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**Impacts of an Innovative Credit +
Insurance Bundle for Marginalized Farmers**

Evidence from a Cluster Randomized Trial in Odisha, India

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Abstract

Smallholder farmers often lack documented land rights to serve as collateral for formal loans, with livelihoods inextricably linked to weather conditions. Resulting credit and risk constraints prevent them from investing in their farms. We implemented a randomized evaluation of KhetScore, an innovative credit scoring approach that uses remote sensing to unlock credit and insurance for smallholders including landless farmers in Odisha, a state in eastern India. In our treatment group, where we offered KhetScore loans and insurance, farmers - and especially women - were more likely to be insured and borrow from formal sources without substituting formal for informal loans. Despite increased borrowing, treated households faced less difficulty in repaying loans, suggesting that insured KhetScore loans transferred risk and eased the burden of repayment. Moreover, the treatment enhanced agricultural profitability by increasing revenues during the monsoon season and reducing costs in the dry season. Positive and significant effects are found among both farmers with unconstrained baseline credit access, and quantity rationed farmers, suggesting that KhetScore helps address supply-side credit constraints. Finally, the treatment significantly enhanced women's empowerment and mental health. In conclusion, remote sensing-enabled financial products can substantially improve landless farmers' access to agricultural credit, risk management, resilience, and well-being.

Keywords: credit, insurance, impact evaluations, gender parity, India

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1 Introduction

In developing countries, financial inclusion and risk management are critical to fostering investments to improve agricultural productivity and enhance rural livelihoods. Amid traditional barriers like limited access to credit and vulnerability to agricultural risks, smallholder farmers often lack the wherewithal to expand their operations or invest in profitable technologies and inputs. For smallholders with limited land holdings, formal financial services like credit and insurance are often considered cost-prohibitive due to high transaction and monitoring costs, along with limited available data on these farmers to identify the risks of insuring or lending to these farmers. For instance, lacking documented land rights or lacking the necessary collateral limits potential borrowers' access to formal credit (Higgins et al., 2018), and consequently potential borrowers are involuntarily limited in their borrowing, resulting in efficiency losses and potential entrapment in a low productivity equilibrium (Croppenstedt et al., 2013). Even when individuals may have access to formal credit, credit markets can be in a disequilibrium in which lenders restrict potential borrowers' access to their desired level of borrowed funds to finance agricultural investments (Stiglitz and Weiss, 1981). These forms of supply-side credit rationing often arise due to information asymmetries between borrowers and lenders. In particular, to the extent that lenders are unable to accurately assess the credit worthiness of potential borrowers (e.g., if they do not have a credit report with a formal credit rating agency), lenders may restrict their lending to avoid potential adverse selection. Further, since it is difficult to monitor borrowers' actions after a loan has been disbursed, lenders may restrict the amount lent to limit potential losses due to moral hazard. In other cases, risk averse borrowers may voluntarily withdraw from the credit market, often because they are afraid of losing their collateral if there is an adverse calamity and they are unable to repay the loan. Such "risk rationing" (Binswanger and Sillers, 1983; Boucher et al., 2008) occurs in any situation in which the lender shifts so much contract risk onto the borrower (in the form of excessive collateral requirements, high interest rates, unfavorable repayment terms, etc.) that the borrower voluntarily opts out of the credit market, even when otherwise qualifying for loans of a desired size.

Innovations in financial markets and financial products can potentially overcome both types of credit rationing and expand investments in agriculture (J-PAL and CEGA, 2022). For example, technological innovations may make it possible for lenders to overcome informational barriers to mitigate adverse selection and moral hazard, which in turn could alleviate quantity rationing in credit markets. For example, using emergent predictive modeling tools (such as machine learning and artificial intelligence), it may be possible to assess a potential borrowers' credit worthiness even in the absence of a formal credit report. Advancements in monitoring technologies may also address concerns over moral hazard in credit markets. Monitoring is essential in addressing moral hazard issues in these markets, since monitoring can reduce private benefits for

borrowers (Fedele, 2005) and allow firms to adjust charges based on monitored behaviors (Chen and Jiang, 2019). Traditional methods for monitoring borrowers' behavior can be quite costly, so increasingly lenders are leveraging technological innovations as a low-cost means of monitoring. Kouzez and Lecointre-Erickson (2019) and Bülbül et al. (2019) point out that advancements in information technology and financial innovations have enhanced the monitoring of credit risk in developing countries, highlighting the extent to which technological progress has contributed to improving the oversight of credit markets in these regions. Additionally, Hyytinen and Toivanen (2004) have noted the potential for monitoring technologies, originally designed for controlling moral hazard, to be repurposed by banks for other uses beyond managing moral hazard, thus potentially increasing the returns to technological innovations.

At the same time, it is also well known that crop insurance has the potential to de-risk agricultural investments through de-coupling production *risk* from production *decisions*, often leading to increased investments in modern technologies (e.g., Miranda and Farrin, 2012; Karlan et al., 2014; Elabed and Carter, 2014; Cai et al., 2015; Hill et al., 2019). Yet, the myriad challenges with agricultural insurance are also well known (e.g., Hazell et al., 1986; Binswanger-Mkhize, 2012; Miranda and Farrin, 2012; Carter et al., 2017; Kramer et al., 2022). In particular, traditional agricultural insurance suffers from high costs associated with monitoring and assessing losses, resulting in high administrative loads; informational asymmetries manifesting as both adverse selection and moral hazard; the highly covariate nature of production risks; and a mismatch between insurance *demand* (which is frequently highest among wealthier farmers) and who would actually most *benefit* from risk transfers (typically poor farmers without the ability to self-insure). Index insurance is frequently advocated as an alternative to conventional agricultural insurance, since it eliminates the need for monitoring and assessing individual losses; is objective; and is transparent. Although index-based insurance programs promoted to address some of these challenges have their own shortcomings (namely, basis risk; the risk that index measurements are incongruous with an insured individual's actual experiences), they have recently been able to achieve modest scales by leveraging remote sensing, for instance to predict livestock mortality (Chantarat et al., 2013) or estimate food insecurity and humanitarian response costs, enabling risk pooling and drought insurance for countries (Kramer et al., 2020). However, to harness the full potential of remote sensing, it is essential to deepen our understanding of the extent to which it can enhance rural credit access, help smallholders manage agricultural risks (Benami et al., 2021), and improve targeted populations' wellbeing and livelihoods.

Recent financial innovations have led to packaging credit and insurance together in a bundled financial product. The theoretical underpinnings of such bundles lie both in financial mediation and risk management, and date back at least several decades. Stiglitz and Weiss (1981) noted that one method to ameliorate rationing

in credit markets with imperfect information is through risk-reducing instruments like insurance. Being able to insure a loan, and thus potentially protect households from having to adopt costly income-smoothing adaptations in the event of an environmental shock or other calamity, could improve livelihoods (Morduch, 1995). To that extent, credit + insurance bundles provide a safety net for farmers, at least partially protecting them from adverse events. But there are other potential impacts of such products. Carter et al. (2016) have demonstrated theoretically how linking credit with insurance can have numerous effects on the terms of the products themselves, the overall agricultural credit market, and important downstream effects on borrowers. Specifically, they demonstrate how interlinking insurance with credit can, compared to what would be feasible either in the absence of insurance or if the insurance were standalone (not explicitly linked to the loan), (i) produce lower interest rates; (ii) flatten lenders’ agricultural loan offer curves, similar to what would be observed if loans were fully collateralized; and (iii) encourage adoption of advanced technologies. Clearly, therefore, there is at least theoretically great potential for interlinked credit and insurance products to slacken both supply- and demand-side credit constraints, while increasing liquidity to facilitate adoption of modern productivity-enhancing inputs.

Combining credit and insurance as a means of simultaneously overcoming liquidity and risk constraints in developing countries has been a topic of interest in academic research in recent years, though with some mixed results (Giné and Yang, 2009; Karlan et al., 2014; Gallenstein et al., 2021; Ndegwa et al., 2020; Mishra et al., 2023). A common thread that emerges among these studies is the importance of both price and non-price factors in the adoption of bundled insurance products, and the challenges posed by incomplete insurance and credit markets in developing countries. But there are also some important differences in the products being offered that may in part contribute to the mixed results. In some cases (e.g., Giné and Yang, 2009; Ndegwa et al., 2024 and the “meso-insurance” product evaluated in Mishra et al., 2023), the insurance products are designed to directly repay the lender, rather than borrower, while in some other cases (e.g., the “micro-insurance” product evaluated in Mishra et al., 2023), insurance payouts go directly to the borrowers. As Carter et al. (2016) have pointed out, this distinction has important theoretical implications for the impacts that these different credit and insurance bundles may yield. In cases where the insurance contract compensates the borrower rather than the lender, knowledge about the existence of the insurance contract is asymmetric, and lenders will operate as if there were no insurance. On the other hand, when the insurance is structured to compensate the lender, there is a direct linkage between the credit and insurance components of the bundle, with the aforementioned theoretical impacts on product terms, market structure, and borrower behavior. In this case, these insurance payouts are used to expunge borrowers’ outstanding debts, thereby minimizing (if not fully eliminating) the need for collateral, but also likely reducing cyclical borrowing

and increased indebtedness. These bundled insurance and credit products are essentially *contingent* credit products, since the amount borrowers ultimately need to repay depends on the realization of a stochastic weather outcome, and whether it is so adverse as to trigger payouts from the associated index insurance product. This contingency in repayment might be thought to increase credit demand and increase investments in riskier agricultural inputs that could enhance productivity and profitability.

Consistent with these theoretical predictions, Ndegwa et al. (2024) find that bundling insurance with credit in Kenya increased the proportion of farmers using chemical fertilizers by over 13 percent relative to the status quo and led to an almost 2,000 Kenyan shilling increase in fertilizer expenditures per acre (roughly USD 20 per acre). What’s more, the increase in fertilizer expenditures with the interlinked credit and insurance product was more than 2.5 times what was observed when farmers had a traditional credit product without accompanying index insurance. In the context of smallholder production in Malawi, Giné and Yang (2009) surprisingly find substantially lower credit take-up when the loans (specifically group liability contracts to purchase hybrid maize seeds) were packaged with an actuarially-fair index insurance product, thus not only contradicting the expectation that the repayment contingency should increase credit demand but also that it should increase investments in productivity-enhancing inputs. Mishra et al. (2023) is an interesting case study in this regard in that it examines the impacts of a credit and insurance bundle in which the insurance and credit are interlinked (i.e., insurance payments are directed toward the lender, what the authors refer to as a “meso-insurance” bundle) as well as a bundle in which the linkages between credit and insurance are less pronounced (i.e., insurance payments are directed toward the borrower, what the authors refer to these as a “micro-insurance” bundle). Although they do not directly examine differences in credit take-up based on the differences in insurance payment terms, they find that only the loans with the “micro”-insurance yielded positive effects on compound fertilizer application, and that these effects were only observed during the second year of lending operations.

This study assesses the impacts of a bundled credit and insurance product that leverages emergent technologies like smart phones, remote sensing, and machine learning. In particular, we evaluate KhetScore – a novel approach to credit scoring using remote sensing and crop analytics.¹ KhetScore was designed to reduce transaction costs and overcome information asymmetries and documentation requirements in the provision of financial instruments for marginal and landless farmers. In the Indian context, female farmers frequently lack documented land rights required by formal lenders as a condition for qualifying for a loan. KhetScore aims to precisely address this issue by producing a lens into the production and profitability potential of a plot of land regardless of who holds the title. Further, since these loans are bundled with insurance, there

¹Technically, “KhetScore” refers *only* to the credit scoring approach, but throughout the remainder of this paper we will reference “KhetScore loans” when discussing the loans that were disbursed at least partly on the basis of a farmer’s KhetScore.

is both less of a need for collateral to secure a loan and a lower risk of collateral loss, since the insurance component would compensate the lender in the event of a catastrophic hazard that destroys a farmer’s crop. Consequently, there is great potential for this kind of product to ease both supply-side rationing on the part of the lender and demand-side rationing on the part of the potential borrower. Therefore, the evaluation pays particular attention to gender and baseline credit rationing status, conditions that have historically been associated with limited credit access and utilization.

Partnering with Dvara E-Registry (DER), a highly-regarded social enterprise operating in more than 12 different states in India, we implemented a cluster randomized trial with an initial pool of around 1,800 potential KhetScore clients from 58 villages in the district of Jajpur in eastern Odisha. These 58 villages were randomized into a treatment arm (29 villages), where farmers were offered KhetScore loans bundled with insurance to protect them from the risk of crop failure, and a control arm (29 villages), where farmers were not offered loans based on the KhetScore credit scoring approach but may have accessed other sources of credit as well as insurance coverage under the Indian government’s national crop insurance scheme (Pradhan Mantri Fasal Bima Yojana, or PMFBY), which requires land titles. We find beneficial impacts of offering farmers loans bundled with insurance over a wide range of outcomes, including financial inclusion, agricultural profitability, gender parity, and mental health outcomes. In particular, the evaluation demonstrates the potential for this novel financial product to have transformative effects on the rural economy by strengthening financial literacy, improving credit and insurance uptake, increasing farm profits, enhancing women’s empowerment, and easing some of the psychological and emotional health stresses that so often accompany near-subsistence agriculture.

The contributions of this paper are threefold. First, we demonstrate that this kind of credit and insurance bundle – and particularly one that determines an applicant’s creditworthiness not based on traditional metrics but based on a metric using emergent digital technologies to determine production and profit potential – can crowd in marginalized groups into financial markets and improve their overall experience with credit. This effect is especially pronounced among women, who are typically excluded from credit markets because they often lack the documented land rights required by lenders as a condition for obtaining a loan. Not only did women take up loans at an accelerated pace compared to their counterparts in the control villages, but they also reported an increase in borrowing from formal sources and a decrease in borrowing from informal sources, therein largely increasing the extent to which they are included in the formal financial sector. We also demonstrate that this kind of credit and insurance bundle comes with terms that borrowers find especially favorable, as there is a significant reduction in the proportion of borrowers who reported difficulties in repaying their loans, and these effects persist even among members of these groups who might otherwise face financial difficulties.

Second, we contribute to the evidence on strategies to alleviate constraints faced by smallholder farmers to increase their agricultural output and productivity. In particular, local average treatment effects estimates indicate that those farm households that received KhetScore loans bundled with insurance were both more likely to cultivate and cultivated a greater area during the Rabi (dry) season – a season in which many farmers in Odisha leave their land fallow due to various constraints, including limited irrigation capabilities. Further, local average treatment effects estimates demonstrate that borrowers were able to attain higher profits per acre across both the Kharif (monsoon) and Rabi agricultural seasons compared with other households that did not procure KhetScore loans. Among those that borrowed funds, revenues and profits per acre during the Kharif season were INR 8,500 (about USD 110) and INR 9,100 (about USD 118) higher than among farmers who did not avail KhetScore loans, while profits during the Rabi season were about 12,800 (about USD 166). The effects are largest among those farmers who were not credit rationed at the time of the project baseline, but the beneficial impacts carry over to those farmers who were quantity rationed at baseline, implying that this product successfully alleviates supply-side constraints to borrowing that could enhance agricultural livelihoods.

Finally, we demonstrate that this innovative credit and insurance bundle has the potential to significantly enhance livelihoods for women, regardless of whether they directly benefit from expanded credit access. We find that women from villages randomly assigned to be offered KhetScore loans expressed enhanced feelings of empowerment, thereby adding to the literature on improving gender parity in agriculture through financial inclusion. We find positive intention-to-treat effects across several different domains of women’s empowerment, but it is worth noting in particular that those women who were the primary clients (with the loan and insurance bundle taken out in their name) reported a significant increase in their contributions to household *financial* decisions, especially those related to the decisions whether to borrow and how to use borrowed funds. But even those women who were *not* the primary clients – but who co-signed on loans taken out by male members of their households (e.g., their husbands) – reported improvements in their empowerment, particularly in terms of their contribution to household livelihood decisions, their control over their own income, and their accumulation of assets. From anecdotal qualitative evidence, these latter results seem to emerge as a consequence of male household members sharing more information with their female household members because they were co-signees on the loans. Although women’s contributions in these decision-making domains remain relatively low, these beneficial impacts hint at this products’ potential to alter intrahousehold dynamics and improve gender parity in financial matters. We also find that women from randomly assigned treatment villages reported significantly lower levels of perceived stress, with these effects concentrated among women who were not the primary clients. This finding highlights the extent to which

women *indirectly* bear risks related to agricultural production through conjugal contracts, and how bundling insurance with agricultural credit can help to alleviate their concerns about the costs of downside agricultural risks.

The remainder of this paper is structured as follows. Section 2 describes the methods employed in our impact evaluation, including an overall description of the intervention (section 2.1), the design and timeline of the evaluation (section 2.2), sampling and data collection methods (section 2.3), and our empirical specification (section 2.4). Section 3 provides an overview of the DER clients that comprise our sample, disaggregated by randomly allocated treatment and control villages. Section 4 presents the results of this impact evaluation across various outcome domains, including financial market development (sections 4.2 and 4.3), agricultural outcomes (section 4.4), intrahousehold gender dynamics (section 4.5), and women’s perceived stress (section 4.6) at both the aggregate level, for male versus female farmers, and based on whether clients were credit constrained (either quantity or risk rationed) at the time of project implementation, where appropriate. Section 5 offers some insights into the cost effectiveness of the overall KhetScore program, and section 6 concludes.

2 Methods

2.1 Context and intervention

We implemented this study in two blocks of Jajpur, a district in the Indian state of Odisha’s low-lying coastal plains. In this district, agricultural activities are one of the primary sources of income, with paddy rice being the dominant crop grown. Irrigation is available in Jajpur due to the vicinity of the Mahanadi River, the largest river of the state. This creates opportunities for farmers to expand production during the Rabi season, but credit and risk are often cited as a primary reason for not investing in crop production during this season. Intercropping is not common, making it suitable for applications that use remote sensing for crop monitoring. Despite relatively higher literacy and education levels compared to farmers in other districts, Jajpur has a large concentration of sharecroppers and marginalized landless farmers who fall outside the purview of state-sponsored credit and insurance schemes (including PMFBY), since they lack documented land rights required by these schemes.²

²PMFBY was initiated in 2016 by the Ministry of Agriculture and Farmers’ Welfare in an attempt to provide affordable insurance for farmers against a spectrum of disaster risks (including yield losses due to natural fire and lightening; storms, hailstorms, cyclones, and tornadoes; floods and landslides; droughts; and pests and crop diseases). Under this program, the government subsidizes a portion of the insurance premiums for small and marginal farmers, with farmers’ share of insurance premiums capped at 2 percent of the sum insured for Kharif (monsoon) crops, 1.5 percent of the sum insured for Rabi (dry season) crops, and 5 percent of the sum insured for commercial and/or horticultural crops, regardless of which season they are grown. Coverage is compulsory for farmers who take out agricultural loans under the Kisan Credit Card scheme (a separate program implemented by the Government of India to provide financing to farmers for agricultural operations). In order to be

To address this challenge, our implementing partner, DER, developed KhetScore, an innovation that uses remote sensing and machine learning approaches to ascertain farmers’ creditworthiness. To provide KhetScore loans, DER (i) digitizes information on a farmer’s land parcels (regardless of whether a farmer owns, rents, or sharecrops the land) using georeferenced smartphone images for verification; (ii) creates plot-level agricultural credit scores by estimating past and current productivity from satellite imagery; (iii) provides loans for farmers with sufficiently high credit scores; and (iv) adds on crop insurance to de-risk these loans using smartphone images taken at regular intervals throughout the crop season to verify any claims of crop damage due to extreme weather or other natural hazards such as pests and disease.

One of the main aims of the KhetScore credit scoring approach is to unlock credit for individuals who do not have an established credit report with formal credit bureaus. The score is an index ranging between 0 and 100 based on six variables with different weights, including productivity (40 percent), changes in crop health (20 percent), moisture (15 percent), past instances of mechanical damage (13 percent), nutrient availability (7 percent), and the area of cultivation (5 percent). A farmer’s KhetScore needs to be at least 40, which is considered to indicate reasonable creditworthiness, in order to be eligible for a loan. Applications may, however, still be rejected if an applicant’s identification card (Aadhaar, India’s unique identification number) does not match bank account details; if the applicant has a poor credit report (as opposed to *no* credit report) with the credit bureau; or if the farmer did not cultivate for three consecutive years during the agricultural season (Kharif or Rabi) for which the farmer is requesting a loan.

KhetScore loans have been designed around agricultural operations, with the loan size explicitly based on the cost of paddy rice production for an acre of land, which is an estimated 30,000 Indian Rupees (INR) (roughly 400 USD).³ The fact that these loans are tailored more to agriculture is perhaps one aspect that differentiates these loans from traditional microfinance loans. The interest rate on these loans is 14 percent, well below the prevailing microcredit interest rate of 24 percent.⁴ Repayment is done in multiple installments, with two to three payments before harvest, and a final bullet repayment soon after harvest. Future loans are provided to farmers subject to timely repayment of prior loans and availability of funds, but farmers are granted a certain degree of flexibility in loan repayment, to maximize the chances that a farmer can repay. Though there is a nominal penalty (INR 5 per day) for late repayment, they can be granted additional time to repay the loan in case they have a legitimate excuse for being unable to repay. In the rare case that

eligible for coverage under PMFBY, farmers must have a valid and authenticated land ownership certificate or a valid land tenancy agreement and must have a valid bank account and valid identify proof at the time of enrollment.

³Over the duration of the study, the prevailing exchange rate fluctuated between 0.012 INR/USD to 0.014 INR/USD. Throughout the remainder of this paper, we use an exchange rate of 0.013 INR/USD for converting monetary sums from INR to USD.

⁴Due to the use of technology to monitor these loans, DER incurred lower operational costs, and thus was able to charge a lower interest rate and be more flexible in their lending terms.

the farmer does not repay a loan, DER staff work with the farmer for proper follow up and counselling. Repayment rates were very high, at 83 percent.

Farmers whose applications were approved would receive a loan bundled with a hybrid crop insurance covering the full value of the loan. The insurance policy covers farmers for any visible crop damage from natural causes, including excess rainfall, hailstorms, winds, animal attacks, pests and diseases, landslides, and localized un-seasonal rainfall. Several of these perils are characteristic of cyclonic activities that regularly impact coastal Odisha. To verify claims of crop damage and ensure adequate crop management leading up to the damage, DER staff took bi-weekly images of insured land until the time of harvesting. For farmers who report damage, claims are calculated by agricultural experts at DER who review the images to declare a percentage of loss. This loss assessment determines the insurance payout, which is then sent into the farmer's bank account.

Within each of the 58 study villages (including both treatment and control villages) DER organized farmer profiling meetings during which they compiled a list of prospective clients who attended these meetings. During these profiling meetings, DER provided product training, introducing farmers to KhetScore and the associated bundled credit + insurance product. After that, farmers in treatment villages could apply for a KhetScore loan. Farmers interested in a KhetScore loan needed the approval of at least one other family member (typically their spouse), and this person would serve as a co-signer on the loan. Any farmer with a KhetScore loan was also enrolled in insurance, with premiums fully subsidized in this initial pilot phase. Farmers who did not apply for a KhetScore loan, or whose applications were rejected, could still apply for the insurance product separately. Farmers in control villages were not permitted to apply for KhetScore loans from DER, but were not prohibited from applying for loans from other lenders nor from accessing crop insurance under the state-sponsored PMFBY crop insurance scheme.

2.2 Design and timeline

To evaluate the impacts of this bundled financial instrument, we implemented a cluster randomized trial with an initial target sample of about 1,800 potential KhetScore clients from 58 villages of two blocks in Jajpur, selected due to their large population of marginalized and landless farmers and DER's existing presence in these areas. DER had lending operations in all villages in these two blocks, except for those deemed unfeasible due to the high risk of flooding. Including these villages in the sample would have increased insurance costs to unsustainable levels and potentially jeopardized the ability of farmers in these villages to repay their loans.

