CGD NOTE 356 | JANUARY 2024

Designing a Randomized Controlled Trial on Livelihoods for Refugees and Hosts: The Case of Re:Build in Kampala

Edited by Thomas Ginn

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Summary

Few livelihood programs for refugees are rigorously evaluated. While findings among other populations are informative, the effects among refugees could differ significantly. This paper outlines our process for jointly developing a livelihood program and randomized study design in Kampala, Uganda. We first review rigorous evidence on interventions targeting wage and self-employment in low- and middle-income countries. Based on this review and input from a variety of stakeholders, we chose to pilot cash grants and different forms of networking programs in a sample of about 100 people, which we summarize in the next chapter. We describe the pilot's design and discuss the lessons that informed the scale-up to 2,600 people in the full study. Finally, we present the intervention and research design that resulted from this process: cash grants and groups with a mentor and three mentees that vary by nationality, gender, and group-level incentives. We hope that sharing our process and evidence base will be helpful for practitioners and researchers leading similar projects. Preliminary results from the randomized trial are expected in May 2024.

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Chapter 1. Introduction

Thomas Ginn

What are the most effective programs to improve livelihoods for refugees and hosts? As policymakers in protracted refugee situations shift from short-term humanitarian responses to longer-term development support, it is critical to identify effective approaches for allocating scarce resources. Hundreds of millions of dollars are spent on livelihood programs for refugees annually, yet almost none of these interventions are rigorously evaluated.[1] For example, in their recent review of evaluations in the humanitarian sector, Benrey and Kenny (2023) find only ten rigorous evaluations on livelihoods.[2] Further, the evidence from similar programs outside the humanitarian space is often disappointing (see McKenzie, 2017), raising significant doubts about much of this spending's cost-effectiveness. Overall, the motivating question—especially for refugees—remains largely open.

In 2021, the IKEA Foundation awarded the International Rescue Committee (IRC) 30 million Euros for a five-year project targeting livelihood outcomes for refugees and hosts in two capital cities of major refugee-hosting countries in East Africa: Kampala, Uganda and Nairobi, Kenya. The project, Refugees in East Africa: Boosting Urban Innovations for Livelihoods Development (Re:Build), includes randomized controlled trials (RCTs) to help build the base of rigorous evidence in forced migration contexts. Over five years, Re:Build will implement two randomized controlled trials in Kampala and two in Nairobi.

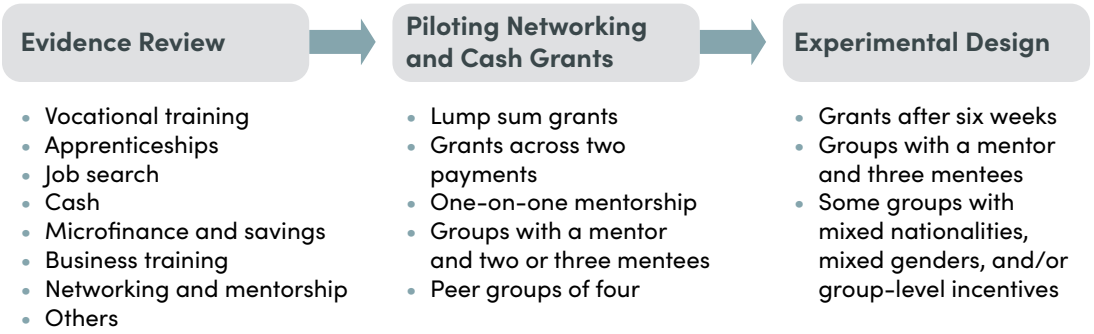
This note outlines the development of Re:Build's first randomized controlled trial in Kampala, Uganda. Researchers and practitioners worked together to design a trial with three main goals:

1. maximize expected positive impacts for the sample with an intervention budget of approximately 700 USD per client;
2. evaluate programs with the potential for scale;
3. address cutting-edge research questions related to forced displacement and livelihoods.

1. Evidence generation and use is inadequate across global development and public policy more generally. See Kaufman et al. (2023) for an overview. However, a number of trials are currently ongoing or under development, in part thanks to the Displaced Livelihoods Initiative, a joint initiative managed by Innovations for Poverty Action and the Abdul Latif Jameel Poverty Action Lab and supported by the IKEA Foundation.
2. Benrey and Keney (2023) include studies that are randomized evaluations or employ regression discontinuity designs.

Suggested citation: Ginn, Thomas. "Chapter 1: Introduction," in *Designing a Randomized Controlled Trial on Livelihoods for Refugees and Hosts: The Case of Re:Build in Kampala*, edited by Thomas Ginn. CGD Note 356. Washington, DC: Center for Global Development, 2024. https://www.cgdev.org/publication/designing-randomized-controlled-trial-livelihoods-refugees-and-hosts-case-rebuild.

Figure 1.1. Design Process Summary



It is important for RCTs in these contexts to include host communities, both to enhance social cohesion (Baseler et al., 2023) and to understand what lessons may or may not apply from studies with people who have not been displaced (Ginn, 2023). We also wanted to go beyond simply applying the same intervention to both groups and consider questions on the economic and social interactions between refugees and host communities.

In this note, we first provide a brief background to the project and the context. Next, in Chapter 2 we summarize existing evidence on interventions that aim to improve livelihoods, using reviews of programs in low- and middle-income settings (which do not necessarily include displaced populations). We used the findings from this review to select potential interventions and develop programs to pilot.[3] We then summarize lessons from the pilot, which was implemented between October 2021 and April 2022, in Chapter 3. We conclude this note by outlining the experimental design that resulted from this process in Chapter 4. The randomized trial was launched in June 2022, and we expect preliminary results—which are not included in this note—to be available by May 2024.

The design process is summarized in Figure 1.1. In the evidence review, we find interventions for entrepreneurs are generally more promising than programs for job-seekers, though results vary significantly across studies for all interventions. Based on the review, we targeted current and aspiring entrepreneurs and allocated the majority of the RCT intervention budget to cash grants. For the remaining intervention budget, we piloted multiple interventions to facilitate business networking and mentorship opportunities: one-on-one mentorship, groups of different sizes, and groups with and without a more experienced mentor. The pilot was instrumental in answering specific design questions for the RCT like which networking program to implement and whether to disburse cash across one or two payments. Finally, we used observations from the pilot, clients' feedback, the team's experience, and logistical considerations to finalize the interventions: lump sum grants, groups with one mentor and three mentees that vary by nationality and gender, and an incentive at either the individual or group level.

3. These findings also informed the design of Re:Build's first RCT in Nairobi; see Khan et al. (2023) for the pre-analysis plan. They did not influence Re:Build's non-RCT programs which did not involve researchers.

Context

Uganda is one of the world's most progressive refugee hosting environments (Ginn et al., 2022; IRC, 2022), and approximately 100,000 refugees live in Kampala.[4] All refugees are allowed to live outside of the settlements, but urban residence means foregoing most assistance like food rations. While some refugees maintain official residency in the settlements and come to Kampala for work, the IRC needed to verify that all refugee clients in the Re:Build program were registered with the United Nations High Commissioner for Refugees (UNHCR) in Kampala. Refugees are able to hold formal jobs and do not need a work permit, though there is often confusion among potential employers and even government officials who sometimes request these documents. In addition, refugees are allowed to start businesses if they obtain the same permits that Ugandans are required to hold. For more on the policy environment in Kampala, see IRC (2022).

