3ie Impact Reference Number: DPW1.1023

Complementarities in microcredit and financial services interventions for financial inclusion and empowerment: evidence from randomised evaluation in South West Nigeria

Pre-Analysis Plan

Prepared by

Dr. Damilola Olajide

IEBDEM Nigeria

1. Study background

The intervention being evaluated is the Amoye Digital Microcredit Initiative for Women Empowerment program (ADMIWE). It is a complementary intervention, having both a demand-side (microcredit) and supply-side (digital banking services) components. The study is being conducted in collaboration with a microfinance provider, Amoye Microfinance Bank, Ikere Ekiti Nigeria (AMfB), which is the implementation agency. AMfB is using the evaluation as part of a learning process and strategy to reach the largely unbanked/underbanked women microentrepreneurs in Ikere community.

The study aims to assess the impacts of these complementary interventions, paying particularly attention to whether microcredit combined with digital banking services offers incremental impact on poverty-related outcomes over what the demand side alone (microcredit) would have offered. The overall goal of the intervention is poverty alleviation through two main sources; (i) increasing economic empowerment (including financial inclusion) of the beneficiaries, and (ii) reducing their vulnerability to shocks.

This Pre_Analysis Plan (PAP) outlines technical details of the study. The PAP as presented here follows the format provided by the project sponsor, the International Initiative for Impact Evaluation (3ie impact). This PAP included such information as research questions, research strategy including sampling method, fieldwork including details of quantitative and qualitative analysis, and details of empirical analysis

2. Research Questions

The main research questions to address are as follows:

(i) Does microcredit combined with digital banking services offer incremental impacts over what the demand side alone (microcredit) would have offered?
(ii) Do these impacts differ between economic empowerment and household vulnerability amongst the beneficiaries?
(iii) Do the impacts of the complementary interventions (if any) exhibit some pattern of heterogeneity (e.g. type of business)?
(iv) What is the nature and extent of externalities generated by the complementary interventions (e.g. on household members, local community)? How do these (if any), affect the overall impact of the interventions?
(v) What is the relative cost-effectiveness of the complementary interventions?

3. Research Strategy

The research strategy adopted for this study, including sampling and identification strategy have been jointly agreed with the officials of the implementation bank. The main research strategy is clustered randomized control trial with fixed blocking. Largely because of the need to capture spillover effects of the intervention however, we propose a Randomized Saturation (RS) design developed by Baird et al (2016). An RS design is a two-level clustered-type randomization design in which within-cluster random assignment to treatment follows some degree of intensity (saturation), which could range from 0% within-cluster random assignment of "saturation" of the treatment to nearly or full treatment (e.g. 99%) (see, Baird, Bohren, Mcintosh, and Ozler 2016, 2016).[1]

3.1. Sampling

The sampling plan for the study is in line with the implementation plans of the bank.

3.1.1. Sampling Frame

The eligible population are women entrepreneurs in Ikere community who meet the eligibility criteria as stipulated by the implementing bank. These are women participants who have been declared by the bank as eligible to receive the microcredit. As shown in Table 1, the eligible population comprises 4,464 or 76% of the total population of applicants who applied for the microcredit and met the eligibility criteria from the entire Ekiti state. Since the implementing bank limits implementation only to Ikere community, the actual eligible population comprises 3,822 or 86% of the eligible population who were from Ikere community. The remaining 1,407 or 24% of the total applications were rejected.

The main characteristics of the eligible population is that they are mainly women microentrepreneurs undertaking the microbusiness in Ikere community. Table 2 shows the ddistribution of eligible population from Ikere community across the five traditional clans and how women groups (clusters) are formed in them.

[1] We are grateful to the authors for their review, comments and approval for our proposed RS design.

Table 1: Details of women population who registered at Amoye Microfinance Bank

	Number	%
All registered participants	5,871	100
All eligible participants	4,464	76
Number of applications rejected	1,407	24
Eligible participants (Ikere community)	3,822	86
Eligible participants (outside Ikere)	642	17

Source: Participant Registration record (AMfB)

Firstly, the share of eligible population from Ikere community are almost equally distributed across the clans as the implementing bank has planned, ranging between 19.8% in Uro/Agbado/Anaye/Oyo clan and 20.4% in Okekere/Are clan. Secondly, the third column shows the number of possible clusters of at least 10-member women entrepreneurs across the clans. The fourth column shows the minimum number of eligible women entrepreneurs to be equally randomly selected from the number of possible women groups in each of the clans, as obtained from power calculation.

The number of eligible beneficiaries from the community are sufficient to generate equal number of women groups across the community clans. Overall, a minimum 375 women groups (women clusters) will be created amongst 3,822 eligible population of women entrepreneurs. As shown in Table 2 below, the number of women groups is just enough to generate sufficient power and sample size required within each community block that meet the needs of the implementation bank.

Table 2 Distribution of eligible population from Ikere community across the five traditional clans and formation of women groups.

Traditional Clans (Blocks)	No. of eligible beneficiaries from Ikere community	Share of eligible beneficiaries in total (%)	Equal numbers of women groups (clusters) to be created from eligible applicants
Okekere/Are	778	20.36	75
Araromi/Ilumoba/Odooro	771	20.17	75
Odooja/Okeosun	758	19.83	75
Uro/Agbado/Anaye/Oyo	755	19.75	75
Afawo/Kajola	760	19.88	75
Totals	**3,822**	**100**	**375**

Source: AMfB Record and authors' calculation

3.1.2. Expected sample size

In line with the implementing bank's plans and the outcome of registration of eligible beneficiaries shown in Table 2 above, the expected sample size is 3,750 (375 clusters x 10), comprising 2,000 in the two treatment arms (microcredit and microcredit + ATM services) and 1,750 in the control arm. This sample size is within the number of eligible beneficiaries (3,822) and as shown elsewhere, it guarantees a minimum statistical power. More importantly, it is in line with the implementation plans of the bank.

Moreover, there is very little difference between the sample size and the eligible population. Firstly, both the sample and eligible population comprise women entrepreneurs from Ikere community, and are similarly distributed across the community clans. Secondly, statistically, the mean age in both the sample and eligible population are almost the same (43.8 years and 44.1 years, respectively). These similarities are not surprising, since the sample size represents about 86% of the eligible population (see Table 1).

3.1.3. Statistical Power

The general nature of the experimental design is randomized clustered sampling with RS design to capture community level spillover effects of the intervention. It is on the basis of the RS design that the minimum detectable effect sizes (i.e. for treated and non-treated) are calculated.

