}; and (2) two R.C.T.s of a similar “graduation” programs conducted by Banerjee et al.\textsuperscript{36} and Bandiera et al.\textsuperscript{32}, on which we base our assumption about future impacts. (Graduation programs aim to “graduate” participants out of extreme poverty by providing an in-kind or cash grant (an “asset transfer”), training and mentorship.)

The R.C.T. by Sedlmayr et al. indicates that the Village Enterprise program had positive impacts on households up to two years after the start of the program.\textsuperscript{30} The study was carefully designed and executed: It had high survey response rates and an adequate sample of participating and non-participating households. Most important, it successfully assigned at random virtually identical households to either receive the program or not, making possible cause-and-effect conclusions about the impact of the program. We therefore borrow Sedlmayr et al.’s estimates of the magnitude of impacts on households’ consumption expenditure and the value of their asset stock, known as the “treatment effect.”
The biggest shortcoming of Sedlmayr et al.’s R.C.T. is its short follow-up period. We therefore turn to the research literature on graduation programs elsewhere to determine the likely duration of the impact of Village Enterprise.

Banerjee et al. and Bandiera et al. each find that impacts increased over the course of seven years after the start of the program. Although both R.C.T.s are high quality studies, the interventions they evaluated differed from the Village Enterprise program in important ways: type of asset transfer, population targeted and duration of the program. The results from both of these seven-year trials are therefore an imperfect guide to the longevity of Village Enterprise’s impact. Nevertheless, the Banerjee et al. and Bandiera et al. studies are the best available evidence on the long-term impacts of graduation programs. In brief, we conclude that the evidence behind Village Enterprise’s duration of impacts is of medium quality, reflecting the limited applicability of the results of these two studies to Village Enterprise’s intervention.

**Figure 2. Quality of Evidence Rating**

<table>
<thead>
<tr>
<th>Source of Evidence (and the assumption it informs)</th>
<th>Quality</th>
</tr>
</thead>
<tbody>
<tr>
<td>SEDLMAYR ET AL., WHICH INFORMS THE TREATMENT EFFECT</td>
<td>HIGH</td>
</tr>
<tr>
<td>BANERJEE ET AL. AND BANDIERA ET AL., WHICH INFORM THE DURATION OF IMPACT</td>
<td>MEDIUM</td>
</tr>
<tr>
<td>OVERALL QUALITY OF EVIDENCE</td>
<td>HIGH</td>
</tr>
</tbody>
</table>

We conclude that the overall quality of evidence behind our final estimates is high. The overall star-rating reflects our confidence in the benefit/cost ratio, which is calculated based on two key variables: (1) the treatment effect; and (2) the duration of impact. We rate separately the quality of the evidence behind each variable. Then, we combine the two ratings. To do so, we weight each variable's rating proportionally to how sensitive our impact estimate is to each variable — in short, we conduct a sensitivity analysis. We rely on statistical methods where available and fall back on informed assumptions where not.

First, we construct a range of plausible values for the first assumption: the treatment effect. The range is informed by the 95 percent confidence interval, the range of values within which the true treatment effect is expected to lie, reported in the Sedlmayr et al.
Next, we define a range of plausible values for the second assumption: the duration of impacts. The minimum duration of impact is two years, the period directly observed in the R.C.T. We calculate the upper bound symmetrically around our base assumption of seven years. That is, since the lower bound is two years (or five years less than the base assumption), the upper bound should be twelve years (five years more than the base assumption).

We then compare the size of the range of plausible values for the two assumptions. We find that the impact estimate is more sensitive to plausible changes in the treatment effect than in the duration of impacts. Accordingly, the quality of evidence behind the treatment effect should have greater bearing on the overall quality rating.

Review

RESULTS OF THE RANDOMIZED CONTROLLED TRIAL ON VILLAGE ENTERPRISE

The Village Enterprise program has been evaluated by independent researchers in an R.C.T. In an R.C.T., individuals from a sample of the population are randomly assigned to treatment and control groups. While the two groups are virtually identical before the start of the intervention (at “baseline”), only the treatment group receives the intervention under review. Any difference in outcomes between the two groups that follow the intervention can be attributed to the program itself since it is the only factor that has changed. Thus, the control group serves as a counterfactual for the treatment group, meaning that the outcomes of the control group represent what would have happened to the treatment group had they not received the intervention.

The Village Enterprise R.C.T. took place in the Western and Eastern regions of Uganda. After identifying these two poor regions, Village Enterprise identified the poorest districts within each region. Villages with at least 70 qualifying households were selected, yielding a total of 69 villages in each of the two regions (a total of 138 villages). To determine household eligibility, Village Enterprise staff performed a “participatory wealth ranking” exercise, in which Village Enterprise staff worked with community leaders in order to rank households.

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xix In fact, the range is not precisely equal to the 95 percent confidence interval reported by Sedlmayr et al. because their results are presented in “per capita” terms (meaning per member of a household that has one member participating in the program), whereas ours are presented per participant (or equivalently, per household).
households in terms of wealth. They then conducted a Poverty Probability Index (P.P.I.) survey to corroborate the likelihood that any given household is living in poverty.\(^{38}\)

The R.C.T. tested four variants and components of Village Enterprise’s flagship microenterprise program: (1) the traditional microenterprise program; (2) the microenterprise program without training sessions on saving and without business savings groups (B.S.G.s); (3) the unconditional cash transfer only; and (4) the unconditional transfer plus behavioral components such as goal-setting. The fifth arm of the R.C.T. was a control group. The first two variants are of interest to this audit, since Sedlmayr et al. analyzed the effects of having received any of the two types of microenterprise program; that is, both treatment groups are combined. This is further explored in the next section.

The 138 eligible villages were assigned to different arms testing the abovementioned variants and components of Village Enterprise’s program. Within each village, households were randomly assigned to treatment and control groups.\(^{xx}\)

Three surveys were conducted to collect data: before the start of the intervention (“baseline”), at the end of the one-year intervention (“midline”) and one more year after that (“endline”). To estimate the impact of the program on households, the researchers performed an “intent-to-treat” analysis. In other words, estimates of impact take into account all households that were invited into the program, including those that participated and those that did not. Intent-to-treat analysis can be useful for funders and nonprofits concerned about the real effect of offering a program to many people, not all of whom can be expected to participate.

Note that the microenterprise program raises consumption and assets for the household representative who participates in the program (the “participant” or “entrepreneur”). However, any benefits that accrue to the participant also accrue to the household because each participant contributes to the household’s finances. Therefore, gains to the participant are synonymous with gains to the household.

The results of the R.C.T. indicate that Village Enterprise’s intervention alleviates poverty. The Village Enterprise microenterprise program increased each household’s consumption (over the last year), value of their asset stock and income (over the last year) by 155,000,

\(^{xx}\) In fact, once the 138 eligible villages were identified, three cohorts of 23 villages each were formed in each region. Cohorts participated in the study in waves. Villages within each cohort were randomly assigned to the different study arms.
97,000 and 80,000 Ugandan shillings, respectively (that is roughly $140, $90 and $70). All three results were statistically significant. Consumption expenditure is defined as the value of goods and services purchased over the last year. Assets refers to the combined value of livestock, durable assets (such as farm equipment) and cash net of all debts. Income refers to money earned over the last year from paid employment, self-employment or farming.

**QUALITY OF THE R.C.T. ON VILLAGE ENTERPRISE**

An R.C.T. is the highest quality of evidence in the social sciences. The Village Enterprise R.C.T., conducted by Sedlmayr et al., not only aimed to evaluate the impact of its usual microenterprise program, but also the impact of variants of the program. On balance, we find that the R.C.T. was carefully designed and well-executed.

The R.C.T. was adequately powered; that is, it had a large enough sample of participants and non-participants to detect the effects of the intervention. However, to estimate the effect of the Village Enterprise microenterprise program, the researchers combined two treatment groups. The first group received the full microenterprise program. The second group received a variant of this program which excluded savings training sessions and eliminated the formation of B.S.G.s, which consist of 30 entrepreneurs who pool their savings and lend them to each other. As such, the aforementioned impact estimates do not represent the true, standalone effects of the original microenterprise program. With that said, Sedlmayr et al. find that the savings group component does not affect consumption or assets. It does, however, encourage non-farm microenterprise activity.