We randomized all 58 villages into a treatment arm (29 villages), where farmers were offered the

KhetScore loans bundled with insurance, and a control arm (29 villages), where DER did not offer any loans or insurance products, though farmers may have accessed other sources of credit (e.g., from other formal lenders or from informal sources) or insurance (e.g., under the national insurance scheme) if they could produce the necessary documentation to overcome the administrative hurdles. The randomized trial was implemented over three consecutive seasons, including the Kharif seasons of 2021 and 2022, and the Rabi season of 2021/2022. A timeline of the study is provided in Figure 1 below.

2.3 Sampling and data collection

All farmers that attended the village-level farmer profiling meetings comprise our sampling frame. At baseline (at the end of 2020 through early 2021), we invited the full list of 1,872 farmers for a phone survey (in-person data collection was not possible due to COVID-19 related restrictions on mobility in Jajpur district). Based on power calculations prepared prior to study implementation, we aimed to survey at least 1,800 farmers at baseline. Assuming a 3 percent attrition rate from baseline to endline, this meant recruiting at least 1,854 clients through product meetings. Some of the farmers (62) invited for the phone survey were not reachable due to incorrect phone numbers or phones being switched off. Others (101) refused to consent to the interview. Consequently, we were able to survey 1,706 households.

The endline survey was conducted in person in November/December 2022, and by then, pandemic-related restrictions on mobility had been lifted. We aimed to re-survey all respondents at baseline, as well as one other family member of the opposite sex (usually the client’s spouse, and typically a co-signer for loans issued by DER). Of the 1,709 clients surveyed at baseline, we were able to track and re-survey 1,621 clients (about 90 percent). We were able to identify and interview another family member for 1,294 of those respondents, for a total sample of 2,915 individuals.

Both baseline and endline surveys included modules to collect information about the respondent’s demographics, household income, financial inclusion, credit practices, women’s empowerment, insurance take-up and renewal, awareness and perceptions of insurance, income from insured crops, and women’s mental health. Questions related to insurance take-up and renewal, awareness and perceptions of insurance, and income from insured crops, were asked only of DER’s primary client, regardless of gender. Questions about women’s mental health (perceived stress) were asked only to the female member of the household, regardless of whether they were the primary client.⁵ Other questions (related to demographics, household income, financial

⁵Our registered pre-analysis plan (study ID RIDIE-STUDY-ID-6110cd09b6003) indicates that we will also evaluate impacts on household food consumption and women’s dietary diversity. These outcomes were included at the request of the project donor, to track such measures among all funded projects. We have not included these outcomes in the impact evaluation presented in this manuscript, both because there were no meaningful impacts observed, but also because there is not an obvious causal mechanism by which we would have expected there to be observed impacts in the time frame of this evaluation.

inclusion, and credit practices) were asked to both respondents in each household. At baseline, we also included a series of questions to determine types of credit rationing in the sample. As alluded to previously, credit rationing frequently takes one of two specific varieties, namely quantity (supply-side) rationing or risk (demand-side) rationing. These are obviously not part of the vernacular for the average farmer in Odisha, so classifying individuals as being quantity rationed, risk rationed, or unconstrained (as it pertains to credit) requires some deduction.

In particular, the baseline questionnaire included two questions, one that asked how much the respondent needed to borrow to finance household activities, and a second that asked how much credit was actually granted. If these two sums were the same, the respondent was deemed to not be credit constrained. If the desired loan amount was less than the loan amount granted, the respondent was deemed to be quantity rationed, since this would indicate a supply-side limit on the amount the respondent could borrow. There were also respondents who indicated that they did not borrow within the last 12 months, and a third question attempted to elicit the reason(s) why the respondent did not borrow in the previous 12 months. If the respondent indicated that they did not borrow because (a) the loan amount offered was insufficient, (b) they were deemed to not be creditworthy, or (c) they lacked sufficient capital to secure a loan, they were classified as quantity rationed, again because these barriers to borrowing stem from supply-side conditions. If, on the other hand, the respondent indicated that they did not borrow in the previous 12 months because (d) they were afraid of losing collateral, (e) the loan terms were not flexible enough, (f) the application costs were too high, or (g) there was a high interest rate, they were deemed to be risk rationed, since these constraints reflect demand-side considerations inhibiting borrowing.⁶ A summary of credit rationing status by experimental arm is discussed in greater detail below in Section 3.1.

2.4 Empirical strategy

To estimate intention-to-treat (ITT) effects, we estimate a series of OLS regressions in which endline values of various outcomes (dependent variables) are regressed on a binary variable indicating whether the farmer was from a village randomly assigned to be in the treatment group controlling for baseline values of the dependent variable and a vector of covariates, with standard errors adjusted for the clustered nature of the experimental design (i.e., by village, since randomization was done at the village level; Abadie et al., 2023).⁷

⁶It should be noted that the risk aversion reflected in these conditions is not solely on the part of the (potential) borrower. High interest rates, inflexible loan terms, and high application costs are ways in which the lender can (or at least attempts to) transfer some of the contract risk on to the applicant, but ultimately it is the applicant that decides to voluntarily withdraw from the credit market due to these conditions.

⁷For some outcomes, we do not have baseline values to include in “pure” ANCOVA regressions. In such cases, we include baseline values of a close proxy in “pseudo”-ANCOVA regressions.

Our base econometric specification will be the following:

$$Y_{ib,1} = \alpha + \beta Y_{ib,0} + \delta_1 T_{ib} + \sum_{j=1}^J \theta_j \mathbf{x}_{ijb,0} + \varepsilon_{ib} \quad (1)$$

where $Y_{ib,t}$ is the measure of outcome Y for individual i from block b in period $t \in 0, 1$, with $t = 0$ and $t = 1$ indicating baseline and endline, respectively; $\mathbf{x}_{ijb,0}$ is a vector of covariates including block fixed effects to account for stratification and variables with statistically significant differences between the treatment versus control at the time of the baseline, as well as demographic controls from the endline survey (caste, literacy, age terciles, and the primary client's gender); ε_{ib} is an identically distributed disturbance assumed to be independently distributed across villages, but correlated among members from the same village; and T_{ib} is a binary variable equal to 1 if individual i was from a village that was randomly selected as part of the treatment arm. This base specification will allow for us to estimate the average effect of the program in treatment villages. These ITT effects will be most relevant for outcomes related to financial market development, including overall credit uptake, the distribution of credit sources (formal vs. informal), insurance uptake and renewal, and, importantly, whether borrowers faced difficulty in repaying loans. We also use this equation to estimate the programmatic effects on (women's) empowerment.

In addition to this base specification, we will be interested in examining heterogeneous effects by (i) gender and (ii) baseline credit rationing status.⁸ To do so, we will modify equation (1) and estimate one of the following two regressions:

$$Y_{ib,1} = \alpha + \beta Y_{ib,0} + \gamma Female_{ib} + \delta_1 T_{ib} + \delta_2 T_{ib} \times Female_{ib} + \sum_{j=1}^J \theta_j \mathbf{x}_{ijb,0} + \varepsilon_{ib} \quad (2)$$

and

$$Y_{ib,1} = \alpha + \beta Y_{ib,0} + \gamma_1 QR_{ib} + \gamma_2 RR_{ib} + \delta_1 T_{ib} + \delta_2 T_{ib} \times QR_{ib} + \delta_3 T_{ib} \times RR_{ib} + \sum_{j=1}^J \theta_j \mathbf{x}_{ijb,0} + \varepsilon_{ib} \quad (3)$$

where, in equation (2), $Female_{ib}$ is a binary variable equal to 1 if individual i from village b was a female (and 0 otherwise), and in equation (3), QR_{ib} and RR_{ib} are binary variables equal to 1 if individual i from village b was quantity or risk rationed, respectively, at the time of project baseline (with unrationed individuals as the omitted category). In equation (2), $Female_{ib}$ is interacted with T_{ib} to test whether the program had differential effects on male (δ_1) and female respondents ($\delta_1 + \delta_2$) within the treatment group. In equation (3),

⁸In our registered pre-analysis plan, we did not specify that we would explore heterogeneous impacts on the basis of baseline credit rationing status. This was a dimension of heterogeneity that was subsequently included based on conversations with participants in a research seminar given by one of the authors.

QR_{ib} and RR_{ib} are each interacted with T_{ib} , allowing us to test whether the program had differential effects on unrationed (δ_1), quantity rationed ($\delta_1 + \delta_2$), or risk rationed individuals ($\delta_1 + \delta_3$) within the treatment group.

For outcome measures related to women’s mental and emotional health, we only have data on female respondents since these questions were only asked of female respondents. Further, these outcome measures were specified differently at the time of baseline and endline data collection, so there are no comparable baseline measures for these outcomes to be included in an ANCOVA regression. For these outcome variables, we therefore modify the regression equation by dropping $Y_{ib,0}$, reducing the estimator to a single-difference comparison of simple (conditional) differences between treated and control samples. However, as a unique feature of our data set, we have data not only on the client, but also on another household member who, in the case of households with KhetScore loans, would have co-signed on this loan. Consequently, we can still estimate effects separately for women in households where the primary client is male (meaning that the female is the “other household member”) versus female (so that the female is the “client”) and estimate a variation of equation (2):

$$Y_{ib,1} = \alpha + \gamma Other_{ib} + \delta_1 T_{ib} + \delta_2 T_{ib} \times Other_{ib} + \sum_{j=1}^J \theta_j \mathbf{x}_{ib,j,0} + \varepsilon_{ib} \quad (4)$$

where all terms are as they were introduced before, though specifically pertaining to female respondents, and $Other_{ib}$ is a binary indicator equal to 1 if the respondent was classified as the “other household member” – implying the female respondent was not a DER client, but rather a female member within the household of a male client (typically his spouse, and a co-signer on the loan). So, the ITT effect among DER’s female clients is captured by δ_1 , and the spillover effects from DER’s male client to his female household member are captured by $\delta_1 + \delta_2$.

Equations (1) - (3) provide estimates of overall impacts of offering loans and insurance based on the KhetScore credit-scoring methodology among farm households in randomly selected treatment villages, regardless of whether a household took up the product. Given some farmers opted not to apply for a KhetScore loan and others’ applications were rejected, and assuming a positive correlation between take-up and the respective outcomes of interest, these ITT effects provide a conservative estimate of the overall program impacts. Compared to the effects on just those households who took up KhetScore loans, such estimates of the overall program impacts in treatment villages may be of greater interest to policymakers interested in the economywide impacts of expanding credit market access, rather than just the effects on those households who took up KhetScore loans. Consequently, for many of the outcomes we assess, we focus on ITT effects. In

other cases, such as for outcomes related to agricultural performance, we also estimate impacts for those households in the sample that decided to take up these products if their applications were approved. We estimated local average treatment effects, or LATEs (Imbens and Angrist, 1994) via instrumental variables (IV) estimation by instrumenting for product take-up with random assignment to the treatment group.⁹ For estimating heterogeneous treatment effects (i.e., among women clients or those that were credit rationed at baseline), we instrument for the interaction between gender or credit rationing status and product take-up with an interaction between gender or credit rationing and random assignment to the treatment group.

For models in which the outcome variable is binary (e.g., cultivating during either the Kharif or Rabi season), a further complication arises in the case of the linear IV estimator for the LATE, namely that predicted probabilities from a linear probability model can be outside the unit interval (0,1), which can be especially pervasive in IV estimations with comparatively low compliance among the treatment group. To circumvent these challenges in estimating LATEs, we implement a recursive bivariate probit estimator, which is appropriate for estimation scenarios in which (1) the outcome variable is binary, (2) the endogenous treatment is binary, and (3) the instrument is binary. Based on recursive bivariate probit estimates, we are able to construct LATEs using the formula presented in equation (11) in Chiburis et al. (2011). To consider heterogeneous LATEs for these binary outcomes (i.e., by gender or by baseline credit rationing status), we estimate recursive bivariate probit models separately for the different subgroups and compute point estimates for LATEs for each subgroup, with corresponding 95 percent confidence intervals for these LATEs constructed using the parametric simulation procedure proposed by Krinsky and Robb (1986).

Regardless of whether the outcome variables in these LATE regressions are continuous or binary, the fact that exposure to the treatment was strictly limited to the members of the randomly assigned treatment villages ensures that there are no “always-takers”; i.e., there were no farmers from control villages who availed KhetScore loans. As a consequence, the LATE estimates that we report can also be interpreted as the average treatment effects among the treated (ATT).

⁹If product take up is relatively low (indicating a non-trivial degree of non-compliance with assignment), the resulting LATE could over-state treatment effects among the treated. Consequently, LATE estimates based on IV regressions can be considered as upper bound estimates of average treatment effects. In the case of a single endogenous variable (product take-up) and a single excluded instrument (random assignment), the F -statistic on the first stage regression is 44.09, so our identification strategy does not suffer from a weak instrument.

3 Descriptive statistics

3.1 Summary statistics

Table 1 provides summary statistics of 1706 DER clients surveyed at baseline. About 70 percent of clients are female and an average respondent is around 42 years of age. About 85 percent know how to read and write, with 75 percent having completed (at most) primary schooling and the remaining 25 percent having completed secondary schooling. Social stratification through the caste system is still quite prevalent in India, despite legislation barring outright discrimination on the basis of caste. In our sample, about 36 percent of clients belong to general castes, with another 36 percent belonging to Scheduled Castes (SC), 18 percent belonging to Other Backward Classes (OBC), and 10 belonging to Scheduled Tribes (ST).¹⁰ There are statistically significant differences in the proportions of general caste and SC between the treatment and control groups, with the treatment group consisting of a higher proportion of general caste than the control group (42 percent to 31 percent, respectively), and the control group consisting of a higher proportion of SC (43 percent to 29 percent, respectively). More than half of households' annual income fall below INR 90,000 (roughly USD 1210) per year, or INR 247 (about USD 3) per day, which puts a 2-member household below the poverty line of USD 2 per person per day. About 30 percent have an annual income between INR 90,000 and INR 120,000 (about USD 1614), while only around 20 percent have an annual income of more than INR 120,000 (USD 1,613).

In terms of clients' experience with credit and crop insurance, about half of the clients in the sample reported having purchased on credit in the past 12 months, though there is a higher proportion of clients from treatment villages that reported purchasing goods on credit (56 percent compared to 42 percent). Around 30 percent have borrowed money over the same period, with an average desired loan size of $\text{INR } 3.762 \times 10^4$ (about USD 506). In contrast, insurance take-up rate is extremely low — only 3 percent have *ever* had crop insurance in the past, only 1 percent *currently* have crop insurance, and fewer than NA percent report having received a pay-out from such crop insurance.

The vast majority of clients (86 percent) cultivated in the most recent Kharif season, while a smaller proportion (75 percent) cultivated in the most recent Rabi season. A significantly higher proportion of clients in the control villages cultivated during the most recent Rabi season compared with clients in the treatment

¹⁰In India, these caste differentiations qualify or disqualify groups for participation in the "reservation system", a form of affirmative action that reserves a certain number (or proportion) of opportunities in education, employment (especially civil service employment), government programs, etc., for members of historically disadvantaged groups. The term "general caste" is a blanket term referring to castes that do not qualify for the reservation system due to not being deemed disadvantaged (at least on the basis of caste). SCs and STs are considered the most economically disadvantaged social groups in India, and were specifically defined in the Constitution of India in an order instituted in 1950. OBC is another collective term referring to castes that are "backward" relative to general castes, but who are not listed among the SCs or STs.

villages (82 percent vs. 67 percent). An average household cultivates about 2 acres of land, with 75 percent of households cultivating fewer than 2.5 acres of land (marginal), 23 percent cultivating between 2.5-5 acres of land (small), and only 2 percent cultivating more than 5 acres of land (with a maximum of 10). Virtually all clients (99 percent) are paddy rice farmers and 26 percent reported having experienced crop damages and loss. An average household reports making a total agricultural revenue of INR 22,940 (roughly USD 309) per acre, incurring a total cost of about INR 13,777 (USD 185) per acre (excluding the implicit cost of own labor), and earning profit of INR 9,164 (USD 123) per acre (again, excluding the cost of own labor). Despite having agricultural revenues and costs statistically similar to those from control villages, households from treatment villages earned greater agricultural profits than households in the control villages, by a margin of INR 10,891 (USD 146) to INR 7,587 (USD 102). We control for these baseline differences in all regressions discussed in section 4 below.

Panel D of Table 1 and Figure 2 summarize the distribution of DER clients' credit rationing status at baseline. Across both treatment and control arms, the plurality of clients responded to survey questions in such a way that they were classified as being quantity rationed (48 percent of clients in the control group and 54 percent in the treatment group), having faced supply-side constraints in accessing their desired level of credit. They could potentially benefit from the credit + insurance bundle to the extent that the KhetScore credit scoring approach circumvents some of the other, more traditional means by which individuals could qualify for loans (e.g., credit reports with formal credit bureaus or documentation of land rights), or to the extent that the accompanying insurance component eases lenders' concerns about borrowers' potential to repay loans, especially in the event of near-catastrophic crop losses that might otherwise impinge upon their repayment ability. Roughly another 30 percent of clients across both treatment and control were classified as being risk rationed, opting out of participating in the credit market because of lenders transferring contract risk onto potential borrowers in the form of higher interest rates, fees, or collateral burdens. The bundled insurance component may benefit them by absorbing some of that contract risk and ameliorating some of these other less attractive terms. Finally, 24 percent of farmers in the control group and 16 percent of farmers in the treatment group were neither quantity nor risk rationed at the time of project baseline, and consequently are identified as unconstrained.

In Table 2, we report summary characteristics of DER clients that were classified as quantity rationed, risk rationed, and unconstrained at the time of project baseline, with a series of *t*-statistics for the differences in means between households that were deemed quantity- or risk-rationed and those that were deemed unconstrained. As seen in columns (4) and (5), there are a large number of statistically significant differences in the group means, and largely consistent with what we might expect. One dimension that is surprising,

however, is that there is a statistically significant difference in the proportion of DER’s unconstrained clients who were female compared to the quantity rationed clients who were female. The proportion of unconstrained clients who were female was roughly 80 percent, compared to a rate of about NaN percent female among quantity rationed clients. A priori, we could reasonably expect female clients to be more likely to report credit constraints, since they typically lack formal credit scores, lack documented land rights for the land they would cultivate with additional liquidity, and have limited personal ownership of assets that could be used as collateral to secure an agricultural loan. It is surprising, therefore, that such a high proportion of the *unconstrained* sub-population would be female. Part of this is likely due to the way in which we characterize clients as being credit constrained or not. In particular, clients were classified as unconstrained if they responded that they did not need to borrow on credit in the last 12 months. While normally this would signal sufficient personal liquidity, it could also signal that the client simply did not have any financial ventures to pursue that would require additional capital. It could reasonably be surmised that many women in rural Odisha might fall into this latter category, and yet would be classified as unconstrained in credit access, simply because their credit access was not tested in the preceding 12 months. Unfortunately, we are unable to distinguish between these two potential reasons for not needing to borrow on credit in the past year.

More consistent with what we would expect, there are also differences in the proportions of clients completing primary and secondary schools, with quantity rationed and risk rationed clients having lower proportions completing primary school and secondary school compared to the unconstrained clients. Unconstrained clients are also less likely to be classified as low income (below INR 90,000 per year) and more likely to be classified as high income (more than INR 120,000 per year) than quantity- or risk-rationed clients. Unconstrained clients also cultivate more area (2.3 acres) than quantity rationed (NaN acres) or risk rationed clients (1.9 acres). While we are not attempting to ascribe causality with regards to credit rationing, it is not unreasonable to at least envision some causal mechanisms whereby some of these factors might contribute to credit constraints.

Overall, variables are fairly well balanced across treatment and control groups, with only a few exceptions. Imbalanced variables include the following: the percentage of DER clients that belong to the General versus Scheduled Castes, the percentage of clients purchasing goods and services on credit in the past 12 months, the percentage of farmers that cultivated in the most recent Rabi season, agricultural profits per acre during the most recent Kharif season, and the percentage of clients that are not credit constrained. While we might reasonably expect some differences between treatment and control groups to emerge by chance, the fact that there are six dimensions for which there are statistically significant differences between the treatment and

control groups suggests that we do not have ideally comparable groups with which to identify treatment effects. This is confirmed by a joint orthogonality test in which we regress the binary treatment assignment indicator on each of these variables (dropping categorical variables where necessary to avoid perfect multicollinearity). The corresponding F test statistic associated with the null hypothesis that all coefficients in this regression are simultaneously equal to zero is 3.712, leading to a rejection of this null hypothesis. Consequently, we control for these experimental imbalances in our impact evaluation estimates in section 4 below.¹¹

3.2 Uptake of the intervention

Table 2 reports monitoring and evaluation (M&E) data concerning the uptake of the intervention. Preceding each season, enrollment initiatives occurred one to two months in advance. Dedicated personnel from DER personally contacted all eligible farmers in each treatment village to encourage them to apply for the KhetScore product. Column (1) indicates, per season, the number of farmers who applied for KhetScore loans. Column (2) denotes the count of farmers eligible for KhetScore and who had the loan disbursed to their bank accounts. Column (3) represents the number of farmers enrolled in insurance during the respective seasons (regardless of loan uptake), and column (4) indicates the count of farmers who received payouts for those seasons.

On average, approximately 28% of farmers offered KhetScore loans applied for it each season. Initially, uptake was subdued due to various challenges. COVID-19 lockdowns necessitated DER staff to contact farmers via phone, leading to issues concerning farmer availability, low mobile penetration in Odisha, incorrect contact information, network connectivity problems, and, perhaps most importantly, establishing and maintaining rapport with potential clients. Over time, these challenges were ameliorated to some degree by transitioning to in-person visits, facilitating continued engagement with farmers over multiple seasons. Additionally, financing KhetScore loans during the pandemic was challenging as institutional lenders were cautious about microloans. However, DER demonstrated the viability of its lending model using the relatively small number of initial KhetScore loans, securing additional funding for subsequent seasons. Consequently, there was a progressive increase in the number of farmers being invited to apply for, applying for, receiving KhetScore loans, and enrolling in the accompanying insurance product.

¹¹Some observations were missing for a few of these variables that demonstrated statistical imbalances between experimental groups at baseline (specifically, whether a DER client had purchased on credit in the preceding 12 months, whether a DER client had cultivated during the most recent Rabi season, and agricultural profits per acre during the previous Kharif season). We imputed values for these missing observations using the village median value. In such cases, we also control for these imputations with a series of binary variable indicating that these observations had been imputed. In addition to these variables, we also include control variables to eliminate potential confounding factors and to increase the precision of our econometric estimates.