Firstly, the key parameters or assumptions used for power calculation and in determining the number of clusters are as follow:

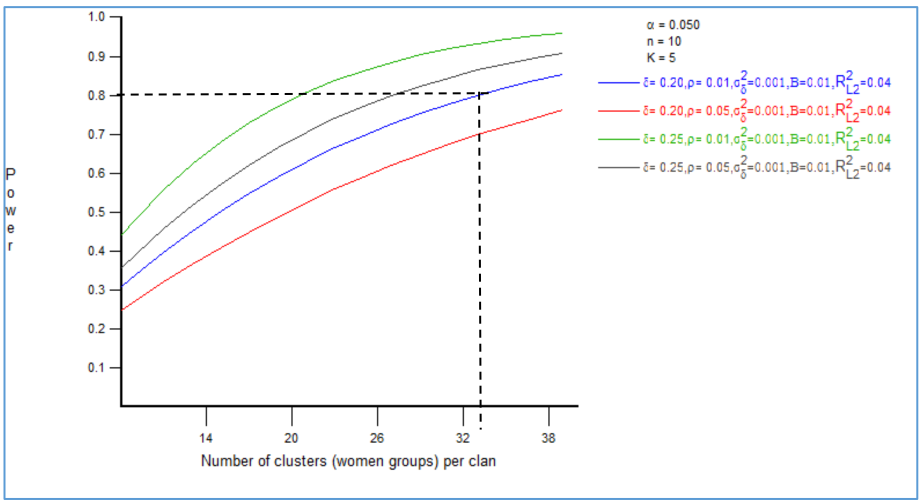
(i) There are 5 fixed blocks as defined by the traditional clans in the community.
(ii) Based on their previous estimates, the implementing bank expects variation in household income across the blocks to be around 0.1%;
(iii) Using data from our previous study on microcredit in this community (Olajide et al 2016), the coefficient of variation (R-squared) from a regression of household income on block-level variables and some controls is approximately 0.04;
(iv) The bank expects the minimum effect size to be detected, delta (δ) = 0.20 to 0.25
(v) When blocks are fixed, the effect size variability, σ^2_δ = 0.001;
(vi) Using data on household income in the community, the intra-cluster correlation, rho (ρ) ranges between 0.01 to 0.05;
(vii) The bank proposes min of n=10 eligible beneficiaries in each women-group (cluster), and n=2,000 women to be treated in the two treatment arms; and
(viii) Significance level (alpha) α =0.05.

The above parameters are used to calculate parameter *J*, the number of women groups (clusters) per block needed to generate an appropriate level of statistical power. Specifically, the interest is in calculating *how many clusters per block are needed to generate a power of at least 0.80*?

Figure 1 plots the graph of the result, using the Optimal Design software (Spybrook et al 2011). As the figure shows, for K=5 fixed blocks and the minimum effect size to be detected at 0.20 (with 0.1% effect size variability), significance level of 0.05, and the intra-cluster correlation at 0.01, we require at least *J* = 33 clusters (women groups) per block leading to a total of (33x5) = 165 clusters in total. Therefore, with at least 10 eligible beneficiaries per cluster and two equal-sized treatment arms, the minimum sample size required to generate sufficient statistical power of 0.80 is (165x10x2) = 3,300.

The required sample size is well within the number of eligible applicants (3,822), and it guarantees a minimum statistical power of 0.80. Thus, the sample is sufficient to detect effect size (20% increase in household income) at power of 0.80. More importantly, the calculated sample size complies with the implementation plans of the bank. In the RS design, these sampling results and parameters are used to determine the minimum detectable effect sizes when considering spillover effects.

Figure 1: **Power calculation to determine number of clusters**



3.1.4. Determination of effect size to detect in RS design

The RS design assumes a clustered structure with fixed blocking, where individuals are nested within clusters, and clusters nested within blocks. The effect sizes to detect in the RS design relate to Treatment effects or Intention-to-treat at saturation *p*, (ITT), comprising treatment effect on the uniquely treated and spillover effect on the treated); and spillover effect on the untreated (SNT).

The two effect sizes are derived as minimum detectable effect for the ITT, min_MDE_T and the minimum detectable effect for the SNT, min_MDE_S. The parameters used are:

(i) Number of clusters, C= 375;
(ii) Number of eligible beneficiaries within each cluster=10;
(iii) Expected power, γ =0.80;
(iv) Significance level, Alpha, α = 0.05;

(v) Rho (ρ) for outcomes = 0.05, 0.25, 1.00;
(vi) Within cluster variation, Sigma2 (σ^2) = (1-ρ);
(vii) Total number of beneficiaries is based on 10 eligible beneficiaries per cluster (i.e. C*n) =3,750; and
(viii) Weight applied = 1.

Table 3 shows the effect sizes to detect under the specific RS design adopted, using the Graphical User Interphase (GUI) software developed by Baird, Bohren, Mcintosh, and Ozler (2016: p.23). Given the above parameters, the table shows the minimum detectable effect for the ITT, *min_MDE_T* and the minimum detectable effect for the SNT, *min_MDE_S* for the estimated sample size and estimates of within-cluster variation, ICC = 0.05, 0.25 and 1 obtained from relevant outcomes from previous studies. As the tables shows, given both effect sizes detectable equal weights (weight=1), the min_MDE_T and min_MDE_S are 0.18, respectively. These correspond to the optimal share of the clusters at ICC=0.25, where n=1,750 and n=2,000 eligible beneficiaries to the control and two treatment arms, respectively. Also, the estimated minimum effects sizes detectable are consistent with the expected 0.20 effect size under the general design.

Table 3: Optimal Design to Detect Pooled Effects under a Partial population experiment

	min. MDE_T and MDE_S at different ICCs		
Rho (ρ) ICC	0.05	0.25	1
Optimal saturation 1: pure control	0	0	0
Optimal saturation 2: treatment probability	0.5	0.5	0.5
Optimal share 1: pure control	0.43	0.47	0.49
Optimal share in treatment saturation 2	0.57	0.53	0.51
Number of clusters (rounded)			
Optimal share: pure control	161	175	184
Optimal share in saturation 1	214	200	191
Total number of clusters	375	375	375
Number of beneficiaries			
Optimal share: pure control	1610	1750	1840
Optimal share in saturation 1	2140	2000	1910
Total sample size	3750	3750	3750
MDE_T	0.13	0.18	0.30
MDE_S	0.13	0.18	0.30

3.1.4.1. Assumptions about statistical power

1. High take-up rate: It is assume that there will be sufficiently high take-up rate.
2. Noncompliance will be less of an issue in this study. Compliance assumes that all of the eligible beneficiaries assigned to the treatment groups are treated and all of the eligible beneficiaries assigned to the comparison group will not get treated. We envisage that there may be partial compliance in Treatment arm 2 (microcredit plus ATM cards), as some of the beneficiaries might not actually use the treatment (ATM cards). It is also possible that some women in the control group may access the treatments by using proxies. The implication is that outcome measures in both groups would not depict the true effect of actual assignment status (treatment or control). Instead, it would show a confounded effect, which would yield biased estimates with low power because the study would be less likely to conclude correctly that the intervention had no statistically significant effect. The power decreases because the real sample size of the study reduces, and the standard error of the estimation of the treatment status of the women entrepreneurs increases.
3. Attrition will be at the barest minimum or insignificant to an extent of reducing power.

3.1.4.2. Assumption about variability in effect size

The main assumption here is that individual women microentrepreneurs within clusters are more likely to be identical relative to individual women microentrepreneurs across clusters. For RS design, this implies that the optimal sample size in saturation decreases as ICC increases. Also, there is fixed number of blocks, hence we expect constant block effects. We do not expect blocks to differ significantly.

3.1.4.3. Number of blocks and Number in each block

In the present study, there are fixed five number of sites/blocks which are traditional Clans into which Ikere community is divided are the blocks. Thus, the randomization process is repeated across the blocks. As shown in Table 1, there are 75 women groups (clusters) in each cluster and in line with the plans of the implementing bank, each cluster contains at least 10 eligible beneficiaries. Therefore, there will be 750 women entrepreneurs in each of the 5 blocks, making a total of (750x5=3,750) women entrepreneurs.