Another strong feature of the R.C.T is high response rates to the two rounds of follow-up surveys (midline and endline). Among the entire sample (all five arms), response rates were 93 and 91 percent for the midline and endline surveys, respectively. For the microenterprise evaluation, they do not find differential attrition between the treatment and control groups. This means that the survey response rates were not statistically

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**Notes:**

- **xxi** We calculate these figures by multiplying the “per capita” gains in consumption, assets and income presented by Sedlmayr et al. (26,061, 16,343 and 13,483 Ugandan shillings, respectively) by the average household size (5.96). This is the pooled (average) result of two surveys: one at the end of the yearlong program and the second about a year after the first.
- **xxii** All impacts in 2016 U.S. dollars, converted from Ugandan shillings at purchasing power parity (P.P.P.) and adjusted for inflation.
- **xxiii** Sedlmayr et al. present income as “total productive cash inflows,” defined as cash earned from farming, income from other self-employment and income from paid employment.
different between the treatment and control groups. We did not identify any other sources of bias.

The greatest weakness of the R.C.T. is its short follow-up period after the intervention, which hampers conclusions about the long-lasting effects of the program. One way to predict future benefits is to establish a trend based on the midline and endline results, spaced one year apart. However, trends established by only two periods of analysis tend to be unreliable predictors of future results. This is further complicated by the fact that estimates at midline and endline likely contain measurement error (hence our preference for the pooled results, which average midline and endline estimates).

**DURATION OF BENEFITS**

The Village Enterprise R.C.T. stopped measuring impacts after two years. To estimate how long the impact of the Village Enterprise program lasts, we consult the external literature on similar programs.

Banerjee et al. conducted an R.C.T. of a similar “graduation” program in West Bengal called Targeting the Hard-core Poor (T.H.P.).

In this study, 991 eligible, ultra-poor households in rural villages in Murshidabad were randomly assigned to either receive the T.H.P. program (the “treatment”) or serve as the control group. Over a seven-year period, treatment and control group members were surveyed three times. The treatment group received a one-time livestock asset transfer and participated in weekly trainings for 18 months. At the end of the 18 months, T.H.P. participants were required to attend a three-day orientation on microcredit. Eighteen and 30 months post-intervention (after the asset transfer), treated households consumed roughly 15 percent and 12 percent more each month than control households. At the seven-year mark, treated households consumed 25 percent more each month than control households. The difference in the value of assets of participants and non-participants was sustained throughout the seven-year period.

Bandiera et al. also conducted an R.C.T. of a graduation program in rural Bangladesh called Building Resources Across Communities (BRAC). The graduation program targeted ultra-poor women and provided an asset transfer to participants. That is, participants receive an in-kind asset paid for by the nonprofit. Women chose from a menu of possible assets which included livestock, a tree nursery or vegetables. Bandiera et al. surveyed 21,000 households in over 1,300 villages four times over a seven-year period. The researchers found positive impacts on consumption. Compared to the control group,

xxiv In fact, the T.H.P. program was modeled after BRAC.

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xxiv
consumption expenditure rose by 11 percent at the four-year mark. To assess long-term impacts, they surveyed the same treatment and control households seven years from the start of the intervention. However, almost half of the control group had since also received the intervention. Nevertheless, Bandiera et al. were able to estimate impact at the seven-year mark by constructing counterfactual estimates for the now-treated members of the control group, who they assumed experienced the same treatment effect as had been observed for members of the original treatment group.\textsuperscript{xv} Using this method, they found that consumption gains at seven years were “at least as large” as gains at the four-year mark. In other words, they could not reject the hypothesis that the impact at the four-year mark and at the seven-year mark were the same. Like Banerjee et al., Bandiera et al. also found sustained impacts on assets (effects at the seven-year mark are at least as large as effects at the four-year mark).

Given these results, we can make the working assumption that impacts of the Village Enterprise intervention could last at least seven years. However, it is important to note differences among the BRAC, T.H.P. and Village Enterprise interventions — differences which might lower our confidence in inferring the duration of Village Enterprise’s impact from the BRAC and T.H.P. studies. First, BRAC and T.H.P. provided direct asset transfers and livestock management, while Village Enterprise provides cash grants and training to develop a microenterprise. Village Enterprise encourages business groups to invest their cash grants in their business. (Business groups consist of three individuals who create a business together and develop a business plan.) The value of assets provided by BRAC and T.H.P. is also greater than the value of Village Enterprise’s cash grant, as we discuss below. Second, unlike BRAC and T.H.P., the Village Enterprise program does not specifically target women, though 81 percent of its participants are female.

Third, the three programs have distinct add-on services. The BRAC program includes health support and training on legal, social and political rights and the T.H.P. intervention includes trainings on social and health-related topics. Village Enterprise, however, does not provide any of these services — services that might contribute to and extend the impacts of the program. (For example, healthier individuals are able to work more. More informed women may be more capable of setting up their businesses successfully or allocating their labor efficiently.) Moreover, while BRAC and T.H.P. grant a subsistence allowance to its participants, Village Enterprise does not. Lastly, not only do the settings of

\textsuperscript{xv} Bandiera et al. calculate three versions of the counterfactual case for now-treated members of the control group, assuming in each that the treatment effect for those members is equal to the median, 25\textsuperscript{th} percentile and 75\textsuperscript{th} percentile treatment effects of the original treatment group. In all scenarios, the seven-year impact of the BRAC program is statistically significant, with the only exception being the impact on assets in the 25\textsuperscript{th} percentile scenario.
the interventions differ, but the durations of the programs also differ: The Village Enterprise program concludes within a year, the BRAC program lasts two years and the T.H.P. program lasts 18 months.

Ultimately, the three programs share the common mission of “graduating” the ultra-poor out of poverty through the accumulation of assets; therefore, the rigorous evaluation of BRAC and T.H.P. can inform us about Village Enterprise’s potential longevity of impacts. As such, we opt to use this seven-year horizon as the benchmark duration of program effects.

Moreover, evaluating the long-term effects of graduation programs can be very costly, requiring a lot of human and financial resources. For starters, tracking the same participants — particularly in rural, poor contexts — is often not possible: they might emigrate, die and be difficult to contact without cell phones. Banerjee et al. and Bandiera et al. have accomplished this difficult feat, tracking participants for a longer period than any other evaluation of a graduation program. We think it is appropriate that Village Enterprise and other graduation implementers borrow, with care, the findings of these studies, which have been expertly designed and executed.

In brief, we conclude there is medium quality evidence behind our estimate of the duration of Village Enterprise’s impact mainly due to the imperfect applicability of the Banerjee et al. and Bandiera et al. results to Village Enterprise’s intervention.

**COMPARISON TO THE RESEARCH LITERATURE**

The impact of the Village Enterprise model is consistent with the results of high quality counterfactual evaluations of similar graduation programs. We identified nine high quality R.C.T.s, reported in four academic papers, of moderate relevance to Village Enterprise. The studies found statistically significant impacts across several measures of economic outcomes, including consumption, expenditure, savings and earnings. All nine studies were variations on a multi-faceted livelihoods intervention targeted at the extreme poor in low-income and lower-middle-income countries, with the most common components of the intervention being: a cash grant or transfer of a productive (income-generating) asset; training to run a microenterprise; and a mechanism for managing savings.

However, there were important differences both among the interventions studied and between those interventions and Village Enterprise. In general, the per-person grant in the Village Enterprise program is, at $114 P.P.P., substantially smaller than the value of

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**xxvi** 120,000 Ugandan shillings converted to U.S. dollars at purchasing power parity (P.P.P.). We use the P.P.P. conversion factor (1,056) as of the weighted-average cash transfer date, following Sedlmayr et al.
grants or assets provided in other programs, which were as high as $1,228 P.P.P. Further, Village Enterprise's program is about half as long as most other graduation programs, which tend to last about two years. Some of the other programs also provided health and nutrition education and stipends for day-to-day spending, both of which Village Enterprise's program does not include. We hypothesize these differences would tend to favor the other graduation programs; that is, the results of the nine R.C.T.s may be higher than the impact that can be expected of the Village Enterprise program.