4 Empirical Results

4.1 Impacts of KhetScore loans

As mentioned previously, we are interested in measuring the effects of KhetScore loans on several important outcomes pertaining to financial market development, enhancing agricultural livelihoods, gender equity and increasing women’s empowerment, and perceived stress. Within each of these outcome domains, we consider several different areas in which we test whether – and the extent to which – KhetScore loans have yielded impacts. There are therefore several hypotheses that are being tested within each outcome domain, with a nontrivial likelihood that the p -values for these different hypotheses tests could be correlated, which in turn could result in a high likelihood of Type I error (false rejection of the null hypothesis). In the sub-sections that follow, in addition to reporting naïve (unadjusted) p -values for the estimated treatment effects (which treat each test as an independent test), we also report sharpened q -values (Benjamini and Hochberg, 1995; Benjamini et al., 2006; Anderson, 2008), which are essentially adjusted p -values that account for the False Discovery Rate (FDR) within each of the different outcome domains. Specifically, we use the two-step procedure initially proposed by Benjamini et al. (2006) and described by Anderson (2008). This two-step procedure detects whether a null hypothesis can be rejected at some significance level q , but alone it does not report the smallest level q at which a null hypothesis would be rejected, which arguably is of greatest practical importance. We follow the suggestion in Anderson (2008) and undertake this procedure for all possible q levels between 1.000 and 0.001 (in downward increments of 0.001), recording the q -value at which the null hypothesis ceases to be rejected. This approach is more forgiving and yields greater statistical power than correcting for the family-wise error rate (e.g., Romano and Wolf, 2005; List et al., 2019), which limits the probability of a family of hypotheses being subject to *any* Type I errors.¹²

4.2 Impacts on credit market development

We first examine the impacts of the program on credit uptake (Table 4) and report ITT effects of random assignment to KhetScore treatment group. We focus on ITT effects rather ATT effects because policymakers

¹²Our pre-analysis plan specified that we would use the Bonferroni correction, which would either adjust the significance level (i.e., the permissible probability of Type I error, α) by dividing it by the number of tests being conducted, or by adjusting the p -value associated with a given test by multiplying it by the number of tests being conducted. Since some of our outcome domains involve a large number of tests (multiple different outcomes, heterogeneity by gender and baseline credit rationing status, ITT vs. LATE), we determined that the application of the Bonferroni correction would severely deplete statistical power and increase the probability of Type II errors to an unacceptable degree. For example, when testing for treatment effects on our agricultural outcomes, we test for two types of treatment effects (ITT and LATE) on six specific outcomes (whether they cultivated in a particular season, area cultivated, revenue, cost of cultivation, profits, return on investment) across two seasons (kharif and rabi), among the full sample, disaggregated by gender (male vs. female), and disaggregated by baseline credit rationing status (unrationed vs. quantity rationed vs. risk rationed). This results in a "family" of $2 \times 6 \times 2 \times (1 + 2 + 3) = 144$ hypotheses being tested. With $\alpha = 0.1$ – the most lenient conventional level of statistical significance – we would require p -values of 0.0007 to reject the null hypothesis of no treatment effect, which would almost certainly lead to the conclusion of a large number of false null effects.

are likely to be most interested in the the overall effects of such a program on credit market development in rural communities on average.

Starting with the overall impact of the KhetScore program among the full sample (Figure 3 and Panel A of Table 4), although there is limited evidence that the program had an overall effect on increasing credit uptake, there is compelling evidence that the KhetScore credit + insurance bundle is effective in encouraging borrowing from formal sources and deterring borrowing from informal sources, which are typically associated with higher interest rates, less favorable loan terms, less transparency, and less regulatory oversight.¹³ In particular, there was a 21 percentage point increase in formal borrowing and a corresponding 20 percentage point reduction in informal borrowing, suggesting that on average clients in treatment villages effectively substituted formal for informal borrowing. Formal borrowing is typically much preferred to informal borrowing, since the former is typically associated with lower interest rates, more favorable terms, and because formal lenders are registered with government regulatory authorities (the Reserve Bank of India, in this case), and consequently are expected to operate under the rules and regulations of the regulators. The latter, on the other hand, although perhaps filling a gap in rural credit markets, have a history of near-usurious interest rates and may be less lenient with delinquencies. The KhetScore bundle also appears to come with more favorable terms, as evidenced by the sizable reduction in the proportion of farmers in treatment villages who indicated that they faced difficulties in repaying loans. Of note, only about 20 percent of borrowers in treatment villages reported facing difficulty in repaying loans, compared with 76 percent of borrowers in control villages. Note that this question was only asked among DER clients who indicated that they had borrowed or purchased goods on credit over the preceding 12-month period. The net result is a 56 percentage point reduction in the proportion of borrowers who reported difficulty in repaying loans. Part of this effect is certainly due to the lower interest rate associated with KhetScore loans (14 percent compared to the prevailing microcredit interest rate of 24 percent), but it certainly seems plausible that much of this could also be attributed to the fact that these KhetScore loans do not require collateral, unlike most other types of loans. Additionally, there is some anecdotal evidence that farmers liked the repayment schedule for KhetScore loans, with loan repayments in periodic installments rather than in a single lump sum (which is often the case for agricultural loans) or in weekly installments (which often applies to microfinance loans), though additional anecdotal evidence suggests some borrowers may have had to work off-farm to enable them to be able to repay the first two or three installments, which were due during the period before they had harvested

¹³It is worth noting that while the p -value associated with the treatment effect on taking up credit from any source (column (1)) is 0.53, the sharpened q -value is only 0.13, close to an acceptable probability of Type I error. Sharpened q -values *can* actually be smaller than naïve (unadjusted) p -values in cases in which many hypotheses within a family are rejected, since it is likely that many of these rejections will be true rejections, and it is possible to tolerate several false rejections while still maintaining a low FDR (McKenzie, 2020).

their crops. This did not, evidently, have a detrimental impact on how they perceived their abilities to repay their loans.

In Figure 4 and Panel B of Table 4, we examine heterogeneous effects among male and female clients. Among male clients, there is a 26 percentage point reduction in borrowing from informal sources, with a 17 increase in borrowing from formal sources, revealing that many male clients swapped out informal loans for formal loans. The uptick in formal borrowing was not quite sufficient to offset the reduction in informal borrowing, so we actually observe a small 8 percentage point reduction in overall credit takeup among male clients ($p = 0.1$; sharpened- $q = 0.03$). Among female clients, on the other hand, there is compelling evidence that the program crowded-in borrowing.¹⁴ In particular, we find evidence of a 26 percentage point increase in formal borrowing among women clients, well exceeding the 10 percentage point reduction in informal borrowing, resulting in a net increase in borrowing among women of roughly 16 percentage points. As envisioned, these KhetScore loans appear to be particularly beneficial in expanding credit access among women who are typically implicitly – if not explicitly – excluded from formal credit markets due to “thin” credit files and lack of documented land rights.

In Figure 5 and Panel C of Table 4, we explore heterogeneity in program impacts among clients based on their baseline credit rationing status. We find evidence that the program led to small and statistically significant increases in credit takeup among unconstrained clients (a 6 percentage point increase; $p = 0.23$, sharpened $q = 0.06$) and small and marginally significant increases in overall credit uptake among quantity rationed (total effect size of 2.5 percentage points; $p = 0.54$, sharpened $q = 0.13$) or risk rationed (total effect size of 3.2 percentage points; $p = 0.45$, sharpened $q = 0.11$) farmers. We generally find that the program facilitated a substitution of formal for informal credit among unconstrained, quantity rationed, *and* risk rationed members of treatment villages, regardless of credit rationing status. In particular, we observe a 24 percentage point increase in formal borrowing among unconstrained farmers, a 20 percentage point increase in formal borrowing among quantity rationed farmers, and a 21 percentage point increase in formal borrowing among risk rationed farmers. At the same time, we observe a 18 percentage point reduction in informal borrowing among unconstrained farmers, a 18 percentage point reduction in informal borrowing among quantity rationed farmers, and a 21 percentage point reduction in formal borrowing among risk rationed farmers. Consequently, although quantity and risk rationed borrowers may not *disproportionately* benefit from the KhetScore credit scoring approach, the results clearly suggest that they benefit *roughly as much*

¹⁴As noted in the footnote to Table 4, the row labeled "Total effect | Female" represents the linear combination of regression coefficients (reported in supplementary appendix A), Table A. Referring back to equation (2), this total effect is reflected by the linear combination $\hat{\delta}_1 + \hat{\delta}_2$, and the standard error for this linear combination computed as $\hat{\sigma}_{\hat{\delta}_1 + \hat{\delta}_2} = \sqrt{\hat{\sigma}_{\hat{\delta}_1}^2 + \hat{\sigma}_{\hat{\delta}_2}^2 + 2\hat{\sigma}_{\hat{\delta}_1, \hat{\delta}_2}}$. Test statistics and standard errors for these linear combinations of regression coefficients were computed using the `-glht-` (general linear hypothesis test) function from the `-multcomp-` package in R. The p -values from these tests have also undergone the aforementioned adjustment to account for the FDR, reported in the line titled "Sharpened q -value".

as those individuals who were *not* credit rationed – and who presumably would have had a more secure financial standing – at the time of the project baseline. Both quantity rationed and risk rationed individuals from treatment villages reported facing difficulty in repaying their loans with a much lower frequency than members of the control group (column 4), and despite the point estimate on this effect being greater among those individuals who were not credit constrained at baseline, the effects are not statistically distinguishable from one another.

4.3 Impacts on insurance market development

Figures 6-8 and Table 5 presents the results on the impacts of the program on outcomes pertaining to insurance, including insurance uptake, insurance renewal, and financial literacy (i.e., understanding of the terms and conditions of crop insurance measured via an insurance knowledge score). Although farmers who took KhetScore loans universally received insurance coverage (since it was bundled with their loan), there is also the possibility that farmers in randomly selected treatment villages – through product training and increased familiarity with how crop insurance operates – could have purchased crop insurance coverage even if they did not specifically take a KhetScore loan (e.g., under the PMFBY crop insurance scheme). Consequently, here, too, we focus on ITT effects rather than ATT effects, for exactly the same reason as elucidated above; namely that these program-wide effects are likely to be of greater policy interest than the effects among those who took up KhetScore loans.

In short, we find overall positive and statistically significant effects of the program on insurance outcomes. Random assignment of villages to the treatment increased the likelihood of clients taking up insurance at least once during the study period by more than 60 percentage points, and the treatment effect does not vary across gender nor baseline credit rationing status. The program also increased the likelihood that once-insured farmers would renew insurance coverage by nearly 30 percentage points (Figure 6 or Panel A of Table 5), though this effect is found to be mainly driven by women (Figure 7 or Panel B of Table 5), whose insurance renewals increased by nearly 60 percentage points, compared an increase of only 7 percentage points among men ($p = 0.38$; sharpened $q = 0.07$).

Lastly, we see that random assignment to the treatment group increased the number of correct responses to a series of questions meant to test respondent’s awareness of the features and nuances of crop insurance, which we use as a proxy for financial literacy (Figure 6 or Panel A of Table 5). In total, random assignment increased the number of correct responses by about 0.5 questions. Although on average this effect is similar between men and women (Figure 7 or Panel B of Table 5), there is much greater variation in the treatment effect among women ($p = 0.21$; sharpened $q = 0.05$). We do not observe significant differences in treatment

effects on insurance outcomes among participants with different rationing status at baseline (Figure 8 or Panel C of Table 5), which further suggests that the KhetScore product does not implicitly discriminate against farmers who faced supply- or demand-side credit constraints at baseline.

4.4 Impacts on agricultural outcomes

In Table 6 and Table 7, we present results on ITT and LATE estimates of the KhetScore program on a series of agricultural outcomes during Kharif (monsoon) and Rabi (winter) seasons, respectively. Figures 9 - 14 illustrate LATEs for the two seasons, among the full sample (Figures 9 and 12), disaggregated by gender (Figures 10 and 13), and based on baseline credit rationing status (Figures 11 and 14). In the case of these agricultural outcomes, both ITT effects and LATEs are likely to be of particular interest to policymakers. LATE results are qualitatively similar to ITT estimates, but, as would generally be expected since they reflect treatment effects among KhetScore loan recipients, LATEs are of considerably larger magnitude, suggesting that the beneficial effects of the KhetScore credit + insurance bundle are primarily experienced among those who decided to take up the loans. Here we mainly discuss the LATE results, which are also ATTs since access to KhetScore loans was restricted to farmers from randomly selected treatment villages, so there is no noncompliance among the control group (i.e., there are no “always takers”).

Starting with the Kharif season in Table 6, we find that farmers that took out KhetScore loans were more likely to cultivate during the Kharif season, despite an already high proportion of farmers cultivating during Kharif. Despite the increase in the likelihood of cultivation, we do not find any evidence that farmers that took out KhetScore loans expanded their area cultivated during Kharif. Perhaps most importantly from farmers’ perspective, we find that agricultural profits per acre increased substantially among those farmers that took out KhetScore loans (on average, by about INR 9,100), or about USD 120 at prevailing exchange rates at the time, and the effect was virtually the same between men and women (INR 9,300), though there was much greater variation in the effect among men. This increase in agricultural profits was primarily driven by an increase in revenues per acre, which increased by nearly INR 8,500 (about USD 110) compared to revenues in the control group. The effects on revenues per acre are more pronounced among male clients, with male clients observing an increase in revenues of INR 10,900 per acre (USD 142), compared with a increase of only INR 6,500 per acre (USD 84.5) among women clients. There is little evidence that farmers who took out KhetScore loans saw an increase in total costs of production, so it seems likely that farmers must have been able to increase crop revenues in a cost-neutral fashion.

KhetScore loans also appear to have had beneficial impacts among those who were previously quantity (supply-side) rationed or unrationed in their access to credit, as revenues per acre increased by approximately

INR 17,400 and INR 8,700 per acre, respectively (roughly USD 221 and USD 117, respectively). The positive effect on agricultural profits per acre is experienced by farmers regardless of their baseline credit rationing status, though farmers who were unconstrained at baseline experienced the largest boost in agricultural profits (of about INR 17,000 or USD 221 per acre), followed by quantity rationed (INR 9,000 or USD 117 per acre) and risk rationed (INR 7,400 or USD 91 per acre).

Turning to the Rabi season in Table 7, we observe that households that took out KhetScore loans were about 22 percent [95 percent CI = (0.06,0.34)] more likely to cultivate paddy rice during the Rabi season (Panel A, column 2), and households that took out KhetScore loans increased their Rabi paddy rice area by about 1.3 acres, more than double the acreage cultivated among control group farmers during the Rabi season (Panel A, column 4). The expansion of land cultivated during the Rabi season was considerably higher for women (expansion of 1.62 acres) than for men (1.05 acres). There is also evidence that the expansion in Rabi cultivation along the extensive margin was more pronounced among farmers who had previously been credit constrained: previously quantity and risk rationed farmers increased their Rabi area by more than 1.53 and 1.06 acres, respectively, compared to an increase of 0.79 acres among those farmers who had not been credit constrained at the time of project baseline. The total cost of production per acre fell by about INR 9,600 per acre (about USD 130) among farmers in the treatment group who took out KhetScore loans, perhaps as treatment farmers generated economies of scale (Panel A, column 8), and the reduction in costs of production were considerably larger among women borrowers than among men decrease than men (Panel B, column 8). We also observe households that took out KhetScore loans earning significantly higher agricultural profits, though the increases in agricultural profits are larger among male farmers and those farmers that had not been credit constrained at baseline. All told, results in Panels B and C imply that this product could make credit more accessible and inclusive, and could help alleviate both supply-side and demand-side constraints to borrowing that could enhance agricultural livelihoods.

How do we explain these observed impacts on agricultural revenues, costs, and profits?¹⁵ In tables 8 and 9, we report treatment effects on various agricultural input expenditures, including seeds, fertilizer, irrigation, machinery, land rentals, and hired labor (disaggregated by gender) for the Kharif and Rabi seasons, respectively, while Tables 10 and 11 report treatment effects on household (unpaid) labor (including own-labor and labor from other male and female household members) for Kharif and Rabi, respectively. The treatment effects estimates are not measured very precisely, perhaps due to very heterogeneous investment behaviors among borrowers, but there is rather compelling evidence that one of the important pathways by which borrowers were able to increase their profits across both seasons is through investing in new seeds. Although

¹⁵In our pre-analysis plan, we did not indicate that we would analyze treatment effects on input use. This analysis was undertaken in an attempt to better elucidate some of the outcomes discussed earlier.

harvested grains can be stored and used for seeds in subsequent seasons, seed viability and the nutritional value of harvested grains tend to decline continually with extended storage time, and declining seed viability tends to be accompanied by lower germination rates and lower yields. Consequently, occasionally purchasing new seeds remains important for ensuring the use of the highest quality and most productive germplasm. In our study, we find that farmers taking KhetScore loans spent an additional INR 1,430 on rice seeds for the Kharif season and an additional INR 1,140 on rice seeds for the Rabi season, or about USD 19 and USD 15, respectively.¹⁶ While these amounts may not seem substantial at face value, they represent increases of 67 percent and 61 percent over seed investments in the control group during Kharif and Rabi seasons, respectively. In considering the gender disaggregation of these effects on seed expenditures, it seems much of the overall effect can be attributable to increased seed purchases by male farmers, with scant evidence of increased expenditures among women farmers.

In Table 10, we find evidence that male borrowers significantly reduce their allocation of labor to agricultural production during the Kharif season, and apparently substitute for their own labor by hiring in female workers. On average, men in the control group allocated about 58.7 days to providing labor for agricultural production on their own farms, yet this amount was effectively eliminated once they took out a KhetScore loan. Instead, they increased their spending on hiring female agricultural labor by INR 5,050, which, at an average daily wage of about INR 240 per day, corresponds to an additional 21 days of farm labor. During the Rabi season, male borrowers essentially maintain their same labor allocation (Table 11, Panel B, column 1), reduce labor supplied by other family members (Table 11, Panel B, columns 2 and 3) and reduce their reliance on hired laborers (Table 9, Panel B, columns 6 and 7), opting instead to supplement this reduced labor supply with machinery (9, Panel B, column 4).¹⁷ We observe male borrowers increasing expenditures on agricultural machinery by INR 3,220, roughly doubling the expenditures on machinery during the Rabi season among farmers in the control group. Female farmers, on the other hand, increase their own labor supply during the Rabi season by about 120 percent relative to the control group, nearly double the amount of labor supplied by other household members, and more than double their expenditures on hiring male and female laborers, respectively. These clear differences in agricultural labor use between men and women is consistent with observed disparities in challenges faced by men and women farmers in accessing agricultural labor. As Adhikari et al. (2023) observes, women face unique challenges in regards to agricultural labor, with more than 50 percent of women in their sample reporting that they faced challenges in hiring labor (compared to only 14 percent among men in their sample), 25 percent reporting that they had to pay extra for labor (compared to

¹⁶For the Rabi season effect on seed expenditures, the sharpened q -value is 0.25 compared to a naïve p -value of 0.09, so we conservatively fail to reject the null hypothesis of no effect on Rabi season seed expenditures

¹⁷We do not have data on Rabi season input expenditures or labor for the 2020-21 Rabi season. Consequently, these regressions do not control for baseline levels of input use or labor.

only 8 percent among men), and 30 percent reporting that laborers did not respect them (compared to only 8 percent among men). As a consequence of these challenges, women are often delayed in crop establishment, which may push the crop growth cycle into periods of the year that are less hospitable. Women farmers were about 26 percent more likely to cultivate during the Rabi season and increased their cultivated area by 1.62 acres, so there is clearly a need for additional labor for land preparation, transplanting, weeding, and harvesting activities. Although female farmers cut their expenditures on fertilizers, the additional labor employed must be sufficiently productive to yield higher agricultural profits and a marginally significant increase in return on agricultural investments, though admittedly these metrics ignore the value of unpaid household labor.

4.5 Impacts on intra-household gender dynamics

In Table 12, we present results of program effects (ITT) on intra-household gender dynamics among DER clients (Panel A) and non-client family members (Panel B). We pay particular attention to several important indicators of empowerment, including whether they (1) contribute to household borrowing decisions, (2) contribute to household decisions about how to spend borrowed money, (3) give input on household livelihood decisions, (4) exercise control over uses of income, and (5) own land and/or other assets. We find sizable and statistically significant improvements in these five empowerment indicators among households in treatment villages, primarily among women. Indeed, we find virtually no evidence that men reported an increase in empowerment, though this is almost certainly because men in the sample villages are all sufficiently empowered. Interestingly, there is considerable heterogeneity in whether the significant effects are driven by female DER clients or non-client female household members. Among female DER clients, random assignment to treatment improves their ability to contribute to household financial decisions, specifically household decisions around borrowing and the use of borrowed funds. Female DER clients were about 30 percentage points more likely to contribute to household decisions about whether to borrow, and about and about 25 percentage points more likely to contribute to household decisions about how to use borrowed funds. Other female household members (i.e., female partners of male clients who are co-signers on loans) reported improvements in their ability to provide input on household livelihood decisions, control over household income, and own land and/or other assets (by 38, 38, and 35 percentage points, respectively). In general, women still trail far behind their male counterparts when it comes to household decision making power, especially when it comes to household financial decisions. In the control group, only about 10 percent of women reported that they contributed to household decisions about whether to borrow or how to spend borrowed funds. Consequently, although there is still much progress to be made toward truly achieving

gender parity, our results provide strong evidence for the effectiveness of financial inclusion (perhaps not limited to KhetScore loans) in empowering women and improving intra-household gender relations. As more women are able to avail credit through inclusive credit scoring approaches and make profitable investments in agriculture or other enterprises, they may be more widely recognized for making valuable contributions to the household’s financial situation, and thus they may be given more agency in important household decisions.

4.6 Impacts on women’s perceived stress

Finally, we consider the effects of KhetScore loans on women’s mental and emotional health. Women in rural settings frequently bear substantial risks in household production, even when they may not be directly involved in agriculture. According to the “conjugal contract” model (Carter, 1997), men and women operate as separate spheres in the household, with both aiming to maximize individual utility subject to separate budget constraints. Men typically manage agricultural production, and the conjugal contract establishes the rules by which men transfer resources to women to enable them to operate non-farm enterprises and manage the household, including childcare. Because the agricultural sphere is subject to various production risks (including, but not limited to climate or environmental conditions), there is a risk of men defaulting on the conjugal contract, often leading women to liquidate their personal assets or reduce their consumption to enable them to continue to satisfy household needs that rural society maintains are the woman’s responsibility (Arteaga et al., 2024). There is, by now, a considerable body of literature that has documented how negative agricultural shocks disproportionately impact women (see, e.g., Hoddinott and Kinsey, 2003; Quisumbing et al., 2018). This *indirect* exposure to negative agricultural shocks may be even more severe when men leverage credit to finance agricultural investments: not only is the household food supply and income stream in jeopardy, but the household may lose collateral or liquidate assets or borrow from another lender to repay borrowed funds. Consequently, bundling insurance with credit has the potential to provide peace of mind that, even in the event of a negative agricultural shock, women’s exposure is limited.

In Table 13 we report the effect of the program on several measures of perceived stress, for both the female client (Panel A) and another female household member, who is likely the co-signee on the loan if the client was a male (Panel B).¹⁸ Using a Likert scale ranging from “never” (1) to “very often” (5), we asked women to rate the frequency with which they felt (i) “unable to control the important things in life”, (ii) “confident in their ability to handle personal problems”, (iii) “confident that things were going their way”, and (iv) “difficulties were piling up so high that they could not overcome”. We evaluate responses to each of these questions individually in columns (2) - (6). For questions (ii) and (iii), responding “very often” would

¹⁸The survey questions regarding perceived stress were only directed toward women respondents.

indicate that individuals felt relative comfort, since such answers would imply they frequently felt capable of handling problems that arose or that things were generally going their way. Conversely, for questions (i) and (iv), responding “very often” would indicate that individuals perceived substantial stress, since such answers would imply they frequently felt incapable of controlling important things in their life or that challenges were becoming almost insurmountable. In column (1) of Table 13, we evaluate program effects on a composite stress score. To construct this composite stress score, answers to questions (ii) and (iii) were recoded so that higher scores indicated a higher degree of stress, and then we took the arithmetic mean of respondents’ answers to the four questions.

It is worth noting that, in the absence of treatment, other female family members of DER’s male clients (e.g., clients’ wives) reported higher levels of stress than DER’s clients on average, with composite stress scores of 2.96 compared to 2.63 among DER’s female clients, suggesting greater mental and emotional health problems among this subsample. This is consistent with the conjugal contracts model described above, with these women likely reliant on somewhat unreliable income transfers from their husbands’ agricultural activities, and thus expressing elevated levels of perceived stress. We find that the program had beneficial impacts on some aspects of women’s perceived stress, but these effects were concentrated only among these other female household members that were not directly targeted by DER. In particular, we see that the program reduced the composite perceived stress score by 0.6 points. Examining the different perceived stress indicators individually, we also find that the program had beneficial impacts among the other female household member of male DER clients in several of these different dimensions. In particular, we find that the program decreased the frequency with which women felt control over important things in their life (by 0.3 points), increased the frequency of feeling confident in their ability to handle personal problems (by 0.5 points), and decreased the frequency with which women felt that difficulties were piling up so high that they could not overcome them (1.2 points). This latter effect is especially pronounced, as the treatment effect is about 35 percent of the average score among the control group, signifying a substantial reduction in women feeling overwhelmed. Indeed, this improvement in perceived stress (frequently feeling difficulties were piling up) as a result of the KhetScore program placement is more than enough to overcome the gap in perceived stress between clients (average score of 2.70) and other household members (average score of 3.36). Among female DER clients (Panel A), we observe that the program increased the frequency of feeling confident in their ability to handle personal problems, and an increase in the frequency with which women felt that things were going their way, though neither of these effects are statistically significant at conventional levels. The program had negligible effects – both in terms of effect size and statistical significance – on clients feeling incapable of controlling things or feeling that difficulties were piling up.