3.1.4.4. Sensitivity of effect size to changes in parameters

As can be observed in Table 3, the estimates of the minimum detectable effect sizes vary with different levels of within-cluster variation, ICC = 0.05, 0.25 and 1. Allowing the MDES to vary with different levels of ICC is consistent with the RS design. The MDES increases with higher ICC, suggesting that effect size increases as more variations take place across clusters. The ICC=1 implies that much of the variations take place across the clusters rather than within clusters.

3.1.5. Assignment to Treatment and control conditions

Figure 2 shows the structure of the RS design for identification strategy and how clusters are assigned into treatment and control arms. The identification strategy illustrates the two-level randomization procedure underlying assignment to treatment and control groups or clusters. As the figure shows, the first level follows a 3-stage random assignment. The first stage involves random selection of women groups (formed from eligible applicants) in each block to form the evaluation sample. The second stage will involve random allocation of the entire 375 clusters (women groups) into control and treatment clusters. This implies 175 and 200 clusters for the pure control and two treatment arms, respectively.

In the third stage, and in each block, the treatment group will undergo further random assignment into equal-sized Treatment cluster 1 (microcredit only, T1=100) and Treatment cluster 2 (microcredit + digital banking services, T2=100). The outcome of the random assignment in the first level is such that it preserves the implementation plans of the bank which target 2,000 beneficiaries in the two treatment arms.

The specific version of RS design to use in second level of randomization is the partial population experiment (PPE), "in which there is a pure control and a single treatment saturation at 50% is optimal" (see, Baird, Bohren, Mcintosh, and Ozler 2016: p.23).[2] The PPE approach is selected for the following reasons:

(i) It allows identification of both the ITT and SNT;
(ii) It minimizes the loss of power arising from trying to estimate the variance of treatment intensities;
(iii) The PPE at the ICC 0.25 best captures the implementation plans of the implementing bank; and;
(iv) based on previous results, it is believed that much of the variations in outcomes will be between clusters rather than within clusters, such that we might not see significant differences across levels of saturations.

As Table 2 shows, the pure control has, and the single 0% treatment saturation has 50% treatment. Hence, when ICC=0.25, the optimal share of the clusters is 47% (or 175 clusters) and 53% (or 200 clusters) for the pure control and treatment saturation, respectively. For n=10 per cluster therefore, these amounts to 1,750 and 2,000 eligible beneficiaries to the control and treatment saturation clusters, respectively.

3.2. Attrition from the Sample and steps and taken to prevent/remedy them

The ToC developed for this study allows for unintended consequences, which can manifest in three major ways, including drop-outs and outright attrition. Drop-outs from the program may arise from repayment defaults for example. Outright attrition may arise from (e.g.) low usage of digital banking services or outright stoppage. In agreement with the implementation bank, a 2% attrition is anticipated. According to the implementing bank, this anticipated share of attrition relates to their experience with customers who default in their loan repayments, either due to death, death of spouse, incapacity, or bad debts in the past 5 years.

Evidence from our previous studies also show that institutional or program design factors (e.g. interest rates)

[2] The authors have helped to jointly review our RS design and suggested the PPE design to us. PPE is more applicable where we do not expect to see significant variations in treatment intensities.

could be an important determinant of drop-outs and outright attrition. In Olajide et al (2016), we found that high interest rates, the mode and frequency of loan repayment constrained the extent to which clients could invest their microloans on income-generating activities.

Thus, it is anticipated that attrition may potentially arise from both the treatment arms and the control clusters. For microcredit only treatment, attrition may arise from default or early repayment leading to account closure. For the second treatment arm, some design and institutional factors such as infrastructure deficiency may result in outright attrition arising from non-usage of the digital banking services. Outright attrition can also occur in the control group, if they become untraceable at follow-up.

Whilst some level of attrition is anticipated, it is not expected to be significant, largely due to practical steps that are being taken in joint agreement with the implementing bank to prevent attrition. Four sets of steps that have been considered, are summarized as follows:

(i) Design-related steps. These include;

a. Oversampling. It is agreed that all the eligible beneficiaries be included in the evaluation sample, rather than just the n=3,750 effective sample size. Oversampling by about 5% should make up for the expected level of attrition.
b. Clustered sampling based on traditional administrative units (clans) and group-lending will ensure that potential drop-outs can be monitored and traced to their households and places of doing business. In the community of implementation, people take pride in the clans they belong. These sampling approaches will also reduce the level of attrition.
c. Sufficient contact information will be obtained at baseline survey, particularly those of 'next of kin' and two other people in the household, neighborhood or friend will be included in the survey questionnaire. Information will include mobile telephone numbers through which clients can be monitored. For example, clients are asked to give contact details of two relatives or next of kin who would be able to provide information about the beneficiary in the next two years. Their guarantors also provide a source of monitoring the clients.

(ii) The 'enablers' steps:

a. Client sensitization and continuous awareness campaign by the bank. The bank has initiated a mass sensitization of the targeted group that will be most impacted by the program, with the objective of creating awareness and provide an opportunity for them to raise issues and concerns.
b. Client education – knowledge of their rights. Field bank staff are trained to regularly engage clients during field visits and to provide feedback for follow-ups. For example, potential drop-outs are followed immediately there is a default or an indication of potential drop-out.
c. Client protection and opportunity for dispute resolution. Clients are encouraged to use the consumer protection departments at the correspondence banks, in addition to the dispute resolution arrangement at the implementing bank.

(iii) Institutional or program design steps include:

a. Multiple correspondent banks. As a way to reducing attrition due to non-usage of digital banking channels, the implementing bank now have correspondence arrangements with four additional banks in the formal banking sector whose digital platform will be used. Since ATM transactions can be made on the digital platform of a correspondent bank at no fees charged, increasing correspondence arrangements with more formal banks will widen access to users, increase usage, thereby reducing the risk of attrition due to non-usage.
b. Beneficiaries in the two treatment arms will access their microloans through their bank accounts, rather than cash disbursement. This will enable monitoring of the accounts directly by the bank and timely follow-up on drop-outs.
c. To reduce attrition in the control group, the clients will be informed of their 'awaiting list status' and are assured of receiving their microloans after the experiment.
d. Removal of access restriction on usage of digital channel. There are 253 bank branches in Ekiti State, with 1,112 ATMs deployment. Ikere community has 14 bank branches (excluding AMfB) and 25 ATMs deployment. Also, access to the ATM booths are good. Deployment are outside the banks, with no restriction on entry and there is disability facility.
e. AMfB is linked to the National InterSwitch Network, which connects all banks on the digital platform. Also, multifunctional ATM system is adopted, which are used worldwide for withdrawals, cash deposit, payments, and funds transfer.

(iv) Providing incentives to encourage take-up and usage: Agreements reached with the implementing bank also have implication for curbing attrition. These include;
a. Opportunity for multiple microloans. Beneficiaries who repay their microloans in short periods will have an opportunity to take another microloan as many times as possible during the experiment period. This will reduce drop-outs and outright attrition.
b. Repayment of the microloan include a compulsory saving component for the beneficiaries. The saving component will increase client retention, thereby reduction non-random attrition.
c. Higher loans for beneficiaries in Treatment arm B (microcredit + digital banking services). It is recognized that beneficiaries in the Treatment arm B will cover the costs of maintaining their ATM banking services, it was also agreed that the banks will cover the usage costs of ATMs such as when a client uses the digital platform of a non-correspondent bank. Amount of charges will be credited back into the account.