In addition, the graduation intervention is particularly complex and several elements, such as participant targeting and training, are highly dependent on the quality of implementation. The results of the nine R.C.T.s are indicative of the impact of Village Enterprise only if the quality of implementation in the nine R.C.T.s and at Village Enterprise are similar.
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</tr>
</thead>
<tbody>
<tr>
<td>Ethiopia</td>
<td>Ghana</td>
<td>Honduras</td>
<td>India</td>
<td>Pakistan</td>
<td>Peru</td>
<td>Bangladesh</td>
<td>Uganda</td>
<td>Uganda</td>
<td>Uganda</td>
</tr>
</tbody>
</table>

**Target population**

<table>
<thead>
<tr>
<th>Most common asset transferred</th>
<th>Sheep and goats</th>
<th>Goats and hens</th>
<th>Chickens</th>
<th>Goats</th>
<th>Guinea pigs</th>
<th>Cows and goats</th>
<th>Cash grant</th>
<th>Cash grant</th>
<th>Cash grant</th>
</tr>
</thead>
</table>

| Value of asset transfer**xxvii** | $1,228 | $451 | $537 | $437 | $1,043 | $854 | $560 | $1,048 | $375 | $114 |

<table>
<thead>
<tr>
<th>Duration of intervention</th>
<th>Two years</th>
<th>Two years</th>
<th>Two years</th>
<th>1.5 years</th>
<th>Two years</th>
<th>Two years</th>
<th>Not available</th>
<th>6 months</th>
<th>One year</th>
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</table>

<table>
<thead>
<tr>
<th>Frequency of interaction with trainers</th>
<th>Weekly</th>
<th>Weekly</th>
<th>Weekly</th>
<th>Weekly, then biweekly or monthly</th>
<th>Every six weeks</th>
<th>Monthly</th>
<th>Not available</th>
<th>Five visits in six months</th>
<th>Weekly for four months; monthly thereafter</th>
</tr>
</thead>
</table>

| Additional intervention components**xxviii** | Training; savings component; in-kind food support | Training; savings component; cash transfers for consumption support; village assistance committees | Training; savings component; training in health; nutrition and hygiene; food transfer; village assistance committees | Training; savings component; weekly health discussions; weekly cash transfers for consumption support; village assistance committees | Training; savings component; health component; weekly cash transfers for consumption support | Training; savings component; health discussions during training; cash transfers for consumption support | Training; consumption support; health component; | Training; supervisory visits; mentorship; business group creation and business savings groups formation | Training sessions; business group creation and business savings groups formation |

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**xxvii** Per participant, converted from local currencies to U.S. dollars at purchasing power parity (P.P.P.), using P.P.P. conversion rates for the years when the interventions took place.

**xxviii** This is not an exhaustive list of the additional components of each program.

[www.impactm.org](http://www.impactm.org)
<table>
<thead>
<tr>
<th>Results</th>
<th></th>
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</tr>
</thead>
<tbody>
<tr>
<td>5% increase in per capita consumption***</td>
<td>96% increase in household savings***</td>
<td>12% increase in total asset value***</td>
<td></td>
</tr>
<tr>
<td>37.5% increase in livestock income*</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>21% increase in participant annual earnings***</td>
<td>38% increase in participant monthly cash earnings***</td>
<td>151% increase in participant monthly cash earnings***</td>
<td></td>
</tr>
<tr>
<td>10% increase in consumption expenditure per adult***</td>
<td>57% increase in business asset ownership***</td>
<td>29% increase in asset value***</td>
<td></td>
</tr>
<tr>
<td>38% increase in participant monthly cash earnings***</td>
<td></td>
<td>7% increase in total income*</td>
<td></td>
</tr>
<tr>
<td>21% increase in participant annual earnings***</td>
<td></td>
<td></td>
<td>4% increase in consumption**</td>
</tr>
<tr>
<td>10% increase in consumption expenditure per adult***</td>
<td></td>
<td></td>
<td>17% increase in asset value***</td>
</tr>
<tr>
<td>38% increase in participant monthly cash earnings***</td>
<td></td>
<td></td>
<td>7% increase in total income*</td>
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<tr>
<td>151% increase in participant monthly cash earnings***</td>
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<tr>
<td>151% increase in participant monthly cash earnings***</td>
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</tbody>
</table>

**Impact at four-year endline, pooled for all six programs and compared to baseline mean**

***Impact at four-year endline, compared to control mean**

****Impact at four-year endline, compared to control mean***

*****Impact at 16-month endline, compared to control mean**

******Pooled estimates, compared to baseline***
QUALITY OF MONITORING SYSTEMS

Rating

Village Enterprise has high quality systems for monitoring activities, targeting, engagement, feedback and outcomes. Although there is small room for improvement, overall Village Enterprise’s monitoring systems are highly robust, indicating that Village Enterprise is consistently delivering a high quality program to its participants.

The main components of the monitoring systems used by Village Enterprise are:

1. **Targeting**: Tracks village selection, Participant Wealth Ranking and Poverty Probability Index used to select participants for the program.

2. **Mentoring and Training**: Tracks implementation of mentoring and training process, ensuring that business mentors are performing their duties and clients are attending and understanding trainings.

3. **Business Tracking**: Tracks progress toward business implementation, key operating goals (which determines either disbursal of second portion of grant or additional support) and business status at completion of program.

4. **Savings Group Constitution and Exit Report**: Monitors the structure and rules and tracks the sustainability of business savings groups.

5. **Entry and Exit Survey**: Tracks client poverty status.

6. **Consumption and expenditure surveys**: Captures data on food, semi-durable and durable goods consumption.\(^43\)

7. **Poverty Probability Index pre-post comparison**: Participants are scored using the Poverty Probability Index not only for targeting purposes at baseline, but
also after program completion to compare their poverty likelihood before and after the program.

In order to conduct this analysis, ImpactMatters interviewed senior management and mid-level managers and reviewed about 300 documents and datasets provided by Village Enterprise.

Table 2. Findings of Quality of Monitoring Systems

<table>
<thead>
<tr>
<th>Data Type</th>
<th>Credible</th>
<th>Actionable</th>
<th>Responsible</th>
<th>Transportable</th>
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<tr>
<td>Activity</td>
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<td>✔️</td>
<td>✔️</td>
<td>✔️</td>
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<tr>
<td>Targeting</td>
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<tr>
<td>Engagement</td>
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<td>✔️</td>
<td>✔️</td>
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<tr>
<td>Feedback</td>
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<td>✔️</td>
<td>✔️</td>
<td></td>
</tr>
<tr>
<td>Outcomes</td>
<td>✔️</td>
<td>✔️</td>
<td>✔️</td>
<td></td>
</tr>
<tr>
<td>CART Ratio</td>
<td>16 / 20</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Corresponding Rating</td>
<td>5 Stars</td>
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</tbody>
</table>

Activities

Village Enterprise collects data on its own activities in a credible and responsible manner. Managers have timely access to relevant data. Reports on monitoring data are structured so as to identify participants (business owners) and staff (business mentors) that have encountered challenges. Managers and executives have demonstrated commitment to address problems as they arise. Managers regularly address common problems facing participants, such as use of business funds for consumption, discord between business co-owners and gender-related issues with business ownership. Reviews are conducted biweekly, quarterly and annually and include discussion of lessons learned from monitoring data.

Business owners’ earnings are closely related to the productivity of their chosen business and local market conditions. One of the tools used by Village Enterprise, the Smarter
Market Analysis Risk Tool (SMART), identifies high-performing business lines in order to steer participants to choosing investments in the most profitable sectors. Village Enterprise could improve this tool by monitoring the climactic and market conditions for its businesses on a routine basis, such as quarterly or annually, and updating SMART with new data.

SYSTEMS

Village Enterprise's core model includes following activities: training of business mentors, targeting of participants, training for business owners, business group formation, seed funding in two installments, business owner mentoring and business savings group (B.S.G.) formation. Village Enterprise monitors these activities using the following systems:

- Knowledge test scores and other documentation to track the recruitment and induction training of business mentors
- Progress Out of Poverty Index reports and Participatory Wealth Ranking reports to track components of targeting
- B.S.G. training spot checks to track training of business owners
- B.S.G. attendance sheets to track business owners’ attendance at trainings
- Application forms to approve the first and second seed capital grants and receipts to track the disbursal of the grants
- Business owner registration forms to track all business owners entering the program
- Business spot checks to track business activity
- A mentoring checklist to track business mentors’ visits to business owners
- B.S.G. spot checks and exit surveys to track the saving, lending and meeting activity of B.S.G.s

CREDIBLE

Village Enterprise’s systems for verifying the completion of activities and counting activities and outputs capture each important component of its model. Village Enterprise has provided ImpactMatters with evidence verifying activities, through data and reports related to targeting, business mentor recruiting and induction training, business owner training, seed capital grant disbursal, business owner registration, business activity, mentoring visits and B.S.G. activity.
Village Enterprise uses standardized data collection instruments and data collection manuals to collect activity data. In addition, supervision at several stages of the data collection process helps ensure data quality. For example, one Village Enterprise country director described how Monitoring and Evaluation (M&E) associates who find unlikely survey timestamps during the data cleaning process can alert Field Coordinators to investigate the issue and ultimately obtain explanations from business mentors for the detected data discrepancies.

 Enumerator induction training sessions on data quality help ensure enumerators understand the importance of and process for collecting unbiased data. Village Enterprise's standardized data collection instruments contain questions that have been worded thoughtfully and neutrally.