4.7 Summarization of KhetScore treatment effects

In sum, the results from the program are highly encouraging. They suggest that the program has wide-ranging beneficial impacts for women, in terms of increasing access and utilization of credit, improving their ability to contribute to household decision-making, their control over income and assets, and reducing their overall levels of stress, thereby improving well-being. These latter two sets of results (those presented in Table 12 and Table 13) also highlight the value of interviewing multiple family members within beneficiary households. Had this research only focused on interviewing the household head or the client – as would typically be done – it is very likely that we would have missed out on observing some interesting – and important – intrahousehold spillovers of this credit and insurance bundle.

5 Cost and revenue analysis

To determine the costs of the intervention, we focused on the costs of disbursing loans and insurance using KhetScore relative to conventional methods for providing financial services, but holding the distribution channel constant, since DER leverages existing lending and insurance operations of financial service providers. We distinguish between the fixed costs of the research and development around KhetScore, and the marginal costs of providing the digital credit and insurance products. Marginal revenues are based on interest receipts and insurance premiums. The cost and revenue estimates are provided in Table 14.

The total one-off R&D costs associated with launching the KhetScore program were roughly USD 73,670. A nontrivial share of these start-up costs (roughly 27 percent) came in the form of a guarantee fund that was provided to DER for initial de-risking of KhetScore loans. As discussed previously, the repayment rates on these loans was quite high (83 percent), so presumably the guarantee fund would no longer be necessary (or at least could likely be reduced). The fixed cost of loan operations, including project management, staff travel, field office costs, app maintenance and communication material, are an additional USD 18,300 per season, though some of these costs (e.g., staff travel and communication material) could likely be reduced or eliminated if the program were to continue. On a per-farmer basis, additional costs include the costs of generating the KhetScore reports (USD 20), field staff costs (USD 4.86), staff transportation and allowances (USD 0.48), and costs associated with printing and stationary (USD 0.16). Total variable costs amount to INR 1,961.46 (USD 25.50).

At an interest rate of 14 percent on a loan of INR 34,000 and 2 percent insurance premium on the full sum of the loan, DER earned INR 4,760 (USD 61.88) per loan in interest and INR 680 (USD 8.84) in insurance premiums, for total marginal revenue of INR 5440 (USD 70.72). Clearly, the revenues from lending

and insurance operations are sufficient to compensate for variable costs of USD 25.50 per farmer, and at a scale of greater than 406 borrowers, revenues from insurance and lending operations are also sufficient to cover fixed costs as well. To the extent that fixed operational costs are relatively scale-neutral, and there are no other costs not captured here that could constrain growth, these findings suggest that this model has great potential for scaling up without substantial external assistance.

6 Conclusion

In this impact evaluation, we found that KhetScore loans have a wide range of significant and meaningful impacts that benefit participants in our study area. The program increased overall utilization of credit, especially among women. Much of this overall increase comes from an expansion in formal credit uptake (by about 20 percentage points on average, and by roughly 26 percent among women) and not merely from a shift from informal to formal credit (though there does appear to be a pattern of substituting formal for informal loans among men). In addition, households in the treatment group were more than 40 percentage points less likely to report facing difficulty in repaying their loans, indicating that the KhetScore credit + insurance bundle had particularly favorable terms. We find evidence of a significant increase in the uptake (60 percentage points higher than the control) and renewal (30 percentage points higher than the control) of agricultural insurance, and an overall increase in familiarity with the terms and conditions of crop insurance.

Local average treatment effect estimates for the 2022 monsoon (Kharif) season found that the treatment increased revenues and profits per acre by more than INR 8,500 (USD 110) and INR 9,100 (USD 120) respectively. During the 2021-2022 dry season (Rabi), local average treatment effects suggest that treated households increased their cultivated area by roughly 1.3 acres, while the total cost of production per acre fell by INR 9,600 (USD 130), perhaps as treatment farmers generated economies of scale. As a result, treatment households had significantly higher agricultural profits across both agricultural seasons, with Kharif profits nearly doubling those in the control group, and Rabi profits offsetting losses suffered by farmers in the control group.

We also observe important impacts on women’s empowerment and mental health. Compared to female DER clients in the control group, those in the treatment group were 30 percentage points more likely to report making contributions to household decisions regarding borrowing, and about and about 25 percentage points more likely to report making contributions to household decisions about how to use borrowed money. What’s more, we find that the program had beneficial effects on women’s empowerment that spilled over to other female household members who were not the direct recipients of the credit + insurance product.

In particular, they were about 40 percentage points more likely to report providing input into household livelihood decisions and to report having control over the use of income, and 35 percent more likely to have increased their reported asset ownership, despite not being the direct recipient of credit. We also find that the program had a beneficial effect on reducing stress levels, and while some of these effects were felt by DER clients, these effects were especially pronounced among the other female household members who co-signed the loans. These findings also underscore the value of surveying not only the (typically male) head of the household, but also other household members since the impacts of an intervention may diffuse to other members of a household.

In this regard, it is worth noting that at baseline, women reported more often than men that the introduction of KhetScore loan bundled with insurance would make them more likely to apply for loans and that they would be interested in borrowing larger amounts. Indeed, over the course of this impact evaluation, we find beneficial impacts over a wide range of agricultural and gender parity outcomes. In particular, this impact evaluation highlights the potential of this innovative financial product in bringing about transformative changes in rural economies by enhancing financial literacy, expanding access to credit and insurance services, boosting agricultural investment and production, promoting women’s empowerment, and alleviating some of the psychological stresses often associated with subsistence farming.

Our findings suggest that the KhetScore credit-scoring methodology, notably its ability to reduce transaction costs for both borrowers and lenders, serves to mitigate credit rationing while simultaneously broadening and enhancing the inclusivity of rural finance. In particular, KhetScore’s elimination of paperwork prerequisites, specifically the need for land titles, extends access to formal credit to a previously under-served group of potential borrowers. Furthermore, the incorporation of crop insurance may alleviate collateral requirements, addressing both risk-related and quantity-related constraints in formal borrowing. These results provide valuable insights for policymakers interested in expanding access to credit and insurance for sharecroppers and tenant farmers who lack documented land rights.

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Table 1: Characteristics of households in randomly allocated treatment and comparison villages

	Full sample		Control		Treatment		Difference (T - C)	
	N	Mean/Prop. (Std. Dev.)	N	Mean/Prop. (Std. Dev.)	N	Mean/Prop. (Std. Dev.)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Socio-demographic characteristics								
Female	1706	0.693	837	0.735	869	0.654	-0.081	0.117
Age	1705	42.309 (8.723)	837	42.293 (8.715)	868	42.325 (8.737)	0.032	1.116
Can read and write	1706	0.853	837	0.839	869	0.867	0.028	0.041
Completed primary school	1706	0.749	837	0.774	869	0.725	-0.049	0.051
Completed secondary school	1706	0.253	837	0.253	869	0.253	-0.000	0.047
Caste								
General caste	1497	0.363	775	0.314	722	0.417	0.103**	0.047
Other backward caste	1497	0.177	775	0.152	722	0.204	0.051	0.051
Scheduled caste	1497	0.359	775	0.427	722	0.287	-0.140***	0.052
Scheduled tribe	1497	0.100	775	0.107	722	0.093	-0.014	0.033
Household income								
Low (below INR 90,000 per year)	1706	0.534	837	0.519	869	0.549	0.030	0.084
Medium (INR 90,000 - 120,000 per year)	1706	0.281	837	0.313	869	0.250	-0.063	0.039
High (above INR 120,000 per year)	1706	0.185	837	0.168	869	0.201	0.033	0.070
Panel B. Experiences with credit and crop insurance								
Purchased on credit in past 12 months	1706	0.489	837	0.416	869	0.560	0.145***	0.052
Borrowed money in past 12 months	1706	0.291	837	0.296	869	0.285	-0.011	0.055
Desired loan amount last season	1704	37,620 (15,588)	835	37,157 (15,129)	869	38,064 (16,013)	907.556	1,860
Ever had crop insurance	1497	0.033	775	0.027	722	0.039	0.012	0.014
Currently has crop insurance	1497	0.015	775	0.012	722	0.018	0.006	0.008
Received crop insurance payout	1504	0.858	735	0.863	769	0.854	-0.008	0.048
Panel C. Agricultural characteristics								
Cultivated in last Kharif [†]	1504	0.858	735	0.863	769	0.854	-0.008	0.048
Cultivated in last Rabi	1681	0.745	820	0.820	861	0.675	-0.145**	0.072
Total cultivated area (acres)	1364	2.051 (1.176)	713	2.012 (1.128)	651	2.093 (1.227)	0.082	0.175
Farm size								
Marginal (0 - 2.5 ac)	1364	0.750	713	0.770	651	0.728	-0.042	0.053
Small (2.5 - 5 ac)	1364	0.229	713	0.212	651	0.247	0.036	0.048
Medium (5 - 10 ac)	1364	0.021	713	0.018	651	0.025	0.006	0.010
Cultivated paddy rice	1364	0.994	713	0.996	651	0.992	-0.003	0.004
Cultivated lentil	1364	0.027	713	0.006	651	0.051	0.045	0.029
Crop was damaged	1364	0.262	713	0.273	651	0.249	-0.025	0.060
Total revenue per acre for all crops	1364	22,940 (14,871)	713	21,400 (13,085)	651	24,628 (16,453)	3,228	2,239
Cost of production per acre for all crops (excl. own labor)	1364	13,777 (6,659)	713	13,813 (5,928)	651	13,737 (7,380)	-75.835	1,276
Profit per acre for all crops (excl. own labor)	1364	9,164 (13,628)	713	7,587 (13,053)	651	10,891 (14,040)	3,304**	1,437
Panel D. Baseline credit rationing status								
Quantity rationed	1706	0.511	837	0.478	869	0.543	0.065	0.047
Risk rationed	1706	0.291	837	0.283	869	0.299	0.016	0.042
Credit unrationed	1706	0.198	837	0.239	869	0.158	-0.081	0.035

Notes: Standard deviations for continuous variables in parentheses in columns (2), (4), and (6). Standard deviations for proportions not reported. Estimates in column (7) are coefficient estimates from a regression of the variable of interest on a binary variable indicating random assignment to the treatment, with standard errors adjusted for clustering at the village level (the level of randomization). We test whether the coefficient associated with random assignment to the treatment group is statistically different from zero, and *, **, and *** symbols in column (7) indicate statistical significance with 10 percent, 5 percent, and 1 percent probability of Type I error. [†] Variable added to the survey form at a later stage of baseline data collection, and is therefore missing for the first batch of survey respondents.

Table 2: Characteristics of quantity rationed, risk rationed, and unrationed households at baseline

	Quantity rationed	Risk rationed	Unconstrained	(1) vs. (3) <i>t</i> -statistic	(2) vs. (3) <i>t</i> -statistic
	(1)	(2)	(3)	(4)	(5)
Panel A. Socio-demographic characteristics					
Female	0.639 (0.481)	0.738 (0.440)	0.769 (0.422)	-4.605*** (0.028)	-0.994 (0.030)
Age	42.530 (8.873)	42.857 (7.537)	40.929 (9.785)	2.617*** (0.612)	3.055*** (0.631)
Can read and write	0.813 (0.390)	0.938 (0.242)	0.831 (0.375)	-0.731 (0.024)	4.611*** (0.023)
Completed primary school	0.713 (0.452)	0.690 (0.463)	0.929 (0.258)	-10.372*** (0.021)	-9.523*** (0.025)
Completed secondary school	0.227 (0.419)	0.205 (0.404)	0.392 (0.489)	-5.455*** (0.030)	-5.787*** (0.032)
Caste					
General caste	0.366 (0.482)	0.330 (0.471)	0.404 (0.492)	-1.106 (0.034)	-2.011** (0.037)
Other backward caste	0.178 (0.383)	0.209 (0.407)	0.111 (0.315)	2.835*** (0.024)	3.608*** (0.027)
Scheduled caste	0.362 (0.481)	0.356 (0.479)	0.390 (0.489)	-0.827 (0.034)	-0.942 (0.037)
Scheduled tribe	0.093 (0.291)	0.106 (0.308)	0.094 (0.292)	-0.035 (0.020)	0.504 (0.023)
Household income					
Low (below INR 90,000 per year)	0.554 (0.497)	0.606 (0.489)	0.377 (0.485)	5.648*** (0.031)	6.659*** (0.034)
Medium (INR 90,000 - 120,000 per year)	0.297 (0.457)	0.209 (0.407)	0.344 (0.476)	-1.563 (0.030)	-4.256*** (0.032)
High (above INR 120,000 per year)	0.149 (0.356)	0.185 (0.389)	0.279 (0.449)	-4.760*** (0.027)	-3.123*** (0.030)
Panel B. Agricultural characteristics					
Total cultivated area (acres)	2.077 (1.206)	1.856 (1.179)	2.271 (1.022)	-2.466** (0.079)	-4.790*** (0.087)
Farm size					
Marginal (0 - 2.5 ac)	0.748 (0.434)	0.774 (0.419)	0.716 (0.452)	0.988 (0.033)	1.672* (0.035)
Small (2.5 - 5 ac)	0.224 (0.418)	0.208 (0.406)	0.273 (0.446)	-1.512 (0.032)	-1.895* (0.034)
Medium (5 - 10 ac)	0.027 (0.164)	0.018 (0.133)	0.011 (0.106)	1.763* (0.009)	0.702 (0.009)

Notes: Standard deviations in parentheses. *, **, and *** symbols in columns (4) and (5) indicate statistical significance with 10 percent, 5 percent, and 1 percent probability of Type I error.

Table 3: Number of participating farmers across implementation stages

	(1)	(2)	(3)	(4)
Season	KhetScore loan applied	KhetScore loan received	Enrolled in insurance	Insurance payout made
Rabi 2020-2021	90	62 (68.9 percent)	95	7
Kharif 2021	300	206 (68.7 percent)	445	10
Rabi 2021-2022	350	319 (91.1 percent)	319	0
Kharif 2022	280	223 (79.6 percent)	430	0

Note: Data reported only for treatment villages, since there was no intervention in the control villages.

Table 4: Program effects on credit-related outcomes

	Took up credit in past 12 months	Credit source: Formal	Credit source: Informal	Faced difficulty in repayment
	(1)	(2)	(3)	(4)
Panel A: All clients				
Treatment	0.022	0.212	-0.199	-0.559
Std. Error	(0.035)	(0.032)	(0.028)	(0.052)
<i>p</i> -value	0.528	<0.001	<0.001	<0.001
Sharpened <i>q</i> -value	0.128	0.001	0.001	0.001
R ²	0.13	0.15	0.09	0.38
Panel B: By gender				
Total effect Male	-0.081	0.165	-0.263	-0.495
Std. Error	(0.050)	(0.045)	(0.041)	(0.067)
<i>p</i> -value	0.100	<0.001	<0.001	<0.001
Sharpened <i>q</i> -value	0.026	0.001	0.001	0.001
Total effect Female	0.156	0.258	-0.102	-0.595
Std. Error	(0.045)	(0.043)	(0.034)	(0.089)
<i>p</i> -value	<0.001	<0.001	0.003	<0.001
Sharpened <i>q</i> -value	0.001	0.001	0.001	0.001
R ²	0.15	0.15	0.13	0.39
Panel C: By baseline credit rationing status				
Total effect Unrationed	0.063	0.241	-0.178	-0.646
Std. Error	(0.053)	(0.054)	(0.033)	(0.075)
<i>p</i> -value	0.232	<0.001	<0.001	<0.001
Sharpened <i>q</i> -value	0.059	0.001	0.001	0.001
Total effect Quantity rationed	0.025	0.204	-0.182	-0.523
Std. Error or 95 percent CI	(0.042)	(0.033)	(0.033)	(0.064)
<i>p</i> -value	0.543	<0.001	<0.001	<0.001
Sharpened <i>q</i> -value	0.128	0.001	0.001	0.001
Total effect Risk rationed	0.032	0.215	-0.207	-0.494
Std. Error or 95 percent CI	(0.043)	(0.053)	(0.046)	(0.091)
<i>p</i> -value	0.452	<0.001	<0.001	<0.001
Sharpened <i>q</i> -value	0.115	0.001	0.001	0.001
R ²	0.14	0.16	0.12	0.39
Observations	1,621	1,621	1,621	632
Control group mean	0.353	0.140	0.223	0.756

Note: Standard errors in parentheses clustered by village. All dependent variables are binary. Data on whether the client faced difficulty in repaying loans is only available among respondents who indicated that they had taken out a loan. All regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). In Panels B and C, rows labeled "Total effect..." columns correspond to linear combinations of regression coefficients (where applicable), including the regression coefficient associated with T_{ib} and the corresponding interaction terms from equations (2) and (3), respectively.

Table 5: Program effects on insurance take-up, renewal and knowledge

	Enrolled in insurance (1)	Renewed insurance coverage (2)	Insurance knowledge (3)
Panel A: All clients			
Treatment	0.614	0.283	0.496
Std. Error	(0.043)	(0.069)	(0.158)
<i>p</i> -value	<0.001	<0.001	0.002
Sharpened <i>q</i> -value	0.001	0.001	0.001
R ²	0.41	0.12	0.19
Panel B: By gender			
Total effect Male	0.627	0.070	0.518
Std. Error	(0.042)	(0.080)	(0.116)
<i>p</i> -value	<0.001	0.384	<0.001
Sharpened <i>q</i> -value	0.001	0.069	0.001
Total effect Female	0.598	0.568	0.459
Std. Error	(0.068)	(0.063)	(0.365)
<i>p</i> -value	<0.001	<0.001	0.209
Sharpened <i>q</i> -value	0.001	0.001	0.052
R ²	0.41	0.16	0.19
Panel C: By baseline credit rationing status			
Total effect Unrationed	0.664	0.344	0.506
Std. Error	(0.055)	(0.101)	(0.253)
<i>p</i> -value	<0.001	<0.001	0.045
Sharpened <i>q</i> -value	0.001	0.001	0.016
Total effect Quantity rationed	0.632	0.290	0.533
Std. Error	(0.049)	(0.071)	(0.140)
<i>p</i> -value	<0.001	<0.001	<0.001
Sharpened <i>q</i> -value	0.001	0.001	0.001
Total effect Risk rationed	0.591	0.199	0.700
Std. Error	(0.049)	(0.118)	(0.326)
<i>p</i> -value	<0.001	0.092	0.032
Sharpened <i>q</i> -value	0.001	0.030	0.012
R ²	0.43	0.10	0.17
Observations	1,621	950	687
Control group mean	0.280	0.394	3.047

Note: Standard errors in parentheses clustered by village. The first two dependent variables (insurance take-up and insurance renewal) are binary. The third, “Insurance knowledge score”, is the number of correct responses to six True/False statements about crop insurance, and consequently takes integer values 0-6. All regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client’s village at the time of baseline (as opposed to initiating new operations as part of this project). We do not have baseline data on insurance renewals or the client’s familiarity with terms and conditions of crop insurance (“insurance knowledge score”), so we control for baseline insurance uptake in all regressions. In Panels B and C, rows labeled “Total effect...” correspond to linear combinations of regression coefficients (where applicable), including the regression coefficient associated with T_{ib} and the corresponding interaction terms from equations (2) and (3), respectively.

Table 6: Treatment effects (ITT and LATE) on agricultural outcomes during the Kharif (monsoon) season

Cultivated during Kharif (*Yes* = 1)		Area cultivated (acres)		Revenue per acre (10,000 INR)		Cost per acre (10,000 INR)		Profit per acre (10,000 INR)		Return on investment (percent)	
ITT (1)	LATE (2)	ITT (3)	LATE (4)	ITT (5)	LATE (6)	ITT (7)	LATE (8)	ITT (9)	LATE (10)	ITT (11)	LATE (12)
Panel A: All clients											
Treatment	0.026	0.276	0.058	0.233	0.213	0.848	0.032	0.227	0.905	0.022	0.087
Std. Error or 95 percent CI	(0.014)	[0.245, 0.299]	(0.073)	(0.285)	(0.066)	(0.261)	(0.271)	(0.068)	(0.277)	(0.068)	(0.270)
p-value	0.064		0.422	0.413	0.001	0.906	0.906	<0.001	0.001	0.746	0.747
Sharpened q-value	0.113		0.411	0.325	0.010	0.006	0.645	0.009	0.006	0.563	0.557
R ²	0.02		0.02		0.05		0.01	0.04		0.01	
Panel B: By gender											
Total effect Male	0.028	0.297	0.109	0.459	0.257	1.095	0.042	0.217	0.932	-0.059	-0.241
Std. Error or 95 percent CI	(0.020)	[0.213, 0.325]	(0.097)	(0.389)	(0.077)	(0.270)	(0.110)	(0.106)	(0.441)	(0.082)	(0.345)
p-value	0.161		0.259	0.238	<0.001	<0.001	0.700	0.041	0.035	0.466	0.485
Sharpened q-value	0.199		0.282	0.232	0.009	0.001	0.563	0.083	0.053	0.426	0.381
Total effect Female	0.016	0.257	-0.016	-0.033	0.166	0.647	-0.036	0.249	0.932	0.131	0.457
Std. Error or 95 percent CI	(0.012)	[0.180, 0.297]	(0.100)	(0.363)	(0.096)	(0.371)	(0.055)	(0.061)	(0.254)	(0.108)	(0.401)
p-value	0.162		0.875	0.928	0.086	0.081	0.515	<0.001	<0.001	0.222	0.254
Sharpened q-value	0.199		0.596	0.645	0.136	0.095	0.478	0.432	0.003	0.003	0.232
R ²	0.03		0.02		0.05		0.01	0.04		0.01	
Panel C: By baseline credit rationing status											
Total effect Unrationed	0.048	0.00003	0.106	0.665	0.265	1.743	-0.045	0.262	1.746	0.008	0.045
Std. Error or 95 percent CI	(0.031)	[-1.000, 0.00006]	(0.121)	(0.717)	(0.113)	(0.825)	(0.075)	(0.473)	(0.660)	(0.118)	(0.744)
p-value	0.119		0.381	0.354	0.020	0.035	0.545	0.567	0.002	0.947	0.951
Sharpened q-value	0.171		0.391	0.302	0.047	0.053	0.495	0.432	0.012	0.624	0.645
Total effect Quantity rationed	0.021	0.265	-0.001	0.002	0.233	0.871	0.035	0.125	0.240	-0.034	-0.122
Std. Error or 95 percent CI	(0.017)	[0.202, 0.294]	(0.103)	(0.377)	(0.082)	(0.298)	(0.126)	(0.462)	(0.124)	(0.108)	(0.395)
p-value	0.221		0.989	0.995	0.005	0.003	0.782	0.787	0.052	0.750	0.758
Sharpened q-value	0.251		0.625	0.645	0.016	0.011	0.579	0.574	0.097	0.563	0.557
Total effect Risk rationed	0.024	0.368	0.101	0.335	0.154	0.543	-0.024	0.216	0.737	0.074	0.236
Std. Error or 95 percent CI	(0.025)	[-0.646, 0.393]	(0.089)	(0.294)	(0.106)	(0.338)	(0.065)	(0.220)	(0.225)	(0.126)	(0.409)
p-value	0.326		0.257	0.255	0.145	0.108	0.711	0.730	0.002	0.558	0.565
Sharpened q-value	0.334		0.282	0.232	0.191	0.118	0.563	0.557	0.011	0.495	0.432
R ²	0.02		0.02		0.05		0.01	0.04		0.01	
Observations	1,621	1,534	1,534	1,534	1,534	1,534	1,534	1,534	1,534	1,534	1,534
Control group mean	0.944	1.953	1.174	1.064	1.117	0.352					

Note: Standard errors adjusted for clustering at the village level in parentheses and 95 percent confidence intervals constructed from the parametric procedure proposed by Krinsky and Robb (1986) in brackets. LATEs in column (2) calculated using the formula presented in equation (11) in Chiburis et al. (2011) after recursive bivariate probit regression. LATEs in columns (4), (6), (8), (10), and (12) based on instrumental variables (IV) regressions. In both recursive bivariate probit regressions and IV regressions, random assignment serves as the identifying excluded instrument, with relevant interactions where appropriate. Cost per acre, profit per acre, and return on investment all estimated *excluding* the implicit cost of household labor (valued at prevailing wage rate). If clients reported that they did not cultivate during Kharif season, area, revenue, cost, profit, and return on investment are all equal to zero. All regressions control for block fixed effects and baseline values for credit uptake (binary) and agricultural profits per acre excluding the cost of own labor, as well as additional control variables including age (categorical), literacy (binary), caste (binary), a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project), and an indicator for whether the client had cultivated during the preceding Rabi season (binary). We also include binary controls if missing observations for control variables were imputed, except for the LATEs in column (2). Panel C, where one of the binary controls for an imputed control suffered from perfect separation. Rows labeled "Total effect..." in Panels B and C correspond to linear combinations of regression coefficients (where applicable), including the regression coefficient associated with T_{it} and the corresponding interaction terms from equations (2) and (3), respectively.

