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Impact of Risk-Contingent Credit and Traditional Credit on Smallholders' Agricultural Investment and Productivity

Experimental Evidence from Kenya

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Notices

Abstract

We use a multiyear, multi-arm randomized controlled trial implemented among 1,053 smallholders in Kenya to evaluate ex-ante investment and ex-post productivity and welfare benefits of two competing lending models: risk-contingent credit (RCC)—which embeds crop insurance with a loan product—and traditional credit (TC). We rely on local average treatment effects to demonstrate the effects of these alternative credit products on borrowers but report the intention-to-treat effects for their broader policy significance. Uptake of RCC increased treated households' farm investments—specifically, adoption of chemical fertilizers—by up to 14 percent along the extensive margins and by more than 100 percent along the intensive margins, while TC's effects were less in both magnitude and statistical significance. Neither type of credit product had a significant effect on the overall area cultivated under maize, hence enhancing agricultural intensification but not extensification. Ex-post, neither type of credit product had a strong direct effect on households' productivity. We conclude that access to credit has potential to increase investment and productivity among smallholders, although improved productivity needs better measurement and extended intervention to be realized. To scale the potential effects of credit, derisking access to credit should be considered to expand access to credit.

Keywords: Credit, insurance-linked credit, investment, productivity, welfare, sub-Saharan Africa, smallholders

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1 Introduction

Sub-Saharan Africa (SSA) has achieved some remarkable agricultural development in recent decades. Agricultural value added more than doubled and has grown faster in SSA than in any region in the world over the 2000–2020 period (World Bank, 2024). Despite this impressive growth in agricultural value addition, about 75 percent of this growth has come from expansion of cultivated areas, with only 25 percent attributable to increases in agricultural productivity (Jayne & Sanchez, 2021). With only 1.6 metric tons per hectare, cereal yields in SSA are the lowest in the world, remain well below the world average (4 metric tons per hectare), and pale in comparison with yields in Europe (5.9 metric tons per hectare) and North America (7.2 metric tons per hectare) (World Bank, 2024). Historically, agricultural productivity in SSA has been constrained by many challenges, not the least of which is the low use of modern agricultural inputs such as improved seed varieties and fertilizers. The limited use of these modern inputs and resulting low level of agricultural productivity have the pernicious effect of trapping farmers in a low-level steady state.

A major contributor to this challenge is that farmers across the region lack the liquidity with which to invest in these productivity-enhancing modern agricultural inputs, and imperfections in credit markets in the form of credit rationing result in excess credit demand equilibria. In the context of agricultural credit in SSA (and other developing regions), credit rationing consists of both quantity (supply-side) rationing and risk (demand-side) rationing. Quantity-rationed households are interested in obtaining credit, but financial institutions consider them too risky and either deny them credit outright or offer them less credit than their demand. On the other hand, risk-rationed households voluntarily exclude themselves from credit markets to avoid losing collateral due to natural hazards (such as drought) and the risk of crop failure (Boucher et al., 2008, 2009; Ndegwa et al., 2020). Failures in rural credit markets limit the ability for capital- and liquidity-constrained smallholder households to capitalize on inputs and technologies necessary for reaching optimal productivity and avoiding poverty traps (Gehlich-Shillabeer, 2008). Insurance can alleviate some risks and crowd-in formal credit because insured households pose less risk to creditors and face lower risk of losing collateral (Alderman & Haque, 2007; Barrett et al., 2007; Carter et al., 2007; Carter et al., 2016; Jensen & Barrett, 2017; Skees & Barnett, 2006).

In this study, we evaluate a novel financial product, risk-contingent credit (RCC), which has the potential to ameliorate risk- and quantity-rationing in rural credit and consequently to increase agricultural investments and enhance agricultural productivity and rural welfare. RCC's novelty is that it bundles insurance directly with agricultural loans, thereby protecting both borrowers and lenders from agricultural risks such as weather or market price fluctuations. Our study relies on a multiyear randomized controlled trial (RCT) involving three experimental arms: a treatment group offered RCC, a treatment group offered a traditional credit (TC) product, and a comparison group that was offered neither RCC nor TC. We evaluate both the ex-ante investment impacts and the ex-post productivity benefits of RCC and TC among smallholder farmers from Machakos County, Kenya. In so doing, we provide some of the first empirical evidence on the impacts of RCC in a real-world, developing-country context.

We estimate the ex-ante effects of RCC and TC on agricultural investments as well as area under maize, this study's main crop of interest. We consider maize yield and farm revenue to be the ex-post productivity indicators. In addition to comparing these treatment arms to the comparison group, we also compare the effects of the RCC treatment to the TC treatment to assess whether RCC produces any additive effects above and beyond what might be realized through increased access to and use of traditional forms of agricultural credit (Mishra et al., 2020; Ndegwa et al., 2020). To evaluate the impacts of the interventions, we rely on local average treatment effects (LATE) with random assignment to one of the two treatments as the exogenous instrumental variable.

Our results suggest that both TC and RCC have some direct beneficial ex-ante effects on agricultural investments, particularly the adoption and intensive use of chemical fertilizers. We do not observe an increase in area cultivated under the primary crop (maize), leaving us to conclude that the increased availability of credit led to an increase in agricultural investments along the intensive margin rather than the extensive margin. Given the limited potential for agricultural extensification in SSA (Jayne & Sanchez, 2021), this suggests a potential avenue for innovative agricultural financing to fill an important gap in modernizing the agricultural sector in SSA. We find some slight evidence that farmers increased their use of improved maize varieties, but because roughly 85 percent of farmers were cultivating improved maize on roughly 75 percent of their maize area at the time of baseline, the positive treatment effects that we observe are modest and not statistically significant. Despite the positive effects we observe on investments in modern agricultural inputs, however, we do not find any meaningful effects on maize yields or farm revenues per acre. Indeed, some of the point estimates we observe are negative, and though not statistically significant, these could suggest a perverse effect of expanded agricultural credit and the stochasticity that characterize the production environment and process in this region (Hoel et al., 2024). Given that many of the ex-post outcomes that we consider are high-order outcomes and are subject to myriad exogenous perturbations, we are cautious to ascribe causality to the credit products evaluated in the present study.

Our study contributes to a narrow but growing literature on the potential of insurance-bundled credit products for agricultural production among the smallholders of low-income countries such as those in SSA While most of the existing literature focuses on the optimization and uptake of index insurance and insurance-bundled credit, and some on the impact of stand-alone index insurance or micro-credit products, much less focus has been given to the impact of insurance-bundled credit, especially going beyond effects on expanding investments in farm operations. This paper empirically explores the ex-ante investment and ex-post productivity impacts of an insurance-bundled agricultural credit product, comparing it with traditional non-insured credit.

The remainder of the paper proceeds as follows. Section 2 provides a historical context of the challenges associated with rural credit and insurance, thereby laying a backdrop against which to contrast the novelty of RCC. Section 3 presents the study and experimental design, where we discuss the study area and sampling, experimental groups and treatment randomization, RCC design, and study implementation process. Section 4 presents the data and descriptive statistics characterizing the sample, with detailed discussions of covariate balancing, sample attrition, and control mechanisms. Section 5 presents the empirical framework followed in the analysis. Section 6 presents the results, discussing the ex-ante and ex-post impacts. Section 7 presents the study's conclusion and policy implications.