With the comprehensive level of practical measures being undertaken to mitigate the risk of attrition as outlined above, we do not expect changes in our power calculations, particularly when oversampling is also being considered.

3.3. Fieldwork

3.3.1. Instruments

The analytical approach in this study follows a mixed method namely; the quantitative aspect and qualitative aspect.

3.3.1.1. Quantitative aspect

The main instrument for quantitative analysis will be Survey Questionnaires designed to collect baseline and follow-up data from the eligible beneficiaries and their household members in both treatment and control arms. The Questionnaire will be electronically based, following Open Data Kit format on KoboToolBox server.[3]

The Survey Questionnaire for the quantitative analysis is developed, using instruments and information from previous studies as a guide, including:

(i) World Bank's Global Findex, which measures financial inclusion around the world (e.g. Demirguc-Kunt, Klapper, Singer, and Van Oudheusden 2015). The Global Findex provides comprehensive data showing how people save, borrow, make payments, and manage risk. According to Demirguc-Kunt et al (2015), the Global Findex is believed to be the world's most comprehensive set of data that provides consistent measures of the use of financial services across economies and over time in over 140 countries, including Nigeria. Several World Bank studies have used the instrument (e.g. Ruiz, 2013, Allen et al, 2012). We have also used the instruments in Olajide et al (2016).

(ii) Women Economic Empowerment Instruments, developed at the International Center for Research on Women (ICRW) (https://www.icrw.org/). Women empowerment instruments developed at the ICRW have been used extensively, as it recognises the multidimensionality of empowerment (e.g. social, economic and political) through which the capacity of women is enhanced towards making choices and to transform those choices into desired actions and outcomes. Examples of studies that have used the ICRW indicators include (see, Golla, Anne Marie Anju Malhotra, Nanda Priya, and Mehra Rekha, 2011), and Olajide et al (2016).

(iii) The Nigeria's Living Standards Measures Study of the General Household Survey-Panel developed at the World Bank (NBS 2017). The Questionnaire is mainly close-ended, comprising 120 questions divided into four main sections. The Questionnaire has also been translated into the local Yoruba language to ensure that enumerators from the survey vendor face no difficulty when they communicate with the women and their household members. Several studies in Nigeria have used the instrument, including Olajide et al 2016).

The main indicators that the Survey Questionnaire will cover include:

(i) Individual level indicators: include beneficiary characteristics (e.g. age, highest education attainment, marital status, type of business), asset ownership, income, sources of income, etc.

(ii) Household member indicators: include household member characteristics (e.g. age, sex, education level, working, occupational status).

(iii) Household level indicators: include household income, source of household income, household size, number working, living conditions, etc.

[3] KoboToolBox enables collection of survey data on Android and other devices such as mobile phone and Tablets, online and offline (http://www.kobotoolbox.org/).

(iv) Financial inclusion indicators: in line with World Bank's Global Findex, these include, ownership of bank accounts – formal and informal financial institutions, saving, borrowing, payments, use of digital banking and financial services, insurance, etc.

(v) Economic empowerment indicators: include contribution to household income, decision making in household, ownership of productive asset, contribution to household income/expenditure, and networking, community activities & self-confidence.[4]

(vi) Household vulnerability indicators: exposure to shocks and type, child labor, health service demand, household asset, and food shortage in household.

3.3.1.2. Qualitative aspect

The qualitative aspect will involve stakeholder engagement workshops and the key instruments will include;

(i) Focused Group Discussions (FGDs), featuring the key stakeholders, including representatives of program beneficiaries, policy makers, the media, etc.
(ii) Interactive and in-depth interviews – designed to follow-up on a particular issue of discussion, clarify ideas, check the reliability of data, and experience of field officers; and
(iii) Descriptive data analysis – to describe the data collected.

The main indicators of qualitative analysis to consider include:

(i) Program implementation: include perception of people about the program implementation, coverage, take-up, etc.

(ii) Perceptions of beneficiaries on the results from quantitative analysis: perceptions on key findings, reasons and options for unintended effects, etc.

(iii) Institutional and design indicators: include correspondence banks, usage of digital banking channels (ATMs cards), repayments, monitoring, etc.

(iv) Beneficiary perceptions on the usage of the microloan, perceived benefits from the program, etc.

(v) Issues and discussion points emerging from FGD.

The instruments for the qualitative analysis are also developed, using instruments from previous studies as guide. The instruments include;

(i) Stakeholder engagement workshop – featuring FGD and interactive interviews: Most researches undertaken at IEBDEM have involved a stakeholder engagement workshop, featuring FGDs with key stakeholders. In those workshops, the key findings from research are discussed amongst participants. These instruments have been used in our studies such as Olajide et al (2016) and Ikenwilo et al (2016).

(ii) Olajide, D, T. Ayodele, O. Sotola, and O. Saibu. Engaging Stakeholders towards a Productive

[4] Empowerment can also include financial inclusion as a key component.

Regulation of Payment Cards in Nigeria. *Main Report*, Initiative for Public Policy Analysis. Lagos Nigeria.

Nevertheless, the instruments will be piloted and field-tested before they are finalized for use in the impact evaluation. Piloting will test the format and phrasing options, the respondent understanding of the questions. Field-testing the instruments in real life situations is important for checking the timing, length and verifying consistence of the format with best practice.

4. Empirical Analysis

4.1. Definition of the main variables

Table 4 in the Appendix provides details of the variables to use in this study, including names, description and definitions of the variables to use in this study. The main variables and definitions are:

(i) Beneficiary characteristics: variables commonly observed in individuals that are fixed at a point in time. These include sex, age, marital status, occupation, and educational attainment.

(ii) Household vulnerability: defined in relation to not only the degree of exposure of members of a household to risks, shocks, and proneness to vagaries of daily life, but also the mechanisms through which households mitigate risks and cope with the effects of shocks (see, Swain and Floro 2012). Variables measuring household vulnerability includes exposure to shocks, child work, food shortage in household, health services demand, migration from household, and household assets.

(iii) Economic empowerment: is the power and agency that enhance capability of individuals to make economic choices and decisions to contribute to, participate in, bargain with, influence, control, and hold accountable institutions that affect their wellbeing (e.g. Golla et al 2011). Variables measuring economic empowerment include contribution to household income and expenditure, decision making in household, networking and community activities, and ownership and control of productive assets.

(iv) Financial inclusion: relates to access to and enhanced capability to use of formal or informal banking and financial services.[5] Variable measuring financial inclusion includes ownership of a bank account, using formal/informal financial services, saving, payments, insurance policy holding, financial knowledge and behavior.

(v) Household member variables: these are variables collected at the level of household members of beneficiaries. These variables are mainly characteristics of household members such as age, sex, education, occupation, marital status.

4.2. Balancing Checks

The validity of the estimates of the treatment effects requires that participants in both the treatment and control groups are similar in characteristics at the baseline (e.g. Gertler et al 2010). Balancing requires that

[5] Sometimes, financial inclusion can be considered as an indicator for economic empowerment.

the treated and control wards are similar at the baseline. For a given variable, the check will be carried out based on observing a statistical significance in the mean difference between the treatment and control groups, using clustered-adjusted *t*-tests (called *clttest*) (for continuous variables) and chi2 test (for categorical variables) (see, Donner and Klar 2000). Standard regression analysis such as ordinary least square (OLS) or Probit can also be used. The balancing check will be carried out on both beneficiary and household level data sets.