 Village Enterprise's sampling strategies for spot checks are appropriate and do not introduce significant bias. Business spot checks after the first grant are conducted on every business, while business spot checks following the second grant are conducted on a random sample comprising at least a third of all businesses. Every B.S.G. receives at least one B.S.G. spot check. There are at least two training spot checks per business mentor, or three spot checks per training module per area.

**ACTIONABLE**

Data collected are almost immediately available to M&E staff, for use in troubleshooting data quality issues, and to country office management staff, to enable corrective managerial action. Interviews with multiple key decision-makers in the organization indicate operational and managerial decisions are largely made on the basis of M&E reports. For example, at the conclusion of each of the three cycles of participant enrolment per calendar year, end-of-cycle reports are jointly reviewed by the M&E team, CEO and Senior Director of Institutional Giving. Adjustments are made such that, within four months, when a new cycle starts, Village Enterprise has either set up new trainings or processes to resolve those issues or has plans to further research those issues.

 Village Enterprise's theory of change rests on the assumption that the seed capital provided to business owners is of a size that will lead to changes in outcomes. Village Enterprise has made efforts to test this assumption, but that testing has been irregular. As part of the annual Innovation Summit in 2016, a group of Village Enterprise staff presented to their peers "considerations for determining a better grant value based on a specific set of business inputs and long-term profits." Summit attendees voted on whether the grant size should increase, and this will vote was to be factored into next
year’s budget. However, Village Enterprise does not continually monitor prices of the most commonly used business inputs or local market and climactic conditions. The 2016 Innovation Summit recalibration of the seed capital amount appears to be an isolated instance rather than part of a continual monitoring effort to ensure the seed capital amount is appropriate given changing market conditions.

Village Enterprise’s strategic plan, M&E descriptive overview and Innovation Summit reports all demonstrate the senior management team takes action based on monitoring reports.

**RESPONSIBLE**

Standard data collection protocols provide enumerators with specific instructions that help increase the efficiency of their interactions with program participants. In addition, the use of digital and cloud-based platforms such as Salesforce and TaroWorks streamlines Village Enterprise’s monitoring processes and minimizes the burden of data collection on Village Enterprise’s own financial and human resources as well as on participants’ time.

Village Enterprise uses standard data collection protocols and enumerator training sessions to convey the importance of collecting data ethically, in terms of confidentiality, courtesy shown to participants and full disclosure of the purpose of Village Enterprise’s surveys.

**TRANSPORTABLE**

Village Enterprise tracks activity data that are closely aligned to its theory of change, including detailed data related to business mentor recruiting and induction training, targeting activities, business owner training, seed capital grant disbursement, business owner registration, business activity, mentoring visits and B.S.G. activity. Village Enterprise is not collecting substantial data that does not relate to its theory of change.

Village Enterprise shared extensive documentation of its activity monitoring with ImpactMatters during the course of the impact audit, including standardized data collection instruments, data collection manuals and protocols, and raw data and data reports related to the activities it carries out. Village Enterprise’s website and annual reports describe its activities and indicate the scale of its activities, such as by reporting the number of businesses started, the number of business owners trained, the amount saved per B.S.G. and the percentage of businesses still in operation after four years.44
Targeting

Village Enterprise collects high quality data on individuals that are targeted for the program. It uses a combination of geographic filters, survey data and community input to identify individuals that meet its poorest-of-the-poor criteria.

Village Enterprise reconciles the findings of the different surveys to minimize the number of non-poor participants who are accepted into the program. Appropriate and frequent reports are provided to managers and executives, who have a demonstrated commitment to ensure that participants are appropriately targeted.

Village Enterprise partially tests its assumption that participants lack financial literacy and business education. Village Enterprise could improve its monitoring of the assumptions in its theory of change by regularly surveying the availability of financial services, both at the level of the districts targeted and also at the level of the beneficiary households.

Village Enterprise's targeting data use a well-accepted standard for proxy assessment of poverty, the Poverty Probability Index (P.P.I.). Widely used proxy measures such as P.P.I. make it simpler for third parties to understand and assess Village Enterprise’s targeting data.

Village Enterprise should be more explicit in its targeting to ascertain the prior business training of participants. In order for Village Enterprise’s training to be effective, it must address gaps in the knowledge of participants. While the targeting criteria for poverty are satisfactory, the targeting criteria for business knowledge, financial access and financial literacy are more assumed than tested.

SYSTEMS

Village Enterprise targets participants through a multi-step process:

- **District or sub-county selection and approval**: Village Enterprises selects districts or sub-counties based on national poverty data and conducts courtesy visit with leaders at the district or sub-county level to both introduce the program and collect population and vulnerability information on villages in the district or sub-county.
• **Village selection:** Villages are shortlisted based on information provided by district or sub-county leaders and business mentors, Field Coordinators and Assistant Country Directors verify the shortlist.

• **Transect walk:** business mentors and Field Coordinators meet with leaders of the shortlisted villages and conduct a walk-through of each village to identify opinion leaders to be invited to the Participatory Wealth Ranking focus group.

• **Participatory Wealth Ranking (P.W.R.):** Opinion leaders define poverty in the context of their village and then rank households from “Very Poor” to “Rich.”

• **Village introductory meeting:** Village Enterprise conducts an open meeting to introduce the program to any member of the village.

• **Poverty Probability Index (P.P.I.):** Households identified as “Poor” and “Very Poor” in P.W.R. are surveyed using the P.P.I. to determine program eligibility. Village Enterprise also has a set of inclusion and exclusion criteria appended to the P.P.I. survey.

• **Community interest meeting:** Eligible households are invited to a meeting where details of Village Enterprise's program are provided and expectations of participants and of Village Enterprise are made clear.

Village Enterprise collects raw data from P.W.R. sessions and P.P.I. surveys, analyzes and summarizes this data into targeting reports. Village Enterprise also spot-checks P.P.I. scores in order to measure targeting accuracy.

**CREDIBLE**

Village Enterprise collects sufficient data to track the P.W.R. and P.P.I. stages of the targeting process, including raw data collected by P.W.R. facilitators and P.P.I. surveyors, reports generated from that data and data from spot checks.

Standardized data collection instruments and data collection manuals help ensure targeting data are collected reliably. The village selection form gives business mentors a defined sequence of questions and standardized phrasing of questions to use when collecting information from community representatives at village introductory meetings. P.W.R. data collection instruments and facilitation guidelines are consistent across all implementation sites and P.P.I. survey instruments and guidelines are consistent within countries. There is minimal written guidance for the less significant stages of the targeting process, such as how to conduct the transect walk, but interviews indicate they have produced reliable results in practice.
Village Enterprise provides detailed guidance to targeting staff on how to create a setting where complete and honest responses can be expected from participants. The P.P.I. and P.W.R. surveys contain questions that are worded thoughtfully and neutrally. In addition, the local leaders Village Enterprise consults on transect walks are recommended by government-appointed chiefs; an interview with Village Enterprise suggests that government involvement increases perceived accountability and that chiefs are trusted individuals in the community, thereby reducing the risk that P.W.R. participants are included for reasons other than genuine opinion leadership.

**ACTIONABLE**

P.P.I. and P.W.R. data collected are almost immediately available to M&E staff and country office management staff. P.P.I. spot check reports and overall targeting reports are produced every cycle (three times a year), enabling M&E staff to identify and resolve ineffective targeting in a timely manner. For example, routine spot check reports brought to attention high variances in regular participant targeting conducted by business mentors versus spot check targeting conducted by M&E enumerators. This led to a reassessment of targeting systems, including providing refresher trainings to business mentors on standardized usage of the P.P.I. instrument. Village Enterprise has also produced a Targeting Effectiveness Report based on baseline data from the R.C.T. currently underway in Uganda. In producing the report, Village Enterprise demonstrated the ability to translate dense technical information presented by the R.C.T. research team into a digestible summary tailored for key decision-makers in the organization.

In carrying out the P.W.R., P.P.I. and the community interest meeting stages of its targeting process, Village Enterprise collects targeting data that address a number of assumptions that undergird its theory of change. This targeting data substantiate the assumptions that participants face credit and savings constraints and lack business training and financial literacy; that participants want to start businesses and want to start them in groups with two other business owners; and that participants want to save money.

Village Enterprise's P.P.I. spot check reports and overall targeting reports demonstrate that managers have a record of acting upon the contents of the reports.

**RESPONSIBLE**

P.P.I. survey respondents concerned about the duration of the survey are informed of the time burden and made aware of the option to complete the survey at a more convenient
time. The P.P.I. and P.W.R. survey instruments are programmed into TaroWorks, reducing the data collection burden on Village Enterprise’s financial and human resources, as well as on survey respondents. There is evidence that Village Enterprise has weighed the costs and benefits of P.W.R. against other methods of targeting data, and has found that the benefits of P.W.R., including fostering community buy-in, justify the four-hour duration.