2 Background on agricultural credit and agricultural insurance in developing countries

Over the years, farmers from developed economies have used formal insurance to hedge against covariate weather risks and crowd-in credit and other financial services. Such products are not widely available to farmers in SSA, however, particularly rural smallholders (Jensen & Barrett, 2017). In response to covariate weather risks and ensuing shocks, SSA smallholders employ conservative ex-ante investment decisions or harmful ex-post risk coping strategies, resulting in suboptimal productivity and negative ramifications on their welfare.[1] Further, they commonly employ informal borrowing and risk-sharing through reciprocal lending within community social networks to smooth consumption. These arrangements are only partially effective at managing idiosyncratic risks and are even less effective against systemic and covariate risks that have the potential to lower yields and incomes of a larger proportion of households at the same time (Farrin & Miranda, 2015; Jalan & Ravallion, 1999; Townsend, 1994; Udry, 1990). Formal insurance can potentially benefit rural farming households' welfare and agricultural productivity by reducing vulnerability and promoting agricultural investments where rain-fed agriculture and financial market failures are common (Dercon, 2005; Hazell & Hess, 2010; Jensen & Barrett, 2017). Formal insurance dominates self-insurance and humanitarian response by positively influencing both ex-ante risk management and ex-post risk coping strategies (Janzen & Carter, 2019).[2]

However, earlier efforts to provide farmers in low-income countries with traditional multi-peril insurance failed, largely due to adverse selection, moral hazard, and high transaction costs.[3] To date, agricultural insurance has suffered from low levels of uptake in the absence of high subsidies, which many governments in low-income countries cannot afford. This is especially true in low-income countries with large agricultural populations, most of whom are smallholders. Similarly, and mostly for the same reasons, previous efforts to use publicly supplied crop insurance to underwrite farm loans issued by specialized agricultural development banks were financially expensive and unsustainable (Meyer et al., 2017). The introduction of index-based insurance products—which compensate farmers on the basis of some easily observed and measured index (usually tied to weather conditions such as rainfall) rather than on the basis of assessed

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[1] Ex-ante, they employ risk mitigation strategies such as low-risk/low-return production portfolios that frequently have negative livelihood impacts in the long run (Carter, 1997; Jensen & Barrett, 2017; Morduch, 1995; Rosenzweig & Binswanger, 1993; Zimmerman & Carter, 2003). Ex-post, they employ detrimental coping strategies such as selling off productive assets, skipping meals, withdrawing children from school, and so on (Hoddinott, 2006; Janzen et al., 2013).

[2] Ex-ante insurance transfers a portion of income risk out of the household's portfolio, and hence farmers can increase investments in higher-risk/higher-return production technologies and practices such as improved seeds, fertilizer, pesticides, and so on (Dercon & Christiaensen, 2011; Morduch, 1995) This implies increased investment in agriculture by households, raising the need for enhanced capital and hence creating a demand for credit. Ex-post, households that anticipate and receive indemnity payments have more response options, therefore reducing their reliance on harmful coping strategies (Carter et al., 2014; Jensen & Barrett, 2017).

[3] Transaction costs include but are not necessarily limited to monitoring and administration costs to minimize adverse selection and moral hazards. These are meant to compensate insurers for information asymmetries between the insurer and the farmers seeking to be insured.

losses—has raised the hopes of adequately providing insurance to the agricultural sector, including rural smallholder households (Carter et al., 2014; Meyer et al., 2017; Skees, 2008). Consequently, development economists and practitioners have devoted substantial effort over the past two decades to designing, testing, and implementing market-based index insurance products with the ultimate objective of promoting investment and technical advancement among poor smallholder farmers in the developing world. The results of these efforts have mostly been disappointing, with limited data with which to construct the indices, design complications, basis risk, low adoption rates, substantial dis-adoption among former adopters, liquidity constraints among the smallholders, low commercial scale-up beyond experimental and pilot stages, among other disappointing pitfalls, all of which question index insurance's sustainability (Binswanger-Mkhize, 2012; Jensen et al., 2018; Jensen & Barrett, 2017; Tafere et al., 2019).

Attention is turning to alternative uses of index insurance by bundling it to credit and other productivity-enhancing investments with the hope of revitalizing the agricultural credit markets and enhancing smallholders' productivity and welfare. One such innovation is RCC, which bundles insurance directly with agricultural loans to protect both borrowers and lenders from agricultural risks such as weather or market prices (Carter et al., 2011, 2007; Carter et al., 2016; Farrin & Miranda, 2015; Giné & Yang, 2009; Karlan et al., 2014; Miranda & Gonzalez-Vega, 2011; Ndegwa et al., 2021; Shee & Turvey, 2012; Shee et al., 2015, 2019; Smith & Watts, 2009).[4] The economic premise is that bundled insurance would reduce the inelasticity of using index insurance to directly manage the systemic loan portfolio risk borne by lenders. Lenders will, in return, be able to offer larger volumes of loans to smallholder farmers at lower interest rates. Farmers will be able to capitalize on modern and productivity-enhancing technologies and practices that they could not previously due to 1) capital and liquidity constraints in the absence of credit, and 2) the risk of crop failure due to drought or other hazards in the absence of insurance and the resulting loss of invested funds or assets used to collateralize their loans. Further, linking insurance with credit can mitigate the high default risk faced by lenders due to borrowers' exposure to covariate risks. This promising intervention forms the backdrop of the present study.

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[4] Other innovations include bundling index insurance with drought-tolerant seeds, as described by Boucher et al. (2021), Ward et al. (2019), and Lybbert and Carter (2015).

Although a sizeable literature exists on the design and uptake of index-based, insurance-bundled credit products (Carter et al., 2007; Casaburi & Willis, 2015; Farrin & Miranda, 2015; Giné & Yang, 2009; Karlan et al., 2011; Meyer et al., 2017; Miranda & Gonzalez-Vega, 2011; Ndegwa et al., 2020; Ndegwa et al., 2022; Shee & Turvey, 2012; Shee et al., 2015, 2019; Skees et al., 2007), there is little evidence on the impacts of such products, especially ex-post impacts. This paper aims to contribute to this literature by providing some of the first evidence on the impacts of insurance-bundled credit.

3 Study and experimental design

3.1 Study area and sampling strategy

This study is based on an RCT initiated in April 2017 in Machakos County, Kenya (Figure 1).[5] Machakos County was purposively selected due to its dynamic (yet mostly semi-arid) agroecological zones and the presence of a financial institution capable of implementing the project's interventions (Equity Bank) with sufficient coverage in the county. Five of 11 sub-counties of Machakos County were selected in consultation with Equity Bank and provincial administration officials. Thirteen locations were then selected from among the five sub-counties. Subcounty and location selection was purposive, excluding urban and peri-urban areas and considering the Bank's coverage and capacity to deliver the proposed products. At the onset of the baseline survey, six villages per location were randomly selected. This was done from the village sampling frame comprising lists of villages per location that were provided by area chiefs, the local administrators in charge of locations. Thereafter, the subchiefs and village elders from the selected villages provided the research team with household rosters that included the names of all households under their jurisdiction. Fifteen households per village were randomly selected from the rosters, resulting in 90 households per location and 1,170 households in total.

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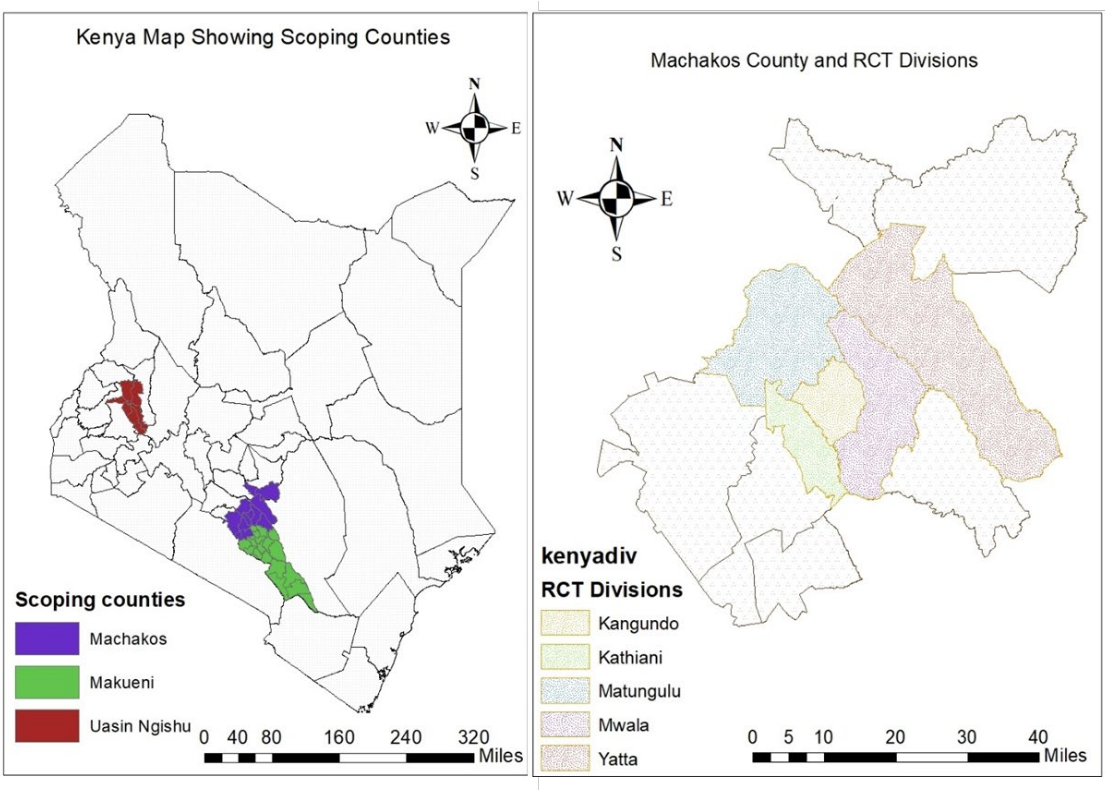
[5] At the inception stage, we conducted scoping missions in June 2016 in which six focus group discussions were conducted in Machakos County and two other counties, namely, Makueni and Uasin Gishu. The aim of the scoping mission was to determine RCC's suitability and feasibility by assessing the historical weather shocks (drought in particular) and their impacts on farming households. We also used these missions to document common farming practices, seasonal patterns, and the 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) were not as good as in Machakos. Uasin Gishu County was not ideal for RCC because drought is not a major production risk there; hence drought insurance was not a viable intervention.