Table 7: Treatment effects (ITT and LATE) on agricultural outcomes during the Rabi (dry) season

Cultivated during Rabi (*Yes* = 1)		Area cultivated (acres)		Revenue per acre (10,000 INR)		Cost per acre (10,000 INR)		Profit per acre (10,000 INR)		Return on investment (percent)	
ITT	LATE	ITT	LATE	ITT	LATE	ITT	LATE	ITT	LATE	ITT	LATE
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: All clients											
Treatment	-0.008	0.219	1.328	0.031	0.119	-0.251	-0.964	0.334	1.282	0.115	0.443
Std. Error or 95 percent CI	(0.038)	[0.065, 0.343]	(0.104)	(0.093)	(0.363)	(0.075)	(0.291)	(0.087)	(0.353)	(0.040)	(0.165)
<i>p</i> -value	0.835		<0.001	0.742	0.743	<0.001	<0.001	<0.001	<0.001	0.004	0.007
Sharpened <i>q</i> -value	0.588		0.009	0.563	0.557	0.009	0.006	0.003	0.003	0.015	0.019
R ²	0.06		0.08	0.06		0.05		0.02		0.05	
Panel B: By gender											
Total effect Male	-0.020	0.211	1.048	-0.059	-0.225	-0.246	-1.021	0.230	5.221	0.120	0.498
Std. Error or 95 percent CI	(0.042)	[-0.017, 0.373]	(0.135)	(0.120)	(0.498)	(0.086)	(0.372)	(0.118)	(1.107)	(0.044)	(0.192)
<i>p</i> -value	0.629		0.068	0.383	0.624	0.004	0.866	0.051	0.352	0.007	0.736
Sharpened <i>q</i> -value	0.522		0.115	0.522	0.514	0.016	0.017	0.097	0.001	0.020	0.022
Total effect Female	0.024	0.263	1.620	0.139	0.463	-0.253	-0.920	0.472	3.887	0.110	0.405
Std. Error or 95 percent CI	(0.063)	[0.047, 0.405]	(0.150)	(0.117)	(0.420)	(0.134)	(0.483)	(0.156)	(1.224)	(0.068)	(0.246)
<i>p</i> -value	0.705		0.003	0.235	0.270	0.060	0.057	0.003	0.001	0.108	0.100
Sharpened <i>q</i> -value	0.563		0.012	0.263	0.234	0.108	0.076	0.012	0.007	0.163	0.112
R ²	0.06		0.08	0.07		0.05		0.02		0.05	
Panel C: By baseline credit rationing status											
Total effect Unrationed	0.033	0.284	0.787	0.163	0.949	-0.206	-1.372	0.316	6.342	0.150	0.944
Std. Error or 95 percent CI	(0.063)	[0.228, 0.392]	(0.156)	(0.130)	(0.831)	(0.143)	(0.933)	(0.182)	(2.645)	(0.095)	(0.625)
<i>p</i> -value	0.600		0.551	0.383	0.210	0.148	0.866	0.083	0.352	0.115	0.736
Sharpened <i>q</i> -value	0.522		0.495	0.325	0.243	0.191	0.143	0.135	0.030	0.169	0.136
Total effect Quantity rationed	-0.008	0.319	1.531	0.014	0.051	-0.252	-0.915	0.414	4.997	0.106	0.390
Std. Error or 95 percent CI	(0.047)	[0.140, 0.430]	(0.155)	(0.134)	(0.482)	(0.100)	(0.376)	(0.147)	(1.262)	(0.047)	(0.175)
<i>p</i> -value	0.857		0.006	0.918	0.915	0.012	0.015	0.005	<0.001	0.023	0.026
Sharpened <i>q</i> -value	0.591		0.019	0.615	0.645	0.030	0.029	0.017	0.001	0.050	0.043
Total effect Risk rationed	-0.068	-0.199	1.056	-0.028	-0.077	-0.355	-1.180	0.282	4.137	0.142	0.477
Std. Error or 95 percent CI	(0.053)	[-0.303, 0.046]	(0.137)	(0.109)	(0.357)	(0.099)	(0.297)	(0.093)	(0.712)	(0.058)	(0.194)
<i>p</i> -value	0.195		0.029	0.799	0.828	<0.001	<0.001	0.003	<0.001	0.014	0.014
Sharpened <i>q</i> -value	0.230		0.063	0.585	0.605	0.006	0.001	0.012	0.001	0.036	0.028
R ²	0.06		0.09	0.07		0.06		0.02		0.05	
Observations	1,621	1,432	1,432	1,432	1,432	1,432	1,432	1,432	1,432	1,432	1,432
Control group mean	0.710	1.146	0.825	0.825	0.825	1.036	1.036	-0.309	-0.309	-0.032	-0.032

Note: Standard errors adjusted for clustering at the village level in parentheses and 95 percent confidence intervals constructed from the parametric procedure proposed by Krinsky and Robb (1986) in brackets. LATEs in column (2) calculated using the formula presented in equation (11) in Chiburis et al. (2011) after recursive bivariate probit regression. LATEs in columns (4), (6), (8), (10), and (12) based on instrumental variables (IV) regressions. In both recursive bivariate probit regressions and IV regressions, random assignment serves as the identifying excluded instrument, with relevant interactions where appropriate. Cost per acre, profit per acre, and return on investment all estimated *excluding* the implicit cost of household labor (valued at prevailing wage rate). If clients reported that they did not cultivate during Rabi season, area, revenue, cost, profit, and return on investment are all equal to zero. All regressions control for block fixed effects and baseline values for credit uptake (binary) and agricultural profits per acre excluding the cost of own labor, as well as additional control variables including age (categorical), literacy (binary), caste (binary), a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project), and an indicator for whether the client had cultivated during the preceding Rabi season (binary). We also include binary controls if missing observations for control variables were imputed. Rows labeled "Total effect..." in Panels B and C correspond to linear combinations of regression coefficients (where applicable), including the regression coefficient associated with T_{1b} and the corresponding interaction terms from equations (2) and (3), respectively.

Table 8: Treatment effects (LATE) on input expenditures during the Kharif (monsoon) season

	Seeds (1)	Fertilizer (2)	Irrigation (3)	Machinery (4)	Land rentals (5)	Male hired labor (6)	Female hired labor (7)
Panel A: All clients							
Treatment	1431.215	325.784	203.509	-532.103	-1557.958	1049.620	2757.320
Std. Error	(300.078)	(953.688)	(352.970)	(1026.232)	(526.703)	(1313.848)	(501.144)
<i>p</i> -value	<0.001	0.733	0.564	0.604	0.003	0.424	<0.001
Sharpened <i>q</i> -value	0.001	0.910	0.791	0.797	0.025	0.704	0.001
Panel B: By gender							
Total effect Male	2517.168	1350.954	751.366	-96.355	-828.108	897.210	5047.627
Std. Error	(518.308)	(1466.801)	(494.705)	(1537.583)	(706.939)	(1885.120)	(757.401)
<i>p</i> -value	<0.001	0.357	0.129	0.950	0.242	0.634	<0.001
Sharpened <i>q</i> -value	0.001	0.665	0.337	0.996	0.491	0.814	0.001
Total effect Female	544.765	-900.008	-95.548	-1970.360	-2371.411	1428.297	739.331
Std. Error	(440.751)	(1110.768)	(463.151)	(1214.118)	(684.254)	(1464.498)	(642.334)
<i>p</i> -value	0.216	0.418	0.837	0.105	<0.001	0.329	0.250
Sharpened <i>q</i> -value	0.457	0.704	0.977	0.282	0.007	0.627	0.497
Panel C: By baseline credit rationing status							
Total effect Unrationed	2136.901	1145.642	705.682	-1657.052	-1170.673	-410.716	5731.813
Std. Error	(924.416)	(2291.649)	(835.328)	(3011.733)	(1177.746)	(3291.646)	(1585.735)
<i>p</i> -value	0.021	0.617	0.398	0.582	0.320	0.901	<0.001
Sharpened <i>q</i> -value	0.102	0.813	0.694	0.791	0.615	0.985	0.006
Total effect Quantity rationed	1614.798	-503.097	199.871	156.213	-1170.402	1498.693	2684.407
Std. Error	(427.362)	(1324.827)	(474.088)	(1202.962)	(556.438)	(1732.497)	(622.799)
<i>p</i> -value	<0.001	0.704	0.673	0.897	0.035	0.387	<0.001
Sharpened <i>q</i> -value	0.004	0.908	0.868	0.985	0.145	0.694	0.001
Total effect Risk rationed	733.827	1426.893	413.467	-856.133	-1881.041	-822.271	1646.711
Std. Error	(424.891)	(908.060)	(374.458)	(1118.816)	(609.034)	(1445.386)	(636.692)
<i>p</i> -value	0.084	0.116	0.270	0.444	0.002	0.569	0.010
Sharpened <i>q</i> -value	0.245	0.307	0.528	0.709	0.018	0.791	0.060
Observations	1,534	1,534	1,534	1,534	1,534	1,534	1,534
Control group mean	2134.181	4448.365	1241.755	3802.746	1198.733	4947.940	1511.982

Note: Standard errors adjusted for clustering at the village level in parentheses. Expenditures Winsorized at the 2.5th (lower) and 97.5th (upper) percentiles of the empirical distributions. All regressions control for block fixed effects and baseline values for credit uptake (binary) and agricultural profits per acre excluding the cost of own labor, as well as additional control variables including age (categorical), literacy (binary), caste (binary), a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project), and an indicator for whether the client had cultivated during the preceding Rabi season (binary). We also include binary controls if missing observations for control variables were imputed. Rows labeled "Total effect..." in Panels B and C correspond to linear combinations of regression coefficients (where applicable), including the regression coefficient associated with T_{it} and the corresponding interaction terms from equations (2) and (3), respectively.

Table 9: Treatment effects (LATE) on input expenditures (INR) during the Rabi (dry) season

	Seeds (1)	Fertilizer (2)	Irrigation (3)	Machinery (4)	Land rentals (5)	Male hired labor (6)	Female hired labor (7)
Panel A: All clients							
Treatment	1140.486	-1708.736	-463.885	2035.359	-587.346	1039.342	401.997
Std. Error	(673.709)	(1440.787)	(539.299)	(1702.059)	(260.709)	(1806.613)	(642.461)
<i>p</i> -value	0.091	0.236	0.390	0.232	0.024	0.565	0.532
Sharpened <i>q</i> -value	0.254	0.486	0.694	0.486	0.114	0.791	0.769
Panel B: By gender							
Total effect Male	1863.512	1828.769	-1481.425	3217.011	-888.975	-3489.107	-441.439
Std. Error	(972.496)	(2021.190)	(808.754)	(2369.477)	(447.512)	(2504.294)	(935.654)
<i>p</i> -value	0.292	0.012	0.013	0.266	0.158	0.002	0.068
Sharpened <i>q</i> -value	0.187	0.675	0.218	0.407	0.169	0.394	0.814
Total effect Female	754.796	-6345.317	854.673	-320.746	-157.950	6181.019	1246.709
Std. Error	(792.051)	(2512.411)	(639.012)	(2344.164)	(283.375)	(2375.452)	(638.592)
<i>p</i> -value	0.341	0.012	0.181	0.891	0.577	0.009	0.051
Sharpened <i>q</i> -value	0.646	0.068	0.416	0.985	0.791	0.060	0.176
Panel C: By baseline credit rationing status							
Total effect Unrationed	1381.474	-8716.486	-2506.889	-2283.435	-1454.194	-5394.664	1854.677
Std. Error	(1821.057)	(4014.436)	(1669.865)	(3566.023)	(606.045)	(4889.188)	(1486.423)
<i>p</i> -value	0.621	0.164	0.258	0.466	0.091	0.208	0.161
Sharpened <i>q</i> -value	0.709	0.133	0.343	0.769	0.092	0.528	0.457
Total effect Quantity rationed	1354.246	332.042	108.023	3941.369	-594.639	2373.630	138.591
Std. Error	(959.909)	(1952.530)	(621.415)	(1959.642)	(364.603)	(1928.041)	(824.557)
<i>p</i> -value	0.158	0.865	0.862	0.044	0.103	0.218	0.867
Sharpened <i>q</i> -value	0.388	0.977	0.977	0.169	0.282	0.457	0.977
Total effect Risk rationed	472.641	-3010.346	-756.785	353.566	-388.920	341.360	-220.514
Std. Error	(689.174)	(1607.914)	(574.331)	(2077.033)	(307.644)	(2419.851)	(664.123)
<i>p</i> -value	0.493	0.061	0.188	0.865	0.206	0.888	0.740
Sharpened <i>q</i> -value	0.749	0.201	0.425	0.977	0.457	0.985	0.910
Observations	1,432	1,432	1,432	1,432	1,432	1,432	1,432
Control group mean	1860.047	3657.849	1101.592	3067.216	384.989	4824.595	863.186

Note: Standard errors adjusted for clustering at the village level in parentheses. Expenditures Winsorized at the 2.5th (lower) and 97.5th (upper) percentiles of the empirical distributions. All regressions control for block fixed effects and baseline values for credit uptake (binary) and agricultural profits per acre excluding the cost of own labor, as well as additional control variables including age (categorical), literacy (binary), caste (binary), a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project), and an indicator for whether the client had cultivated during the preceding Rabi season (binary). We also include binary controls if missing observations for control variables were imputed. Rows labeled "Total effect..." in Panels B and C correspond to linear combinations of regression coefficients (where applicable), including the regression coefficient associated with T_{it} and the corresponding interaction terms from equations (2) and (3), respectively.

Table 10: Treatment effects on household labor supply (days) during the Kharif (monsoon) season

	Own labor	Other family member – Male	Other family member – Female
	(1)	(2)	(3)
Panel A: All clients			
Treatment	-38.603	8.697	2.975
Std. Error	(11.147)	(8.111)	(8.253)
<i>p</i> -value	<0.001	0.284	0.719
Sharpened <i>q</i> -value	0.007	0.556	0.908
Panel B: By gender			
Total effect Male	-56.953	6.721	4.307
Std. Error	(17.033)	(10.365)	(10.953)
<i>p</i> -value	<0.001	0.517	0.694
Sharpened <i>q</i> -value	0.009	0.769	0.903
Total effect Female	-6.540	8.507	10.721
Std. Error	(15.365)	(10.197)	(7.881)
<i>p</i> -value	0.670	0.404	0.174
Sharpened <i>q</i> -value	0.868	0.694	0.407
Panel C: By baseline credit rationing status			
Total effect Unconstrained	-53.567	12.674	5.728
Std. Error	(22.793)	(26.022)	(17.567)
<i>p</i> -value	0.019	0.626	0.744
Sharpened <i>q</i> -value	0.096	0.814	0.910
Total effect Quantity rationed	-30.574	31.718	2.774
Std. Error	(14.198)	(10.104)	(9.431)
<i>p</i> -value	<0.001	0.604	0.943
Sharpened <i>q</i> -value	0.006	0.797	0.996
Total effect Risk rationed	-30.574	31.718	2.774
Std. Error	(14.198)	(10.104)	(9.431)
<i>p</i> -value	0.031	0.002	0.769
Sharpened <i>q</i> -value	0.133	0.016	0.910
Observations	1,534	1,534	1,534
Control group mean	45.680	23.591	12.000

Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for block fixed effects and baseline values for credit uptake (binary) and agricultural profits per acre excluding the cost of own labor, as well as additional control variables including age (categorical), literacy (binary), caste (binary), a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project), and an indicator for whether the client had cultivated during the preceding Rabi season (binary). We also include binary controls if missing observations for control variables were imputed. Rows labeled "Total effect..." in Panels B and C correspond to linear combinations of regression coefficients (where applicable), including the regression coefficient associated with T_{ib} and the corresponding interaction terms from equations (2) and (3), respectively.

Table 11: Treatment effects on household labor supply (days) during the Rabi (dry) season

	Own labor	Other family member – Male	Other family member – Female
	(1)	(2)	(3)
Panel A: All clients			
Treatment	6.632	-5.775	-5.370
Std. Error	(9.477)	(6.813)	(5.209)
<i>p</i> -value	0.484	0.397	0.303
Sharpened <i>q</i> -value	0.749	0.694	0.576
Panel B: By gender			
Total effect Male	-12.421	-28.107	-18.392
Std. Error	(13.483)	(9.942)	(7.833)
<i>p</i> -value	0.484	0.397	0.303
Sharpened <i>q</i> -value	0.749	0.694	0.576
Total effect Female	33.478	15.123	8.794
Std. Error	(12.130)	(7.566)	(4.334)
<i>p</i> -value	0.006	0.046	0.042
Sharpened <i>q</i> -value	0.040	0.169	0.169
Panel C: By baseline credit rationing status			
Total effect Unconstrained	-22.322	-4.767	-8.475
Std. Error	(28.652)	(16.089)	(12.583)
<i>p</i> -value	0.436	0.767	0.501
Sharpened <i>q</i> -value	0.706	0.910	0.750
Total effect Quantity rationed	14.662	-5.051	-5.307
Std. Error	(10.312)	(8.504)	(6.312)
<i>p</i> -value	0.155	0.553	0.400
Sharpened <i>q</i> -value	0.388	0.791	0.694
Total effect Risk rationed	-3.255	-11.063	-7.441
Std. Error	(12.348)	(6.115)	(4.316)
<i>p</i> -value	0.792	0.070	0.085
Sharpened <i>q</i> -value	0.910	0.223	0.245
Observations	1,432	1,432	1,432
Control group mean	45.680	23.591	12.000

Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for block fixed effects and baseline values for credit uptake (binary) and agricultural profits per acre excluding the cost of own labor, as well as additional control variables including age (categorical), literacy (binary), caste (binary), a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project), and an indicator for whether the client had cultivated during the preceding Rabi season (binary). We also include binary controls if missing observations for control variables were imputed. Rows labeled "Total effect..." in Panels B and C correspond to linear combinations of regression coefficients (where applicable), including the regression coefficient associated with T_{ib} and the corresponding interaction terms from equations (2) and (3), respectively.

Table 12: Program effects on contributions to household decision making

	Decision whether to borrow money	Decision on how to use borrowed funds	Input on livelihood activities	Control use of own income	Asset ownership
	(1)	(2)	(3)	(4)	(5)
Panel A: Dvara E-Registry clients					
Total effect Male	0.014	0.006	-0.030	0.054	0.045
Std. Error	(0.039)	(0.041)	(0.048)	(0.051)	(0.018)
<i>p</i> -value	0.710	0.892	0.529	0.294	0.012
Sharpened <i>q</i> -value	1.000	1.000	0.944	0.676	0.031
Total effect Female	0.277	0.251	0.056	0.071	0.023
Std. Error	(0.054)	(0.062)	(0.050)	(0.048)	(0.039)
<i>p</i> -value	<0.001	<0.001	0.263	0.141	0.560
Sharpened <i>q</i> -value	0.001	0.001	0.676	0.394	0.944
R ²	0.32	0.33	0.09	0.09	0.03
Observations	1,621	1,621	1,621	1,621	1,621
Control group mean	0.427	0.450	0.803	0.716	0.893
Panel B: Other household member (co-signer)					
Total effect Male	-0.070	-0.026	-0.051	0.005	0.024
Std. Error	(0.057)	(0.056)	(0.056)	(0.057)	(0.028)
<i>p</i> -value	0.222	0.643	0.359	0.933	0.379
Sharpened <i>q</i> -value	0.637	1.000	0.676	1.000	0.676
Total effect Female	0.030	-0.002	0.379	0.376	0.351
Std. Error	(0.031)	(0.022)	(0.052)	(0.054)	(0.038)
<i>p</i> -value	0.338	0.923	<0.001	<0.001	<0.001
Sharpened <i>q</i> -values	0.676	1.000	0.001	0.001	0.001
R ²	0.47	0.51	0.18	0.16	0.16
Observations	1,294	1,294	1,294	1,294	1,294
Control group mean	0.373	0.368	0.562	0.525	0.744

Note: Standard errors adjusted for clustering at village level in parentheses. All regressions control for block fixed effects and baseline values for insurance uptake (binary) and agricultural profits per acre excluding the cost of own labor, as well as additional control variables including age (categorical), literacy (binary), caste (binary), a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project), and an indicator for whether the client had cultivated during the preceding Rabi season (binary). We also include binary controls if missing observations for control variables were imputed. Rows labeled "Total effect..." correspond to linear combinations of regression coefficients (where applicable), including the regression coefficient associated with T_{ib} and the corresponding interaction terms from equation (2).

Table 13: Program effects on mental and emotional stress

	Composite stress score	Unable to control important things	Confidence in ability to handle personal problems	Felt that things were going her way	Felt that difficulties were piling up
	(1)	(2)	(3)	(4)	(5)
Panel A: Dvara E-Registry clients					
Treatment	-0.032	0.070	0.158	0.110	0.070
Std. Error	(0.080)	(0.146)	(0.150)	(0.127)	(0.151)
<i>p</i> -value	0.685	0.629	0.293	0.389	0.643
Sharpened <i>q</i> -value	0.657	0.657	0.414	0.501	0.657
R ²	0.04	0.02	0.10	0.05	0.03
Observations	772	772	772	772	772
Control group mean	2.633	2.907	3.677	3.404	2.704
Panel B: Other household member (co-signer)					
Treatment	-0.558	-0.296	0.504	0.166	-1.171
Std. Error	(0.073)	(0.083)	(0.119)	(0.129)	(0.138)
<i>p</i> -value	<0.001	<0.001	<0.001	0.198	<0.001
Sharpened <i>q</i> -value	0.001	0.001	0.001	0.313	0.001
R ²	0.24	0.18	0.16	0.14	0.27
Observations	740	740	740	740	740
Control group mean	2.963	3.381	3.639	3.249	3.361

Note: Standard errors adjusted for clustering at village level in parentheses. All regressions control for block fixed effects and baseline values for insurance uptake (binary) and agricultural profits per acre excluding the cost of own labor, as well as additional control variables including age (categorical), literacy (binary), caste (binary), a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project), and an indicator for whether the client had cultivated during the preceding Rabi season (binary). We also include binary controls if missing observations for control variables were imputed. Rows labeled "Total effect..." correspond to linear combinations of regression coefficients (where applicable), including the regression coefficient associated with T_{ib} and the corresponding interaction terms from equation (2).

