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# Uptake and evaluation of innovative insurance-embedded credit for promoting resilience and livelihoods among smallholder farmers in Kenya

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# Uptake and evaluation of innovative insurance-embedded credit for promoting resilience and livelihoods among smallholder farmers in Kenya

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## Summary

Weather variability—particularly deficient rainfall or drought—is one of the principal causes of Kenya's low agricultural productivity. New technologies provide an avenue to increase productivity and improve farm incomes, however many farmers are unable to access the credit needed to benefit from them due to involuntary quantity rationing (e.g., because of excessive collateral requirements) or risk rationing (e.g., because of information asymmetries). Consequently, many farmers in Kenya remain in a steady state of low productivity growth and are mired in persistent poverty traps.

To address these challenges, we work with a private sector lender in Kenya to introduce an innovative, market-based credit solution: namely, risk-contingent credit. This is a financial product that embeds within its structure an insurance contract that, when triggered, offsets loan payments, thereby providing a risk-efficient balance between business and financial risks.

Because this insurance coverage serves as a substitute for collateral, it is more financially inclusive than conventional credit and has the potential to bring quantity-rationed and risk-rationed farmers into the credit market, with expected concomitant benefits in terms of higher productivity and improved livelihoods. It is very important to assess the uptake and impacts of RCC loans on farmers' production decisions. We use mixed methods to evaluate the effects of RCC and to understand the mechanisms through which the effects are realized. We employ a randomized control trial to study the impacts of RCC as compared with traditional credit (TC) (a status quo comparison) and no credit (a pure control).

Data from a baseline survey and two follow-up surveys are used to investigate short- and medium-term patterns of RCC utilization and impacts relative to TC and no credit. Along with our baseline household survey, we identified credit constraints faced by participating farmers through a specialized survey, finding that only 10 percent of households are quantity-rationed, whereas 42 percent of households are risk-rationed.

The RCC mechanism is very relevant for this population, as it has the potential to bring these credit-rationed farmers into the credit market by acting as a substitute for more traditional forms of collateral. We compare the control group (no credit) with RCC farmers, then the control with TC farmers, and finally RCC with traditional credit farmers at baseline, separately to conduct a balance test.

We check for baseline covariate balances between and among the experiment groups to assess whether randomization was effective in delivering comparable groups. Our t-test results indicate that, aside from a few variables, there was no statistically significant difference between the control and either RCC or TC farmers. This indicates that the treatment assignment was balanced and therefore selection bias (common in small samples) was not a major concern in our impact evaluation, nor is there strong evidence of any potential confounding variables.

We analyze the empirical impact results of RCC and TC, comparing: (1) farmers who took RCC with the control households, (2) those who took RCC with those who took TC, and (3) those who took TC with the control households. To estimate the intervention's average treatment effect, we applied the local average treatment effect approach, with random assignment to treatment or control, as the instrumental variable.

We find that both RCC and TC had direct, positive and significant effects on agricultural investment—particularly fertilizer spending. They both seemingly did not have a direct effect on the ex-post productivity and welfare outcomes, but results from structural equation modelling reveal that such credits would enhance productivity and welfare indirectly through intermediary outcomes.

Also, compared to TC, RCC had higher ex-ante investment benefits; this was particularly high and significant in the adoption of chemical fertilizers. This indicates that access to credit, regardless of the type, may potentially have a positive effect on smallholders' investment, productivity, and welfare. Investigations for such effects must acknowledge that higher-level ex-post outcomes may be affected indirectly, and therefore the analysis of impact pathways is critically important to reach a meaningful conclusion. This is particularly true in areas such as Machakos County, where agricultural credits have been perennially absent, especially among smallholder households.

Moreover, the uptake of insurance-bundled credit was significantly higher than that of TC. This finding suggests that policymakers must ensure the provision of safe and inexpensive production credit products for agricultural households if widespread benefits of agricultural credit are to be realized.

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## Abbreviations and acronyms

| | |
|---|---|
| **ANCOVA** | Analysis of covariance |
| **CSI** | Coping strategies index |
| **HDDI** | Household Dietary Diversity Index |
| **IFPRI** | International Food Policy Research Institute |
| **ITT** | Intent-to-treat |
| **Ksh** | Kenyan shilling |
| **LATE** | Local average treatment effect |
| **RCC** | Risk-contingent credit |
| **RCT** | Randomized controlled trial |
| **SEM** | Structural equation modelling |
| **SMD** | Standardized means difference |
| **TC** | Traditional credit |

# 1. Introduction

Rural households from many low-income countries have identified unfavorable weather conditions as one of the most significant risks affecting productivity and household resilience (Cole and Xiong 2017). For instance, analysis of household risks from Malawi, (Giné and Yang 2009), Ethiopia (Dercon and Christiaensen 2011), and India (Cole et al. 2013) confirm that rainfall variability and shocks are the largest sources of risk to agricultural productivity and consumption.

Uninsured risks are also a major cause of low agricultural productivity in Kenya. Estimates published by the Kenyan government state that four consecutive years of drought (2008–2011) led to a loss of US$12.1 billion for the country, including lost assets and disruptions to the flow of economic activity across all sectors. Recovery from such severe shocks cannot be financed by the government and donor community alone. Moreover, lack of capital and perceived risk limit farmers' ability to access credit to purchase agricultural inputs, thereby contributing to perpetually low agricultural productivity.

Over the past two decades, index insurance has been promoted around the world to help farmers manage weather-related risks; however, this has largely been hampered by low demand (Cole and Xiong 2017; Jensen and Barrett 2017; Marr et al. 2016; Miranda and Farrin 2012; Binswanger-Mkhize 2012). Economists and practitioners are now focusing on new approaches to the design and implementation of insurance programs, which call for the bundling of index insurance with term credit. The primary objective of this is to reduce the high default risk faced by lenders due to exposure to widespread droughts and other natural catastrophes.

In this project, we are working with a private sector lender in Kenya to introduce an innovative, market-based credit solution, namely risk-contingent credit (RCC). RCC is a financial product that embeds within its structure an insurance contract that, when triggered, offsets loan payments, providing a risk-efficient balance between business and financial risks.

The concept of RCC was developed to remove liquidity constraints and indirectly increase agricultural insurance coverage. In dealing with poor agricultural farmers, the upfront payment of premium in standard insurance not only imposes liquidity constraints but also transfers income across time. Literature shows that a lack of liquidity and time preferences are significant constraints to insurance uptake. By removing both the liquidity constraint and the effects of climate risk, the RCC mechanism can better target poorer farmers.

Since embedded insurance coverage is effectively a substitute for collateral (no collateral is required from the farmers to access it), the RCC mechanism has the potential to expand credit access to quantity-constrained farmers and encourage otherwise risk-rationed farmers (including women farmers) to take loans without risking collateral. Since the indemnity from the embedded insurance is applied to the underlying debt obligation if triggered by adverse weather conditions, RCC will reduce the probability of default, build trust between creditor and borrower, and boost availability of and access to credit.

Further, because the probability of default is significantly reduced, the risk premium applied to RCC loans could also be reduced, thereby lowering the overall interest rate and the cost of borrowing, and potentially further increasing farmers' utilization of formal credit. RCC also eliminates the drawbacks of standalone index insurance products, as it does not require farmers to pay premiums upfront, which can also positively influence uptake.

High-quality evidence on the impacts of insurance-linked credit is scant, and we plan to develop short- and medium-term evidence on the impacts of RCC on agricultural productivity, resilience, and farm household nutrition. Identifying the causal effects of any form of credit is typically difficult because credit uptake decisions are usually correlated with observed (and potentially unobserved) household characteristics. Therefore, participants may be systematically different from non-participants, resulting in biased estimates of credit impacts (e.g., on farm productivity outcomes). This is why we have implemented a randomized controlled trial (RCT) at the individual level to create statistically identical treatment and control groups for comparing impacts.

# 2. Intervention

## 2.1 Description

The intervention under study is RCC, an innovative product for providing finance to households in rural Kenya, particularly those that are quantity-rationed or risk-rationed in accessing credit. In particular, the product embeds an index-based insurance product with a conventional credit product, thereby eliminating the need for borrowers to provide collateral as a condition for accessing credit.

As a result, access to credit becomes more inclusive. The insurance contract (APA Insurance) pays indemnities to the creditor (Equity Bank) upon the occurrence of an adverse event that might otherwise inhibit the borrower's ability to repay the loan, thereby reducing default risk. To the best of our knowledge, this study marks the first time this type of product has been tested and evaluated in a real-world setting. Nevertheless, the product has been rigorously developed and simulation exercises have demonstrated its effectiveness (Ndegwa et al. 2020; Ndegwa et al. 2022; Shee, Turvey, and Woodard 2015; Shee, Turvey, and You 2019; Shee and Turvey 2012).

During this study, treatment interventions were implemented in two phases, covering the traditional long rain season from mid-October to mid-January. The insurance-linked credit in the first phase (October 2017–January 2018) was based on Climate Hazards Group InfraRed Precipitation with Station data rainfall measures for the traditional long rain season in Machakos County from October 15 to January 15 (Shee, Turvey, and You 2019).

In brief, historical dekadal (10-day) rainfall data from 1981 to the present were collected for each of the 11 divisions of Machakos County. Cumulative rainfall measures were fit to a project evaluation and review technique distribution, with a cumulative rainfall "trigger" set at the twentieth percentile for each sub-county. Correlated Monte Carlo simulation was used to compute actuarial rates, assuming a Kenyan shilling (Ksh) 300 tick value for every millimeter of rainfall below the trigger. The tick value was determined by the amount required to pay off the loan principal in a worst-case scenario.

Although the trigger value and probability distributions differed by sub-county, the actual premiums averaged approximately 12% across districts/sub-counties. With a 25% loading factor imposed by the insurer,[1] the yield as a percentage of the loan amount was set at 14% for each sub-county, even though each subcounty had a distinct trigger against which indemnities were to be calculated.

At the end of the first phase season, the product suffered significant basis risk in terms of false negatives and intra-seasonal basis risk, warranting some improvements to better capture actual losses experienced by the farmers. For the second phase, we modified the insurance component of RCC to better represent the within-season patterning of rainfall.

To accomplish this, we re-engineered RCC from a seasonal cumulative rainfall model to a multiple-event model based on 21-day cumulative rainfall, setting the trigger at 50 percent of the historical average rainfall measure over the years and the tick value at Ksh 50. We define specific events as 21 fixed days that are overlapping in measurement but non-overlapping in indemnity, implying a multi-event dynamic trigger that can only indemnify once within a period of 21 days (Turvey, Shee, and Marr 2019).[2]

Up to four non-overlapping events, each paying an indemnity, make up the total indemnity. This structure resolves several problems that have plagued the implementation of rainfall-based index insurance in the past. It directly addresses the problem of intra-seasonal drought conditions while providing indemnities to rainfall across phenological growth stages across the growing season. Furthermore, it reduces the incidence of Type I (receiving an indemnity with no crop loss) and Type II (receiving no indemnity when crop loss occurs) errors.

In both instances (first and second phases), we captured loss measure by determining the difference between the actual rainfall and the trigger value by $Max(0, Z - R(T))$, where the trigger level is $Z$ and the level of rainfall for the specific period T is $R(T)$. To capture the economic loss, we multiply the above loss measure by a “tick” value, which was determined in consultation with the participating insurance company. The tick value was determined by the amount required to pay off the loan principal in a worst-case scenario.

Under this study, 702 households were offered credit (351 RCC and 351 traditional credit [TC]) during each round of implementation. To assess the demand elasticity of RCC, an additional subsample of 117 households was offered RCC with subsidies on risk premiums, distributed equally to 25%, 50%, and 75% subsidy groups. Since many farmers had no prior interaction with the local lender (Equity Bank), treatment farmers were invited to mandatory meetings where they received further training on the bank’s loan processes and, where necessary, opened a bank account. This was a bank-led process, with the bank applying conventional rules in due diligence to determine creditworthiness.

[1] Loading factor is the percentage added above the actuarially fair premium by the insurer to cater for administrative costs and their markup.

[2] We have also developed a multi-event product that considers crop water requirement/evapotranspiration as the triggering event for rainfall index insurance—a promising product, but yet to be empirically evaluated in the field (Ndegwa et al. 2022).

To ensure that the credit offered was used only in agricultural production processes, the loan amount was not advanced in cash. Instead, farmers were given vouchers that they used to collect specified farm inputs from preselected local agrovets within their communities. The maximum loan amount was set at Ksh 10,000 (approximately US$ 100), which was deemed sufficient to provide improved inputs for maize (and intercrops) production for one acre for one season.

For the RCC group, the Ksh 1,400 insurance premium (14%) was added to the base loan. The premium was automatically transferred to the insurance firm (APA Insurance). The lender also applied the standard interest rate loan, which was 14 percent per annum at the time. After input collection by farmers who agreed to accept the credit, the project and bank staff conducted follow-up visits to ensure they adhered to the process and to offer any support required. At the end of the season, the TC farmers were allowed up to three months to pay off their loans. The same would apply to the RCC farmers if the weather index did not trigger an insurance payout or if a partial payout was declared.

The intervention was implemented in Machakos County in Kenya's Eastern Province; this area was selected for our pilot interventions for two reasons. First, Machakos has semi-arid and hilly terrain that receives very low levels of annual rainfall (approximately 700 millimeters per year), with average rainfall in long- and short-rain seasons measuring 315 and 266 millimeters, respectively (GOK 2014).

The study area in Machakos where RCC operations are ongoing in 13 locations is depicted in Appendix C. Agriculture is practiced by smallholder farmers, with maize being the primary staple crop. We therefore expect RCC to be attractive and significantly beneficial to the smallholders in this county. Second, our key implementing partner, Equity Bank, has several branches in Machakos County, which can facilitate RCC disbursement.

The sample was randomly selected from 13 locations in five sub-counties (Matungulu, Kangundo, Kathiani, Mwala, and Yatta). In each location, we randomly selected six villages, and ultimately chose 15 households from a list of families in each village. The sample farmers satisfied two criteria: (1) they were interested in receiving a loan for agricultural investment purposes; and (2) they would meet the bank's loan appraisal process. We ultimately selected 1,150 households for our research design.

## 2.2 Theory of change

The planned theory of change of our project is presented in Appendix figure A.1. The question addressed is: Does RCC improve the production strategies and welfare of smallholder farmers by increasing investments in agricultural inputs or encouraging the adoption of new technologies?

The theory of change diagram links actual needs to programmatic inputs, expected outputs, outcomes, and long-term impacts. Therefore, the chain starts with the needs or problems, which are adverse weather (drought) and limited access to credit. This creates a need for a risk-transfer mechanism. We used a satellite-derived drought indicator combined with household crop yield information to design RCC.

Since the embedded insurance component of RCC is a substitute for collateral, it can attract risk-rationed farmers to the credit market. In RCC, the indemnity from the insurance reduces farmers' debt obligations when droughts threaten their agricultural production, thereby reducing default risk, improving risk-bearing ability and trust, increasing the uptake of RCC, and ultimately increasing investments in agricultural inputs. RCC does not require farmers to pay the insurance premium upfront; rather, farmers receive credit up front and their loan repayment depends on the production risk they face. Consequently, RCC can contribute to the high and sustained uptake of credit (and by extension, insurance) by farmers.

There are several explicit assumptions that underlie the various links in our envisioned impact pathway. First, we assume that there is a conducive regulatory environment for the implementation of RCC. This assumption ensures that the intervention complies with local regulations. Since the RCC product has already been approved by the Kenya Insurance Regulatory Authority, we do not feel this assumption is particularly risk-heavy.

Second, we assume that the research team will be able to maintain a productive working relationship with partners and stakeholders. This is ultimately important for successful implementation. The research team has considerable experience in working with several of the project partners and stakeholders, and we expect to maintain these relationships in a mutually beneficial fashion throughout the duration of this pilot.

Third, we assume that farmers will understand a product as complex as RCC; this will ensure that they are duly informed when deciding whether to take up RCC and will ultimately translate into increased agricultural investments. This is the link between outputs and outcomes. As mentioned previously, participants will be engaged in financial literacy training, and evaluations of this training demonstrate a fairly comprehensive understanding of RCC. Nevertheless, we will continue to monitor comprehension.

Fourth, we assume sufficient uptake of RCC. This is necessary to ensure our ability to detect a minimum desired treatment effect. We expect that many farmers have historically been quantity- or risk-rationed in their access to credit; therefore, introducing RCC and relaxing those constraints should be very beneficial for farmers, and thus we do not expect deficient demand.

Finally, we assume that governments will be amenable to policy and institutional change. This is necessary for taking RCC to scale following the successful culmination of this pilot. This may be a strong assumption, but one can hope that governments will be interested in policy reforms that improve their constituents' welfare, as we expect RCC to do, and so we expect governments to be amenable to evidence-based policy reform recommendations that emerge from this research.

## 2.3 Monitoring plan

The research team monitored implementation of the RCC intervention (including the financial education component), which was initiated in September 2017. This monitoring was conducted by tracking a series of activities undertaken by Equity Bank, based on a signed bilateral agreement between the International Food Policy Research Institute (IFPRI) and Equity Bank. This monitoring is carried out by Equity Bank and a field officer retained by IFPRI. The specific implementation activities being tracked include:

1. Delivery of RCC and traditional loans to maize farmers. The loans take the form of a purchase order for agricultural inputs (such as fertilizer, seeds, and pesticides) and the implementation follows the bank's usual appraisal process.
2. Building institutions and services to improve market effectiveness. Equity Bank has developed RCC distribution channels through its branches and village banking agents.
3. Supporting the project's research, monitoring, and evaluation effort.
4. Working with the project lead to determine the contract features which trigger the strike level (for the embedded insurance component) as well as the pricing of the RCC product.
5. Filing the RCC product with regulatory authorities for approval.
6. Working with the insurance agency and underwriter on product specification. Equity Bank also liaises with the reinsurer and negotiates for an agreeable price.
7. Branding the RCC product as one of the bank's own products, developing a marketing and publicity strategy, and preparing marketing materials.
8. Identifying and training sales agents on RCC and connecting government extension workers with these sales agents on the ground.
9. Improving financial literacy in the area by working with IFPRI and the Natural Resources Institute to develop appropriate financial training materials.