Figure 1. Map of Kenya showing scoping counties and map of Machakos County showing RCT divisions

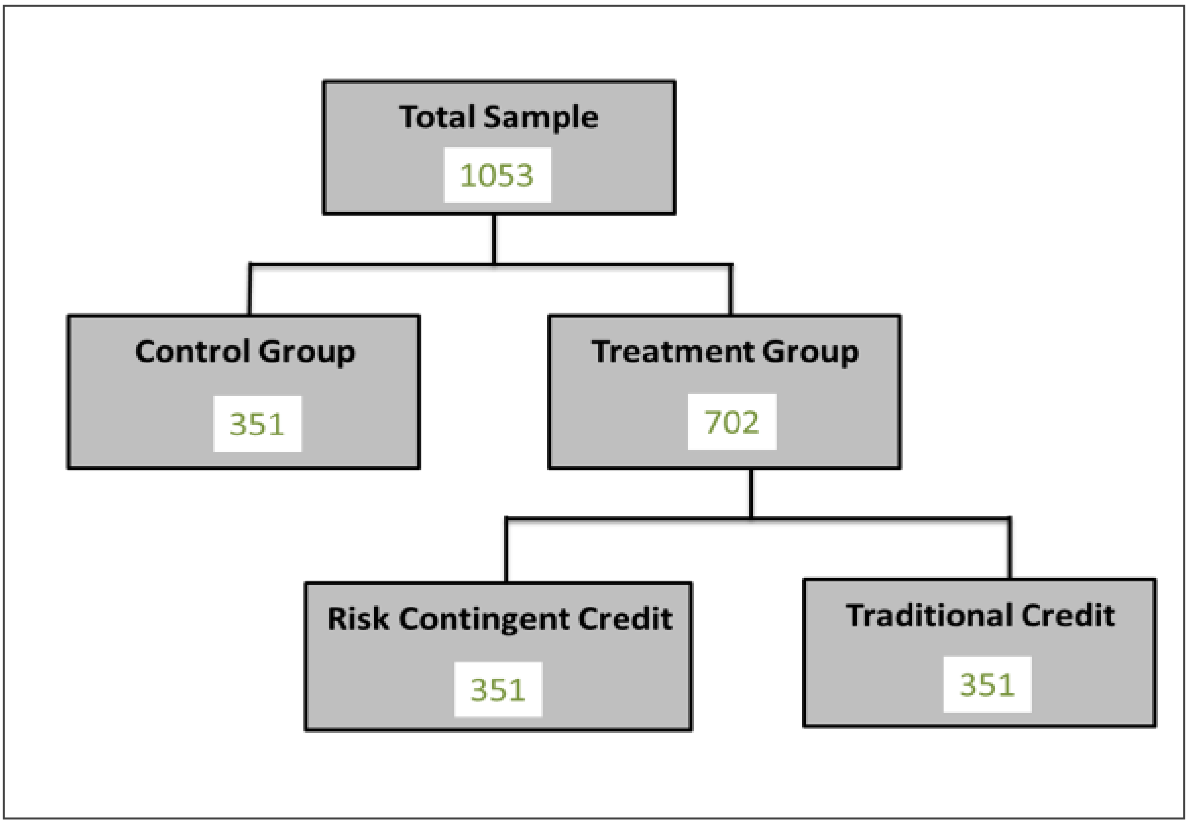


Source: Generated by the authors using Kenya National and County shapefiles.
Note: RCT = randomized controlled trial.

3.2 Randomization and experimental groups layout

The RCT comprised a control group and two treatment arms, RCC and TC. We chose individual randomization over a village-level cluster design because (1) the credit intervention is not likely to create competition in the area, and hence spillover effects are not an issue for this study; and (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. In the end, and to balance out the experimental groups within all the locations, 351 households were randomly assigned to each experimental group for a total sample of 1,053. As such, to estimate the impact of our intervention, we compare the control households with RCC and TC households separately. The RCT protocol is illustrated in Figure 2.

Figure 2. Schematic experimental design



3.3 Study implementation process

The implementation timelines are shown in Figure 3. The study involved a baseline survey, two phases of project implementation (marketing of RCC and TC loans), and two follow-up surveys (midline and endline). The baseline survey was conducted in May–June 2017, followed in October of that year by the first phase of implementation, during which input loans were offered for the long rain season running from October 2017–January 2018. The first follow-up (midline) survey was conducted in May–June 2018, roughly eight months from the time the first phase loans were disbursed. Input loans for the second phase of implementation were offered in October 2019,[6] to be used in the long rain season from 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. Furthermore, whereas baseline and midline surveys were through face-to-face interviews using a long-form,

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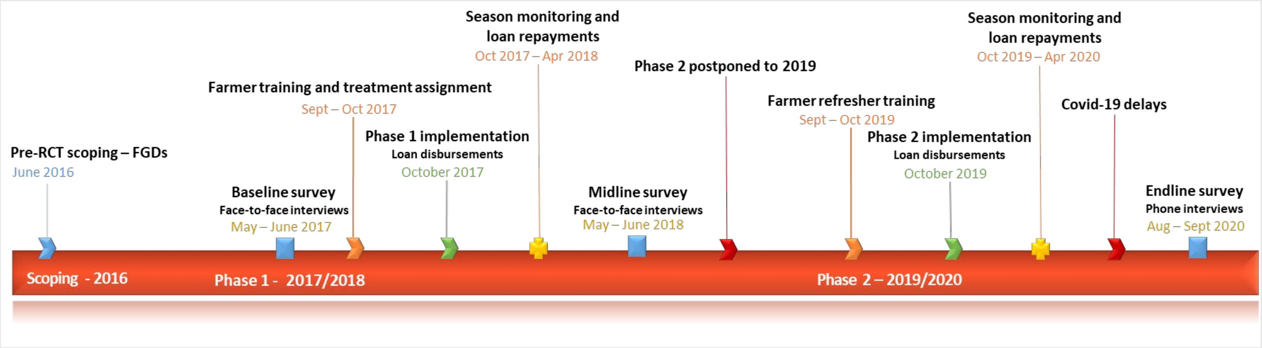
[6] The initial plan was to implement the interventions in two consecutive years (2017 and 2018), targeting the long rain season spanning from October to January of the following year. However, our local implementing partner, Equity Bank, was unable to implement in the 2018/19 season and requested we postpone implementation to the following year (2019/20 season).

structured questionnaire, the endline survey was conducted via telephone. Although the questionnaire itself was streamlined so that the interview could be completed within roughly 30 minutes, we undertook extensive efforts to ensure that we tracked all key variables needed to complete the study and that data quality did not suffer.

At the onset of each phase of implementation (September–October 2017 and September–October 2019), the selected farmers were invited to sensitization and training meetings where they received training on RCC, household finance, and credit management. The meetings were conducted at the location level, where all 90 households (either the household head or spouse) were invited. During the first phase meetings, the households were also randomly assigned into the experimental groups. This random allocation was done within locations through public lotteries wherein each participant took turns to blindly draw a printed chip once from an urn. The outcomes for each participant were announced publicly and recorded on the household list. Those who did not attend the meeting were proxied by the chiefs, subchiefs, village elders, or someone else who knew them. There were 27 chips printed "Control," 27 printed "Normal" (for TC), and 27 printed "RCC," for a total of 81 chips—exactly equal to the number of selected households in a location. This was a transparent procedure of assigning participants into the treatments for the study. Random assignment was done once, and throughout the duration of the study, participants remained in the same groups to which they were originally assigned.

Figure 3. Timelines diagram



Note: RCT = randomized controlled trial; FGDs = focus group discussions.