The IRC, the research team, and the IKEA Foundation agreed on a set of inclusion criteria for the RCT sample. The sample comprises a roughly equal share of hosts and refugees, including many of the refugee nationalities that reside in Kampala. This includes Congolese, Rwandans, Somalis, Burundians, Ethiopians, Eritreans, Sudanese, and South Sudanese. The sample is balanced by gender, between the ages of 18 and 35, interested or already engaged in some livelihood activity,[5] and who officially resided in Kampala, Uganda.[6] We expected this sample to be interested in a variety of sectors, with tailoring, hair-dressing, restaurants, and retail as the most popular. Finally, partners agreed that the control group would receive a similar program, determined later, within approximately eighteen months of the RCT's launch.

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IRC. "An Analysis and Evaluation of Refugee-Related Policies and Legislation," 2022.

Kaufman, Julia, Amanda Glassman, Ruth Levine, and Janeen Madan Keller. "Breakthrough to Policy Use: Reinvigorating Impact Evaluation for Global Development." Washington, DC: Center for Global Development, 2022.

4. UNHCR reports 105,892 refugees and 30,995 asylum seekers live in Kampala as of December 2023.
5. Other services are available through the IRC and partners for those with more specialized needs who are unable to work.
6. This criterion ruled out programs to facilitate migration like the one studied by Bryan et al (2014), for example.

Khan, Sana, Elizabeth Paluck and Andrew Zeitlin. "What are the benefits of mentorship for aspiring micro-entrepreneurs? An examination of mixed gender, same gender, and refugee-host and host-host mentoring pairs and the effects of mentoring and mentoring with perspective-sharing over simple cash transfers." AEA RCT Registry, 2023.

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Chapter 2. Evidence Review

Thomas Ginn, Jimmy Graham, Claire Manley[1]

Introduction

To design an intervention, we looked first to reviews that summarize existing evidence. Some reviews offer comprehensive overviews of active labor market policies, including Blattman and Ralson (2015) on fragile settings; McKenzie (2017) on low- and middle-income countries generally; Card, Kluve, and Weber (2018) who include findings from high-income countries; Datta et al. (2018) on youth unemployment; and Jayachandran (2021) on microentrepreneurship in low- and middle-income settings. Others examine specific interventions, including cash transfers (Baird et al., 2018), savings programs (Steinert et al., 2018), and business training (McKenzie et al., 2023). Reviews by Benrey and Kenny (2023), Schuettler and Caron (2020), and the World Food Programme and World Bank (2022) specifically address evidence from forced displacement contexts. We supplement these insights with selected recent evidence, noting that a comprehensive overview of all relevant literature is beyond the scope of this paper.[2] [3]

We specifically review research that includes business or employment-related outcomes and emphasize studies with an initial sample of individuals interested in labor force participation—for example, studies that test the effects of cash grants on microenterprise owners. Since we planned to include a balanced sample across gender and refugee status, we also highlight studies that include displaced populations or disaggregate results by gender.

We categorize interventions into two broad spheres: wage employment programs, which help job-seekers find and prepare for hired work, and self-employment programs, which help entrepreneurs open or improve their businesses. While most programs primarily target one sphere, many could affect either wage or self-employment outcomes.

1. Authors are listed alphabetically.
2. The initial review was conducted in 2021 and is supplemented here with additional results published after the trial was launched.
3. Other reviews include Grimm and Paffhausen (2015) on low- and middle-income contexts and Brown et al. (2023) on forced migration studies.

Suggested citation: Ginn, Thomas, Jimmy Graham, and Claire Manley. "Chapter 2. Evidence Review," in *Designing a Randomized Controlled Trial on Livelihoods for Refugees and Hosts: The Case of Re:Build in Kampala*, edited by Thomas Ginn. CGD Note 356. Washington, DC: Center for Global Development, 2024. https://www.cgdev.org/publication/designing-randomized-controlled-trial-livelihoods-refugees-and-hosts-case-rebuild.

Typical interventions for wage employment include:

- **Vocational training** offers specialized training for specific trades or professions;
- **Apprenticeships and internships** provide funding and matching for on-the-job training;
- **Job search assistance** includes resume building and interview preparation, or offers access to professional networks.

Typical interventions for self-employment are:

- **Cash transfers** provide direct, often unconditional financial support;
- **Savings and microfinance services** provide information and access formal savings accounts or enable entrepreneurs to access credit at affordable interest rates;
- **Business training** offers education in essential business skills like accounting, marketing or management;
- **Networking and mentorship** involves pairing individuals in groups with peers or with experienced mentors who can offer guidance and support;
- **Cash plus and graduation programs** supplement direct cash or asset transfers with other interventions, such as skills training or information.

This list is far from exhaustive, and researchers and practitioners are regularly testing new approaches to improve cost-effectiveness. Many programs also combine strategies to harness potential complementarities. While we begin with evidence on traditional, standalone programs, we also consider relevant combined programs.

As Blattman and Ralston (2015) note, "there is no off-the-shelf program that will work in all contexts." In our case, however, we found the evidence on self-employment interventions to be more promising than interventions targeting wage employment. In multiple trials, grants to entrepreneurs have led to short- and medium-term increases in business ownership and profits, though the findings are less promising for women entrepreneurs and limited evidence exists for long-run outcomes. Business training programs have yielded small but significant gains. Networking interventions like mentorship show promise in a few studies and could be even more impactful for groups such as women and refugees who typically have fewer business connections than local men. Combining interventions can be effective, though most of the evidence on programs like graduation comes from rural settings. The evidence on wage employment is less encouraging. Blattman and Ralston (2015) note, for instance, that "it is hard to find a skills training program that passes a simple cost-benefit test," though slightly more promising results have been published since their review (Alfonsi, 2020). Standard apprenticeships and job search assistance programs also have disappointing overall records. We decided that interventions targeting self-employment—specifically, cash grants combined with a networking intervention—are the best combination to meet our goals noted above: cost-effectiveness, potential for scale, and academic relevance.

Wage Employment

Vocational Training

In an analysis of 12 studies, McKenzie (2017) finds modest positive effects from vocational training in LMICs, noting an average employment increase of 2.3 percentage points. He notes that the returns to vocational training are similar to other types of education. McKenzie (2017) also estimates that it would take about 50 months of these gains to participants to offset program costs, while multiple studies find these effects diminish over time. Blattman and Ralston (2015) conclude that

> "After repeated studies of technical, vocational, and business skills training programs, most programs do not have positive impacts…those that do are often so expensive that costs far outweigh benefits. And most poor people turn these programs down or drop out."

Regarding displaced individuals specifically, Schuettler and Caron (2020) concur that the "track record of skills only interventions is not promising." They suggest that pairing employment training with additional interventions, like assistance with job searching or work permit applications, could yield better outcomes. However, Lyall et al. (2019) randomized vocational training, cash (75 USD), and the combination of the two for a population sample of "at-risk" hosts and internally displaced persons in Afghanistan. They find training alone led to "modest" gains on the likelihood of earning income but no effects from cash alone or the combined intervention. They hypothesize their sample needed the cash to meet immediate basic needs and weren't able to invest in employment ventures.