The field officer retained by IFPRI liaises between farmers, Equity Bank, and IFPRI. This individual's primary role is to support the project's action research, monitoring and evaluation efforts, collecting training and loan-related data, and improving financial literacy in the target area.

Through these monitoring activities, we were able to check implementation fidelity to assess whether implementation was conducted as intended. We followed three main steps: (1) implementation tracking - to tally the activities and process of the program implementation; (2) implementation assessment - to monitor the program by collecting data and making corrections to the process; and (3) implementation evaluation - to retrospectively determine whether the program is implemented as designed. Since our unit of randomization was individual households, we administered the randomization process at the household level, which prevented any spillover effect. Hence, the potential outcome of one unit should not be affected by the particular assignment of the other units, making our inference internally valid.

# 3. Evaluation questions, design, methods, sampling, and data collection

## 3.1 Primary and secondary evaluation questions

This project aims to evaluate the RCC intervention by exploring the following research questions:

1. Does RCC increase quantity- and risk-rationed farmers' access to credit?
2. Does uptake of RCC differ among farmers due to inherent differences in their preferences or observable characteristics?
3. How does RCC affect farmers' productive behavior and welfare (e.g., investments in modern agricultural inputs or new technologies), both in the short term (immediately following the intervention) and the medium term (three years post-intervention)?

4. How do these impacts differ from those of traditional loans or in the absence of loans altogether?
5. Do the impacts of RCC differ among farmers with different preferences or observable characteristics?

## 3.2 Evaluation design and methods

This study is an RCT comprising a control group and two treatment groups: RCC and TC. Identifying the causal effects of RCC and TC in a non-experimental environment is typically problematic because uptake decisions are usually correlated with observed and unobserved household characteristics; therefore, adopters are frequently systematically different from nonadopters.

An RCT effectively eliminates this selection bias by creating statistically identical groups for which differences in outcomes at follow-up can be directly attributed to the introduction of a policy or program intervention. We preferred individual randomization over a cluster-level randomization design because: (1) RCC is not likely to create competition in the area, and therefore we are not interested in measuring spillover effects; (2) individual randomization eliminates the need to adjust standard errors for intra-cluster correlation, and consequently provides better statistical power compared to cluster design (especially with only a few clusters); and (3) treatment would be administered at the individual household level.

To help examine the impact of RCC, 351 households were randomly assigned to each experimental group, where we compare the control households with RCC and TC households separately, and then RCC with TC households. The study also includes a small group (117 households) who were offered subsidies on premiums meant for estimating demand elasticity for agricultural credits and not for impact estimation.[3] The RCT protocol is illustrated in Appendix Figure A2.

At the onset of each phase of treatment implementation (September–October 2017 and September–October 2019), the selected farmers were invited to sensitization and training meetings on RCC, household finance, and credit management. These were conducted at the location/cluster level, where all 90 households were invited (either the household head or the spouse). During the first phase meetings, the households were also randomly assigned to the experimental groups.

This random allocation was carried out as public lotteries, wherein each participant would blindly draw a printed chip from an urn. The outcome would be announced publicly and recorded on the household list. Those who did not attend the meeting were proxied by the chiefs, sub-chiefs, village elders, or someone else who knew them.

A total of 27 chips were printed with “control,” 27 were printed with “normal credit” (TC), 27 were printed with “RCC,” and 3 chips were provided from each subsidy category (25%, 50%, and 75% subsidy on premium), for a total of 90 chips—equal to the number of selected households in a location. This was a transparent procedure to assign participants into the study treatments. Random assignment was performed once, and the groups were maintained throughout the study. As previously mentioned, the nine

[3] See Ndegwa et al. (2020) for these results.

households from each study location who were offered RCC with a subsidy for the premium are not included in the impact assessment.

### 3.3 Ethics

We received approval for both the baseline and follow-up household surveys from IFPRI's institutional review board and the Machakos county government, and informed consent from all study participants. The participants' involvement was voluntary, and they were told that they could withdraw from the study at any time. The study team informed them that all information would be kept on a password-protected computer, would not be shared with any third parties outside the project, and would be deleted at the end of the project.

One concern was that households randomly allocated to the control group would not be eligible for traditional loans through Equity Bank and therefore may not have access to the credit needed to make important investments in productive capital or agricultural inputs. This is not an ethical concern, per se, since allocation will be carried out randomly, rather than due to any form of discrimination by the research team or implementing partner. Since the research team has had to underwrite the loans, scarce resources have made it impossible to underwrite credit offers to those beyond the subsample of participants randomly selected into T1 or T2.

Furthermore, while those randomly allocated to the control group may be precluded from accessing loans from Equity Bank, they may still qualify for loans from other lenders; therefore, the intervention does not eliminate their access to credit.

Confidential data have been stored on secure, cloud-based servers available only to individuals directly engaged in this study and would not be shared with anyone not directly involved.

### 3.4 Sampling and data collection

This study is based on an RCT initiated in April 2017 in Machakos county, Kenya.[4] This county was purposively selected due to its dynamic yet mostly semi-arid agroecological zones, its sufficient coverage by the project's implementing financial institution (Equity Bank), its capacity to implement the project's interventions, and its proximity and accessibility to Nairobi. Machakos' population is also a good representation of the rural smallholder households who are the main target of the RCC intervention.

Five of its 11 sub-counties were selected in consultation with Equity Bank and provincial administration officials. Thirteen locations were then selected from these sub-counties. Sub-county and location selection was purposive, excluding urban and peri-urban areas and considering the bank's coverage and capacity to deliver the proposed products.

[4] At the inception stage, we conducted scoping missions in June 2016, wherein six focus group discussions were conducted in Machakos, Makueni, and Uasin Gishu counties. The aim of the missions was to determine the suitability and feasibility of RCC by assessing historical weather shocks (drought in particular) and their impacts on farming households. We also used these missions to document common farming practices, seasonal patterns, and average farm sizes in the areas. Makueni county was also considered a good fit for the study, but the presence and coverage of the project's implementing partner (Equity Bank) was not as high as in Machakos. Uasin Gishu county was not ideal for RCC since drought is not a major production risk there (and therefore drought insurance was not a viable intervention).

Later, at the onset of the baseline survey, six villages per cluster were randomly selected. This was done using a village sampling frame comprising lists of villages per location provided by the area chiefs (the local administrators in charge of locations). Thereafter, sub-chiefs and village elders from the selected villages provided the research team with household rosters including the names of all households under their jurisdiction. Fifteen households per village were randomly selected from the rosters, giving us 90 households per location, and 1,170 households in total.

Appendix Figure A3 presents the RCT's timeline, including data collection and intervention implementation. The study involved a baseline survey, two phases of project implementation (marketing of RCC and TC loans), and two follow-up surveys. The baseline survey was conducted in May–June 2017, followed by the first phase of implementation in which input loans were offered in October 2017 for the long rain season (October 2017–January 2018).

The first follow-up (midline) survey was conducted in May–June 2018, roughly eight months after the first phase loans were disbursed. Input loans for the second phase of implementation were offered in October 2019 to be used in the long rain season (October 2019–January 2020). The endline survey was planned for May–June 2020 (to maintain consistent timing), but this was disrupted by the COVID-19 pandemic, and the survey was postponed until August–September 2020.

Further, whereas baseline and midline surveys were conducted through face-to-face interviews using a long-form, structured questionnaire, the endline survey was conducted via telephone, and the questionnaire itself was revised and significantly reduced so the interview could be completed within roughly 30 minutes. However, we undertook extensive efforts to ensure that we tracked all key variables needed to complete the study.

# 4. Findings

## 4.1 Intervention implementation fidelity

Overall, 819 farmers were offered credit (TC or RCC). Despite a perception of heightened demand for credit, only 33 percent (230) accepted the offer of credit in the first phase, and 16 percent (105) accepted in the second phase. The difference in uptake between the two implementation phases was statistically significant at $p < 0.001$.

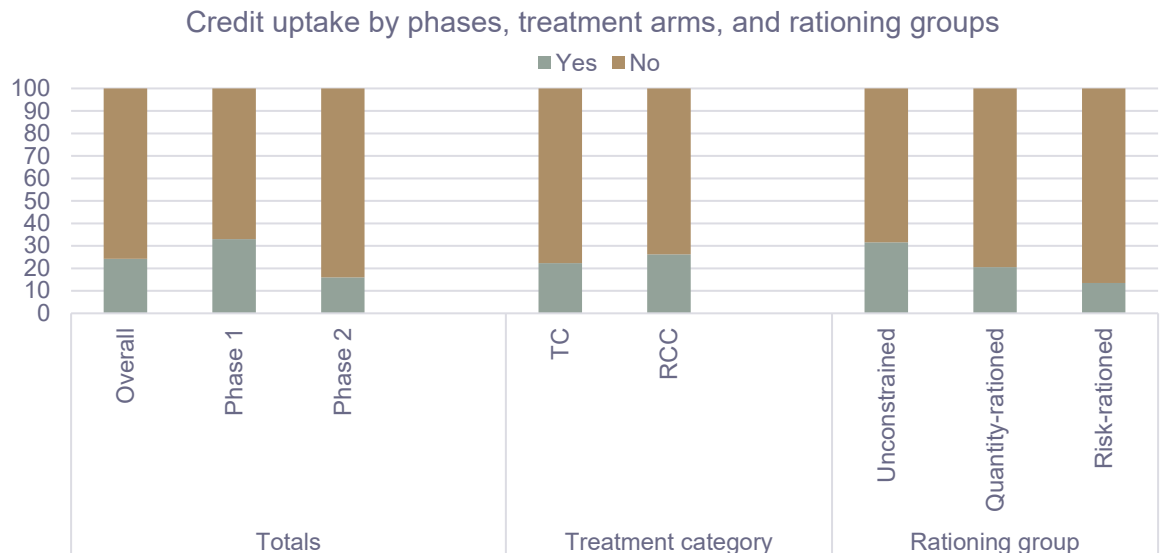
Two factors likely played a significant role in their decisions. The first was that the loans were restricted to agricultural practices and could not be used for any other purpose. The second factor could be delays (1–2 weeks) in loan disbursement and distribution of input vouchers, which may have discouraged some farmers from taking a loan. This view is supported by Duflo, Kremer, and Robinson (2011), whose study showed that the delivery timing affected the uptake of fertilizer, with early provision increasing uptake.

The difference in loan uptake was not substantially different among the treatments, with 22% uptake among TC farmers and 26% among the RCC categories (RCC without subsidies). The difference in uptake between TC and RCC was however marginally significant at $p < 0.1$.

Among the RCC farmers, those offered insured credit with a 25% subsidy on the premium had the highest uptake (37%), followed by RCC without any subsidy (35%) and with a 50% subsidy (35%). The lowest uptake was that of RCC combined with a 75% subsidy (23%). These results suggest that credit demand is more inelastic than we thought. An overriding consideration is that most participants had never before received a formal loan, and therefore there was significant pent-up demand for credit of any type. In other words, the shadow price of the liquidity constraints facing these farmers was sufficiently higher than the cost of credit even with the additional charge of RCC, which corroborates some existing literature (e.g., Barry, Baker, and Sanint 1981).

Further, we compare uptake among the credit rationing groups.[5] The unconstrained farmers had markedly higher uptake (32%) than quantity-rationed and risk-rationed farmers at 21% and 13%, respectively. The difference in uptake between unconstrained farmers and the other two categories (quantity- and risk-rationed) was highly statistically significant at $p < 0.001$, while the uptake difference between the two rationing groups was not statistically different (Figure 1).

**Figure 1: Up-take descriptive - by treatment categories and rationing group**




[5] We adapted and modified the direct elicitation method proposed by Boucher, Guirkinger, and Trivelli (2009) to capture households' rationing status. We asked the respondents a set of credit-related questions and used the responses to rate and classify them into three groups, namely: (1) **unconstrained** (also called **price-rationed**), consisting of farmers who either were being approached by banks to take credit, would normally receive the full credit they applied for, or did not require credit as they are not capital constrained; (2) **quantity-**rationed (also commonly referred to as **credit-constrained**), implying a supply-side rationing whereby farmers either receive less credit than they request, or they are denied credit altogether; and (3) **risk-rationed**, comprising farmers who excuse themselves from credit markets due to fear of losing collateral or other implications of defaulting on loan repayment.

As such, the identification method was conclusive, and indeed all households fell into one of the categories. Out of the total sample, 48% were unconstrained, 11% were quantity-rationed and 41% were risk-rationed. In the impact analysis section, we create a binary response variable equal to one for those who were rationed in either way (risk- or quantity-rationed) and equal to zero otherwise/if unconstrained.

## 4.2 Impact analysis

### *4.2.1 Descriptive statistics and balance tables*

We use the full sample means at baseline in Columns (1) of Table 1, below, to characterize the participating households at baseline. A majority (68%) used fertilizer on their maize fields, spending approximately Ksh 1,200 on average for maize fertilizer per acre. A total of 85 percent were already adopting improved maize varieties and put, on average, 76 percent of their maize fields under improved varieties. They on average had two acres of land under maize. Average maize yields were about 900 kilograms after adjusting for intercropping, earning them a revenue of about Ksh 7,000 per acre, which grows to about Ksh 13,000 per acre when revenue from the main intercrops (common beans and cow peas) is added.

They scored approximately six points along a 56-point coping strategies index (CSI) scale, which measures food insecurity severity by assessing the frequency of application of predetermined food insecurity coping strategies. This implies low food insecurity severity on average, although there were a good number of households in the sample who were severely food insecure with a score of up to 49.

The average dietary diversity score was about 11 points on a 15-point scale, which implies modest levels of consumption of diverse foods. About half of the households were credit-rationed in some way, either due to quantity (11%) or risk (42%). Most household heads were male (80%) with an average age of 56 years and eight years of formal education, indicating a low education level (primary school).

The average household size was roughly six, with a modest dependency ratio of 0.8 percent, implying about one dependent per worker. Households had an average of one acre per-capita land ownership and access. The average tropical livestock unit holding was seven, implying that all livestock in the household was equivalent to seven cows.

Households had subjective welfare mean score of 2.8 out of a possible 5, implying that they were just above average. Only 28 percent indicated having access to agricultural extension services, indicating very low extension services access among the households in the study area. The average risk aversion score (constant relative risk aversion coefficient) was 0.4, which indicates that on average, the households were moderately risk averse.[6]

[6] Constant relative risk aversion was computed from six risk aversion categories collected through a behavioral risk game. The six risk aversion categories are: 0 = extreme; 1 = severe; 3 = intermediate; 4 = slight to neutral; and 5 = neutral to preferring risk. To place the respondents into these categories, they were asked whether they wanted to participate in a risk game (by risking a Ksh 300 award they received from the survey) with gains and losses possibility, where gains increased with the risk level. Those who chose not to risk their cash are listed as extremely risk averse, while those who chose to participate in the game are categorized according to the risk option they chose. The categories were then converted into a 0–1 continuous/fractional variable following Binswanger's (1980, 1981) conversion coefficients. The sample was distributed as follows: 2% were extremely risk averse, 26% were severely risk averse, 24% were intermediate, 31% were moderate, 12% were slight-to-neutral, while 5% were neutral to risk (Appendix figure A.4).

Further, we conduct checks for baseline differences by running a series of ordinary least squares models, with respective outcome or control variables on the left-hand side and treatment assignments on the right-hand side. We start by comparing RCC households with controls and then TC households with controls, as shown in Columns (2) of Table 1. Later, as shown in Columns (3), we use the same strategy to compare RCC and TC households. The ordinary least squares results indicate that, apart from its dependency ratio, the control group was not systematically different from the treatment groups. Further, the two treatment arms (RCC and TC) were similar in all outcome and control variables. This suggests that the treatment assignment was balanced, and therefore selection bias was not a major concern.

Further, we test for joint orthogonality at baseline where we run a multinomial logit with categorical treatment assignment (0 = control; 1 = TC; 2 = RCC) on the left-hand side and the study outcome and control variables on the right-hand side. The results (not in the table) are consistent with the ordinary least squares models, where only the dependency ratio seems to be correlated with treatment assignment, and only marginally so, with a low statistical significance of $p = 0.1$.

The F-test for joint significance yielded a p-value of 0.977, implying that joint orthogonality can be assumed and therefore selection bias into treatments was absent. Nevertheless, for the sake of robustness, besides univariate models we also control for household heterogeneity at baseline and therefore run multivariate regression models with baseline controls. We also rely primarily on fixed effects models for our intent-to-treat (ITT) and local average treatment effect (LATE) estimations to take care of any endogeneity that may bias our results.