Village Enterprise trains staff to collect P.P.I. data, conduct courtesy visits and conduct community interest meetings ethically, in terms of confidentiality, courtesy shown to participants, and full disclosure of the purposes of surveys.

**TRANSPORTABLE**

Village Enterprise’s targeting data do not directly validate that the targeted population in fact demonstrates the expected characteristics described in Village Enterprise’s theory of change: namely, that the population faces credit and savings constraints, lacks business training and financial literacy and takes on inefficient levels of risk due to behavioral biases.

Village Enterprise has shared with ImpactMatters standardized data collection instruments, data collection manuals, and raw data and data reports for its targeting activities. Village Enterprise’s website describes its targeting strategy and the general profile of its target population.²⁷,⁴⁵

**Engagement**

Village Enterprise does an outstanding job of gathering high quality data on participants’ engagement. Using a combination of its own personnel and quasi-independent enumerators, it surveys participants both regularly and through random spot-checks. The findings of these surveys are directly tied to key process metrics in the theory of change. The findings also directly measure the key risks of the project, such as whether participants are able to direct financial assets toward business purposes; whether participants can maintain harmony in small partnerships and large savings groups; whether businesses are profitable; and the threat of family members interfering in the operation of the business.
The organization uses timely engagement data to manage at several levels of the organization, including partners (business mentors), personnel (field managers), and executives. Spot checks and surveys are conducted using computer assisted personal interviews (CAPI) and databases that streamline the data collection and quality assurance processes. Minimizing the reporting burden on participants and protecting confidentiality are two of the priorities described by executives in their discussion of engagement data systems.

**SYSTEMS**

Village Enterprise collects the following take-up and engagement data:

- **Participant business tracking:** Business groups’ application forms for seed capital grants and business spot checks track business group formation and subsequent business activity
- **Grant tracking:** Seed capital receipts and business spot checks track whether grants are collected and used for business activity
- **Mentoring:** Mentoring checklists track business owners’ participation in mentoring visits from their business mentors
- **B.S.G. engagement:** B.S.G. attendance sheets, B.S.G. training spot checks and B.S.G. exit surveys track business owners’ attendance and participation in trainings
- **B.S.G. activities:** B.S.G. spot checks and B.S.G. exit surveys track saving activity

**CREDIBLE**

Village Enterprise has provided raw data and data reports that track participant take-up and engagement at every touch-point of its activities, including trainings, B.S.G.s, business groups, receiving and spending seed capital, business activity and mentoring visits.

Standardized data collection instruments and data collection manuals are used to ensure participant take-up and engagement data are collected in a reliable manner.

Data collection protocols and training sessions ensure enumerators and business mentors understand the importance of and process for collecting unbiased engagement data. The standardized instruments for collecting engagement data contain questions that have been worded thoughtfully and neutrally. In addition, the sampling strategies for engagement-related spot checks are appropriate and do not introduce significant bias.
ACTIONABLE

Reports on take-up and engagement data are produced at appropriate and regular intervals, monthly and per cycle (three times a year). Country office management staff members are the primary consumers of these reports. Spot check reports are also often used for internal research presented at Innovation Summits, which feeds into Village Enterprise's strategic planning.

Engagement data help Village Enterprise substantiate the assumptions that the business selection process is effective and that businesses selected are viable for the market by tracking business survival rates, reasons for business failure and the industries in which business groups are operating. Engagement data complement outcomes data by allowing Village Enterprise to determine which industries are correlated with successful business outcomes. In addition, Village Enterprise's investment in developing the Smart Market Analysis Risk Tool and carrying out its Business Decisions Study demonstrate an organizational commitment to substantiating the assumption that participants are selecting effective businesses.

Village Enterprise also faces risks surrounding the ways in which participants relate to each other while engaging in Village Enterprise's activities. Business spot checks identify which of the original members of the business group are still active in running the business and ask for reasons why members are no longer active. B.S.G. exit surveys ask about the gender split in B.S.G. membership, the composition of the executive committee and reasons why members left the B.S.G., allowing Village Enterprise to track trends of female members voluntarily or involuntarily leaving, in light of concerns that women are prevented from engaging in activities due to cultural norms. A group of Village Enterprise staff analyzed these data, supplemented by other monitoring data such as training attendance, and presented their findings at the 2015 Innovation Summit. To track the risk that B.S.G.s break down due to interpersonal conflicts, Village Enterprise is able to refer to B.S.G. spot check and exit survey data, which measure B.S.G. meeting attendance, B.S.G. membership, the quality of leadership at the B.S.G. meeting observed, membership attrition and reasons for leaving the B.S.G.

The Village Enterprise senior management team demonstrates commitment to take action based on engagement data.
RESPONSIBLE

Attendance sheets, seed capital receipts and mentoring checklists all pose negligible data collection burdens to program participants, while spot checks, the B.S.G. exit survey and seed capital surveys are generally brief and pose low data collection burdens. The use of Salesforce and TaroWorks also minimizes the data collection burden on Village Enterprise's own human and financial resources.

Data collection manuals for collecting the various forms of take-up and engagement data described do not contain explicit instructions for ensuring data collection is ethical. However, based on Village Enterprise's close observation of data misuse policies and other policies to protect the interests of participants in its other data collection activities, as evidenced by data collection manuals for other survey instruments and key informant interviews, it is likely that Village Enterprise's staff collect take-up and engagement data with the same attention to confidentiality.

TRANSPORTABLE

The engagement data collected enables Village Enterprise to track whether participants are taking up and engaging with activities as expected in its theory of change. Village Enterprise is not collecting substantial engagement data that does not relate to its theory of change.

Village Enterprise has shared with ImpactMatters standardized data collection instruments, data collection manuals and raw data and data reports related to participant engagement. Village Enterprise's website and annual reports describe in sufficient detail the specific ways in which program participants are expected to engage with Village Enterprise activities, and disclose approximate numbers of business started and business owners actively participating in the program.27,45
Feedback

Feedback from line level staff (including business mentors) and participants (business owners) is gathered at many stages of the project and throughout the year. Participant surveys include ratings of different experiences with the program and free-text comment boxes. Participants are encouraged to share their views of the project with staff, including business mentors and field managers. Independent enumerators, who do not have operational responsibilities or share the same chain of command as the project staff, also survey a percentage of participants.

Managers use feedback in their biweekly and quarterly meetings to assess whether the project is on track. Executives provide regular forums for review and response to participants’ concerns. The annual Innovation Summit includes a panel based on the ideas and concerns of participants.

SYSTEMS

Program participants have substantial opportunities to provide feedback on the program. Business owners’ feedback is channeled through their respective business mentors to the country management team and other business mentors at bi-weekly meetings. Business participants are interviewed during both exit surveys and business spot checks.

Several Village Enterprise Accelerator pilot and research studies are dedicated to developing new programmatic services based on business owner feedback. Examples include the Business Decisions Study, which seeks to understand how business owners rank different business types according to criteria such as risk and profitability; the Risk Study, in which farmers rank their crops in order of risk and profitability; and the Household Portfolio pilot, which gauges business owners’ understanding of nutrition, farming practices and their attitudes about working in a group.

Staff members also provide feedback. At bi-weekly meetings, business mentors and country office management staff provide feedback on the design and implementation of each B.S.G. training session.

Annual Innovation Summits give Village Enterprise staff members at most levels of the organization the opportunity to participate in Village Enterprise’s strategic planning process. About six months prior to the Summit, staff members submit ideas for Summit topics, which the Field Management Team discusses and consolidates into a final list of
topics to be shared organization-wide. Teams of Village Enterprise staff are assigned to topics. Topic teams research and prepare to present at the Summit. Presentations made over the first four days of the Summit are used as the basis of strategic planning (performed with the group at large) on the fifth day, when staff members vote on the next steps for pilots, research and changes to programmatic elements.

Village Enterprise presents feedback from participants on its website under the heading “Most Significant Changes”. However, Village Enterprise does not provide supporting documentation for how or why these stories were chosen. Since the context of these communications is on the public-facing website, it is understandable that an incomplete methodology is presented.

Feedback mechanisms for partner organizations vary from partner to partner. Examples include surveying Community Connector Officers (USAID/FHI360 staff trained by Village Enterprise) for feedback after the program and holding review meetings with Forest Monitors (Wildlife Conservation Society staff trained by Village Enterprise).