However, one paper in this setting focusing on Ugandans finds significant effects from standard vocational training. Alfonsi et al. (2020) find significant positive effects from vocational training programs for Ugandans that increase over time and are largest at the latest endline in their study, after 3 years. Vocational training increased employment and earnings, and the authors estimate a 22% internal rate of return for the training if gains last for 15 years. The effects, though, are largely driven by the manufacturing sector; for service sectors—occupations like tailoring, hair-dressing, and baking which we expected would include a large proportion of Re:Build's sample—the effects of training on the index of employment outcomes are insignificant.

Takeaway for Re:Build RCT

The evidence on cost-effectiveness of vocational training programs was too weak to pursue further.

Apprenticeships

On-the-job training is another potential path to enhance livelihoods through skill development and long-term employment. These programs can be implemented via worker vouchers at businesses (i.e. Levinsohn et al., 2014), or as subsidies for firms to hire temporary workers. While take-up is often high, which leads to short-term effects on employment, McKenzie (2017) ultimately cautions that

> "accumulated evidence suggests that wage subsidies are unlikely to be very effective in generating additional employment under standard labor market conditions, and may also even not be very effective in playing a distributional role in determining which individuals get to access jobs."

In their review on employment interventions among young people, Kluve et al. (2017) concurred that the "effects of employment services and subsidised employment were negligible or statistically insignificant." Alfonsi et al. (2020), in the study discussed above on vocational training in Kampala, show large initial effects on employment and earnings from on-the-job training that fade over time, finding that "the steady-state effects on employment and earnings for VT (vocational training) workers are almost twice as large as those for FT (firm-provided training) workers." This is in part because only 24% of firms end up implementing the training, which is consistent with another study on wage subsidies in Jordan (Groh et al., 2016). Firm interest and quality appear to be key constraints; Hardy et al. (2019) found that an apprenticeship program in Ghana was only effective for trainees who were paired with more experienced trainers.

For refugees, apprenticeships and wage subsidies additionally offer a unique opportunity to overcome misinformation and employer discrimination related to working with a new, foreign population. Schuettler and Caron (2020) document positive case studies in high-income countries, though the evidence for refugees in lower-income contexts is scarce. More recently, Loiacono and Silva-Vargas (2023) find that Ugandan employers in Kampala who received subsidies to employ skilled refugees for one week are significantly more likely to have refugee employees after eight months. This effect is driven by a subset of "positive matches" where Ugandan employers and refugees each held initial attitudes that were more positive toward the other group at baseline.

Takeaway for Re:Build RCT

We explored the feasibility of apprenticeship programs, as the gains especially to refugees could be meaningful. However, the literature suggests an important criteria is the potential match quality. After light-touch scoping of employers, we determined that the likelihood of finding quality matches for 2,000 clients was too low to pursue further.

Job Search

Another set of interventions aim to make finding jobs easier by facilitating access to resources including job fairs, transport subsidies, and assistance with applications. McKenzie (2017) finds that only one of ten job search interventions studied had a statistically significant impact on employment. Among studies on refugees, Schuettler and Caron (2021) report promising evidence from high-income countries, but find the evidence for refugees in low- and middle-income countries "not very promising." In Jordan, Caria et al. (2021) randomize three job search interventions for unemployed Syrian refugees and hosts: 92 USD in cash to help offset search costs, informational assistance on job interviews, and a series of reminders and nudges to encourage participants to adhere to an application schedule. Cash led to the strongest effects among Syrians, including a 3.8 percentage point (70%) increase in employment after four months. They find suggestive evidence that the second program—assistance on job interviews—also increased employment for Syrians, but they find no evidence that any of the programs affected outcomes for Jordanians. Another intervention to facilitate employment for refugees could be an information treatment to potential employers explaining that hiring refugees is legal in Uganda, addressing an information gap documented in Kampala by Loiacono and Silva Vargas (2019).

Takeaway for Re:Build RCT

The evidence on cost-effectiveness of job search programs was too weak to pursue further. Additionally, cash grants intended for entrepreneurship may help facilitate job search for clients who decide they want to pursue wage employment.

Self-employment

Cash

Cash transfers of various sizes have been tested across a range of populations and outcomes. A number of studies explore the effects of small-scale multipurpose cash assistance, or offer transfers in the form of monthly stipends. This review focuses specifically on the impact of lump sums (at least 100 USD) which are most often targeted at people who want to be entrepreneurs on business-related outcomes (e.g. profit, business retention, or number of employees hired). In a 2018 review, Baird et al. study the effects of cash on labor market outcomes. They note five studies that find positive effects from one-time transfers to entrepreneurs on business profits, including in Mexico (McKenzie and Woodruff 2008), Sri Lanka (de Mel et al. 2008 and 2012), Ghana (Fafchamps et al. 2014), and India (Hussam et al. 2022).

While de Mel et al. (2012) observed sustained effects for at least five years, a few studies find that the average effects from grants dissipate after 1 to 2 years. For instance, in their experiment with women entrepreneurs in Kenya, Brudevold-Newman et al. (2017) find positive effects from cash at 7 to 10 months that decline after one year post-treatment. In Rwanda, McIntosh and Zeitlin (2022) find similar, large effects on profits and other business outcomes from cash transfers between USD 317 and USD 750. The effects, however, diminished over time. Haushofer and Shapiro (2016) compare lump-sum transfers with monthly transfers across nine months for households in rural Kenya and find "an increase in monthly revenue from agriculture, animal husbandry, and enterprises of $16 PPP relative to a control group mean of $49 PPP" but no effects from cash on business profits.

Among refugees in rural Uganda, Gupta et al. (2024) finds strong effects on business outcomes from an unconditional, one-time cash transfer of 1,000 USD. After 19 months, recipients were 37% more likely to own a business and generated 41 USD more in monthly revenues—a 65% increase—over the control group. Notably, they do not find any differences between male- and female-headed households. For internally displaced persons and hosts in urban Somalia, Abdullahi et al. (2023) find that one-time medium (500 USD) and large (1,000 USD) business grants affect business ownership after three years, relative to a smaller business grant of 175 USD, either given as a lump sum or paid out over two months.

A few studies do not find positive effects from cash on business outcomes. Among Ugandans in Kampala, Baseler et al. (2023) find no effects of a 135 USD grant on economic outcomes, though the grant was disbursed close to the COVID-19 lockdowns. In rural Kenya, Egger et al. (2022) find significant effects on wellbeing from a large, one-time cash transfer of 1,000 USD but did not find statistically significant effects on individual business profits. The sample for this study was representative and not targeted to entrepreneurs, but their findings on the general equilibrium effects of the large transfer are particularly noteworthy. The authors estimate a local transfer multiplier of 2.5, indicating significant positive spillovers on untreated households in this rural setting. In a randomized trial in semi-urban areas of Uganda, Fiala (2018) tested a cash grant of 200 USD against the effects of a similarly sized loan and finds no effects on business outcomes from the cash treatment. In this context and others, cash transfers were found to be less effective for women business owners (Baird et al., 2018, Brudevold-Newman et al., 2017, Fiala, 2018). This disparity may stem from limited access to business networks or information (Brudevold-Newman et al., 2017) which could potentially be addressed by cash-plus programs.

Takeaway for Re:Build RCT

The evidence on cash transfers, especially on medium-sized grants within the Re:Build budget, was promising overall. However, the evidence was less promising for women business-owners, suggesting cash supplemented with another intervention was the best bet to maximize cost-effectiveness.