**Table 1: Baseline covariates balancing and joint orthogonality test**

| Variables | (1) Pooled sample | | (2) Treatments versus control | | | | (3) Comparing treatments | |
|---|---|---|---|---|---|---|---|---|
| | | | RCC - Control | | TC - Control | | RCC - TC | |
| | Mean | SD | Coeff | SE | Coeff | SE | Coeff | SE |
| ***Outcome variables*** | | | | | | | | |
| Binary maize fertilizer use | 0.68 | 0.467 | 0.003 | 0.035 | -0.023 | 0.036 | 0.026 | 0.036 |
| Amount spent on maize fertilizer – Ksh "000" | 1.20 | 1.356 | -0.032 | 0.103 | -0.074 | 0.103 | 0.042 | 0.102 |
| Binary improved maize variety adoption | 0.85 | 0.359 | 0.007 | 0.027 | -0.022 | 0.028 | 0.028 | 0.027 |
| Share of land under improved varieties | 75.92 | 36.789 | 0.712 | 2.757 | -1.709 | 2.834 | 2.422 | 2.818 |
| Area under maize | 2.05 | 1.981 | -0.044 | 0.156 | 0.016 | 0.142 | -0.060 | 0.152 |
| Maize yields "00" | 8.96 | 7.979 | -0.481 | 0.600 | 0.069 | 0.062 | -0.550 | 0.591 |
| Acreage overall revenue – Ksh "000" | 13.49 | 15.767 | -0.201 | 1.069 | 1.523 | 1.227 | -1.724 | 1.267 |
| Coping strategy index | 5.63 | 7.172 | -0.311 | 0.567 | -0.362 | 0.537 | 0.051 | 0.531 |
| Dietary diversity index | 10.56 | 1.687 | 0.023 | 0.124 | 0.011 | 0.13 | 0.011 | 0.128 |
| ***Socioeconomic controls*** | | | | | | | | |
| Risk-rationed | 0.42 | 0.493 | -0.020 | 0.037 | -0.030 | 0.037 | 0.010 | 0.037 |
| Quantity-rationed | 0.11 | 0.307 | 0.009 | 0.023 | 0.015 | 0.023 | -0.006 | 0.024 |
| Age of household head | 56.19 | 13.237 | 0.111 | 0.993 | -1.188 | 1.023 | 1.299 | 0.981 |
| Household head gender = male | 0.79 | 0.407 | 0.028 | 0.031 | 0.020 | 0.031 | 0.009 | 0.03 |
| Head's years of education | 8.62 | 3.868 | -0.071 | 0.299 | 0.026 | 0.292 | -0.097 | 0.286 |
| Dependency ratio | 80.14 | 77.175 | -10.441* | 6.146 | -10.822* | 5.893 | 0.381 | 5.393 |
| Per capita land ownership | 0.84 | 3.277 | 0.310 | 0.296 | 0.015 | 0.079 | 0.295 | 0.299 |
| Tropical livestock units holding | 7.13 | 60.935 | -1.999 | 4.605 | -1.104 | 5.175 | -0.895 | 3.949 |
| Extension services access index | 0.28 | 0.448 | -0.051 | 0.033 | -0.066** | 0.033 | 0.014 | 0.032 |
| CRRA coefficient | 0.40 | 0.23 | 0.004 | 0.018 | -0.001 | 0.017 | 0.005 | 0.017 |

*Notes: CRRA = constant relative risk aversion. Columns (1) report means and standard deviations for the pooled sample at baseline. Columns (2) report results from a series of ordinary least squares regressions comparing RCC households with control households, and then TC households with control households for each outcome and covariate variable. Columns (3) report results from a series of ordinary least squares regressions comparing RCC households with TC households for each outcome and covariate variable. SE is robust standard errors. For simplicity, Y-intercept, N, and other model measures from all the regressions are not reported*.

### *4.2.2 Testing for attrition bias*

Appendix Table A2 shows the attrition rate within each experimental group and overall, at both midline and endline. Of 1,053 households interviewed at baseline and enrolled in the study, we managed to re-interview 1,020 and 1,002 at midline and endline, respectively. This indicates a low attrition rate of 3 percent (33 households) at midline and 5 percent (51 households) at endline, respectively, which is comparable to other studies in the region (Jensen, Barrett, and Mude 2017; Matsumoto and Yamano 2011; Omotilewa et al. 2018).

Nevertheless, to test for random attrition or check for attrition bias in our sample, we generated a binary indicator for each follow-up round (midline and endline) equal to one if a household dropped out of the study at the respective survey round, and 0 otherwise. We then estimated a series of probit models with these indicators as dependent variables to analyze the association between attrition and (1) random assignment to control and treatment arms, (2) outcome variables, and (3) socioeconomic control variables for the baseline sample. This was performed separately for each experimental group and then for the pooled/overall sample. These results can be found in Appendix Table A4.

From Column (5) of Appendix Table A4, which tests for attrition bias for the entire sample, we found no systematic difference between attritted and remaining households for all treatment assignments, outcomes, and socioeconomic controls. When considering different treatment assignment groups, we do not find evidence that attritted households were systematically different from remaining households, apart from the dependency ratio. Control and TC households who left the study had a higher dependency ratio than their colleagues who stayed.

The low attrition rate among our treatment and overall sample, coupled with the failure to find significant systematic differences between the attritted and remaining households, suggests that attrition bias is likely not a major issue in our study. However, we used Lee (2009) bounds to verify that our estimates were robust to attrition.

### *4.2.3 Empirical framework*

Our primary objective in this study is to provide evidence of the efficacy of RCC to engender transformation and modernization of agriculture in Sub-Saharan Africa. To do so, we test whether RCC and TC yield ex-ante investment decisions (before and during the season) and ex-post productivity and welfare impacts on agricultural households in our sample area. For ex-ante impacts, we consider treatment effects on farm investment, specifically decisions to adopt improved maize varieties and chemical fertilizers, as well as the intensity with which these are used.

Further, to investigate whether the credit intervention influences agricultural intensification and/or extensification, we also estimate the effects of RCC and TC on the overall area under maize (whether improved or local varieties). For ex-post impacts, we consider household productivity and welfare. The outcome variables for productivity were maize yields and acreage revenue from maize and the main intercrops (common beans and cow peas). We consider the food security situation for households as the welfare indicator where the CSI and household dietary diversity index (HDDI) were outcome variables.

For all the outcome variables, we first estimate ITT effects, followed by LATE. In our case, ITT estimates the average effect of being randomly offered either RCC or TC,

whether one takes the credit or not. Credit uptake, or whether a farmer in either treatment group accepts or rejects a loan offer, is generally referred to as compliance in the RCT literature. Our protocol (and ethical guidelines) made no demands on the farmers to accept the loan offered; it was their choice to accept or reject it.

In this context, we experienced substantial imperfect compliance among treatment households, with average uptake of 32 percent and 16 percent during the first and second implementation phases, respectively. As such, ITT effects are diluted by noncompliance among the treatment households (Angrist 2006). However, ITT effects are still important, especially for policy formulation, since governments and development agencies are unable to enforce full compliance with their interventions (Duflo, Glennerster, and Kremer 2007; Ogutu et al. 2018). Further, it is imprudent to completely omit subjects who deviate from a treatment protocol, as there is not full confidence that the remaining groups are comparable (Angrist & Pischke 2008; Armitage 1979; Huang 2018; Soderbom et al. 2015).

On the other hand, LATE estimates the actual effects of taking the offered credit (RCC/TC) on the investment, productivity, and welfare of households that received it. As already indicated, this study suffered substantial imperfect compliance, and therefore we primarily rely on LATE estimates to assess the causal effects of RCC and TC on participating households.

To estimate both ITT and LATE, we employ three different estimators, which also serve as a robustness check. They are (1) standardized means difference (SMD), (2) analysis of covariance (ANCOVA), and (3) individual/household fixed effects. SMD and ANCOVA are estimated from post-intervention data (midline and endline). ANCOVA, however, controls for baseline heterogeneity in both household characteristics by use of covariates and in the baseline outcome of interest by including baseline measurements of the outcome variable as a control variable—hence, a more efficient estimator than SMD.

The fixed effects estimator, on the other hand, is estimated from baseline and post-intervention observations. It has an advantage over the other two estimators in that it removes the correlation between all predictors (treatment dummies and covariates) and the time-invariant individual unobserved heterogeneity. Fixed effects is a much more efficient estimator than both SMD and ANCOVA, with more reliable results.

**ITT**

We use the equation below to estimate the ITT effects of RCC and TC:

$$y_i = \alpha + \beta_{SMD1} RCC_i + \beta_{SMD2} TC_i + \varepsilon_i \qquad (1)$$

We first estimate this as SMD using the post-treatment observations, where $y_i$ is a vector of the observed outcome variables for household $i$ post-treatment; $RCC_i$ and $TC_i$ are household-level treatment indicators equal to one if a household was randomly assigned to the respective credit group (RCC and TC) and zero otherwise, and $\varepsilon_i$ is the idiosyncratic error term. The parameters of interest are $\beta_{SMD1}$ and $\beta_{SMD2}$, which capture the ITT simple means difference effects (that is, the effect of being assigned to RCC or TC, respectively).

Next, we estimate the equation using the ANCOVA approach, where we include selected baseline socioeconomic controls and control for baseline heterogeneity in the outcome variable, as shown in the equation below:

$$y_i = \alpha + \beta_{\text{ANCOVA1}} RCC_i + \beta_{\text{ANCOVA2}} TC_i + \beta_3 X_{i0} + \beta_4 y_{i0} + \varepsilon_{it} \qquad (2)$$

Where, in addition to the already described abbreviations, $X_{i0}$ is a vector of household socioeconomic characteristics at baseline that include: the head's gender, age, and education, and the household's credit rationing status, dependency ratio, risk aversion score, per-capita land ownership, tropical livestock unit holding, and access to extension services. Further, $y_{i0}$ represents the baseline measurement of the outcome variable, and hence is an autoregressive model. Here, the parameters of interest are $\beta_{ANCOVA1}$ and $\beta_{ANCOVA2}$ giving the ITT effects (that is, the effect of being assigned to RCC or TC, respectively).

We lastly use the individual fixed effect estimator:

$$y_{it} = \alpha + \beta_{FE1} RCC_i + \beta_{FE2} TC_i + S_t + \sigma_i + \varepsilon_{it} \quad (3)$$

Where $S_t$ is the time categorical variable equal to zero at baseline, to one at midline, and to two at endline; $\beta_{FE1}$ and $\beta_{FE2}$ are RCC and TC (correspondingly) ITT effects estimated with individual fixed effects; and $\sigma_i$ is the unobserved time-invariant household heterogeneity. RCC and TC are both equal to zero at baseline since there was not any treatment assignment and intervention prior to baseline data collection.

From all the equations, we test the null hypotheses that: (1) random assignment to RCC did not have any effect on the households' outcomes expressed as $\beta_1 = 0$; (2) random assignment to TC did not have any effect on the households' outcomes expressed as $\beta_2 = 0$; and (3) the difference between RCC and TC effects was not different from zero expressed as $\beta_1 - \beta_2 = 0$.

**LATE**

As indicated above, we had imperfect compliance in the study with 32 percent and 16 percent uptake rates in the first and second implementation phases, respectively. ITT is therefore an underestimation of the RCC and TC treatment effects on those who took the credit offered. We therefore estimate LATE by implementing the instrumental variables technique to account for imperfect compliance. We employ the two-stage least squares approach, where uptake of either RCC or TC is instrumented by random assignment to either RCC, TC, or control group, respectively.

In RCTs, random assignment to treatment and control groups is the ideal instrument that meets the validity, relevance, and strength conditions as shown in **Error! Reference source not found.** in the appendix (Abadie and Cattaneo 2018; Angrist and Pischke 2008; Huang 2018; Khandker, Koolwal, and Samad 2009; Soderbom et al. 2015).[7] In the first stage, uptake of RCC and TC is regressed on assignment to the respective treatment group and other socioeconomic covariates used in the second stage. The first-stage estimation equations used in Equations (4) and (5) predict uptake of RCC and TC, respectively:

$$RCC_{it} = \alpha + \delta_{Stage1} T_i + \beta_{Stage1} X_i + \varepsilon_{it} \quad (4)$$

[7] A valid instrument should only affect the outcome variable indirectly through another regressor. For it to be a strong instrument, its association with the regressor for which it instruments should be substantial and statistically significant. In this study, being assigned to a group that was offered credit would not directly impact households' farm investment, productivity, and welfare unless they took the credit offered. On the other hand, uptake depended on whether one was offered credit or not, and the type of credit that was offered. As such, treatment assignment could not directly affect the outcome variables, but it indirectly would only through uptake.

$$TC_{it} = \alpha + \delta_{Stage1} T_i + \beta_{Stage1} X_i + \varepsilon_{it} \quad (5)$$

We then estimate the LATE effects using the instrumental variable specification, as below, where we use the predicted uptake of RCC and TC in the second-stage equation to estimate the actual effect of uptake of the respective credits on the outcome variables. We start with a short model with both treatment arms (RCC and TC) only as the predictor variables, estimated from post-treatment observations as the simple means difference:

$$y_i = \alpha + \delta_{SMD1} \widehat{RCC}_i + \delta_{SMD2} \widehat{TC}_i + \varepsilon_i \quad (6)$$

Where $\widehat{RCC}_{it}$ and $\widehat{TC}_{it}$ are the first-stage fitted values of treatment, that is the actual uptake of RCC and TC (correspondingly) by household i. $\delta_{SMD1}$ and $\delta_{SMD2}$ are the instrumental variable parameters of interest, which give the LATE for RCC and TC, respectively. Again, we adjust and estimate the above equation with ANCOVA and include some selected baseline socioeconomic covariates (X) and the baseline measurement of the respective outcome variable ($y_{i0}$) to control for any observed baseline heterogeneity among the treatment and control groups and estimate the equation below:

$$y_i = \alpha + \delta_{ANCOVA1} \widehat{RCC}_i + \delta_{ANCOVA2} \widehat{TC}_i + \beta_1 X_{i0} + \beta_2 y_{i0} + \varepsilon_i \quad (7)$$

Lastly, we attempt to deal with fixed individual heterogeneity and therefore estimate the following fixed-effects instrumental variables model, where $\sigma_i$ represents the unobserved time-invariant household heterogeneity:

$$y_{it} = \alpha + \delta_{FE1} \widehat{RCC}_{it} + \delta_{FE2} \widehat{TC}_{it} + S_t + \sigma_i + \varepsilon_{it} \quad (8)$$

Similar to the ITT estimations, for all equations we test the null hypotheses that: (1) uptake of RCC did not have any effect on the households' outcomes expressed as $\delta_1 = 0$; (2) uptake of TC did not have any effect on the households' outcomes expressed as $\delta_2 = 0$; and (3) the difference between RCC uptake and TC uptake effects are not different from zero expressed as $\delta_1 - \delta_2 = 0$.

**Impact pathways analysis with structural equation modelling**

We use structural equation modelling (SEM) to analyze the impact pathways on our ex-post productivity and welfare outcomes.[8] Figure 2 below presents the hypothesized impact pathways between the treatments, intermediate, and main outcomes. Here, we consider maize yield and overall acreage revenue as our productivity indicators, and CSI as our welfare indicator. As such, we have three main SEM models—one for each outcome. We consider credit rationing and investment ability to be the intermediate factors between credit uptake and household productivity. Both variables (credit rationing and investment ability) are further considered as mediating for welfare, where one of the productivity indicators (revenue) is included as an additional mediator between credit uptake and household welfare.

Credit rationing is a binary choice variable equal to one if a household was credit rationed in any form (either risk- or quantity-rationed) and zero otherwise. Investment

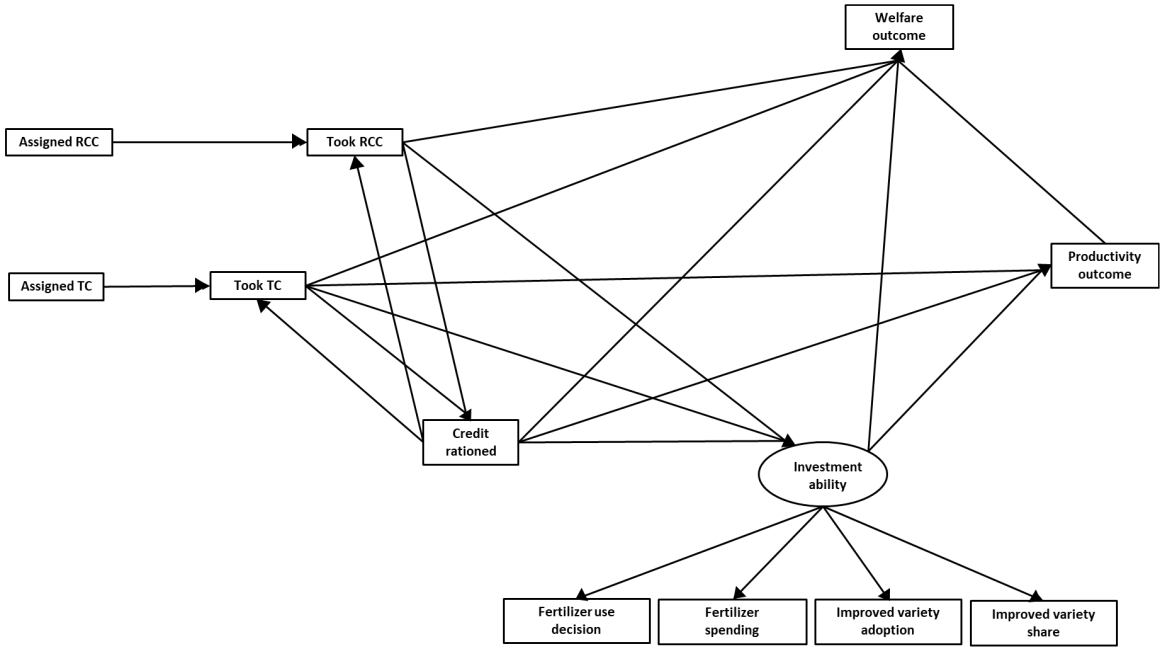
[8] See Heckert, Olney, and Ruel (2019) for a recent and closely comparable application of SEM in impact and mediation analysis.

ability is a latent endogenous variable which is the unobserved capacity driving households' adoption decisions and intensity of higher-yielding improved varieties and chemical fertilizers (Figure 2).[9]

Apart from these main mediation paths, we also assess whether credit rationing has any mediating effect between credit uptake and investment ability. This is an important mechanism assessment, since we have already reported that uptake of either RCC or TC positively and significantly influenced household farm investment (that is, the use and intensity of chemical fertilizers and improved maize varieties).

Finally, to account for imperfect compliance among the treatment households, we include a path from random assignment to credit uptake for respective credits and allow for covariance between the fitted uptake and the main outcomes' error terms. This controls for any endogeneity due to unobserved heterogeneity in the estimations and is the SEM's equivalent of standard instrumental variable estimation (Allison 2018). For simplicity and clarity, we do not show the correlation curves in the diagram. Since we use SEM here to exclusively assess the intermediation and impact pathways but not to report causal effects, we are not interested in computing direct, indirect, and total effects.

**Figure 2: Structural equation model showing the causal pathways from treatments (RCC and TC) to the intermediate and main (productivity and welfare) outcomes**




[9] Latent variables are unobserved constructs that cannot be measured directly but are inferred from a set of observed indicators. In our context, investment ability is the unobserved capacity to invest in certified seed and chemical fertilizer for enhanced productivity. We construct investment ability for four observed indicators, namely: (1) binary indicator of fertilizer use on maize; (2) continuous indicator of the amount of money spent on maize fertilizer per acre; (3) binary indicator of adoption of certified hybrid seeds; and (4) proportional indicator of the share of one's maize area under certified hybrid seeds.