Table 14: Implementation costs and revenues

		Cost per unit	Number of units	Total cost	
				INR	USD
Research and development (one-off costs)					
Personnel				1,463,060	19,020
Office/facilities				690,000	8,970
IT services				243,994	3,172
Data collection/licensing				1,670,086	21,711
Guarantee fund for initial	10% per INR		450	1,530,000	19,890
de-risking of KhetScore	34,000 loan		farmers		
Total R&D costs				5,666,890	73,670
Fixed cost per season					
Project management				367,481	4,777
Staff travel				36,627	476
Field office costs				690,000	8,970
App/portal maintenance				248,400	3,229
Communication material				68,626	892
Total fixed cost per season				1,411,135	18,345
Variable cost per farmer					
KhetScore reports				1,538.46	20.00
DER field staff				374	4.86
Staff transport, allowance				37	0.48
Printing, stationary				12	0.16
Total variable cost per farmer				1,961.46	25.50
Interest and insurance premiums per farmer					
Interest	14% of loan	INR 34,000		4,760	61.88
Insurance	2% of loan	INR 34,000		680	8.84
Revenues per farmer				5,440	70.72

Note: An exchnage rate of 0.013 USD per INR was used for converting expenses.

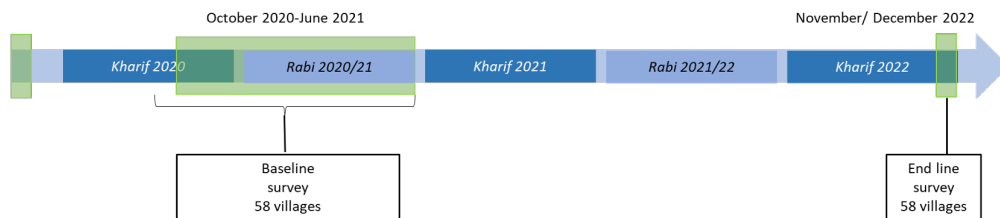


Figure 1: Study timeline

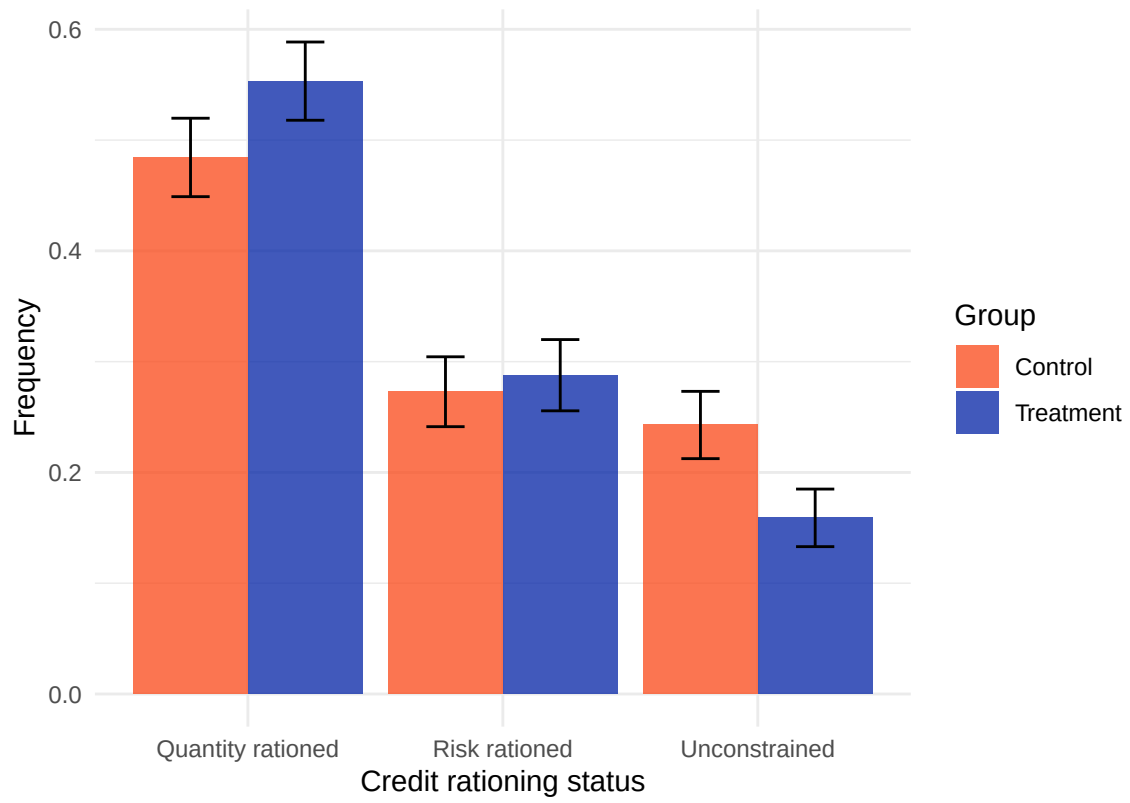


Figure 2: Baseline credit rationing, by experimental arm

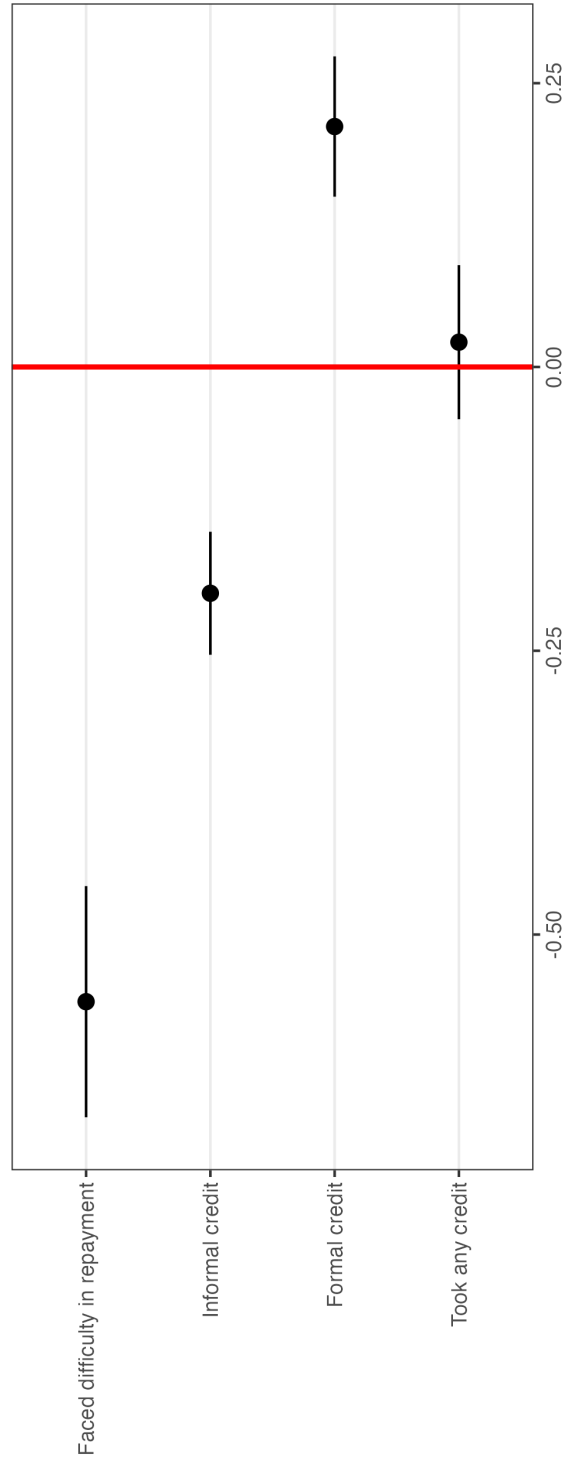


Figure 3: Program effects (ITT) on credit uptake, formal vs. informal credit sources, and challenges with loan repayment among full sample
Note: Bars indicate 95 percent confidence intervals.

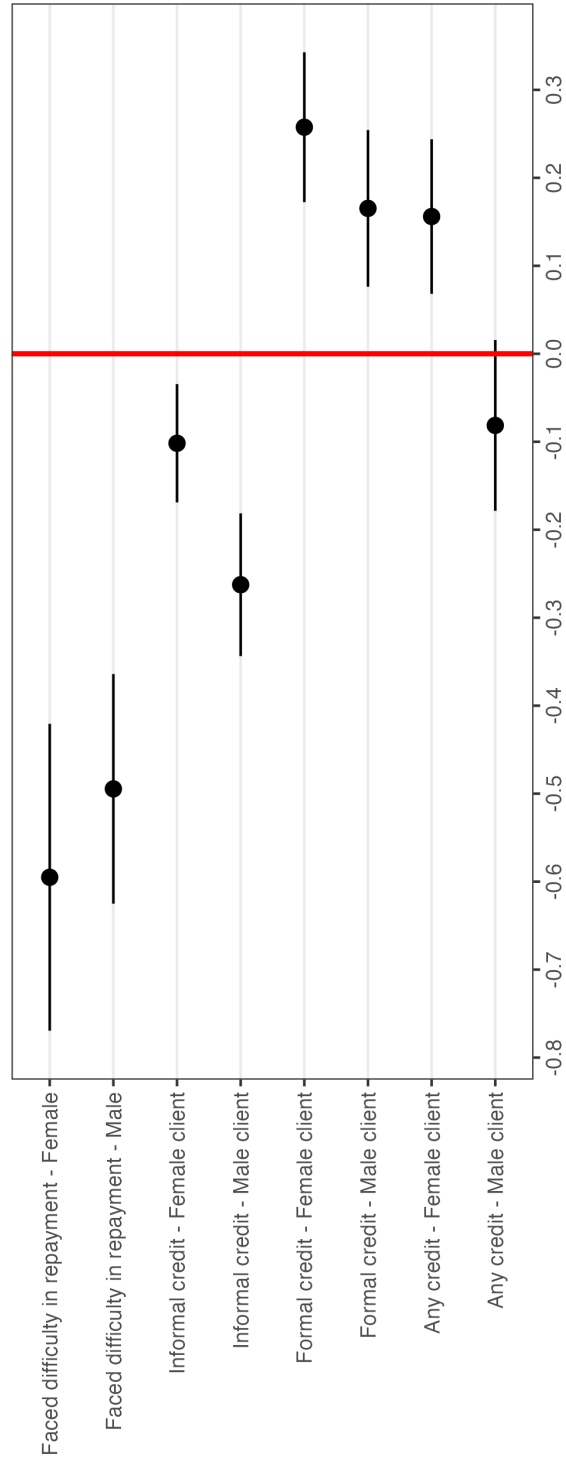


Figure 4: Program effects (ITT) on credit uptake, formal vs. informal credit sources, and challenges with loan repayment, by gender
 Note: Bars indicate 95 percent confidence intervals.

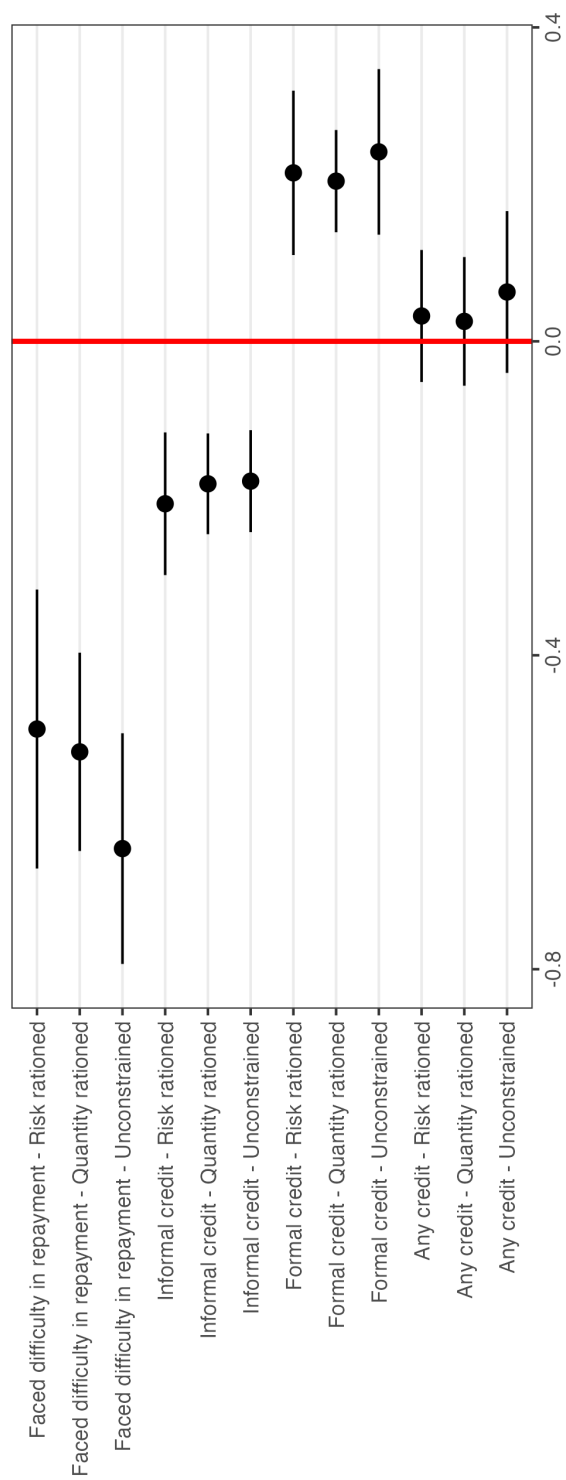


Figure 5: Program effects (ITT) on credit uptake, formal vs. informal credit sources, and challenges with loan repayment, by baseline credit rationing status
 Note: Bars indicate 95 percent confidence intervals.

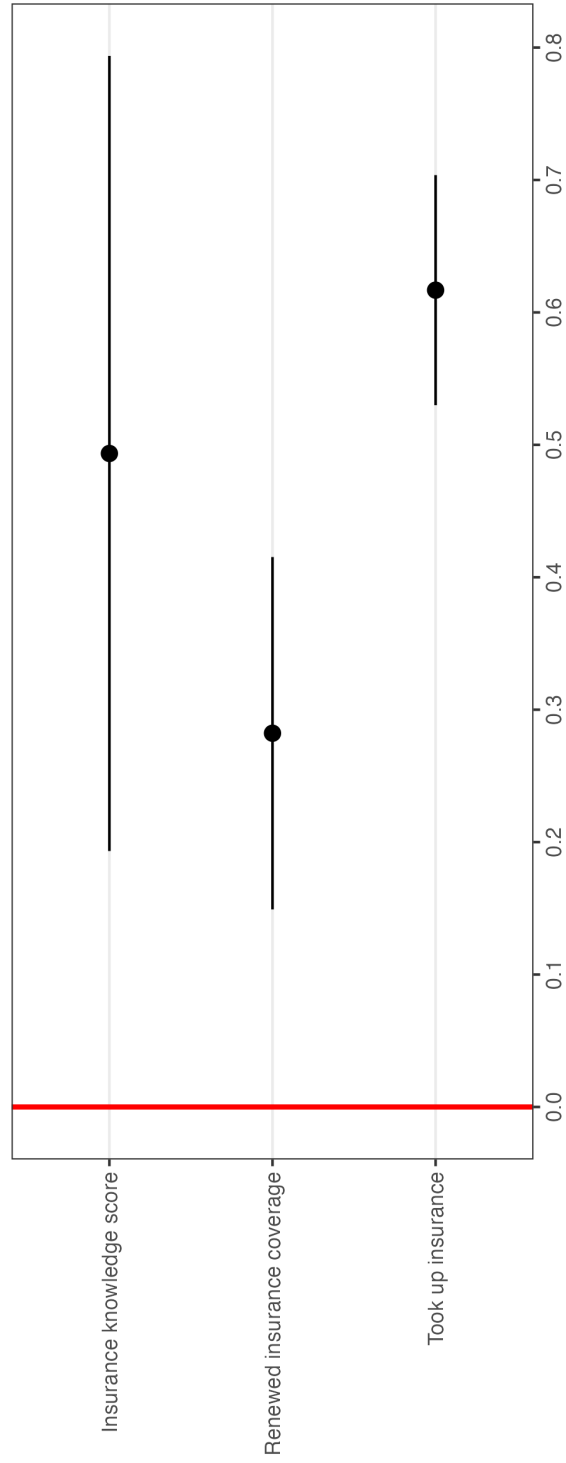


Figure 6: Program effects (ITT) on insurance uptake, insurance renewal, and insurance knowledge score among full sample
Note: Bars indicate 95 percent confidence intervals.

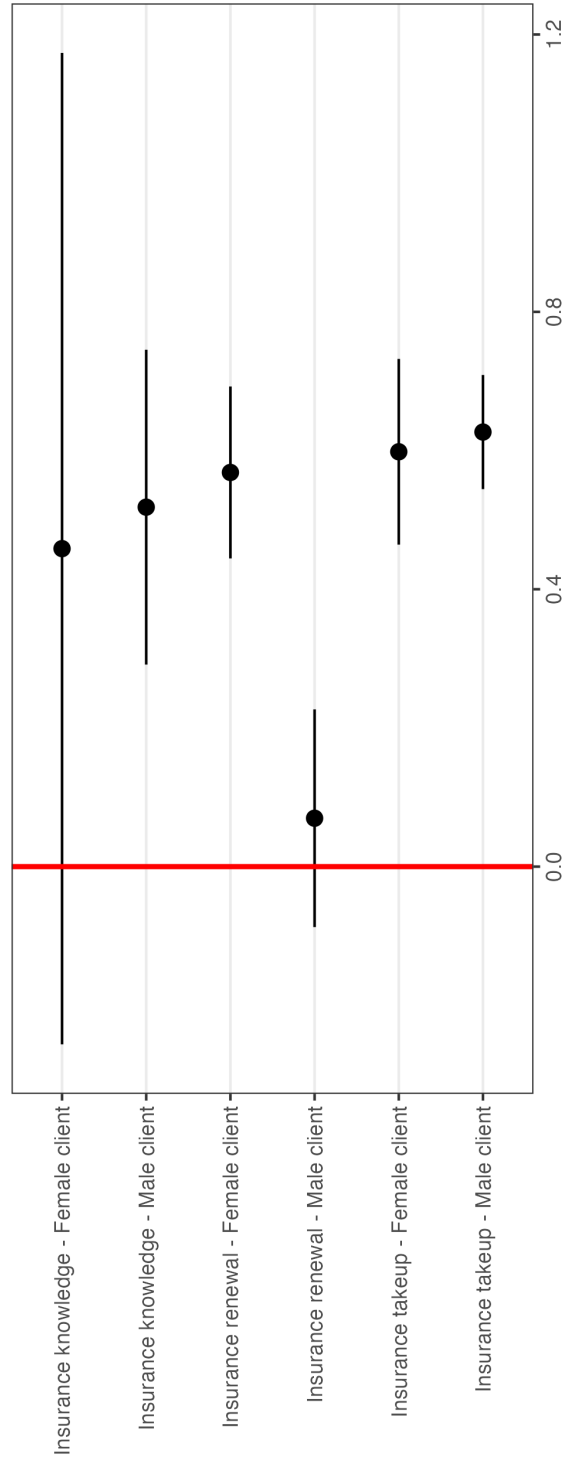


Figure 7: Program effects (ITT) on insurance uptake, insurance renewal, and insurance knowledge score, by gender
Note: Bars indicate 95 percent confidence intervals.

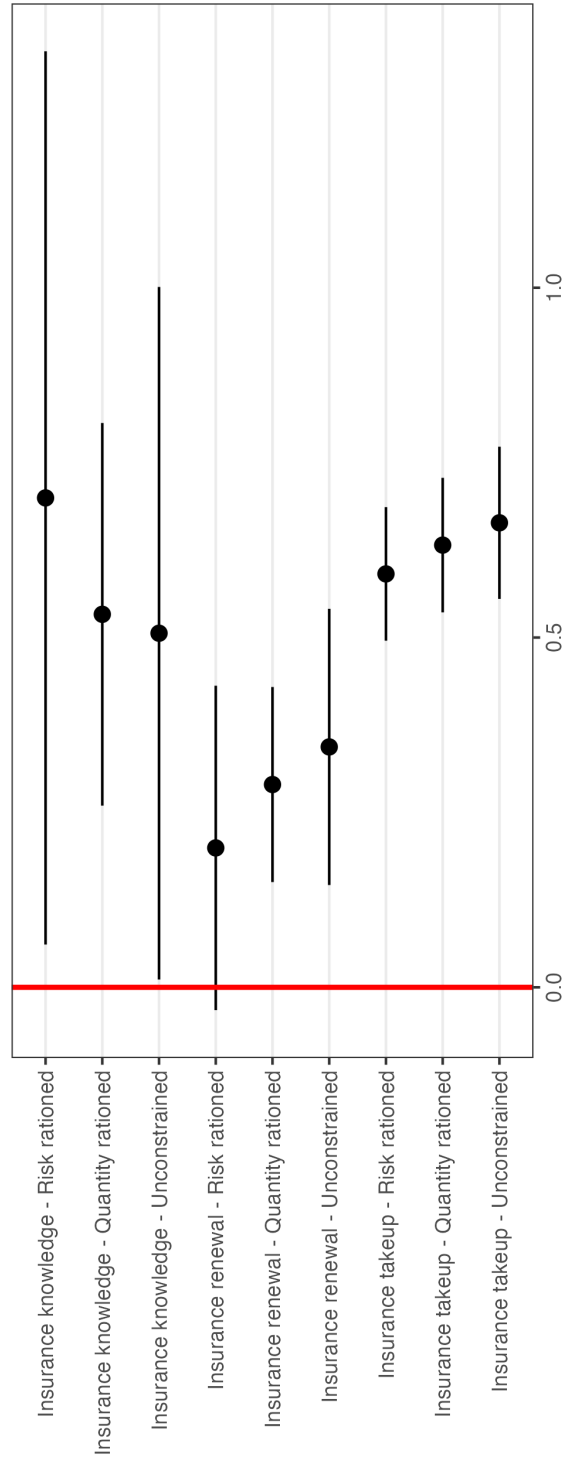


Figure 8: Program effects (ITT) on insurance uptake, insurance renewal, and insurance knowledge score, by baseline credit rationing status
 Note: Bars indicate 95 percent confidence intervals.

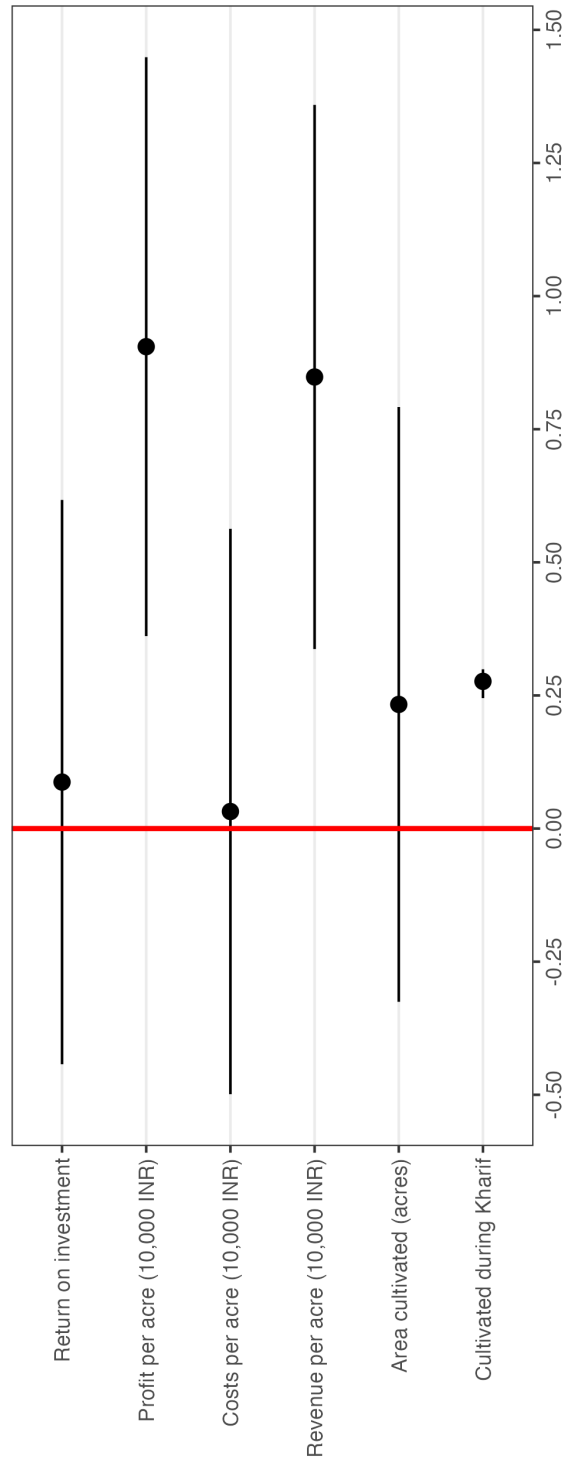


Figure 9: Average treatment effects (ATT) on agricultural outcomes during the Kharif season among full sample
 Note: Bars indicate 95 percent confidence intervals.