Since there are two treatment arms; Treatment 1 (microcredit only) and (Treatment 2) microcredit plus digital banking services, and one control group (pure control), the balancing test will be carried out at three related stages, namely;

(i) Balancing test between Treatment 1 and control group;
(ii) Balancing test between Treatment 2 and control group; and
(iii) Balancing test between Treatment 1 and Treatment 2.

Using clustered-adjusted *t*-tests in STATA software (STATA Corps. Inc.), for continuous variable *k*, the specification for the balance test is of the form;

$$clttest\, k, by(treatment_j)\ cluster(womengroup_s) \qquad (1)$$

where observations of *k* are clustered within women-group, *s*, and the comparison is made between the two groups identified by treatment *j*. This follows since the variable Treatment *j* (j=1,2,) is a binary variable taking the value 1 if treatment group *j*, and value 0 for the pure control. The option $cluster(womengroup_s)$ is a categorical variable denoting clusters of observations (beneficiaries). For categorical variable *k*, then the specification is;

$$clchi2\, k, by(treatment_j)\ cluster(womengroup_s) \qquad (2)$$

The variables to include in the balancing check are the baseline characteristics of the eligible beneficiaries (e.g. age, type of business, clan, family size, education, occupation, household asset, etc.) and where data are available, outcomes variables can also be included (e.g. household income, and indicators of financial inclusion, household vulnerability, and economic empowerment).

In both models, balancing test relates to testing the null hypothesis for variable *k,* is that the difference in mean of variable *k,* between the treatment *j* and control group is zero. This implies testing the hypothesis that the mean of variable *k* is the same between the treatment *j* and control group, such that the difference is zero: H_o*: mean(diff) = 0*, whilst the alternative hypothesis is that the difference in mean of variable *k,* between the treatment *j* and control group is different from zero. H_a*: mean(diff) ≠ 0*. Inference is given as student *t* statistic and *p-value* (*P > t = 0*). Statistical significance will be based on rejecting the null hypothesis.

4.2.1. Balancing check between attritors and non-attritors.

The importance of balance check between attritors and non-attritors in our study is to understand not only whether similar baseline characteristics explain attrition status in each of the treatment arms and the comparison group, but also to actually understand the predictor of attrition, which is relevant for program design.

Thus, a binary variable called *attritionstatus* will be generated taking the value of 1 if the individual has dropped-out, and value 0, otherwise. Then, for each treatment arm and control, the specification will be a probit model, the Stata format can be stated as:

$$probit\,attritionstatus\,i.treatment\ \ X_k,\ vce\,(cluster\,womengroup) \qquad (3)$$

Treatment is the treatment status of the individual, and X_k is a vector of covariates to be included, including baseline characteristics and key outcomes such as age, type of business, clan, family size, education, occupation, household asset, etc. The model to estimate included an error term, assumed to be independent and identically distributed (i.e. the error term and the X_k are uncorrelated). A 5% level of statistical significance is pre-decided.

4.3. Treatment Effects

The RS design adopted in this study implies that estimates of causal impacts will take into account both the treatment and spillover effects of the intervention, conditional on the degree of within-cluster treatment intensity (saturation) (see, Baird et al 2016). Hence causal (effect) of the intervention on treatment assignment or Intention-to-treat at saturation *p*, (ITT(*p*)).

In our application of the RS design, the (causal) effects to be estimated are as follows;

(i) Causal effects of treatment assignment or Intent-to-treat at saturation *p*, (ITT(*p*));
(ii) Spillover effects from treatment assignment on the untreated (SNT) - estimated by comparing the outcome of the untreated beneficiaries within treated clusters and pure control group.
(iii) Total causal effect (TCE) – is the overall effectiveness of the intervention on both the treatment and untreated beneficiaries. It is measured as the cluster-level effect of the intervention on clusters treated at saturation level *p*, compared to within-cluster pure control (i.e. 0% saturation), and applying saturation weights to both the treatment and spillover sides; and
(iv) Value of treatment to the beneficiary of receiving the treatment, VT(p). Measured as the direct impact of the treatment at a given saturation *p*, VT(p).

An empirical model to estimate the different (causal) effects stated above can be specified as follows;

$$y_{ic} = \beta_0 + \beta_{1p} T_{jic} + \beta_{2p} S_{ic} + e_{ic} \qquad (4)$$

where y_{ic} is an outcome of beneficiary *i* in cluster *c*, β_0 is the coefficient for the pure control group; T_j is the treatment status for treatment arm *j* (*j* = Treatment 1, Treatment 2); S_{ic} captures the spillover effects from treatment assignment on the untreated. In estimation, Eq. (4) will include X_k vector of baseline characteristics as controls to improve the precision of the estimates, and the standard errors will be adjusted for clustering. According to Baird et al (2016), OLS estimate of Eq. (4) yields unbiased estimate of β vector of parameters.

Statistical tests for the presence of treatment and spillover effects at saturation *p* are $\hat{\beta}_{1p} \neq 0$ and $\hat{\beta}_{2p} \neq 0$, respectively.

4.3.1. Intent to Treat

For a given treatment arm *j*, the Intent-to-treat at saturation *p*, (ITT(*p*)) – compares outcome of the treated beneficiaries and pure control group. Using specification in Eq (4), ITT(p) = β_{1p}. The X_k controls to include are baseline characteristics such as age, type of business, education, occupation, marital status, family size, etc.

4.3.2. Treatment on the Treated

Estimating the (causal) effect of the receipt of the treatment is necessary under situation of imperfect compliance. Situations that can bring about imperfect compliance have been discussed earlier. We will define treatment-specific 'compliers' and 'non-compliers' to the intervention. Defining 'non-compliers' is important to account for the difference between those who were assigned treatment and those who actually got treated. We expect some level of non-compliance in Treatment arm 2, particularly relating to usage of digital bank services, and in Treatment arm 1, relating to those who did not take-up the treatment eventually. For each of the treatment arm therefore, we will derive '*compliance rate*' for treatment arm *j* as:

$$\text{Compliance rate } j = \frac{\text{number of beneficiaries in treatment } j \text{ who actually received the treatment}}{\text{Total number of beneficiaries assigned to treatment } j}$$

Treatment 1 (microcredit only), we will use data on those who did not take-up the treatment eventually to derive 'compliance rate'. For Treatment 2, we will use usage data in digital transactions to derive the compliance rate.

Thus, TOT_j estimate will be computed by dividing the ITT(pj) estimate by the rate of compliance.

$$TOT_{(p)j} = ITT_{(p)j} / compliance\,rate_j \qquad (5)$$

Note that computing the TOT estimate this way will not change the *p*-values and thus the probability of rejecting the null hypothesis. The only effect would be on the coefficient value (see, Angrist, Imbens, and Rubin 1996).

An alternative way to estimate the TOT is to treat actual participation in the program as endogenous. For example, largely because not all the women microentrepreneurs are eligible due to eligibility criteria imposed by the implementing bank, compliance or participation in the program is therefore endogenous, as it is a function of observed and unobserved factors which may correlate with an observed outcome of interest. Thus, the TOT model can be explicitly specified and estimated to correct for this potential endogeneity, using an instrumental variable (IV) estimation. In the IV estimation, the treatment status of the beneficiary serves as instrument for participation. The treatment status serves as a valid instrument because it is generated from a random process that is unlikely correlated with other factors.