### *4.2.4 Results and discussions*

**Ex-ante investment impacts**

As indicators of households' agricultural investment, we consider five outcomes: (1) the use of fertilizer on maize as a binary choice variable equal to one if chemical fertilizer was used on maize, and zero otherwise; (2) the intensity of chemical fertilizer use, proxied by the amount of money (in Ksh) spent on chemical fertilizers for maize; (3) the adoption of improved maize varieties as a binary choice variable equal to one if improved maize varieties were planted at all, and zero otherwise; (4) the intensity of improved maize adoption, measured as the share of maize fields under improved varieties; and (5) the area under maize overall.

The structure of the results tables is as follows: in Column (1), we present the SMD ITT effects, in Column (2) the ITT estimated with ANCOVA, and in Column (3) the ITT, estimated with individual fixed effects. In Column (4), we present the SMD LATE, in Column (5) the LATE estimated with ANCOVA, and in Column (6) the LATE estimated with individual fixed effects. We estimate all LATEs using two-stage least squares where random assignment to either RCC or TC is used as the instrumental variable for uptake of the respective credit type.

**Fertilizer use**

Table 2 presents the ITT and LATE effects of a household being randomly offered either RCC or TC on its decision to use chemical fertilizer (Panel A) and the acreage fertilizer use intensity (Panel B), proxied by funds spent on maize fertilizer in Ksh per acre.

When considering Panel A, the SMD estimates presented in Column (1) show that random assignment to either RCC or TC did not have any significant effect on the decision to use chemical fertilizers on maize fields. The ANCOVA estimates in Panel (2) show that assignment to RCC increased the likelihood of applying chemical fertilizers in maize fields by 3.6 percentage points, and this was statistically significant at p-value < 0.1, while assignment to TC did not have any significant effect on the same outcome. The fixed effects estimates in Column (3) indicate that assignment to either RCC or TC had a positive but statistically insignificant effect on the likelihood of using chemical fertilizers on maize fields.

The LATE results in Columns (4), (5), and (6), though higher in magnitude, follow a similar trend. SMD estimates in Column (4) indicate that uptake of either RCC or TC did not have a statistically significant effect on the likelihood of using chemical fertilizers on maize fields. ANCOVA estimates in Column (5) indicate that uptake of RCC increased the same by 13.5 percentage points, and this was significant at p-value < 0.1, while uptake of TC did not significantly affect this outcome. The fixed effects estimates in Column (6) indicate that uptake of either RCC or TC had positive but statistically insignificant effects on the likelihood of using chemical fertilizers on one's maize fields. Across all the ITT and LATE specifications, we could not reject the null hypothesis that the difference between RCC and TC effects was equal to zero.

Panel B presents the ITT effects and LATE on fertilizer use intensity, measured as the amount of money in Ksh spent on maize fertilizer per acre. As shown in Column (1) where ITT is estimated as SMD, assignment to RCC increased acreage spending on maize fertilizer by about Ksh 490, and this was statistically significant at p-value < 0.01, while assignment to TC did not lead to any significant effect on the same outcome.

ANCOVA estimates in Column (2) indicate that assignment to RCC increased acreage maize fertilizer spending by about Ksh 517, and this was significant at p-value < 0.01, while TC increased the same by about Ksh 169, with a low significance of p-value < 0.1. Fixed effects estimates in Column (3) indicate that assignment to RCC increased acreage maize fertilizer spending by about Ksh 518 and was significant at p < 0.01, while assignment to TC increased the same by about Ksh 214 and was significant at p-value < 0.1.

As expected, the LATE results indicate a similar trend, with substantially higher effects in terms of magnitude but similar in statistical significance. SMD estimates in Column (4) indicate that uptake of RCC increased acreage spending on maize fertilizer by about Ksh 1,840, and this was significant at p-value < 0.01, while uptake of TC did not have a significant effect on the same outcome.

ANCOVA estimates in Column (5) indicate that uptake of RCC increased acreage spending on maize fertilizer by about Ksh 1,922 and this was significant at p-value < 0.01, while uptake of TC increased the same by about Ksh 742 with p-value < 0.1. Finally, fixed effects estimates in Column (6) indicate that uptake of RCC increased acreage spending on maize fertilizer by Ksh 1,938 with p-value < 0.01, while uptake of TC increased the same by Ksh 970 with p-value < 0.05.

Across all the models, we find strong evidence that RCC effects on maize fertilizer spending (both ITT and LATE) were significantly larger than the TC effects as shown by the “RCC = TC” row in Panel B.

**Table 2: Treatment effects on use of chemical fertilizers**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| VARIABLES | ITT SMD | ITT ANCOVA | ITT FE | LATE SMD | LATE ANCOVA | LATE FE |
| Panel A: Binary response | | | | | | |
| RCC | 0.033 | 0.036* | 0.036 | 0.125 | 0.135* | 0.137 |
| | (0.022) | (0.019) | (0.029) | (0.084) | (0.073) | (0.111) |
| TC | 0.013 | 0.029 | 0.047 | 0.060 | 0.132 | 0.216 |
| | (0.023) | (0.020) | (0.029) | (0.103) | (0.088) | (0.134) |
| Outcome var at baseline | | 0.423*** | | | 0.415*** | |
| | | (0.017) | | | (0.021) | |
| Individual fixed effects | No | No | Yes | No | No | Yes |
| Baseline covariates | No | Yes | No | No | Yes | No |
| Constant | 0.773*** | | 0.670*** | 0.773*** | 0.536*** | 0.699*** |
| | (0.016) | | (0.008) | (0.016) | (0.066) | (0.008) |
| RCC = TC: p-value | 0.373 | 0.740 | 0.710 | 0.479 | 0.971 | 0.529 |
| Observations | 2,015 | 2,008 | 3,068 | 2,015 | 2,008 | 3,068 |
| R-squared | 0.00 | 0.25 | 0.04 | 0.02 | 0.27 | |
| Panel B: Fertilizer spending | | | | | | |
| RCC | 491.371*** | 517.162*** | 518.216*** | 1,839.850*** | 1,922.156*** | 1,937.909*** |
| | (108.579) | (99.324) | (121.484) | (398.997) | (372.558) | (447.625) |
| TC | 145.706 | 168.746* | 214.126* | 638.941 | 741.569* | 969.931** |
| | (98.978) | (99.756) | (110.504) | (425.911) | (391.744) | (489.926) |
| Outcome var at baseline | | 0.495*** | | | 0.469*** | |
| | | (0.030) | | | (0.033) | |

| VARIABLES | (1)<br>ITT SMD | (2)<br>ITT ANCOVA | (3)<br>ITT FE | (4)<br>LATE SMD | (5)<br>LATE ANCOVA | (6)<br>LATE FE |
|---|---|---|---|---|---|---|
| Individual fixed effects | No | No | Yes | No | No | Yes |
| Baseline covariates | No | Yes | No | No | Yes | No |
| Constant | 1,723.676***<br>(67.095) | | 1,320.081***<br>(26.816) | 1,723.676***<br>(67.044) | 1,746.599***<br>(352.646) | 1,215.534***<br>(31.028) |
| RCC = TC: p-value | 0.002 | 0.000 | 0.014 | 0.006 | 0.004 | 0.049 |
| Observations | 1,956 | 1,941 | 3,005 | 1,956 | 1,941 | 3,005 |
| R-squared | 0.01 | 0.17 | 0.07 | 0.06 | 0.19 | |

*Notes: FE = fixed effects. For simplicity, RCC and TC rows show the ITT and LATE estimates for all the estimators, namely ITT SMD in Column (1), ITT ANCOVA in Column (2), ITT FE in Column (3), LATE SMD in Column (4), LATE ANCOVA in Column (5) and LATE FE in Column (6). All ANCOVA models had baseline covariates, including outcome variable baseline measurement. The first stage predicted uptake levels are in Panel A of Appendix table A1. Robust standard errors are shown in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.*

**Improved maize seed adoption**

Table 3 presents the ITT and LATE effects of RCC and TC on the decision to use improved maize seeds (Panel A) and the intensity of improved maize seed adoption (Panel B), proxied by the share of land under maize that was planted with improved maize varieties. Across all the specifications, those who were offered RCC and those who took it were more likely to use improved maize varieties and had a higher share of their maize fields under improved maize varieties. These improvements were, however, marginal and statistically insignificant. On the other hand, those who were offered and those who took TC were less likely to use improved maize varieties and had a smaller share of their maize fields under improved maize varieties. This negative effect was however statistically insignificant.

Looking at the “RCC = TC: p-value” rows in both panels, there is some ground to reject the null hypothesis that the difference between RCC and TC estimates was equal to zero where RCC estimates are larger than TC ones. This is particularly so with ITT SMD, ITT ANCOVA, and LATE SMD in Panel A and ITT SMD, ITT ANCOVA, LATE SMD, and LATE ANCOVA in Panel B. This suggests that RCC had potentially higher benefits than TC on the households’ likelihood and intensity of use of improved varieties on their maize fields.

**Table 3: Treatment effects on the adoption of improved maize seeds**

| VARIABLES | (1)<br>ITT SMD | (2)<br>ITT ANCOVA | (3)<br>ITT FE | (4)<br>LATE SMD | (5)<br>LATE ANCOVA | (6)<br>LATE FE |
|---|---|---|---|---|---|---|
| Panel A: Improved maize seed use - binary | | | | | | |
| RCC | 0.019 | 0.018 | 0.011 | 0.072 | 0.069 | 0.043 |
| | (0.017) | (0.017) | (0.026) | (0.066) | (0.061) | (0.098) |
| TC | -0.015 | -0.010 | 0.006 | -0.069 | -0.044 | 0.026 |
| | (0.019) | (0.017) | (0.027) | (0.084) | (0.079) | (0.124) |
| Outcome var at baseline | | 0.291*** | | | 0.289*** | |
| | | (0.020) | | | (0.028) | |
| Individual fixed effects | No | No | Yes | No | No | Yes |
| Baseline covariates | No | Yes | No | No | Yes | No |
| Constant | 0.877*** | | 0.854*** | 0.877*** | 0.675*** | 0.854*** |
| | (0.013) | | (0.006) | (0.013) | (0.062) | (0.006) |

| VARIABLES | (1) ITT SMD | (2) ITT ANCOVA | (3) ITT FE | (4) LATE SMD | (5) LATE ANCOVA | (6) LATE FE |
|---|---|---|---|---|---|---|
| RCC = TC: p-value | 0.056 | 0.101 | 0.828 | 0.065 | 0.118 | 0.994 |
| Observations | 1,998 | 1,983 | 3,047 | 1,998 | 1,983 | 3,047 |
| R-squared | 0.00 | 0.12 | 0.00 | -0.01 | 0.12 | |
| Panel B: Share of land under improved maize seed | | | | | | |
| RCC | 1.519 | 1.128 | 0.419 | 5.791 | 4.321 | 1.599 |
| | (1.852) | (1.762) | (2.661) | (7.029) | (6.469) | (10.157) |
| TC | -2.143 | -1.854 | -0.442 | -9.760 | -8.458 | -2.045 |
| | (1.932) | (1.764) | (2.651) | (8.901) | (8.221) | (12.289) |
| Outcome var at baseline | | 0.335*** | | | 0.334*** | |
| | | (0.020) | | | (0.024) | |
| Individual fixed effects | No | No | Yes | No | No | Yes |
| Baseline covariates | No | Yes | No | No | Yes | No |
| Constant | 80.556*** | | 77.739*** | 80.556*** | 65.249*** | 77.731*** |
| | (1.345) | | (0.589) | (1.344) | (6.237) | (0.588) |
| RCC = TC: p-value | 0.052 | 0.089 | 0.754 | 0.056 | 0.089 | 0.925 |
| Observations | 2,015 | 2,008 | 3,068 | 2,015 | 2,008 | 3,068 |
| R-squared | 0.00 | 0.15 | 0.00 | -0.01 | 0.14 | |

*Notes: FE = fixed effects. For simplicity, RCC and TC rows show the ITT and LATE estimates for all the estimators: ITT SMD in Column (1), ITT ANCOVA in Column (2), ITT FE in Column (3), LATE SMD in Column (4), LATE ANCOVA in Column (5) and LATE FE in Column (6). All ANCOVA models had baseline covariates, including outcome variable baseline measurement. The first stage predicted uptake levels are in Panel A of* ***Error! Reference source not found.*** *in the appendix. Robust standard errors are shown in parentheses.* $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

**Area under maize**

Table 4 presents ITT and LATE effects on the overall area cultivated with maize. This investigates whether RCC and TC lead to the expansion of maize-cultivated land. This is important to examine, since we already see they had a substantial positive and significant effect on agricultural investment, particularly the adoption of chemical fertilizers and the intensity of their use (particularly RCC).

Across all the estimations, random assignment and uptake of either RCC or TC are positively correlated with the area cultivated with maize overall. However, estimates from all the ITT and LATE models are small in magnitude and statistically insignificant. This suggests that being randomly assigned to either RCC or TC, and even eventually taking the credit offered, did not result in a significant change in the area cultivated with maize. We then can conclude that there is evidence that uptake of RCC promoted agricultural intensification but not extensification, where we see increased adoption and intensified use of high-quality inputs (fertilizer) without any significant expansion in the area cultivated with the crop of interest. We also do not find a significant difference between RCC and TC (both ITT and LATE) effects on maize area expansion.

**Table 4: Treatment effects on the area under maize**

| VARIABLES | (1) ITT SMD | (2) ITT ANCOVA | (3) ITT FE | (4) LATE SMD | (5) LATE ANCOVA | (6) LATE FE |
|---|---|---|---|---|---|---|
| RCC | 0.035 | 0.063 | 0.090 | 0.132 | 0.237 | 0.342 |
| | (0.107) | (0.096) | (0.138) | (0.406) | (0.357) | (0.526) |
| TC | 0.105 | 0.102 | 0.063 | 0.474 | 0.462 | 0.291 |
| | (0.103) | (0.096) | (0.139) | (0.466) | (0.432) | (0.639) |
| Outcome var at baseline | | 0.403*** | | | 0.402*** | |
| | | (0.021) | | | (0.044) | |

| VARIABLES | (1) ITT SMD | (2) ITT ANCOVA | (3) ITT FE | (4) LATE SMD | (5) LATE ANCOVA | (6) LATE FE |
|---|---|---|---|---|---|---|
| Individual fixed effects | No | No | Yes | No | No | Yes |
| Baseline covariates | No | Yes | No | No | Yes | No |
| Constant | 1.902*** | | 2.031*** | 1.902*** | 0.112 | 2.011*** |
| | (0.074) | | (0.030) | (0.074) | (0.329) | (0.030) |
| RCC = TC: p-value | 0.502 | 0.683 | 0.985 | 0.435 | 0.572 | 0.919 |
| Observations | 2,007 | 1,992 | 3,056 | 2,007 | 1,992 | 3,056 |
| R-squared | 0.00 | 0.19 | 0.00 | 0.00 | 0.19 | |

*Notes: FE = fixed effects. For simplicity, RCC and TC rows show the ITT and LATE estimates for all the estimators: ITT SMD in Column (1), ITT ANCOVA in Column (2), ITT FE in Column (3), LATE SMD in Column (4), LATE ANCOVA in Column (5) and LATE FE in Column (6). All ANCOVA models had baseline covariates, including outcome variable baseline measurement. The first stage predicted values for uptake are in the Panel A of Table A1 in the appendix. Robust standard errors are shown in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.*

**Ex-post productivity and welfare impacts**

As indicators of households' productivity, we consider maize yields and acreage revenue. The yield variable used has been adjusted for intercropping, where we divide the farmer-reported yields by the inverse of the number of reported intercrops. Acreage revenue was computed as a summation of the value of acreage production of the three main crops (maize, normal beans, cow peas) grown by almost all households in the study area, and whose production was also to be directly affected by the loans.

The main crop was maize, for which normal beans and cow peas were the common intercrops. The value of the crops used was the actual market price of the commodities at Machakos market at the end of each season (information obtained from the Kenyan Ministry of Agriculture). For indicators of household welfare, we evaluated their food security using the CSI and dietary diversity using the HDDI. CSI measures the unavailability and insufficiency of food, while HDDI measures the diversity (and hence balance) of their diets. The structure of the results tables here is similar to the ex-ante results tables explained above.

Ex-post impacts of insurance and credit uptake, as one would expect, are higher-level impacts that are harder to achieve and estimate, especially with limited periods (T = 2) in the panel structure and low uptake, as in the case of this study. As such, although we make every effort to control for any form of endogeneity, we cautiously interpret and claim causality here but recommend that the results and interpretations are treated as indicative, and motivation for further investigation (especially with longer panels, a variety of credit and insurance products, and higher uptake).

**Maize yield and acreage revenue**

Panel A of Table 5 presents the ITT and LATE effects of TC and RCC on maize yields. ITT SMD estimates in Column (1) and ITT ANCOVA estimates in Column (2) show that random assignment to either RCC or TC was negatively associated with maize yields, and this was significant in the case of TC with p-value < 0.1. LATE SMD and LATE ANCOVA estimates in Columns (4) and (5), respectively, show similar results with larger estimates. Fixed effects estimates, both ITT and LATE, indicate a negative but statistically insignificant association between both treatments (both RCC and TC) and maize yields.

Panel B of Table 5 presents the ITT and LATE effects of TC and RCC on acreage farm revenue. ITT SMD estimates in Column (1), ITT ANCOVA estimates in Column (2), and ITT fixed effects estimates in Column (3) indicate that random assignment to either RCC or TC, although negatively associated with farm revenue outcome, did not have any significant effect on that outcome. LATE results follow a similar trend but with larger estimates.

The “RCC = TC: p-value” rows show no evidence that the differences in RCC and TC effects were statistically significant.