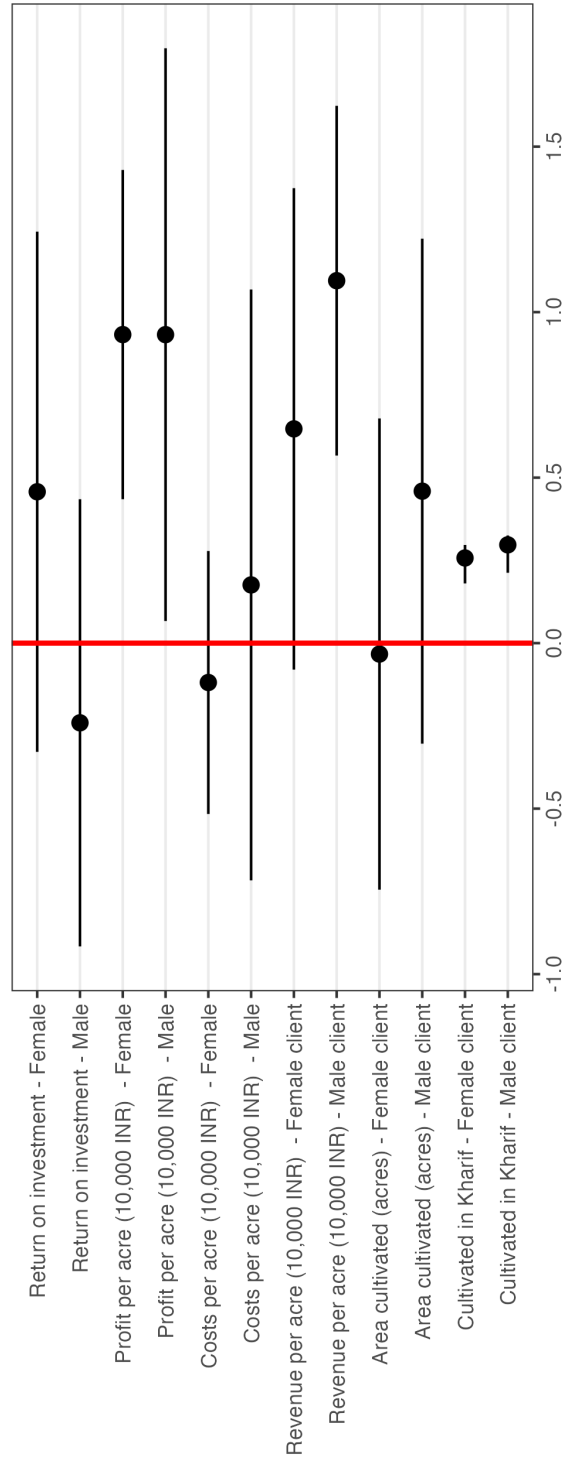


Figure 10: Average treatment effects (ATT) on agricultural outcomes during the Kharif season, by gender
 Note: Bars indicate 95 percent confidence intervals.

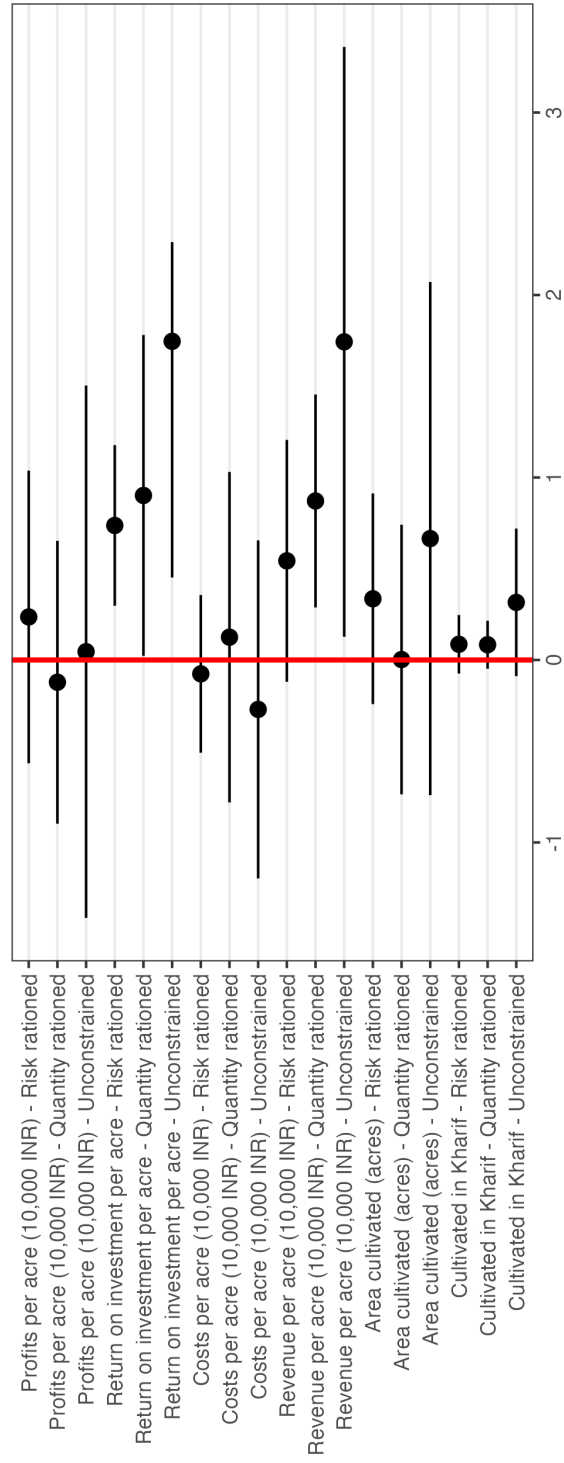


Figure 11: Average treatment effects (ATT) on agricultural outcomes during the Kharif season, by baseline credit rationing status
Note: Bars indicate 95 percent confidence intervals.

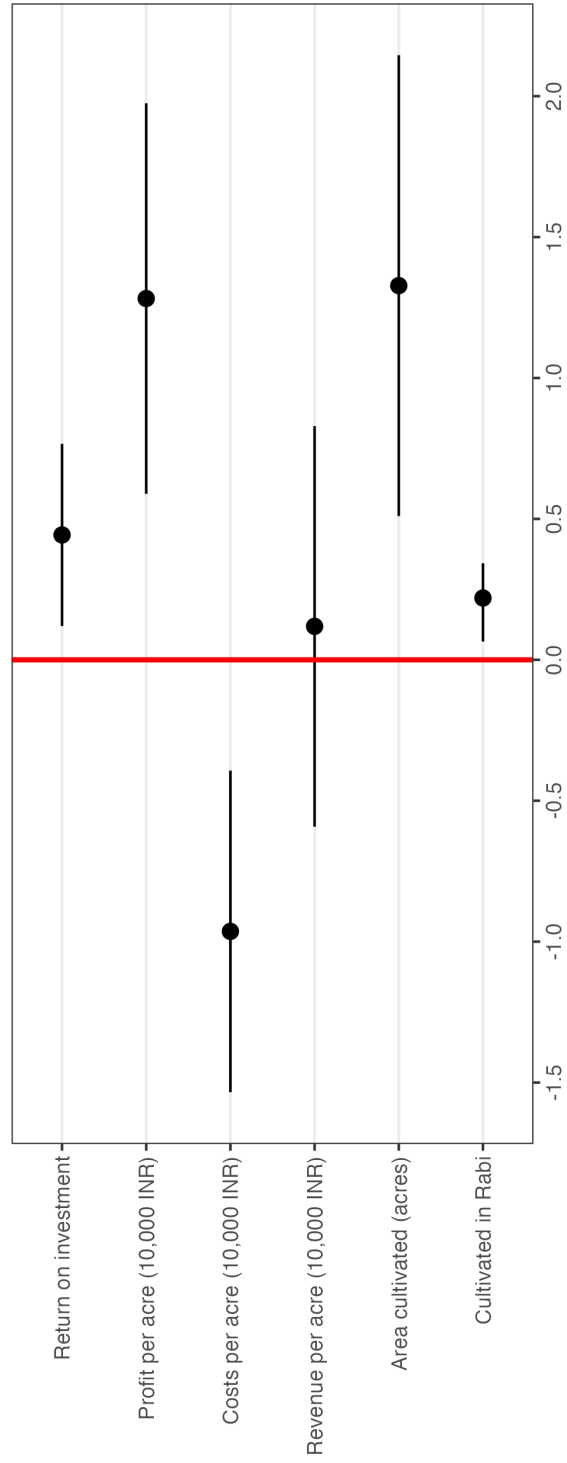


Figure 12: Average treatment effects (ATT) on agricultural outcomes during the Rabi season among full sample

Note: Bars indicate 95 percent confidence intervals.

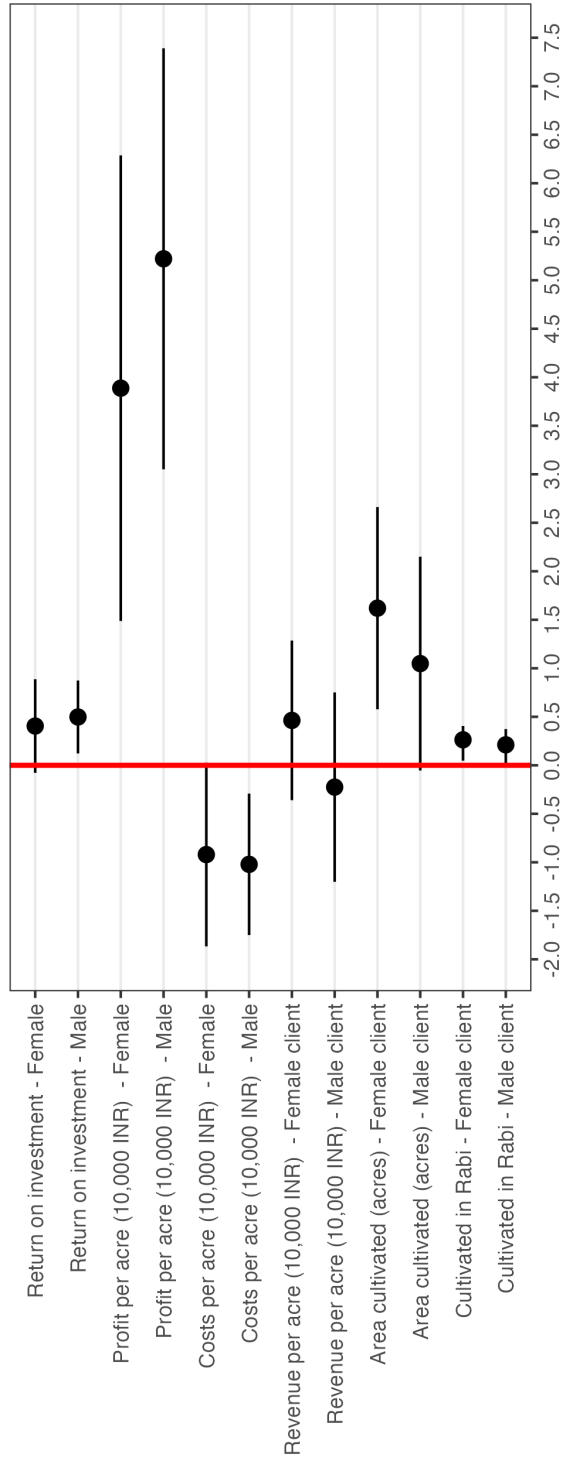


Figure 13: Average treatment effects (ATT) on agricultural outcomes during the Rabi season, by gender
Note: Bars indicate 95 percent confidence intervals.

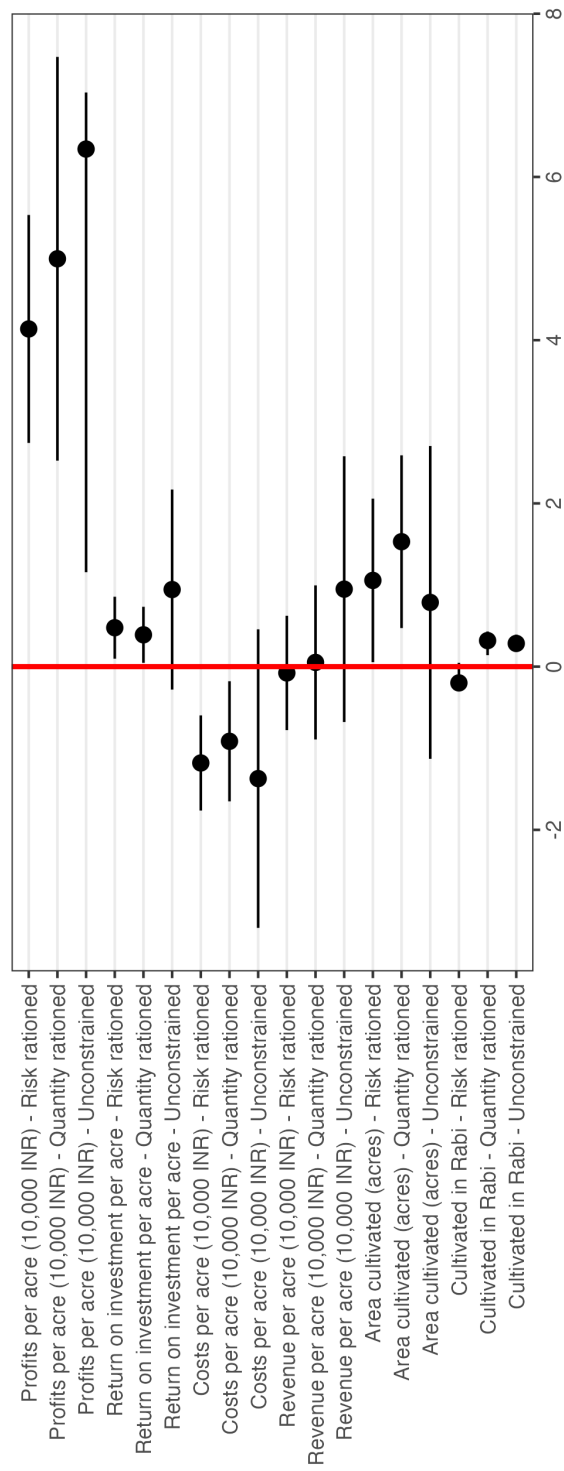


Figure 14: Average treatment effects (ATT) on agricultural outcomes during the Rabi season, by baseline credit rationing status
Note: Bars indicate 95 percent confidence intervals.

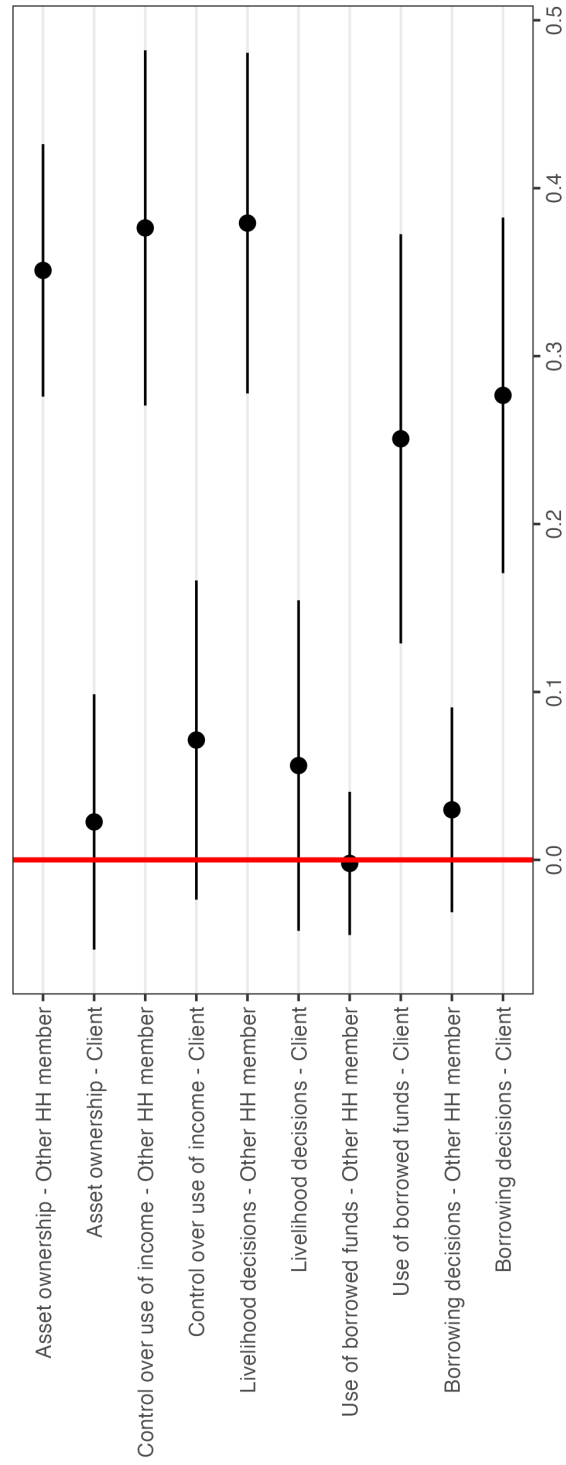


Figure 15: Program effects (ITT) on women's contributions to household financial decision making, control over income, and asset ownership: Female DER clients vs. other female family members
 Note: Bars indicate 95 percent confidence intervals.

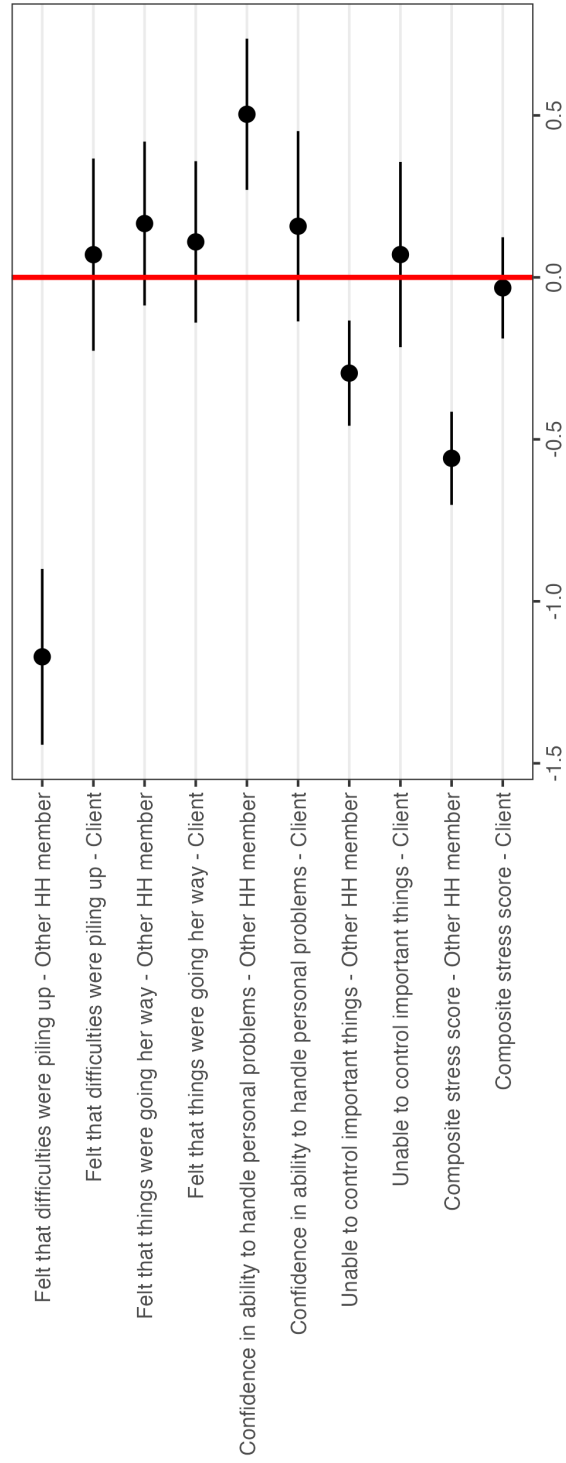


Figure 16: Program effects (ITT) on perceived stress: Female DER clients vs. other female family members
 Note: Bars indicate 95 percent confidence intervals.

A Regression tables: Program effects (ITT) on credit uptake, formal vs. informal credit sources, and challenges with loan repayment

Table A1: OLS regressions: Program effects (ITT) on credit outcomes among full sample

	Took up credit	Credit source: Formal	Credit source: Informal	Faced difficulty in repayment
	(1)	(2)	(3)	(4)
Treatment	0.022 (0.035)	0.212*** (0.032)	-0.199*** (0.028)	-0.559*** (0.052)
Controls	Yes	Yes	Yes	Yes
Observations	1,621	1,621	1,621	632
R ²	0.128	0.147	0.090	0.376

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All dependent variables are binary. We do not have baseline data for credit source (formal vs. informal) nor whether the client faced difficulty in repaying loans, so we are unable to control for baseline values of dependent variables in columns (2)-(4). Data on whether the client faced difficulty in repaying loans is only available among respondents who indicated that they had taken out a loan. All regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

Table A2: OLS regressions: Program effects (ITT) on credit outcomes by gender

	Took up credit	Credit source: Formal	Credit source: Informal	Faced difficulty in repayment
	(1)	(2)	(3)	(4)
Female	-0.188*** (0.048)	0.002 (0.038)	-0.207*** (0.049)	-0.039 (0.094)
Treatment	-0.081 (0.050)	0.165*** (0.045)	-0.263*** (0.041)	-0.495*** (0.067)
Treatment $\times$ female	0.237*** (0.068)	0.092 (0.066)	0.161*** (0.053)	-0.100 (0.111)
Controls	Yes	Yes	Yes	Yes
Observations	1,621	1,621	1,621	632
R ²	0.145	0.152	0.128	0.386

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All dependent variables are binary. We do not have baseline data for credit source (formal vs. informal) nor whether the client faced difficulty in repaying loans, so we are unable to control for baseline values of dependent variables in columns (2)-(4). Data on whether the client faced difficulty in repaying loans is only available among respondents who indicated that they had taken out a loan. All regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

Table A3: OLS regressions: Program effects (ITT) on credit outcomes by baseline credit rationing

	Took up credit (1)	Credit source: Formal (2)	Credit source: Informal (3)	Faced difficulty in repayment (4)
Quantity rationed	0.053* (0.031)	0.033 (0.022)	0.022 (0.029)	-0.110* (0.062)
Risk rationed	0.137*** (0.044)	0.081** (0.036)	0.082* (0.047)	-0.159* (0.089)
Treatment	0.063 (0.053)	0.241*** (0.054)	-0.178*** (0.033)	-0.646*** (0.075)
Treatment $\times$ quantity rationed	-0.037 (0.061)	-0.037 (0.064)	-0.003 (0.036)	0.123 (0.099)
Treatment $\times$ risk rationed	-0.031 (0.059)	-0.027 (0.066)	-0.029 (0.057)	0.152 (0.120)
Controls	Yes	Yes	Yes	Yes
Control group mean	0.353	0.353	0.14	0.14
Observations	1,527	1,527	1,527	599
R ²	0.143	0.163	0.119	0.394

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All dependent variables are binary. We do not have baseline data for credit source (formal vs. informal) nor whether the client faced difficulty in repaying loans, so we are unable to control for baseline values of dependent variables in columns (2)-(4). Data on whether the client faced difficulty in repaying loans is only available among respondents who indicated that they had taken out a loan. All regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls for control variables if missing observations for control variables were imputed.