Microfinance and Savings

Microfinance is another avenue to directly alleviate credit constraints for entrepreneurs. Reviews by Banerjee et al. (2015) and Jayachandran (2021) find that while microcredit may assist some businesses, it generally does not have significant positive effects on business performance. Blattman and Ralston (2015) note the high costs and short repayment terms of most microfinance programs, making them less suitable for investments in businesses that require longer maturation periods. In the same study on cash discussed above, Fiala (2018) identifies up to a 54% increase in profits from a loan program for male entrepreneurs in Uganda; however, among women business owners, he finds no effects on either the loan or cash treatment.

Savings programs provide formal bank account access or give information on saving and investing. In their review of savings programs in Sub-Saharan Africa, Steinert et al. (2018) find seven studies that report effects on business investments and profits. Pooling the studies together, they find a significant impact on business investments and profits but note that the programs tend to be less effective for women. In Tanzania, a randomized experiment offering mobile savings account access for women entrepreneurs did not find effects on business investment or profits (Bastian et al., 2018). However, those treated with savings account access in combination with a business training program were 4.6 percentage points more likely than the control to own a second business.

Takeaway for Re:Build RCT

The evidence on the cost-effectiveness of microfinance was too weak to pursue further. The evidence on savings programs was promising, and savings interventions were considered for testing. However, the team decided it was not feasible to design an additional intervention and incorporate it into the program launch event, which was already complex.

Business Training

Business training interventions aim to provide entrepreneurs with a suite of essential business skills. A recent meta-analysis (McKenzie, 2021) and subsequent update (McKenzie et al., 2023) of 16 studies found an average increase from entrepreneurship training programs of 6% in sales and 12% in profit. Previous reviews concluded that increased business knowledge did not necessarily translate to higher profits (i.e. Cho and Honorati, 2014). However, studies tended to use "relatively small samples and therefore lacked the statistical power necessary to rule out sizable positive (or negative) impacts of training" (McKenzie, 2021).

Many of these programs are classroom-based with wide variations in costs. McKenzie et al. (2023) document a number of modifications to standard training programs that researchers and practitioners are testing for more cost-effective approaches. For example, in Nigeria, Anderson and McKenzie (2020) investigated whether training microentrepreneurs in business skills is more effective than offering them free services like consulting or accounting. Their study showed that

while direct training had limited impact, providing external business services improved practices and profits for up to two years. Dalton et al. (2021) study the provision of handbooks of best practices combined with a documentary on successful microentrepreneurs and/or 30-minute visits by a local facilitator. They find little impact of the handbook alone, but a 35% and 21% increase in profits from adding the visit and all three interventions, respectively. They do not disaggregate the effects by gender but find no differences by the gender of the local facilitator. Overall, their results suggest that improved business practices can potentially be achieved with more cost-effective delivery than the traditional classroom-based, time-intensive delivery.

Takeaway for Re:Build RCT

The evidence on business training for entrepreneurs was promising. However, standard classroom training would have required a significant expense and Zeitlin et al. (2024), discussed below, recently evaluated training in a similar setting with the IRC. We therefore sought a more cost-effective version to deliver training, following on the models of Dalton et al. (2021) and a mentorship intervention.

Networking and Mentorship

Facilitating business networks could benefit entrepreneurs by creating a channel for information and skill exchange, or by providing role models and encouragement. Three experiments illustrate how interventions targeting networks can increase profits. Within a business plan competition in Ethiopia, Tanzania, and Zambia, Fafchamps and Quinn (2018) find that judges are more likely to adopt practices on VAT registration and banking when randomly assigned to committees with more judges who implement these practices at baseline. Cai and Sziedl (2018) find that inviting firm managers in China to monthly meetings of about 10 firms for one year led to significant increases in revenue and profits one year after the meetings ended. Brooks et al. (2018) found that matching women entrepreneurs in urban Kenya to more experienced business owners significantly increased profits in the short-term but faded out after about one year. They randomly assign other firms to a standard business training program, which shows no evidence of an effect.

Baseler et al. (2023) implement a similar one-on-one mentorship program for Ugandan microentrepreneurs in Kampala. Interest in the program was high, but the authors find no evidence of effects on profits from mentorship by either a Ugandan or refugee mentor, though business activity (and mentorship) was significantly interrupted by the COVID-19 pandemic. In Kenya, McKenzie and Puerto (2022) find no effect from adding mentorship onto standard business training. Finally, Bakhtiar et al. (2022) find minimal effects from mentoring by mentors trained in a traditional classroom program among women in Ethiopia. Nevertheless, for refugees, Schuettler and Caron (2021) write that "providing refugees and internally displaced people with networking opportunities might be promising."

Takeaway for Re:Build RCT

While the evidence on networking and mentorship is mixed, some evidence was promising, the potential to affect women and refugees was relatively high, the cost was low, and it offered designs to test questions of interest on potential business relationships between hosts and refugees.

Cash Plus and Graduation

Blattman and Ralston (2015) note that capital injections have demonstrated a capacity to encourage self-employment and potentially increase long-term earning potential, particularly when complemented with other low-cost interventions. Programs that combine cash grants or asset transfers with various skills training are called cash plus or, in some forms, graduation programs.

In Uganda, Blattman et al. (2016) studied a cash plus program (WINGS) that provided a 150 USD cash grant and a short business skills training program to a sample of mainly women. The program led to increases in employed hours and earnings, as well as business ownership: over a year after receiving the grants, 80% of treated participants owned a non-farm business relative to 37% in the control group. An additional encouragement treatment connected participants to form groups in the months following the business training program, which doubled earnings because "groups spurred informal finance as well as labor-sharing and cooperative cash cropping."

Zeitlin et al. (2024) measure the impacts of a cash-plus program in Nairobi among both refugees and Kenyans. The program compared the effects of 1) business grants alone, 2) grants and a short business training program, and 3) a bundle of grants, business training, saving education, and mentorship. All three programs had significant effects on employment status and productive assets relative to the control group but different effect sizes across treatment groups were largely indistinguishable. Similarly, McIntosh and Zeitlin (2022) discussed above found that business training programs had positive impacts on business outcomes relative to the cash arms but were otherwise less effective in most dimensions than cash transfers alone. A combined training and cash treatment also led to smaller gains than cash alone.

Graduation programs have been geared towards aiding the ultra-poor in rural settings. They generally include productive asset transfers (such as livestock) and some combination of skills training, saving support, or education on the assets they receive. A review by Banerjee et al. (2015) found graduation programs to be relatively consistent in their impact on poverty reduction but notes that they tend to be expensive (and potentially less cost-effective than other interventions) because of the comprehensive support they offer. Brune et al. (2022) sought to test various forms of the graduation approach in rural Uganda among both refugees and hosts. Their study included various levels of coaching and activity components and cash transfers. The lower-cost version of the

graduation approach, in which participants were coached in groups rather than one-on-one, showed similar effects to the more expensive version of the treatment and was therefore more cost-effective.

Takeaway for Re:Build RCT

The evidence on programs to complement the effects of direct cash transfers was promising and could potentially address some of the concerns with cash for women and over the long-term. Some of the graduation approaches were also promising though beyond the Re:Build budget and mostly tested in rural areas. However, the evidence on combining asset transfers with groups and training was informative for our design.