**Table 5: Treatment effects on maize yield and acreage revenue**

| VARIABLES | (1)<br>ITT SMD | (2)<br>ITT ANCOVA | (3)<br>ITT FE | (4)<br>LATE SMD | (5)<br>LATE ANCOVA | (6)<br>LATE FE |
|---|---|---|---|---|---|---|
| Panel A: Maize yield | | | | | | |
| RCC | -105.210 | -72.228 | -30.771 | -399.441 | -268.911 | -116.984 |
| | (67.783) | (66.207) | (75.843) | (256.421) | (250.964) | (287.402) |
| TC | -130.127* | -122.608* | -123.113 | -584.692* | -553.808* | -564.813 |
| | (66.521) | (66.356) | (75.934) | (299.396) | (294.278) | (348.804) |
| Outcome var at baseline | | 0.259*** | | | 0.272*** | |
| | | (0.034) | | | (0.037) | |
| Individual fixed effects | No | No | Yes | No | No | Yes |
| Baseline covariates | No | Yes | No | No | Yes | No |
| Constant | 1,236.527*** | | 969.420*** | 1,236.527*** | 1,120.348*** | 968.879*** |
| | (47.680) | | (16.144) | (47.644) | (222.059) | (17.521) |
| RCC = TC: p-value | 0.710 | 0.444 | 0.240 | 0.507 | 0.295 | 0.560 |
| Observations | 1,996 | 1,981 | 3,045 | 1,996 | 1,981 | 3,045 |
| R-squared | 0.00 | 0.05 | 0.01 | 0.00 | 0.06 | |
| Panel B: Overall average revenue | | | | | | |
| RCC | -1,828.576 | -1,490.954 | -1,435.601 | -6,942.390 | -5,628.192 | -5,466.198 |
| | (1,161.209) | (1,165.607) | (1,363.335) | (4,388.495) | (4,332.049) | (5,184.118) |
| TC | -687.463 | -795.872 | -2,231.676 | -3,107.516 | -3,644.003 | -10,250.567 |
| | (1,191.856) | (1,168.760) | (1,481.033) | (5,378.405) | (5,295.947) | (6,846.371) |
| Outcome var at baseline | | 0.173*** | | | 0.175*** | |
| | | (0.030) | | | (0.037) | |
| Individual fixed effects | No | No | Yes | No | No | Yes |
| Baseline covariates | No | Yes | No | No | Yes | No |
| Constant | 17,887.031*** | | 14,602.036*** | 17,887.031*** | 15,258.268*** | 14,594.419*** |
| | (846.088) | | (321.535) | (845.453) | (4,066.231) | (333.383) |
| RCC = TC: p-value | 0.324 | 0.549 | 0.660 | 0.430 | 0.678 | 0.924 |
| Observations | 2,002 | 1,989 | 3,052 | 2,002 | 1,989 | 3,052 |
| R-squared | 0.00 | 0.03 | 0.00 | 0.01 | 0.04 | |

*Notes: FE = fixed effects. For simplicity, RCC and TC rows show the ITT and LATE estimates for all the estimators: ITT SMD in Column (1), ITT ANCOVA in Column (2), ITT FE in Column (3), LATE SMD in Column (4), LATE ANCOVA in Column (5) and LATE FE in Column (6). All ANCOVA models had baseline covariates, including outcome variable baseline measurement. The first stage predicted values for uptake are in the Panel A of Table A1 in the appendix. Robust standard errors are shown in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.*

**Coping strategy and dietary diversity**

Here, we investigate RCC and TC treatment effects on households' food security and dietary diversity. We use baseline and midline data, since the endline variables were incompatible (as the scales were simplified to accommodate a phone survey).

To evaluate the food insecurity situation, we used the CSI as defined by Maxwell and Caldwell (2008). We selected the eight most common food insecurity coping strategies and asked respondents how many days in the past seven days they may have employed each. Our scale ranges from 0–56, or simply the product of the maximum possible number of days (7) and the number of coping strategies (8).

The score for a household is the summation of the number of days a respondent said they may have employed the respective strategies. The higher the score, the more times a household had to employ one of the coping strategies, and hence the worse their food insecurity. As such, a negative treatment effect is expected, which would imply food security improved (food insecurity lessened) as a result of our experimental treatments.

To construct the HDDI, we asked the respondents if in the past seven days they had consumed a host of food items, which were categorized into 15 discrete food groups. We then created binary response variables corresponding to the food groups, whose values were one if at least one food item in the group was consumed at least once in the past seven days, and zero otherwise. We then summed up the food groups for each household, which gives us their score on a 15-point scale. The higher the score, the more diverse the household's diet; therefore, positive effects are desired.

Panel A of Table 6 presents ITT and LATE effects on the CSI, while Panel B presents similar ITT and LATE effects on household dietary diversity. The results are consistent with other ex-post impact outcomes where SMD, ANCOVA, and fixed effects estimates, both ITT and LATE, suggest that assignment to and uptake of either RCC or TC did not have a significant effect on household welfare, measured with CSI and HDDI.

**Table 6: Treatment effects on food insecurity coping strategies**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| VARIABLES | ITT SMD | ITT ANCOVA | ITT FE | LATE SMD | LATE ANCOVA | LATE FE |
| Panel A: CSI | | | | | | |
| RCC | 0.490 | 0.504 | 0.729 | 1.366 | 1.392 | 2.027 |
| | (0.398) | (0.361) | (0.574) | (1.110) | (1.003) | (1.596) |
| TC | 0.391 | 0.416 | 0.797 | 1.267 | 1.350 | 2.591 |
| | (0.380) | (0.362) | (0.528) | (1.238) | (1.103) | (1.720) |
| Outcome var at baseline | | 0.228*** | | | 0.228*** | |
| | | (0.023) | | | (0.031) | |
| Individual fixed effects | No | No | Yes | No | No | Yes |
| Baseline covariates | No | Yes | No | No | Yes | No |
| Constant | 3.323*** | | 5.387*** | 3.323*** | 4.326*** | 5.387*** |
| | (0.268) | | (0.086) | (0.268) | (1.348) | (0.095) |
| RCC = TC: p-value | 0.8046 | 0.8074 | 0.8794 | 0.9347 | 0.9693 | 0.7108 |
| Observations | 1,020 | 1,016 | 2,073 | 1,020 | 1,016 | 2,073 |
| R-squared | 0.00 | 0.18 | 0.05 | -0.01 | 0.18 | |
| Panel B: HDDI | | | | | | |
| RCC | -0.088 | -0.060 | -0.102 | -0.245 | -0.165 | -0.283 |
| | (0.124) | (0.112) | (0.140) | (0.345) | (0.308) | (0.390) |
| TC | -0.091 | -0.070 | -0.089 | -0.296 | -0.226 | -0.288 |

| VARIABLES | (1) ITT SMD | (2) ITT ANCOVA | (3) ITT FE | (4) LATE SMD | (5) LATE ANCOVA | (6) LATE FE |
|---|---|---|---|---|---|---|
| | (0.122) | (0.112) | (0.143) | (0.399) | (0.361) | (0.465) |
| Outcome var at baseline | | 0.315*** | | | 0.316*** | |
| | | (0.029) | | | (0.035) | |
| Individual fixed effects | No | No | Yes | No | No | Yes |
| Baseline covariates | No | Yes | No | No | Yes | No |
| Constant | 11.033*** | | 10.638*** | 11.033*** | 6.200*** | 10.638*** |
| | (0.087) | | (0.022) | (0.087) | (0.514) | (0.023) |
| RCC = TC: p-value | 0.978 | 0.929 | 0.912 | 0.893 | 0.855 | 0.619 |
| Observations | 1,020 | 1,016 | 2,073 | 1,020 | 1,016 | 2,073 |
| R-squared | 0.00 | 0.20 | 0.03 | -0.01 | 0.20 | |

*Notes: FE = fixed effects. For simplicity, RCC and TC rows show the ITT and LATE estimates for all the estimators: ITT SMD in Column (1), ITT ANCOVA in Column (2), ITT FE in Column (3), LATE SMD in Column (4), LATE ANCOVA in Column (5), and LATE FE in Column (6). All ANCOVA models had baseline covariates, including outcome variable baseline measurement. The first stage predicted values for uptake are in the Panel A of Table A1 in the appendix. Robust standard errors are shown in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.*

#### *4.2.5 Mediation and pathways analysis*

The results of the three SEM models are presented in Table 7. Each model bears the title (as indicated in the column heading) of the main productivity or welfare outcome whose impact pathways are being assessed. A model is comprised of simultaneous regression equations, and in the table, the outcome for each individual equation is in bold, while the predictors are indented.

First, all three models exhibit average fitted uptake of RCC and TC (27.6% and 23.3%, respectively) which is consistent with the first stage results of the standard two-stage least squares models reported above and shown in Table A1 in the appendix. Further, we find that credit-rationed status did not significantly or substantially affect uptake of the credit offered, whether RCC or TC. This eliminates reverse causality and simultaneity concerns when credit rationing is used as an intermediary variable for other impact outcomes.

We find that across all the models, uptake of either RCC or TC had a desirable and statistically significant effect on the main intermediate variables (credit rationing and investment ability). Both RCC and TC reduced credit rationing while increasing investment ability. The effect of credit rationing on investment ability, although negative across all the models, was small and statistically insignificant. We therefore do not find evidence that credit rationing moderated the impact of RCC or TC on households' investment ability.

The productivity rows under the yield model column show that the direct paths from credit uptake (either RCC or TC) were positive but statistically insignificant. We also see that credit rationing had a negative and significant effect on maize yield, while investment ability had a positive and significant effect on maize yield. The results here confirm that the impact of RCC and TC on maize yield is fully mediated by credit rationing and investment ability. Results indicate that the negative effect of credit rationing on maize yields can be abated by access to credit.

Similarly, the positive effects of investment ability on maize yields can be further augmented through enhanced access to credit. The case is similar to revenue, where

acreage revenue reduces with credit rationing and increases with investment ability, and the two intermediates can be enhanced with credit access, which will eventually lead to better farm revenues. The only difference here is that although the impact of RCC uptake on farm revenue is fully mediated by credit rationing and investment ability (a positive but insignificant coefficient), that of TC uptake is partially mediated by the same variables. There could possibly be other mediation routes between credit uptake and farm revenue that are not included in this analysis.

Finally, looking at welfare outcome rows under the food insecurity column, we find that the uptake effect of both RCC and TC on the CSI is fully mediated by credit rationing, investment ability, and household productivity (which in this model is proxied by acreage revenue). Food insecurity reduces alongside an increase in investment ability and farm revenue and increases with credit rationing. Although highly significant, the revenue effect coefficient is very low, while those of credit rationing and investment ability are substantially high. We suspect that the mediating effects of credit rationing and investment ability on farm revenue could explain this.

**Table 7: Results of structural equation models for yield, revenue, and food insecurity index**

| | Yield model | | Revenue model | | Food insecurity (CSI) | |
|---|---|---|---|---|---|---|
| | Coef. | SE | Coef. | SE | Coef. | SE |
| **Uptake of RCC** | | | | | | |
| Assigned RCC | 0.276*** | 0.009 | 0.276*** | 0.009 | 0.276*** | 0.009 |
| Credit rationed | -0.003 | 0.016 | -0.003 | 0.016 | -0.003 | 0.016 |
| **Uptake of TC** | | | | | | |
| Assigned TC | 0.233*** | 0.009 | 0.233*** | 0.009 | 0.233*** | 0.009 |
| Credit rationed | 0.01 | 0.017 | 0.01 | 0.017 | 0.01 | 0.017 |
| **Credit rationing** | | | | | | |
| Took RCC | -0.17** | 0.081 | -0.17** | 0.081 | -0.17** | 0.081 |
| Took TC | -0.278*** | 0.097 | -0.278*** | 0.097 | -0.278*** | 0.097 |
| **Investment ability** | | | | | | |
| Took RCC | 0.13*** | 0.025 | 0.13*** | 0.025 | 0.13*** | 0.025 |
| Took TC | 0.121*** | 0.027 | 0.12*** | 0.027 | 0.121*** | 0.027 |
| Credit rationed | -0.009 | 0.012 | -0.009 | 0.012 | -0.009 | 0.012 |
| **Productivity outcome** | | | | | | |
| Took RCC | 209.1 | 183.212 | 297.17 | 3,245.4 | 287.387 | 3,245.499 |
| Took TC | 243.558 | 217.09 | 7,523.47** | 3,867.1 | 7,512.314** | 3,867.151 |
| Credit rationed | -149.485*** | 42.668 | -3,227.554*** | 757.191 | -3,227.587*** | 757.097 |
| Investment ability | 535.75*** | 66.885 | 7,243.822*** | 1,188.3 | 7,236.618*** | 1,187.758 |
| **Welfare outcome -** | | | | | | |
| Took RCC | | | | | -0.386 | 0.441 |
| Took TC | | | | | -0.635 | 0.479 |
| Credit rationed | | | | | 1.491*** | 0.209 |
| Investment ability | | | | | -0.788** | 0.348 |
| Revenue | | | | | -0.001*** | 0.000 |

*Notes: SE = standard error. All coefficients are unstandardized and correspond to the paths in Figure 2. In bold are left-hand variables and indented are right-hand variables for each equation.*

### *4.2.6 Assessing for heterogeneity in treatment effects*

We have seen that our treatments, especially RCC, had favorable mean treatment effects on one of our investment outcomes (fertilizer spending). It is possible, however, that the benefits were not homogenously spread within the sample, with some households (based on some equity dimensions) benefiting more than others. For all the outcomes considered above, we assess for treatment-effects heterogeneity by gender,

age, education (of the household head), and respondents' risk aversion as recorded at baseline. We only assess for heterogeneity in uptake effects – where we converted the LATE fixed effects Equation 8 by interacting uptake at the second stage of two-stage least squares with equity indicators (*Evar*). The results tables for heterogenous treatment effects are in Appendix C.

$$y_{it} = \alpha + \delta_{FEIV1}(\widehat{RCC * Evar})_{it} + \delta_{FEIV2}(\widehat{TC * Evar})_{it} + \beta_1 RCC_{it} + \beta_2 TC_{it} + \sigma_i + \varepsilon_{it} \quad (9)$$

Except for RCC interaction with gender and constant relative risk aversion for the fertilizer spending outcome, as well as RCC and TC interactions with education for the CSI and HDDI outcomes, all other results indicate that treatment effects did not differ by gender, age, education, and risk aversion of the household heads. The interaction between RCC uptake and the gender dummy yielded a statistically significant positive coefficient on fertilizer spending outcome (Appendix **Error! Reference source not found.**). This indicates that RCC uptake had higher fertilizer spending benefits in male-headed than female-headed households.

The interaction between RCC uptake and constant relative risk aversion yielded a statistically significant positive coefficient on the same outcome (Appendix **Error! Reference source not found.**). This suggests that farmers who were more risk-averse benefitted more from the uptake of RCC than their risk-neutral and risk-preferring counterparts. This follows a higher uptake of RCC compared to TC across all risk aversion categories, as shown in Appendix figure A.5. This difference is particularly notable for the extreme risk-averse group, where none took TC while about 17% of those offered RCC took the loan.

Furthermore, the interaction between uptake of both RCC and TC with education yielded positive and significant coefficients for both credit types on CSI outcome (Appendix). This indicates that uptake of either RCC or TC had higher benefits in reducing food insecurity among households whose heads had lower educational attainment. It should be noted that higher values of CSI indicate increased food insecurity and vice-versa; therefore, a positive coefficient in these interactions favors those with lower education levels. Similarly, the interaction between uptake of RCC and education yielded a negative and significant coefficient on HDDI outcome (Appendix), suggesting that uptake of RCC yielded greater improvement in household diet diversification among those with lower educational attainment.

# 5. Discussion

## 5.1 Significance

Drought has been identified as one of the major risks to agricultural productivity for farmers in Sub-Saharan Africa. The impact of drought in this region is exacerbated by an absence of effective options for managing risk, such as formal insurance. Lately, weather index insurance has been trialed in this region and other low-income countries but has suffered frustratingly low uptake and unsustainability beyond pilot and experimental phases. Insurance-linked credit has emerged as a promising intervention that links insurance directly to agricultural credit to protect both borrowers and lenders against specified contingent risks. There is limited literature on the uptake of such innovations,

however, and the gap in the empirical literature on the potential impacts of such products on smallholders from such regions is even more pronounced.

Using data from an RCT in Kenya, we estimate the impact of RCC and TC on rural smallholders' farm investment, productivity, and welfare. We also test the hypothesis that the difference between RCC and TC effects is not different from zero. Finally, we analyze the impact and mediation pathways via which RCC and TC influence households' productivity and welfare. We present both the ITT and LATE.

Results demonstrate that random assignment to and uptake of RCC had positive and significant effects on the decision to use chemical fertilizers on maize fields, as well as the amount of money spent on maize fertilizer. Assignment to and uptake of TC, although positively associated with fertilizer usage, did not have a statistically significant effect on the decision to adoption fertilizers, while its effect on the amount of money spent on fertilizer was marginally significant.

There is evidence that the difference between RCC and TC effects (both ITT and LATE) on maize fertilizer spending per acre was statistically different from zero, with RCC having higher effects than TC. Also, random assignment to and uptake of either RCC or TC did not significantly influence either their decision to adopt improved maize varieties or the share of their maize fields cultivated with improved varieties.

However, while the association between improved maize varieties outcomes and RCC is positive, we note a negative association between the same outcomes and TC. These results, especially on fertilizer outcomes, suggest that compared to TC, RCC had a higher positive impact on investment decisions. This corroborates the long-held beliefs that: (1) access to credit enhances access to high-yielding but capital-intensive technologies; and (2) hedging farmers' investments and capital against downside production risks such as drought can encourage the adoption of higher-risk, higher-yielding agricultural technologies and practices (and therefore enhanced investment decisions ex-ante).

Although the chapter reports enhanced farm investment in terms of the use of higher-yielding inputs (more so from RCC), there is no evidence that either of the two credit types had a statistically significant effect on the area cultivated with maize overall. We conclude that in the short run, both RCC and TC encourage agricultural intensification as opposed to expansion, at least for maize.

In recent literature, increasing households' farm production—and thereby enhancing their livelihoods and welfare—via the sustainable intensification route is a subject that has gained attention. Indeed, it is currently considered a climate-smart agricultural practice. The results of this chapter confirm that agricultural credit, particularly when coupled with cover against production risks, has a significant role to play in promoting sustainable intensification in climate-smart agriculture.