The Stata code for the two-stage least square (2SLS) model to estimate can be specified as;

ivregress 2sls y $\$X_k$ *(participation=*$\$X_s$*), cluster(womengroup)*

where *y* is the outcome of interest, X_k is a vector of characteristics included as controls, the variable 'participation' is a binary variable, taking the value 1 if the beneficiary actually received the treatment and value 1, otherwise. X_s is a vector of characteristics and the treatment status of the beneficiary.[6] The treatment status instruments for participation. The specification in (6) will be estimated using two-stage-least square estimator (2SLS), in which the specification in brackets is the first stage.

4.3.3. Other parameters to estimate in the RS design

From Eq. (4), the remaining parameters to estimate are;

(i) $\widehat{SNT(p)} = \hat{\beta}_{2p}$: Spillover effect on untreated beneficiaries. The sign on the coefficient of $\hat{\beta}_{2p}$ determines whether treatment create negative or positive externalities on untreated individuals;

(ii) $\widehat{TCE(p)} = p\hat{\beta}_{1p} + (1-p)\hat{\beta}_{2p}$: Total causal effect, arising from comparing the outcome of the untreated individuals within treated clusters and pure control group. In this specification, individuals not assigned treatment, either in pure control group or within treated clusters can only experience a spillover effect.

(iii) $\widehat{VT(p)} = \hat{\beta}_{1p} - \hat{\beta}_{2p}$: Value of treatment as defined above. $VT(p) < 0$ implies that the value of treatment to the beneficiary is decreasing in the share of other beneficiaries treated and

[6] The \$ sign is for global command in Stata used to define a vector.

spillover effects dominates treatment effect. $VT(p) > 0$ implies that the value of treatment to the beneficiary is increasing in the share of other beneficiaries treated. In this case, spillover effects complement treatment effects. $\hat{\beta}_{1p} \neq \hat{\beta}_{2p}$ determines whether the value to treatment is non-zero; and $\{\hat{\beta}_{1p} \geq 0, \hat{\beta}_{2p} \leq 0\}$ tests for offsetting/diversionary effect at saturation *p*.

4.4. Heterogeneous Effects

The main beneficiaries of the interventions are women microentrepreneurs in the local community, with limited access to formal financial services. One of the key research questions to address in the study relates to whether the complementary interventions exhibit some pattern of heterogeneity (Question 3). Addressing this question requires undertaking statistical analyses to examine whether the observed treatment effects differ by some baseline characteristics of the participants.

In line with the evaluation design, our approach will consider two aspects of heterogeneous treatment effects. First, we will identify the subgroups of beneficiaries for whom the microcredit program is relatively more beneficial. That is, how treatment effects vary across beneficiaries. Second, we will also examine how (causal) treatment effects differ across the treatment arms (Treatment 1 and Treatment 2).

The treatment effect heterogeneity will focus on contextual factors in the following key beneficiary characteristics and program outputs:

a. *Beneficiary characteristics:*
   - (i) Age group (e.g. under40 vs 40-and-above age groups)
   - (ii) Household income (e.g. by income quintiles)
   - (iii) Household size (e.g. large vs small household size)
   - (iv) Type of microbusiness (e.g. agricultural microbusiness vs. general trading)
   - (v) Location of beneficiaries (e.g. clan level)

b. Output variable:
   - (vi) Use of the microloan
   - (vii) Financial knowledge and behaviour

The literature and the findings from our previous collaborative study with the implementation bank (Olajide et al 2016) have informed the selection of these key characteristics and output for subgroup analysis. Participants in microcredit schemes tend to differ by these characteristics and 'use of loan', may impose differential treatment effects. In our quasi-experimental study (Olajide et al 2016) or example, we found that loan usage was an important factor mediating the impact of microcredit on indicators of household vulnerability. We will also examine whether the location of beneficiaries make a difference. Different locations may have different access to economic opportunities.

Another aspect of the heterogeneous treatment analysis will be a block level analysis to examine whether treatment effects differ across the five blocks. Generally, we do not expect treatment heterogeneity by clan. This is because the Ikere community is largely a homogeneous community in which the inhabitants share common characteristics and traits, and there are no clear differences in the level of economic activities across the clans.

In terms of modelling and estimation, we will focus on the treatment part of the basic model in Eq. (4). The interest is to estimate intervention effects for Z vector of subgroup variables, which could be a subset of baseline covariates X_k. To estimate the heterogeneous treatment effects, the basic model is specified by interacting the subgroup variables with the treatment indicators, T=T_1 and T_2:

$$y_i = \alpha + T\left(\gamma + \sum_{l=1}^{L} \theta_l Z_{li}\right) + \sum_{k=1}^{K} \beta_k X_{ki} + e_{i1a}$$

(6)

In Eq. (7), θ is a vector of parameters measuring heterogeneous treatment effects by subgroup of Z characteristics. For example, suppose that we want to estimate heterogeneous treatment impacts for agricultural and trading business types represented as a binary indicator taken the value of 1 and 0, respectively. Let trading business indicator variable be included in X and Z. The impact for agrobusiness type is γ, the impact for trading business is ($\gamma + \theta$), and the difference in impacts between the two business types (i.e. the heterogeneous treatment effect interaction) is θ. [7]

Standard *t*-tests and *F*-tests will be used to examine the statistical significance of these effects. In the above example, a rejection of the null hypothesis that θ=0 will suggest that business type imposed heterogeneous impact on the treatment effect or moderated the treatment effect. Also, an omnibus randomization inference test of statistical significance will be used to examine the overall impact across the treatment indicators.[8]

4.5. Standard Error Adjustments

4.5.1. Accounting for clustering in data

Accounting for clustering in our data is essential to adequately address the research questions and obtain more precise estimates of treatment effects. The power calculations we undertake ensures that we have the required number of clusters (C=375) for precise hypothesis testing. To account for clustering in our data, the regression that we run will adjust for clustering in the estimation of the standard errors, so that we obtained clustered standard errors. In Stata, clustering standard errors are obtained as option in model specification, as shown in Stata codes above (i.e. vce(cluster *clustvar*), where *clustvar* is the cluster variable - *womengroup*.

---

[7] To clarify, our model specification in (6) indicates heterogenous treatment effects rather than subgroup analysis.

[8] For details on the omnibus randomization inference test, see, Young (2016).

4.5.2. Addressing multiple hypothesis testing

Multiple hypothesis testing arises in the analysis since we are estimating models for multiple outcomes. Also, undertaking treatment effects heterogeneity creates problem of multiple hypothesis testing. We plan to follow two procedures and compare the results;

4.5.2.1. Bonferroni correction

The Bonferroni *p*-value correction procedure will be used to correct for multiple inference. The correction involves dividing the nominal alpha value (e.g. 0.05) by the number of subgroup parameters to give corrected alpha value. E.g. for an indicator that is statistically significant at the nominal alpha value of 0.05 and there are 12 outcome indicators in the estimated model, the corrected alpha value will be 0.05/12 = 0.0042. That is; $P<0.0042$ to be significant.