Other Interventions

Researchers and practitioners continue to delve into novel approaches to improve livelihoods. Our review is not exhaustive, and there are other interventions that merit mention despite having less research available to date. For instance, soft skills training teaches personal initiative and innovation—traits closely associated with entrepreneurial success—and positive results from Campos et al. (2017) in Togo are promising. Skills certifications aim to formally recognize an individual's competencies through assessments or references, with initial positive findings from Bassi and Nansaba (2022) in Uganda. Finally, an additional type of intervention assists with business formalization, as many microenterprises are not registered with government entities and may be "deterred by the financial costs and red tape of the application process or because they lack accurate information about the benefits of formalization" (Jayachandran, 2021).

Takeaway for Re:Build RCT

While some initial results are intriguing, none of these or other types of livelihoods interventions was supported with a sufficient body of evidence to include in the treatment bundle.

Summary and Takeaways

Overall, we find the evidence on livelihoods interventions in low- and middle-income contexts is quite mixed. Interventions—including within the same category—and their effects vary significantly, potentially depending on the context, sample, and implementation. The need to evaluate more programs across more contexts is apparent, especially within the refugee population with unique needs. In the aggregate, however, McKenzie (2017) argues that the evidence suggests these labor markets are generally efficient, and there may be less scope to correct market failures through outside interventions than is often assumed in program design.

Based on the evidence summarized here and the IRC's past experience, we decided that cash would be a central component for the design of the Re:Build RCT. However, we also concluded—in line with most reviews—that cash was likely not sufficient, especially for women and longer-term effects. While the evidence on mentorship is mixed, we believed an intervention modified for our context was the most promising to pilot that would allow us to test economic and social questions about relationships between refugees and hosts while maximizing expected inputs within the provided budget.

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Chapter 3. Piloting

Travis Baseler, Thomas Ginn, Grace Han, Ibrahim Kasirye, Belinda Muya, Reva Resstack, Andrew Zeitlin[1]

Introduction

Based on the evidence review, our clients' preferences, and the collective experience of the team, we designed the randomized controlled trial (RCT) around two key components:

- An unconditional cash grant
- A networking intervention including mentoring and groups

We wanted to test the effect of networking between refugees and hosts. In order to isolate the effect of the networking across nationalities from the effect of networking generally, we wanted to compare an intervention linking hosts and refugees with the same intervention linking within nationalities. Two additional questions emerged. First, the networking intervention could also be constructed to test connections across genders compared to within genders. Campos et al. (2018) suggest that male group members and connections can potentially help women entrepreneurs close the gender gap in business profits. Second, group-level incentives could lead to more positive effects within the networking intervention, especially across groups with more social distance like refugees and hosts.

We then designed a pilot to determine how to implement these components in our setting. The pilot is based on a small sample of clients and is specific to our context and purposes. Nevertheless, we found the pilot to be an invaluable tool for scaling up the programs from about 100 people in the pilot to 2,600 in the randomized controlled trial.

1. Authors are listed alphabetically.

Suggested citation: Baseler, Travis, Thomas Ginn, Grace Han, Ibrahim Kasirye, Belinda Muya, Reva Resstack, and Andrew Zeitlin. "Chapter 3. Piloting," in *Designing a Randomized Controlled Trial on Livelihoods for Refugees and Hosts: The Case of Re:Build in Kampala*, edited by Thomas Ginn. CGD Note 356. Washington, DC: Center for Global Development, 2024. https://www.cgdev.org/publication/designing-randomized-controlled-trial-livelihoods-refugees-and-hosts-case-rebuild.

Questions

The pilot was intended to answer four main questions.

1. How should the networking intervention be structured?

One-on-one mentorship provides the most personalized advice and most flexibility for meeting times but carries a higher risk that the mentor is not an effective match and aggregates less total information than a group. In comparison, mentored groups, in which multiple mentees meet with one mentor, allow mentees to learn from each other in addition to the mentor. However, in group settings, mentors might focus their time on the most promising entrepreneurs at the expense of others' issues. In both of one-on-one and mentored group models, the mentors with at least three years of business experience were recruited from outside of the Re:Build pool of clients. In the third structure we tested, peer groups without designated leaders consisted entirely of Re:Build clients under 36 years old, though the experience profile still varied within the group. In these peer groups, clients designated a leader or rotated leading the meetings.

2. Could mixed gender networking interventions be implemented in a culturally appropriate and safe manner?

While the potential upside—helping women access business networks that are majority male—is important, we had concerns about how this intervention would be received in the community among both refugees and Ugandans, given possible concerns over perceptions of mixed-gender partnerships.

3. What kind of group-level incentives are feasible to implement?

The outcomes we chose to measure were designed to be simple enough to verify without distorting the measurement of our key outcomes and to incentivize additional effort within the pair or group. We measure whether a client's business is open—defined as owning relevant capital, working close to full-time, accessible to customers, and ideally with its own space, though some clients may work from home. The IRC staff observed this measure.

We wanted to frame group-level incentives as a bonus to both the client with the open business and their assigned pair or group. However, we also wanted to avoid negative effects if the client did not have an open business and, in effect, cost their assigned network. Therefore, in addition to limiting the size of the payouts, we also implemented the incentive as a lottery, where entry into the lottery was determined by business openness. The pair or group was only notified whether they won or lost the lottery, not whether their group members or mentee were entered. Missing out on a lottery payout, therefore, was not necessarily because their connection did not meet the criteria; it could be because their connection(s) were not randomly selected. The lottery also significantly reduced monitoring costs for the IRC staff.

4. How should the cash grant be distributed?

A larger payout early on (a week after the launch, for example) may expand clients' opportunities for initial investments. However, saving some payments until about six weeks until after enrollment may allow the client to learn more about the industry and build trust with their mentor or group members in order to employ some of the shared advice. In groups with and without a leader, the clients will know that other members of the group are receiving the grant and could jointly invest in collaborative projects.

Design

The pilot phase was an iterative process.[2] Mentors and mentees were selected in two cohorts, and short-term outcomes and process improvements were incorporated as soon as possible. We collected short surveys for structured data on main questions as well as qualitative interviews for additional depth.

Table 3.1. Pilot Sample Sizes

	ONE-ON-ONE PAIRS	MENTORED GROUPS OF 3 MENTEES	PEER GROUPS OF 4 MENTEES	MENTORED GROUPS OF 2 MENTEES
Cohort 1				
Mixed Nationality & Same Gender	6	2	2	0
Same Nationality & Mixed Gender	6	2	2	0
Total Pairs/Groups	**12**	**4**	**4**	**0**
Total Individuals	**24**	**16**	**16**	**0**
Cohort 2				
Mixed Nationality & Same Gender	4	0	2	2
Same Nationality & Mixed Gender	4	0	2	2
Mixed Nationality & Mixed Gender	2	0	1	1
Total Pairs/Groups	**10**	**0**	**5**	**5**
Total Individuals	**20**	**0**	**20**	**15**

2. See Dempster and Herbert (2023) for a discussion of the adaptive management practices more broadly within the Re:Build project.

We explored including only those who were aspiring to open a business for the first time, or those opening a business in a new industry, but this was a relatively small segment of the population and not feasible to verify. Similarly, we intended to create groups within the same industry, but this was also not feasible to achieve while incorporating demographic, location, and language constraints in a small sample. Half of the pilot participants received cash two weeks after the launch, while the other half received two transfers: half of the cash in the second week and half in the sixth week.