CREDIBLE

Interviews with Village Enterprise staff indicate that the business mentors’ bi-weekly meetings effectively channel feedback from business owners to country management staff. Raw data and data reports made available verify that the exit survey, B.S.G. exit survey and business spot checks collect valuable feedback data from participants on a wide range of programmatic elements, including which aspects of the Village Enterprise program as a whole were most and least valuable; how satisfied they were with the B.S.G. experience and how useful they found savings practices like using a passbook; suggestions for refresher training topics for business owners; and open-ended suggestions for improving the Village Enterprise program. Village Enterprise also provides opportunities for staff at all levels of the organization to voice their feedback.

Standardized data collection instruments for the exit survey, B.S.G. exit survey and business spot checks help ensure the reliability of feedback data collected from program participants. Village Enterprise also discourages interaction between business mentors and M&E enumerators conducting spot checks to ensure the independence of enumerators. Village Enterprise’s other methods for collecting feedback tend to be more unstructured and discussion-based, such as business mentor bi-weekly meetings and review meetings with staff from partner organizations. However, this is appropriate for the oftentimes idiosyncratic and potentially sensitive nature of feedback data.
The feedback questions included in surveys are framed in a neutral way. However, there is a risk of response bias if business owners believe their answers will influence their chances of receiving the second seed capital grant, future training or other Village Enterprise services. Interviews indicate Village Enterprise takes steps to mitigate this: surveyors are instructed to assure business owners that their feedback will have no impact on their inclusion in the program and that they should feel comfortable answering candidly. Moreover, some feedback questions are open-ended, encouraging flexibility and freedom in responses. Village Enterprise also supplements feedback from current business owners with feedback from business owners who have already graduated from the program, thereby minimizing the effects of response bias on the overall set of data collected. Lastly, Village Enterprise has created an organizational culture that prizes input from staff at all levels of the organization, creating an environment where staff members likely feel safe to provide honest feedback.

**ACTIONABLE**

Participant feedback shared at business mentor bi-weekly meetings are recorded in meeting minutes and raised at higher-level meetings, such as Field Management Team meetings. Feedback data from participant surveys are collated in monthly and end-of-cycle reports for use by country management teams, M&E staff and senior management. Feedback data from ex-participants are collected by enumerators and shared with program associates, who raise issues to the Director of Monitoring, Evaluation and Learning at weekly meetings. These data feedback loops allow Village Enterprise staff to address problems as they arise.

Risks and assumptions are largely addressed by monitoring data other than feedback data. There are no unaddressed risks and assumptions that ought to have been studied using feedback data.

Interviews with Village Enterprise management and summaries of the findings and next steps of Village Enterprise Accelerator pilot and research studies consistently show that managerial staff members demonstrate a strong commitment to taking action based on feedback data.

**RESPONSIBLE**

The bulk of the feedback data collection burden falls on business mentors rather than program participants, who are only asked to answer feedback questions within other surveys, such as business spot checks and exit surveys. The burden on business mentors
is justified by how valuable the data have been in informing programming and operational decisions.

Data collection manuals train enumerators and business mentors in ethical data collection.

**TRANSPORTABLE**

Village Enterprise collects participant feedback on essential components of its program to identify problem areas where operational and programmatic improvements can be made. Village Enterprise is not collecting substantial data that does not relate to its theory of change.

Village Enterprise has shared with ImpactMatters standardized data collection instruments, data collection manuals, raw data and data reports for surveys that contain feedback questions. Village Enterprise has also made available information related to the Village Enterprise Accelerator pilot and research studies that are designed to capture program participants’ detailed feedback, as well as summit proceedings and examples of staff presentations from the last two annual Innovation Summits. Publicly, Village Enterprise does not describe what feedback data it collects and how they are collected. However, Village Enterprise regularly publishes Most Significant Change stories of randomly selected business owners. Most Significant Change is a technique for evaluating complex interventions and a way of holding organizations accountable to program participants.\(^{43,46}\) Each business mentor interviews three business owners at the start and end of every programmatic cycle, using written notes or audio recordings to capture their stories, which are then published on the Village Enterprise blog.\(^ {47}\)

**Outcomes**

Village Enterprise uses an appropriate methodology to monitor the survival of businesses and business savings groups. Rapid poverty assessments are used in tandem with random, independent, in-depth Consumption and Expenditure surveys that track key outcomes in detail.

Field managers and executives closely watch the findings from biweekly, quarterly, and annual reports. Reports that indicate trouble receive appropriate attention to remedy problems within days or weeks. Monitoring, Learning and Evaluation (M.L.E.) staff have
invested in an enterprise database for rapid reports on monitoring data. Quality assurance processes are reasonable and presumed to be effective, although ImpactMatters did not review data cleaning procedures.

Village Enterprise considers and mitigates the burden of survey response on participants. It uses computer-assisted personal interviews (CAPI) and a cloud database in order to streamline data collection and quality assurance. Length of interview and confidentiality are considered in the design of monitoring systems.

Though Village Enterprise’s public communications are transparent in terms of publishing quantitative changes in outcomes as measured by pre-post comparisons, they also tend to misrepresent these changes in outcomes as constituting evidence of impact. Pre-post comparisons use a poor-quality counterfactual and therefore cannot be the basis for claims of program impact. While consumers of Village Enterprise’s public communications likely appreciate the outcomes data published, they risk being misled if such data are not presented with greater care.

**SYSTEMS**

Village Enterprise’s outcomes data come from P.P.I. surveys, Consumption and Expenditure surveys, Entry/Exit surveys (each collected at the start and end of every cycle; there are three cycles per year) and B.S.G. exit surveys (collected at the end of each cycle). Village Enterprise uses these survey instruments to track its process metrics (number of participants enrolled, business formation and survival rates, B.S.G. formation and survival rates) and to construct non-counterfactual, pre-post comparisons of outcome metrics (consumption, expenditure and savings).

**CREDIBLE**

Village Enterprise’s detailed Consumption and Expenditure and Exit/Entry surveys capture the full scope of its intended outcomes. The P.P.I. is a validated country-specific predictive tool for targeting, but its use in longitudinal outcomes assessment may not be as suitable. However, because pre-post P.P.I. data are supplemented by Consumption and Expenditure and Entry/Exit surveys, overall outcomes data collected by the three instruments are considered sufficiently comprehensive.

Village Enterprise ensures outcomes data are reliable by using standardized data collection instruments and manuals for the P.P.I., Consumption and Expenditure survey, Entry/Exit survey and B.S.G. exit survey.
Village Enterprise takes appropriate measures to ensure the surveying environment promotes unbiased answers, such as training enumerators in neutral probing techniques, uniformity of question administration and being critical of questionable responses from participants if there is obvious evidence to the contrary.

**ACTIONABLE**

Outcomes data are reported each programmatic cycle in country-specific as well as organization-wide performance reports, which are jointly reviewed by the M&E team, C.E.O. and Senior Director of Institutional Giving. These periodic reviews enable senior management to be responsive and take corrective action before the next cycle begins.

Outcomes data provide substantiation for the assumptions that undergird the theory of change; namely, that program participants face credit and savings constraints, a lack of business training and financial literacy and undertake inefficient levels of risk due to behavioral biases. To gauge credit and savings constraints, the Entry/Exit survey asks whether the household currently saves money, the value of those savings and how much money certain household assets would be worth if liquidated on the market today; to gauge levels of business training and financial literacy, it asks which types of business participants personally have knowledge about and whether they have had any formal business training; and to gauge risk aversion due to behavioral factors, it examines whether business owners feel they have the power to change the course of their lives and whether they have future opportunities they feel are worth saving for. As previously mentioned, a combination of outcomes data and engagement data are used to validate the assumed market viability of businesses selected by business owners. Outcomes data also address the risk that there are cultural norms that prevent women from participating and that B.S.G.s break down due to interpersonal conflict.

According to interviews with Village Enterprise's senior leadership, Village Enterprise is pursuing outcomes-based funding to tie its outcomes directly to additional funding. Explicitly subjecting outcomes to funders' scrutiny and decision-making demonstrates Village Enterprise's commitment to take action based on outcomes data.

**RESPONSIBLE**

The reporting burden associated with the three major outcomes surveys is approximately six hours per participant. Though this is not an insignificant amount of time, there is consistent evidence from interviews that staff have been continuously making efforts to condense the approximately two-hour-long Consumption and Expenditure survey, which
was previously three hours long. Interviews also indicate that enumerators administering surveys are cognizant and respectful of participants’ time, offering the opportunity to postpone surveys to more convenient times.

Anecdotal evidence from interviews with the Village Enterprise team and data collection manuals for the three major outcomes surveys demonstrate that Village Enterprise takes adequate measures to ensure surveys are carried out ethically. Staff members are careful to uphold confidentiality, to protect and limit access to data once they are synced to the central Salesforce database and to be transparent about Village Enterprise's objectives for collecting data.