3.4 RCC design and treatment implementation

The index-based insurance linked to agricultural credit in the RCC product evaluated in the present study was based on Climate Hazards Group InfraRed Precipitation with Station (CHIRPS) data rainfall measures for the traditional long rain season in Machakos County from October 15 to January 15 (Shee et al., 2019, 2023). In brief, historical dekadal (10-daily) rainfall data from 1981 to present were collected for each of the 11 divisions of Machakos County. Cumulative rainfall measures were fit to a PERT distribution, with a cumulative rainfall "trigger" set at the 20th percentile for each subcounty. Correlated Monte Carlo simulation was used to compute actuarial rates assuming Kenyan shilling (Ksh) 300 tick value for every millimetre 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-counties, the actual premiums averaged about 12 percent across districts/sub-counties. With a 25 percent loading factor imposed by the insurer, the yield as a percentage of the loan amount was set at 14 percent for each subcounty, even though each subcounty had a distinct trigger against which indemnities were to be calculated.

A total of 702 households were offered credit (351 RCC and 351 TC) during each round of implementation. Because 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. To ensure that the credit offered was used only in agricultural production processes, the loan amount was not advanced in cash; instead, farmers were provided with vouchers that they used to collect specified farm inputs from preselected local agricultural input supply stores in 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 intercrop) production for one acre per season. For the RCC group, the 14 percent (Ksh 1,400) insurance premium was added to the base loan and was automatically transferred to the insurance firm (APA Insurance). The lender also applied the standard interest rate (14 percent per annum at the time) on the principal.[7] For the RCC product, the interest was applied to a composite principal that included the actual amount loaned to the farmers and the insurance premium paid to the insurer on their behalf. After input collection by the farmers who accepted to take the credit, the project and bank staff conducted follow-up visits to ensure they adhered to the process and to offer any support they required. At the end of the season, the TC farmers were allowed up to 3 months to pay off their loans. The same would apply to the RCC farmers if the weather index did not trigger a payout by the insurance or if a partial payout was declared.

4 Data and sample analysis

4.1 Covariate balancing and descriptive statistics

We report the full sample means at baseline in column (1) of Table 1 to characterize the participating households at baseline. Most households (68 percent) were using fertilizer on their maize fields, spending about Ksh 1,200 on average for maize fertilizer per acre. Most participating households (85 percent) were already adopting improved maize varieties and put on average 76 percent of their maize fields under improved varieties. On average, participating households had 2 acres of land under maize. Average maize yields were about 900 kilogram (kg) per acre after adjusting for intercropping, earning them a revenue of about Ksh 7,000 per acre, which increased to about Ksh 13,000 per acre when revenue from the intercrops (common beans and cow peas) was added. About half of the households were credit rationed in some way, either quantity rationed (11 percent) or risk rationed (42 percent). Most of the household heads were male (80 percent), with an average age of 56 years and 8 years of formal education, indicating a low education level of just primary education. The average household size was roughly six persons, with a modest dependency ratio of 0.8 percent, implying about

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[7] The interest rate for commercial loans had been capped at 14 percent per annum by the Kenyan government since 2016.

one dependent per worker. The households had an average of one-acre per-capita land ownership and access. The average tropical livestock unit (TLU) holding was 7, implying that all livestock in the household was equivalent to seven cows. Households had a subjective welfare mean score of 2.8 out of a possible 5, implying they were just above average. Only 28 percent reported access to agricultural extension services, indicating very low access to extension services among the households in the study area. The average risk aversion score—the constant relative risk aversion (CRRA) coefficient—was 0.4, indicating that on average, the households were moderately risk averse.[8]

We test for baseline differences among the experimental groups by regressing a series of outcome or control variables on treatment assignment. In assessing baseline balance, we compare RCC households with controls and then TC households with controls in (column 2), and then compare RCC and TC households (column 3). Apart from their dependency ratio, the control group is not systematically different from the treatment groups. Further, the two treatment arms (RCC and TC) are similar on all outcome and control variables.

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 lefthand side and the study outcome and control variables on the righthand side. The results are consistent with the individual variable balance tests, where the F test for joint significance yielded a *p* value of 0.977, implying that joint orthogonality can be assumed and hence selection bias into treatments was absent. Nevertheless, for robustness' sake, besides univariate models, we also control for households' heterogeneity at baseline and hence run multivariate regression models with baseline controls.

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[8] CRRA 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, we asked if they wanted to participate in a risk game (by risking the Ksh 300 award they got from the survey) with a gains and losses possibility in which 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 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) conversion coefficients.

Table 1. Baseline covariates balancing and joint orthogonality test

	(1)		(2)		(3)
	Pooled sample		Treatments vs. control (coeff.)		Comparing treatments (coeff.)
Variable	Mean	SD	RCC vs. control	TC vs. control	RCC vs. TC
Outcome variable					
Binary maize fertilizer use	0.68	0.467	0.003	−0.023	0.026
Amount spent on maize fertilizer (Ksh “000”)	1.20	1.356	−0.032	−0.074	0.042
Binary improved maize variety adoption	0.85	0.359	0.007	−0.022	0.028
Share of land under improved varieties	75.92	36.789	0.712	−1.709	2.422
Area under maize (acre)	2.05	1.981	−0.044	0.016	−0.060
Maize yields (kg)	896.37	797.857	−48.054	6.987	−55.042
Acreage maize revenue (Ksh “000”)	6.83	7.235	−0.225	0.493	−0.718
Acreage overall revenue (Ksh “000”)	13.49	15.767	−0.201	1.523	−1.724
Socioeconomic control					
Risk rationed	0.42	0.493	−0.020	−0.030	0.010
Quantity rationed	0.11	0.307	0.009	0.015	−0.006
Age of household head	56.19	13.237	0.111	−1.188	1.299
Household head gender = male	0.79	0.407	0.028	0.020	0.009
Head’s years of education	8.62	3.868	−0.071	0.026	−0.097
Dependency ratio	80.14	77.175	−10.441*	−10.822*	0.381
Per capita land ownership	0.84	3.277	0.310	0.015	0.295
TLU holding	7.13	60.935	−1.999	−1.104	−0.895
Extension services access index	0.28	0.448	−0.051	−0.066**	0.014
CRRA coefficient	0.40	0.23	0.004	−0.001	0.005

Note: Coeff. = coefficient; CRRA = constant relative risk aversion; RCC = risk-contingent credit; TC = traditional credit; TLU = tropical livestock unit. Column (1) reports means and standard deviations for the pooled sample at baseline. Column (2) reports results from a series of OLS regressions comparing RCC households with control households and then TC households with control households for each outcome and covariate variable. Column (3) reports results from a series of OLS regressions comparing RCC households with TC households for each outcome and covariate variable. For simplicity, standard errors and intersects from all the regressions are not reported. The F test for joint orthogonality results from a multinomial logit regression comparing the treatments against control yielded a *p* value of 0.977. The results of the F test showing the coefficients for individual variables are excluded from the table because they do not offer any insight. *,** means statistic significance at 10% and 5%.

4.2 Testing for attrition bias

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

Nevertheless, to test for random attrition or check for attrition bias in our sample, we generate a binary indicator for each follow-up round (midline and endline) equal to 1 if a household dropped from the study at the respective survey round, and 0 otherwise. We then use a series of probit regressions to analyse the associations 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 done separately for each experimental group and then for the pooled/overall sample, and the results can be found in Table A3 in the Appendix. For the overall sample, we start with a univariate model with random assignment as the predictor variable and then extend this to include baseline socioeconomic controls.

Results from column (5) of Appendix Table A3, which tests for attrition bias for the entire sample, indicate no systematic difference between attritted and re-interviewed households for all experimental assignments, outcomes, and socioeconomic controls. Considering different experimental assignment groups, there still was no evidence that attrited households were systematically different from re-interviewed households, apart from the maize revenue for the control group and dependency ratio for the control and TC groups. The control households who left the study had statistically significant higher maize revenue per acre, although the difference is not substantial in size. Further, control and TC households who left the study had higher dependency ratio than their colleagues who stayed.

The low attrition rate among the treatment and overall sample, coupled with there being no significant systematic differences between the attrited and re-interviewed households, suggests that attrition bias is likely not a major issue in this study. However, we use the Lee (2009) bounds to test if the estimates were robust to attrition.

5 Empirical framework

The primary objective of this study is to provide evidence on the efficacy of RCC to engender a transformation and modernization of agriculture in SSA. To do so, we test whether RCC and TC yield ex-ante and ex-post 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 if the credit intervention influences agricultural intensification and/or extensification, we also estimate the effects of RCC and TC on 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).