Findings

In this section we summarize our contemporaneous notes and takeaways from the pilot that were used to decide the details of the study design. The findings are based on regular short surveys conducted with the pilot sample, observations from the IRC implementing team, and a site visit by multiple authors of the study.

1. How should the networking intervention be structured?

Almost every mentee across both cohorts found their mentors' and/or group members' business experiences to be relevant and helpful. **When asked which group structure was ideal, 94% percent of mentees who responded, across all group structures, said mentored groups were preferred.** This was also reflected in conversations during a PI's site visit. Mentees said groups provided more ideas, perspectives, and potential partners. Mentors also mentioned that they wanted their efforts to reach more people. We saw returns to scale in translating, where co-nationals could help each other with language issues. We inquired about the downsides of groups, including whether splintering (especially by gender or nationality) occurred, whether groups were inhibited by the slowest learner, and whether coordinating schedules was a significant hassle. Clients did not think these were important issues.

One question was whether to have mentors—someone with more experience than the profile of the Re:Build sample—or peer groups. Among pilot participants, qualitative work suggested a theme of wanting a leader and more structure. While some leaders emerged in peer groups, it took more engagement from IRC facilitators to keep the conversation flowing. However, with either one-on-one or mentored groups, one downside is the possibility of a bad mentor—one who is uninterested, encounters an issue with transport or communication, etc. We observed this in one of the launch meetings with a mentor who clearly wasn't ready for this role. **The main takeaway was the need to effectively screen the mentors** by asking about past training or total business experience (not just with their current business) and including a subjective assessment of their motivations for wanting to be a mentor.

If we included mentors, an additional question was how to compensate them, especially as their characteristics—successful business owners over the age of 35—did not meet the agreed-upon inclusion criteria. In the pilot, mentors were initially compensated 240,000 UGX plus transport and potential winnings from the lottery, relative to the mentees' grant of 1,400,000 UGX. **The large gap in payouts between mentors and mentees was one of the most common concerns.** Mentors highlighted

Figure 3.1. Mentorship groups participate in their first meetings at the Livelihoods Resource Center during piloting

that their needs are often larger than those of mentees (e.g. larger families) even if their businesses are doing slightly better. After discussing the reasons for the disparities, mentors often suggested that they should get at least half of what their mentees are receiving.

We found it most appropriate to **group by nationality instead of refugee status**. For example, during the site visit, one "all refugee" group had four people; three Congolese refugees and one Sudanese refugee. When the Congolese group members would make specific references or use French, the Sudanese woman said she felt excluded. Groups of 2 and 2 for both similarities and differences seemed appropriate.

While some clients said they preferred group members from the same industry to discuss specific suppliers and skills, most (88%) said they appreciated learning from other sectors and were able to find new opportunities for collaboration (i.e. a photographer recommending a juice caterer for his hired weddings). There were no clear indications that same-industry groups would have high payoffs, while at least 2 people switched industries based on their group's feedback. The groups therefore may become mixed-industry regardless and ultimately to the group's benefit. **Generally, it appears that distance is a more important criteria to match on rather than industry.** Groups that operated closer to each other were more likely to check in on each other outside of the meeting.

The **most significant challenge in mixed-nationality groups is language.** For all of the mixed-nationality groups to be comparable to same-nationality groups, everyone had to be eligible to be randomized into either one. Essentially, this meant that speaking conversational English or Luganda was a requirement for the sample unless there were a substantial number of Ugandan French or Swahili speakers.

Another open question was how much to structure the conversations. Some structure could convey helpful information and save the mentors time but could also crowd out time for mentees to raise the issues they wanted most to address. We provided little structure for the pilot, and the majority of mentees who responded said they **preferred more structured curriculum provided by the IRC**. Mentors who created their own curriculum or brought their own reference materials to the meetings also preferred more structure from the IRC, as observed during the site visit. Some outline of the curriculum should be provided, while allowing the mentors to maintain flexibility to teach topics they deem important. This provides an opportunity to incorporate the positive effects of business training, as described in the evidence review, and deliver it at a lower cost, though with likely higher variance in quality relative to a classroom training program.

We were unsure how much engagement was reasonable to expect, or to ask for, from mentors. We found that most mentors were excited to be involved and looking for additional ways to make the program successful. For example, mentors in cohort 2 began organizing meetings among themselves. All 8 mentors who came to the mentor meeting during the site visit mentioned that they would appreciate a formal weekly mentor meeting. Mentors in attendance said they would appreciate having the opportunity to discuss the challenges they were all facing as mentors and having the same structure for their mentees, which reiterated the need for a curriculum.

2. Could mixed gender networking interventions be implemented in a culturally appropriate and safe manner?

Mixed gender and mixed industry groupings were overwhelmingly preferred. Some people said mixed-gender groups were preferred "because we deal with both genders when doing business so we should know how to interact with both genders to know their ideas" and even "because when ladies are alone in a group, there are always misunderstandings." A small percentage of participants expressed a preference for same-gender pairings, demonstrating the need to include information on mixed-gender groups in the registration period. Overall, 94% of mentees expressed a preference for mixed-gender groups. A few people chose not to meet at the mentor's business but instead chose a public location.

While most mixed gender one-on-one pairs were fine with the arrangement, one pair was not; both the male mentor and the female mentee expressed discomfort with being alone with the other. The mentee said it was intimidating to go to his business alone (but she would be fine to do it in a group), and the mentor said the mentee dressed provocatively and was afraid the community was asking questions. The mentee said she would have been more comfortable meeting at the IRC's office, but the initial script had suggested meeting at the mentors' business. They instead found space at a local church, but they stopped meeting by the time of the interview and gave different reasons as to why. This was another reason why groups were eventually preferred to the one-on-one model.

3. What kind of group-level incentives are feasible to implement?

Spot checks for openness should be interpreted generously, as the goal is not to incentivize clients to sit at their business for when enumerators come by. The majority of mentees across all group types discussed and understood the lottery design. Most people perceived it to be fair as well, though we see a greater discrepancy in this measure between different group types in cohort 1—a much higher share of those in peer groups found it fair than those in one-on-one pairs. There is more of a consensus, however, that the lottery encouraged mentees to keep their businesses open.

However, it was clear from site visits that groups did not understand the intention of the incentive and lottery. People thought it was based on their own business and not that of their peers. The script did not adequately emphasize the details of the lottery. During the site visit, we ran a separate example exercise, which was much more engaging and yielded much higher comprehension. This exercise was adopted during the launch of the main study.

There was one client, however, who reported the high pressure that their mentor was putting on them to open a business and enter the lottery. This demonstrated the need to develop a protocol that details acceptable and unacceptable levels of pressure from mentors.

4. How should the cash grant be distributed?

The majority of participants in cohort 1 said they wanted the money early and in a lump sum (56%), though a sizable minority preferred multiple payouts. In cohort 2, most people said that they would prefer some of the cash grant at the beginning but the rest of the cash grant at a later date (59%). There were some interesting expenditures soon after enrolling in the program. For example, one person was paying a "broker" to help find a business location. Such an expense might be beneficial to buy soon after enrollment.

If people do not invest the cash disbursement in the first two weeks between the payment and the first lottery, it seemed more likely to go toward expenses unrelated to the business. Roughly 40% of mentees in cohort 1 and 18% in cohort 2 were open about using some of the cash for expenses outside the business. There was some discussion as to whether to suggest a budget for personal vs. business use; however, including personal use in the budget at all may condone more personal use than intended.