4.5.2.2. Construction of composite variables

An alternative but more favored approach to correct for multiple hypothesis testing is to aggregate multiple variables into composite variables or index. This approach is more consistent with the key outcomes in this study; household vulnerability, financial inclusion and economic empowerment. These outcomes are essentially multidimensional in nature (i.e. involving multiple measures), hence are best measured as composite variables or indexes. Indeed, the impact evaluation literature has favored this approach relative to undertaking post-estimation statistical tests. For example, see Olajide et al (2016), Ikenwilo et al (2016), Notenbaert et al (2013), Golla et al 2011, and Garikipati 2008.

Details of the construction of the composite variables are as follows. For an outcome *m*, the response to specific questions by the respondents will be used to construct an index based on points awarded for each question or a subset of response options within a question. The indexes for the primary outcomes are developed, using formula for developed by Notenbaert et al (2013).

For the m^{th} outcome (*m* = financial inclusion, vulnerability, and empowerment, the index *D* for the h^{th} household is calculated using the following formula (suppressing the subscript for indicator);

$$D_h = \sum_{i=1}^{H} w_{ih} R_{ih} \qquad (7)$$

where *D* = Index for household *h* (*h*=1, 2, ….*H*); *R*=response point for the i^{th} question, such that (+1, 0, -1); and *w* = weight attached to each question. In application, each response is given equal weight, so that w=1 for all responses.

Previous evaluation studies that have also used composite variables derived in this way include Olajide et al (2016), Ikenwilo et al (2016), Galiani et al (2014), Notenbaert et al (2013), Golla et al (2011), FANRPAN (2006).

4.5.3. Outcomes with limited variation

Outcomes with limited variation reduces power to detect any impact and has very limited use in econometric analysis as they cause noise in the analysis. Addressing this issue relates to limiting the noise caused by this kind of variable. Following suggestion by David Mackenzie (2012), variables for which 95 percent of observations have the same value within the sample will be omitted amongst the component variable that constitute an indicator or index. Indeed, the relevant variable will be excluded from the analysis altogether and reported as having limited variation.

References

1. Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin (1996). Identification of causal effects using instrumental variables. *Journal of the American statistical Association*: 444-455.
2. Young, Alwyn (2016). Channelling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results. *Working Paper*. Accessed June 1, 2017 from http://personal.lse.ac.uk/YoungA/ChannellingFisher.pdf
3. Garikipati, S. (2008): The Impact of lending to Women on Household Vulnerability and Women's Empowerment: Evidence from India. *World Development*, vol. 36 (12): 2620-2642.
4. Golla, Anne Marie Anju Malhotra, Nanda Priya, and Mehra Rekha (2011). Understanding and Measuring Women's Economic Empowerment: Definition, Framework and Indicators. International Center for Research on Women.
5. Baird, S., J.O. Bohren, C. McIntosh, and B. Ozler (2016). Optimal Design of Experiments in the Presence of Interference. Poverty and Applied Microeconomics Seminar Series, World Bank.
6. Rubin, D. B. (2004). Direct and Indirect Causal Effects via Potential Outcomes. *Scandinavian Journal of Statistics*, Vol 31: 161–170.
7. Demirguc-Kunt, Asli, Leora Klapper, Dorothe Singer, and Peter Van Oudheusden. 2015. "The Global Findex Database 2014: Measuring Financial Inclusion around the World." Policy Research Working Paper 7255, World Bank, Washington, DC.
8. Ruiz, C. 2013. "From Pawn Shops to Banks: The Impact of Formal Credit on Informal Households." Policy Research Working Paper 6643, World Bank, Washington, DC.
9. Allen, F., Demirgüç-Kunt, A., Klapper, L., and Martinez Peria, M.S. (2012). The foundations of financial inclusion: understanding ownership and use of formal accounts. *World Bank Policy Research Working Paper* No. 6290.
10. Notenbaert, A., Karanja S.N., Herrero, M., Felisberto, M., and Moyo S. Derivation of a household-level vulnerability index for empirically testing measures of adaptive capacity and vulnerability. *Regional Environmental Change* 13, 2013: 459–470.
11. Swain, B. R., and Wallentin, F.Y. (2007). Does Microfinance Empower Women? Evidence from Self Help Groups in India, *Working Paper* 24, Department of Economics, Uppsala University, 2007.
12. Golla, Anne Marie Anju Malhotra, Nanda Priya, and Mehra Rekha. Understanding and Measuring

Women's Economic Empowerment Definition, Framework and Indicators. *International Center for Research on Women*, 2011.

13. National Bureau of Statistics, NBS (2017).
14. Olajide, D., M.L. Alzua, and Dammert Ana (2016). Randomized Evaluation of the Unconditional Cash Transfer Scheme for the Elderly in Ekiti State Nigeria. *Working Paper* No. 2016-21, Partnership for Economic Policy, Canada.
15. Ikenwilo, D. D. Olajide, O. Obembe, N. Ibeji, and R. Akindola. The Impact of a Rural Microcredit Scheme Targeting Women on Household Vulnerability and Empowerment: evidence from South West Nigeria. *Working Paper No. 2016-01*, Partnership for Economic Policy, Canada.
16. CBN (2012). National Financial Inclusion Strategy. Summary report. Roland Berger Strategy Consultants, Abuja.
17. Karlan, D. (2007). Impact Evaluation for Microfinance. *Doing Impact Evaluation* No. 7. The World Bank.
18. Karlan, D. and Zinman (2011). Microcredit in Theory and Practice: Using Randomized Credit Scoring for Impact Evaluation. Science 332 (6035): 1278-1284.
19. Galiani, S. Gerthler, P, and Bando, R. (2014). Non-contributory pensions. *NEBR Working Paper No*. 19775, NEBR.
20. Food, Agriculture and Natural Resources Policy Analysis Network, FANRPAN (2006). Developing a statistical index - The Household Vulnerability Index (HVI) for quantifying vulnerability as a means of improving targeting of impact responses. *Southern Africa Trust*.