**TRANSPORTABLE**

Village Enterprise collects data that directly measure its intended outcomes as per its theory of change.

Village Enterprise shared with ImpactMatters standardized data collection instruments, data collection manuals, raw data and data reports for the Consumption and Expenditure survey, Entry/Exit survey and the P.P.I. Village Enterprise's Annual Report and Our Impact webpage publicly state the quantitative change in household consumption, expenditure and savings, based on cumulative 1987-2015 data and collected by conducting pre-post surveys on 20% of business owners.\textsuperscript{44,45} One remaining concern is that Village Enterprise's public communications consistently present pre-post changes in outcomes as evidence of impact. Claiming impact based on changes in outcomes using a poor-quality counterfactual, as in a pre-post study or a study of self-selected comparison groups, is discouraged. Such data are suggestive rather than conclusive, and it is recommended that Village Enterprise guide its audience in interpreting that data carefully and realistically.
LEARNING AND ITERATION

Rating

Village Enterprise makes systematic and continuous change on the basis of high quality data. Iterations to the Village Enterprise model are routinely subjected to testing using a counterfactual model, and the iterations that are scaled are supported by reasonable evidence. Each of the iterations discussed below had either a counterfactual test of impact or a strong effect sizes based on a large pilot test. Most of the successful iterations occurred within a systematic innovation framework, with recognizable components of Plan-Do-Study-Act cycle.\textsuperscript{48} Iterations take place within a well-defined pipeline of tests, with a calendar of tests and results prior to scaling up. The iteration testing function is distinct from other monitoring and evaluation processes.

RATING RUBRIC

<table>
<thead>
<tr>
<th>Criteria</th>
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<tr>
<td>Iteration is based on data</td>
<td>YES</td>
</tr>
<tr>
<td>Data are of high quality</td>
<td>YES</td>
</tr>
<tr>
<td>Iteration is systematic and periodic</td>
<td>YES</td>
</tr>
<tr>
<td><strong>Corresponding Rating</strong></td>
<td>5 STARS</td>
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Iterations

Village Enterprise recently participated in a randomized controlled trial (R.C.T.) with multiple treatment arms that tested Village Enterprise's standard program and several
variants and components of the standard program, including a variant that excludes business savings groups (B.S.G.s), a Business-in-a-Box variant that provides business assets and inputs instead of or in addition to cash, an unconditional cash transfer, and an unconditional cash transfer modified with a behavioral intervention. This randomized trial has just concluded and no iterations have been made to Village Enterprise’s program based on the findings, but Village Enterprise worked closely with the researchers to design the study and has expressed a commitment to make iterations to its core program if they are suggested by the findings of the study.

Over the past three years, Village Enterprise adopted six iterations, three of which are considered “minor” iterations and three of which are considered “additive” iterations, in that they are additional components to the intervention rather than modifications.

All six iterations implemented over the past three years were reviewed in assessing Village Enterprise’s Learning and Iteration. However, additive iterations are considered to have less of a burden of proof and are not included in the written analysis presented here. The three minor iterations reviewed are the Savings With A Purpose component, the Family Support Training module and the Leadership training modules.

- **Savings With A Purpose (SWAP):** Through SWAP, Village Enterprise trains business owners to save money toward a set goal with a set time frame. Pooled funds in the B.S.G. are organized into primary account, emergency fund, and SWAP account savings, and one business owner per B.S.G. is elected into the role of SWAP Officer.

- **Monitoring for Sustainability (M4S):** The M4S training module is intended to enable business owners to lead their own B.S.G.s more effectively, for instance by making informed decisions during B.S.G. executive committee elections, having executive committees that monitor businesses, and sharing business knowledge and experiences with each other during B.S.G. meetings. The ultimate goal is to ensure the sustainability of the B.S.G. and businesses after business owners exit the Village Enterprise program.

- **Family Support Training Module:** This module aims to educate business owners’ family members about the Village Enterprise program and the benefits of the program that accrue to the entire family, thereby minimizing instances of unsupportive family members spurring program attrition and poorer outcomes.

The three additive iterations by Village Enterprise were Passbooks (Record Keeping for the Illiterate), Smarter Market Analysis Risk Tool (SMART) and Visual Year Long Plan.

- **Passbooks (Record Keeping for the Illiterate):** Village Enterprise provided stamps and inkpads to enable illiterate business owners to track their savings.
• **Smarter Market Analysis Risk Tool (SMART):** SMART guides business owners through the business selection process by providing business owners with information on profit, demand, risk, price variability and sustainability. Information comes from several data sources: focus groups with “good” farmers, agronomists from local universities, village and urban point-of-sale surveys and agricultural-veterinary shop surveys.

• **Visual Year Long Plan:** In 2013, Village Enterprise introduced a new system to enable illiterate business owners to draw pictorial business plans. In 2016, Village Enterprise aims to make it easier to create, use and interpret the business plans by providing business owners with pre-drawn images and tables.

One of the primary methods through which Village Enterprise determines which iterations to adopt is an annual weeklong Innovation Summit for staff to present their ideas for, and the outcomes of, iteration testing. Summit proceedings and presentation slides from the 2015 and 2016 Innovation Summits were reviewed to assess Village Enterprise’s Learning and Iteration. M&E plans and internal reports for each iteration were also reviewed. In addition, the following analysis draws heavily from a summary document, managed by Village Enterprise’s Innovations Team, that tracks the iteration pipeline and decisions about implementation for all iterations over the past three years.

### Data Quality

Village Enterprise’ R.C.T. is a high quality test of different iterations of the Village Enterprise microenterprise model. Such a high quality test to specifically benchmark different program designs and determine which components of the program are most effective is exceptionally rare and Village Enterprise deserves strong credit for designing, funding and participating in the study. Two of the three minor iterations over the past three years to the Village Enterprise model were adopted based on high quality data. The third was adopted on the basis of some data, although the data used could likely have been of higher quality. Nonetheless, Village Enterprise has demonstrated that it typically uses high quality data to make decisions.

Village Enterprise conducted an A/B test on Savings With A Purpose (SWAP). Four business mentors representing three Village Enterprise implementation geographies were selected to pilot SWAP. Business mentors chose one of their B.S.G.s to serve as the treatment group, using their other B.S.G. as a control group. Baseline data was collected using a separate SWAP survey, endline data was collected six months into saving and monthly
saving activity was continuously monitored through spot checks. Comparing B.S.G.s with the SWAP iteration against those without, Village Enterprise found 31 percent higher total savings in Soroti, Uganda, 73 percent higher total savings in Hoima, Uganda, and no data from Kenya at the time of analysis. Despite a small sample size of only eight B.S.G.s, the effect on savings was large and positive, justifying Village Enterprise’s adoption of the SWAP iteration.

Village Enterprise also tested a new training module known as Monitoring for Sustainability. M4S was pilot tested with 14 B.S.G.s across Village Enterprise’s three areas of operation at the time: Hoima and Soroti (Uganda), and Eldoret (Kenya). Treatment group B.S.G.s were equipped with leadership training and a self-monitoring system based on spot checks, while control groups received the program in its original form. Village Enterprise used routine monitoring systems (exit surveys and spot checks) modified with M4S-specific survey questions to evaluate the program. At endline, Village Enterprise found a higher percentage of treatment group B.S.G.s reviewed their B.S.G. constitutions every meeting and a consistently higher percentage of treatment group business owners reported perfect attendance at B.S.G. meetings, but there were no clear trends in business owners’ ratings of B.S.G. leadership quality, in executive committee members’ attendance and in the percentage of B.S.G.s that discussed business challenges during meetings. While these are modest results, Village Enterprise used a reasonable (albeit based on a small sample size) estimate of the counterfactual, giving it strong enough grounds to adopt the iteration.

To test Family Support Training, Village Enterprise conducted interviews with business owners and their family members who participated in a pilot Family Support Training session. Family members reported having a better understanding of the program and of the importance of their support. However, the test collected only feedback data and did not observe changes in process metrics. As a result, this test did not provide high quality data to support the iteration, particularly as this form of self-reporting can lead participants to provide socially desirable responses. After rolling it out, business mentors were instructed to measure attendance at trainings and provide feedback during bi-weekly team meetings to better evaluate the new training module.