For all outcome variables, we estimate both intention to treat (ITT) effects and local average treatment effects (LATE). The ITT estimates present the average effect of being randomly offered either RCC or TC, regardless of whether the participant ultimately receives a loan. Our protocol (and ethical guidelines) made no demands on the farmers to accept the loan offered, and it was their free choice to accept or reject the loan offer. In the present study, compliance with treatment intention (i.e., whether households randomly selected to be offered one of the two credit products opted to take a loan) was far from perfect, with average uptake at 32 and 16 percent during the first and second implementation phases, respectively. As such, ITT effects are diluted estimates of average treatment effects due to noncompliance among households randomly assigned to the treatment group (Angrist, 2006). However, ITT effects are still important, especially for policy formulation, because in reality, governments and development agencies are unable to enforce full compliance of their interventions (Duflo et al., 2007). Further, it is imprudent to completely omit participants who deviate from a treatment protocol, as there is no total confidence that the remaining groups are comparable (Angrist & Pischke, 2008; Armitage, 1979; Huang, 2018; Soderbom et al., 2015).

With that said, policymakers are also generally going to be interested in the effects of an intervention on those directly impacted. In the present context, the LATE estimates the actual effect of taking the offered credit (RCC or TC) on the investment, productivity, and welfare of the households who took loans under this project. As already indicated, compliance was far from perfect, and hence we mainly rely on LATE estimates to assess the causal effects of RCC and TC on participating households. Since there are no “always takers” in the present context (as a direct consequence of the research design), the LATE provides a direct estimate of the treatment effect on the treated (TOT).

To estimate both ITT and LATE, we provide two alternative specifications—simple means difference (SMD) and analysis of covariance (ANCOVA)—that aim to demonstrate the consistency of the main results. Both specifications are estimated from post-intervention data (midline and endline), but ANCOVA controls for baseline values of the outcome of interest, and hence is a more efficient estimator than SMD. We also include an additional vector of household characteristics in the ANCOVA specifications to control for imbalances between experimental arms at baseline that may otherwise confound the estimation of treatment effects.

5.1 Intention to treat (ITT) effects

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

$$y_i = \alpha + \beta_{SMD1} RCC_i + \beta_{SMD2} TC_i + \varepsilon_i \tag{1}$$

We first estimate this as an SMD(simple mean difference) 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 1 if a household was randomly assigned to the respective credit group (RCC for risk-contingent credit and TC for traditional credit) and 0 otherwise, and ε_i is the idiosyncratic error term. The parameters of interest are β_{SMD1} and β_{SMD2}, which capture the ITT SMD effects, and the effect of being randomly 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:

$$y_i = \alpha + \beta_{\mathrm{ANCOVA1}} RCC_i + \beta_{\mathrm{ANCOVA2}} TC_i + X'_{i0}\gamma + \theta y_{i0} + \varepsilon_i \tag{2}$$

where, in addition to the already described abbreviations, X_{i0} is a vector of household socioeconomic characteristics at baseline, which include head’s gender, age, and education; household’s credit rationing status, dependency ratio, risk aversity score, per capita land ownership, TLU holding, and access to extension services. Further, y_{i0} represents the baseline measurement of the outcome variable. Here, the parameters of interest are $\beta_{ANCOVA1}$ and $\beta_{ANCOVA2}$ giving the ITT effects (the effect of being assigned to RCC or TC, respectively).

From these ITT regression specifications, we test the following hypotheses:

(A)
$$H_0: \beta_1 = 0$$
$$H_A: \beta_1 \neq 0$$

(B)
$$H_0: \beta_2 = 0$$
$$H_A: \beta_2 \neq 0$$

(C)
$$H_0: \beta_1 = \beta_2$$
$$H_A: \beta_1 \neq \beta_2$$

Hypothesis (A) tests whether there is a meaningful effect of being randomly allocated to the RCC treatment group. Hypothesis (B) tests whether there is a meaningful effect of being randomly allocated to the TC treatment group. Last, hypothesis (C) tests whether the effect of being randomly allocated to the RCC treatment group is equivalent to that of being randomly allocated to the TC treatment group.

5.2 Local average treatment effects (LATE)

As indicated above, we had imperfect compliance in the study. 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 (2SLS) approach, in which uptake of either RCC or TC is instrumented by random assignment to either RCC, TC, or control group. In RCTs, random assignment to treatment and control groups is the ideal instrument that meets the validity, relevance, and strength conditions, as shown in Table A1 in the Appendix (Abadie & Cattaneo, 2018; Angrist & Pischke, 2008; Huang, 2018; Khandker et al., 2009; Soderbom et al., 2015). 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. We estimate the LATE using the IV specification below, where we use the predicted uptake of RCC and TC from the first stage to estimate the actual effect of uptake of the respective credit product on the outcome variables. We start with a parsimonious model with both treatment arms (RCC and TC) only as the predictor variables, estimated from post-treatment observations as SMD:

$$y_i = \alpha + \delta_{SMD1}\widehat{RCC}_i + \delta_{SMD2}\widehat{TC}_i + \varepsilon_i \tag{3}$$

where $\widehat{RCC}_{it}$ and $\widehat{TC}_{it}$ are the first stage fitted values of treatment; that is, a prediction of actual uptake of RCC and TC (correspondingly) by household i based on random assignment to one of the two treatment groups. The δ_{SMD1} and δ_{SMD2} terms are the parameters of interest that gives the LATE for RCC and TC, respectively. As previously mentioned, because the LATE yields the treatment effect among those who always comply with their assignment, and because we have no "always takers" by virtue of our research design, the LATE will yield the TOT. Again, we modify the above specification and control for baseline measures of the outcome variables (y_{i0}) as well as regressors that were imbalanced at baseline (X) in an ANCOVA specification:

$$y_i = \alpha + \delta_{ANCOVA1}\widehat{RCC}_i + \delta_{ANCOVA2}\widehat{TC}_i + X'_{i0}\gamma + \theta y_{i0} + \varepsilon_i \tag{4}$$

where $\widehat{RCC}_{it}$ and $\widehat{TC}_{it}$ are defined above.

As we did with the ITT estimations, we test a series of hypotheses related to these LATE specifications:

(A′) $H_0\colon \delta_1 = 0$
$H_A\colon \delta_1 \neq 0$

(B′) $H_0\colon \delta_2 = 0$
$H_A\colon \delta_2 \neq 0$

(C′) $H_0\colon \delta_1 = \delta_2$
$H_A\colon \delta_1 \neq \delta_2$

These hypotheses have similar interpretations to hypotheses (A), (B), and (C) above but pertain to actual take-up of these credit products, rather than merely being randomly assigned to be offered one of these credit products. Hypotheses (A′) and (B′) test whether there is a meaningful effect of taking up the RCC or TC product, respectively, while hypotheses (C′) tests whether the effect of taking up the RCC product is equivalent to the effect of taking up the TC product. Rejection of the null hypothesis in (A′) and (B′) would suggest that there are statistically significant effects of these two credit products, while rejection of the null hypotheses in (C′) would suggest differential impacts of these two competing credit products. Together with hypotheses (A), (B), and (C) above, these hypotheses form the basis of much of the empirical work that follows.

6 Results and discussions

6.1 Ex-ante investment impacts

As indicators of households' agricultural investment, we consider five outcomes: (1) use of fertilizer on maize (a binary variable equal to 1 if chemical fertilizer was used on maize and 0 otherwise), (2) chemical fertilizer use intensity proxied by amount of money (in Ksh) spent on chemical fertilizers for maize, (3) adoption of improved maize varieties as a binary variable equal to 1 if one planted improved maize varieties at all and 0 otherwise, (4) improved maize adoption intensity measured as the share of maize fields under improved varieties, and (5) area under maize overall.

Tables 2–4 show the results of estimating treatment effects. The table structure is as follows: The SMD ITT effects are reported in column (1); ITT effects based on ANCOVA regressions are reported in column (2); the SMD LATE are reported in column (3); and LATE based on ANCOVA regressions are reported in column (4). We estimate all LATE using two-stage least squares (2SLS), where random assignment to either RCC or TC is used as the excluded instrument for uptake of the respective credit product. Because assignment to either the RCC or TC treatment groups is random, it should have no independent effect on the outcome variables (satisfying the exclusion restriction criterion), nor should it be correlated with the disturbances (satisfying strict exogeneity) but should induce (at least some) participants to take up the credit products they were offered as part of the project (satisfying the relevance criterion).