Appendix

Table 4: Description and definition of variables

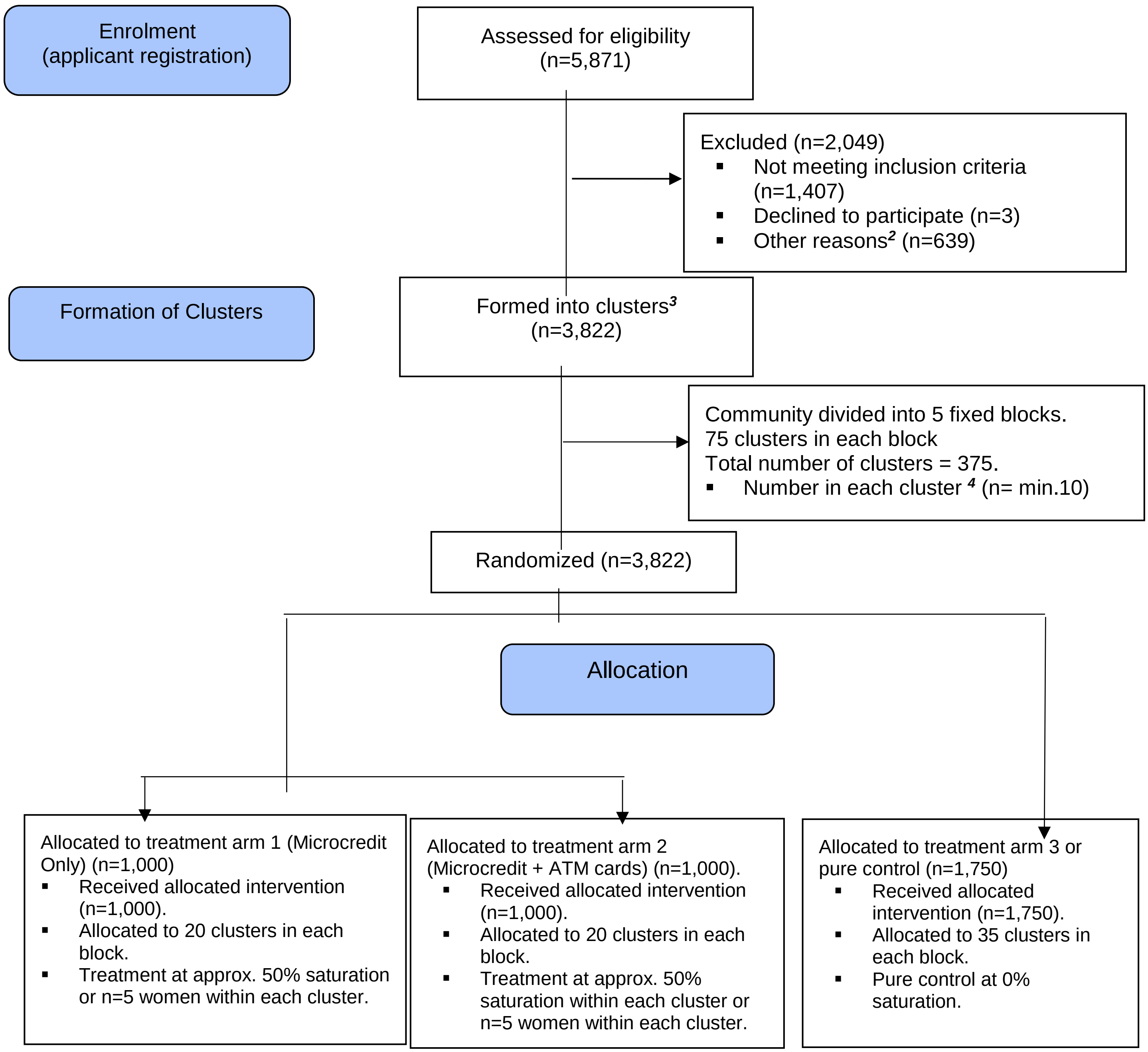
Variable	Description and definition
Beneficiary level variables	
Interview timing	
interview_start_time	time interview started
interview_end_time	time interview ended
Identification	
personal_id	Personal identification number
clan	clan or block

area	area or agboole
Household_number	Household number
GPS information	
GPS_latitude	_GPS_latitude
GPS_longitude	_GPS_longitude
GPS_altitude	_GPS_altitude
GPS_precision	_GPS_precision
Respondent characteristics	
hhold_size	Number of people living in household
currently_working	How many people are currently working in household
hhold_major_source_of_income	Household's major sources of income
amount_of_income_received_ last year	How much income did you receive from salaries
source(s)_of_income_received_ last year	Source(s) of income receive from salaries last 1 year
Household vulnerability indicators	
household_assets	Type of Household asset
value_of_household_assets	Value of household assets
child_work	Children or grandchildren (less than 18 years), male or female work to support family in the past 1 year
hhold_member_eat_less_than_3times	Any member of household who ate less than 3 times a day in the past week
food_shortage_in_hhold	Shortage of food to eat in household in the past week
how_meet_food_shortage	How household meet food shortage in household
health_services_demand	Types of health service(s) sought by members of your household in the past 1 year
payment_for_health_services	Who usually pay for the health services provided to household members in the past 1 year?
number_migrated_in_the_past_year	Members of household have migrated to a neighboring community in the past 1 year
number_returned_from_migration	Members of household who returned from migration in the past 1 year
copying_with_shocks	How household members cope with shocks event
Empowerment indicators	
contribution_to_hhold_income	Amount contributed to household income in 1 month
type_of_hhold_expenses_contributed_to	Type of household expenses contribute to
contribution_to_children_medical_expenses	Amount contributed to children medical / health expenses in 1 month
contribution_to_children_school_expenses	Amount contributed to children school-related expenses in 1 month

contribution_to_children_other_expenses	Amount contributed to children other expenses in 1 month
contribution_to_spouse_mdedical_expenses	Amount contributed to spouse's medical/health expenses in 1 month
contribution_to_spouse_other_expenses	Amount contributed to spouse's other expenses in 1 month
contribution_to_non_hhold_member_expenses	Amount contributed to non-household members expenses in 1 month
networking_activities	Ways undertaking networking activities in the past 1 year.
community_life_and_contribution_to_wellbeing	Feel community life and contribution to the well-being of others in the past 1 year
active_within_peer_group_and_friends	Have been active within my peer group and amongst friends in the past 1 year
Ownership of productive assets	
type_of_personal_asset(s)_purchased	Purchase own personal assets and type in the past 1 year, apart from family asset(s).
value_of_personal_asset(s)_purchased	Value of the types of the own personal assets purchased in the 1 year.
Decision making in household	
hhold_expenditure_decision_final_say	Household expenditure decisions having a final say in the past 1 year
decision_on_additional_income_in_hhold	Decision on additional income in household in the past 1 year
decision(s)_negotiated_with_spouse	Decision(s) negotiated with spouse in the past 1 year
Financial inclusion indicators	Access to and use of formal/infomal financial services
use_of_formal_informal_financial_services	Formal / informal financial institutions used in the past 1 year
ownership_of_bank_account	ownership and use of a bank account in the past 1 year
type_of_use_of_bank_account	Type of usage of bank account in the past 1 year
type_of_services_received_formal_ fin_institutions	Type of service(s) received from formal financial institutions in the past 1 year
frequency_of_business_transaction_on_bank_acct	Frequency of transacting business on bank account in the past 1 year
main_mode_of_access_to_formal_bank_acct	Main modes of access to a formal bank account in the past 1 year
saving_in_formal_financial_institution_type	Saving in a formal financial institution and type of financial institution in the past 1 year
amount_saved_in_formal_financial_institution	Amount saved in a formal financial institution

types_of_payments_made_on_bank_account	Type(s) of payments did you use a formal banking account for in the past 1 year
methods_used_for_receiving_or_make_payments	Methods used for receiving or making payments in the past 1 year
insurance_policy_holders_in_hhold	Number of insurance policy holders in household
Financial knowledge & Behavior	
familiarity_with_basic_financial_concepts	Familiarity with basic financial concepts
percception_of_barriers_facing_bank_account_o	Perception of the main barriers facing you from operating a bank account
Household member level variables	
Identification	
hhold_id	Household ID
hhold_index	number_in_hhold
hhold_member_name	household member's name
relationship_with_beneficiary	Relationship with eligible beneficiary
Household member characteristics	
hhold_member_sex	Household member sex - male or female
hhold_member_age	Household member age
hhold_member_marital	Household member marital status
hhold_member_education	Household member highest education or grade completed

Figure 2: Three Stage Random Assignment to Treatment and Control under RS Design -Revised [1]



Notes: [1] The style of this randomization flow diagram used example from CONSORT; http://www.consort-statement.org/consort-statement/flow-diagram; [2] Other reasons for exclusion include applicants from outside Ikere community of implementation; [3] A Cluster comprises at least 10 eligible women microentrepreneurs; [4] Some clusters have up to 13 women.