B Regression tables: Program effects (ITT) on insurance uptake, renewal, and familiarity with crop insurance terms and conditions

Table B1: OLS regressions: Program effects (ITT) on insurance, take up, and renewal among full sample

	Took up insurance	Renewed insurance	Insurance knowledge score
	(1)	(2)	(3)
Treatment	0.614*** (0.043)	0.283*** (0.069)	0.496*** (0.158)
Controls	Yes	Yes	Yes
Control group mean	0.28	0.394	3.047
Observations	1,621	950	687

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. Insurance uptake and renewal variables are binary, while insurance knowledge score is an ordinal variable (ranging from 0 - 6) reflecting the number of correct responses to a series of 6 questions about crop insurance. We do not have data on insurance renewals or insurance knowledge from baseline, so all regressions control for baseline insurance take up to control for prior experience with crop insurance. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

Table B2: OLS regressions: Program effects (ITT) on insurance, takeup, and renewal by gender

	Took up insurance	Renewed insurance	Insurance knowledge score
	(1)	(2)	(3)
Female	-0.017 (0.063)	-0.455*** (0.075)	0.447 (0.354)
Treatment	0.627*** (0.042)	0.070 (0.080)	0.518*** (0.116)
Treatment $\times$ female	-0.028 (0.071)	0.499*** (0.089)	-0.060 (0.375)
Controls	Yes	Yes	Yes
Control group mean	0.28	0.394	3.047
Observations	1,621	950	687
R ²	0.411	0.162	0.191

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. Insurance uptake and renewal variables are binary, while insurance knowledge score is an ordinal variable (ranging from 0 - 6) reflecting the number of correct responses to a series of 6 questions about crop insurance. We do not have data on insurance renewals or insurance knowledge from baseline, so all regressions control for baseline insurance takeup to control for prior experience with crop insurance. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

Table B3: OLS regressions: Program effects (ITT) on insurance, takeup, and renewal among full sample

	Took up insurance	Renewed insurance	Insurance knowledge score
	(1)	(2)	(3)
Treatment	0.614*** (0.043)	0.283*** (0.069)	0.496*** (0.158)
Controls	Yes	Yes	Yes
Control group mean	0.28	0.394	3.047
Observations	1,621	950	687

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. Insurance uptake and renewal variables are binary, while insurance knowledge score is an ordinal variable (ranging from 0 - 6) reflecting the number of correct responses to a series of 6 questions about crop insurance. We do not have data on insurance renewals or insurance knowledge from baseline, so all regressions control for baseline insurance takeup to control for prior experience with crop insurance. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

C Regression tables: Program effects (ITT) on agricultural production, revenues, costs, profits, and return on agricultural investments during the Kharif season

Table C1: OLS regressions: Program effects (ITT) on agricultural outcomes during the Kharif season among full sample

	Cultivated rice during Kharif (=1)	Area cultivated (ac)	Revenue per acre (in 10,000 INR)	Cost per acre (in 10,000 INR)	Profit per acre (in 10,000 INR)	Return on investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.026* (0.014)	0.058 (0.073)	0.213*** (0.066)	0.008 (0.068)	0.227*** (0.068)	0.022 (0.068)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0.944	1.953	1.174	1.064	0.117	0.352
Observations	1,621	1,534	1,534	1,534	1,534	1,534
R ²	0.020	0.020	0.052	0.012	0.040	0.012

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls for missing observations for control variables were imputed.

Table C2: OLS regressions: Program effects (ITT) on agricultural outcomes during the Kharif season, by gender

	Cultivated rice during Kharif (=1) (1)	Area cultivated (ac) (2)	Revenue per acre (in 10,000 INR) (3)	Cost per acre (in 10,000 INR) (4)	Profit per acre (in 10,000 INR) (5)	Return on investment (6)
Female	0.044** (0.017)	0.122 (0.091)	0.011 (0.075)	0.048 (0.066)	-0.057 (0.069)	-0.144 (0.113)
Treatment	0.028 (0.020)	0.109 (0.097)	0.257*** (0.077)	0.042 (0.110)	0.217** (0.106)	-0.059 (0.082)
Treatment $\times$ female	-0.011 (0.020)	-0.125 (0.128)	-0.092 (0.109)	-0.078 (0.112)	0.032 (0.112)	0.191 (0.132)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0.959	1.953	1.174	1.064	0.117	0.352
Observations	1,621	1,534	1,534	1,534	1,534	1,534
R ²	0.028	0.022	0.053	0.012	0.040	0.014

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

Table C3: OLS regressions: Program effects (ITT) on agricultural outcomes during the Kharif season, by baseline credit rationing

	Cultivated rice during Kharif (=1) (1)	Area cultivated (ac) (2)	Revenue per acre (in 10,000 INR) (3)	Cost per acre (in 10,000 INR) (4)	Profit per acre (in 10,000 INR) (5)	Return on investment (6)
Quantity rationed	0.021 (0.024)	0.105 (0.081)	0.151 (0.101)	0.110 (0.125)	-0.024 (0.117)	0.065 (0.134)
Risk rationed	0.015 (0.033)	-0.034 (0.094)	0.149 (0.100)	0.093 (0.067)	0.006 (0.076)	-0.007 (0.161)
Treatment	0.048 (0.031)	0.106 (0.121)	0.265** (0.113)	-0.045 (0.075)	0.262*** (0.086)	0.008 (0.118)
Treatment $\times$ quantity rationed	-0.027 (0.027)	-0.107 (0.112)	-0.032 (0.130)	0.080 (0.155)	-0.022 (0.153)	-0.042 (0.143)
Treatment $\times$ risk rationed	-0.023 (0.039)	-0.004 (0.141)	-0.110 (0.125)	0.021 (0.082)	-0.046 (0.096)	0.066 (0.173)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0.944	1.953	1.174	1.064	0.117	0.352
Observations	1,527	1,442	1,442	1,442	1,442	1,442
R ²	0.023	0.023	0.055	0.014	0.041	0.011

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

D Regression tables: Program effects (ITT) on agricultural production, revenues, costs, profits, and return on agricultural investments during the Rabi season

Table D1: OLS regressions: Program effects (ITT) on agricultural outcomes during the Rabi season, among full sample

	Cultivated during Rabi (=1) (1)	Area cultivated (ac) (2)	Revenue per acre (in 10,000 INR) (3)	Cost per acre (in 10,000 INR) (4)	Profit per acre (in 10,000 INR) (5)	Return on investment (6)
Treatment	-0.008 (0.038)	0.343*** (0.104)	0.031 (0.093)	-0.251*** (0.075)	0.334*** (0.087)	0.115*** (0.040)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0.71	1.146	0.825	1.036	-0.309	-0.032
Observations	1,621	1,432	1,432	1,432	1,432	1,432
R ²	0.056	0.081	0.062	0.053	0.020	0.045

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

Table D2: OLS regressions: Program effects (ITT) on agricultural outcomes during the Rabi season, among full sample

	Cultivated during Rabi (=1) (1)	Area cultivated (ac) (2)	Revenue per acre (in 10,000 INR) (3)	Cost per acre (in 10,000 INR) (4)	Profit per acre (in 10,000 INR) (5)	Return on investment (6)
Female	-0.116* (0.060)	-0.071 (0.116)	-0.107 (0.116)	-0.023 (0.157)	-0.192 (0.184)	0.005 (0.066)
Treatment	-0.020 (0.042)	0.247* (0.135)	-0.059 (0.120)	-0.246*** (0.086)	0.230* (0.118)	0.120*** (0.044)
Treatment $\times$ female	0.044 (0.069)	0.204 (0.189)	0.197 (0.144)	-0.007 (0.154)	0.241 (0.199)	-0.010 (0.078)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0.71	1.146	0.825	1.036	-0.309	-0.032
Observations	1,621	1,432	1,432	1,432	1,432	1,432
R ²	0.065	0.083	0.065	0.054	0.021	0.045

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

Table D3: OLS regressions: Program effects (ITT) on agricultural outcomes during the Rabi season, by baseline credit rationing

	Cultivated during Rabi (=1) (1)	Area cultivated (ac) (2)	Revenue per acre (in 10,000 INR) (3)	Cost per acre (in 10,000 INR) (4)	Profit per acre (in 10,000 INR) (5)	Return on investment (6)
Quantity rationed	-0.002 (0.042)	-0.098 (0.127)	0.064 (0.094)	-0.051 (0.100)	-0.035 (0.210)	-0.021 (0.065)
Risk rationed	0.104** (0.043)	0.092 (0.122)	0.236** (0.102)	0.147 (0.094)	0.101 (0.155)	-0.006 (0.072)
Treatment	0.033 (0.063)	0.093 (0.156)	0.163 (0.130)	-0.206 (0.143)	0.316* (0.182)	0.150 (0.095)
Treatment $\times$ quantity rationed	-0.042 (0.067)	0.334 (0.230)	-0.149 (0.153)	-0.045 (0.156)	0.097 (0.235)	-0.043 (0.093)
Treatment $\times$ risk rationed	-0.102 (0.065)	0.206 (0.179)	-0.191 (0.155)	-0.149 (0.152)	-0.034 (0.188)	-0.008 (0.105)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0.71	1.146	0.825	1.036		-0.032
Observations	1,527	1,356	1,356	1,356	1,356	1,356
R ²	0.064	0.087	0.069	0.062	0.021	0.046

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

E Regression tables: Average treatment effects (LATE) on agricultural production, revenues, costs, profits, and return on agricultural investments among borrowers during the Kharif season

Table E1: Recursive bivariate probit regression: Program effects on decision to cultivate during Kharif

	Took up credit ("Yes" = 1) (1)	Cultivated during Kharif ("Yes" = 1) (2)
Panel A: Full sample		
Random assignment to treatment	1.405 (0.129)	
Took up credit		1.395 (0.259)
Controls	Yes	Yes
Control group proportion	0.353	0.944
Observations	1,621	1,621
Panel B: By gender		
<i>Men</i>		
Random assignment to treatment	1.630 (0.202)	
Took up credit		1.439 (0.402)
Controls	Yes	Yes
Control group proportion	0.471	0.927
Observations	849	849
<i>Women</i>		
Random assignment to treatment	1.264 (0.178)	
Took up credit		1.333 (0.422)
Controls	Yes	Yes
Control group proportion	0.228	0.962
Observations	772	772
Panel C: By baseline credit rationing status		
<i>Unrationed</i>		
Random assignment to treatment	1.109 (0.333)	
Took up credit		4.123 (64.369)
Controls	Yes	Yes
Control group proportion	0.247	0.930
Observations	307	307
<i>Quantity rationed</i>		
Random assignment to treatment	1.546 (0.195)	
Took up credit		1.441 (0.407)
Controls	Yes	Yes
Control group proportion	0.247	0.930
Observations	792	792
<i>Risk rationed</i>		
Random assignment to treatment	1.545 (0.253)	
Took up credit		4.843 (237.531)
Controls	Yes	Yes
Control group proportion	0.247	0.930
Observations	428	428

Note: Note: Standard errors in parentheses. Both dependent variables are binary. All regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed, except in Panel C, where one of the binary controls for an imputed control suffered from perfect separation..

Table E2: 2SLS regressions: Average treatment effects (LATE) on agricultural outcomes during the Kharif season, among full sample

	Area cultivated (ac) (1)	Revenue per acre (in 10,000 INR) (2)	Cost per acre (in 10,000 INR) (3)	Profit per acre (in 10,000 INR) (4)	Return on investment (5)
Treated	0.233 (0.285)	0.848*** (0.261)	0.032 (0.271)	0.905*** (0.277)	0.087 (0.270)
Controls	Yes	Yes	Yes	Yes	Yes
Control group mean	1.953	1.174	1.064	0.117	0.352
Observations	1,534	1,534	1,534	1,534	1,534

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed. Random assignment to treatment group serves as the excluded instrument for identifying treatment effects among self-selected borrowers.

Table E3: 2SLS regressions: Average treatment effects (LATE) on agricultural outcomes during the Kharif season, by gender

	Area cultivated (ac)	Revenue per acre (in 10,000 INR)	Cost per acre (in 10,000 INR)	Profit per acre (in 10,000 INR)	Return on investment
	(1)	(2)	(3)	(4)	(5)
Female	0.129 (0.099)	0.006 (0.086)	0.054 (0.072)	-0.073 (0.076)	-0.162 (0.123)
Treated	0.459 (0.389)	1.095*** (0.270)	0.176 (0.455)	0.932** (0.441)	-0.241 (0.345)
Treated $\times$ female	-0.492 (0.459)	-0.448 (0.390)	-0.295 (0.437)	0.0002 (0.443)	0.698 (0.512)
Controls	Yes	Yes	Yes	Yes	Yes
Control group mean	1.953	1.174	1.064	0.117	0.352
Observations	1,534	1,534	1,534	1,534	1,534

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed. Random assignment to treatment group and the interaction between random assignment and gender (binary variable equal to unity if the client is a female) serve as the excluded instruments for identifying treatment effects among self-selected borrowers.

Table E4: 2SLS regressions: Average treatment effects (LATE) on agricultural outcomes during the Kharif season, by baseline credit rationing status

	Area cultivated (ac)	Revenue per acre (in 10,000 INR)	Cost per acre (in 10,000 INR)	Profit per acre (in 10,000 INR)	Return on investment
	(1)	(2)	(3)	(4)	(5)
Quantity rationed	0.109 (0.081)	0.177* (0.099)	0.111 (0.119)	0.003 (0.111)	0.065 (0.132)
Risk rationed	-0.032 (0.096)	0.158 (0.100)	0.093 (0.065)	0.016 (0.077)	-0.007 (0.162)
Treated	0.665 (0.717)	1.743** (0.825)	-0.271 (0.473)	1.746*** (0.660)	0.045 (0.744)
Treated $\times$ quantity rationed	-0.663 (0.607)	-0.872 (0.833)	0.396 (0.662)	-0.845 (0.760)	-0.167 (0.745)
Treated $\times$ risk rationed	-0.330 (0.724)	-1.200 (0.790)	0.195 (0.437)	-1.009 (0.653)	0.190 (0.836)
Controls	Yes	Yes	Yes	Yes	Yes
Control group mean	1.953	1.174	1.064	0.117	0.352
Observations	1,442	1,442	1,442	1,442	1,442

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed. Random assignment to treatment group and the interaction between random assignment and credit rationing (series of binary variables equal to unity if the client is either quantity rationed or risk rationed) serve as the excluded instruments for identifying treatment effects among self-selected borrowers.

F Regression tables: Average treatment effects (LATE) on agricultural production, revenues, costs, profits, and return on agricultural investments among borrowers during the Rabi season

Table F1: Recursive bivariate probit regression: Program effects on decision to cultivate during Rabi

	Took up credit ("Yes" = 1) (1)	Cultivated during Rabi ("Yes" = 1) (2)
Panel A: Full sample		
Random assignment to treatment	1.536 (0.131)	
Took up credit		0.577 (0.227)
Controls	Yes	Yes
Control group proportion	0.353	0.710
Observations	1,621	1,621
Panel B: By gender		
<i>Men</i>		
Random assignment to treatment	1.676 (0.199)	
Took up credit		0.558 (0.311)
Controls	Yes	Yes
Control group proportion	0.471	0.798
Observations	849	849
<i>Women</i>		
Random assignment to treatment	1.357 (0.181)	
Took up credit		0.720 (0.330)
Controls	Yes	Yes
Control group proportion	0.228	0.617
Observations	772	772
Panel B: By baseline credit rationing status		
<i>Unrationed</i>		
Random assignment to treatment	1.076 (0.324)	
Took up credit		1.732 (0.529)
Controls	Yes	Yes
Control group proportion	0.247	0.651
Observations	307	307
<i>Quantity rationed</i>		
Random assignment to treatment	1.608 (0.192)	
Took up credit		0.908 (0.302)
Controls	Yes	Yes
Control group proportion	0.247	0.651
Observations	792	792
<i>Risk rationed</i>		
Random assignment to treatment	1.626 (0.238)	
Took up credit		-0.588 (0.371)
Controls	Yes	Yes
Control group proportion	0.247	0.651
Observations	428	428

Note: Note: Standard errors in parentheses. Both dependent variables are binary. All regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

Table F2: 2SLS regressions: Average treatment effects (LATE) on agricultural outcomes during the Rabi season, among full sample

	Cultivated during Rabi (=1)	Area cultivated (ac)	Revenue per acre (in 10,000 INR)	Cost per acre (in 10,000 INR)	Profit per acre (in 10,000 INR)	Return on investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.033 (0.155)	1.328*** (0.417)	0.119 (0.363)	-0.964*** (0.291)	1.282*** (0.353)	0.443*** (0.165)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0.71	1.146	0.825	1.036	-1.905	-0.032
Observations	1,621	1,432	1,432	1,432	1,432	1,432

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed. Random assignment to treatment group serves as the excluded instruments for identifying treatment effects among self-selected borrowers.

Table F3: 2SLS regressions: Average treatment effects (LATE) on agricultural outcomes during the Rabi season, by gender

	Cultivated during Rabi (=1) (1)	Area cultivated (ac) (2)	Revenue per acre (in 10,000 INR) (3)	Cost per acre (in 10,000 INR) (4)	Profit per acre (in 10,000 INR) (5)	Return on investment (6)
Female	-0.116* (0.067)	-0.123 (0.131)	-0.128 (0.129)	0.001 (0.179)	0.529 (0.477)	-0.006 (0.075)
Treated	-0.096 (0.184)	1.048* (0.562)	-0.225 (0.498)	-1.021*** (0.372)	5.221*** (1.107)	0.498*** (0.192)
Treated $\times$ female	0.177 (0.273)	0.572 (0.654)	0.688 (0.534)	0.101 (0.597)	-1.334 (1.433)	-0.093 (0.277)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0.71	1.146	0.825	1.036	-1.905	-0.032
Observations	1,621	1,432	1,432	1,432	992	1,432

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed. Random assignment to treatment group and the interaction between random assignment and gender (binary variable equal to unity if the client is a female) serve as the excluded instruments for identifying treatment effects among self-selected borrowers.

Table F4: 2SLS regressions: Average treatment effects (LATE) on agricultural outcomes during the Rabi season, by baseline credit rationing

	Cultivated during Rabi (=1)	Area cultivated (ac)	Revenue per acre (in 10,000 INR)	Cost per acre (in 10,000 INR)	Profit per acre (in 10,000 INR)	Return on investment
	(1)	(2)	(3)	(4)	(5)	(6)
Quantity rationed	-0.002 (0.042)	-0.060 (0.124)	0.067 (0.091)	-0.079 (0.098)	-0.411 (0.546)	-0.008 (0.064)
Risk rationed	0.105** (0.043)	0.107 (0.125)	0.236** (0.101)	0.137 (0.095)	0.017 (0.299)	-0.002 (0.070)
Treated	0.199 (0.423)	0.787 (0.978)	0.949 (0.831)	-1.372 (0.933)	6.342** (2.645)	0.944 (0.625)
Treated $\times$ quantity rationed	-0.237 (0.407)	0.744 (1.097)	-0.898 (0.784)	0.457 (0.911)	-1.344 (2.860)	-0.554 (0.592)
Treated $\times$ risk rationed	-0.423 (0.387)	0.270 (0.958)	-1.027 (0.816)	0.192 (0.872)	-2.204 (2.473)	-0.468 (0.596)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0.71	1.146	0.825	1.036	-1.905	-0.032
Observations	1,527	1,356	1,356	1,356	939	1,356

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed. Random assignment to treatment group and the interaction between random assignment and credit rationing (series of binary variables equal to unity if the client is either quantity rationed or risk rationed) serve as the excluded instruments for identifying treatment effects among self-selected borrowers.

G Regression tables: Program effects (ITT) on contributions to household decision making

Table G1: OLS regressions: Program effects (ITT) on measures of women's empowerment, among DER clients

	Borrowing decision (1)	Use of borrowed funds (2)	Input in livelihoods (3)	Control use of income (4)	Asset ownership (5)
Female	−0.521*** (0.035)	−0.521*** (0.041)	−0.217*** (0.044)	−0.158*** (0.052)	−0.063** (0.032)
Treatment	0.014 (0.039)	0.006 (0.041)	−0.030 (0.048)	0.054 (0.051)	0.045** (0.018)
Treatment × female	0.262*** (0.071)	0.245*** (0.083)	0.087 (0.063)	0.017 (0.069)	−0.022 (0.041)
Controls	Yes	Yes	Yes	Yes	Yes
Control group mean	0.427	0.45	0.803	0.716	0.893
Observations	1,621	1,621	1,621	1,621	1,621
R ²	0.320	0.331	0.094	0.091	0.032

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for a baseline index of empowerment. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

Table G2: OLS regressions: Program effects (ITT) on measures of women's empowerment, among other household members

	Borrowing decision (1)	Use of borrowed funds (2)	Input in livelihoods (3)	Control use of income (4)	Asset ownership (5)
Female	−0.521*** (0.043)	−0.512*** (0.042)	−0.385*** (0.055)	−0.339*** (0.050)	−0.238*** (0.035)
Treatment	−0.070 (0.057)	−0.026 (0.056)	−0.051 (0.056)	0.005 (0.057)	0.024 (0.028)
Treatment × female	0.099 (0.065)	0.024 (0.059)	0.431*** (0.069)	0.371*** (0.067)	0.327*** (0.040)
Controls	Yes	Yes	Yes	Yes	Yes
Control group mean	0.373	0.368	0.562	0.525	0.744
Observations	1,294	1,294	1,294	1,294	1,294
R ²	0.470	0.511	0.183	0.160	0.254

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level. Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for a baseline index of empowerment. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

H Regression tables: Program effects (ITT) on perceived stress among women

Table H1: OLS regressions: Program effects (ITT) on measures of stress and emotional health, among DER clients

	Composite stress score	Unable to control important things	Confidence in ability to handle personal problems	Felt that things were going her way	Felt that difficulties were piling up
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.032 (0.080)	0.070 (0.146)	0.158 (0.150)	0.110 (0.127)	0.070 (0.151)
Controls	Yes	Yes	Yes	Yes	Yes
Control group mean	2.633	2.907	3.677	3.404	2.704
Observations	772	772	772	772	772
R ²	0.044	0.020	0.095	0.054	0.028

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level.

Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

Table H2: OLS regressions: Program effects (ITT) on measures of stress and emotional health, among other household members (non-clients)

	Composite stress score	Unable to control important things	Confidence in ability to handle personal problems	Felt that things were going her way	Felt that difficulties were piling up
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.558*** (0.073)	-0.296*** (0.083)	0.504*** (0.119)	0.166 (0.129)	-1.171*** (0.138)
Controls	Yes	Yes	Yes	Yes	Yes
Control group mean	2.633	2.907	3.677	3.404	2.704
Observations	740	740	740	740	740
R ²	0.243	0.180	0.155	0.144	0.269

* Significant at 10% level; ** Significant at 5% level; *** Significant at 1% level.

Note: Standard errors adjusted for clustering at the village level in parentheses. All regressions control for baseline values of dependent variable. In addition, all regressions control for block fixed effects and baseline imbalances (specifically, insurance uptake, agricultural profits per acre excluding the cost of own labor, caste, and whether the client had cultivated during the preceding Rabi season) as well as additional control variables including age, literacy, and a binary indicator for whether Dvara E-Registry had ongoing operations in the client's village at the time of baseline (as opposed to initiating new operations as part of this project). We also include binary controls if missing observations for control variables were imputed.

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