**Systematic Change**

All three of the minor iterations over the past three years to the Village Enterprise model were adopted through a systematic process.
The SWAP iteration was adopted after undergoing a Plan-Do-Study-Act learning cycle, wherein the iteration idea was sourced; tested; analyzed and summarized for decision-makers; and then accepted and implemented systematically. Village Enterprise sourced the idea for SWAP from monitoring data that revealed many participants were not fully utilizing B.S.G.s: Participants were saving, but not concurrently taking out useful loans. After conducting an A/B test of SWAP, Village Enterprise staff analyzed test findings in an internal report for decision-makers. The decision to adopt the new component was based on positive results was documented in a collaborative spreadsheet managed by the Innovations Team and presented at the 2016 Innovation Summit to all staff.

Village Enterprise reports that many program participants express a desire for continued Village Enterprise staff presence even after exiting the program and that past training modules, though valuable for financial literacy and business management, provide less guidance on leadership. Village Enterprise documented the design and findings of iteration tests for two new training modules, Leadership I and II, and presented the modest, but high quality, results in internal reports as well as at the 2015 and 2016 Innovation Summits. As with other Village Enterprise iterations, the decision-making process was centrally documented by the Innovations Team.

Need for the Family Support Training module surfaced when Village Enterprise staff and business mentors observed that female participants were not receiving adequate support from family members with regard to their new roles as business owners. Village Enterprise ran a pilot Family Support Training session with positive qualitative results, which were cited in the Innovations Team’s documentation of its decision-making process and presented to all staff at the 2015 Innovation Summit.

Village Enterprise has an Innovations Team, composed of Village Enterprise staff from various country offices and levels of the organization, that manages current and planned projects, pilots and studies.

Periodic Change

Village Enterprise has systems for periodically considering and adopting iterations to its core model.

The primary mechanism for sourcing and testing iterations is based around the annual Innovation Summit. Each year, Village Enterprise staff members submit their ideas for potential iterations about six months in advance of the Summit. Based on proposed ideas, the Field Management Team decides on a final list of iterations and staff members are
assigned to small teams responsible for researching, pilot testing and presenting those iterations at the Summit. The Innovations Team also presents the findings of its pilot studies and research conducted throughout the year.

The Summit brings together staff at almost all levels of the organization. After four days dedicated to iteration testing presentations, Village Enterprise convenes a strategic planning day, during which all staff vote on next steps for pilots, research priorities and programmatic changes. Specific takeaways and resultant changes to the Village Enterprise model are recorded and disseminated to staff after the Summit. Village Enterprise has thus developed a system for routinely “crowd-sourcing” ideas for iterations from those who know the program and program participants most intimately.

Documents shared and interviews conducted with Village Enterprise team members demonstrate that staff members, at multiple levels of the organization, are able to articulate which iterations are under consideration and which are under testing. In particular, the Innovations Team centrally manages a collaborative document that lists all iteration studies conducted during the 2014-2016 period, along with their respective goals, findings, next steps, reasons for not pursuing, and a red, yellow or green light indicator representing whether the iteration will be adopted.

With a focus on innovation, Village Enterprise has conducted an impressive number of iteration tests in the past three years. The periodicity of iteration testing will likely only increase as Village Enterprise launches its Accelerator program to formalize internal R&D efforts and leverage partnerships with external research institutions.
ANNEX

Nonprofit Information

NAME Village Enterprise
CHARITABLE STATUS 501(c)3 nonprofit
WEBSITE www.villageenterprise.org
CONTACT EMAIL info@villageenterprise.org
ADDRESS Mailing:
751 Laurel Street, PMB 222
San Carlos, CA 94070

Physical:
1161 Cherry Street, Suite M
San Carlos, CA 94070

Audit Information

RELEASED February 12, 2018
PERMALINK www.impactm.org/a/village-enterprise/1
STANDARD Version 0.3
ACTIVITIES Literature review, document and data review, senior management interviews, field staff interviews and key informant interviews.
AUDIT TEAM Tamsin Chen, Ben Mazzotta and Veronica Vazquez Ugalde
REVIEW TEAM Elijah Goldberg and Michael Weinstein
| CONFLICT DISCLOSURES | Innovations for Poverty Action (I.P.A.) conducted the randomized controlled trial of Village Enterprise's program in Uganda. Dean Karlan is on the board of directors of ImpactMatters and I.P.A. |
Glossary

Bias
Bias is a non-random error in a statistical estimate. Whenever estimates are based on a sample from a larger population, there will be random error in that estimate: no two samples will produce exactly the same estimates. An estimate is biased when those errors lead it to be consistently above or below the true value that is being estimated.

Comparison Group; Control Group
A comparison group, in contrast to the treatment group, is a group that did not receive the intervention. Comparison groups enable nonprofits and researchers to compare what happened to participants of their program to what might have happened if they were not in the program. ImpactMatters refers to comparison groups as “control groups” if they were constructed using probabilistic sampling, meaning if control-group members were chosen at random from the same population as the treatment group.

Counterfactual; Counterfactual Evidence
The counterfactual is what would have happened in the absence of a program or other event. Understanding the counterfactual is essential to understanding the impact of a program. Participant outcomes may change over time for many different reasons not related to the program. By comparing the difference between participant outcomes and counterfactual outcomes, the impact of a program can be estimated.

The counterfactual cannot be directly measured, as researchers cannot observe the same participant both participating and not participating in the program. However, it can be approximated by randomizing participants into an intervention group and a control group, and then comparing outcomes across the two different groups.

Discount Rate
People tend to value benefits in the future less than benefits in the present, for three primary reasons. First, benefits today can be reinvested and generate some return. Second, the future is uncertain, and we are often uncertain if future benefits will actually materialize. Third, most people are impatient, and prefer immediate gratification over future gratification. A discount rate captures this by discounting or reducing future benefits compared to current benefits.
Effect Size
How large the measured impact was on outcomes in the group receiving the program compared to a similar group that did not receive the intervention.

GRADE
Grading of Recommendations Assessment, Development and Evaluation (GRADE) is an approach to rating the quality (or certainty) of evidence and strength of recommendations. ImpactMatters' assessments of quality of evidence are inspired by the GRADE approach.

Impact
Impact is a change in beneficiary outcomes attributable to a nonprofit's intervention, net of counterfactual effects.

Independent Evaluator
An independent evaluator can include a research organization or academics engaged to analyze the impact of a program. Independent evaluators are not directly employed by the program, although they may be paid through program resources.

Intervention
An intervention is what researchers study and nonprofits implement. An intervention includes anything from a medical procedure to a conditional cash grant. ImpactMatters studies the intervention that a nonprofit implements, mapping that intervention to the evidence base on that particular intervention. Also referred to as the nonprofit's program.

Purchasing Power Parity
The purchasing power of a currency is the quantity of the currency needed to purchase a common basket of consumer goods and services. P.P.P. equalizes the purchasing power of two given currencies by accounting for differences in the cost of living and inflation in the two countries.

Quasi-experimental Design
A study with a quasi-experimental design tests a causal hypothesis but lacks random assignment of test subjects to treatment and control groups, perhaps due to logistical or ethical constraints.
Randomized Controlled Trial (R.C.T.)
A randomized control trial is an evaluation design by which individuals (or groups) are randomly allocated into treatment and control groups, where the treatment group receives the program. The outcomes of the two groups are then compared in order to estimate effect size.

Sample; Sample Size
The sample is the portion drawn from a population for testing or analysis that is intended to enable statistical estimates of the behavior or attributes of the whole population. The sample size is the number of units that comprise the sample; a large enough sample size allows inferences about the whole population to be made.

Social Costs or Societal Costs
Social costs include all costs incurred by society as a result of the nonprofit's program. Different from accounting costs, which include just the costs that appear on the nonprofit's accounting statements, social costs may include, for instance, the opportunity costs of participants' time spent in the program and the costs to other organizations and governments of helping to delivering the program.

Statistical Power
Statistical power is the probability that a test will correctly reject the null hypothesis (the hypothesis that there is no statistically significant difference between the samples being compared). An underpowered test will likely yield large p-values and confidence intervals and will lack the evidence to reject the null hypothesis.

Statistical Significance
A statistically significant result (often a difference of means of the main outcome of interest) is a result that is unlikely to arise as a result of chance. This doesn't mean the finding cannot be due to chance – just that it is very unlikely.

Systematic Review
A type of literature review that collects and analyzes multiple research studies in order to answer a research question. After a research question is defined and appropriate research studies identified, data from the studies are extracted, assessed for their quality, analyzed, sometimes statistically combined in meta-analyses, and reported in such a way as to address the research question.
Theory of Change

A theory of change connects the problem to the intervention the nonprofit runs to expected process and outcome metrics. The objective of a theory of change is to provide a testable hypothesis for why the intervention is solving some problem that will lead to positive changes for the targeted beneficiaries.

Treatment Group

In an experiment, the treatment group is comprised of experimental subjects that receive the treatment being evaluated.
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