Summary

The piloting phase provided invaluable insights that allowed us to refine our complex research and implementation design. Piloting directly informed each of the questions and led to other ideas on how to make the project both feasible and effective. It was critical to collect surveys and qualitative interviews from pilot participants. Fewer clients, however, did not equate to less time from researchers and IRC staff. This phase, including generating the materials for the first time and creating different scripts for each group structure in the pilot, required a considerable investment. These materials and lessons then formed the foundation of the trial that launched three months later.

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Chapter 4. Research Design

Travis Baseler, Thomas Ginn, Ibrahim Kasirye, Belinda Muya, Andrew Zeitlin[1]

Introduction

This study is a randomized controlled trial testing a cash transfer and group mentorship program for microentrepreneurs. The study has multiple treatment arms and a control arm, which will be offered the treatment after conclusion of the study. All treatment arms receive a cash grant after six weeks. Participants can also earn additional cash rewards that depend on the success of either their or their group members' businesses'. These rewards are paid out by lottery every two months.

Some clients are assigned to mentorship groups, consisting of 3 microenterprise clients and a mentor. For some groups, lottery payouts depend only on their individual business performance; for other groups, the payouts also depend on their group members' business outcomes. By giving group members a stake in each others' success, a pay-for-performance structure could encourage the group to invest additional effort in each other or disclose valuable information or techniques. We call this the "shared fate" model. Finally, some groups mix nationalities (2 Ugandans and 2 refugees of the same nationality) or genders (2 men and 2 women) to evaluate the value of heterogeneous groups compared to homogeneous groups in these dimensions.

The specific research questions are:

1. What is the effect of the cash grants, cash grants with mentorship ("basic mentorship"), and cash grants with mentorship and group-level incentives ("shared fate") on economic and social cohesion outcomes?
    a. How do these effects differ between hosts and refugees?
    b. How do these effects differ between men and women?

2. What is the effect of assignment to a mixed mentorship group (either mixed refugee and host or mixed genders) compared to a homogeneous mentorship group on economic and social cohesion outcomes?
    a. How do these effects differ between groups with the individual and group-level incentives?
    b. How do these effects differ between hosts and refugees?
    c. How do these effects differ between men and women?

1. Authors are listed alphabetically.

Suggested citation: Baseler, Travis, Thomas Ginn, Ibrahim Kasirye, Belinda Muya, and Andrew Zeitlin. "Chapter 4. Research Design," in *Designing a Randomized Controlled Trial on Livelihoods for Refugees and Hosts: The Case of Re:Build in Kampala*, edited by Thomas Ginn. CGD Note 356. Washington, DC: Center for Global Development, 2024. https://www.cgdev.org/publication/designing-randomized-controlled-trial-livelihoods-refugees-and-hosts-case-rebuild.

Cost-effectiveness is an important additional question for all of the interventions. The costing analysis is done by the IRC's Best Use of Resources team.

Sample

The sample of mentees consists of clients who currently run or want to run their own business. They have been recruited through partner organizations, advertisements, and word-of-mouth to come to the IRC's Livelihoods Resource Center (LRC). Clients should want to spend at least 20 hours per week managing the business and be the owner of the business, defined as the person who manages the business and keeps the profit. Since participants may be assigned groups with mixed nationalities, participants must speak basic levels of either English or Luganda, which is true of 88% of refugees and all Ugandans in the initial registration data. Mentees must be willing to commit 3 hours per week over the next six months to the project, including the weekly mentorship meetings and the potential for surveys. Finally, the mentees are selected to include an even composition of women, men, refugees, and hosts. Within these demographic constraints, clients are chosen based on location, industry, and nationality to facilitate match possibilities, and then randomly if needed to reach 2,000 clients. Clients must complete the registration and the baseline survey to be eligible for the services in the randomized controlled trial. Clients who decline either survey will be informed about other IRC services and replaced in the RCT.

Mentors are also recruited through partner organizations, advertisements, and word-of-mouth, and then selected from this drop-in list. Mentors should own their business and have at least three years of business experience in Kampala. As with potential mentees, potential mentors are asked whether they can commit 3 hours per week over the next six months. During registration, potential mentors will also be evaluated for their potential as an effective mentor, assessed subjectively by IRC staff. Like mentees, mentors are ultimately selected to include an even composition of women, men, refugees, and hosts, and the industries and locations will be selected to match the distribution of mentees. Mentors must also complete the registration and the baseline survey to be eligible to be a mentor. Mentors who decline either survey are informed about other IRC services and replaced in the RCT.

Study Design

The final study sample of mentees are randomly assigned to one of four groups:

0. **Control group:** Cash grants provided after 18 months
1. **Cash grant and lottery only**
2. **Basic mentorship:** Cash grant and mentorship, with lottery payments depending only on individual outcomes
3. **Shared fate mentorship:** Cash grant and mentorship, with lottery payments depending on individual and group outcomes

Then, microenterprise participants in group (2) and (3) are further randomly assigned to different levels of matched identity based on gender and refugee status:

i. Partial match: either same gender or same refugee status;
ii. Full match: same gender and same refugee status.

In parallel, mentors are randomly assigned to three groups:

M0. **Control group:** mentors identified and selected by the IRC as qualified, but did not receive an active placement with a participant
M1. **Basic group mentors:** paired with a basic mentorship group to mentor, receiving fixed payments to coincide with mentees' lottery payments
M2. **Shared fate group mentors:** paired with a shared fate group to mentor, with lottery payments depending on group performance

Mentorship Groups

The pilot showed most of the clients and research team preferred the three mentees with one mentor structure. Groups of four allowed for a diverse range of opinions and were small enough for everyone to talk about their specific context. Including a mentor as one of the four members provides structure and perspective that mentees appreciated.

The groups follow a structured curriculum based on the IRC's Learn 2 Earn business training program and other similar initiatives. Materials include a handbook with business principles and exercises, animated videos for the main topics distributed to all clients with a smartphone, and discussion questions for each week. Some weeks will have more material than others, to allow for discussion of other topics based on the group's needs and preferences. The discussion questions include topics on business goals and challenges, personal histories, and encourage the sharing of specific examples.[2]

Mentors meet once per month with each other and the IRC for the first four months to discuss progress and challenges.

Cash Grants and Lotteries

The cash grant of 2,000,000 UGX (525 USD) to the treatment groups is distributed six weeks after the launch, and clients are encouraged to invest the money in their business. The six weeks gives the groups enough time to build trust and learn from each other how to invest the money wisely, and the cash group enough time to plan their spending. The mentors receive a grant of 1,000,000 UGX, also paid in full after six weeks, and an additional 200,000 UGX at the end of the program. After two

2. The participant handbooks, which include the program description, explanation of the lottery, project timeline, information on the IRC, code of conduct, mentorship meeting guides, and consent forms can be found on the Re:Build website. The business training videos are uploaded here.

weeks, each treatment arm is given a transport stipend: 15,000 UGX for the cash arm, 200,000 UGX for mentees assigned a mentorship group, and 240,000 UGX for mentors.