6.1.1 Fertilizer use

Table 2 presents the ITT effects and LATE of a household being randomly offered either RCC or TC on its decision to use chemical fertilizer (panel A) and total fertilizer expenditures per acre (panel B), which serves as a proxy for fertilizer use intensity. The SMD estimates presented in column (1) show that random assignment to either RCC or TC did not have any significant effect on the proportion of farmers choosing to use chemical fertilizers on maize fields. The ANCOVA estimates in panel (2), however, show that assignment to RCC increased the likelihood of applying chemical fertilizers in maize fields by 3.6 percentage points ($p < 0.1$), while assignment to TC did not have any significant effect on the same outcome. The LATE results in columns (3) and (4), albeit higher in magnitude (as would be expected), follow a similar trend. SMD estimates in column (3) 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, though ANCOVA estimates in column (4) indicate that uptake of RCC increased the likelihood of fertilizer use by 13.5 percentage points ($p < 0.1$) while, again, uptake of TC did not significantly affect this outcome under any econometric specification.

Panel B presents the ITT effects and LATE on the fertilizer use intensity, measured as the amount of money in Ksh spent on maize fertilizer per acre. Across various specifications, we find that RCC had persistent and economically meaningful impacts on increasing fertilizer use intensity. Across the ITT specifications, assignment to RCC increased per-acre spending on maize fertilizer by about Ksh 500 ($p < 0.01$) while assignment to TC only increased fertilizer expenditures by about Ksh 150 ($p < 0.1$ only under the ANCOVA specification). LATE results indicate a similar trend, with substantially higher effects in terms of magnitude. As with the ITT estimates, LATE based on ANCOVA regressions demonstrate a higher overall effect relative to SMD estimates. The SMD estimates in column (3) indicate that uptake of RCC increased acreage spending on maize fertilizer by about Ksh 1,850 ($p < 0.01$), while uptake of TC increased fertilizer spending per acre by only about Ksh 650 ($p < 0.1$). ANCOVA estimates in column (4) indicate that uptake of RCC increased acreage spending on maize fertilizer by more than Ksh 1,900 ($p < 0.01$), while uptake of TC increased fertilizer spending per acre by about Ksh 750 ($p < 0.1$). Comparing the estimated effects of RCC and TC on fertilizer spending per acre, we find convincing evidence that RCC effects on maize fertilizer spending were significantly larger than those generated by TC (see *p* values from this test in the "RCC = TC" row in panel B).

Table 2. Treatment effects on use of chemical fertilizers

VARIABLE	(1) ITT SMD	(2) ITT ANCOVA	(3) LATE SMD	(4) LATE ANCOVA
		Panel A: Use of chemical fertilizers (binary)		
RCC	0.033	0.036*	0.125	0.135*
	(0.022)	(0.019)	(0.084)	(0.073)
TC	0.013	0.029	0.060	0.132
	(0.023)	(0.020)	(0.103)	(0.088)
Outcome variable at baseline		0.423***		0.415***
		(0.017)		(0.021)
Individual fixed effects	No	No	No	No
Baseline covariates	No	Yes	No	Yes
Constant	0.773***		0.773***	0.536***
	(0.016)		(0.016)	(0.066)
RCC = TC: *p* value	0.373	0.740	0.479	0.971
Mean of control	***0.773***	***0.773***	***0.773***	***0.773***
Observations	2,015	2,008	2,015	2,008
R-squared	0.00	0.25	0.02	0.27
		Panel B: Fertilizer spending per acre (Ksh)		
RCC	491.371***	517.162***	1,839.850***	1,922.156***
	(108.579)	(99.324)	(398.997)	(372.558)
TC	145.706	168.746*	638.941	741.569*
	(98.978)	(99.756)	(425.911)	(391.744)
Outcome variable at baseline		0.495***		0.469***
		(0.030)		(0.033)
Individual fixed effects	No	No	No	No
Baseline covariates	No	Yes	No	Yes
Constant	1,723.676***		1,723.676***	1,746.599***
	(67.095)		(67.044)	(352.646)
RCC = TC: *p* value	0.002	0.000	0.006	0.004
Mean of control	***1724***	***1724***	***1724***	***1724***
Observations	1,956	1,941	1,956	1,941
R-squared	0.01	0.17	0.06	0.19

Note: ANCOVA = analysis of covariance; RCC = risk-contingent credit; SMD = simple mean difference; TC = traditional credit. For simplicity, RCC and TC rows show the ITT and LATE estimates for all the estimators, that is, ITT SMD in column (1), ITT ANCOVA in column (2), LATE SMD in column (3), and LATE ANCOVA in column (4). All ANCOVA models had baseline covariates, including outcome variable baseline measurement. The first-stage predicted uptake that applies is in panel A of Table A1 in the Appendix. Robust standard errors are shown in parentheses.
${}^{*}p < 0.1$; ${}^{**}p < 0.05$; ${}^{***}p < 0.01$.

6.1.2 Improved maize seed adoption

Table 3 presents the ITT effects and LATE of RCC and TC on the decision to use improved maize seeds (Panel A) and the intensity of improved maize seeds adoption (Panel B), measured as the proportion of total maize area planted with improved maize varieties. As a general observation, we do not find much evidence that either form of credit had a meaningful effect—either positive or negative—on the use of improved maize seed. We attribute the lack of a meaningful effect to the relatively high level of adoption of these improved maize varieties at the time of the project baseline. In the pooled sample, roughly 85 percent of farmers were already using improved maize varieties and were cultivating improved maize varieties on roughly 75 percent of maize cultivated area at the time of baseline, so it would be difficult to detect a meaningful effect of expanded credit access on these outcomes. 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 only marginal, however, and not statistically significant. There is consistent evidence that suggests that those who were offered and took TC were less likely to use improved maize varieties and had a smaller share of their maize fields under improved maize varieties, although, as with the positive effects of RCC, the negative effects of TCC on improved maize seed use were marginal in overall magnitude and not statistically significant. However, we do find rather consistent and convincing evidence that the effects of the RCC treatment on these improved maize outcomes are different from the effects of the TC treatment (for example, see the values reported in the "RCC = TC" rows in both Panels A and B).

6.1.3 Area under maize

We also consider the possibility that these credit products could have led farmers to increase their cultivated area, particularly by expanding their area under maize (the primary crop grown in the sample area). As we have previously noted, RCC (and, to a lesser extent, TC) had a positive effect on agricultural investments (adoption of and expenditures on chemical fertilizers per acre). The observed effect on fertilizer expenditures per acre is consistent with expanding production along the intensive margin, so it is also worth exploring whether these credit products led to an expansion of production along the extensive margin. Table 4 presents ITT effects and LATE on the overall area cultivated with maize. Across all specifications, we find little convincing evidence that either of the credit products had a meaningful effect on maize area. Although we find positive effects of both RCC and TC on the area under maize, these effects are rather small in overall magnitude and very imprecisely measured. Consequently, we conclude that there is convincing 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 (maize).

Table 3. Treatment effects on adoption of improved maize seeds

VARIABLE	(1) ITT SMD	(2) ITT ANCOVA	(3) LATE SMD	(4) LATE ANCOVA
Panel A: Use of improved maize seed (binary)				
RCC	0.019	0.018	0.072	0.069
	(0.017)	(0.017)	(0.066)	(0.061)
TC	−0.015	−0.010	−0.069	−0.044
	(0.019)	(0.017)	(0.084)	(0.079)
Outcome variable at baseline		0.291***		0.289***
		(0.020)		(0.028)
Individual fixed effects	No	No	No	No
Baseline covariates	No	Yes	No	Yes
Constant	0.877***		0.877***	0.675***
	(0.013)		(0.013)	(0.062)
RCC = TC: *p* value	0.056	0.101	0.065	0.118
Mean of control	***0.877***	***0.877***	***0.877***	***0.877***
Observations	1,998	1,983	1,998	1,983
R-squared	0.00	0.12	-0.01	0.12
Panel B: Proportion of land under improved maize seed				
RCC	1.519	1.128	5.791	4.321
	(1.852)	(1.762)	(7.029)	(6.469)
TC	−2.143	−1.854	−9.760	−8.458
	(1.932)	(1.764)	(8.901)	(8.221)
Outcome variable at baseline		0.335***		0.334***
		(0.020)		(0.024)
Individual fixed effects	No	No	No	No
Baseline covariates	No	Yes	No	Yes
Constant	80.556***		80.556***	65.249***
	(1.345)		(1.344)	(6.237)
RCC = TC: *p* value	0.052	0.089	0.056	0.089
Mean of control	***80.56***	***80.56***	***80.56***	***80.56***
Observations	2,015	2,008	2,015	2,008
R-squared	0.00	0.15	−0.01	0.14