However, it is possible that such financial products can be associated with expansion in the long term. For instance, when optimal acreage productivity is reached, and a farm experiences diminishing marginal returns from additional investment, a rational household may then decide to put additional credit into a new field (e.g., using its own land previously left fallow, renting land, or even borrowing land).

Further, random assignment to and uptake of RCC did not have any direct, statistically significant effect on all ex-post productivity (maize yield and acreage revenue) and welfare (CSI and HDDI) outcomes. TC effects, both ITT and LATE, are similar except for maize yield, which, besides yielding a negative coefficient, is marginally significant.

Finally, using SEM, we evaluate the impact pathways via which ITT and TC could be influencing the outcomes. First, we find that credit rationing did not affect the uptake of the offered credit products. This might sound disappointing to the credit-rationing theory and literature but need not be. It does not invalidate the fact that credit rationing, either quantity or risk rationing, is a hindrance to smallholders' access to credit when in their natural habitat. Contrary to this, it reveals that in the absence of rationing, demand for credit will be equal for the (currently) rationed and non-rationed households. This is because the study involved a random sample, and credit offering was not subject to their pre-existing characteristics (credit rationing inclusive).

SEM results indicate that credit rationing is indeed an intermediate factor between credit uptake and households' productivity and welfare. Rationed households had lower yields, realized lower revenues, and were more food insecure. Luckily, both RCC and TC reduced the likelihood of being credit rationed in the subsequent year/season.

Notably, credit rationing also had a negative effect on investment ability, although this was not statistically significant. It is also evident that investment ability was a strong positive mediator between credit uptake and higher-level productivity and welfare outcomes. Yields and farm revenues increased while food insecurity decreased in line with investment ability. This may sound obvious, but it is not. The central finding here is that investment ability is the main route through which credit uptake influences productivity and welfare. In fact, in the SEM models for yield and revenue, RCC and TC are positively but insignificantly associated with the two outcomes, which means that their impact on household productivity is fully mediated by investment ability, credit rationing, and possibly several other factors not included in the model.

Lastly, household productivity is also a mediator between credit uptake and welfare, wherein more productive households (in terms of yields and farm revenue) were less food secure. The direct paths from RCC and TC to CSI were fully mediated by credit rationing, investment ability, and productivity and revenue.

## 5.2 Robustness checks

To validate the results, we conducted a number of robustness checks. First, to determine whether the estimates were robust to attrition, we use the Tauchmann (2014) approach to bound ITT estimates for all the outcomes following Lee (2009). In these estimations, we also weight the observations with an inverse probability of being re-interviewed/reached at midline and endline to account for any possible form of non-random attrition. These tests are based on ITT SMD. The Lee bounds results can be found in Appendix table A3. Most of the point estimates and statistical significance in the main result tables are close to the estimated bounds.

Second, we conduct multiple hypothesis test corrections. In this study, we estimate the treatment effects of two credit types (RCC and TC) on nine outcomes, translating to 18 hypotheses tests in total. This raises the false discovery rate concern where significant

coefficients may emerge by chance when there are a large number of measured outcomes and tested hypotheses, even when there are no true treatment effects on the outcomes (Anderson 2008; Benjamini, Krieger, and Yekutieli 2006; Omotilewa et al. 2018; Romano and Wolf 2005; Williams, Jones, and Tukey 1999).

To check for the robustness of our results to these potential biases, we conduct multiple hypotheses correction tests following Omotilewa et al. (2018) and Ksoll et al. (2016), who used sharpened q-values proposed by Benjamini, Krieger, and Yekutieli (2006) and empirically validated by Anderson (2008). We conduct these procedures for the ITT fixed effects and LATE fixed effects estimations separately. Comparing the standard p-values with the respective sharpened q-values (in Appendix table A5 for ITT, and Appendix table A6 for LATE) confirms that the results are largely robust across the specifications where q-values would not change the conclusions already reached with p-values.

Third, we appreciate that both the control and treatment households would possibly access credit from sources other than the project offerings. Indeed, 16% of the control and 10% of the treatment households reported having taken credit from other sources during the first phase of the project (September 2017–February 2018 season), while about 20% of both control and treatment households reported having taken such credit during the second phase of the project (September 2019–February 2020).

This could contaminate the experimental design and therefore compromise the validity of the results. Access to other types of credit by control households could lead to attenuation bias in the ITT and LATE estimates, while access to such credit by treatment compliers would lead to inflated estimates. For the treatment non-compliers, access to such credit will inflate the ITT estimates and attenuate LATE.

We test for the robustness of our results to this contamination by dropping contaminated households (who received credit from outside the intervention) and running the treatment effect estimations for all outcomes again. We test this for the fixed-effects models only (ITT fixed effects and LATE fixed effects). The treatment effects results in which contaminated households are dropped can be found in Appendix table A7. The results are largely consistent even after dropping the contaminated households. Most have minor and non-monotonic variations between the original and contamination-adjusted coefficients.

Lastly, we have also run various model specifications—namely, SMD, ANCOVA, and fixed effects—to estimate both ITT and LATE. Estimates from the three estimators are similar and consistent, with minor differences among them, which speaks to the robustness of the results. Further, as expected, ITT effects were much lower (but consistent among themselves) than LATE effects (also consistent among themselves). These consistencies within ITT and LATE and what is known about them, coupled with robustness shown through other means, reaffirm the confidence in our estimations.

## 6. Conclusions and recommendations

Drought has been identified as one of the major risks to agricultural productivity for the farmers in Sub-Saharan Africa. The impact of drought in this region is exacerbated by households' limited and ineffective options for managing risk, and the fact that access to formal insurance is often not one of them. In the last few decades, weather index crop insurance has been attempted in this region and in other low-income countries but has

been marred by frustratingly low uptake and unsustainability beyond pilot and experimental phases.

Insurance-linked credit has emerged as a promising intervention that links insurance directly to agricultural credit to protect both borrower and lender against specified contingent risks. There is limited literature on the uptake of these innovations, and a particular lack of empirical literature on the potential impacts of such products on smallholders from these regions. Using baseline data and two follow-up survey datasets from an RCT in Kenya, we analyze the impact of RCC on farm investment, productivity, and welfare of sampled households from Machakos County, Kenya.

Our impact evaluation results indicate that access to credit, regardless of the type, can improve farmers' agricultural investment, which in turn helps to transform their livelihoods and quality of life. We find that both RCC and TC had direct, positive and significant effects on agricultural investment, particularly fertilizer spending. They both seemingly did not have a direct effect on ex-post productivity and welfare outcomes, but results from SEM reveal that such credits would enhance productivity and welfare indirectly, through intermediary outcomes.

Additionally, compared to TC, RCC had higher ex-ante investment benefits, and this was particularly high and significant in the adoption of chemical fertilizers. This indicates that access to credit, regardless of the type, may potentially have a positive effect on smallholders' investment, productivity, and welfare. Investigations for such effects must acknowledge that higher-level ex-post outcomes may be affected indirectly; therefore, the analysis of impact pathways is a critically important means of reaching a meaningful conclusion. This is particularly true in areas such as Machakos County, where agricultural credits have been perennially absent, especially among the smallholder households. Moreover, the uptake of insurance-bundled credit was significantly higher than that of TC. This calls for policymakers to ensure that safe and inexpensive production credit products are provided among agricultural households if widespread benefits of agricultural credit are to be realized.

Our findings suggest that developing policies to hedge smallholders against systemic shocks, such as drought, is one way of enhancing access to credit. The use of formal insurance markets is one such viable policy, as it transfers risk away from households and therefore protects their collateral. Bundling insurance with credit also minimizes the risk of default by smallholder borrowers, which abates financial risks to lenders that threaten their business stability—a common phenomenon when rural agricultural production systems experience systemic shocks such as drought.

Financial institutions keen on growing their agricultural lending portfolio may then consider insurance-linked products to offer cheaper and safer loans to farmers. However, insurance and credit providers should provide greater transparency to farmers through continuous index updates using a simplified pictorial tool. In this regard, digital technologies such as mobile phones could be an effective communication tool. In a situation where climate change is expected to place unprecedented strain on smallholder agricultural systems, RCC can not only help to increase the resilience of the African food system, but can also improve smallholders' productivity and growth outcomes.

Although we scientifically designed RCC as a product that can reduce intertemporal basis risk by incorporating crop phenology into the insurance pricing framework, the lack of reliable historical crop-yield data has been a hindrance to designing an improved insurance mechanism that could significantly reduce basis risk. Ideally, estimating and predicting yield using the weather indices would be more preferred. This is common in low-income economies.

Therefore, we recommend that governments promote large-scale data collection and make this available to researchers. In this regard, big data and machine learning techniques could improve index construction by inducing improved model specifications and prediction of expected crop yields. Further research in this strand of literature is warranted, particularly in trying alternative indices, building composite indices, developing multiple crops, and/or whole-farm index insurance and insurance-linked products.

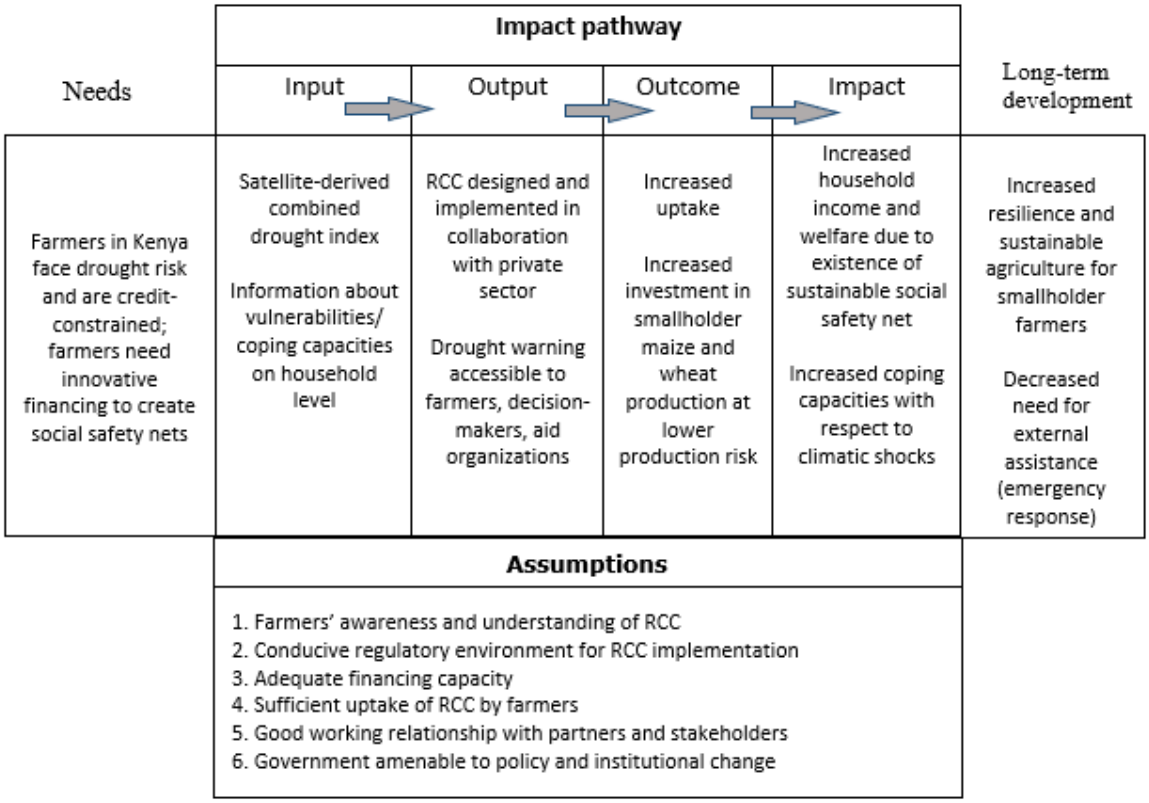
Productivity, welfare, and resilience outcomes are higher-level benefits that may be harder to achieve and estimate with limited experiment periods. Also, this study may have suffered low power due to the low uptake of credit offered. As such, although every effort to control for any form of endogeneity was considered, we cautiously interpret and claim causality and recommend that the impact results and interpretations are treated as indicative. We hope that this motivates further investigation, particularly with longer panels, a variety of credit and insurance products, and higher uptake.

Further, financial literacy remains very low among smallholder farmers in low- and middle-income country settings. Although financial education through Equity Bank was part of the RCC protocol, we recommend investigation into the ideal education and extension methods for training farmers on such products, ensuring that key details are covered while maintaining a level of simplicity congruent with low literacy and numeracy among the rural smallholders in the region. In this regard, providing financial education using simplified games through extension services at the county level could be an impactful government policy.

Finally, as recent literature suggests, women are important agricultural producers, especially in Africa south of the Sahara, but they face greater credit constraints than men. It is, therefore, worth exploring how more women farmers can be included in the RCC program and how it improves women’s access to credit and empowerment in society.

# Appendix A: Supporting figures and tables

## Figure A1: Theory of change and logical framework




## Figure A2: RCC and TC: Impact study design and randomization structure

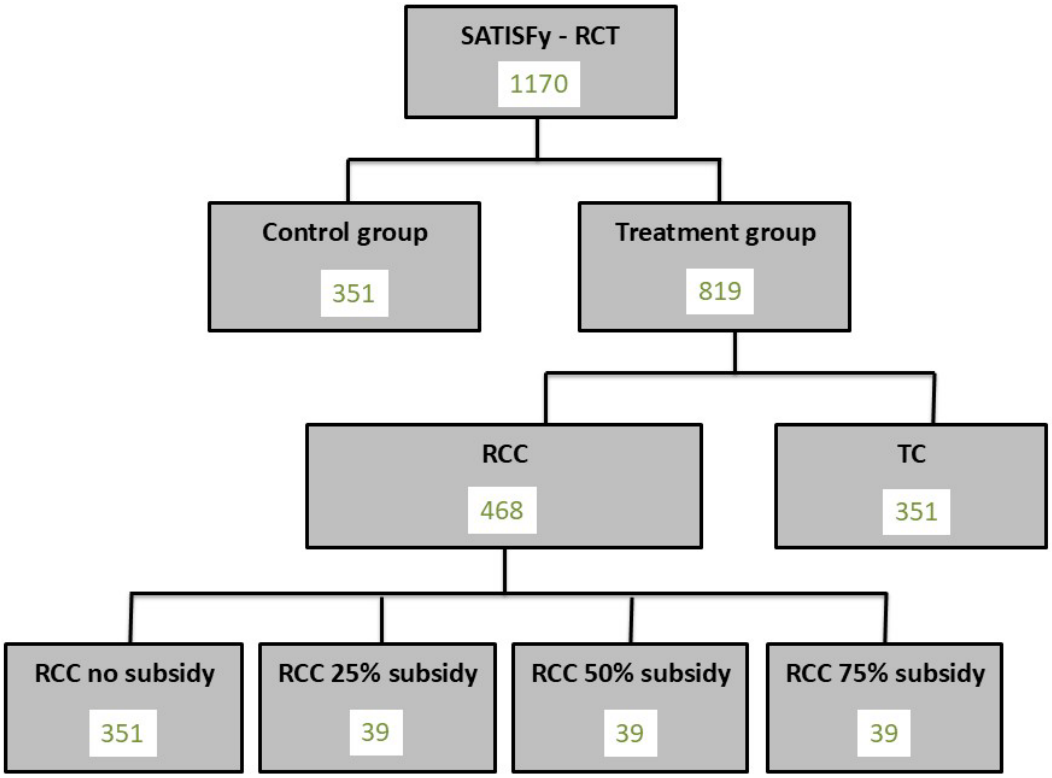


**Figure A3: Timeline for the RCT**

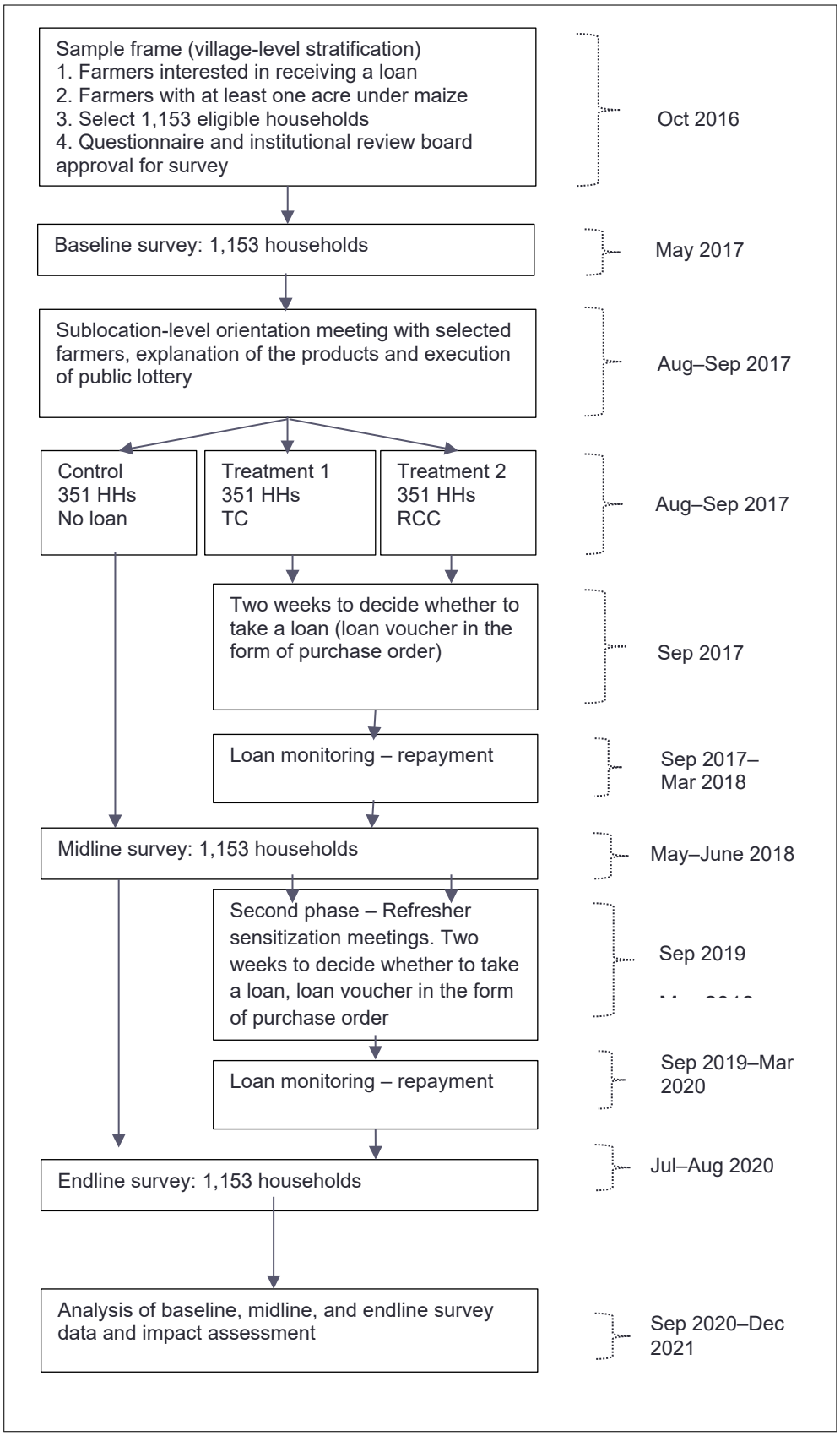


**Table A1: First stage results for the two-stage least squares models**

| VARIABLES | Panel A: Complete panel: T=0, T=1 and T=2 | | | Panel B: At midline: T=0 and T=1 | | |
|---|---|---|---|---|---|---|
| | Pooled uptake | RCC uptake | TC uptake | Pooled uptake | RCC uptake | TC uptake |
| Assigned RCC or TC = 1 | 0.241*** | | | 0.334*** | | |
| | (0.010) | | | (0.013) | | |
| Assigned RCC = 1 | | 0.263*** | | | 0.360*** | |
| | | (0.009) | | | (0.012) | |
| Assigned TC = 1 | | | 0.218*** | | | 0.308*** |
| | | | (0.008) | | | (0.011) |
| Constant | -0.003 | -0.021 | 0.020 | -0.045 | -0.040 | -0.003 |
| | (0.043) | (0.031) | (0.029) | (0.050) | (0.036) | (0.034) |
| Observations | 3,057 | 3,057 | 3,057 | 2,065 | 2,065 | 2,065 |
| R-squared | 0.16 | 0.22 | 0.18 | 0.26 | 0.32 | 0.28 |

*Standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10.*

**Table A2: Attrition rate by experimental groups and survey round**

| | Baseline | Midline | | | Endline | | |
|---|---|---|---|---|---|---|---|
| Exited/dropped | | No | Yes | Attrition % | No | Yes | Attrition % |
| Control | 351 | 337 | 14 | 4.15 | 337 | 15 | 4.45 |
| TC | 351 | 340 | 11 | 3.24 | 333 | 17 | 5.11 |
| RCC | 351 | 343 | 8 | 2.33 | 332 | 19 | 5.72 |
| Overall | 1,053 | 1,020 | 33 | 3.24 | 1,002 | 51 | 5.09 |

**Figure A4: Distribution of risk aversion for pooled sample and by treatment groups**

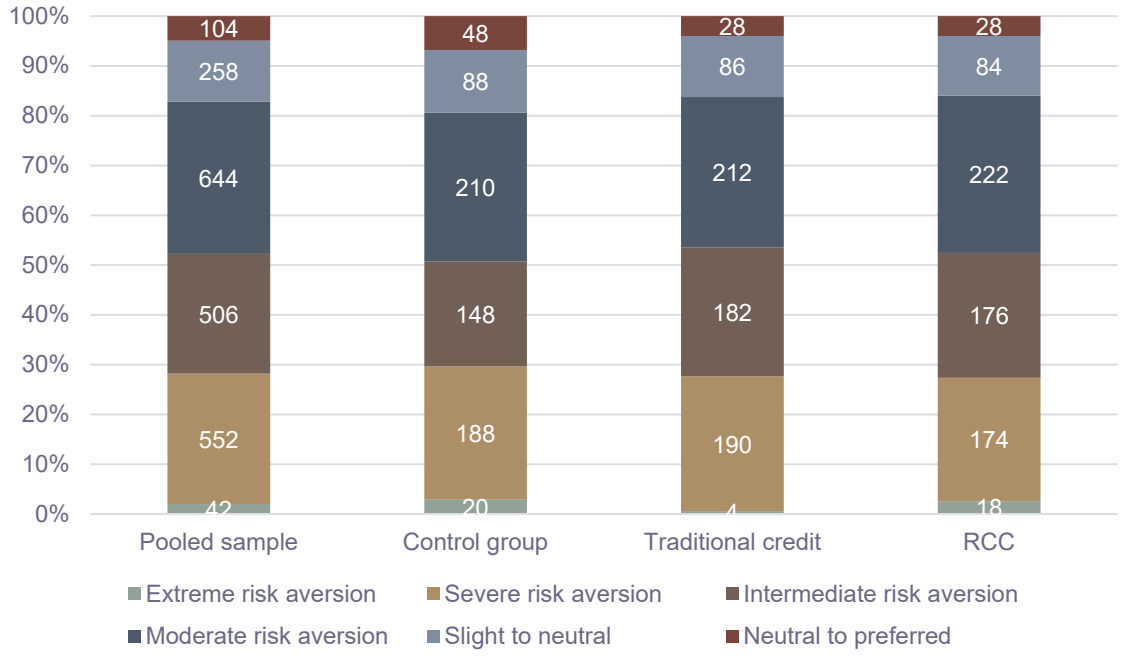


**Figure A5: Uptake of TC and RCC by baseline risk-aversion categories**

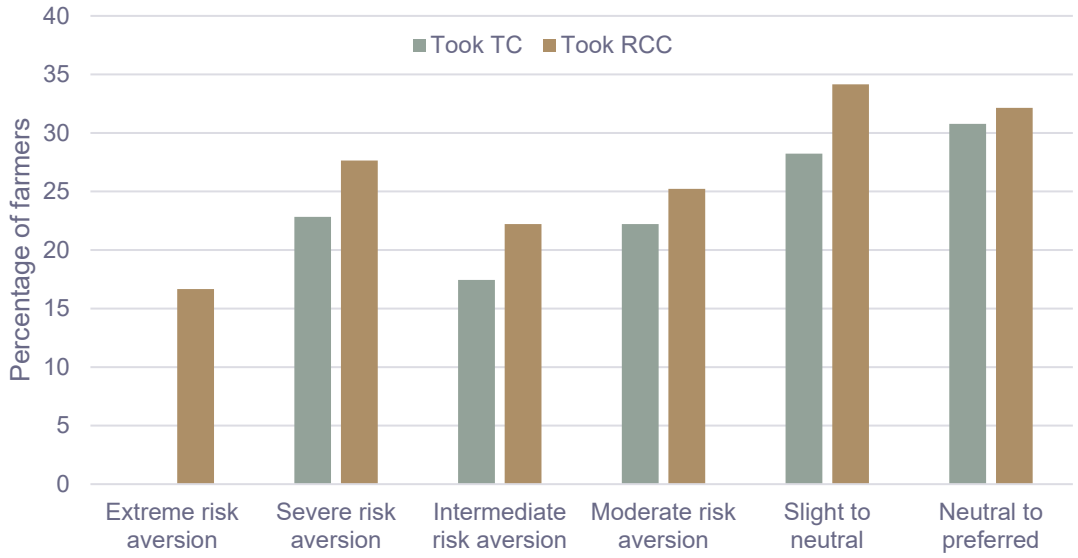



**Table A3: ITT fixed effects-based Lee bounds estimates to account for attrition bias on treatment effects**

| | RCC | | TC | |
|---|---|---|---|---|
| | Lower | Upper | Lower | Upper |
| Fertilizer binary | 0.025 | 0.032 | -0.005 | 0.000 |
| | (0.019) | (0.021) | (0.020) | (0.021) |
| Fertilizer spending | 378.192*** | 439.604*** | -128.219 | -99.507 |
| | (112.559) | (101.579) | (100.142) | (93.077) |
| Improved maize variety binary | .026* | .034* | -0.026 | -0.021 |
| | (0.015) | (0.017) | (0.016) | (0.018) |
| Improved maize variety share | 2.477 | 3.227* | -3.051* | -2.535 |
| | (1.615) | (1.789) | (1.688) | (1.839) |
| Area under maize | -0.12 4 | -0.004 | 0.023 | 0.098 |
| | (0.130) | (0.094) | (0.116) | (0.091) |
| Maize yield | -66.244 | -28.421 | -98.96 | -76.711 |
| | (68.708) | (59.817) | (66.387) | (58.432) |
| Overall acreage revenue | -2,445.862* | -1,329.124 | -520.768 | 261.131 |
| | (1,349.731) | (1,009.930) | (1,411.771) | (1,036.463) |

*Notes: Robust standard errors are in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.*

## Table A4: Testing for random attrition

| VARIABLES | (1) Control | (2) T. Credit | (3) RCC | (4) Overall | (5) Overall multivariate |
|---|---|---|---|---|---|
| ***Treatment variables*** | | | | | |
| Assigned TC | | | | -0.194 | -0.119 |
| | | | | (0.151) | (0.153) |
| Assigned RCC | | | | -0.225 | -0.195 |
| | | | | (0.153) | (0.156) |
| ***Outcome variables*** | | | | | |
| Assigned TC | -0.430 | 0.402 | -0.222 | | -0.158 |
| | (0.324) | (0.272) | (0.334) | | (0.181) |
| Binary maize fertilizer use | 0.000 | -0.000 | -0.000 | | 0.000 |
| | (0.000) | (0.000) | (0.000) | | (0.000) |
| Amount spent on maize fertilizer | 1.219* | -0.118 | 0.513 | | 0.355 |
| | (0.663) | (0.570) | (0.629) | | (0.360) |
| Binary improved maize variety | -0.011* | 0.001 | -0.005 | | -0.004 |
| | (0.006) | (0.006) | (0.006) | | (0.003) |
| Share of land under improved | -0.008 | -0.101 | 0.011 | | -0.001 |
| | (0.066) | (0.080) | (0.056) | | (0.035) |
| Area under maize | 0.000 | 0.000 | -0.000 | | -0.000 |
| | (0.000) | (0.000) | (0.000) | | (0.000) |
| Maize yields | -0.000 | -0.000 | 0.000* | | -0.000 |
| | (0.000) | (0.000) | (0.000) | | (0.000) |
| Acreage overall revenue "000" | 0.020 | 0.007 | -0.024 | | 0.003 |
| | (0.015) | (0.016) | (0.022) | | (0.010) |
| Coping strategy index | -0.001 | 0.030 | -0.088 | | -0.006 |
| | (0.064) | (0.063) | (0.082) | | (0.041) |
| Dietary diversity index | -0.004 | 0.010 | -0.011 | | 0.003 |
| | (0.009) | (0.008) | (0.010) | | (0.005) |
| ***Socioeconomic controls*** | | | | | |
| Age of household head | -0.262 | -0.201 | 0.024 | | -0.132 |
| | (0.274) | (0.262) | (0.323) | | (0.168) |
| Household head gender = Male | -0.063* | -0.001 | -0.004 | | -0.014 |
| | (0.033) | (0.029) | (0.037) | | (0.019) |
| Head's years of education | 0.055 | 0.100 | 0.085 | | 0.057 |
| | (0.229) | (0.207) | (0.251) | | (0.138) |
| Risk rationed | . | 0.167 | -0.177 | | -0.271 |
| | | (0.299) | (0.445) | | (0.246) |
| Quantity rationed | -0.000 | -0.001 | -0.003 | | -0.001 |
| | (0.001) | (0.002) | (0.002) | | (0.001) |
| Dependency ratio | 0.408*** | 0.121** | -0.007 | | 0.011 |
| | (0.139) | (0.061) | (0.024) | | (0.015) |
| Per capita land ownership | -0.098 | -0.031 | -0.000 | | -0.003 |
| | (0.061) | (0.044) | (0.005) | | (0.006) |
| Tropical livestock units holding | 0.028 | 0.181 | -0.150 | | -0.021 |
| | (0.196) | (0.171) | (0.187) | | (0.108) |
| Extension services access index | 1.285** | 0.475 | -0.055 | | 0.523* |
| | (0.506) | (0.426) | (0.520) | | (0.275) |
| Constant | -1.705 | -3.062*** | 0.450 | -1.975*** | -1.722** |
| | (1.180) | (1.088) | (1.327) | (0.194) | (0.733) |
| Observations | 626 | 697 | 698 | 2,117 | 2,089 |
| Number of unique households | 313 | 349 | 349 | 1,065 | 1,045 |

*Notes: Robust standard errors are in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.*

**Table A5: Multiple hypotheses testing using the ITT fixed effects models**

| | Fertilizer use binary | Fertilizer spending | Improved varieties adoption | Improved varieties share | Maize area | Maize yields | Overall acreage revenue | CSI | HDDI |
|---|---|---|---|---|---|---|---|---|---|
| RCC | 0.0358 | 511.7*** | 0.0114 | 0.428 | 0.0669 | -33.26 | -1483.4 | 0.739 | -0.104 |
| p-value | (0.217) | (0.000) | (0.658) | (0.872) | (0.634) | (0.662) | (0.276) | (0.197) | (0.455) |
| Sharpened q-value | [0.598] | [0.001] | [0.931] | [0.931] | [0.931] | [0.931] | [0.598] | [0.598] | [0.931] |
| TC | 0.0462 | 214.6* | 0.00547 | -0.449 | 0.0643 | -121.0 | -2,232.3 | 0.818 | -0.0894 |
| p-value | (0.110) | (0.052) | (0.839) | (0.865) | (0.642) | (0.112) | (0.131) | (0.121) | (0.531) |
| Sharpened q-value | [0.552] | [0.552] | [0.931] | [0.931] | [0.931] | [0.552] | [0.552] | [0.552] | [0.931] |

*Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}P < 0.1$.*

**Table A6: Multiple hypotheses testing using the LATE fixed effects models**

| | Fertilizer use binary | Fertilizer spending | Improved varieties adoption | Improved varieties share | Maize area | Maize yields | Overall acreage revenue | CSI | HDDI |
|---|---|---|---|---|---|---|---|---|---|
| RCC | 0.137 | 1,918.5*** | 0.0434 | 1.636 | 0.255 | -126.9 | -5,664.7 | 2.060 | -0.291 |
| p-values | (0.217) | (0.000) | (0.658) | (0.872) | (0.633) | (0.661) | (0.275) | (0.197) | (0.4560 |
| sharpened q-values | [0.667] | [0.001] | [0.950] | [0.950] | [0.950] | [0.950] | [0.667] | [0.667] | [0.950] |
| TC | 0.212 | 964.8** | 0.0249 | -2.061 | 0.293 | -551.1 | -10,175.9 | 2.650 | -0.290 |
| p-values | (0.110) | (0.047) | (0.838) | (0.865) | (0.642) | (0.113) | (0.133) | (0.122) | (0.531) |
| sharpened q-values | [0.607] | [0.607] | [0.950] | [0.950] | [0.950] | [0.607] | [0.607] | [0.607] | [0.950] |

*Notes: $^{***}p < 0.01$, $^{*}p < 0.05$, $^{*}p < 0.1$.*

**Table A7: Treatment effects (ITT and LATE) on all outcomes after dropping the contaminated observations/households**

| VARIABLES | (1) Fertilizer use binary | (2) Fertilizer spending | (3) Improved varieties adoption | (4) Improved varieties share | (5) Maize area | (6) Maize yields | (7) Overall acreage revenue | (8) CSI | (9) HDDI |
|---|---|---|---|---|---|---|---|---|---|
| *Panel A: Contamination adjusted ITT FE estimates* | | | | | | | | | |
| Assigned RCC | 0.036 | 545.233*** | 0.013 | 0.932 | 0.043 | -13.364 | -1,024.018 | 0.624 | -0.053 |
| | (0.031) | (129.511) | (0.027) | (2.773) | (0.149) | (79.717) | (1,415.757) | (0.625) | (0.150) |
| Assigned TC | 0.038 | 253.719** | 0.008 | -0.265 | 0.048 | -102.993 | -2,088.517 | 0.695 | -0.052 |
| | (0.030) | (117.266) | (0.028) | (2.739) | (0.148) | (80.000) | (1,527.470) | (0.569) | (0.153) |
| Constant | 0.671*** | 1,203.557*** | 0.850*** | 76.911*** | 2.028*** | 903.680*** | 13,666.489*** | 5.932*** | 10.545*** |
| | (0.008) | (31.987) | (0.007) | (0.697) | (0.037) | (19.725) | (375.938) | (0.110) | (0.028) |
| Observations | 2,748 | 2,689 | 2,729 | 2,748 | 2,736 | 2,727 | 2,734 | 1,944 | 1,944 |
| R-squared | 0.0571 | 0.128 | 0.00529 | 0.0136 | 0.0258 | 0.278 | 0.226 | 0.0970 | 0.0450 |
| *Panel B: Contamination adjusted LATE FE estimates* | | | | | | | | | |
| Took RCC | 0.134 | 1,978.850*** | 0.049 | 3.455 | 0.157 | -49.750 | -3,795.440 | 1.726 | -0.148 |
| | (0.115) | (457.871) | (0.100) | (10.279) | (0.550) | (293.795) | (5,228.396) | (1.728) | (0.416) |
| Took TC | 0.165 | 1,097.278** | 0.035 | -1.107 | 0.209 | -442.579 | -9,025.684 | 2.240 | -0.168 |
| | (0.132) | (486.077) | (0.121) | (11.933) | (0.641) | (345.814) | (6,624.766) | (1.834) | (0.493) |
| Constant | 0.671*** | 1,204.376*** | 0.850*** | 76.909*** | 2.028*** | 903.069*** | 13,652.786*** | 5.932*** | 10.545*** |
| | (0.008) | (31.003) | (0.007) | (0.697) | (0.037) | (19.766) | (375.419) | (0.110) | (0.028) |
| Observations | 2,748 | 2,689 | 2,729 | 2,748 | 2,736 | 2,727 | 2,734 | 1,944 | 1,944 |
| R-squared | 0.0666 | 0.152 | 0.0116 | 0.0151 | 0.0256 | 0.278 | 0.225 | 0.0961 | 0.0449 |