All treatment arms (besides mentors) are eligible for the lotteries which are conducted every two months. Clients are asked to inform the IRC when they have opened their business, and an IRC enumerator then visits the business to collect information that informs the spot-check for the lottery winners. All businesses that are open by the lottery's deadline are eligible to win; the probability of winning the lottery in each round is approximately 1/3, depending on the number of open businesses entered. All lottery winners are spot-checked for openness. Enumerators look for any evidence the business is open and the client is investing significant time. Winning clients receive 75,000 UGX each lottery.

Some of the groups are randomized to receive lottery payments that are based on their group members' success, in addition to their own. This "shared fate" model is intended to incentivize the whole group – both the mentor and mentees – to invest in the mentees' business with their ideas and encouragement. For each of the clients' businesses in this treatment arm that wins the lottery and is successfully spot-checked, all 3 clients receive the 75,000 UGX payment. If all three mentees win, the mentees could therefore win 225,000 UGX each round. The mentors will receive a 100,000 UGX payment for each business that is open and could receive 300,000 UGX if all three mentee businesses are randomly drawn and successfully spot-checked.

In the cash and basic mentorship arms, mentees and mentors also receive a separate, unannounced payment of 150,000 UGX in month 3. The purpose of this lottery is to equalize the expected cash payouts across all three treatment arms and match the payouts that the shared fate mentees and mentors can get for their group members' performance. The individual incentive for mentees to remain open is the same across the three groups, with the 75,000 UGX payout in each round. This additional payment at month 3 is distinct in multiple ways: unannounced, independent of the business openness (there will be no spot-checks), and disbursed at a different time than the others to emphasize that this lottery is different from the rounds at months 2, 4, and 6. All mentors in the basic mentorship arm also get a fixed payment of 75,000 UGX at months 2 and 4, and 200,000 UGX in month 6 at the end of the program.

Timeline

The sequence of major events in the RCT is below. The data collection timeline is described separately.

- **Recruitment:** clients' contact information added to the list of "drop-ins"
- **Registration:** basic characteristics are collected, from which the initial sample will be drawn

- **Baseline:** the external data firm collects additional outcomes from all mentees and mentors, and the final sample is drawn and randomization completed

- **Launch (Week 0):** Clients and mentors are invited to the location on a specific date to match their treatment arm: i.e., "shared fate" groups will be launched together. Clients start by watching a video that explains the IRC, the program details, and connect the program to the refugee presence. They also receive a handbook with program information. The handbooks for mentorship groups also include the business training curriculum, and short videos for some lessons are available for download via a hotspot to save on smartphones where possible. The introductory video, handbook, and business training videos are available in 5 languages: English, Luganda, Swahili, French, and Somali. Clients can take one in their native language and one in English or Luganda to facilitate communication. After the introductory video, IRC staff members that speak the relevant language answer questions about the program and go through a skit with examples of the lottery, which is different for the "shared fate" group than the other treatment arms. They also go through an example of the Interactive Voice Response (IVR) questions and save the IVR number in clients' phones to increase response rates. Finally, the mentorship groups meet for the first time to introduce themselves, discuss their business, exchange contact information, and decide on future meeting times and locations.

- **All subsequent weeks for six months:** clients in mentorship groups meet for unsupervised meetings once per week for approximately one hour

- **Weeks 1–8:** clients in the treatment arms inform the IRC when their businesses are open, and the IRC visits to collect information to be used for the subsequent spot-checks. IRC staff also conduct a short survey about the mentorship group, asking whether clients are comfortable with their group members, the meeting location, etc.

- **Week 6:** cash payouts

- **Week 8:** lottery winners are selected from all open businesses

- **Week 9:** lottery winners are spot-checked by IRC staff to ensure the business is open. Openness is defined as evidence that the client is working at least 20 hours on the business (i.e. there are materials and a location, the client is present when the IRC makes a random visit, or the neighbors know the business, etc. The 20 hours threshold was not directly verified). The important part is that clients believe they will be spot-checked before they can win the lottery payout.

The lottery and spot checks repeat every two months, or three times in total for the six-month program. For clients who are not open by week 8, they can still enroll in the next lottery by informing the IRC they have opened the business and having their information collected. Mentors meet at the LRC approximately once per month to discuss upcoming materials and strategies for overcoming challenges. At the conclusion of the six months, clients and mentors are provided a certificate of completion.

Data Collection

There are multiple rounds of data collection, coordinated across the IRC and the external data firm to minimize survey fatigue where possible. The first point of data collection is the registration survey, conducted by the IRC. These data are used to collect contact information and basic demographics and socioeconomic outcomes which is used to determine eligibility for the mentees and mentors, as well as stratification. This information is then shared with the external data firm, the Consortium for Research in Uganda (CRU).

Data collection for the RCT consists of the baseline survey and in-person follow-ups of approximately 1.5 hours at 3, 6, 9, 12 and 18 months following the start of the program. In addition, SMS and IVR surveys are used to ask about group meetings and business outcomes in the intervals between the in-person surveys. A small number of focus group discussions occur at 4 and 8 months after the start of the program. Finally, the IRC collects process data on whether respondents are comfortable with their group, plans for the upcoming cash payment, and any other concerns between weeks 1 and 8 after the program starts. The IRC also collects a satisfaction survey on a subsample of the treated clients at the end of the six months.

Data collection is always subject to consent. Microenterprise clients that do not consent to registration and baseline data collection stay registered with the IRC and enrolled with the IRC to potentially receive the services of other programs. Consent to subsequent data collection after treatments have been randomly assigned will not impact microenterprise clients' participation in the program.

Implementation

The IRC hosted 41 launch events for the trial's programs between July 2022 and February 2023. The control group was contacted by phone. In total, the sample consists of 2,000 clients and 600 mentors. By nationality, the sample is approximately 50% Ugandan, 30% Congolese, 5% Burundian, 4% Somali, 3% Eritrean, 3% South Sudanese, 2% Ethiopian, 1% Rwandan, and 1% Sudanese.

Clients were randomly assigned to the following interventions, with each arm having approximately the same number of Ugandans, refugees, men, and women.

Table 4.1. Study Sample Sizes

	TOTAL MENTEES	TOTAL MENTORS
Control	350	200
Cash	450	0
Basic Mentorship: Aligned	300	100
Basic Mentorship: Misaligned Nationality	222	74
Basic Mentorship: Misaligned Gender	228	76
Shared Fate: Aligned	150	50
Shared Fate: Misaligned Nationality	150	50
Shared Fate: Misaligned Gender	150	50
Total	**2,000**	**600**

Our pre-analysis plan, which includes the main survey questions, template for variable construction, and strategy to analyze the data is posted on the website of the American Economics Association (Baseler et al. 2023). As of January 2024, most surveys through the 12-month round are completed and data analysis is ongoing. We expect to release preliminary results in a working paper by May 2024.

References

Baseler, Travis, Thomas Ginn, Ibrahim Kasirye, Belinda Muya, and Andrew Zeitlin. "Re:Build: Cash grants and mentorship to strengthen refugee economic and social integration in Uganda." AEA RCT Registry, 2023.

The authors would like to thank Clare Clingain, Priscilla Dembetembe, David Musiime, Brian Ssebunya and an anonymous reviewer for their comments. We appreciate the hard work of the IRC staff to develop the project and implement the pilot, especially Shela Kabasele, David Musiime, Doris Nassuuna, and Barri Shorey. The authors are grateful to the IKEA Foundation for their role as a partner and funder of Re:Build.

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