Note: ANCOVA = analysis of covariance; ITT = intention to treat; RCC = risk-contingent credit; SMD = simple mean difference; TC = traditional credit. For simplicity, RCC and TC rows show the ITT and LATE estimates for all the estimators, that is, ITT SMD in column (1), ITT ANCOVA in column (2), LATE SMD in column (3), and LATE ANCOVA in column (4). All ANCOVA models had baseline covariates, including outcome-variable baseline measurement. The first-stage predicted uptake that applies is in panel A of Table A1 in the Appendix. Robust standard errors are shown in parentheses.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 4. Treatment effects on area under maize

VARIABLE	(1) ITT SMD	(2) ITT ANCOVA	(3) LATE SMD	(4) LATE ANCOVA
RCC	0.035	0.063	0.132	0.237
	(0.107)	(0.096)	(0.406)	(0.357)
TC	0.105	0.102	0.474	0.462
	(0.103)	(0.096)	(0.466)	(0.432)
Outcome variable at baseline		0.403***		0.402***
		(0.021)		(0.044)
Individual fixed effects	No	No	No	No
Baseline covariates	No	Yes	No	Yes
Constant	1.902***		1.902***	0.112
	(0.074)		(0.074)	(0.329)
RCC = TC: *p* value	0.502	0.683	0.435	0.572
Mean of control	***1.902***	***1.902***	***1.902***	***1.902***
Observations	2,007	1,992	2,007	1,992
R-squared	0.00	0.19	0.00	0.19

Note: ANCOVA = analysis of covariance; ITT = intention to treat; RCC = risk-contingent credit; SMD = simple mean difference; TC = traditional credit. For simplicity, RCC and TC rows show the ITT and LATE estimates for all the estimators, that is, ITT SMD in column (1), ITT ANCOVA in column (2), LATE SMD in column (3), and LATE ANCOVA in column (4). All ANCOVA models had baseline covariates, including outcome variable baseline measurement. The first-stage predicted uptake that applies is in panel A of Table A1 in the Appendix. Robust standard errors are shown in parentheses.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

6.2 Ex-post productivity impacts

Whereas the results presented in Tables 2–4 represent the ex-ante effects of these two credit products, we now turn our attention to their ex-post effects. These ex-post impacts of credit and insurance are higher-level impacts, and as one would expect, are harder to observe especially over few time periods and with relatively low uptake. Consequently, we find very little evidence that these credit products had any meaningful effect on these ex-post outcomes. Although we endeavored to control for any form of endogeneity, we cautiously interpret and claim causality here. We recommend that the results and interpretations be treated as indicative and a motivation for further investigation, especially with longer panels, a variety of credit and insurance products, and higher uptake.

As indicators of households' agricultural productivity, we consider maize yields and acreage revenue. Maize yield has been adjusted for intercropping, where we divide the farmer-reported yields by the inverse of the number of reported intercrops. We compute acreage revenue as a sum of the value of production per acre of the three main crops (maize, normal beans, and cow peas) grown by almost all the households in the study area, and whose production was also to be directly affected by the loans.[9] The computed value of the crops is based on the actual market prices (obtained from Kenya's Ministry of Agriculture and Livestock Development) of the commodities at Machakos market at the end of each season. The structure of the result tables (Table 5) is similar to that of the ex-ante result tables explained above.

6.2.1 Maize yield and acreage revenue

Panel A of Table 5 presents the ITT effects and LATE of RCC and TC on maize yield. Consistently, across all specifications, and for both RCC and TC, the results suggest a null or moderately negative effect of these credit products on maize yields. The TC treatment in particular shows a consistent and statistically significant negative effect on maize yields. In particular, among those who took up the TC product, maize yields were 550–580 kg per acre lower than in the control group ($p < 0.1$). Point estimates for the ITT effect and LATE of the RCC treatment are also negative but are imprecisely measured and therefore not statistically significant at conventional levels.

Panel B of Table 5 presents the ITT effects and LATE of RCC and TC on acreage farm revenue. Here, too, we observe both credit products having a null or negative effect on revenues per acre, though the effects are imprecisely measured and not statistically different from 0. Whereas we observe a more pronounced negative effect on maize yields in the TC treatment group (panel A), the results in panel B indicate larger negative effects on farm revenues per acre among the RCC treatment group (though, again, these effects are not measured precisely). We do not find any evidence that the effects of the RCC treatment are statistically different from the effects of the TC treatment (see the *p* values reported on the "RCC = TC: *p* value" rows in both panels A and B).

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[9] The main crop was maize, for which normal beans and cow peas were the common intercrops.

Table 5. Treatment effects on maize yield and acreage revenue

Variable	(1) ITT SMD	(2) ITT ANCOVA	(3) LATE SMD	(4) LATE ANCOVA
	Panel A: Maize yield			
RCC	−105.210	−72.228	−399.441	−268.911
	(67.783)	(66.207)	(256.421)	(250.964)
TC	−130.127*	−122.608*	−584.692*	−553.808*
	(66.521)	(66.356)	(299.396)	(294.278)
Outcome variable at baseline		0.259***		0.272***
		(0.034)		(0.037)
Individual fixed effects	No	No	No	No
Baseline covariates	No	Yes	No	Yes
Constant	1,236.527***		1,236.527***	1,120.348***
	(47.680)		(47.644)	(222.059)
RCC = TC: *p* value	0.710	0.444	0.507	0.295
Mean of control	***1237***	***1237***	***1237***	***1237***
Observations	1,996	1,981	1,996	1,981
R-squared	0.00	0.05	0.00	0.06
	Panel B: Overall average revenue			
RCC	−1,828.576	−1,490.954	−6,942.390	−5,628.192
	(1,161.209)	(1,165.607)	(4,388.495)	(4,332.049)
TC	−687.463	−795.872	−3,107.516	−3,644.003
	(1,191.856)	(1,168.760)	(5,378.405)	(5,295.947)
Outcome variable at baseline		0.173***		0.175***
		(0.030)		(0.037)
Individual fixed effects	No	No	No	No
Baseline covariates	No	Yes	No	Yes
Constant	17,887.031***		17,887.031***	15,258.268***
	(846.088)		(845.453)	(4,066.231)
RCC = TC: *p* value	0.324	0.549	0.430	0.678
Mean of control	***17887***	***17887***	***17887***	***17887***
Observations	2,002	1,989	2,002	1,989
R-squared	0.00	0.03	0.01	0.04

Note: ANCOVA = analysis of covariance; ITT = intention to treat; RCC = risk-contingent credit; SMD = simple mean difference; TC = traditional credit. For simplicity, RCC and TC rows show the ITT and LATE estimates for all the estimators, that is, ITT SMD in column (1), ITT ANCOVA in column (2), LATE SMD in column (3), and LATE ANCOVA in column (4). All ANCOVA models had baseline covariates, including outcome variable baseline measurement. The first-stage predicted uptake that applies is in panel A of Table A1 in the Appendix. Robust standard errors are shown in parentheses.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

6.3 Robustness checks

To validate our results, we conduct several robustness checks. First, to check if 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 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 Table A4 in the Appendix. Most of our 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 seven outcomes translating to 14 hypothesis tests in total. This raises concerns about the false discovery rate, 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 et al., 2006; Omotilewa et al., 2018; Romano & Wolf, 2005; Williams et al., 1999). To check for robustness of our results against the risk of false discovery, we conduct multiple hypotheses correction tests following Omotilewa et al. (2018) and Ksoll et al. (2016), who use sharpened *q* values proposed by Benjamini et al. (2006) and empirically validated by Anderson (2008). We conduct these procedures for the ITT ANCOVA and LATE ANCOVA estimations separately. Comparing the standard *p* values with the respective sharpened *q* values in the Table A5 for ITT and 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 acknowledge that both control and treatment households could potentially access credit from other sources besides what the project was offering. Indeed, 16 percent of the control and 10 percent of the treatment households reported having taken credit from other sources during the project's first phase (September 2017–February 2018 season), while about 20 percent of both control and treatment households reported taking credit from other sources during the second phase of the project (September 2019–February 2020). This could contaminate the experimental design and hence 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 noncompliers, access to such credit will inflate the ITT estimates and attenuate LATE.

We test for robustness of the results to this contamination by dropping contaminated households (who received credit from outside of our intervention) and running the treatment effect estimations for all the outcomes again.[10] We test this for the ANCOVA models only, both ITT ANCOVA and LATE ANCOVA. The treatment effect results in which contaminated households are dropped can be found in Table A7 in the Appendix. 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.