*Notes: FE = fixed effects. Robust standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10.*

# Appendix B: Checking for heterogeneity in the treatment/uptake effects

### Table B1: RCC and TC heterogenous treatment (uptake) effects on use of fertilizer on maize fields

| Variables | Fertilizer use – binary response | | | | Fertilizer spending (KES) | | | |
|---|---|---|---|---|---|---|---|---|
| | Male | Age | Educatio | CRRA | Male | Age | Education | CRRA |
| RCC | -0.008 | -0.018 | -0.052 | 0.503 | 5,021.181* | 179.935 | 315.435 | 6,182.489* |
| uptake # | (0.493) | (0.029) | (0.044) | (0.540) | (2,132.705 | (122.307) | (212.725) | (2,410.271 |
| TC uptake | 1.232 | 0.045 | 0.009 | 1.033 | -1,112.276 | -70.484 | -119.840 | -3,001.676 |
| # equity | (0.795) | (0.028) | (0.048) | (0.766) | (2,938.702 | (107.255) | (191.218) | (2,796.781 |
| RCC | 0.111 | 1.113 | 0.567 | -0.095 | - | -9,237.914 | -1,889.590 | -1,490.403 |
| | (0.575) | (1.635) | (0.397) | (0.210) | (2,512.965 | (6,942.420 | (1,967.975 | (992.652) |
| TC uptake | -1.363 | -2.423 | -0.012 | -0.331 | 1,956.398 | 4,524.072 | 1,752.039 | 1,815.835* |
| | (0.917) | (1.557) | (0.431) | (0.296) | (3,411.666 | (5,888.300 | (1,754.650 | (1,091.999 |
| Post | 0.103** | 0.103** | 0.102*** | 0.103** | 571.102*** | 578.932*** | 577.353*** | 572.578*** |
| | (0.013) | (0.013) | (0.012) | (0.012) | (50.986) | (51.701) | (51.501) | (51.068) |
| Constant | 0.671** | 0.671** | 0.671*** | 0.671** | 1,218.403* | 1,218.709* | 1,219.177* | 1,218.791* |
| | (0.008) | (0.008) | (0.008) | (0.008) | (32.314) | (33.616) | (32.053) | (31.575) |
| Observatio | 3,068 | 3,068 | 3,068 | 3,068 | 3,005 | 3,005 | 3,005 | 3,005 |
| Number of | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 |
| $R^2$ | 0.0084 | 0.0041 | 0.0207 | 0.0138 | 0.0450 | 0.0212 | 0.0468 | 0.0449 |

*Note: CRRA = constant relative risk aversion. Robust standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10.*

### Table B2: RCC and TC heterogenous treatment (uptake) effects on adoption of improved maize varieties

| Variables | Improved maize seed adoption (binary) | | | | Improved maize seed intensity (percentage) | | | |
|---|---|---|---|---|---|---|---|---|
| | Male | Age | Education | CRRA | Male | Age | Education | CRRA |
| RCC uptake # | -0.031 | -0.008 | -0.012 | 0.202 | -29.170 | -1.582 | 0.000 | 16.707 |
| equity variable | (0.431) | (0.026) | (0.039) | (0.574) | (45.429) | (2.674) | (4.173) | (57.033) |
| TC uptake # | -0.054 | 0.005 | -0.046 | 0.312 | -24.739 | -0.095 | -4.098 | -10.828 |
| equity variable | (0.722) | (0.026) | (0.045) | (0.759) | (69.072) | (2.392) | (4.485) | (72.562) |
| RCC uptake | 0.100 | 0.498 | 0.171 | -0.015 | 40.421 | 95.681 | 6.238 | -0.269 |
| | (0.504) | (1.472) | (0.352) | (0.224) | (53.178) | (151.254) | (37.275) | (22.312) |
| TC uptake | 0.146 | -0.208 | 0.504 | -0.036 | 35.934 | 12.434 | 44.614 | 11.399 |
| | (0.835) | (1.398) | (0.406) | (0.294) | (79.844) | (130.929) | (40.901) | (28.129) |
| Post | 0.017 | 0.017 | 0.017 | 0.017 | 2.172* | 2.102* | 2.189* | 2.143* |
| | (0.012) | (0.012) | (0.011) | (0.012) | (1.149) | (1.155) | (1.151) | (1.150) |
| Constant | 0.849*** | 0.849*** | 0.849*** | 0.849*** | 77.030*** | 77.035*** | 77.039*** | 77.032*** |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.716) | (0.715) | (0.712) | (0.709) |
| Observations | 3,047 | 3,047 | 3,047 | 3,047 | 3,068 | 3,068 | 3,068 | 3,068 |
| Number of | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 |
| $R^2$ | 0.0113 | 0.00564 | 0.00268 | 0.00633 | 0.00573 | 0.00515 | 0.00229 | 0.00899 |

*Notes: CRRA = constant relative risk aversion. Robust standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10.*

**Table B3: RCC and TC heterogenous treatment (uptake) effects on area under maize**

| Variables | Male | Age | Education | CRRA |
|---|---|---|---|---|
| RCC uptake # equity variable | -2.632 | -0.058 | -0.012 | -1.276 |
| | (2.837) | (0.149) | (0.204) | (2.904) |
| TC uptake # equity variable | 2.258 | 0.113 | -0.016 | 0.544 |
| | (3.105) | (0.126) | (0.249) | (3.310) |
| RCC uptake | 3.456 | 3.652 | 0.480 | 0.875 |
| | (3.323) | (8.407) | (1.843) | (1.128) |
| TC uptake | -2.460 | -6.057 | 0.314 | -0.044 |
| | (3.590) | (6.888) | (2.252) | (1.273) |
| Post | -0.127** | -0.129** | -0.130** | -0.129** |
| | (0.059) | (0.060) | (0.059) | (0.059) |
| Constant | 2.039*** | 2.039*** | 2.039*** | 2.039*** |
| | (0.037) | (0.038) | (0.037) | (0.037) |
| Observations | 3,056 | 3,056 | 3,056 | 3,056 |
| Number of unique households | 1,053 | 1,053 | 1,053 | 1,053 |
| $R^2$ | 7.96e-05 | 0.000553 | 0.000336 | 6.71e-06 |

*Notes: CRRA = constant relative risk aversion. Robust standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10.*

**Table B4: RCC and TC heterogenous treatment (uptake) effects on maize yield and acreage farm revenue**

| Variables | Maize yield per acre | | | | Acreage overall revenue | | | |
|---|---|---|---|---|---|---|---|---|
| | Male | Age | Education | CRRA | Male | Age | Education | CRRA |
| RCC uptake # equity variable | 1,227.455 | 79.144 | 124.128 | 1,848.859 | 24,580.005 | 1,207.042 | 118.472 | 35,432.341 |
| | (1,291.982) | (77.255) | (123.123) | (1,427.820) | (24,575.151) | (1,437.629) | (2,293.431) | (25,215.742) |
| TC uptake # equity variable | -362.582 | -27.996 | -1.388 | -1,782.756 | -28,751.212 | -1,001.596 | -341.400 | -43,412.690 |
| | (2,034.237) | (73.621) | (129.815) | (1,878.136) | (38,424.006) | (1,485.214) | (2,665.244) | (37,636.215) |
| RCC uptake | -1,961.123 | -4,997.072 | -1,631.234 | -1,247.173** | -38,656.979 | -78,096.988 | -10,947.589 | -23,740.617** |
| | (1,523.848) | (4,386.150) | (1,124.898) | (585.913) | (29,014.878) | (81,461.627) | (20,748.730) | (10,215.339) |
| TC uptake | -245.381 | 866.259 | -654.917 | 20.806 | 23,905.688 | 45,388.187 | -6,405.158 | 7,242.175 |
| | (2,380.740) | (4,057.416) | (1,205.217) | (747.285) | (44,741.117) | (81,789.554) | (24,581.337) | (14,719.236) |
| Post | 344.123*** | 347.389*** | 345.676*** | 344.099*** | 4,918.027*** | 4,970.766*** | 4,957.337*** | 4,923.017*** |
| | (34.161) | (34.213) | (34.117) | (34.137) | (640.553) | (641.923) | (634.714) | (639.841) |
| Constant | 904.169*** | 903.848*** | 904.270*** | 904.099*** | 13,603.490*** | 13,596.756*** | 13,605.310*** | 13,602.918*** |
| | (20.400) | (20.957) | (20.409) | (20.348) | (392.668) | (401.194) | (388.489) | (389.701) |
| Observations | 3,045 | 3,045 | 3,045 | 3,045 | 3,052 | 3,052 | 3,052 | 3,052 |
| Number of unique households | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 |
| $R^2$ | 0.0110 | 0.00728 | 0.0140 | 0.0110 | 0.00630 | 0.00512 | 0.0107 | 0.00607 |

*Notes: CRRA = constant relative risk aversion.* Robust standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10

**Table B5: RCC and TC heterogenous treatment (uptake) effects on household food security measured with CSI and HDDI**

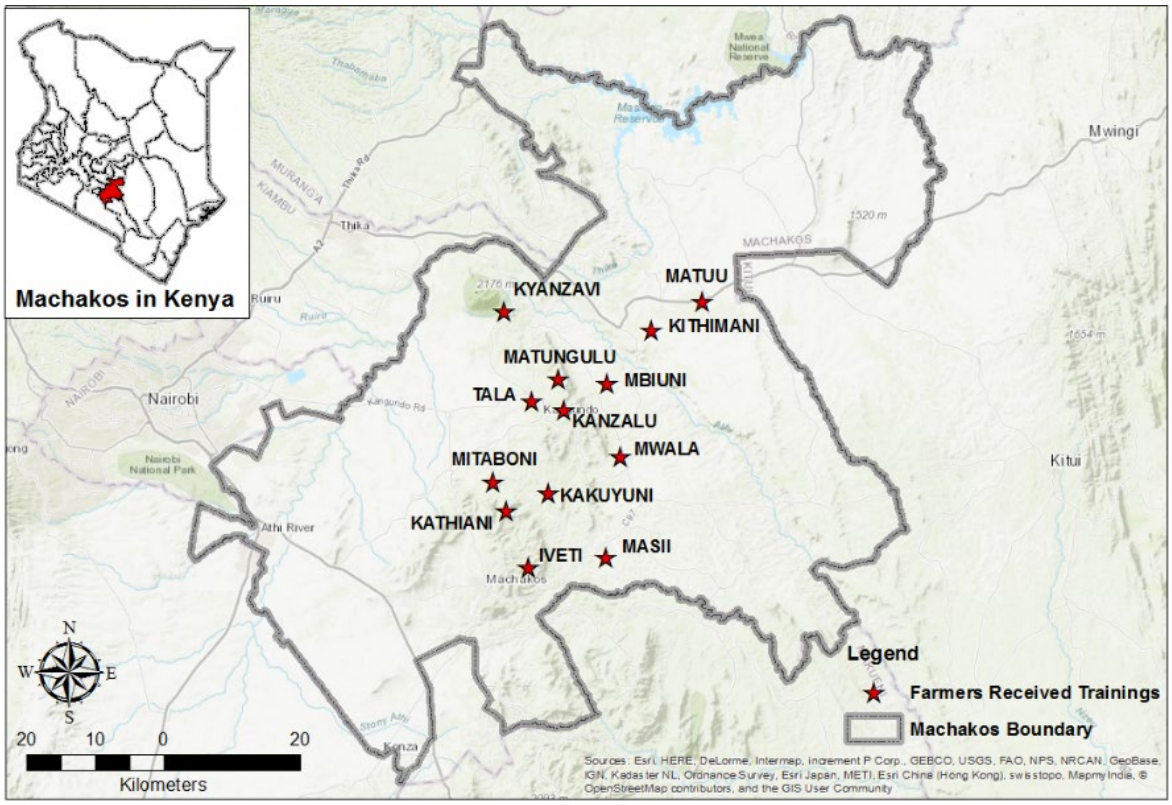
| Variables | Coping strategy index (CSI) | | | | HDDI | | | |
|---|---|---|---|---|---|---|---|---|
| | Male | Age | Education | CRRA | Male | Age | Education | CRRA |
| RCC uptake # equity variable | -11.228 | -0.292 | 1.352* | -10.316 | -1.908 | -0.064 | -0.390* | 0.587 |
| | (10.805) | (0.550) | (0.743) | (10.607) | (2.204) | (0.123) | (0.201) | (2.346) |
| TC uptake # equity variable | -6.492 | 0.170 | 1.437* | 2.535 | 1.695 | -0.032 | -0.108 | 3.962 |
| | (13.342) | (0.458) | (0.744) | (11.851) | (3.190) | (0.119) | (0.209) | (3.150) |
| RCC uptake | 15.219 | 18.587 | -9.979 | 6.109 | 2.595 | 3.951 | 3.835** | 0.130 |
| | (12.614) | (31.087) | (6.743) | (4.155) | (2.581) | (6.962) | (1.836) | (0.931) |
| TC uptake | 9.573 | -7.287 | -11.078 | 1.059 | -1.594 | 2.124 | 1.359 | -1.153 |
| | (15.430) | (25.065) | (6.896) | (4.546) | (3.700) | (6.505) | (1.932) | (1.222) |
| Post | -4.297*** | -4.312*** | -4.315*** | -4.301*** | -0.027 | -0.032 | -0.029 | -0.028 |
| | (0.218) | (0.220) | (0.222) | (0.218) | (0.056) | (0.055) | (0.056) | (0.056) |
| Constant | 5.814*** | 5.816*** | 5.813*** | 5.814*** | 10.560*** | 10.560*** | 10.560*** | 10.560*** |
| | (0.139) | (0.137) | (0.139) | (0.136) | (0.034) | (0.034) | (0.034) | (0.034) |
| Observations | 3,068 | 3,068 | 3,068 | 3,068 | 3,068 | 3,068 | 3,068 | 3,068 |
| Number of unique households | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 | 1,053 |
| $R^2$ | 0.0745 | 0.0932 | 0.0507 | 0.102 | 0.00164 | 0.000344 | 0.000225 | 0.000516 |

*Notes: CRRA = constant relative risk aversion. Robust standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10.*

**Table B6: SEM results with regressions**

| | Yield model | | Revenue model | | Food insecurity (CSI) model | | Regression models | |
|---|---|---|---|---|---|---|---|---|
| | Coef. | SE | Coef. | SE | Coef. | SE | Specification | Abbreviations |
| **Uptake of RCC** | | | | | | | $Y_i = \alpha + \beta_1 T_i + \beta_2 R_i + \varepsilon_i$ | - Y represents the respective outcome variable in bold in the first column |
| Assigned RCC | 0.276*** | 0.009 | 0.276*** | 0.009 | 0.276*** | 0.009 | | |
| Credit rationed | -0.003 | 0.016 | -0.003 | 0.016 | -0.003 | 0.016 | | |
| **Uptake of TC** | | | | | | | $Y_i = \alpha + \beta_1 T_i + \beta_2 R_i + \varepsilon_i$ | - T represents treatment categories where T = 0 if control group, T = 1 if assigned to TC group, and T = 2 if assigned to RCC group |
| Assigned TC | 0.233*** | 0.009 | 0.233*** | 0.009 | 0.233*** | 0.009 | | |
| Credit rationed | 0.01 | 0.017 | 0.01 | 0.017 | 0.01 | 0.017 | | |
| **Credit rationing** | | | | | | | $Y_i = \alpha + \beta_1 \widehat{RCC_\iota} + \beta_2 \widehat{TC_\iota} + \varepsilon_i$ | - R represents credit rationing where R = 1 if a household is credit rationed and R = 0 if not credit rationed |
| Took RCC | -0.17** | 0.081 | -0.17** | 0.081 | -0.17** | 0.081 | | |
| Took TC | -0.278*** | 0.097 | -0.278*** | 0.097 | -0.278*** | 0.097 | | |
| **Investment ability** | | | | | | | $Y_i = \alpha + \beta_1 \widehat{RCC_\iota} + \beta_2 \widehat{TC_\iota} + \beta_3 R_i + \varepsilon_i$ | - $\widehat{RCC}$ and $\widehat{TC}$ represent predicted uptake of RCC and TC, respectively |
| Took RCC | 0.13*** | 0.025 | 0.13*** | 0.025 | 0.13*** | 0.025 | | |
| Took TC | 0.121*** | 0.027 | 0.12*** | 0.027 | 0.121*** | 0.027 | | |
| Credit rationed | -0.009 | 0.012 | -0.009 | 0.012 | -0.009 | 0.012 | | |
| **Productivity outcome** | | | | | | | $Y_i = \alpha + \beta_1 \widehat{RCC_\iota} + \beta_2 \widehat{TC_\iota} + \beta_3 R_i + \beta_4 X_i + \varepsilon_i$ | - X represents investment ability – latent/unobserved capacity driving the households' adoption decision and intensity of higher-yielding improved varieties and chemical fertilizers |
| Took RCC | 209.1 | 183.212 | 297.17 | 3,245.433 | 287.387 | 3,245.499 | | |
| Took TC | 243.558 | 217.09 | 7,523.47** | 3867.181 | 7,512.314** | 3,867.151 | | |
| Credit rationed | -149.485*** | 42.668 | -3,227.554*** | 757.191 | -3,227.587*** | 757.097 | | |
| Investment ability | 535.75*** | 66.885 | 7,243.822*** | 1,188.38 | 7,236.618*** | 1,187.758 | | |
| **Welfare outcome - CSI** | | | | | | | $Y_i = \alpha + \beta_1 \widehat{RCC_\iota} + \beta_2 \widehat{TC_\iota} + \beta_3 R_i + \beta_4 X_i + \beta_5 Z_i + \varepsilon_i$ | - Z represents productivity indicator, proxied by farm revenue in this case |
| Took RCC | | | | | -0.386 | 0.441 | | |
| Took TC | | | | | -0.635 | 0.479 | | |
| Credit rationed | | | | | 1.491*** | 0.209 | | |
| Investment ability | | | | | -0.788** | 0.348 | | |
| Revenue | | | | | -0.001*** | 0.000 | | |

## Appendix C: RCC study area- Machakos county, Kenya



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