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

7 Conclusion and policy implications

Drought has been identified as one of the major risks to agricultural productivity for farmers in SSA. The impact of drought in this region is exacerbated by the absence of effective options of managing risk, such as formal insurance and production credit. Lately, weather index insurance has been trialled in this region and other low-income countries, but there has been 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. However, there is limited literature on the uptake of such innovations, and the gap in empirical literature on the potential impacts of such products on smallholders is even more pronounced.

Using data from an RCT in Kenya, we estimate the impact of an insurance-bundled credit product, RCC, and an uninsured product, TC, on rural smallholders' farm investment and

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[10] This would potentially introduce selection bias due to endogeneity in uptake of other loans outside our experiment. We check for this bias by running the baseline balance checks for outcome variables and covariates after dropping the "contaminated" observations at both midline and endline. Results indicate that our sample is still balanced across experimental groups, and hence selection bias after dropping the "contaminated" households is not a major concern.

productivity. We also test the hypothesis that the difference between RCC and TC effects is not different from 0.

Results demonstrate that random assignment to, and uptake of, RCC had some positive and significant effect on the decision to use chemical fertilizers on maize fields as well as on the amount of money spent on maize fertilizer. Assignment to and uptake of TC, although positively associated with fertilizer usage, the effect on adoption decision of improved seeds was not statistically significant, while the effect on 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 0, 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 take note of the 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 their investment decisions. This corroborates the long-held belief that (1) access to credit enhances access to high-yielding but capital-intensive technologies, and (2) hedging farmers' investment and capital against downside production risks such as drought can encourage the adoption of higher-risk, higher-yielding agricultural technologies and practices and hence enhanced investment decisions ex-ante.

Although we report enhanced farm investment in terms of use of higher-yielding inputs (more so from RCC), there is no evidence that either of the two credit types had any 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, the main crop of interest in this study. In the recent literature, increasing households' farm production and hence enhancing their livelihoods and welfare via the sustainable intensification route has gained traction. Indeed, this is currently considered among the climate-smart agricultural (CSA) practices. Our results confirm that agricultural credit, more so when coupled with cover against production risks, has a significant role to play in promoting sustainable intensification as a CSA. It is possible, however, that such financial products will be associated with expansion in the long run. For instance, when optimal acreage productivity is reached and a farm starts realizing diminishing marginal returns from additional investment, a rational household may then decide to put additional credit into a new field, either own land previously left fallow, rented-in land, or even borrowed-in land.

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

In summary, both RCC and TC had direct positive and significant effects on agricultural investment, fertilizer spending in particular. They both seemingly did not have a direct effect on the ex-post productivity outcomes. Also, compared to TC, RCC had higher ex-ante investment benefits, which were 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 and productivity, but investigations for such effects need to acknowledge that higher-level ex-post outcomes are harder to measure and realize in the short term. They may be affected indirectly. This is particularly true in areas such as Machakos County, where agricultural credits have been perennially absent, especially among smallholder households. Moreover, in our previous paper, uptake of insurance-bundled credit was significantly higher than that of TC (Ndegwa et al., 2020). This calls for policymakers to ensure that safe and inexpensive production credit products are provided to agricultural households if widespread benefits of agricultural credit are to be realized.

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7.1 Appendix

Table A1: First-stage results for the second-stage least squares (2sls) models

	Panel A, complete panel: T = 0, T = 1, and T = 2			Panel B, at midline: T = 0 and T = 1		
Variable	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

Note: RCC = risk-contingent credit; TC = traditional credit. Standard errors are shown in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A2: Attrition rate by experimental groups and survey round

		Midline			Endline		
Exited/dropped	Baseline	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

Note: RCC = risk-contingent credit; TC = traditional credit.

Table A3: Testing for random attrition

Variable	(1) Control	(2) TC	(3) RCC	(4) Overall univariate	(5) Overall multivariate
Treatment variable					
Assigned TC				−0.194	−0.119
				(0.151)	(0.153)
Assigned RCC				−0.225	−0.195
				(0.153)	(0.156)
Outcome variable					
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 “000”	1.219*	−0.118	0.513		0.355
	(0.663)	(0.570)	(0.629)		(0.360)
Binary improved maize variety adoption	−0.011*	0.001	−0.005		−0.004
	(0.006)	(0.006)	(0.006)		(0.003)
Share of land under improved varieties	−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)
Socioeconomic control					
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 (TLU) holding	0.028	0.181	−0.150		−0.021
	(0.196)	(0.171)	(0.187)		(0.108)
Subjective welfare – 5-point scale	−0.151	0.183	−0.054		0.057
	(0.273)	(0.223)	(0.289)		(0.150)
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 HHIDs	313	349	349	1,065	1,045

Note: HHID = Household ID; RCC = risk-contingent credit; TC = traditional credit. Robust standard errors are shown in parentheses.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A4: ITT-based Lee bounds estimates to account for attrition bias on treatment effects

	RCC		TC	
Variable	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 varieties 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	−0.124	−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)

Note: ITT = intention to treat; RCC = risk-contingent credit; TC = traditional credit. Robust standard errors are shown in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A5: Multiple hypotheses testing using the ITT ANCOVA models

Variable	Fertilizer use binary	Fertilizer spending	Improved varieties adoption	Improved varieties share	Maize area	Maize yields	Overall acreage revenue
Risk-contingent credit							
p value	0.081	0.000	0.269	0.484	0.647	0.143	0.084
Sharpened *q* value	0.368	0.001	0.474	0.474	0.503	0.401	0.368
Traditional credit							
p value	0.136	0.057	0.669	0.391	0.399	0.028	0.303
Sharpened *q* value	0.401	0.368	0.503	0.474	0.474	0.319	0.474

Note: ITT = intention to treat; ANCOVA = analysis of covariance.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A6: Multiple hypotheses testing using the LATE ANCOVA models

Variable	Fertilizer use binary	Fertilizer spending	Improved varieties adoption	Improved varieties share	Maize area	Maize yields	Overall acreage revenue
Risk-contingent credit							
p value	0.078	0.000	0.252	0.477	0.652	0.157	0.084
Sharpened *q* value	0.335	0.001	0.536	0.672	0.704	0.431	0.335
Traditional credit							
p value	0.134	0.033	0.676	0.399	0.402	0.028	0.309
Sharpened *q* value	0.431	0.231	0.704	0.672	0.672	0.231	0.576

Note: ANCOVA = analysis of covariance; LATE = local average treatment effects.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A7: ANCOVA-based treatment effects (ITT and LATE) on all outcomes after dropping the contaminated observations/households

Variable	(1) Fertilizer use binary	(2) Fertilizer spending	Improved varieties adoption	(4) Improved varieties share	(5) Maize area	(6) Maize yields	(7) Overall acreage revenue
Panel A: Contamination-adjusted ITT ANCOVA estimates							
Assigned RCC	0.045**	567.926***	0.020	1.745	−0.038	−61.287	−1,473.958
	(0.021)	(107.780)	(0.019)	(1.926)	(0.106)	(62.140)	(1,079.653)
Assigned TC	0.032	224.104**	−0.004	−1.023	0.039	−128.575**	−1,411.267
	(0.021)	(109.453)	(0.019)	(1.950)	(0.107)	(62.999)	(1,094.458)
Observations	1,695	1,633	1,673	1,695	1,680	1,671	1,679
R-squared	0.26	0.18	0.12	0.16	0.15	0.25	0.23
Mean of control	**0.765**	**1,686**	**0.871**	**79.44**	**1.922**	**1,209**	**17,472**
Panel B: Contamination-adjusted LATE ANCOVA estimates							
Took RCC	0.167**	2,057.251***	0.074	6.463	−0.139	−221.999	−5,437.154
	(0.077)	(382.351)	(0.067)	(7.042)	(0.394)	(235.701)	(4,110.981)
Took TC	0.140	962.015**	−0.018	−4.487	0.169	−561.948**	−6,293.886
	(0.094)	(407.105)	(0.086)	(8.798)	(0.485)	(273.184)	(4,812.613)
Observations	1,695	1,633	1,673	1,695	1,680	1,671	1,679
R-squared	0.28	0.23	0.12	0.16	0.15	0.24	0.22
Mean of control	**0.765**	**1,686**	**0.871**	**79.44**	**1.922**	**1,209**	**17,472**

Note: ANCOVA = analysis of covariance; ITT = intention to treat; LATE = local average treatment effects; RCC = risk-contingent credit; TC = traditional credit. Robust standard errors are shown in parentheses.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.